CHAPTER ONE

Introduction to Volume One

This volume brings together a series of essays relating to the present state of sociology that are of a critical and a programmatic character. In the critical essays, I discuss a number of chiefly methodological issues that arise with certain current styles of sociological work—issues that, I believe, have not been given the attention they deserve. In the programmatic essays, I put forward various suggestions about how a new mainstream sociology might be formed that would be characterised, above all, by a closer and intellectually more productive integration of research and theory than is typically apparent today. In this introductory chapter I have two main aims. First, I seek to supply some general background to the essays, and in particular to update the background I provided in the first edition of *On Sociology*. Second, I outline my purposes in writing each essay and, in the case of those that have stimulated commentary and debate, I note criticisms that have been advanced and make some response to what seem to me the more consequential points that have been raised. I take this opportunity of thanking all those who have been ready to read my work and to react to it, even if from positions of radical disagreement.

THE BACKGROUND

In the Introduction to the first edition, I argued that sociology was in a state of general intellectual disarray and one that, were it to continue, must threaten to undermine the substantial institutional progress that sociology has made, within both academia and national research communities and organisations, over the course of the twentieth century. I identified three main sources of this disarray that were, I suggested, of cumulative significance.

> 1. A manifest lack of integration of research and theory—the scandal of sociology. Chief responsibility for this situation must lie with those sociological or 'social' theorists who do not accept that the prime purpose of theory is to provide a basis for the systematic *explanation* of social phenomena, as established by empirical investigation, and who would rather treat theory, as van den Berg (1998: 205–206) has aptly put it, 'as a sub-discipline in its own right, and one with its own criteria of utility and relevance that would seem wholly divorced from the needs and concerns of practising sociological researchers'.
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> 2. A collective failure on the part of sociologists to decide just what kind of discipline sociology is or ought to try to be. The so-called 'reaction against positivism' that developed from the later 1960s led to a return to nineteenth-century preoccupations with the differences between *Geisteswissenschaften* and *Naturwissenschaften* and to a divisive exaggeration of these differences, especially on the part of sociologists not directly engaged in research.
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> 3. Second-order disagreement over how the disagreement over the nature of sociology should itself be regarded. This is often expressed in—or, rather, covered up by—an appeal to 'pluralism', but to a pluralism that is intended not to promote a vigorous, possibly mortal, competition among differing views but rather an accommodation among their adherents. Such pluralism is justified pragmatically by the need to preserve a semblance of disciplinary cohesion; but often, too, philosophically, by the adoption of some kind of postmodernist stance from which the idea of rational argument as a means of approximating objective truth can be rejected in favour of cognitive, or epistemic, relativism.

Writing now some six years later, I would see little reason for any major revision of this analysis of the condition of sociology.[1] Intellectual disarray persists (cf. Cole, ed., 2001). At the most manifest level, witness the often quite startling differences that can be found in the content even of graduate courses in sociology from one university department to another; or, again, in the character of the papers that could be taken as representative of different sociological journals.[2] Attempts are from time to time made to suggest that this state of affairs is of no great concern or is inevitable or even perhaps in some way desirable (e.g., Abbott, 2001: ch. 1). But such attempts are scarcely convincing—at all events to those viewing sociology from without. Far greater weight has still, in my view, to be given to Huber's argument (1995) that sociology's evident lack of a clear disciplinary core—'what must be retained if the discipline is to continue to exist'—leaves it especially vulnerable to external threats to its intellectual standing and institutional consolidation alike.

However, while I would thus continue to regard the state of sociology as being a precarious if not critical one, I do at the same time believe that, of late, it has become possible to discern a number of developments that provide grounds for a degree of optimism for the future that did not earlier exist or that, at very least, serve to bring greater clarity than before to some central issues. The nature and significance of these developments I can best set out in relation to the three sources of the intellectual disarray of sociology that I listed above, and by taking these in reverse order, that is, from the more general to the more specific.

