The Maturity of Social Theory

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Social theory is ordinarily not thought of as an autonomous academic field, a genuine and complete academic identity, or as an appropriate, or feasible, academic career choice, or as much more than an amorphous publishing category for books that are non-empirical, not strongly identified with a discipline, which make some “social” reference. Sociologists typically think of it as the theoretical side of the discipline of sociology or as a sub-field of sociology, thus a secondary identity that is submerged in, and subordinate to, the primary identity of “sociologist.” Non-sociologists think of it as a kind of supradisciplinary collection of themes traditionally associated with sociology or with left wing politics, and now associated with cultural studies and feminism. In view of this, the claim I wish to make is a somewhat startling claim. Social theory is not only a field but a mature one; one that is essentially complete and self-sufficient as a coherent and valuable form of intellectual activity: a voice in the conversation of mankind, with its own internal conversation of considerable complexity and depth.

What I mean by mature field I do not propose to specify directly at the outset, though what I have in mind will become evident in what follows. It will suffice for now to think of a field in social terms, as a body of persons with not only common intellectual concerns, but a common set of communicative ties. A discipline is a field or set of fields that is also organized as a closed labor market for Ph.D.s with a single disciplinary label (Turner 2000). What I mean by maturity will also become clearer in what follows, but in the case of social theory it can be most easily seen by a comparison with another mature field that it is increasingly closely related to, and perhaps decreasingly distinguishable from, social theory: political theory. Political theory is mature in the following way. Its basic themes and methods are already essentially established, and the main activity appropriate to it is the reinterpretation and application of these themes to new problems and the gleaning of past texts for relevant insights. The reasons for the maturity of political theory are not “internal,” that is to say, not the product of some intellectual development that defined it. Unlike the chemistry of the periodic table, for example, or anatomy, it is not complete and a matter of so much consensus that there is nothing left to argue about. Rather, the themes and methods are so closely woven into the actual activities of which it is an account that there is no reasonable prospect of a genuine novelty, such as a paradigm shift, without a corresponding and correspondingly radical shift in the activity of politics itself. In the case of political theory, liberalism and anti-liberalism, both as ideas and political traditions, are continuations of discussions and social forms of discussion which are found in the eighteenth
In the case of social theory, I shall argue, the situation is similar. As with political theory, the striking emergence of various new movements and critiques of the existing order has not resulted in any particularly novel ideas. Rather, the same basic conceptual equipment available in the era of Weber and Durkheim is simply redeployed, with suitable modifications, in the service of new topics. It must be said that this thesis may seem offensive to thinkers who believe themselves to be radically innovating or shifting paradigms by their theoretical work. In any case, it is hardly the kind of argument that may be made with great analytic precision. Nevertheless, I think it is an argument worth making, precisely because it is at odds with some commonplace ideas about the status and prospects of disciplines, particularly the discipline of sociology. In part what I will have to say amounts to the continuation of a dialogue begun by Donald Levine on the potential intellectual role of the actually existing discipline of sociology (1995), a topic I will return to in the conclusion.

Originality vs. Maturity in Social Theory

One of the reasons that any argument about originality cannot be formulated with any great historical or analytic precision is that it involves a number of relative judgements about the innovativeness of particular claims and the degree to which the claims differ from previous versions of the same general ideas. To assess Habermas, for example, one must question whether Habermas's contributions are essentially ways of formulating and tidying-up the classical tradition or a means of supplanting the classical tradition. These are assessments which require one to say whether or not the alterations that Habermas suggests are genuinely significant. In a very simple and crude sense the problem comes down to this. Habermas's most striking innovation, the one on which claims of genuine and radical originality must rest, is in the idea of distorted communication and in a related idea that immanent in every communicative act is the purpose of undistorted communication, a purpose that is imperfectly realized under present social conditions.

Is this an innovation or not? Of course it is. But it is an innovation in a fully developed tradition, and not, so to speak, a paradigm shift or a discourse-founding event. Obviously the whole notion of untrammelled discussion is central to traditional liberalism. It is, after all, the central theme of such famous texts as Mill’s *On Liberty*, an argument that was well familiar to classical social theorists. Weber dismissed it with the withering remark that he did not accept the metaphysics that believed that the truth, the one right view, arose out of the free competition of ideas ([1914] 1949, p.18; cf. Marianne Weber 1975, p. 325). Schmitt took it a step closer to Habermas by arguing that one of the conditions of parliamentary democracy was that representatives in parliament had to be willing to be persuaded, a condition he believed no longer obtained in the Weimar Republic’s Parliament (Schmitt 1985). In a sense, Habermas's argument simply generalizes Mill’s by taking into account limitations on discussion that arise as a result of not simply the use of state power to suppress speech but of “social” strictures against forms of expression that implicitly select against and filter the kinds of claims, that if heard or formulated, could not be publicly entertained.

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1 Bronner’s recent survey (1999) of contemporary ideology is a good gauge of this. The novel ideological themes of the end of the last century turn out to be themes of race or culture and gender that are articulated in argumentative forms that writers of the German anti-enlightenment such as Hamann and Herder pioneered.
would move discussion in a different direction. In another sense it utopianizes Schmitt, by arguing that the standard of free discussion ought not only to be the possibility of persuasion, but the guarantee of the social conditions required so that the better argument always does persuade.

The cogency of Habermas’s claims need not be assessed here, other than to make the following point: this is an argument that reveals an enduring tension in which social theory and the problem of truth meet; one in which different social theoretical claims imply different approaches to the notion of truth, especially with respect to the question of what is a candidate for “truth” in the first place— a question which arises in social theory in the form of the fact-value distinction, the theory of ideology, and today in such forms as feminist standpoint theory. The classical liberal response is to say that the selection processes that distort communication, such as the fact-value distinction, are themselves matters of opinion. The classical liberal solution, Mill’s for example, would be to have these matters of opinion argued out “freely” as a means of enabling, though not assuring, advance. It is questionable whether Habermas adds anything to this other than the notion that we are trapped into communicative distortions that cannot be eliminated without a special kind of questioning that may be interdicted or excluded because it is not available within the worldview that underlies the customary modes of the public speech of the community. This argument is a bit of historicism that was not only available to Weber but was endorsed by him, and perhaps informed his skepticism about the virtues of tolerance. Stated in this way, Habermas's great achievement is no more than the sort of achievement one would expect of the practitioner of a mature field. The conceptual furniture is not changed but merely rearranged in a different set of adjacencies and conjunctions, and a concept that figures in a minimal form in a past thinker is formulated in a maximized form.

