EXPLANATIONS WITHOUT CAUSES AND CAUSES WITHOUT REASONS
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ABSTRACT

Much of the current discomfort with mainstream methodology in the social sciences is rooted either in outright rejection of or ambivalence toward a Humean model of causation. The Humean legacy has different parts, however. It is not just a question of constant conjunction. Another part of the putative legacy is the dualism of belief and desire (reason and the passions) that underpins a simple and influential model of instrumentalism. As it stands however, current debates about methodology, particularly the innovations of those participants who are self-consciously anti-Humean, whether in a strong or weak form, do not appear to have recognized that a full-blown challenge requires the rejection of instrumentalism and not simply the rejection of the constant conjunction model of causation. And they therefore do not recognize that what may be left at the heart of the human sciences are explanations without causes.

In order to examine this possibility – that there are explanations without causes lodged in the heart of social science – this paper examines reason (or intentional) explanations and their place in the human sciences. At least since Davidson, the dominant position in the philosophy of action has been that reasons are causes and that reason explanations are causal explanations. However there is a growing literature in the philosophy of action which challenges instrumentalism, in part by arguing that reason explanations are not causal.

This paper examines the place of explanations without causes in the social and human sciences. First of all, do such explanations exist? Second, if they do exist, what is their logical structure? Third, what is their import? And, finally, assuming for the moment that they exist, what are their problems?

The paper is both an argument and a thought experiment. As argument, it examines reason explanations construed non-causally. As thought experiment, it considers the consequences of introducing non-causal reason explanations into the methodology of comparative social research and into social theory.

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This paper examines the place of “explanations without causes” in the social sciences. First of all, do such explanations exist? Second, if they do exist, what is their logical structure? Third, what is their import? And, finally, assuming for the moment that they exist, what are their problems?

By “explanations without causes”, I have in mind reason explanations. For some time, it has been assumed that these are causal explanations. The work of Donald Davidson, in particular, has provided support for this assumption.

His arguments, however, have been recently challenged. The result is that this assumption can no longer be made simply by automatic reference to the arguments of Davidson, or without more detailed defense. Further, the challenges to his work raise the possibility that reason explanations indeed do explain, but are not causal. Hence reason-explanations can be explanations without causes.

The paper examines recent work in the philosophy of action which challenges Davidson’s arguments. In developing the concept of a reason explanation, and considering its logical structure, the paper draws as well on an earlier literature on propositional attitudes.

The relevance of this examination of reason explanations is demonstrated in two further ways: by considering the implications of reason explanations for the methodology of comparative social research and for social theory.

If reason explanations are not causal, one alternative in the social sciences (aside from reasons as causes) is causal explanation without reasons. In examining reason
explanations, I therefore also consider the plausibility of this alternative form of explanation -- that is causal explanation without reasons.

In separating cause and explanation, I am proposing that there are explanations which are not causal. This proposal is made for something fairly fundamental – human action\(^1\). One way to escape this construction of explanations without causes is to deny the importance of human action by claiming that it is not the core subject matter of the social sciences. This move works by conceding that the explanation of action is not causal but makes the further claim that action is not central to the social sciences. Social science then can rest on causal explanation precisely because the social sciences do not study action. Yet if action is not central to the social sciences, then how would we describe its subject matter? If, on the other hand, it is conceded that action is core to our subject matter, then we should consider the place of explanations without causes – reason explanations – in the social and human sciences\(^2\).

II

The most general form of explanation relative to persons in the human sciences is the reason explanation. A reason explanation converts an empirical association into an explanation. While there is a long standing argument that reason-explanations are causal (Davidson, 2001[1963]), there are more recent and persuasive arguments that reason explanations are explanations without causes, that is that they are non-causal (see, for example, Schueler, 2003; Vogler, 2002). This is the position taken up and developed in

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\(^1\) I am here restricting causal explanations to those which use efficient causes. The issue is not whether reasons are causes in some ordinary non-technical sense of ‘cause’ such that reason explanations of actions are causal by fiat, because any explanation about how something happens is a causal account. Instead the issue is whether reason explanations have explanatory force without any essential reference to efficient causation. See Schueler (2003: 18-20).

\(^2\) I do not want to presuppose too much in identifying these intellectual activities and practices – hence I move back and forth in the paper between these two descriptors.
this paper. Reason explanations rationalize action. Reason explanations are relative to persons and what they explain – action – has an intentional structure which is calculative in form. This is to say that a reason explanation converts an empirical association into an explanation in a particular way – that is, without invoking or depending on a causal relation or mechanism.

The conversion of an association into an explanation typically has come to rest on the importance of the specification of causal mechanisms (eg. Hedström and Swedberg, 1998). Viewed in light of the previous paragraph, however, it would be a mistake to think of reason explanations as causal mechanisms and, perhaps, as mechanisms at all. If mechanisms must be causal to be mechanisms then it follows that reason explanations are not mechanical and reasons are not mechanisms.

The arguments adduced in support of the turn to mechanisms do not really motivate the interest in reason explanations here in this paper. Reason explanations do not matter because of their status as a kind of middle-ground between the nomothetic and the ideographic, as a substitute for covering-law models of causal explanation, although this is one influential justification for the introduction of mechanisms (Elster 2007, 1993). Nor are reason explanations important because they contribute to solutions to the small N problem of case study-oriented qualitative research, which has been another important justification for the turn to mechanisms (eg. Bennett and George, 2004; Mahoney, 2003: 363-365, 2000). More generally, the interest in reason explanations is not motivated by the metaphor of the “black-box” which must be opened up and specified before a satisfactory causal explanation is achieved. The problem with these kinds of justifications
for introducing mechanisms into explanations is that they do not give full weight to the importance of action.

III

Action has an internal structure which is not adequately recognized in work on causality, even that literature on mechanisms which is designed to open up the black box.

First of all, action is intentional; not to impose this qualification would be to admit involuntary behaviors and gestures (Vogler, 2002: 218).

Second, action which is intentional under one description need not be intentional under another description (Anscombe, 1957).

Third, non-causal intentional explanations place action in intensional contexts – the indirect contexts of propositional attitudes. In contrast, Davidson (2001[1963], 1967; cf. Churchland, 1970) placed action in an extensional context so that he could talk of the same act under different descriptions. The first argument emphasizes sense, the second emphasizes reference (Frege, 1952[1892], cf. Hintikka, 1969; Stine, 1973; Kripke, 1972)\(^3\).

Fourth, intentional action is structured by the internal relation of means to ends. This relationship is general because formal: nothing is said about the substantive contents of particular ends or means.

Fifth, the central relation is calculative – the matching of means to ends – so that “any ordinary intentional action is a candidate for calculative reasons explanations” (Vogler, 2002: 150; cf. Schueler, 2003).

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\(^3\) Stine provides an argument which works to preserve the distinctiveness of propositional attitudes – their indirect contexts – against both essentialism and other possible world semantics which seek to avoid essentialism. This debate harkens back to Frege’s discussion of sense and reference, Kripke’s work on rigid designators figures in her discussion as a form of essentialism, Hintikka’s work enters as a possible world semantics for propositional attitudes which is not essentialist.
Sixth, the reason explanation takes the general “in order to” form rather than the “because of” form (Schutz, 1951; Vogler, 2002: 143, 169; Schueler, 2003, Setiya, 2007: 51-52).4

Seventh, the rational structure of intentional action is not reduced to psychological antecedents. Intentionality is not something behind, so to speak, or outside action (von Wright, 2004[1971]: 115).

Eighth, non-causal intentional explanation is not instrumental; that is, it is not based in belief-desire models of motivation. Non-causal intentional explanation, if not instrumentalist, is still calculative in form, however.

Finally, almost by way of summary of the above features and, as Weber recognized, the dualism of ends and means does not map onto the dualism of cause and effect5.

If reason explanations have explanatory force but are not causal how do reason explanations manage to explain? Both Vogler and Schueler consider this question. Schueler (2003: 50-55; 84-87) takes up an argument in Nagel (1990: 115)6. As Schueler (50) points out, Nagel’s point is like Davidson’s (but inverted). The latter holds, that unless reasons are causes, reason explanations really do not explain at all. “Nagel thinks that reasons explanations are not causal (since they apply to autonomous choices), hence

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4 Setiya’s account (2007: 56-59) is teleological while retaining a qualified place for causation. In taking up Anscombe’s (1957, 1983) arguments, he reserves a place for causation because being moved to act is being caused to act (57). This seems to say that, in some sense, I cause myself to act or that my intentions, which are on his reading desire-like beliefs, either cause me to act or cause my acts independent (in some sense) of myself. My reading here, however, may be one which Setiya thinks he can counter when he denies that we are passive when we act on our intentions (58). When he goes on to argue that “these causes are made effective by one’s intention in acting”, he does seem to say that in acting we are not passive transmitters of causal effects but, in so doing, also seems to weaken the contribution of intentions qua causes to the explanation of action.

5 I elaborate on this point about Weber later in the paper.

6 Vogler (2002: 18-24) has an excellent discussion of a related problem: how desire is ‘transferred’ from end to means.
he thinks that reasons explanations do not really explain” (50). Schueler shows how reason explanations include character traits of persons. Reasons can have explanatory force if we presume character-formation. This is to say that there is an empirical self doing the intending and that action presupposes a community of will – an intentional community. This move by Schueler preserves the internal non-causal structure of action.

The way that this explanatory gap is filled in by Vogler is through her specification of the means/ends relationship which structures intentional action. Given her attention to desirability characterizations of action and, more directly, her attention to the point of wrongdoing, I treat her solution as compatible with that of Schueler⁷. Vogler, moreover, provides support for a conclusion that actions which are done for a reason are all of intentional action -- that is, that reasons are a “defining or essential feature of action” (Schueler, 2003: 128).

Mele (2003: 80) has argued that “as long as Davidson’s challenge to noncausalists remains unmet, causalism will be the biggest game in town, if not the only one”. The arguments of Schueler and Vogler go some way toward meeting this challenge⁸.

This is not to say that the debate about action is anywhere near over, it continues in a renewed way. Discussion is now wider. The debates are now more open – this is a ‘post-Davidson’ period in the philosophy of action. The parameters to this debate, set in large part as a consequence of his arguments, are less settled. To some extent, it is the causalists and the ‘Humeans’ who now are on the defensive.

⁷ I will point to several differences between them shortly.
⁸ There are others as well who have argued recently against causalism and the Humean model of instrumentalism. There is a larger critical literature to which Schueler and Vogler make contributions which is not surveyed in this paper.
Nor is this to say that there are not important differences among the critics. I have emphasized for example the convergence between Schueler and Vogler. Yet there are relevant differences between them. At a minimum, both are resisting an account of action modeled on bodily movement, under which action is an event connected to antecedent events or states through a law-like relation. Davidson, however, is much more of the target in Schueler than in Vogler. Schueler is concerned with two separate problems: the belief-desire account of reason explanations and the claim that such explanations are causal. Vogler, in contrast, takes up a version of this first problem but via a different route – through the investigation of the structure of intentional action – while not directly taking up the second problem. Schueler is committed to a teleological alternative to causal reason explanations; Vogler is not explicitly committed to this alternative, although the ends-means relationship is generally taken to be teleological in form. The issue of explanation is not as central to her as it is to Schueler. It would be too much to say that Vogler would or should be committed to teleological explanation, once or if she turned her attention to explanation.