To start, then, with problems of second-order disagreement over the nature of sociology as a discipline, the possibility of more productive debate has been significantly enhanced by the waning influence of the cognitive relativism that previously provided an apparent legitimation for the avoidance of all foundational questions and in turn for a pluralism of an essentially mindless kind. In particular, postmodernism, the major source of cognitive relativism in the later twentieth century—with its claim that truth is not discovered, through rational procedures, but is rather in various ways 'made'—would now seem to be losing much of its former allure.

In part, this has to be reckoned as the consequence of the internal dynamics of intellectual fashion, to which sociologists appear unduly susceptible, and also of the increasingly damaging criticism to which postmodernist arguments became subject in the course of the 'science wars' of the 1990s (cf. Gross and Levitt, 1994; Gross, Levitt, and Lewis, eds., 1996; Koertge, ed., 1998; Sokal and Bricmont, 1998).[3] However, one should further note, in more positive vein, a number of powerful reassertions by leading philosophers of the continuing validity of what Searle (1993) has called 'the western rationalist tradition' of scientific and scholarly enquiry and of the idea of objective truth as both an intellectual goal and cultural value (see esp. Searle, 1993, 1995, and Nagel, 1997; also Haack, 1998; Williams, 2002). Of particular significance in the present context are Searle's robust defence of the correspondence theory of truth—the theory, anathema to postmodernists, that truth is a matter of correspondence to 'facts in the world'; and Nagel's demonstration of the contradiction inherent in the rejection of the idea of reason as a universal category of thought—since this idea 'is necessarily employed in every purported challenge to itself' (1997: 7).[4]

In this changing intellectual climate, it has then become more difficult than before for sociologists to take an anti-foundational stand and to believe that a relaxed, 'anything goes' attitude in regard to methodology and theory is not only advantageous from the point of view of disciplinary politics but indeed a mark of philosophical sophistication. Rather, greater pressure has been placed on individuals and schools to decide and clearly set out what kind of intellectual enterprise they see themselves as being engaged in and what basic rules of the game they are ready to play by. Clear advantage follows from this.

An illustration is, I believe, provided by the statements of position recently made by the editors of two important collections, the main purpose of which is to uphold comparative historical and other qualitative approaches in macrosociology (and political science) against criticism from more 'positivistically' inclined quantitative researchers and rational action theorists—myself included. In earlier replies to such criticism, and indeed still in some more recent ones (see, e.g., Somers, 1998; and cf. Steinmetz, 2004, 2005), the main defensive resort is to versions of cognitive relativism, such as 'historical epistemology' and radical social constructivism, as a result of which the possibility of any meaningful dialogue about the specifics of research methods and theory formation is removed. However, Mahoney and Rueschemeyer in their introduction to *Comparative Historical Analysis in the Social Sciences* (2003: 22–24) make it clear that they would not wish to appeal to postmodernist or other arguments that 'assume that valid knowledge is inherently illusory'. Rather, they insist that demonstrating empirical regularities and seeking testable theoretical accounts of their causation are integral concerns of comparative historical work. And, in similar vein, Brady, Collier, and Seawright in the first chapter of *Rethinking Social Inquiry* (2004: 18–20) acknowledge, chiefly in response to King, Keohane, and Verba (1994), that although qualitative and quantitative researchers may have different aims, follow different strategies, and use different research tools, both need to follow the same 'logic of inference' and in turn to apply the same principles and standards in the linking of argument and evidence (cf. also Rueschemeyer, 2003: 324–28).

As will later become apparent, I for one am unconvinced by a number of the particular methodological claims that are made in the collections cited, and would wish in these respects to maintain my previous critical stance. But the more important point is that a sufficient degree of consensus does now exist for a potentially productive exchange of views to take place, and thus for a genuine, rather than a merely spurious, pluralism to prevail.[5]

Turning next to issues concerning the kind of discipline that sociology is or should aim to be, I would see the declining influence of cognitive relativism as here again an encouraging development, and likewise the apparent fading out, at long last, of the reaction against positivism—if only as a result of 'positivism' having become used in so many different, and often quite unwarranted, senses as to lose all meaning except, perhaps, as a term of general disapprobation applied to survey-based and other quantitative work.

