Realist Metatheory and Qualitative Methods

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Abstract

INTRODUCTION

Since at least Dilthey, there have been two contradictory perspectives in social science. One holds that, like the natural sciences, the social sciences seek causal explanations; for the other, since meaning is the critical feature of all social interaction, explaining action requires identifying the meanings involved in action. Typically, the former view, associated with "hard science" methods, assumes the Deductive-Nomological model of explanation. On this view, an outcome is explained by showing that it is an instance of a "law": If C, then E; C; hence E. But as Blumer saw, we need to reject the assumption that behavior can be explained by appeal to regularities between "causative" factors and "the behavior they are supposed to produce." As he observed, "the typical sociological scheme ascribes behavior to factors such as status position, cultural prescriptions, norms, values, sanctions, role demands, and social system requirements" (Blumer 1969: 7). The fallacy was obvious to him: "The meanings for things for the human beings who are acting are either bypassed or swallowed up in the factors used to account for behavior." Moreover, they fail to see that "the use of meanings by a person in his action involves an interpretative process."

This insight needs to be preserved. But it can be preserved in a causal model, once we reject the Humean understanding of causality and the Deductive-Nomological (D-N) model of explanation on which it depends. Remarkably, it is not just that this model cannot do justice to the explanation of action; it cannot be sustained as regards explanation in the physical sciences. I need here to be very brief.[1]

[1] For more discussion, see Manicas, *A Realist Philosophy of Social Science* (Cambridge: Cambridge University Press, 2006, Chapter 2).

In the first place, theories are not logical systems in which conclusions –what is to be explained, follow logically from premises—ideally in the language of mathematics. As Harré writes: "In fact, in actual science, deductive systems are quite rare: fragments of such systems can be found in physics, but mostly scientists come up with descriptions of structures, attributions of powers and laws of change, related by having a common object, not by being then and there deducible from a common set of axioms."[2] This requires an entirely different understanding of scientific theory, sketched in what follows.

Second (and following on the foregoing), it is easy to show that entailment is the wrong relation for explanation. That is, on the D-N conception, the *explanandum* follows logically from the premises: If P, then necessarily Q. If the *explanans*, P, is true, we have provided sufficient grounds for believing that the *explanandum*, Q, is true. So, presumably, Q is "explained."[3] But not only is this contrary to our intuitions about explanation, it is easy to construct counter-examples of D-N "explanations" with true premises which are just plain silly. They not only do they not explain, but they do not even provide grounds for believing that the *explanandum* is true.

The realist alternative argues that scientific explanation is *causal* explanation but it rejects causality understood in Humean terms as mere relations of constant conjunction. This is the key point: It argues, instead, that causes are productive powers. "Things," ("powerful particulars") have the causal powers they have by virtue of theorized causal mechanisms. To take the simplest case, on D-N view, if one has the "law," "If salt is put in water, it dissolves," you can then predict that in a particular instance, a consequence of putting salt in water will be that it will (probably) dissolve. But similarly, on this view, (allegedly) one explains why some instance of salt dissolved by appeal to the same "law." On this view, explanation and prediction are symmetrical. But we are very often in a position to explain some outcome when it could not have been predicted. And the reasons are clear: in the real world, there is always causal complexity and the sequence of time is important. Thus, again to take the simplest case, some particular instance of salt, may never be dissolved if, for example, it is never put in water! [4] Of course, in some particular instance, it is part of the explanation of its dissolving that the salt was put into water and this explains some of the attractiveness of the D-N model of explanation. And indeed, while science may well begin by identifying patterns and regularities in the world, a scientific explanation does not aim at establishing "law-like" relations of variables, no matter how probable.

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[2] Rom Harré, *Principles of Scientific Thinking*, p. 10.

[3] Hempel acknowledged a weaker form, "inductive statistical explanation." However, in these we lose all hold on the individual case. Sam wants to know why he got lung cancer and doesn't smoke and Harry, who smokes, is free of cancer! Explaining this will require a understanding, currently unavailable, of the causal mechanisms involved and detailed biographies of how, in real time, these mechanisms related.

[4] The solar system is the worse possible paradigm. For all practical purposes, this system is closed: There are as many independent equations as there are independent variables, no large mass is likely to enter the system, and time, accordingly, is irrelevant. But the world is not Laplacean. A far better example for social science is evolutionary biology.