Further, for Schueler, reason explanations are intrinsically normative while, for Vogler, calculative reasons stand alone, they are not inherently normative – they do not need to be normative to provide a structure for intentional action. Indeed, one central point in her argument is that instrumentalism goes wrong because of its moral

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9 For an argument which parses reason explanations by arguing that reasons are not causes while treating reason explanations as causal explanations, see Hornsby (1997a). For Hornsby, ‘causation’ and ‘explanation’ are inextricably linked concepts and both are introduced when we are told why someone did something (1997a: 133). However her conception of ‘causal-explanatory’ in relation to action preserves a different standard of causal intelligibility when the subject matter is action, a standard which does not assimilate actions to the “impersonal world of causes” (133).

10 Compare the different uses they make of the work of Korsgaard (1997, 1986) and see also Dancy (2003).
psychological commitments and that a calculative view of practical reason does not turn on any specific moral psychology (Vogler, 2002: 23-24).

These are important differences, in particular the latter one. Later discussion will show that I tend to lean toward Vogler while acknowledging that the question of the normativity of action is still not settled. In particular, there is now in place an argument which rejects instrumentalism but which questions whether the nature of agency or of practical thought in themselves are enough to specify standards of practical reason, and which looks instead for those standards in the virtues of character (Setiya, 2007).

The remainder of the paper is both an argument and a thought experiment. As argument, it examines the import of reason explanations construed non-causally\textsuperscript{11}. As thought experiment, it considers the consequences of introducing non-causal reason explanations into the methodology of comparative social research and into social theory.

IV

Reasons, calculative or deliberative, are neither events nor relations nor conditions, which makes them difficult to construe in causal terms. The work on mechanisms is insufficiently sensitive to the possibility that there are explanations without causes because the model of explanation in play in this work is external. Relations between causes and effects are external relations, whether these relations hold over events, conditions, or processes. The model is extensionalist, whether the particular terms of art are conditions (necessary or sufficient), associations (linear, monotonic etc.) or processes (generative). But reasons are not external, extensionalist or objective in these

\textsuperscript{11} I will sometimes move back and forth between ‘reason explanations’ and ‘intentional explanations’ in this paper without conflating reasons and intentions.
senses, and so it does not follow directly that they are causes, even if they provide grounds for reason explanations.

The work on mechanisms does not recognize what is entailed when the importance of action in the human sciences is taken seriously. Very simply, this is so because of the continuity between mechanism talk and the deductive-nomological model of explanation. From its origins, and particularly as developed by Hempel (1942, 1965, 1966), this has been a model which explicitly assimilated explanation in the human sciences to explanation in the natural sciences (and to some form of physicalism). It is an expression of “methodological monism” and a commitment to the unity of scientific method (von Wright, 2004[1971]:4). The modifications to this model introduced via mechanisms are significant but they are still limited. They leave largely intact a methodology which is rooted in comparison and generalization in the search for external causes: No explanation without comparison and generalization (Norkus, 2005: 372) and no explanation which is not causal\textsuperscript{12}. This is the core of quasi-experimental comparative method\textsuperscript{13} in comparative political sociology from whence spring all of the methodological vices, from selection bias to omitted variables to conceptual stretching\textsuperscript{14}.

The way to assimilate action to a deductive-nomological model has been to argue, following Davidson (2001[1963], 1966, 1967) and in a move designed to preserve the

\textsuperscript{12} I do not want to run Hempel and Hume together here. Hempel provides an account of explanation which “subsumes” causation within a D-N model of explanation without treating explanation and causation as identical (Hempel, 1965: 347-348; see also 463-487); Hume’s influence lies in his work on causality specifically. His theory of causation is not a “subsumption” theory of explanation. In practice, and in rather loose terms, these approaches may be run together: Consider the contemporary interest in some quarters in (a) deductive theories of universal scope joined to (b) the inductive search for regularities.

\textsuperscript{13} The classical sources on quasi-experimental comparative method in political science are Lijphart (1971) and Przeworski and Teune (2001[1970]). An important background text is Campbell and Stanley (1966).

\textsuperscript{14} There are two skeptical problems in play in comparative method. One is the skeptical problem about generalization: “Can any number of observed instances, short of a complete survey, ever make it reasonable to believe a generalization?” This is closely related to the skeptical problem about the future, “which doubts that any known facts about past objects or events give any reason for beliefs about future objects or events.” (Hacking, 1975: 176).
unity of method, that reasons are causes and, correspondingly, that beliefs and desires are psychological antecedents of acts. Intentional explanation then is causal explanation\textsuperscript{15}. We could continue to take this line but it is now much less defensible on its own terms.

If we borrowed Davidson’s arguments at one point in time because of their importance in the philosophy of action and of mind and because they dovetailed with a methodological predisposition in the budding fields of social science, what should we do once his arguments have been effectively challenged in his own field? And, if we abandon his arguments, because we now conclude that they are defective, where are we left methodologically? What happens if there are no longer grounds for arguing that reasons are causes or that intentional explanations are causal explanations? This is the upshot of the interest in intentional explanations.

Reasons and reason explanations may look like a thin wedge but they raise important issues. Monism has always depended on the denial of difference between the natural and the social with regard to explanation. Yet if reasons cannot be assimilated to causes, then there are, for example, explanations without comparison or generalization (On the latter, compare Abell, 2003). If reasons are not causes, methodological monism has been violated pretty much at its heart. So, then, the interest in reason explanations is motivated by the thought that attention to them introduces a critical perspective on causes and mechanisms which is not internal to statistical, covering-law or mechanistic models of explanation.

\textsuperscript{15} For our purposes here, Davidson does point out how his account of reasons as causes differs from Hempel’s general treatment of scientific explanation, precisely because his (Davidson’s) account emphasizes the role of causality (Davidson, 2001[1976]: 262): belief and desire can explain an action only if they caused it. More specifically, Davidson’s monism is “anomalous”. Nonetheless, Hornsby has argued that Davidson’s anomalous monism is still physicalist since this version of monism is not so anomalous that mental events are not physical. She also points out that anomalous monism rests on the position that a law subsumes any two events between which a causal relation obtains (Hornsby, 1997: 61, 73). Davidson’s version of physicalism thus depends on a nomological thesis about causation (1997: 12 and 46-80).
If reasons are not causes then there is a form of explanation – intentional explanation – which reduces to neither causal explanation nor, it can now be added, to functional explanation. Then our options are richer – we are not reduced to a choice between causes and functions. This is to say that intentional explanation does not reduce to causal explanation because reasons are not causes; it does not reduce to functional explanation because reasons are relative to persons and because the central mechanism in functional explanation – natural selection – is explicitly not relative to persons. Notably, this is the feature which makes natural selection mechanical. “Since natural selection is purely mechanical, this means that functional explanations require no references to persons (or to God)” (Schueler, 2003: 23)\textsuperscript{16}. Shouldn’t every mechanism share this feature with the most important natural mechanism of all, and shouldn’t this make us skeptical of the current interest in mechanisms in the social sciences since little or none of this work would want to take on board the desideratum that their mechanisms must not be relative to persons?

Evolutionary theory has a difficult time dealing with intentions, and propositional attitudes generally. It wants to assimilate them to the mechanistic core of evolutionary theory. Evolutionary theory is hostile to intelligent design and action, which is designed and intelligent, is a problem for evolutionary theorists. They are reluctant, for different reasons than the Davidsonians, to leave intentions free-standing. So for someone like Dennett (1987, 1991) animals who adopt the “intentional stance” are drawing on an

\hspace{1cm}\textsuperscript{16} Vogler (2002: 19-20) effectively deflates a claim that evolutionary mechanisms account for how belief-desire models of motivation get purchase on practical reason.
evolved response to a hostile environment and are expressing an innate urge. More generally, they look for ways to naturalize humans in ways compatible with natural selection: make the mind equivalent to the informational functions of the brain (Pinker, 1997), make the mind identical to the physical brain so that all humans have the same minds (Lakoff and Johnson, 1999), convert “folk psychology” (propositional attitudes) into physical brain-states (Churchland, 1986; 1981)\textsuperscript{17}.

The point here, then, is that causal and functional explanations cannot deal with intentions because intentions are of free-standing relevance when the subject matter is action. Causalists and functionalists have tried to read intentions out of their accounts or have tried to assimilate intentions to their own theoretical terms but have not done so successfully.

VI

But, one might ask at this point, what difference do reason explanations make for methodology? What do they rule in and what do they rule out in terms of research practices? These are questions which help to situate the arguments here.

Mainstream methodology in comparative social research is parasitic on a set of assumptions about the structure of the social world. Mainstream methodology is causalist and one of the assumptions that makes it work, particularly via the simple model of instrumentalism associated with or produced by this assumption, has been that reasons are causes. If reasons are not causes, then this set of assumptions must be reworked and, in the process, methodology revised. So the methodological point of an ‘reason-explanation account’ is that, in treating intentions in the way that it does, a social ontology different than the one which motivates methodologies rooted in an ontology in

\textsuperscript{17} See Johnson (2003: 2-6, ff.).
which reasons are causes is introduced. It is a different ontology because, if it were not
different, then intentions could be assimilated either to causes or to functions.

In a thoughtful argument, Hall (2003) has proposed that, as these assumptions
(ontologies) have changed in the field of comparative politics in political science and
closely related fields in sociology so, too, have methodologies. Hall’s qualitatively-
oriented methodological revisions take into account an emerging ontology which, he
argues, is too causally complex (because characterized by endogeneity, heterogeneity of
various kinds, including time, interaction effects) for mainstream methodology rooted in
quasi-experimental comparison and regression analysis to handle. There is much to be
said for this argument, both in its diagnosis and its prescription.

The virtue of this methodology is that it is built to handle a complex causal
structure. There is considerable continuity, however, between the older methodologies
and this newer one. Both are causalist and variable-centered. Nevertheless, this proviso is
added: “Although systematic process analysis does not necessarily entail it, the
perspective suggests some sympathy for Weber’s argument that the researcher should ask
whether the theory is consistent with evidence about the meanings the historical actors
attributed to their action” (Hall, 2003: 394), which takes up Weber’s injunction that
explanation should be adequate at the level of meaning. The reason why this proviso is
qualified, I believe, is that if process analysis entailed adequacy at the level of meaning, it
could not be either causalist or variable-centered and nothing else.

For variable-centered causal analysis, attention to action is merely a canard. (One
can always raise the specter of some kind of interpretive relativism following on a
theoretical interest in action and what action entails). For Hall to go even this far is to betray more interest in intentions than is healthy for causal analysis.