One indication of this attitudinal shift is the growing number of sociologists, and especially among those with some involvement in or awareness of interdisciplinary work in, say, the medical, educational, or environmental fields, who would now in effect to agree with Popper (1972: 183–86) that 'labouring the difference' between the natural and the social sciences is 'a bore', and often indicates a lack of understanding of the methods of the former as well as of the latter. And, more generally, there would today appear to be a greater readiness once again to make out the case for the social sciences as an enterprise that has a basic methodological commonality with the natural sciences, even if at the same time involving some inevitable differences (see, e.g., Steuer, 2002).[6] In this context, then, the issue does become more sharply posed of whether or not sociology should seek to be part of the social sciences as thus understood.

There is, to be sure, no shortage of authors who, even if not sharing in the postmodernist rejection of the very idea of science, would still adhere to the view that for sociology to aim for scientific status is mistaken and indeed vain (see, e.g., Bryant, 1995; Flyvbjerg, 2001; Jenkins, 2002). The sciences proper—in effect, the natural sciences—are concerned, it is held, with inert physical entities but, in contrast, sociology is concerned with 'self-reflecting humans' (Flyvbjerg, 2001: 32) and it is they who together construct and reconstruct, through their own subjectivity, what counts as social reality. Moreover, sociologists cannot set themselves apart from this process. *Their* concepts are inevitably dependent on, and in interaction with, the concepts that are embedded in the everyday lives of 'lay actors'. Thus, the attainment of cumulative, theoretically grounded sociological knowledge is continually subverted by the very way in which human society is constituted. All general propositions that may be advanced by sociologists are necessarily rendered unstable since such propositions will need to change in response to changes in lay actors' own understandings and interpretations of their social worlds—that is, of social reality—and including those changes that may be prompted by the practice of sociology itself. From this 'impossibilist' position, the ultimate purposes of sociology have then to be seen not as cognitive but rather as moral and political. Sociology, it is argued, should aim to be a mode of public discourse or 'conversation' that offers citizens new perspectives on society, new insights into their own experience within society, and new value positions and vocabularies that can serve as a basis for both social critique and praxis.

However, while arguments on these lines retain wide support, they are now being more often challenged by those who would believe that they amount in effect to selling sociology short; and, further, as setting up an unfortunate division between sociology and other obviously related disciplines, such as economics or psychology, whose practitioners are far less inhibited in their scientific ambitions.

For example, in an important intervention, Boudon (2001) recognises, as a matter of fact, that sociology is 'a house of many mansions' and that what he labels as 'expressive' and 'critical' sociology are prominent within it. Nonetheless, he insists that privilege must be given to 'cognitive' sociology—or sociology as social science—as 'the sociology that really matters'. This is so because while sociology may well serve to express and illuminate individual experience or to inform sociopolitical dissent, it can do so *validly* only on the basis of defensible knowledge claims. Issues of the logical coherence of sociological analyses and of their relation to the findings of systematic investigation cannot be avoided.[7] Moreover, as regards the assertion that a scientific knowledge of society, in the sense of knowledge of a cumulative, theoretically grounded kind, will always remain out of reach, Boudon resorts to a straightforward empirical rejection. In both classical and contemporary sociology, he maintains, there is in fact ample evidence of such knowledge actually being achieved. A tradition of scientific sociology extending from the nineteenth century through to the present can be documented (see Boudon and Cherkaoui, eds., 1999).[8]

At the same time, it is important also to note that in course of the philosophical reaction against cognitive relativism, to which I earlier referred, several elements of the impossibilist position on social science are directly called into question. Thus, Searle (1995: ch. 9) complements his general defence of the correspondence theory of truth with the further argument that while social facts may differ from the more 'brute' facts of nature in depending on human recognition and agreement, this does not prevent a version of the correspondence theory from being viable in their investigation. More specifically, it does not follow, as the impossibilists would appear to suppose, that because the mode of construction of social reality means that it has an *ontologically subjective* character, this must preclude its treatment by social scientists as *epistemologically objective*. Similarly, Hacking (1999: ch. 1 esp.) stresses the error of extending the idea of social construction from the formation of concepts to the 'facts in the world' to which these concepts are applied, including ontologically subjective social facts. Because the concepts of, say, 'market' or 'economy' are socially constructed, it does not follow that the social activities to which these concepts refer have no existence independently of them.[9] And to this it can then be added that although in the case of the social world there may be interaction between the concepts of researchers or theorists and those of lay actors, such interaction does not necessarily occur nor, where it does, must it lead to problems of the fundamental kind that impossibilists would imply.[10]

