
Deflation and Construction: Rendering Social Causes Meaningful

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In commenting upon Isaac Ariail Reed's interesting book, I shall first indicate what seems to me especially valuable in its approach and style of argument. Second, I set out what I take to be the gist of that argument. And third, I shall pose two questions that that argument raises.

Reed's book revisits, in a fresh and insightful way, the old question of the relations between the natural and social, or human, sciences. Reed makes no distinction between the human and the social (thereby embracing psychology as social) and he does not discuss what might be special to particular disciplines. He proceeds throughout by citing, with just sufficient detail, some telling examples of sociological and anthropological studies, to support both what he criticises and what he advocates. He criticises, at a general level, the kind of 'scientism' that presumes that social or human, like natural, science is engaged in 'reconstructing sense experience and observation in terms of a vast and precise set of theories and laws'. [Reed 2011: 36]. More specifically, within the social sciences he criticises what he calls 'realism' for presuming that their goal is to 'reveal underlying layers of social life', 'underlying mechanisms or structures' that are taken to exist 'in all sorts of times and places' [ibid.: 40], and that bring together disparate cases under the same general scheme. In particular, he rejects the idea, spelled out by Jack Goldstone, that social scientific explanation involves 'making deductions about how events are linked over time, drawing on general principles of economics, sociology, psychology, and political science regarding human behavior' [Goldstone 2003: 48]. He also criticises the anti-scientistic, but also anti-explanatory rhetoric of post-modern hermeneutics and the philosophers, notably Richard Rorty, who encourage and endorse it. He advocates seeing both natural and the human sciences as interpretive all the way down, though in significantly different ways, the

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former affording, via the language of mathematics, a degree of abstract certainty unavailable to the latter. He rightly presents explanation as auxiliary to the 'larger category of understanding' [Reed 2011: 35], thereby, as we shall see, raising the central question of what it is to explain. And he pursues the suggestion of the early Habermas that there are distinct interest-driven epistemic modes: one seeking prediction and control of the natural and social world, and another, commonly called 'critique,' guided by 'emancipatory' or 'utopian' aspirations, seeking to answer the normative question of '*how, when and where* the good can be or was made actual, in actors' minds or in social institutions' [ibid.: 86].

Reed's distinctive argument, as I understand it, is both deflationary and constructive. He seeks to undermine the plausibility of the approach—'still hegemonic in American sociology'—that posits a probabilistic 'law-based and hyper-generalized model of explanation' and relies on quantitative methods and an 'often quite explicit commitment to the idea that the unity of the sciences derives from the unity of their methods' [ibid.: 133]. His positive suggestion, distinguishing him from standard anti-positivist advocates of interpretive approaches, is to propose an alternative method for the human sciences that retains the idea that explanation is causal but promises to offer the rudiments of a distinctive account of social causation. His idea is that social causes operate within what he calls 'landscapes' of meaning, as in a Brueghel painting: that 'meaning' intersects with such causes 'by giving their *force* concrete *form*, and thus that the interpretation of meaning is central to constructing causal explanations in the human sciences' [ibid.: 135].

This sounds remarkably close to Max Weber on meaning and causality. Reed quotes Weber on 'the uniqueness of the reality in which we move' and his wish to

understand on the one hand the relationships of the cultural significance of individual events in their contemporary manifestations and on the other the causes of their being historically *so* and not *otherwise* [Weber 1949: 72].

What Reed offers, in deploying this approach, is the suggestion that social mechanisms, viewed as 'efficient causes in the social realm' [Reed 2011: 144], become explanatory when, and only when, rendered meaningful (given 'form') by reference to the context (the 'landscape') in which they operate. Thus, in Weber's account in *The Protestant Ethic and the Spirit of Capitalism*, the mechanism of salvation anxiety only makes sense 'inside the meaning-system of Calvinist (and, more broadly, Protestant) Christianity' [ibid.: 140] Thus the 'Putnam-led social capital format of analysis' is empty until grounded in an ethnographically specified context, such as revealed by the study of volunteer church groups in the American Midwest by Paul Lichterman in his book *Elusive Togetherness: Church Groups Trying to Bridge America's Divisions*: otherwise, 'network analysis without interpretation remains blind' [ibid.: 128]. Thus the ritual of the confession, as described by

Foucault, 'is given its lifeblood, its shape and effectiveness, by the discursive formation through which it takes place' [ibid.: 159] Thus the comparative historical sociology of Barrington Moore and Theda Skocpol errs in making 'realist' causal claims that purportedly dismiss ideological content as non-explanatory (though he rightly questions whether they succeed in doing so).

The deflationary aspect of Reed's argument is, then, best seen as making a case against what Jon Elster has called 'excessive ambitions' in the practice of social science. Elster, like Reed, embraces the concept of 'mechanisms' as a way of identifying efficient causes and, also like Reed, distinguishes between explanatory and normative theories, deploring excessive ambitiousness in both [see Elster 2009a, 2013]. Unlike Reed, however, Elster sets out in some detail the ways in which currently dominant approaches—rational choice theory that prevails in economics and political science, behavioural economics, and statistical data analysis, widely practiced in sociology—are, in his view, remarkably unsuccessful in the business of explanation.

Rational choice theory, according to Elster, fails for two reasons: first, it fails to deliver determinate predictions, and, second, agents are widely irrational, failing to conform to the predictions of the theory. Beliefs are indeterminate and behaviour cannot be uniquely predicted, for people generally lack 'the capacity to make the calculations that occupy many pages of mathematical appendices in the leading journals' [Elster 2009a: 7] and imputing 'as-if' or bounded rationality fails to solve the problem. In short, except in a 'rough-and-ready sense', 'the sophisticated models that are the pride of the profession' fail to 'explain, predict or shape behavior' [ibid.: 9]. As for behavioural economics, its undoubted successes are mainly found in the laboratory, and what it reveals is that human behaviour

seems to be guided by a number of *unrelated quirks* rather than by consistent maximization of utility. In fact, there are so many quirks that one suspects that for any observed behavior, there would be a quirk that fits it [ibid.: 12].