If action and meaning matter, however, it is because the structure of the social world is different than the structure of the physical world. But then methodology should be designed to take into account that structure. If action and meaning are not enough to make an ontological difference, then it is not clear why we should gesture to them or to Weber at all. Sympathy is not enough to motivate an optional interest in action. Either action makes a difference or it does not. If it does, we must follow through methodologically; if it doesn’t, we need not bother.

To point to this proviso is not to propose that we now turn to Weber for methodological inspiration, although there is much to learn from Weber’s work. The argument to this point has depended on more recent sources. Rather, I want to point out that the qualification to this proviso effectively produces a methodology which is not designed relative to persons. There are no domain-specific assumptions that distinguish the subject matter of process analysis because, despite the methodological modifications, there remains a deep and fundamental continuity between the old and the new social ontologies. Process analysis is continuous with older methodologies, so that we can continue to aspire to replace the proper names of social systems with systems of variables (Przeworski and Teune, 2001, [1970]). Process analysis is explicitly variable-centered. It is causes and variables all of the way down. And causes are causes, data is data and variables are variables: issues of endogeneity, heterogeneity and interaction – non-linear dynamics – arise in physical systems of all sorts.
Action, actors and meaning are not constitutive features of physical systems. Explanations of these systems specify causes without reasons. When intentional action is our concern, in contrast, reason explanations specify explanations without causes.

My reading is that the argument about process analysis incorporates a commitment to action in a way that motivates an interest in trying to take meaning into methodological account via the gesture toward Weber, even if action is still marginalized. The commitment is contained in the way that cases continue to play a role in process analysis. The importance of cases to this argument is obscured by the treatment of cases as “units”. The cases of interest are particular kinds of “units” – they are social artifacts, relative to persons, and thus intrinsically linked to reason explanations. But, to see this, we need to deal more directly with the distinction between cases and variables.

A social ontology in which reasons are not causes should not yield a methodology which is variable-centered. But does that mean that reason explanations are case-centered? Given the way our methodological choices in comparative research are currently set out, this would appear to be our only alternative. However, from the perspective of this paper, this is a false opposition because the choice between cases and variables does not really identify what is at stake.

There is something important in the literature on cases but the strength of case-oriented approaches depends on how these approaches trade on intentional action. What makes this literature an alternative to variable-centered causal analysis is its attention to action. It is not that case research is more intensive and detailed; it is not that case research identifies mechanisms, it is not that case research takes on board complex causation. Rather case research recognizes the distinctiveness of the social world. Case
research is action-centered. This is what distinguishes case research from variable-centered research in the first instance. So what is at issue here is no simple opposition between ‘variables’ and ‘cases’. Rather, it is whether or not to put action at the center of the human sciences and what follows if we do.

That said, then, the basic distinction is not between cases and variables. There is variable-centered work which works by reconstructing choice situations but it has tended to do so using a version of the instrumentalist model I pointed to earlier. If reasons are not causes, this model is not available, but this does not mean that the reconstruction of choice situations is no longer appropriate or feasible. Rather, the instrumentalist model must be replaced by an account of intentional action in which action is structured by the relationship between ends and means – a relationship which is not causal.

While there is some ambiguity about how to define a case, whether “as a row entry in a data set, column entries being reserved for variables” or as a “spatially and temporally extended whole” (Norkus, 2005: 363; cf. Ragin and Becker, 1992), a case-centered methodology is a real alternative only if its takes in some way the form of the latter. From the variable-centered viewpoints which have influenced the field of comparative politics, case-centered approaches are inferior, and what is being criticized of course are ‘cases’ of the latter sort. The controversy here is framed largely in terms of the “small N problem” in relation to the virtues of generalization (eg. King et. al. 1994; Hall, 2003; Campbell, 1975 and the arguments in Ragin, 1997, 1997a), whichever side of this issue one takes. As a consequence, however, case-oriented researchers are then reduced to various kinds of special pleading for case-oriented research, which creates methodological and theoretical space for cases at the margins of social science, but which
is not a powerful challenge to variable-centered methodologies. This debate misses what is actually at stake.

The issue is to some extent, once again, ontological. A whole which has spatial and temporal extension should have an identity, so that it is self-identical across time and space. (It is more than a “unit”). The debate about cases versus variables is really about a problem of identity. Two related kinds of identities – spatially and temporally extended wholes -- come to mind here: persons and associations.

The wholes we have in mind here go some way toward addressing the issue of domain-specificity alluded to above. We are interested in human associations; more particularly we have, by convention, settled on states as the wholes or associations of primary interest in sociology and political science. Comparative research in political sociology is overwhelmingly set up as comparison among these associations – that is, across states or within states. States are those proper names which are to be replaced by systems of variables in variable-centered approaches There is a plausible justification for part of this position. The pluralists, both British and American, got part of it right. Before they are anything else -- structures, containers, institutions etc. -- states are a kind of association, on all fours with other associations. But, contra the pluralists, we treat them as first among equals (Runciman, 1997) 18.

The issue is whether these spatially and temporally extended wholes – human associations -- can be decomposed into variables and observations and causes and mechanisms, and nothing else without loss and, more fundamentally, without committing a category mistake which treats the social as if it was natural. This issue has

18 There is more to be said about this convention and how it might be justified but these issues should be taken up separately.
nothing directly to do with the problems of size of the N, degrees of freedom, or
generalization. Further, if associations reduce to anything, it is to persons, who are in turn
a rather special kind of case or “unit”.

If they can be so decomposed into nothing but causes, variables and mechanisms,
human associations are no longer truly cases – their self-identity (extension in space and
time) is epiphenomenal. But, as well, then they are no longer social artifacts – relative to
human aims and cognitions – they are mechanically caused rather than socially
constructed. That is, there are brute facts and there are institutional facts (Searle, 1997)
and the latter are relative to intentional action in communities of will (cf. Von Wright,
2004[1971]: 114). Human associations must be understood relative to human aims and
cognitions. As mundane and as obvious as this position sounds, it is incompatible with
accounts which are strictly mechanistic and causal.

There are great practical difficulties in making such a position good: the problem
of unintended consequences of actions, the problem of sincere testimony, the problems of
social interdependencies and indeterminacy, the problem of self-knowledge, but a
methodology built on this position would make its purpose the analysis of these problems
and the search for solutions. And, of course, we have already made some progress in
these areas.

These are problems which do not deny but rather presuppose intentional action,
internally structured by means-ends relationships. Much of what emerges as problematic
under these various descriptions is produced by the complex consequences of intentional
action in communities of will. But none of this is a challenge to the importance of
practical reasoning in human associations -- which is another less formal way to summarize the centrality to the human sciences of intentional action.

The point to these problems is that they are not problems of *causal inference*. These are very different *kinds* of problems altogether, and they are produced by the internal structure of intentions and of intentional action. One might ask, then, how any progress on these problems has been possible, if the social sciences have been built around causes, including reasons-as-causes? The answer, I believe, is that the instrumentalist model does provide some oblique purchase on these problems, not because of its causal assumptions, but because of what it shares with reason explanations which are not causal – that is, some attention to agency and to choice. Much of the progress, moreover, has occurred in areas which do not depend on the instrumentalist model.

The virtue of case-oriented approaches, when compared to variable-oriented approaches, is that they are in an important sense superior, but not in ways that have been fully recognized by current justifications and defenses of case-oriented research. They are truer to the social world. Nevertheless, case-centered research still tends to be encumbered by a causal language of explanation which is inconsistent with the internal structure of intentional explanations.

VII

Much of the current discomfort with mainstream methodology is rooted either in outright rejection of or ambivalence toward a ‘Humean’ model of causation. The Humean legacy has several parts, however. It is not just a question of constant

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19 The scare quotes are entered here in order to indicate that this paper is not an exegesis of Hume’s work and, if it were, it would have to consider readings of Hume under which what is now termed the Humean model of motivation was not Hume’s.
conjunction. Another part of the legacy is the dualism of belief and desire (reason and the passions), which underpins the simple model of instrumentalism discussed earlier and the argument that reasons are causes. As it stands, however, the current debates about methodology, particularly the innovations of those participants who are self-consciously anti-Humean, whether in a strong or weak form, do not appear to have recognized that a full-blown challenge to the Humean legacy requires the rejection of instrumentalism and the dictum that reasons are causes, and not simply the rejection of the constant conjunction model of causation. And they therefore do not recognize that criticisms of the instrumentalist model do not dislodge intentional action. What remains at the heart of the social sciences, then, is action, structured internally by the calculative relation between ends and means.

This state of affairs is in part a result of a rather casual attitude toward the notion of a cause. Case-oriented research, which is critical of variable-centered work, can refer to causes without really identifying an alternative approach to explanation from that which informs variable-centered work, while drawing implicitly or informally on intentional explanations which are not causal.

There might be another position here, namely that there are causes which are not Humean causes. But these alternatives, whether conditional, relational or processual, are like the Humean model in that they do not recognize the internal structure of intentional action. And in the case-oriented literature, “causes” continue to be invoked but the notion of a cause has been a little emptied of meaning.

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20 For an argument that Hume was not an instrumentalist, see Setiya (2006).
There is some warrant as well in treating reason-explanations as invoking final causes, but the Aristotelian corpus may not be fine-grained enough for our purposes to stand on its own\textsuperscript{22}, in light of more recent work by Anscombe, Vogler and Schueler and others.\textsuperscript{23}. There is a principled and well-developed literature on intentional action and reason explanations which recognizes that there are explanations without causes. We will be better off treating reason explanations as explanations without causes and proceeding from there.

VIII

The difference between variable-centered and case-centered comparative research is not a difference between large Ns and small Ns; it is not a question of quantitative versus qualitative work; it is not a question of correlations versus mechanisms; it is not about less detailed and more detailed research or about more or less intensive research. All of these oppositions are at best misplaced and at worst are red herrings. The more revealing opposition is between causes and reasons. This opposition is more revealing because it shows more clearly, more cleanly, and more deeply the issues at stake.

The problem with the causal model is that it is not true to the structure of the social world. \textit{If it was true, its treatment of intentional explanations could be made good.} But this treatment is increasingly less defensible. This means that any piece of research which claims to be causal, but which invokes intentions and relies on intentional (reason) explanations, risks incoherence because it is trading on explanations which are not causal.

\textsuperscript{22} This must be a provisional conclusion. See the different arguments in Hacker (2007) and Lear (2000).
\textsuperscript{23} There is an interesting alternative to the evolutionary interpretation of the origins of the intentional stance, an alternative which turns on a reading of Aristotle, in Lear (2000: 1-60). This is also an illuminating discussion of the limitations to a teleological understanding of human being (eg. 79-83, 123, 161-163).
But, further, social research which invokes causes and forgoes intention altogether is incompatible with the structure of the social world.

Reasons are not causes. Reasons are not causes because of the internal structure of intentional action. This internal structure is defined by the calculative relation between means and ends which is not a causal relation. This is why reason explanations matter.

IX

In this section, I consider some implications of the argument and what is suggested regarding further work. For the most part here, I present objections and questions, followed by responses.