One could go through virtually all of the major ideas of present theorists and apply the same sort of analysis. The fascination of Anthony Giddens with "detraditionalization" and the supposed postmodernist situation of the equal validity of all truths is a restatement of the fin de siècle notions that Weber reproduces in "Science As Vocation" in which his contemporaries were celebrated as the generation which had eaten of the Tree of Knowledge, and thus knew that there was no rational basis for choosing between worldviews and that the multiplicity of options was ineradicable. Ideas about the mind's slavery to cultural forms which are at the basis of present

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2 One might unfold the argument of this paper in the form of an analysis of this speech and its readers and users, since it is an especially clear example of the independence of social theory from the discipline of sociology. The excellent collection produced by Lassman and Velody (1986) on the reactions to Weber’s “Science as a Vocation,” shows how the speech was first responded to: the reaction includes a great deal of post-modernism avant la lettre, all produced in the Weimar period. We now know, from Safranski, that Heidegger took this speech very seriously as a target for his own thinking (Safranski 1998), as did his ally Scheler (cf. Turner and Factor 1984). The continuity of social theory as a larger and more encompassing form of thinking, independent of disciplines can be seen in relation to this text in the thought of Karl Jaspers, and subsequently of his student Hannah Arendt. Of course, American sociologists later appropriated Weber’s talk as a defense of a particular mode of sanitized disciplinary speech related to the project of a scientific sociology. It is worth noting at least in passing that this appropriation was left to the second-raters. This was a distortion and diminution of the speech. But there was a more interesting distortion. Robert Merton drew extensively on the speech in his own famous paper on science and democracy, in which he identified the “norms of science.” Characteristically, Merton claimed that these norms have been derived by him from an empirical
day cultural studies, and the work of writers like Foucault, are certainly there to be found in Durkheim's *The Elementary Forms of Religious Life* and in his essay with Mauss on classification. The constitutive character of culture is simply not a new idea, though the applications of this idea to such topics as the construction of gender may well be novel. But novel applications of classical mechanics are not novel mechanical theories. What social theory possesses that gives it its strong coherence is a certain more or less stable tension between alternative strategies, none of which has yet been able to crowd out the alternatives. The curious feature here is not that there are alternatives, for if they weren't seen to be more or less about the same thing they would not be seen as alternatives at all, or part of the same discipline, but that the alternatives are so astonishingly stable. Classical sociology was, in part at least, an attempt to respond to the model of the rational intentional agent or “economic man.” The thing that sociologists had as a topic to theorize about, which seemed then to be out of the reach of the economic conception of man was, as Parsons (as well as Adam Smith, Spencer, Sumner, and others) quite correctly saw, the idea that people had obligations, were governed by norms, had *mores*, values, and so forth. The present successes of rational choice approaches, however one may choose to evaluate them, do not really change the situation of tension between the two approaches, the normative and the economic. The ways in which rational choice approaches today “explain” norms is itself fundamentally problematic, and the problems will not be resolved by methodological arguments. The ways in which normativity has been conceptualized are full of troubles too: the problem of the reality of collective objects, whether these be states, societies, or practices, will not go away either. And as we think through the ramifications of these problems we are characteristically driven back to the standard formulations and modes of reasoning of established social theory, whether we are historically conscious of or sensitive to them or not.

Examination of scientists’ writings, though, apart from a few references, he never said where the evidence was to be found. In fact, the paper, a basic text in the sociology of science, is essentially a disguised work of commentary on Weber’s text. There is nothing wrong with this, indeed I will argue that all sound social theory has the character of community. What is wrong is Merton’s failure to acknowledge this, and his pretense that the essay is “empirical sociology.” It is very striking that within a few years Merton wrote an elaborate and influential denunciation of historians of social thought, “History and Systematics of Sociological Theory” (1945), which valorized “systematics.” Two texts may be pointed to here: Warren Schmaus’s discussion of contemporary constructionism in science studies in terms of Durkheim (1994) and Mark Cladis (1999). Jeffrey Alexander, in his paper on the centrality of the classics, argues that this repeated recourse to classical thinkers and their themes is essentially a discursive convenience, that is to say, the classic thinkers serve as common markers in terms of which one can identify positions and formulate present arguments (1996). Discursive conveniences come and go, and thus Alexander does not give an argument to the effect that the classics will always be central. As far as it goes, then, Alexander’s argument is congruent with mine. Many canons might do very nicely as the canon. I strongly suspect that social theory could get by just as well with Habermas, Bourdieu, Giddens, and Coleman, perhaps with an interpretivist or constructivist thrown in, for the simple reason that their writings will contain the main themes, major argumentative forms, and main tensions that appear in the canon. So I do not want to say that the classics, either in the sense of the present canonical figures in social theory or the major themes which have hitherto
I suppose that, upon examination, this claim really is no more than the claim made earlier to the effect that works of apparent originality in the present typically do little more than rearrange and extend elements of past social theory. If I put the non-originality argument in terms of the notion of a discipline, it would perhaps be better to say that innovations are discipline preserving rather than discipline-disrupting or discipline-overthrowing in character. What makes Foucault, for example, a social theorist is not that he discovers a new category, discourse, which systematically operates in such a way as to serve and constitute a certain regime of domination, but that “discourse” replaces Marx’s “relations of men to men” as the medium of this domination-- making it an alternative solution to more or less the same problems.

One kind of non-disciplinarity would be this: the world would have Marxists, Critical Theorists, Foucauldians, Weberians, Lacanians, and so forth, but each of these groupings would be sects-- as the followers of Norbert Elias sometimes seem in fact to be-- in which mutual criticism and continued regard for the issues raised by the other side would cease to be important. In fact, of course, this is not what happens in social theory. Habermas, for example, devoted his *Theory of Communicative Action* to a careful delineation of the meaning of various “classic” texts and a dialectical examination of them with an eye to appropriating them for his own project. Foucault criticized and examined liberalism. And the Habermasians and Foucauldians have continued to carry out the same critical dialogue. The dialogue is oriented not merely to the application of the thought of the master, but to the tensions that informed their thought. The problem of the public sphere, the problem that Mill, Schmitt, and Habermas came down on different sides of, remains for them an open problem.  

One reason for this has to do with the relation between present texts in social theory and the “classic” texts that they are designed to extend, supplant, and criticize. The point of the texts of Marx, for example, is never quite separable from the critiques of Hegel and of classical economics that is contained within them, and similarly for Durkheim and Rousseau, Weber and Nietzsche, and indeed Liberalism and classical natural right, as the works of Leo Strauss make clear. To understand the more modern texts is to understand the reflections on the earlier texts that they contain. The relationship between two great texts in social theory is never reducible to “unit ideas” that can be stripped from the one text and merely employed in the other. And the

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been distilled from them are simply discursive conveniences. And I wish to raise, in a more elaborate way than Alexander does, the question of who these discursive conveniences are convenient for-- social theory as an autonomous field, or the real existing discipline of sociology. I do not think that any canon can serve the discursive purposes of social theory nor that the conditions of calling something a discipline are simply that it has a canon with suitable tensions between its elements. What I wish to argue instead is that any future canon will more or less articulate the same general themes and tensions of previous canons.

5 The work of James Bohman (1997) may be cited in this regard as a model, and it may be noted that there is a large continuing series of programs, collective works, meetings, and mutually reinforcing scholarship-- involving scholars from several disciplines-- on the public sphere as a theoretical problem.

6 Althusser made this point in *Reading Capital*; his program in that text, in which he “reads Marx according to the rules of a reading in which he gave us a brilliant lesson in his own reading of classical political economy” (1971, p. 30) is a self-conscious example of social theory as commentary, which recognizes its own character, as a rethinking of a previous rethinking in which the hidden presences and the unnoticed gaps of the previous texts are made manifest.
complexity and holism of texts, the need to consider the whole of an argument rather than the concepts that can be separated from it, is a sign of what is different about social theory as a discipline when it is compared to other disciplines, such as sociology.

What I have given here is no more than a broad hint by what I mean by saying that social theory is a mature field: that it has enduring tensions, that there is a set of texts that constitute a common point of reference and whose mastery is an entry ticket to the understanding of the field, and that there is a certain familiarity or permanence to these texts and tensions, so that “originality” typically amounts to a re-use or extension of something that is already there. This is a model that is familiar enough from the humanities: the modern discipline of philosophy, for example, may be thought of as an extended thinking through of the problem of subject and object in the light of the difficulties in its two main resolutions in Hume and Kant. But before elaborating a portrayal of social theory, it is important to deal with the problem of the dead hand of the discipline of sociology, and particularly of burden of the relationship of condominium that the various parts of “sociology” have with one another.