Finally, then, as regards the lack of integration of research and theory in sociology, the main development that here provides grounds for optimism is the widening interest in a new style of theorising that I would see as holding great potential. This style of theorising is distinctive in that it is explicitly concerned with the explanation of social phenomena—rather than with metatheoretical issues or simply the elaboration of concepts; and, further, in that it seeks such explanation through the identification and analysis of the specific *processes* or *mechanisms* by which such phenomena are generated and sustained or perhaps disrupted and changed (see, e.g., Elster, 1989b; Coleman, 1990; Esser, 1993–2001; Hedström and Swedberg, eds., 1998; Boudon, 2003b; Barbera, 2004; Cherkaowi, 2005; Hedström, 2005).

So far, this new style has been pursued mainly via a commitment to methodological individualism and to primarily micro-to-macro explanations grounded in versions of rational action theory. And this, I should say, is the particular approach for which I subsequently argue both in the programmatic essays in this volume and in the complementary illustrative essays that appear in Volume II. However, such an approach is not integral to theory construction in terms of mechanisms. This could, for example, proceed in a macro-to-micro fashion, with, say, a prime emphasis on the mechanisms that are involved in the structural or cultural conditioning of social behaviour. Opportunity is indeed created for such differing approaches to be set in direct competition with each other as regards the explanatory success that they achieve.

The new style of theorising makes for a closer relationship between research and theory in two main ways. First, it does, at least at its best, start out from regularities already established by systematic empirical investigation and offers explanations of how these come to be as they are—rather than elaborating possible generative processes for social phenomena that may, or may not, be in evidence. In this way, it is then made harder for social researchers to maintain an attitude of general indifference to theory that was not in fact unreasonable for so long as theory remained, to revert to van den Berg's phrase 'wholly divorced from [their] needs and concerns'. Second, mechanism-based explanations of social phenomena are ones that are in turn open, at least in principle, to empirical test. Insofar as the generative processes that are seen as adequate to produce the regularities to be explained are spelled out in some detail, further research can then be undertaken—of, perhaps, a quite different kind to that which established the regularities in the first place—in order to determine whether it is the mechanisms proposed that are indeed at work. Research and theory can thus be brought into a state of continuous interaction.[11]

One other point concerning theory construction in terms of generative processes might be made, especially in relation to the impossibilist position on sociology as social science. Impossibilists lay great stress on the crucial role played in explanation in the natural sciences by general laws that in turn provide the basis for reliable prediction; and sociology, it is observed, has conspicuously failed to arrive at such laws, as indicated by its lack of predictive capacity (see, e.g., Flyvbjerg, 2001: 30–32, 38–40; Jenkins, 2002: 24–27). However, it has for some time been recognised that explanation in the natural sciences, and especially outside of physics, may well not conform to the covering-law model. In the biological sciences mechanism-based explanation would in fact appear quite standard. Explanation consists in determining causal processes or mechanisms that operate at some 'deeper' level than that at which the phenomena of interest are themselves observed (cf. Cox, 1992).[12] It is true that such processes are then typically given a far more unified and cogent theoretical grounding than in sociology. Nonetheless, it remains the case that the explanations produced may still not attain complete generality (as, say, in ecology) and that they may not allow in any strict sense for prediction (as, say, in evolutionary biology).[13]

In other words, the quite radical discontinuity between the natural and the social sciences that the impossibilists would wish to set up is not here apparent. As Lieberson and Lynn (2002) have argued, in regard to models of explanation and more generally, it is in fact the biological sciences, far more than physics, that offer instructive parallels for sociology as social science. Rather ironically, in their preoccupation with the significance of general laws and prediction, impossibilists would seem to fall victim to the kind of misleading preoccupation with physics of which 'positivist' sociologists have been so often accused.