Rather, explanation comes with identifying the causal mechanisms at work. In this example, there is something about salt *and* water, such that when salt is put in water, it tends to dissolve-- and not (say) to explode or turn the water to gin! People knew that salt tended to dissolve in water before we had molecular chemistry, but they did not know why. Indeed, the idea that the goal of science is to identity and confirm "laws" which explain by subsumption, obscures the critical role of theory and model building in real science.

In what follows, it is suggested that at the heart of social science explanation is the idea of a social mechanism with persons as causal agents. Society is the causal outcome of humans, acting with "materials at hand"—with nature and the inherited legacy of prior actions and interactions. But critically also, they need not be aware of the conditions and consequences of their actions and they may not see that the practice which their action is reproducing involves a social mechanism. As Bourdieu writes: The task of sociology is to "uncover the most profoundly buried structures of the various social worlds which constitute the social universe, as well as the 'mechanisms' which tend to ensure their reproduction or their transformation" (Bourdieu and Wacquant 1992: 7). But since persons are the dominant causes of what occurs in society, [5] the first problem for the social scientist is to understand action as it is understood by the actors. A second step, not always acknowledged by qualitative researchers, is to determine whether the actors have an adequate understanding of their action. This is especially critical for a realist account of social science and we return to this.

QUALITATIVE RESEARCH

Realizing this first step requires qualitative research. Indeed, while qualitative research includes a widely various set of assumptions, methods and techniques, a generic definition would have it that it seeks to understand social phenomena in terms of the beliefs and understanding which people bring to them. We need to establish what the phenomenon means to the members. Critically, this is always an interpretive problem. That is, since meanings are not directly accessible, this always involves an effort on the part of the researcher to understand the "other." Ethnography, of course, is a typical, perhaps paradigmatic interpretative effort, but such research includes as well, more narrowly conceived structured and unstructured interviews, translation and participant observation. But as in "culture studies," it may consider a wide variety of empirical materials, including "discourses," texts, historical documents, life stories, films, advertising, indeed, any material which is meaningful to actors and which therefore demands interpretation. While there is considerable overlap in strategies of

[5] We say "dominant cause" instead of "sole cause" since there are also physical causes which have serious impact on what happens in society. Hurricanes and pandemics are only the most obvious of these. This is a consequence, more generally, of the fact that persons have bodies and are part of a nature which we do not construct and to which we must respond.

achieving interpretation of meaning, there is little agreement on assumptions and goals of such research. Thus, it may draw on the naturalism of the symbolic interactionist tradition, the hermeneutics of Hans-Georg Gadamer, the phenomenological approaches of Edmund Husserl and Alfred Schütz, psychoanalysis, versions of Marxism, feminist theory, or more recently the "deconstructive" and "genealogical" techniques of Jacques Derrida and Michel Foucault.

"OBJECTIVITY" AND QUALITATIVE RESEARCH

But despite differences, a key point of agreement in these meta-theoretical approaches derives from a common problem of qualitative research as defined here. Since these approaches share in rejecting a positivist theory of science in which "data" are "given" and unproblematically available for "objective" inquiry and since all these perspectives assume that all meaningful "data" requires interpretation, the question arises: What are the problems of "understanding" and what is the status of the conclusions at which one arrives? We can distinguish two different sorts of answers.

One can hold that at the conclusion of competent inquiry, one has achieved a more or less adequate understanding of the social phenomena as that is understood by members, Speaking broadly, this is the posture of the naturalism of SI theory, the approach of critical realism and most readings of the hermeneutic tradition. For these perspectives, the problem is an instance of the more general fallibilism of any assertion (hypothesis, interpretation, etc.). That is, from this point of view, one must acknowledge that any hypothesis or interpretation may be wrong—even radically so, but also insist that this is true of all science. As regards interpretation, we have what people say and what people do, we have texts and artifacts, and one must engage these critically. Just as the field linguist approaches the construction of a translation of an alien language, the problem is to consider reflexively our hypotheses regarding meaning. That is, the researcher must consider what he is assuming and with this as essential background, make judgments about the meaning of action of the observed. Is the researcher making unnoticed assumptions which distort the effort to comprehend? Does the interpretation make sense? Are their anomalies which need to addressed? Are their alternatives that need to be considered? Does the interpretation stand up to behavioral tests? Etc. Similarly, since (as the hermeneutic tradition insists), meanings are cultural products which do not present themselves neutrally or with one voice, and since the observer and the observed are situated spatially and historically, we cannot hope for an absolutely objective, theory-neutral interpretation.