At the start, I want to address one preliminary question. Is this account of reason (intentional) explanations ‘constructivist’? This account is (obviously) not classically realist (that is, as realism is understood in international relations theory); it is not scientifically realist, it is not positivist; it is not foundationalist; it is not classically rationalist. Yet it does not follow that it is constructivist, even if this is what must follow according to constructivists. This therefore is to say that constructivism is based on a misplaced opposition and this account escapes that opposition altogether. It would be constructivist only if non-causal intentional explanations could be found at the theoretical core of constructivism but they cannot be found there.

(i). Is this argument really any different than the argument(s) associated with the distinction between structure and agency or with the distinction between macro and micro-levels of analysis?

Both of these distinctions typically rest on a causal model of explanation. Aside from structures and other macro-phenomena, which typically are treated as fixed
conditions which have effects (and which are discussed later in this section), it is not difficult to think informally of agents as causes in the sense that agents bring things about. However, if causation is a relation between events or states, then agents cannot be causes because agents are neither events nor states (Alvarez, 2005: 53). The further problem is that agent-causation cannot distinguish between (a) causing something to happen which was not intended and (b) bringing about something which was intended. Under (a) there might be causation but what is caused is not intended (and what is intended and brought about is not caused in my account); under (b) there is an agent but that agent is not a cause24.

There is another view of agent causation that depends on ‘substance causation’ and which could be invoked here, but it too has difficulties25. This position would have the consequence of accepting the existence of inanimate agents. “[T]he possession and exercise of the power to make something happen – such as to make a lump of zinc dissolve – is sufficient to make the volume of acid an agent” (Alvarez and Hyman, 1998: 245). If to act is to exercise a causal power, this also seems to further imply that action is a term which can be extended to inanimate nature.

We should also note that these distinctions between structure and agency and macro and micro were introduced at roughly the same time and in relation to the interest in mechanisms, and emerged primarily in case-oriented literature. Some important work in the statistical variable-centered tradition (such as King et. al. 1994) is rather skeptical of mechanisms, in large part it appears, because the claim is that mechanisms must

24 Alvarez and Hyman (1998) parse this set of issues by arguing that agents cause the results of their actions but not their actions. This move is produced by their argument that actions are not events (and so cannot be caused); it works to preserve a place for causation by separating an action from its result, the latter which presumably is then an event and thus the right kind of thing to be implicated in a causal relation.
themselves be explained (and explained causally). This shows that some distance from statistical arguments is introduced when mechanisms or a micro-level of analysis is introduced. Yet the clear tendency on the part of those doing the introducing is to think of themselves as continuing to do causal analysis. The point of the skepticism, then, is to force the issue: Can one do causal analysis by introducing mechanisms or micro-foundations? The answer from the skeptics seems basically to be ‘no’. While the point of their objection is well-taken, it is not obvious that we must accept their conclusions – that is, that we must return to a form of associationism --- the hallmark of statistical analysis.

Measures of association are essential to statistical analysis and inference. It is true of course that statistical thinking can provide a critical perspective on ‘folk’ associations and spurious association. Yet the correction which is available from within statistical arguments is simply to replace one association (spurious) with another (non-spurious).

The skeptics are right to press the issue, however. There is real distance between the statistical point of view and the introduction of mechanisms. Statistical arguments are by their nature associationist. The point to the turn to micro-foundations and to mechanisms is to argue that association is not enough in explanation, so this turn is potentially something of a modification to associationism.

However, when mechanisms are joined to micro-foundations, what is introduced is something which might be disconnected from association altogether. Statistical association is not intrinsically relative to persons. We should recall that much of the foundational work on statistical analysis and research design (such as the work of Fisher and Wright26) has its origins in the study of plant genetics in agricultural field stations. The first application of path analysis was to the birth weight of guinea pigs and the

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26 See for example Wright (1921) and Fisher (1926).
transpiration of plants. Fisher’s work involved the application of nitrogenous manure and its effects on yields of winter oats. The selection problems in play are rooted in nature and the mechanism of natural selection, which has the key feature of introducing random variation into processes of change. Nature solves, so to speak, its own selection problem.

One other way, then, to point to the contrast with the animate world in which reason is present, in addition to what has been said thus far in the paper, is to ask: Why is there a problem of self-selection when we are dealing with persons? “Plots of ground do not respond to anticipated treatments of fertilizer, nor can they excuse themselves from being treated” (Heckman, 1992: 215)\(^{27}\). The problem of self-selection shows the importance of non-random generation of the social world.

Self-selection renders moot the standard variable-driven way of dealing with problems of identification and endogeneity via the use of instrumental variables. The latter are designed for problems of selection in nature and they fail in the face of heterogeneity produced by self-selection, which at base is a consequence of autonomous choice. The use of instrumental variables in the face of heterogeneity (of responses to treatment in the methodological literature) requires an assumption that intentional agents are either irrational or ignorant (Heckman, 1997)\(^{28}\). This reveals the deep problems in applying a method designed for inanimate nature and animate beings without human reason to the study of intentional action. It should then be no surprise to note the strong

\(^{27}\) Although not aimed at Robert Putnam’s description of his research on Italian politics, this point does have application to it. Heckman’s point rules out the following analogy: “Just as a botanist might study plant development by measuring the growth of genetically identical seeds sown in different plots, so a student of government performance might examine the fate of these new organizations, [regional governments] formally identical, in their diverse social and economic and cultural and political settings” (Putnam, 1993: 7). The point has implications well beyond Putnam since this kind of analogy runs through work on quasi-experimental research design.

\(^{28}\) A very similar issue arises in work on common priors, where heterogeneous beliefs are denied possibility by arguing that differences in beliefs are a result of information-processing errors or (implicitly) are irrational. There is some further discussion later in this section.
historical and conceptual relationship between path analysis and the invention of instrumental variables (Goldberger, 1972; Stock and Trebbi, 2003).

(ii). The instrumental model discussed earlier is perhaps the dominant way to reconcile the associationist logic of statistical points of view and micro-mechanisms, once the latter are introduced. This model rests on an associationist psychology and model of the human mind which makes it compatible with the associationism of statistical arguments. The issue then is whether this model can capture the ways in which action occurs, whether, for example (to use the point just made about selection) associations in the mind produced by simple observations of events in the world are enough to account for self-selection or, more generally, to account for action.

Further, without the assumption of random variation in nature where are we left when it comes to dealing with socially constructed persons and communities of will? Either the social world is randomly generated or it is not; if it is not we should give up locutions which essentially implies that we should treat the social world as if it was randomly generated (even if it is not) since these locutions are grossly question-begging.

There is a further problem which arises because the social world is not generated randomly. The problem of self-selection reinforces the earlier discussion of the problem with associationism. Self-selection essentially means that associations, whether in statistical approaches to inference or in instrumental models of mind, can in principle never provide a theoretical account of human motivation and action.

(iii). But isn’t the distinction in this paper between the instrumental model and an internal means-ends model of action too fine a distinction to bear any weight? Isn’t any discussion of means and ends relationships merely a discussion of instrumental
rationality and thus of efficient choice\textsuperscript{29}? Isn’t this analysis then simply a way to smuggle in a narrow instrumental notion of rationality?

First of all, there is this difference: Extant treatments of instrumental rationality continue to be causal in their self-presentation and the model of action I am presenting is not causal. There is that difference, but I agree that this response does not exhaust the point of the questions.

To respond further, I would say that the relationship of rationality to action will change under my treatment and in ways that must mean that the internal model of action is not narrowly rational. Here, however, I must qualify the language used earlier when I first introduced the notion of a reason-explanation (page 4), where I said that reason-explanations rationalize action. I would now say, in light of these questions, that the ascription of rationality to a person presupposes being able to give a reason explanation for his action (Davidson, 2001: 266). This means however that a reason explanation of action is not intrinsically an explanation of \textit{rational} action. And this must mean that rationality is not an empirical law since it is not required to identify an action qua action or to specify the reasons for action.

In contrast, under the logic of instrumental rationality, an assumption of rationality is typically required and the assumption is doing explanatory work. This is particularly evident in the work that has been influenced by the D-N model of Hempel – in this work the assumption of rationality works like a covering law. In other work, in which rationality is treated more like a mechanism, its contribution to explanation seems to be more Humean – it accounts for regularity. One place where an assumption of rationality does not do explanatory work is in fact in the work of Davidson (Davidson, 2001: 266).  

\textsuperscript{29} For a particularly useful discussion of efficiency see Setiya (2005).
(iv). Then what work does rationality perform if it is not needed in explanation -- either as law or as mechanism? As I see it, it is not doing empirical work in explanation; rather it is doing evaluative/critical and therapeutic work. In the first instance we should not ask, is this action rational but rather, something like, what was its point – its aim -- and this depends on the identification of ends and means and only then can we assess rationality against some particular standard. A standard may in the first instance be internal – indeed it may have to be internal in the first instance because an action which is rational under one description (end) may not be rational under another description (end).

And so we can eventually ask for example: If this is what you wanted to accomplish, was this a good way to go about it? Was that a good reason to do y? This kind of critical and therapeutic exercise presumes that action has an internal structure that can be evaluated along these lines. It is because reasons link means to ends, and to persons, that action can be rational at all. For an action to be rational under any description preserves that this internal relation among means and ends makes rational action possible – that is, the very possibility that an action is rational (under a description) is built into this relation. It may not then be overstepping boundaries to say that
intentional action has a rational structure, even if we can be critical of particular reasons for particular actions. It is the internal structure of action which in making action possible also makes possible criticism and evaluation. Furthermore, it is not merely that I as an observer can evaluate your reasons, you as an agent can evaluate them as well.

There can as well be external standards, perhaps standards introduced by an observer and not in the first instance a part of the internal structure of the action in question. We would want to be able to think about the following kind of situation (Pippin, 2005: 125; following Hollis [1996: 109-130]): “One might, for example, have very good reasons for a revenge murder if one is a member of a Sicilian mafia clan. It would indeed be clearly irrational to be a member of such a clan and not plot such revenge. But, one might argue, there are no good prudential or moral reasons to participate in such an institution, and the objective structure and rules of the institution might also be in themselves irrational”. One important question here is in what sense prudence and morality are objective. A related question is the relationship of morality and rationality and whether, even if values are objective, this is enough to secure an external standard of rationality (Vogler, 2002, in particular the stunning passage at pp. 192-203 and compare Setiya [2006: 26-27]).

(v). Where does this leave formal models of rational choice?

These models are extensionalist rather than intensionalist, they do not provide intentional explanations. Strictly speaking, therefore, they are not models of action. This

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30 Is prudence something other than enlightened self-interest?
31 An interesting feature of Vogler’s work, when measured against typical analytical work in the philosophy of action and of mind, is its use of a piece of philosophical-historical anthropology, which shows up in two ways. Vogler points out the consequences of talking about reasons to be moral without a theological background – what she calls a godless variant of Thomism – and this as a way of describing why we have trouble explaining the force of external ‘ought’ statements (195). The second, related, way is the recognition that vice is parasitic on social order (eg. 194).
claim will raise some eyebrows since some of this literature takes it for granted that these models are models of rational action. However, a closer look at this literature will show that these models rest on explanations which are either causal or functional. Further, the purpose of these models is to convert intensional belief into purposeless information. They work to efface the difference between first person and third person perspectives on action. Another difference is that even if interdependent choice is consistent with reason (intentional) explanations, the concepts of equilibrium and of optimality are not basically central to them (which, for many, constitute grounds for immediate disqualification).