The Mutual Irrelevance of Empirical Sociology and Social Theory

The dead hand of sociology still exerts a very substantial influence on the way in which the past of social theory has been understood, and I would be remiss if I failed to point out some of the implications of this history and for this history of the argument presented here. There are two sides to this problem, one of which is organizational, the other intellectual. The hundred years of disciplinary sociology are the product, in my view, of a serious intellectual task performed under the pernicious influence of a defective ideal, namely the ideal of a science of society. Those of us who devoted our early careers to overcoming this fatuous ideal may take some satisfaction in the fact that no one today, apart from a few amateurs and a few other persons with extremely idiosyncratic views of “science,” takes it seriously. But there is little satisfaction in exposing an error, especially an error which, as the discussion of it reveals, was essentially incoherent.

The ideal has persisted nevertheless, in a much weakened form, as a way of understanding and legitimating sociology as a disciplinary arrangement. Empirical sociology is able to claim that it is more than just useful statistics such as market research provides because it employs a vocabulary of hypothesis testing and makes an occasional gesture to past social theorists. Social theorists get employment in departments of sociology for contributing to this image. Sociology also includes a vast body of teaching and research that deals with issues relating to matters of social welfare, racism, sexism, equality, crime, and so forth. Much of it is bureaucratic in character, political in motivation, and relentlessly local in scope. This body of activity is an important part of the economic machine of sociology, for it is these issues that have been the traditional means of attracting students. Some of it has content that relates to social theory, so the connection between sociology in this sense and social theory is not entirely empty.

But the connection is very weak. It is a simple fact that the people who do social theory do not read standard “empirical” sociology, refer to it in their writings, or think that it is relevant to what they have to say. It is similarly true that people who do “empirical sociology,” by which I mean, essentially, causal modeling, do not concern themselves with social theory. Theory, Culture and Society and The American Sociological Review are journals that, for all practical purposes, are not only in different disciplines, but in disciplines that are more widely separated than, say, sociology and economics. The standard bibliometric methods, if they were to be
applied to the question of distance between literatures, would bear this out.  

Empirical sociologists sometimes have something to say about the relation between actually existing theory and actually existing empirical sociology, as do theorists who are employed in sociology departments and are faced with the usual questions about their role. Much of what is said along these lines recapitulates things that were said in the period shortly after 1945. I will turn to this anomalous moment, and its leading figures, shortly. But first it is necessary to take a dry-eyed look at some technical matters in causal modeling—necessary because this enables us to see that the post-1945 “Great Instauration,” as I will style it, was based on a misunderstanding of the relation between statistical techniques and “theory.” The argument I will make here is not original, nor is it particularly controversial, though it has been slow to be absorbed into the literature of sociology. It has been stated most succinctly by the philosopher of science Clark Glymour, in an article called “Social Science and Social Physics” (1983) but elaborated in great detail in other texts. (Cf. Glymour, Scheines, Sprites, and Kelly 1987; Glymour 1997; Spirtes, Glymour, and Scheines 1993.)

The simplest way into this problem is perhaps to consider the very interesting body of work developed by Glymour and his associates, as well as by others, such as computer scientist Judea Pearl (2000). These authors have attempted to formalize, through the creation of computerized artificial intelligence procedures, the reasoning of conventional multiple regression causal analysis in the social sciences, and particularly the problem of selecting a causal model on the basis of correlational data. Their basic idea is this. If you have a set of causal and outcome variables that you are interested in, these variables are correlated with other variables, perhaps hundreds of other variables. There is a large but not infinite class of possible models, meaning directional arrow graphs (DAGs) representing causal relations (such as path diagrams), that could fit this data. The empirical problem of causal analysis is to use the data to select the causal models from this class.

One can think of this class in the first instance as the class of all possible models that could fit a given set of correlations at a given selected level of significance. This class would include many models in which the arrows went in ways that were absurd, such as arrows from a person’s income to their grandfather’s ethnicity. On no known interpretation of the correlation between these two things has it ever been supposed that there was any ambiguity about the direction of this relationship. However much money you might make, your grandfather will still have been Polish or not Polish, for example. But we eliminate all the models from the class of possible models that involve arrows going in absurd directions, we can substantially, at least in many cases, reduce the size of the class.

If we had a way of telling, on the basis of the data together with the very limited and unproblematic background knowledge about causation we have already employed, which relationships were produced by latent causal variables and were therefore spurious, we could eliminate a great many more models. We could also generate new models outside of the original class we defined, which employed those latent variables. Glymour provides such a technique. The technique of detecting latent variables involves determining where there are correlations that ought not to be there if the model were true. The basis of the technique involves the basic reasoning behind partialling, the idea that a relationship that should operate entirely through

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7 The same methods have already been applied to show something less obvious, and quite startling: that sociology as a discipline in the bibliometric sense—a coherent and identifiable group of co-citation clusters—effectively disappeared in the eighties (Crane and Small 1992).
another variable should be fully screened-off by that variable, so that there should be no excess correlation after the relationship is partialled for the middle variable. The method works: one should get down to a very small set of models that are not ruled out by this set of selection procedures. These are the possible true models. One won't necessarily be in a position to make any sense of these results, which is to say to identify the latent variable that produces a relationship. But one has a result that depends entirely on the data, together with a small amount of uncontroversial background knowledge.

As I have suggested this method is designed to mimic and thus in a sense explicate the assumptions of standard methods of analysis in the social sciences. So it is to be expected that the results using these methods would not always be the same, but might frequently be the same, as those already in the social research literature. The difference is that these methods exhaust the possibilities, while the older strategy was to find a single intelligible model that fit and that illuminated the material. One would expect, then, that the older models would be among those remaining after the selection procedure produced the set of possibly true models. Yet when some of the classics of quantitative sociology have been re-analyzed, the results have been startling. In re-analyzing Blau and Duncan's classic *American Occupational Structure*, for example, Glymour and his associates found that every single relationship in the model was spurious (1997a, p. 223). This is a striking result if only because it suggests that if Blau and Duncan managed to get the relationships completely wrong their inferiors almost certainly got their share of relationships wrong. If that is true, then a great many of the articles to be found in standard sociology journals employing these methods are wrong as well, and indeed they go on to say, of Duncan, Featherman and Duncan's classic status attainment paper (1972) that “This data set and its analysis have served as a model for quantitative sociology: unfortunately, I think that the only reasonable conclusion is that it tells us almost nothing about which of these variables influences others, and how much (1997a, p. 223). As Glymour puts it, that this is a result that is obviously highly unwelcome (1997a, p. 246). What it suggests is that the vast effort that quantitative sociologists have expended in producing their works has been in vain, at least as far as producing agreed and definitive results is concerned, and this is after all the point of the attempt to make sociology a cumulative science and the justification of its use of the language of hypothesis testing.

The point I want to make about these methods and their relation to theory however is a little different. As I have suggested, what causal modelers actually do (and Glymour is no exception to this) is to employ relatively uncontroversial background knowledge to eliminate most of the cases and use the data to eliminate the rest. Whether they do so consciously or through an explicit procedure, as Glymour does, or more mystically, as Blau and Duncan obviously did, they are in the same business. They accept the plausible relationships that they find to be supported by the data and then contrive some sort of "theoretical" account of the phenomena that makes sense of these relationships. It is this last step of “making sense” that is especially important to understand if one is to understand the relationship between social theory and these methods.