At best we can have but qualified objectivity—indeed, exactly parallel to the objectivity available in *all* science. That is, all post-positivist philosophies of science acknowledge that there is no "logic" of discovery, confirmation or falsification and no algorithm which assures a scientific consensus (when it is achieved). Indeed, recent valuable work in the sociology of science, inspired in part by the work of Thomas Kuhn, shows both that while scientific practice does not obey the constraints of "rationality" as

envisioned by Vienna logical empiricism, it does answer to contextually responsible forms of rationality. At the very least, the practices of the successful physical sciences have evolved norms regarding inquiry which practitioners acknowledge, usually tacitly. Publicity and consideration of evidence is one. Acknowledgement of a stubborn reality and fallibilism is another. Thus, while work in the sociology of science gives us a deeper understanding of the actual practices of the sciences, few, if anyone, would go so far as to say that scientific practices are indistinguishable "methodologically" from non-scientific practices. Similarly, given agreement on critical norms governing inquiry, there may well differences in techniques and even in criteria for evaluating outcomes.

The social scientist does have, however, an already noticed special burden. For social science there is a "double hermeneutic." The natural scientist is interrogating a mind independent nature, but arriving at consensus among colleagues regarding its processes is a hermeneutic process. That is, the physical scientist must communicate with, and convince colleagues, that her claims deserve acceptance. The social scientist, by contrast, is interrogating a meaningful social world. Getting an understanding of this is the "first" hermeneutic. But then, like her colleagues in the physical sciences, she must seek a consensus among colleagues regarding her claims about this meaningful world, the second hermeneutic process. We can perhaps see more clearly what this means by considering the second option, which can conveniently be termed "ethnographic skepticism."

ETHNOGRAPHIC SKEPTICISM

Ethnographic skepticism is very much influenced by so-called "post-modern" epistemologies. It asserts either that even a qualified objectivity is impossible or, more radically, since there is nothing "objective" about socially constructed meanings, the question of true or false does not even arise. On this view, granting that there is no God's eye view of the world leads to the conclusion that there are only *different* views of the world and there can be no adjudicating between them. Thus, for Lyotard, there is an "heterogeneity of language games" with no consensus possible as regards "rules" or "meta-prescriptions." Each is subject to its own "pragmatic rules," and they are not "isomorphic." That is, lacking a universal set of "rules," these many "discourses" are not inter-translatable. Hence any "consensus" must be "local." "The game of science is thus put on a par with the others." One may take a feminist perspective or the perspective of the colonized. These will provide very different conclusions regarding the understanding of society. The question then may asked, are the likely different conclusions merely "true for them," females or the colonized"? Is the account *merely* a feminist point of view, or is the account true (period)—as much true as quantum theory is true? The difference is fundamental. To say that some assertion is true is to claim authority for the assertion: we are obliged to accept the claim despite our "opinion" to the contrary. "True for me," or "true for us," makes no such claim. But of course it also ends argument. Scientific claims are fallible exactly in the sense that argument is always possible.

We need first to notice our taken-for-granted ability to understand one another in our everyday lives. As Weber pointed out, this involved what he called *Verstehen* ("understanding"), the human capacity to grasp the meaning of another's actions. We must not think of *Verstehen* as some sort of special, intuitive, sympathetic understanding, a reliving of the experience of others. *Verstehen* is something we all always do! We are engaged in *Verstehen* in judging that a person on a ladder is painting the house, in judging that the expression on mother's face is distress produced by our careless remark, and so forth. We learned to do this, indeed, when we learned to use language! There is nothing dubious about such judgments since, as with any judgment, they require evidence and may, subsequently, be rejected. While it can hardly be denied that we are able to understand one another, philosophers have argued about how this is possible. The biggest difference is between theories which follow George Herbert Mead's "social behaviorism," those that take a position which derives from the phenomenology of Edmund Husserl and those that follow the "hermeneutics" of Hans Geog Gadamer. The differences here are philosophically important but need not bear on concrete inquiry. Indeed, most qualitative research usefully employs insights derived from these competing philosophical positions.