Most decision theory is related to the instrumental model in a straightforward way. The key terms in the instrumental model are belief and desire and they stand in a causal relation to action. In decision theory the key terms are formalizations related to belief and desire: probability, and utility. Decision theory also imposes restrictions on preferences. Essentially a preference relation must be complete, ordered and transitive to be consistent with rationality. If preferences do not have these properties, the persons who hold them can always be taken advantage of by enterprising entrepreneurs, otherwise known as “Dutch bookies”; alternatively such a person is a “money pump” (Schick, 1986); still more generally, “if most agents are irrational, then a rational individual can make a lot of money” (Arrow, 1982: 7). This is to say that there is an adaptive advantage in rationality.

Notice the similarity here to the basic intuition often used to justify Bayesian rationality: “[W]e ought to be Bayesian-rational wherever the Bayesian logic applies because those who follow that logic wind up being the better off for it” (Schick, 1997: 130). This is to say that rationality has a survival value, to further imply that Bayesian
rationality works over time as a selection device and then also to imply that Bayesian rationality is in some way (either in itself or via some causal relation) a natural disposition, shading off into a biological instinct. The result is to naturalize reason. It is to argue that Bayesian rationality is a result of natural selection.

This is a disposition which evidently has had a long time to be fine-tuned, even if the reflexive awareness of probability and its ensuing formalization (Bayes’ Rule) has occurred only relatively recently. Surely we have got it right by now. If rationality is a natural disposition then it should be something which happens to us. Clearly, however, once we are aware of the basic rule, we do not stand in an involuntary ‘blind’ relationship to its impersonal demands. Evidently we are too smart or too autonomous (or both) for our own good since we can refuse to be rational if being rational means being Bayesian rational.

There is a small equivocation in this literature on rationality. On the one hand, the arguments above can be read as claims that this is how persons do behave. If human evolution selects for Bayesian rationality, then by now the structure of the human world must be consistent with it. When persons choose, they do so according to the laws of Bayesian rationality. Since it is a natural disposition we might assume that it is normally distributed and that some are more fit than others.

On the other hand, the arguments can be read as claims about how persons should behave. But to say that this is how I should behave is to say also that I might choose not to behave this way, even if I should. And to say that I might not behave this way is to deny that “this way” -- Bayesian rationality -- is a natural disposition implanted by natural selection. We are free enough from this putative disposition not to behave as if

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32 I am adapting here for my own purposes the language of Nagel (1997: 111-112).
Bayes’ Rule was a law. We do not stand in the same relation to our biological dispositions as do animals because our reason is too autonomous to be naturalized in this way\textsuperscript{33}.

Rational choices must always be extensional as well. (Schick, 1997: 55). More directly, “a fundamental element of rationality, so elementary that we hardly notice it, is, in logicians’ language, its extensionality” (Arrow, 1982: 6, emphasis in original; cf. Satz and Ferejohn, 1994). Without this further constraint these results do not hold (Schick, 1997, 1986). This constraint does a great deal of work (Schick, 1997: 142, 147).

The consequence, however, if rationality is extensional, is that intensionality is intrinsically irrational. Moreover, a further consequence is that rational choice explanations are either causal or functional because, in linking intensionality to irrationality, the argument about extensionality denies that rational choice explanations are intentional (reason) explanations.

Rational choice depends on either function or cause - neither of which is relative to persons – note the dependence on the instrumental model, the evolutionary justification of Bayesian logic, and, finally, the natural selection-like features of the initial move by nature in related work by Harsanyi.

In this literature on games with incomplete information begun by Harsanyi (1967-68), there is a similar move which works to naturalize reason by setting limits on the degree of heterogeneity of beliefs deemed consistent with rationality. Again, the

\textsuperscript{33} Can rationality fail not only when there is ‘too little’ of it; but also when there is ‘too much’ of it? Consider the implications of the hyper rationality of obsession or of delusions. There are suggestive remarks in Vogler (2002: 91-93); Goldstein (2005: 204-205) and Lear (2000: 123). See also the related observations in Shapiro and Nissa (1999 [1965]).
argument depends on a selection device modeled on natural selection and on the conversion of intensional beliefs into purposeless information.

In these bargaining models, nature initially assigns all individuals to types. This device turns these ‘types’ into random variables over which Bayes’ Rule can apply -- that is, it is needed in order to make these agents Bayesian players. Thus this initial move is much like a selection device modeled on the most basic natural mechanism of all – natural selection. In this tradition, an assumption of common priors is also essential. “Harsanyi showed that a game with incomplete information can be reduced to a standard game of imperfect information with an initial move by nature, if and only if, individuals could share a common prior over payoffs in some state space” (Morris, 1995:230).

Suppose there were differences in prior beliefs. These differences would motivate different interpretations of new information. As a result, there would be no necessary convergence of beliefs. Without differences in prior beliefs, different interpretations of new information cannot arise. The assumption of common priors then has consequences for information-revelation: “…[I]f people have common priors, then the revelation of information about either player’s current beliefs must cause convergence in their posterior beliefs” (Smith and Stam, 2006: 616)\(^{34}\). As information is revealed, the existing bargaining range is at the same time revealed in this bargaining model.

In the literature on common priors, there is an explanation at hand, if different beliefs are entertained in the face of the same information, which preserves the assumption of common priors and which works to limit heterogeneity. It is to assign this apparent failure of the assumption to information processing errors rather than to concede that differences can be produced by heterogeneity. There are no differences in prior

\(^{34}\) See also Fey and Ramsay (2006).
beliefs which do not have their origin in information processing errors (Morris, 1995: 228); differences in prior beliefs are accounted for only by differences in information. If beliefs are heterogeneous rather than common, the argument that an initial move by nature assigns individuals to types would not do the work assigned to it of “converting a problem of belief to a problem of available information” (Smith and Stam, 2006: 615), that is of converting a propositional attitude into purposeless information. By this move, the assumption of common priors is preserved and any differences are linked to mistakes in the processing of information – mistakes which are systematic and differently distributed across the parties (otherwise, if the parties made exactly the same mistakes, their beliefs would be exactly the same, given the assumption of common priors).

The implied definition of common priors in one influential treatment is so restrictive or so odd (it is hard to tell) that it cannot be satisfied without violating basic and commonly held notions of individual identity. “When we say all relevant information, we mean all: the schools the players attended, their childhood experiences, even their genes…What we are saying that if people have precisely the same information about all these factors – no matter how much or how little it is – then it is not unreasonable to assume that they entertain the same beliefs” (Aumann, 1998: 932. Emphasis in original). The import of this statement is difficult to establish since it appears not to be able to avoid the following construction: Suppose someone named Gerry Adams had the exact genetic makeup as someone named Iain Paisley. Suppose further that he has had exactly the same experiences as Paisley in every area of life at the same times and in the same order and all of these experiences were interpreted and stored
in memory in exactly the same way, and so on. On the one hand, then, he would be a Unionist. On the other hand, if he had these features he would not be Gerry Adams.

There is a further ambiguity. Despite the importance of the distinction between public and private information here, there is no qualitative or quantitative marker by which to distinguish private from public information as information. What makes information private is not a result, however, of an unobservable randomizing natural mechanism which assigns persons to types but, rather, that this person has a first-person perspective on his or her experience (Moran, 2001; Thompson, 2007: 306-311; Schueler, 2003: 158-165). ‘Private’ information in the form of propositional attitudes is not information at all, since information is not intensional. Finally, starting from heterogeneous beliefs rather than common priors does not relieve us of the problem of dissembling. The incentive to dissemble is built into this situation via the motivating condition – the presence of relevant differences -- and dissembling is made possible in the first instance as a result of the privileged position of the first-person perspective in action. Moreover, when beliefs are heterogeneous, we cannot fall back on the analytical device of the initial move by nature. This device works only together with the assumption of common priors.

(vi). In securing a defense against the naturalization of reason, a new problem is created and then left unsolved. The argument about information and privacy, by pointing to the problem of distinguishing public and private information, seems to assume the ‘privacy’ of other minds. Are you not leaving as a remainder something like the problem of other minds?
The essential argument is that there is a distinction between first person and third person perspectives in action. The point of this objection therefore would be either that this distinction cannot be made or that, if made, a commitment to a thesis such that there is a private language is entailed. In response, this distinction must be made, it cannot be avoided; furthermore, making this distinction does not imply a private language. Not to make this distinction is to take on a thoroughly external perspective on action – it is to assimilate action to the ‘thing-language’ of physicalism. Action is parasitic on thought in ways that instinctual drives, involuntary reflexes and efficient causes are not. Intentional objects are not external objects which conform to the ‘laws’ of physicalism.

If intentional objects are not external, they must be, by the logic of contrasts, internal. But is this a pernicious result? ‘Internal’ here does not mean ‘in the head’ or even ‘in the mind’. These are locutions which seek to give location to thinking; they are ways of talking which are already infected by the mistake of physicalism applied directly to thought and hence to action. On the contrary, in the specific discussion above of the problem of dissimulation and of strategic presentation of self it seems to be, depending on the case, that I can understand you but disagree with you or understand what you do and perhaps still be unclear of your intentions. I might or might not correctly identify the means-end relationship internal to a particular action. But, in any of these instances, the bridge between a first person perspective and my third person perspective as the person doing the explaining is built, so to speak, into the notion of acting intentionally itself: The action to be explained is construed as forming from the same sort of process the person doing the explaining engages in when acting for some reason\textsuperscript{35}. The first person point of view remains central since we must identify the reasons for acting as conceived by the

\textsuperscript{35} This is a paraphrase of Schueler (2003: 164).
actor. The content of those reasons is vital and the ineliminability of content makes reason explanations intensionalist. Such explanations are not acts of empathetic understanding or imagination. Rather it is the rational structure of action which provides the bridge between first and third person perspectives.\(^{36}\)

(vii). The argument draws attention to reason explanations by drawing on a literature on propositional attitudes and emphasizes that the key terms in the instrumental model are belief and desire. There is a literature in comparative political economy and sociology which introduces the concept of ideas. Where do “ideas” fit in this argument?

The notion of an idea relevant to our purposes here was introduced in the literature on the state and institutions. Ideas were introduced by political scientists and sociologists in order either to account for change after their models and theories had boxed them into an emphasis on stasis and continuity, or to introduce an alternative to accounts based theoretically on choice and agency which emphasized interests. In the first instance, then, ideas functioned to explain change and were contrasted particularly with interests.\(^{37}\) However the concept of an “idea” has never been well-defined in this literature. I can offer a clear definition of an “idea” here, consistent with my argument, and then consider briefly the advantages of this treatment.