The causal modeler arrives, in the end, at a DAG, a directional arrow graph, that is not possible to rule out on common sense grounds, because, for example, the temporal order is wrong for the causal mechanisms that are plausible, as in the case of grandparents’ ethnicity being caused by annual income. The rhetoric of hypothesis testing and “theory” applied to this activity is appropriate in one sense: the researcher does not know in advance which of the causal
relations will survive the analysis and thus what the causal structure will look like. But characterizing these “structures,” which essentially consist of sets of arrows indicating the relationships consistent with the data, as “theory” in the same sense as “social theory” (or for that matter in the same sense as physical theory, as Glymour points out) is not appropriate. Causal models depend on a kind of local common sense knowledge. The relationships in question are causal, but they are not causal in the sense of laws, and especially not in the sense of universal principles. Indeed, one of their key features is that the boundaries of their application are not known, and cannot be inferred from more general principles. Suppose we have, for example, a relationship between having a Polish grandfather and some other variable, such as net worth. We may very well discover that this relationship survives the empirical selection process that eliminates other relationships and even that there is no latent variable that produces the association. Then what? We have “tested an hypothesis” and found that it stands up to a test. But do we know why the relationship holds, and why it holds where it does? We do not know any of this based on the successful “test” of the hypothesis. Moreover, as is inevitable in this kind of relationship, it is a very good guess that there are populations for which it does not hold. It is a “local” relationship, in this sense: we do not say “then it is not really causal” if indeed it does fail to hold in different settings. Our rule for using this causal relationship is “it applies where it applies” and not “it applies everywhere.”

Glymour’s point about the kind of causality found in causal modeling is that there is no getting behind it, no further explanation of why it applies where it applies, and thus no way to turn these kinds of causes into laws or theories in the usual scientific sense of the term (Glymour 1983). But he suggests that it amounts nevertheless to a perfectly good form of knowledge and one that social scientists ought to be happy with. Suppose we accept this? We still have the following question: what does this activity of modeling have to do with social “theory”? To make the point in an abstract way, the relationship is this: the facts that concern casual modelers are, to the extent that they are facts, trivially true for the social theorist, and for he most part the reverse is also true. The two are, consequently, for the most part, mutually irrelevant: theory typically doesn’t supply hypotheses for empirical sociologists to test, and virtually no result of an exercise in causal modeling has any impact on, or significance for, social theory. There is perhaps a small zone of mutual relevance, which I will consider later. But for the most part, theorists and conventional empirical sociologists are right to ignore one another, for they have

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8Just what is “tested” when a hypothesis about a causal model is tested? The background knowledge is not tested, in any sense: nothing in the way of results would cause the modeler to decide that, for example, present gender might indeed cause Polish grandfathers. The hypothesis that x predicts y is tested. But the test is simply a determination that the relationship is predictive in this particular setting. The “theory” represented in the form of the DAG is “tested” only in the sense that we see whether it applies in this setting. The setting is “local” in the following sense: there is no fact or theory that is part of the hypothesis testing process described here that enables us to say where the model applies and why it applies where it applies. This is simply an unanswerable question, within the framework of causal modeling. And the framework of causal modeling, and its variants, are a more or less self-contained world, a universe of methodological notions that defines problems, defines solutions, defines data as meaningful within the universe, and produces empirical results. It just happens that within this universe there is no way of answering the question of why a model applies where it applies and why it fails to apply (i.e. to predict) where it fails to apply.
almost nothing to say to each other.

Prediction vs. Application Of Concepts

Consider the argument in *The Protestant Ethic* as a paradigmatic bit of “theory.” Weber says that at one particular moment in history, a new psychological sanction emerged, associated with a novel theology of “callings,” that led indirectly to the creation of certain forms of rationalization in the sphere of work and economic life. Now-- is this claim true? If we consider the narrow issue of whether there were any people to whom this analysis applies, we have a historical, interpretive question, for which we can supply a certain amount of evidence, such as diaries, sermons, and biographical information about the conduct and beliefs of people. This kind of evidence helps us with the question of whether there were any people who fit Weber’s model. Weber’s thesis about the rationalization of work and economic life presents some different problems, but he is careful, in his lectures on economic history, to supply a larger narrative argument to the effect that the emergence of modern capitalism depended on a series of contingencies, of which the specifically religious development, the novel psychological sanction, was only one. Obviously there are alternative accounts of the emergence of capitalism that might fit with this, or might conflict. But neither Weber’s, nor most of the alternative accounts, can be adequately represented as a DAG.⁹ So any dispute between these accounts and between these accounts and DAGs cannot be settled in terms of the considerations that establish DAGs, that is to say the process of selecting empirically between alternative DAGs. If the dispute can be settled at all, it must be settled on “theoretical” grounds-- in terms of which is the better account in general.

Can Weber’s characterization of early Protestants be tested? Of course it can, in the formal sense that we can decide whether there is anyone to whom the ideal-typical description, which associates a unique theology to a unique psychological state, applies. But if there is anyone, the results of empirical sociology with respect to these people will be trivially true: if the same people who affirm the theology exhibit the effects of the psychological sanction, and both are unique to the relevant population, we can predict the one from the other. But this is only part, and a small part, of what we mean by saying that the ideal-typical description applies. So the fact about prediction is already there as a trivial consequence of what we are affirming when we say that the description applies: “testing the hypothesis” that we can predict one from the other adds nothing.¹⁰

What a DAG does is to predict, but it predicts only within a local domain. But this is nevertheless something important to do, and understandable as an activity. Theory also predicts,

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⁹ An attempt to do so is found in Collins (1986). The problem with this attempt is that Weber did not conceive the causal links in this way (cf. Turner 1985, 1999). Another is that the conditions--such as rational accounting--are logical parts of the thing to be explained--parts of the definition of rational capitalism--thus conceptual rather than causal conditions.

¹⁰ To be sure, social theorists sometimes begin with statistical facts, as in the opening chapter of *The Protestant Ethic*. But, contrary to the claims of various misinterpreters (e.g. Hamilton 1996), Weber never says that these represent the present causal situation, or are a test of his hypothesis, and, as the text makes clear, they could not be, for the relevant events were in the past. The statistical facts are thus no more than the continued effects of a historical event that is now long past.
but does so trivially. Its real job is different, but not entirely unrelated. “Theory” poses and answers questions of a different kind. What are these questions? To put it simply, they are questions about whether, and how, past social concepts continue to apply, and about the implications of their failure to apply. Does “the family” mean today what it once meant, and what follows from this? Does the category “woman” mean now what it meant in the past? Does “representation,” or “democracy,” or “citizenship” mean today what it meant in the past? Did such concepts ever mean, that is to say apply, in the way that past theorists thought they did? What concepts do we need to understand the changes? This kind of problem often takes the different form of extending concepts-- often the very same concepts, such as democracy or representation-- to new domains of application, and asking why they do or do not apply. Social theory may do more than this, of course, but it at least does this: it is conscious, and reflexive, about the problem of the application of concepts in new circumstances, and makes the problem part of the topic of theory.

Of course, not only does casual modeling not answer questions about the application of concepts, the very forms of reasoning on which it depends, the background knowledge about the relationships that enables us to treat them as possibly causal, is itself often conceptualized in terms of the very concepts whose changing application is the core subject of the social theorists’ reflections, and the conditions for the application of a DAG are in large part a matter of whether the concepts apply. From one point of view-- that of empirical sociology-- this is trivial background knowledge. Everyone knows what a woman, or a Polish grandfather, is, and also knows something very uncontroversial about the possible causal relations into which these concepts can enter. Someone’s being a woman, for example, cannot cause their having a Polish grandfather. So any correlations between the two facts cannot be represented by a directional arrow from gender to Polish grandfather possession. But it is not trivial knowledge when the relationships break down and the DAG ceases to predict. Causal modeling never does, and never can, break out of its dependence on background knowledge, however much it attempts to minimize this dependence or limit it to the most innocuous kind of background knowledge. So it is dependent on the very concepts that are the subject matter of social theory. But the questions of social theory are largely independent of the “facts” of causal modeling.