Second, our ethnographer is not a Martian, but a human being. And even if the culture she studies is very, very different than her own, it remains a human culture: hence, *verstehen* will still be critical. In the worst case, accordingly, the researcher has available the same evidence that members have—the actions and products (for example, texts, artifacts, etc.) of members. Some actions will be immediately understood: they are seeking food, building a shelter. Eventually interactions succeed, expectations are realized, there is communication and understanding. Of course, this will take some time and considerable skill, and of course, our ethnographer might be wrong—perhaps in detail, perhaps in some fundamental way.

PRIVILEGING PERSPECTIVES

But doesn't the "native" have a "privileged" understanding that is inaccessible to the "other?" Consider then possible "privileged" points of view: "the colonized," "women," "Black women," "women of color," "upper-class women of color," "urban lower class women of color," etc. The issue is not whether these "voices" have been suppressed in white male dominated positivist social science: They have. Nor is it argued that much qualitative work is poorly done, distorted in this way or that. The issue rather is epistemological: Because each of us, logically, has a unique biography and position in society, each person's viewpoint is strictly speaking unique. We seem driven beyond "relativism" to a radical subjectivism—a position which is ultimately incoherent! I cannot be a native but I cannot be you either. But this is a pseudo problem: Meanings are not in the "minds" of actors. Meanings are "out there" in the practices themselves. They are "intersubjective," neither "subjective" nor "objective." Thus, the problem of understanding "the other" begins at home. In everyday life, we do not turn a problem into an

impossibility! Indeed, as Mead and Schütz insisted, if I am to communicate with you at all, I must in some measure take your position.

One cannot, to be sure, overestimate how difficult it may be to achieve an adequate interpretation. Granting that such objectivity is "situated" and not "absolute," a "situated objectivity" will require "reflexivity" in Bourdieu's sense. That is, in addition to being aware of the obvious potential positional biases, there is question of "the objective space of possible intellectual positions offered to him or her at a given moment…" and finally, there is "the intellectualist" bias of construing the world as "a spectacle." On this view, "knowing" is not a "reflection" of reality nor a mere construction, but is "disciplined by the otherness with which it engages." Not only may informants lie or distort what they report—sometimes for very good reasons, but they may not be self-conscious of meanings that their behavior seems to confirm. Plainly, reflexivity puts strong requirements on questions in interviewing and on perceptions on what is going on in participant observation, but it does *not* require that positivist criteria of validly, reliability and generalizability need be satisfied. It suggests also that considerable trust will be demanded on both sides. Finally, any viewpoint will leave much out. Accordingly, as Schütz insisted, the social scientist is obliged to take care that "other voices" are heard, that the account is as "objective" as is humanly possible. Patricia Hill Collins makes the point: "Each group speaks from its own standpoint and shares in its own partial, situated knowledge. But because each group perceives its own truth as partial, its knowledge is unfinished. Each group becomes better able to consider other group's standpoints without relinquishing the uniqueness of if its own standpoint or suppressing other groups' partial standpoints" (Collins, 2000: 46).

THE ROLE OF QUALITATIVE RESEARCH

A second fundamental question regarding qualitative research regards its role in social science. One view holds that it is the whole of social science, that one cannot do any more than provide a description of the meanings shared by members. Indeed, on this view, the idea that we can find general laws, or causal explanations, is completely misplaced. As noted, this posture has roots in the *Methodenstreit* in the late nineteenth century. Resting on entirely different "foundations" from the natural sciences, the human sciences employ entirely different methods. Clifford Geertz speaks, accordingly, of "thick description." On this view, the boundaries between "science" and non-science are, at best, blurred. A good deal of cultural anthropology (often also influenced by postmodern ideas, above) takes this path. On this perspective, there is nothing in social science which even faintly resembles theory in the physical sciences.

Another posture accepts a distinction between "micro-sociology" and "macro-sociology" and offers that qualitative methods are the critical component of micro-sociology. Presumably such work complements macro work which, operating a different level of abstraction, seeks structural explanations of action. On this view, the macro sociologist offers explanatory theories, for example, that some system functions to assure

allocation or legitimation. But whether macro work in fact is complementary to micro work is contestable. Schütz's criticism of Parson's structural functionalism shows why. Schütz wrote:

> Professor Parsons has the right insight that a theory of action would be meaningless without the application of the subjective point of view. But he does not follow this principle to its roots. He replaces subjective events in the mind of the actor by a scheme of interpretation of such events, accessible only to the observer, thus confusing objective schemes for interpreting subjective phenomena with these subjective phenomena themselves (Grathoff 1978: 36).