An ‘idea’ is nothing more and nothing less than a proposition ‘p’. Ideas however occur in oblique or indirect contexts. Ideas therefore have a particular structure: “I believe that p”. Ideas then are propositional attitudes.

\(^{36}\) The work of Hollis (1996: 191-281) provides a powerful examination of the problem of other minds and other cultures using as the bridgehead rational belief.

\(^{37}\) For illustrations of the role of ideas in explanation, taken from a large literature, see Hall (1989), Goldstein and Keohane (1993), Blyth (2002), Lieberman (2002). For a similar conclusion, reached by a different route and with different implications drawn, see Blyth (1997).
Ideas may be widely distributed – that is, they may be widely believed. They may be narrowly distributed. Ideas may be disputed or contested. The propositions which are part of ideas may have direct reference but they may not. Direct reference is not a necessary property of a proposition which is believed – that is, which is embedded in a propositional attitude.

This is a mundane understanding of what an idea is because, on this understanding, ideas are on all fours with propositional attitudes. There are two reasons to take this mundane route, however. The first reason is that it links ideas to a larger and well-developed literature on propositional attitudes. In introducing ideas, then, we do not need to reinvent the wheel. The second reason is that it encourages those who have introduced the concept to defend their usage. Ideas are either propositional attitudes or they are not. This is the first line of inquiry. To take the latter position – that ideas are not propositional attitudes -- then will require elaboration and defense. If ideas are something else altogether than propositional attitudes, then what are they? If the presuppositions of the literature on propositional attitudes are incompatible with the theoretical desiderata which motivate the introduction of ideas into social and political theory, then what are these specious presuppositions, what should replace them, and how then can ideas be defined relative to these other (putatively more acceptable) presuppositions, keeping in mind that in taking this route ideas can never again be understood as propositional attitudes? A second line of defense would be to agree or to concede that ideas are propositional attitudes but then argue that they are a special subset of propositional attitudes. This defense would require a specification of the properties which distinguish this subset from other propositional attitudes.
Either way, our understanding of ideas and their place in political and social theory would be advanced. Ideas may turn out to be something other than propositional attitudes but at least now we will know why. Ideas may turn out to be a special class of propositional attitudes but, once again, we will have learned what distinguishes them from other propositional attitudes.

As is evident, this kind of exercise has nothing to do with the research programs in which the concept of ideas first emerged and got traction. The question which motivates this current exercise is: What is an idea? This question gets lost if we understand the concept of an idea only in relation to a research program because, from within a particular research program such as the political economy of modern welfare states or such as the sources of international cooperation, ideas are identified according to the theoretical function they perform. In general ideas are introduced to fill gaps in arguments in these research programs, often to account for remainders.

In this light, then, I introduce one example of what I take to be a proposition that is an idea. Consider this proposition: “It is a question of catching the right instant. Too early you break your neck; too late you lose your chance” 38. This proposition is first-person – these are the words of a political activist involved in a movement for political and social change. An interesting feature of this proposition is that we can identify and analyze it independent of its local context. What would it mean to give a causal analysis of the import of this idea – an analysis of its effects and of its causes?

In stating this proposition – that it is a question of catching the right instant – the activist is expressing one of his beliefs. We can always ask whether this expression is

38 The words of a Breton nationalist looking back on the failed Celtic revolution of the interwar period. Olier Mordrel to John Legonna, November 11, 1968. John Legonna Papers, National Library of Wales. There is more to be said about the context of this speech-act but this is not the place to go into it.
sincere. Suppose we (as observers) think this activist is dissembling. Even in these circumstances, we can make sense of the proposition; when we conclude he is insincere, we have simply concluded that this interlocutor does not believe the proposition. This is a third-person perspective on this belief and proposition. If we adopt a first-person perspective, a similar point emerges however. Actors do not have to believe this proposition in order to understand it. An actor who contests this belief for example (more accurately, this proposition, see below) recognizes and understands it.

There is as well an implied contrast. “It is not a question of catching the right instant”. Thus the proposition has an existence independent of belief. An interlocutor can recognize the proposition without believing it. The proposition has some kind of status independent of belief. It has a status within a particular language game. This is a reflexive observation made from outside the language game in question and it thus introduces an element of objectification.

The language game is part of what must be presupposed in order to make sense of this proposition, and outside of this language game, the proposition is meaningless and without context. It cannot be interpreted. But within this language game, it is a proposition that need not be believed. There is disagreement within this game as well: not to believe this proposition is to disagree with those who believe it. This appears to be a disagreement about the fact of the matter – about the truth of the proposition. Is there an excluded middle like this: Either it is a question of catching the right instant or it is not?

With some rephrasing, we can restate this proposition in light of a hypothesis about the nature of this language game as: “In politics, timing is everything; we have to

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39 One can imagine that there exists, as in an existence proof, an objective function with one maximum (and two minima) which satisfies this question. But this is not to specify a truth-value; to write out the objective function is simply to restate this particular proposition in a formal syntactic language.
seize the opportunity when opportunity beckons and we have to delay if opportunity does not beckon”. And a rival proposition might be phrased this way: “In politics we are not at the mercy of events, we make our opportunities rather than wait for them”. Each proposition implies a different orientation in the world and a different course of action; otherwise there would no point in articulating these different propositions within the language game.

Do we know and can we know, as observers, which one of these propositions is true independent of the language game? Is there a fact of the matter outside the game? There is no doubt that political scientists and political sociologists recognize this language game. The proposition is a classic position in the politics of change, particularly of radical change. It might be the case that this game occurs wherever there is politics. But even if this is true, it is not the same as testing the truth of these two rival propositions. Are we in a position to test which one of these propositions is true? Sometimes it appears as if we, as observers, think we are, so that for example we conclude that some moments are riper than others for conflict resolution; that this particular state timed its challenge to a hegemonic state too late (or too early); that this group played its hand too early; that this leader did not wait on events and successfully reframed her political possibilities. These positions are articulated in various literatures. What we can examine is where and when this language game occurs and how this game is played in different contexts, how these propositional attitudes are acted upon, and how these debates internal to the language game are resolved in practice but none of this is to offer a causal analysis dominated by attention to classic problems of causal inference. This is not to say that all ideas must be analyzed in this particular fashion but it does
illustrate the force of thinking about ideas as propositions within oblique or indirect contexts – the contexts of propositional attitudes.

(viii). Is the argument of this paper really any different than the general notion that choice is embedded – that is, that the internal means-ends relationship is embedded in something which is external?

In the first instance, this is just to say that whatever the ends or means – however these are embedded -- the relationship between them in intentional action is an internal relationship.

(ix). But is this response simply an evasion? Is it not the case that the external character of particular ends (and perhaps particular means) make ends causes? That is, once we ‘fill in’ a formally empty end by giving it particular substantive content, does that end not then stand in a causal relation to intentional action?

In response, once again, whatever the end or means, their relation in action is internal.

(x). What about the backend of action – don’t actions have consequences in the world and what is wrong with thinking of these consequences in causal terms, even if the key relationship generating action is an internal and non-causal relation?

Action has consequences which can be treated as causal effects when these consequences occur independently of actions by others. That is, if these hypothetical consequences depend on the action of others, action that is structured in the same way by the internal relation of means and ends linked by reasons then there are consequences, but consequences are not effects because effects presuppose causes. In general, a
consequence of an act is not the effect of a cause. If consequences are not effects, then neither acts, nor the constitutive elements that make them acts, are causes.

(xi) Surely, however, we can think of causality in terms of conditions – whether sufficient or necessary for example? This notion has been given new and sophisticated life in recent arguments in comparative social research⁴⁰. In particular, comparative research which is oriented toward macro-level analysis, specifically structural and institutional arguments, have been drawn to this position. The attractiveness of this position lies in large part in the sense in which something – a condition – is taken as given or fixed (Mackie, 1974). Then when this condition is present, effects follow.

My objection is that this literature is ambiguous about ‘conditions’. The literature is ambiguous between conditions treated as brute facts and conditions treated as institutional facts. However, brute facts and institutional facts are ‘given’ in very different ways⁴¹. The former are parts of causal relations. This literature assumes that institutional facts stand in the same relation to causal relations as do brute facts, or assumes that they can be treated as if they were. But, then, this literature in comparative social research must be able to establish explanatory force with reference only to causal relations and processes.

⁴⁰ See Mahoney (2007), Goertz and Starr (2002), Baumoeeller and Goertz (2000). Some of this work draws on Mackie (1965); his arguments in this piece are now part of a larger argument in Mackie (1974).

⁴¹ Institutional facts are endogenous to human action in a way that brute facts are not. They are given in a different way. But of course this does not mean that brute facts cannot be modified. “The crucial thing setting people apart from all other living things is their ability to change their niche at will.” (Colinvaux, 1978: 219). This basic feature is fundamental to long-run accounts of human evolution. There is a rich vein here to be mined in relation to the built environment and human being. One general issue is how intentional action can set in motion causal processes in nature and how changes to nature related directly to intentions or to unintended consequences of intentional action have consequences for human being beyond a cross-section of time or space. The locution, ‘set in motion’ is here a placeholder for a more thorough discussion at some later time.
The fundamental question is: What gives conditions explanatory force in explaining action? Explanatory force depends either on causes or it does not. In this work, explanatory force is quite clearly supposed to depend on causal relations. But the problem in this position lies in specifying how a condition causes anything with regard to action. How does a condition do this causing (cf. Schueer, 2003: 13) given that, in the name of consistency, non-causal explanations cannot be used to provide explanatory force? Here enter mechanisms once again, as one possible response to this question. But the mechanisms in question must be causal mechanisms.

Either there are causes available which are causes without reasons or there are reason explanations available which are causes. But the first may not be available because the counterfactuals of structural and institutional explanations imply premises which implicate intentions. The question then becomes whether reason explanations are causal and the argument in this paper is that the explanatory force of reason explanations depends essentially on the internal relations between means and ends which is not a causal relation.

Here what conditions and causal mechanisms together do in this argument which is being questioned here is to specify causal processes. However the relevant literature in social research does not make this position good because it routinely trades on intentional (reason) explanations in order to establish the explanatory force of conditions. What is doing the explaining is not causal. Finally, this line of argument cannot be reconstructed so that conditions are necessary and reasons are sufficient, or so conditions are sufficient

42 This is a kind of skeptical question: how does a condition have the force or power to necessarily bring about its effect? I draw here on McBreen’s (2007: 426ff.) discussion of Hume.
43 James (1984: 143-145) makes this point. This is a problem for structural arguments whether or not action is to be explained causally. See Taylor (1989, 1988).
and reasons are necessary, and so on. If conditions are neither necessary or sufficient on their own, in whatever combination one might want to specify and in combination with related causal mechanisms, then the point of view which assumes ‘conditions’ might have to be abandoned altogether. What is ‘given’ in this approach is not given in the right way to figure in a reason explanation.