Can causal modeling or “empirical sociology” generally contribute anything to the answering of questions of social theory? Do these activities tell us anything about how and why concepts change in the minds of historical persons or change in their application? To be sure, there might be some modest overlap between these kinds of questions. Idealizations of patterns of understood action may be applied to aggregated bodies of action, such as political choice, just as rational-choice idealizations in economics are tested for their predictive application to actual markets situations. But what is tested is as much the idealization as the explanation. Changes in the application of concepts of ethnicity might show up in the form of changes in correlations, or changes in the causal system producing answers to questions about ethnic self-identification. But this is a weak relation of mutual dependence. More importantly, there is nothing about this relation that is special: the same relation holds, for example, between social memory and the evidence of mentalities and the possession of concepts that can be found in novels and memoirs. The memoirs of Casanova, for example, are excellent sources for an understanding of a wide range of clientelistic European societies of the eighteenth century: what “methodological” reason is there for discounting it? Changes in belief occur for particular people in particular times and places, something that might be illuminated by the kind of data used by empirical sociologists.
But for the most part this data is simply too crude to have any bearing on the topics of concern to theory, other than perhaps by providing an preliminary orientation to a problem.

Nevertheless, a great deal actually occurs in this zone of overlap, motivated in part by the enforced condominium of theorists and sociologists within departments of sociology, in part by the remains of the idea of a scientific, empirical sociology, particularly of the model of sociology championed in the post war era by Merton and Parsons, and in part by a disciplinary politics that encourages theorists to justify themselves as empirical sociologists. On examination, however, much of this activity proves to be either bad theory or not theory, or to involve evidence that bears only very slightly on the theoretical claims in question. Bourdieu, for example, makes claims about “practices” based on modest empirical correlations, but it is evident that facts involving the concept of practices, which involves occult collective entities, could not possibly be established by any evidence of this sort. Skocpol has various historical theses. But it is very doubtful that the kind of “empirical” evidence she presents for her theoretical interpretation could possibly support it (cf. Liebersohn 1988) or support it against other plausible interpretations of the same material. Erik Olin Wright is both a Marxist theoretician and an assiduous empirical sociologist about class. But, as with Skocpol, it seems that he is unconcerned with the theorists’ question of whether there are better and different accounts of the same material. Both are concerned to solve problems that arise, and are meaningful, only if one assumes a generally Marxist account of the nature of social reality. In this respect they are not theorists, for they are not in a conversation with other theorists who do not share these premises. In another respect they are: both attempt to extend and modify concepts which no longer apply in their original senses.

A better model for understanding the relations between the two enterprises might be this: Marxism predicted, and its predictions failed. This was perhaps in part because the theory was false, or because it was incomplete, failing to consider the effects of technology on productivity, for example, but it was also a failure because the concepts it employed ceased to apply. Empirical sociology is based on the presupposition that these questions can be separated, at least temporarily, over some span of time and place, so that the data can decide the predictive questions. “Theory” treats the application of concepts as an open issue, and thus denies that any final separation between the three kinds of question is possible. But theorists need not deny that a set of concepts may apply temporarily in a stable way and that in the moment of stability predictions of the sort that empirical sociological data can test may be made. Nevertheless, whatever the results of this kind of “testing,” it has little to do with the larger questions of social theory. The “data” for social theory is not restricted to the sorts of data that empirical sociology valorizes, and theory does not rest on it. The great piece of data for nineteenth century social theory was the French revolution and urbanization. “Theorizing” these topics was a matter that statistics could, and did, contribute little to. The real puzzle is why anyone thought that statistics should have some sort of special relevance, and it is to this puzzle that I now turn.

The Great Instauration of 1945

Much has been written about the emergence of mainstream sociology and the methodological rationale for the use of statistics as the basis of a scientific sociology, and I will not add to this literature here. But it is important to understand something about this history in order to understand the dead hand of sociology on social theory. Before a period of compromise between
theory and empirical sociology that began roughly in 1945, in such texts as Parsons’ “The Present Position and Prospects of Systematic Theory in Sociology,” there was a serious conflict within sociology between those people who wished to see sociology become a science, and who thought that this meant becoming a statistical field exclusively and those who thought that the history of social thought was an important part of the field with a continuing significance for it. The date of 1945 is a convenient point to break this history, because the year saw, on the side of those who wished to preserve the history of social thought as a field, a startling admission of defeat—Charles Ellwood’s bitter “valedictory”—and also an absurdly triumphalist prediction of a great future for systematic social theory by Parsons. Merton’s essay on the “History and Systematics of Sociological Theory” (1945) which began life as a comment at an ASA meeting, was delivered soon after. Bacon, in The Great Instauration, urged book-burning as a condition for the emergence of the New Science. Merton quoted Whitehead to the effect that a science that hesitates to forget its founders is lost. To understand what the struggle of the post war era in sociology was about, it is important to understand what books were burned, and what founders were forgotten.

In spite of the fact that Parsons and Merton were themselves historically sophisticated, and indeed knew very well that one would learn more from reading a neglected passage in Simmel than from another social survey conducted by the Bureau of Applied Social Research, they nevertheless pretended that this was not so and proclaimed not only their own connection to the activity of empirical sociology but their ultimate dependence and obeisance toward it. The distortions arose because the historical figures they publically commended were figures who could either supply hypotheses or contribute to “scientific” social theory, and they commended them as a source of hypotheses or as a source of material with which to build systematic theory. They disdained and ridiculed those of their contemporaries and near predecessors who considered the appreciation of the insights of past scholars to be valuable in itself, or valuable as a mirror of the present, as a means of recognizing the nature of the inferences that we today tacitly make.\footnote{Donald Levine provides an excellent characterization of this process in the case of Simmel whom Parsons disavowed and inoculated his students against (2000, p. 68).}

What was lost within professional sociology was a connection to the kind of social theory that thinkers like Dewey, in such texts as Human Nature and Conduct (1922) and Problems of Man (1946), formulated. One can see the loss in the writings of Parsons himself. In “The Place of Ultimate Values in Sociological Analysis” of 1935, Parsons directly engaged the pragmatists and their conception of human values, a conception that appears in the texts by Dewey and in C. L. Stevenson’s classic paper on emotivism. When “the sociology begins” for Parsons, however, the dialogue stops: his conception of values is made into the constitutive factual domain of the new discipline of sociology. Sociology leaves the larger conversation about values, except to the extent that it is a conversation about “science.”