But,

> The answering of our question, 'What does the social world mean for me, the observer?' has as a prerequisite the answering of the quite different questions, "What does this social world mean for observed actors within this world, and what did he mean by his acting within it?' With these questions, we no longer naively accept the world and its current idealizations and formalizations as ready-made and meaningful beyond all doubt, but undertake to study the process of idealizing and formalizing as such, the genesis of the meaning which social phenomena have for us as well as for the actors, the mechanism of the activity by which human beings understand one another and themselves (Wagner 1983: 48).

The upshot of much macro-theory has been a tendency to assume that one has the meanings of participants without actually doing qualitative research, that meaning is captured without examining the concrete contexts of action. But there is a wide range of work which is generally deemed "micro" which takes Schütz's advice and seeks "to study the process of idealizing and formalizing as such, the genesis of the meaning which social phenomena have for us as well as for the actors, the mechanism of the activity by which human beings understand one another and themselves." This includes the work of Goffman and the ethnomethologists (of course with differences) and might also include the work of symbolic interactionists. However, it is not clear whether this work is best seen as seeking to complement macro-orientations or as offering a different *kind* of social science, one which is perhaps closer to Geertz.

A clear third choice is provided by some versions of Weber and some versions of Marxism, critical realism, the work of Giddens, Bourdieu and perhaps also Foucault. These orientations all reject a micro-macro division and all hold that "social structure" cannot be understood without acknowledging that society is the product of individual agents working with materials at hand. It follows then that all *explanatory* social science has an ethnographic moment. But critically, this is seen as but the first step.

The key point for these writers is that once one gets an understanding of the meaning which social phenomena has for actors, it must now be asked: Is their understanding adequate? That is, while actors need to have practical knowledge

sufficient to carry on practices in society, they need not have an understanding of the conditions and consequences of action. They may, accordingly, *mis*understand what is happening in society. More generally, it is possible that they are acting on false beliefs and that, indeed, if they were to come to this conclusion, they might act otherwise. Consider, for example, the belief that women are capable only of domestic and reproductive activities. This belief will surely support and legitimize patriarchal social relations and practices in the institutions of society. But if women (and men) came to believe otherwise, actors would be motivated to change these practices. On this view, it is a critical task of social science to explain why people have the beliefs they have. Thus, by explaining our beliefs and by making clear the conditions and consequences of social action, social science is potentially emancipating.

Moreover, on this view, theory has a role analogous to theory in the natural sciences. Thus, the rusting of iron is made intelligible by molecular theory which provides an explanatory causal mechanism. By analogy, social outcomes are made intelligible by theorizing social mechanisms. These provide accounts of action in terms of the meanings and beliefs of actors *and* an explanation of why the outcomes are as they are.

SOCIAL MECHANISMS

The idea that the explanatory goals of social science require theories of social mechanisms is hardly new, even if the idea is very often an unarticulated background assumption of studies. This is evident in mainstream economics,[6] and among writers we think of as doing agent-centered work, e.g., symbolic interactionists, Erving Goffman, rational choice theorists and the recent work of James Coleman. But as well, and less noticed, it is at least in the background of many more historically oriented writers, for example, Barrington Moore, Marshall Sahlins, E.P. Thompson, Richard Sennett and Jonathan Cobb, Charles Tilly, William Sewell, Arthur Stinchchome, Stephan Vlastos, Michael Burawoy, Mark Granoveter, Pierre Bourdieu, Anthony Giddens, Raymond Boudon, and many, many others. More recently, it has played a central role in the provocative debate generated by Margaret Somers over the role of general theory in historical sociology[7] and re-articulated by a number of very recent writers.[8]

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[6] I argue in "Austrian Economics and Critical Realism" (2008), that the problem with neo-classical economics is not the absence of a social mechanism, but its failure to address reality: What is postluated in the mechanism is not true of the actors and their situation.

[7] See Manicas, *A Realist Philosophy*, Appendix C, "Rational Choice Theory and Historical Sociology."