(xii). Does this argument of this paper entail a commitment to the position that persons are not animals? The answer should be: Of course not. Must this argument then take up as a desideratum to be satisfied that it be consistent with evolutionary theory and its core mechanism – natural selection? The answer should be ‘yes’ but, again, the solution here will depend on what is entailed by “consistency”. The evolutionary interpretation of intentional action is too one-sided. It is not just that intentional action must be consistent with natural selection; natural selection must be consistent with intentional action44.

This interest in some form of reconciliation might motivate and justify a search for a form of naturalism which is not physicalist (Hornsby, 1997). The further point here is that naturalizing humans will not have some of the consequences often assumed. It is not the case for example that naturalizing humans entails that their nervous systems process information in the computationalist sense rather than create meaning (Thompson, 2007: 13ff.; cf. Petitot, et. al. 1999)45, or that the human mind is literally a computational system (Pinker, 2007: 259).

(xiii). What does this argument imply about the historical relationship of past and present? It implies that this kind of distinction is a construction placed on an ongoing

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44 An animal who can think about evolution is an interesting creature indeed.
45 This position requires careful development. See Bennett, Dennett, Hacker and Searle (2007) and Bennett and Hacker (2003).
process of social generation in order to make experience and the analysis of the social world and of time more tractable. Every ‘past’ was at one time a ‘present’ and the generative process was no different for the past than it is for the present, keeping in mind that the distinction between past and present is typically relative to some kind of human association and to persons so that the past typically is taken to condition the present in some way -- as a legacy, for example, or a turning point and so on. But if the social or human world is generated by action there may be a past which is not historical because this internal relationship may not yet be present in the animate world. In the non-historical past, that is, there is no action.

The generative process might be time-dependent in ways that the previous paragraph does not fully acknowledge, however, notably if the ‘breaks’ that count are tied to our self-understandings and in particular how we conceive of the human mind. The larger point at issue is not just external movement across time but how our understandings of ourselves have developed and the consequences of these developments. An important problem, however, is produced as a result.

(xiv) This problem is well-described in Bourdieu (1994: 223-226): What do we do once human subjectivity is incorporated into our understandings and practices, when that incorporation seems to be thoroughly historical – that is, rooted not just in a particular time but also in a particular place?

To acknowledge this problem, however, is also to propose that the arguments of this paper do not fall prey to the errors which concern Bourdieu. The critical discussion above of associationism should show that the arguments of this paper avoid the “gravest epistemological error in the human sciences” of putting “un savant dans la machine”
(Bourdieu, 1994: 222; cf. Bourdieu, 2003: 75-131). The way the relation of action to rationality has been treated above should provide further reason to conclude that I have not been trapped by the “scholastic abstractions” of rational action theory (Bourdieu, 2003a: 315-318). The discussion to this point should also show that the arguments are not of a piece with the “intellectualism of Kant” which Bourdieu criticized (Bourdieu, 1994a: 126-127). Yet all of these errors have been avoided without privileging, as does much of Bourdieu’s work, the analysis of dispositions embedded in the preflexive background of action, an approach which he argues is needed in order to avoid these mistakes.

Habituation is not enough of a platform on which to build social and political theory. The identification of a preflexive realm already implies too active an observer; a passive description of the preflexive world might be able to end by leaving the world as it is (cf. Lear, 1982) but once the preflexive world is individuated in contrast to the reflexive world, the necessary passivity is lost. It is difficult to have said that you have left things alone once you begin to think and talk about those things. There is certainly something valuable in the ethnomethodological exercise of interrupting the dispositions of the natural attitude (Garfinkel, 1984[1967]) but, once brought to reflexive light, dispositions turn out to have neither the weight of instinct, nor the automaticity of involuntary reflexes, nor the imprint of efficient causes.

That said, however, there is still a way for a Bourdieu-type argument to press back. Bourdieu rejects the distinction between reasons and causes – calling this distinction positivist; in part it appears that this position stems from his criticism of a objective/subjective dualism which he argues runs through most of social theory. The

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46 This is the basis of the ascetic’s dilemma – how to live in the world without leaving a trace, how to interpret the world without intervening in it.
distinction in question simply reproduces or is correlated with this master dualism. But this response does not yet meet the arguments in the previous paragraph. If this paper escapes the problems which Bourdieu points to and which are noted above, its arguments are not likely grounded in positivism. Nor is it likely that the argument of the paper is dualist. But that does not mean that it must put dispositions at its theoretical core.

The puzzle about his treatment of the distinction between reasons and causes as a distinction grounded in positivism remains. It might be that Bourdieu thought that taking on physicalism by taking up its contrast with reason was still to work within the ambit of some form of positivism. Whether this reduces most of philosophy, including what might normally be taken to be pre-positivist philosophy to positivism (perhaps positivism simply brings into view and to a kind of conclusion what is already contained in the philosophic enterprise from the Greeks onward), is another question. His denial of this distinction, however, might be aimed at another dualism – at the sharp line drawn by some between understanding and explanation. The point about intentional explanations however is that they aim to be explanations – they are not designed to be something else, such as understandings or strictly construed, as in the hermeneutic tradition, as interpretations. Non-causal explanations bridge this dualism – they are neither strict interpretations, nor are they causal explanations.

Finally, there is another way to read his concern about the distinction between reasons and causes, a reading, however, which asks whether the preflexive background, of such importance to Bourdieu, is exhaustively constituted by dispositions. There is something about the social world which is strictly speaking not caused but which is not or should not be explained intentionally. This feature of the social world escapes the
distinction between causes and reasons altogether — it cannot be captured by this
distinction. It is a kind of permanent remainder which, given the limits of reflexivity,
ever shrinks to nothing and, as such, must be much more than mere remainder, and
much more than disposition. This reading of Bourdieu on these points would push the
discussion into fundamental ontology.

(xv). What does this discussion suggest with regard to the question of how the
social world is generated? The argument in this paper is partially consistent with
Przeworski’s (1995) treatment of this issue. There are two parts to his argument: (1) the
social world is non-randomly generated and (2) the social world is generated relative to
ends. This is a different treatment of the question of social generation than any of those
discussed in the first part of this paper, and it sheds light on the interest in data-generating
mechanisms in the social sciences. The first part means that the similarities and
differences which motivate quasi-experimental research design in social research are not
random variables. I take the second part to mean that the differences and similarities of
interest to us cannot be explained by causes.

The social world is endogenously and non-randomly generated. Przeworski makes
this argument in order to point to the difficulty of matching cases in comparative
research. The Przeworski of Przeworski and Teune (1970) would have made the social
world endogeneous to a system of variables; in the later argument in 1995, the social
world is endogenous to action and choice and more specifically to the relationships
between means and ends. “The conditions under which we live are somehow created by
people in pursuit of their ends…We thus must treat the observable world as having been

47 Perhaps one way to put the point is that this feature of the social world is non-teleological and yet is
uncaused.
produced by ‘us’, that is, as having been generated endogenously.” (1995:17). Przeworski, however, does not then argue that this process of generation is at its heart non-causal which is what would be implied by my treatment of the internal calculative or deliberative relation of means and ends in this paper48.

Yet the questions about effects which Przeworski then asks must still assume this core generative process. One then can ask whether the effects for example of coups on poverty or electoral systems on political participation (1995:16) are independent of complex chains of means-ends relationships in intentional action and complex combinations of people pursuing their ends, when these complex chains and combinations constitute the generative process. If these effects are independent, then there are causes with effects but then what might have to be given up is the argument about the process which generates the social world. If this argument remains intact and central, then the consequences of coups for poverty or of electoral systems for participation, for example, are not the effects of causes because of the argument introduced earlier: if these hypothetical consequences depend on the actions of others, then these consequences occur, but they are not the effects of causes.

48 One correction for endogeneity – the use of instrumental variables – is relative to systems of variables. This is generic to data – the variables (and thus the instrument) could be measuring the dynamics of water flow in a river, for example. Another correction – the Heckman correction (Heckman, 1978, 1976) -- is relative to choice. In subsequent work, Przeworski in fact uses the Heckman correction. See Przeworski (2004), Przeworski, Alvarez, Cheibub, Limongi (2000), Przeworski and Vreeland (2000). In completing this paper I have now had the chance to read Przeworski (2007) as well. I have not modified my argument in light of this last piece but it is even clearer here that Przeworski does not draw the hard distinction that I do between endogeneity relative to persons and endogeneity relative to physical nature since, for him, the social world has a causal structure If the natural order is a causal order, then the social world is natural; what distinguishes the natural and the social is that the causal structure of the natural world is given, the causal structure of society is not (Przeworski and Sprague, 1986: 7).
(xvi). Why is the social landscape so cluttered with institutions, constitutions, rules, roles, laws, conventions, customs and so on – how can the complexity of the social world be reconciled with action?

The arguments of this paper neither imply nor entail a pristine ‘uncluttered’ social landscape. Are institutions endogenous to a system of variables or to choice? All of the above are human devices and thus endogenous to action, every human association is rule-bound in some fashion. This kind of social furniture is a consequence of complex chains of means-ends relationships through time and of complex combinations, via coordination and conflict, of persons pursuing their ends. As a result, therefore, it is important not to reify these social artifacts by treating them as if they have a life independent of human purposes, even if there is a tremendous amount of social sedimentation over time. The weight of the past, whether treated as a legacy or as a turning point or a critical juncture which fixes some possibilities rather than others via rule-formation or institution-formation for example, or which makes some things customary and others not, is shorthand for the consequences of choices made by some in the past for the choices made by others in the present, as the consequences of choices made in the present will themselves become legacies for others who come later. These actions in some present may configure future possibilities by reconfiguring rules or institutions or they may not. Of course, the putative fact that we can become progressively and reflexively aware of all of this and that it is in some way a consequence of our own construction of time might also make the ‘past’ a precondition for human freedom rather than simply a constraining ‘weight’, at least up to some limit on how much reflexivity can be achieved.
(xvii). If the process generating the social world is endogenous to choice, where does that leave the social scientific observer, particularly in light of the common distinction between experimental manipulation and field observation in the social sciences, notably in comparative social research?

The activities of social scientists can feed back into the processes of social generation which are observed and have consequences for chains and combinations of intentional acts. Second, observations which feed back are like an intervention or manipulation. They of course are not classically experimental manipulations. But, as quasi-manipulations, they might be construed as causes or as producing causal relations according to one line of thought (Woodward, 2003; Holland, 1986) which emphasizes the relationship between causation and manipulation. The analogy to experimental settings is quite deliberate in this literature.

The claim is that “it is heuristically useful to think of explanatory and causal relationships as relations that are potentially exploitable for purposes of manipulation and control” – that is as relations relative to an agent. Nevertheless this is just a heuristic and preliminary device. As it stands, according to Woodward, this intuition is too anthropocentric and subjectivist and, at the end of the day, there are “circumstances in which manipulation by humans is not possible” [and] “these will be circumstances in which the relevant experience of agency is unavailable” (Woodward, 2003: 123). So the goal here is the construction of an agent-less theory of causality which, once the heuristic device has done its preliminary work, takes the position that interventions are “hypothetical interventions on X, of what would happen to Y under such interventions, and hence of X causing Y, even if manipulation of X is not within the technological
abilities of human beings, and indeed even in circumstances in which humans beings or other agents do not exist” (Woodward, 2003: 128. Emphasis added). Of course this line of argument is unavailable if, as is argued here, the process generating the social world is endogenous to action. It is odd to contemplate a theory of causality which claims to be applicable to human action and, at the same time, applicable in a hypothetical world in which human beings do not exist.