The losses were profound. Even the early scientizers in American sociology, such as Giddings, had something serious to say about such issues as the nature of progress (1918) and imperialism (1900), which stand up to scrutiny today, and show, in the manner made familiar to us by Foucault, that the “problems” that later emerged in the course of the American Century were there from the beginning. Giddings wrote Democracy and Empire (1900) in response to William Graham Sumner’s anti-imperialist essay “The Conquest of the United States by Spain,” a classic document in modern liberalism ([1899]1992). Between them, they manage to formulate
incisively the key problems of a democratic imperialism, the issue of imperial overstretch, the problems of identity, the peculiar dialectic between the supposed victims and the supposed beneficiaries of imperialism, and the complexity of the interrelation between mutual interest and ideation about mutuality. They would each fit comfortably in a present-day discussion of the European Union, or of the Cold war and the difficulties this war presented for the United States and the world. Giddings was a co-operativist who had faith in international organization,\textsuperscript{12} but who understood perfectly well that co-operation only worked under conditions that were difficult to establish, but understood the burdens of the imperial role in which it would have placed the United States; Sumner was a liberal who foresaw the domestic costs, particularly the costs to a democratic and free political culture, of the burden of international activism. Sumner as a liberal and Giddings as an old co-operativist had behind them, and in a sense epitomized, the whole of the nineteenth century tradition in Social Theory. The quality of their thinking is a rebuke to present writers who consider American sociology to have been a vehicle and expression of Imperialism

They were not the last sociologists to take up these issues, though they were perhaps the greatest. The problems that concerned them point to a whole dimension of social and political thought that has effectively vanished from the history of sociology but which had absolutely central impact on many of the thinkers whom Parsons and Merton disdained, such as Charles Ellwood and Harry Elmer Barnes. I certainly do not mean to imply that sociologists were the best or the most interesting participants in these discussions. But when one recognizes that now forgotten figures \textit{in sociology} like Charles Ellwood, who were Christian pacifists and anti-conscriptionists who understood their beliefs in both religious and social theoretical terms, were supplanted by figures like the theologian Reinhold Neibuhr, the political guide of Jimmy Carter, who were \textit{not in sociology}, one begins to grasp the thread: sociology paid a high price for its self-mutilation. The moralism of American intervention in Kosovo is rooted in the thinking of the late nineteenth century (indeed, it resembles the early period of American involvement in the Philippines).

Commentary: The Basic Method of Social Theory

If we were to imagine an article in \textit{Foreign Affairs} on Sumner’s essay and Giddings’ response that utilized them fully, one can see something of the strength of social theory. Thinking through these texts could tell us something about what has changed and what has not in the past century, about what concepts apply and what concepts now have become so transformed that they cease to apply. In the manner of Foucault— and for that matter Leo Strauss— reconsidering the thinking of these figures allows us to make sense of the present, but also enables us to challenge it by allowing us to get outside it in a way that newspaper editorialists typically cannot. The analysis of texts like those of Giddings and Sumner can of course be a tedious, pedantic activity. But where they can engage us by challenging our expectations and prejudices, they are potentially much more, and a certain amount of tedious pedantry is needed to get them right in the first place. But getting them right, especially getting them right historically, is sometimes largely

\textsuperscript{12} Giddings created the American Sociologic Society in the 1880's as a vehicle for co-operativism, and treated international co-operation as the highest form of co-operation. He was Wilson's successor at Bryn Mawr, and had a hand in Wilson's later effort to produce a league of nations.
irrelevant to the roles past thinkers play in present conversations, and this fact is a source of confusion.

There are various modes of commentary. And since social theory is primarily commentary, it is crucial to understand something about these modes, and why, together with their subject matter, they suffice to make social theory an autonomous field. It is a kind of capitulation to the rhetoric of science to call modes of commentary “methods,” though it is appropriate: commentary comes in forms, the forms produce different kinds of problems and results, and can be taught and understood as a means of self-reflection, as a part of the meta-field of social theory.

Leo Strauss famously dismissed Sabine’s standard history of political thought by remarking that for Sabine the history of political thought was merely the history of error. Strauss wanted to make it something more, namely the history of truths imperfectly apprehended. And he took as his model for this the Platonic dialogues and their ascent from opinions, understood as soiled fragments of the pure truth, and truth itself which might best be acquired by stringent analysis of opinions, especially good opinions, designed to remove the soil. By assembling a variety of opinions in such a way that the errors in each could be illuminated by the truth in others a synthetic and truer understanding emerge. This is one way of thinking of the dead. Foucault’s way, of seeing in the writings of the dead the un concealed forms of the founding errors of modes of rule in the present, is another. Both involve revealing our own prejudices: they use the dead as a method of reflexive self-discipline. Another kind of reflexivity is Schluchter’s, who, in his work on Weber, reflects on Weber’s errors and limitations in order to improve and revise him. There is some of this in the writings of Habermas and Giddens, as well. But here the interest shifts. The dead are dealt with more roughly than the true historian or antiquarian would tolerate. But they are never dealt with so roughly that they are unrecognizable, nor do they appear to be merely the products of the intentions of the interpreter. They are engaged not quite as equals but they are nevertheless engaged as, so to speak, senior partners in a common enterprise, parties to a continuing conversation; and commentary is governed by the need to keep the conversation interesting in the present. This is the most common form of writing in social theory.

The great thinkers in the canon, Weber and Durkheim and Marx, and including Parsons and Habermas at the next level, wrote works that were almost entirely commentary and conceptual improvement and revision. Even such a text as Durkheim's *Suicide* has virtually no original content as distinct from commentary. Indeed only a very few of the tables in Durkheim's text are not copied from standard texts of the day. What Durkheim did, and did very dramatically, was to lend a very simple order to a great many facts which had previously been commented on in terms not far removed from Durkheim's but which had never been brought together in a conceptually coherent common structure. Weber is almost entirely commentary as well. If we do not recognize it as such, it is because he did not identify very explicitly the sources on which he is commenting, and typically we are not aware of them. But usually they are easy to identify and Weber's original contribution is clear. Weber of course had a kind of general system of categories into which the texts he commented on were reinterpreted, and this gave the material a great deal of coherence. But what he says is nevertheless commentary, though, as with Durkheim, it is commentary on figures who are not as great in retrospect as he.

The difference between Anthony Giddens today and Max Weber a century ago is not that they have radically different working procedures. Both are primarily commentators, and each has
a distinctive agenda in their commentaries. The difference is in the circumstance that Weber's commentaries in some sense cannot be made dispensable. There is some point to arguing about the meaning of charisma, revising the notion of charisma, applying it in new ways, and so forth, as himself did when he took it over from Rudolph Sohn. There is perhaps even a point in coming up with some alternative conception of leadership that covers more or less the same material but which does not appeal at all to the notion of charisma. But in the latter case, one cannot expect the concept of charisma to vanish. In part it is because it is constitutive. One can not understand the career of Al Gore without understanding the concepts. Charisma is as much a part of twentieth century politics as the distinction between the King’s two bodies was a part of medieval politics. And it stands in much the same relationship to politics. It is a part of the fabric of concepts that constitutes politics. If Giddens’ articulation of the Third Way actually eventuates in a novel political and social regime, it too will pass into political discourse.

With other concepts of social theory matters are perhaps different. The concept of negotiated order, for example, probably is not constitutive of anything in social life. Yet even it is conceptually kin to a way of talking about phenomena as constructions which is a constitutive part of contemporary social life. It is difficult to imagine the guilt of Dreyfus being regarded in the way that the guilt of O. J. Simpson is regarded, as not so much a fact but a product of lawyers, prior opinions, the media, and so forth. No novel social theory is going to take the “constructed” character of such matters as the guilt of O. J. Simpson away, and consequently no theorist is going to take the concept of construction away. This is not to say that it will not be revised, critiqued and altered. But it is to say that it is not a matter solely in the hands of theorists.