[8] See especially, the collection of essays in Peter Hedstrom and Richard Swedberg (eds.) 1998; Doug McAdam, Sidney Tarrow, Charles Tilly, 2001, more recently, the special issue of *Philosophy of the Social Sciences*, vol. 34, no. 2 , 2004 and the panels of the American Political Science Association meeting of August 2003 with papers by Andrew Bennett, James Mahoney and John Gerring. (Online source: http://www.asu.edu/clas/polisci/cqrm/Bennett_APSA_2003.pdf). Replace "Mahoney" and "Gerring" for "Bennett" to access the other two papers.

THE STRUCTURE OF SOCIAL MECHANISMS

To construct a theory of a social mechanism, as regards the actors, four key questions need to be answered. Thus, (a) What do they want? We assume that actors act purposively, but they need be "rational" in the sense of recent rational choice theory. (b) What do they know? We need to distinguish discursive knowledge from practical knowledge. Actors always know more than they can articulate and know much less than they would like. (c)What can they do? This involves identifying their social relations, resources, their understanding of the norms and rules of their interaction, and other elements of what is usually termed "social strucuture."[9] (d) What are their attributes? This overlaps with (c) but might include demographic facts, their age or health. Our qualitative research has provided the pertinent data. Given this information, then, we explain the typical interactions of typical actors.

A sketch of the mechanism developed in Goffman's *Asylums* (New York: Anchor, 1961) may provide a useful example. Goffman identifies two antagonistic types of actors standing in a well-defined social relation: "the managers" and "the managed" ("professionals" vs. "clients," staff vs. inmates). By virtue of their place in these social relations, there are resources available which enable the construction of their identities and capacities, including having appropriate credentials and dress along with a number of specific capacities characteristic of the institution. Abstractly, the managed must be constructed as something less than a full person, while the manager is constructed as competent to "treat" the managed. Thus, "social distance is typically great and often formally prescribed" (7). Each of the two parties has goals (which "provide a key to meaning") and each has a system of beliefs (for the managers, an "interpretative scheme" which includes "a theory of human nature"). For each group, there are structured capacities for achieving their goals. Typically, "the managed" undergo "mortification" the construction of a different self, role dispossession: (14-15), "personal defacement" (20-21.), "contaminative exposure" (23), and a diminishing of the capacity of the managed to control action. A mechanism employed in stripping of power is "looping," where a disruptive response from an agent becomes the target of the next attack (35-36.). But the managed also have resources. Resistance by them takes on a number of forms, including contesting the meaning of rules, "fraternization," and "playing it cool" (61-65). "Institutional ceremonies," including, for example, a newsletter produced by inmates, an annual party, and an open house, are regular events in the life of the institution. These are intended to produce a joint commitment to the official goals, even if, to be sure, everyone "on the inside" knows better.

The social mechanism is well developed by Goffman who very convincingly shows how the beliefs of actors, true and false, promote behaviors which have as their

[9] A currently hotly contest argument in realist social science regards the ontology of social structure. For some discussion, see my *Realist Philosophy,* pp. 67-74.

outcome the reproduction of an institution in which there is a manifest disjunction between the "official goals" of the institution and the actual outcomes, and how actors unintentionally act in self-defeating ways that sustain the conditions of their own oppression. But many writers offer what I call "promissory notes" of mechanisms, hints or sketches of a mechanism, for example, of "urbanization," "racism," etc. Since explanation is a pragmatic notion, sometimes, such a sketch is all that is demanded.[10]