(xviii). But isn’t the choice implied in this discussion of Woodward and causation too forced a choice? Isn’t the implied choice in fact false or misleading, if there are ways of thinking about the social world which are neither strictly “subjectivist” nor strictly “objectivist”, such as in the literatures on networks, or on practices, as well as more methodologically focused work on non-teleological treatments of action.

The question is whether social relations are construed in these literatures as causal relations. If they are construed causally in external terms, then they will reduce to or be parasitic on a form of extensionalism; if they are construed non-causally, then they will be parasitic on action, understood as it has been in this paper. Practices and networks are, after all, much like institutions or conventions and the like. If the latter do not pose a deep problem for reason explanations understood non-causally, as argued above, then networks or practices should not either.

Joas (1996: 148-167ff.) understands action non-teleologically, as part as his alternative to Parson’s work on the structure of social action. This is an instructive discussion of the limits to action understood teleologically. His treatment may be superior to Parson’s arguments, but it appears a little unstable: has he identified a non-teleological form of action or a form of teleology which does not depend on action? Joas’ argument
about the superiority of his alternative seems to depend on the assumption that means-ends relationships entail rational action models of causal explanation. But I have reconfigured this linkage above; the internal model of action in which reasons are not causes has been separated from rational action models because I deny that the assumption of rationality does explanatory work. Hence his discussion of non-teleological action (or teleology without action, keeping in mind the ambiguity above) may not address my reworking of the relation between means and ends and rational action.

The thrust of these literatures is anti-Humean but then if those working in these literatures are to provide causal arguments they need a notion of cause which does not tie them to Hume and, moreover, that notion cannot tie them to the external objectivism of a Woodward-type argument. Some of this literature may then be drawn, as a substitute, toward notions of natural necessity via an interest in constitutive causes but, if this is the case, then they will have to deal with the implication of treating societies as natural kinds, which I believe is what arises as an issue when natural necessity is applied to social things. What is attractive, however, about natural necessity is the promise it holds out of causes which are not external but which are not subjective; or differently put, of internal relations which are causal but which are not tied to reasons.

An interesting exemplar might be Durkheim who in several ways can be seen in this light, as treating societies as natural kinds. First, in his treatment of concomitant variation and his reworking of Mill’s (and Hume’s) methods of similarity and difference, where Durkheim essentially argues that he is correlating the deep nature of things (their essential structures, not their phenomenal appearances49); second, in his argument about

49 This feature is striking. The Rules of the Sociological Method and The Elementary Forms of the Religious Life in particular are shot through with discussion of internal causes, impersonal hidden forces, surface and
the constitutive features of the social world – its deep structure -- in his answer to the
question, how is society possible (which is set up as a quasi-Kantian transcendental
deduction but which in Kantian terms is a transcendental paralogism\(^{50}\)) and, third in his
interest in functionalism (teleology without action). I note as well that this points to the
basic problem in Parson’s work on social action – the attempt to marry this argument
about natural kinds to a very different kind of neo-Kantianism in Weber who did not
think of societies in these ways as natural kinds.

(xix) A qualification to an earlier remark about Weber (page 6) is now entered, a
qualification relevant to the discussion at this point. Weber does argue that “the
proposition X is the only means by which Y can be attained is the reverse of Y is the
effect of X” (1949: 26) thus seemingly bringing together not only science and politics but
potentially as well causes and intentions. I want however to draw attention to the
limitation to this assertion which Weber also introduced. He restricted this argument to
ends which were unambiguously given, keeping in mind of course that the subject matter
of sociology, “decisive for its status as a science” (Weber, 1968: 24), was social action
and, in so doing, undid the assimilation of science to politics which this argument seemed
to establish. In effect, his argument was that politics does not assume settled ends and, as
a consequence, problems of social policy can never be resolved merely on the basis of
purely technical considerations (1949: 86). The distinctive characteristic of social policy

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\(^{50}\) A transcendental paralogism collapses the distinction between epistemic and ontological conditions of
possibility (Allison, 1983). Since it is such a gross error, a principle of charitable interpretation might
suggest that Durkheim’s argument is not actually Kantian (or if Kantian very neo-Kantian), although there
is a long tradition of linking the two even while recognizing other influences on Durkheim. The treatment
of Durkheim by Tiryakian (1977) is interesting in this regard.
was precisely the absence of settled ends and the distinctive characteristic of natural science was precisely its answers to the question of what we must do if we wish to master life technically (1946: 144, cf. 1949: 35, 46-52). This position in fact drives a deep Kantian-type wedge between the realm of autonomy and choice via self-legislation and a realm of necessity and causal relations. Politics is from this point of view much more an activity than a causal process.

Weber’s construction of this realm of self-legislated autonomy is notoriously wilder than the Kantian version. This realm is populated by unbelievers, hedonists, ascetics, charismatics, warring gods and so on. All of these characters are consistent with ‘bare’ self-legislation strictly understood. The basic characteristic of this realm is one of conflict and of deep disagreements about values. Weber’s construction is not tempered as was Kant’s by a kind of pietism (Hunter, 2001) which limits the results of self-legislation. Weber’s version might be Kantian in form but not in tone or substance. Hence this realm is not disciplined by a shared horizon of meaning, so where a form of pietism is available to Kant and acts as a kind of disciplining force from within, Weber must locate discipline elsewhere and does so in the externally objectified forces of rationalization which disenchant at the same time as they discipline modern humanity.

(xx) The Kantian dualism thus establishes a distinction between the transcendental self or ego and the empirical self and from this distinction flows much of the subsequent discussion of the Geisteswissenschaften. There is then another way to read this dualism in light of an interest in reasons and causes. If this division is to be strictly maintained, then everything empirical must be caused and everything transcendental, if it is strictly transcendental, must be uncaused. A pure transcendental ego must be able to
create the grounds for its own rational freedom without being conditioned by causes, without invoking or depending on causes which are strictly empirical. This is the bootstrapping operation essential to the transcendental deduction which, if successful, guarantees the autonomy of the pure transcendental ego by distinguishing the synthetic a priori from a posteriori knowledge.

But if everything empirical is caused and if social action is empirical, then it should follow that social action is caused. And in this case, means must stand to ends in causal relations. Everything which is a posteriori is caused and there are no exceptions, action included. If an exception is made for action, this is to blur the fundamental division between the transcendental and the empirical self.

The difference between the reasons-as-causes account and the non-causal reason-explanation account is that the first denies this division altogether while the second seems to violate it while still drawing on it. In the first account, this is a division or distinction which is not needed. And so action is not exempted, it is explained causally by the empirical conjunction of beliefs and desires. After Kant, the issue is a little different.

So on the second account, we might be seen as agreeing with Kant that human reason is not naturalized and that when reason explains, it does not do so causally but, against the Kantian grain, I allow reason a place in the empirical realm so that, when we turn to action, we have available explanations of action, with its characteristic structure of means and ends, and these explanations are not causal. On this second account, the Kantian dualism is reworked; in effect what I have done is to introduce into the realm of the empirical self a notion of autonomy\textsuperscript{51}. In the second account we no longer work with

\textsuperscript{51} By this move, the Kantian distinction between autonomous choice and heteronomous choice is effaced. This is not to say that it cannot be preserved in some other form.
the basic distinction between the transcendental and the empirical, we do not invoke noumenal beings but we do recognize the importance of self-legislation in human associations\textsuperscript{52,53}. That said, this account thus is not dualist but it is also not reductive.

(xxi) Efficient causes lack explanatory force when the subject matter is action. Do the arguments of this paper then entirely eliminate recourse to causal language in explanation? Actually not completely, but they do clearly subordinate causal explanation to intentional explanation.

Many of the chains and combinations of actions which are generated in human associations are complex, they are also sedimented in time; these might be treated informally and non-technically as causal relations as long as such constructions remain consistent with intentional explanations, and as long as they are recognized as second best substitutes for such explanations, to be used when a reason/intentional explanation is awkward or cumbersome. Here causal language functions informally and non-technically as a kind of shorthand. There are as well byproducts of action, particularly unintended consequences, not merely of individual atomic acts but within social interdependencies, which might be construed in causal terms, but once again only against the backdrop of a basic commitment to intentional (reason) explanation. Human associations, moreover, are rule-bound in various ways that shape action; we might think of this kind of relation

\textsuperscript{52} Among other issues which arise here, I note just one – the relationship between ‘voluntary’ which was introduced earlier in the paper as a characteristic feature of intentional action and personal ‘autonomy’ (alternatively ‘freedom’, keeping in mind that some of their contrasts do not cash out as equivalents: non-interference versus non-domination for example). The latter, ‘autonomy’, is more a characteristic of some human associations (rather than others) and it shades off into a human value (in some associations) in a way that ‘voluntary’ does not. The contrasts are correspondingly different: ‘involuntary’ does not stand in the same relationship to ‘autonomy’ as it does to ‘voluntary’. Thus action can be voluntary without being autonomous and an act which is voluntary but not autonomous is not for that feature efficiently caused.

\textsuperscript{53} Perhaps, for some Kantians, to say that autonomy is a value is to say that it is conditioned in a way that violates the purity of autonomy when the latter is secured by its association with the transcendental self (Pippin, 2001). If we go the route of this paper, and rework the Kantian dualism, is there a way to preserve a place for unconditioned autonomy of some kind, or a way of reconstituting the transcendental realm? For further discussion of transcendental arguments, see Sacks (2000).
between rules and action informally as a causal relation, realizing that rules are not causes when we want to cash in this relation in an explanation\textsuperscript{54}.

In other words, causal language, when used for these reasons and in these ways, is metaphorical. We treat some of the complex processes and outcomes generated in action as if they were causal relata. We allow this language because, unlike some other “as if” constructions discussed earlier, this construction does not appear to be question-begging. We recognize what we doing. When we must make good an explanatory claim, however, we abandon these uses of ‘cause’ and move to intentional explanations.

X

This concludes the argument and thought experiment. The most general, and the easiest, conclusion to take away from this paper might be put: The use of causal language in the explanation of action in the human sciences needs further development and defense in light of continuing work in the philosophy of action. The specific treatment of intentional action and intentional explanation in the paper has shed critical light on a set of assumptions and arguments in the social sciences and indicated some of the issues which need further analysis when a critical treatment of causal language in the human sciences is undertaken, particularly when that treatment takes into account the peculiarities of human action. The distinctive feature of the argument and thought experiment is the refusal to begin from the position that, criticisms notwithstanding, causal language must and should be preserved in the social sciences. We can accord

\textsuperscript{54} Compare Schueler (2003: 16).
causality its proper place only after adopting a more skeptical stance than this original position admits.

REFERENCES