The social theorist is thus in a radically different position from the scientist. Lavoisier’s achievement was to make sense of combustion without reference to phlogiston. The same thing cannot be done with such a concept as the “underclass.” The underclass is, in part at least, the product of policies, policies or ultimately understood in terms of the notion of the underclass and revised in accordance with it. Combustion is not such a process.\(^{13}\)

When something passes from being simply a theory to being part of phenomenon, it acquires the kind of solidity that makes the theories about it impossible to simply supplant with “better” theories. The world certainly can change in such a way that the theories are no longer relevant, and the theory of the king’s two bodies is no longer relevant. But the theory has not lost its constitutive connection to the events it described, and they simply become unintelligible to the person who has no access to the theory. Identifying the concepts that have come to assume this constitutive role in contemporary social life is difficult to do, but a few examples should suffice. The Protestant Ethic, “racism,” the idea of a “role,” the idea of the underclass, each have been part of the justification of practices, policies, therapies, or problem solving methods which had consequences and still have consequences. Each idea is bound up with other ideas not known to the newspaper columnist users of them but nevertheless conceptually inseparable from them.

This means that social theory cannot simply be a succession of closer approximations to theoretical closure, as writers like Parsons supposed, but must remain a relentlessly critical and reflexive activity. Understanding the uncharismatic character of Al Gore requires us to understand what charisma once meant, and what it means now in the context of television and mediated news. And to understand the phenomenon it is obviously not enough to “apply” the

\(^{13}\) This is perhaps not what a radical constructionist would say about fire, but it is difficult to imagine a constructionist sufficiently radical as to say that Lavoisier invented fire.
concept, either in Weber’s form or in some off-the-shelf version from a contemporary textbook of sociology. It is necessary for us to understand the concept and its implications and relations more fully, to understand how its meaning changes in new contexts, and perhaps why the topic of charisma has become more rather than less prominent in this century, and this typically requires an understanding of the arcana of the intellectual history of academic disciplines. If we do not know that the plebiscitarianism of Napoleon and the electoral plebiscitarianism of Gladstone were the models for Weber, our capacity to think through Gore’s problem-- the problem of acquiring plebiscitary acquiescence today-- will be limited to the kinds of capacities that the newspaper columnist (or the sociologist) has, of applying a term unreflexively and unhistorically.

To put the point very bluntly: social theory has its origins in an existential situation, the situation in which concepts that individuals in society use to understand one another and to understand their social world cease to apply as they once did. The social theorist tries to understand the changes, to theorize, often by supplying second-order concepts that enable the change, and the problem with the concept, to be understood. Durkheim and Mauss did this for the concept of the person, and Durkheim tried to make sense of the novel claims of “individualism” in terms of the moral development of society. Weber, similarly, sought to understand the problem of the novel legitimacy of the Rechtsstaat of his own day, and the inadequacy of older accounts of legitimacy to comprehend it, which he did in terms of a broader set of categories of legitimacy which he applied transhistorically but understood as themselves subject to the problem of historical impermanence. This problem of understanding the failure of social concepts to apply in new situations is the core business of social theory. And today we can recognize, as Weber himself did, that the second order concepts themselves almost invariably also cease to apply or come to mean new things in new situations: this was the key to the eternal youth granted to the historical sciences.

Social theory is thus inevitably a field which scrutinizes its own concepts and considers the world in the light of the problems of applying its concepts in new settings. Commentary, which mixes a consideration of the concepts themselves and of the circumstances in which their application is under threat or unstable, is the main form in which this scrutiny is articulated. The modes of commentary differ: some emphasize the changes in circumstance, others the deeper significance and therefore relevance of concepts that superficially seem to be no longer relevant; some emphasize the second order difficulties of concepts whose problems of application are ignored by the naive. The classic social thinkers produced what we might call technologies for dealing with the problem of application, such as Ideologiekritik and “Genealogy,” which were substantial and powerful means of engaging problems, and have become entrenched in public discourse. Social theory thus is not only like political theory, but in effect the same thing: a discipline driven by the problem of making sense of the concepts that inform activity which pursues these problems through commentary on past concepts. Commentary might make some use of actually existing “sociology,” but it is not likely to make much use of it: the main facts that bear on the problem of the application of concepts in new settings are to be found elsewhere.

Feminism as a Test Case

What I mean by a mature field, and why I believe social theory to be, like (and perhaps with) political theory, in this category, should now be somewhat more clear. By “mature” I mean...
autonomous, that is to say with its own purposes and problems, but also sufficiently rich in its means of approaching these problems, and sufficiently balanced between the alternative ways of approaching problems, that it is not likely to collapse into a sectarian school of “application.” The obvious rejoinder to what I have said about the maturity of social theory as a discipline is that it is in fact the case that a massive amount of innovative “theoretical” work is being done and continues to be done under the banners of postmodernism, feminism, cultural studies, and so forth, so the argument that social theory is primarily commentary and that the main themes are more or less set is simply false. In feminism there is plenty of sectarianism, novelty, and also the potential for a radically altered understanding of social life-- a “new paradigm” of precisely the kind that the argument denies.

Certainly I do not deny the existence of this body of work. However, I take it to be work that is in conformity with the basic idea that social theory is a mature discipline, and also in conformity with the basic picture I have given of the existential situation which is the ground of social theory, the situation in which concepts fail to apply. Feminist social theory, like social theory generally, is predominantly commentary. The commentary focuses on themes of gender difference, and employs the usual methods of social theory: inversion (Judith Butler's “everyone is in drag”), genealogy (the use of the explicit writings of earlier theorists to understand the practical difficulties that arise later), and particularly the exploitation of the fact that past theoretical ideas about gender difference are part of the phenomenon of gender difference itself. The core situation in which feminist social theory is rooted is the idea that dominant perspectives fail to apply to women or to the particular phenomenon of gender difference, and that the dominant perspectives are “blind” to these phenomena in various ways. The ways in which the dominant perspectives fail to apply are the basis of feminist theoretical reflection and reflexivity.

There are some additions to the picture I have given that the case of feminism enables. Feminism illustrates in a particularly clear way one of the crucial “market” features of social theory. Feminism, like comparative historical sociology, management studies, and dozens of other fields, is a consumer of theoretical “perspectives.” Part of the practical problem of teaching Women’s Studies is to provide perspectives for students to employ in their own thinking: in this respect feminism is a form of social theory, which has its own pharmacopeia, with its own version of the tensions of social theory. Part of the practical problem of writing feminist analyses is that the analysis of some revealing or problematic case requires perspectives: in this respect feminism is a buyer and consumer.

However much one would like to think of feminism or queer studies as somehow providing a new paradigm in social science, the reality is that the basic conceptual tools of these supposed revolutions are in fact quite traditional. Indeed a good deal of feminist theory consists simply of substituting "women" for "the proletariat" in the analysis of gender oppression. The whole question of the relationship between Marxism and feminism has been the subject of extensive discussion within feminist circles and the very titles of works in feminist social theory often point often to their origins in these well established traditions of thought.

Is this generally true of feminist theory? This is a matter of relative judgement. Feminists have been avid consumers of the more radical variations on traditional perspectives, in their efforts to deal with the problem of application. In attacking and revising Freud, for example, the

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14 This is done in a particularly clear, almost mechanical way in Nancy Hartsock’s classic essay. Yet despite the mechanical character of the substitutions, a whole range of novel and difficult issues are opened up (1987).
revisions made by Lacan have been particularly useful. In this respect feminist social theory is very far from being hide-bound or traditional. Nevertheless, in terms of the conceptual machinery of these theories, there is very little that is radically new. Most of the novelty, even among the theorists for whom one could claim something more than the kind of cookie cutter substitution mentioned above, is designed to bridge the gap between what one wants to say about gender oppression and existing conceptual machinery for talking about it. Thus this conceptual revolution is not anything like Freud's conceptual revolution. It is a creative extension and revision of Freud. Carol Gilligan's notion of a distinctly feminine “ethics of care” is the inversion and revalorization in a positive direction of very familiar 19th century ideas about the sentimentality of the weaker sex. There is of course nothing wrong with inverting these older valorizations. Inversion is a technique-- one well developed by Nietzsche. But, inverting is very different from inventing value orders in the first place.