GENERALIZATION, ABDUCTION AND ASSESSING THEORIES OF SOCIAL MECHANISMS

Generalizations do not explain; they need explaining. But it easy enough to see also that generalizations[11] will be the point of departure for a theory of a mechanism. That is, where there is some pattern or regularity, there are but two possibilities: Either the regularity is the product of some mechanism or combination of mechanisms at work, or it is not. So compare, "A relatively small proportion of children from poor neighborhoods in the UK continue into higher education," and "For the past three months, as the prices of real estate in Honolulu went up, so too did the number of tourists." Both propositions might be true, but there is little reason to believe that there is mechanism at work which could explain the latter. The correlation seems wholly accidental, perhaps a statistical anomaly. Where the regularity seems not to be accidental, Lawson (1997) suggests the idea of a "demi-regularity," or "demi-reg" for short. He defines it as "a partial event regularity which prima facie indicates the occasional, but less than universal actualization of a mechanism or tendency, over a definite region of time-space" (204). "Demi-regs" prima facie suggest a mechanism exactly in the sense that based on what we know, the connection is not likely to be accidental. Of special interest, then, is what he calls "contrastive demi-regs." He gives a number of examples, some commonsensical, some not so obvious: "Women look after children more than men do." "Average unemployment rates in the western industrial societies are higher in the 1990s than the 1960s," "In the 1990s UK firms are externalizing or 'putting out' more parts of the production process than twenty years ago," 'Government persons tell more lies in war-time." As Weber rightly noted, there are countless numbers of these functioning both in ordinary life and in more sophisticated science. We can offer a number of important observations regarding such generalizations.

First, theory construction does not begin from nothing. Not only is it problem-driven, but the theorist has a stock of knowledge which will be the materials of the effort. Struck

[10] In *Realist Philosophy of Social Science,* I consider a large range of examples from writers normally thought of as doing macro-sociology, including here Marx. Barrington Moore, Tilly, Stinchcombe and many others.

[11] Generalizations include both universal and statistical assertions, "All Fs are Gs," "Most Fs and Gs" and patterns and regularities which can always be expressed in these forms.

then by what is an interesting (and well-established!) "demi-reg," inquiry into the possible mechanism or mechanisms begins.

Second, the demi-regs may well be the product of descriptive work, either quantitative or qualitative. Stephen Kemp and John Holmwood (2004) have argued that identifying unknown patterns is a particularly important task of statistical techniques. Thus, drawing on work by Stewart and his colleagues (Stewart et al 1980) they ask whether there is a mechanism which would explain the unclear relation between class background and type of school with the number of years a student pursued education. Kemp and Holmwood argue that regression techniques were effectively employed to show that the strongest pattern regarded class and high status schools, a pattern not discernible without the use of these methods. But, of course, on the present view, the inquiry could not stop here. It is unfortunate that so much solid descriptive work is so often mistakenly taken to be explanatory when it is not. Given the identified pattern, the problem now becomes what explains it. Indeed, some aspects of the pertinent mechanism would seem to be involved in some of our previous examples. The interested reader might well test her theoretical ingenuity.

Third, contrastive demi-regs force inquiry into looking for differences which point to the probable causally relevant features. "….[We] notice the effects of sets of structures through detecting relatively systematic differences in the outcomes of prima facie comparable types of activities (or perhaps similar outcomes of prima facie different activities in different space-time locations, or differences in types of position-related activities on comparable space-time locations, and so forth" (208-209). So, as is obvious enough, differences in domestic responsibilities between men and women suggests powerfully that we need to understand the mechanism which explains the existing division of labor. There are, no doubt, mechanisms of gender discrimination at work, but as with racism, these need to be spelled out and confirmed. Similarly, increased unemployment rates suggests differences in productivity or rates of profit which in turn suggests changes in mechanisms explaining productivity or the rate of profit.

The idea of "contrastive demi-regs" is at the bottom of considerations regarding comparison, long recognized to be a tool of macro-and historical sociology. That is, where outcomes are different, we seek differences in the causes. Comparative inquiry provides opportunities to identify the pertinent mechanisms at work in one case but not the other, or to identify pertinent differences in a similar mechanism which explains the differences in outcome.

Finally, as in natural science, the mode of inquiry here is neither deduction nor induction, but what C.S. Peirce called "abduction." Given a demi-reg, can we identify the causal mechanism which explains it? And if there are several plausible mechanisms, can we arrive at some valid, if still, fallible, conclusion? To be sure, a host of difficulties attend this, including, as already noted, the fact that experiment is generally not possible in the social sciences.[12] The absence of the possibility of a controlled experiment is an

[12] So-called "natural experiments" are not experiments in any useful sense. But there are what are sometimes termed "quasi-experiments." An excellent example is the descriptive longitudinal study,

important difference between the natural and social sciences, but it need not lead to the conclusion that a human science is quite impossible.