And as for data analysis, Elster suggests that, being subject to 'an almost infinite number of potential temptations, pitfalls and fallacies' [ibid.: 16], it is neither a science nor an art but a craft, governed by informal norms shared by elite scholars. He quotes Chris Achen, author of the significantly titled *Interpreting and Using Regression*, writing that 'wise investigators know far more about true variability across observations and samples than any statistical calculation can tell them' and that the 'process of testing and eliminating counterhypotheses is a subtle skill that cannot be reduced to rote' [Achen 1982: 40, 52]. On which Elster comments that 'substantive knowledge is often indispensable' and that 'deep familiarity with the field in question' may be needed to distinguish causal from spurious correlations [Elster 2009a: 16]. But what is the nature of this 'substantive knowledge' based on skill and yielding wisdom? Is it knowledge of meanings—interpretive but not yet causal? These suggestions of Elster seem very close to Reed's

suggestion that we need ‘in-depth knowledge of cases’, that ‘in the interpretive epistemic mode the counterfactuals of a given case or set of cases emerge from the holistic knowledge of the meanings active in a case’ [Reed 2011: 156] What Elster calls a ‘field’ Reed prefers to call a ‘landscape’ (disliking the agonistic connotations of the former and the idea that fields are formally isomorphic), but both appear to agree (with one another and with Achen) that the interpretation of statistical data cannot dispense with context-relative substantive knowledge, whose acquisition depends on wisdom, skill and the following of informal norms.

Elster’s case is essentially a plea for modesty, even humility. The future, or at least the hope, for social science, he concludes, lies not in looking for general laws, but rather in ‘the cumulative generation of mechanisms and their application to individual cases’ [Elster 2009a: 24] and in replacing the aim of prediction with that of retrodiction. We should just accept that mechanisms are ‘frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences’ [ibid.: 23; emphasis removed]. That is why he calls Alexis de Tocqueville the ‘First Social Scientist’ [Elster 2009b]. And he makes a similar plea for modesty and humility with respect to normative theorising [see Elster 2013]. How should democratic institutions, such as trial juries, political assemblies and electoral systems, be designed in order to produce good outcomes? To answer this, we need to know what counts as a good outcome, about which legal and political philosophers notoriously disagree, without prospect of rationally-based resolution. And we need, with respect to any chosen conception of goodness, a causal theory. Elster’s view is that we have a double indeterminacy here—‘of plausible-sounding but unprovable normative views, and of plausible-sounding but unprovable causal theories’, leading to ‘a deep disillusionment in public debates’ [Elster 2013: 4]. He concludes by arguing against positive institutional design that aims at producing good decisions, selecting good decision-makers or creating good decision-making bodies, and in favour of designing institutions that ‘insulate decision-makers as much as possible from the influences of self-interest, passion (emotion or intoxication), prejudice and cognitive bias’ [Elster 2013: 5]. In making this case, his guiding spirit is Jeremy Bentham. What he takes from Bentham is not the familiar maximising, aggregative utilitarianism but rather a conservative, practical meliorism that can, however, have radical implications for institutional reform.

Reed has relatively little to say about the normative epistemic mode, though his writing of ‘maximal interpretations in the normative epistemic mode’ does suggest something more ambitious than Elster’s cautious Benthamism. It is Reed’s attempt to unite explanation via causal mechanisms with the interpretation of meaning through ‘forming causes’ that suggests an agenda more ambitious than Elster’s, constituting ‘only the very beginning of a project of historicized social explanation and theoretical pluralism’ [Reed 2011: 169].

This raises questions for his readers, and so I shall conclude by briefly indicating two. Not surprisingly, given the immense scope of the topic—are the

human sciences a branch of natural science?—addressed by Reed’s slim volume, these are, indeed, large and difficult questions.

First, on what account of causation does he rely in writing of ‘motivation and mechanism’ as ‘the forcing causes of social life’? His interesting foray into the exegesis of Aristotle on causation, his introduction of ‘formal’ causes in order to introduce interpretation and his endorsement of reasons as causes do not really help to answer this question. We have, as Brian Epstein has recently written, ‘scads of examples of events that stand in causal relations to one another, but there is basic disagreement about the characteristics of causation’, and yet ‘that does not prevent the term “causation” from picking out a particular metaphysical relation, even though we do not quite know what it is’ [Epstein 2015: 81]. What is clear is that Reed wants to reject law-based accounts of causality. My suggestion is that he could usefully help himself to John Mackie’s so-called INUS account according to which a cause is an insufficient but necessary part of a condition which is itself unnecessary but sufficient for the result [see Mackie 1980].

Second, given his rejection for the human sciences of the search for laws and of the notion of ‘general principles ... regarding human behavior’, on the one hand, and his acceptance of the need for ‘in-depth knowledge of cases’ and their ‘peculiarities’ [Reed 2011: 109], on the other, the question arises of the level at which explanatory mechanisms are to be sought. Here the metaphor of ‘landscapes’ and the reference to a Brueghel painting, as opposed to that of ‘fields’, suggests a context, or ‘regime of signification’ [ibid.: 109], that looks very local. On what scale are such contexts or regimes to be conceived and thus how abstract or general are the mechanisms that social scientists should seek in their explanatory quest? How are we to determine where the boundaries of ‘landscapes of meaning’ lie? And how does the human science of political economy fit into Reed’s scheme in our ever-more globalised capitalist world?

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