For a particularly good example of what I have in mind, let me briefly turn to the writings of Dorothy Smith which are among the most fully developed forays into social theory from a feminist point of view. Dorothy Smith, in such books as The Conceptual Practices of Power, takes over the familiar Foucauldian idea that the ultimate machinery of oppression is the concepts with which a society operates which define and categorize actions and people in ways which prevent them from recognizing their oppression and acting upon it. The notion of conceptual practices of power is a kind of updated version of the notion of false consciousness, in which practices rather than consciousness produce the falsity. Smith argues that the social world is constituted by practices and adds to this the notion that these practices are specifically gendered and produce the oppression of women and their alienation from themselves and so forth and so on. Nothing about this idea of practices is new. Its history reaches back at least to Montesquieu’s notions of customs and through the 19th century in the form of ideas such as Sumner's powerful and recalcitrant mores which resist the power of reform.

To say this is certainly not to denigrate Dorothy Smith's achievements. Applying a basic and important idea in social science in a novel way to produce novel results is a great thing, and it takes a great deal of originality to think through the problems of making these kinds of extensions. Nevertheless they are extensions. They might alter our picture of society by succeeding but in the end they are themselves not new concepts although they may in the course of extension lead to new concepts which provide a kind of bridge between the existing theory and the new phenomena. Another example of such a bridge concept is the notion of human capital developed by Gary Becker. The idea of capital is obviously not new, nor are the basic economic implications of the idea of capital accumulation and so forth. But translating this idea into a form in which it can apply to such things as education and the possession of skills, that is to say nontransferable and nonsaleable things, requires a fairly elaborate effort at formulating bridge concepts and rethinking the problem of valuation and the like. No one denies that this is original. No one, however, thinks that it is a replacement of classical, or in this case neoclassical, economics. On the contrary, it depends on it and cannot do without it. Similarly for Dorothy Smith. If the practice model is bogus, then so are Dorothy Smith's theoretical arguments. And this points to a feature of feminist discourse that indicates something about its status-- as lying somewhere between social theory itself and the kind of perspective-consuming disciplines mentioned earlier.

A Post-Script: Goodbye to Sociology
Merton quoted Whitehead’s saying that a science that hesitates to forget its founders is lost. He would better have used a different quotation from Whitehead: that the history of philosophy is a series of footnotes to Plato. Social theory is a humanistic discipline. It is in the business of commentary, and it has a sufficiently rich set of Platos, including Plato himself, to operate in the fashion of political theory, that is to say to use commentary as a means of analysis of the present. Social theory is not dependent on a single canon. Its relations to various empirical and practical disciplines have varied over time.

If we return to 1936, we can see where the road forked, though the tensions, and indeed overt expressions of hostility to “theory” or the wrong kind of theory can be traced to an earlier stage. In this year Parsons published The Structure of Social Action, a subsidized publication with few sales. Ellwood published A History of Social Philosophy, both in a full and an abridged form. The abridged form was an alternate book of the month club selection. Ellwood’s book was a typical humanistic text, in the fashion of his former colleagues Lovejoy and Sabine, which traced the tension between two views of society, represented in developed form by the liberalism of Sumner and the social system interventionism of Ward. The names are antique, but of course the problem is not. When rational-choice theory subsequently emerged in the social sciences as a major approach, crossing disciplinary lines, it reproduced the same core tensions. Substitute Elster for Sumner, and one sees that Ellwood was a better prophet than Parsons, in this respect at least: the “convergence” that Parsons claimed to discover as an empirical fact in The Structure of Social Action proved to be an illusion, the tension discussed by Ellwood much more deeply rooted.

Why did Parsons appear for so long to be right? To understand this, it is necessary to return to the crude “material” concept of disciplines for a moment, and to think of the power of disciplines in the post-1945 period as a kind of historical anomaly. The twenty-five years from 1945 to 1970 saw the expansion and transformation of universities world wide, consequently the creation of a market for academic employment that was completely unprecedented in history. The market was segmentary, that is to say divided into separate parts, and disciplines were the primary boundaries. The new opportunities, at least once the markets were sufficiently developed that growing departments did not need to employ non-disciplinary outsiders, were for persons with training in the field itself, and training in the field came to mean something quite specific and demanding, because competition occurred within these internal, segmented markets. This meant that the academic careers that Durkheim, Weber, Simmel, Sumner, Small, Lazarsfeld, Reisman, Parsons, Sorokin, and many others had, which began in one field and traversed others before arriving at sociology, were no longer possible. Ellwood understood that this was the end of an older model of sociology, his own, in which empirical sociology, practical action, and theory, understood in his terms, could co-exist. The new bargain allowed theory in sociology as the handmaiden of scientific, empirical sociology. Departments competing for status were compelled to hew a narrow path: “the history of social philosophy,” as Ellwood could still title his book on social theory in 1936, was now an expensive luxury, and even an embarrassing, non-scientific burden.

The benefits of the bargain were clear enough: the new-model sociology was well-funded, grew dramatically, and provided for the possibility of far better careers. A recent book on C. Wright Mills and Hans H. Gerth (Oakes and Vidich 1999) shows in great detail how the atmosphere had changed, and how Gerth, the traditional, overly-Germanic scholar, was left in
the dust by the ambitious Mill, who had analyzed the problem of becoming a “Big Shot.” Parsons, Merton, and their students benefitted from this deal; others with an interest in theory in the post-war period were told that it was not a marketable speciality, and that they should describe themselves as interested in “social change,” a topic sufficiently amorphous that a theoretically-minded teacher of this subject could survive in a new-model department of sociology.  

In time, the Faustian character of the Parsons-Merton bargain with empirical sociology became clearer. Ironically, it was hostility to the bargain, which was characterized in terms of its supposed philosophical roots as “positivism,” and to Parsonian theory, that was characterized in terms of its supposed “conservatism,” that kept theory alive in sociology, long after it became clear that the rapprochement between Parsonism and empirical sociology was an empty illusion: arguing against Parsons and positivism became the primary concern of a whole generation of sociologists concerned with theory.

How did the devil collect? The demands of the bargain with sociology, the burdens of condominium, eventually had the effect, within American sociology, of pushing the community of social theory below the level of reproduction. The remaining theorists had less and less in common, and there were few of them. The dominant mode of interest in theory became amateur interest, avocational interest. Few students could take more than a course or two in theory-- the rest they had to learn on their own. And what they learned was different from what others with an interest in theory learned. The thinkers on whom their attention was focused were European, or non-sociologists. So, to the extent that there was common ground, it was ground belonging to others. Thus did the devil collect. Today, when one opens a textbook in social theory, one is as likely as not to find no American sociologist in it, and increasingly, no sociologists at all. Social theory thrives, but not in American sociology.

References


Abbott’s book on Chicago sociology reports the shifting attitudes of the department of sociology and Edward Shils toward one another during the post-war period, and some of this is quite revealing. The department was interested in Shils after Parsons had made the new theory scientifically respectable; Shils, however, grasped that the bargain was too constraining, and that to the extent that he was concerned with fundamental questions about the nature of society, he was better off in the interdisciplinary Committee on Social Thought. Shils could choose– others had to face the music in sociology departments. (1999)


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