Two lines of argument may be noted. First, there is the sort of direct evidence produced by Goldstein, Brownstein, Ryan and Belluci (1977) to test their three different explanatory models of drug use. The psychopharmalogical model is the most straightforward: It offers that drug use causes temperamental changes in individuals which lead to violence. The economic compulsion model offers that craving drugs, persons feel compelled to engage in economic crimes to finance their drug use. Here there is a clear goal by users along with a judgment on effective available means. The systemic model "suggests that violence stems from the exigencies of working or doing business in an illicit market—a context in which the monetary stakes can be enormous but where the economic actors have no recourse to the legal system to resolve disputes" (116). The authors then offer some statistical data to test the models. Examining a sample of 414 homicides, they show that only 7.5 percent were caused by the effects of drugs; 2 percent were motivated by economic gain and 39.1percent were clearly the outcome of the systemic factors, violence between dealers or dealers and users. One might notice here that 47.5 percent of the sample (which reports only homicides) were not drug-related. There are, of course, also the mechanisms which produce and reproduce an ideology regarding drug use, an ideology which mystifies reality and which is promoted by perhaps even well-intentioned media.[13]

Second, among competing explanatory mechanisms, there are different consequences and these are testable. Hernes offers a wonderful example, the effort to explain why, as reported by Norwegian media, women more often than men are stung by wasps. It shows clearly how on realist grounds, a theories of generative social mechanisms might be tested.

He offers four possible explanations: (1) "The Rambo theory." "Women are a more tender species than men….For a real man it would be disgracefully effeminate to call a doctor for a dinky distress" (Hernes 1998: 76). The mechanism has it that men fail to report bites. (2) "The outdoors theory": "Women spend more time in the open air than men, walking their babies and playing with their children." This mechanism involves gender differences in roles. (3) The hysteria theory: Women panic when they see a wasp, agitating them to sting. Men do not panic. The mechanism here makes women the cause of increased biting. (4) The scent theory: Women use fragrances which "beguile wasps, but which then sting because they become aroused and then aggrieved when they discover that the bouquet stems not from flowers and react to frustration by aggression" (77). Critical to this mechanism are assumptions about wasp behavior.

"Lifetime Effects: The High/Scope Perry Preschool Study through Age 40." (See David Kirp, "Life *Way* After Head Start," *New York Times Magazine,* November 21, 2004. Unfortunately, we remain unclear as regards the social mechanisms which would explain the remarkable differences in patterned outcomes. It will be certainly be a complicated story.

[13] For some discussion of this see Morgan and Zimmer (1997), and Glassner (2000).

Each of these could explain the outcome. But which (if any) is true? Hernes points to standard methodology: Take the Rambo theory. If women are more tender, then they should be less tolerant of pain. But does any research that supports this? For each of these theories, we can test the truth of assumptions with evidence and argument. Very often this requires drawing out the implications of the assumptions, and accordingly, it requires a strenuous effort to see exactly what those assumptions are. Unfortunately, not only will this not be easy, but it is easy to fail to notice that assumptions which may be critical are being made.

Lawson (1997) also provides a wonderful example. He cites Leamer's account (1983) of the predicament of the applied econometrician:

> The applied econometrician is like a farmer who notices that the yield is somewhat higher under the trees where birds roost, and he uses this for evidence that bird droppings increase the yield. However, when he presents his findings…another farmer…objects that he used the same data but came up with the conclusion that moderate amounts of shade increase the yields….A bright chap…then observes that these two hypotheses are indistinguishable, given the available data (214).

Lawson answers:

> The obvious response of course, albeit one that econometricians occupied with fitting a line to given sets of data rarely contemplate, is to add the 'available data.' Specifically, the aim must be to draw consequences for, and seek out observations on, actual phenomena which allow the causal factor responsible to be identified. If, for example, bird droppings is a relevant causal factor then we could expect higher yields wherever birds roost. Perhaps there is a telegraph wire that crosses the field which is heavily populated with roosting birds, but which provides only negligible shade.…Perhaps too there is a plot of land somewhere close to the farm house which is shaded by a protruding roof, but which birds avoid because of a patrolling cat….The fact that it is not possible to state categorically at this abstract level the precise conditions under which substantive theories can be selected amongst, i.e., without knowing the contents of the theories themselves or the nature or context of the conditions upon which they bear, is an unfortunate fact of all science (214).

Lawson's more general conclusion deserves quoting: "Science is a messy business. It requires an abundance of ingenuity, as well as patience, along with skills that may need to be developed on the job."

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