THE CRITICAL ESSAYS

The first four of the essays that follow are of a critical character, and the first three, relating, in turn, to historical sociology, to case-oriented as opposed to variable-oriented approaches in comparative macrosociology, and to sociological ethnography, to some extent go together. My intention, I must stress, is *not* to dismiss these versions of qualitative sociology out of hand but rather to raise methodological issues that I would regard as both serious and unduly neglected. As an indication of the depth of the methodological difficulties that arise with these styles of enquiry, I show how in each case alike their proponents, even while inveighing, often rather imprecisely, against positivism, are, paradoxically, themselves led into positions that are in fact positivistic in quite basic and well-established senses.

The first essay, 'The Uses of History in Sociology: Reflections on Some Recent Tendencies', starts from a rejection of the view advanced by Abrams (1980), following Giddens (1979), that 'history and sociology are and always have been the same thing'. In the essay, I focus on one particular, methodological difference between the two disciplines that, I argue, is of a highly consequential kind: namely, that while historians have to rely solely on evidence in the form of what I call 'relics'—that is, the physical remains of the past of one kind or another—sociologists, insofar as they work in present-day societies, have the further possibility of using various kinds of research procedure in order to generate evidence that did not exist before. I then discuss the implications of this difference for sociologists' research strategies and the more specific problems that it raises for the practice of historical sociology. I conclude with a critique of what I label as 'grand historical sociology': that is, historical sociology that usually aims to deal with large macrosociological issues and that is written on the basis not of relics themselves—or, in other words, of primary sources—but rather on the basis of the preexisting work of historians. I seek to show how, in thus using such secondary (or yet more derivative) sources as their main empirical materials, grand historical sociologists are led, willy-nilly, into accepting what historians themselves have for long recognised and criticised as a positivist conception of historiography, and, in turn, into various formidable methodological problems that they have so far often failed to appreciate, let alone resolve.

The first reaction to this essay came in the form of four critical comments by Bryant, Hart, Mann, and Mouzelis that appeared in the *British Journal of Sociology* (vol. 45, no. 1, 1994). These seemed chiefly motivated by sorrow or anger that I had seen fit to question the methodological foundations of grand historical sociology and thus to show disrespect to such apparently iconic works as Barrington Moore's *The Social Origins of Dictatorship and Democracy* (1966). I have nothing to add here to the reply that I made (1994) at the time.

Subsequent reactions have also centred on my criticism of the use of secondary sources in grand historical sociology. In some cases, the attempt has been made to pass this over as being of relatively minor importance. Thus, Calhoun (1996: 312) argues that what I have to say on this matter could 'largely be rephrased as useful advice' to grand historical sociologists to 'take care' over evidence. But he has then nothing to suggest about the specific methodological procedures that might be followed by way of exercising such 'care': that is, procedures of the kind for which, as I note, Skocpol (1984: 382) called—apparently in vain—over 20 years ago. Mahoney and Rueschemeyer (2003: 18) are in effect yet more cavalier in simply asserting that the use of secondary sources 'need not result in any systematic error' because, typically, the full 'population' of sources is covered, and, in any case, the validity of comparative and related theoretical arguments 'does not hinge on a particular reading of the secondary literature'. I cannot see how this argument stands up. What else *could* the validity of such arguments hinge on, where no primary work has been undertaken? How else is the underlying theory to be tested? Moreover, already in my essay I give the examples of Moore (1966), Anderson (1974b), and Wallerstein (1974–89: vol. 1) all neglecting, where they do not unwarrantedly disparage, studies relating to the English Civil War that do not fit with their preferred interpretation of it as a 'bourgeois revolution'; and I would not, I believe, have much difficulty in presenting further, more recent cases where a similar rather blatant partiality arises.[14]

A far more considered response is that of Lustick (1996). In direct contrast to Mahoney and Rueschemeyer's claim of complete population coverage, Lustick formulates the problem of secondary sources as being the expression, in the context of grand historical sociology, of the more general problem of selection bias in data that occurs in one form or another across virtually the whole range of sociological research. Lustick's main critical—and factually correct—observation on my essay is then that, having identified the key issue of how grand historical sociologists should choose among rival or contradictory sources without undue bias, I do not offer any solution to it; and, he speculates (1996: 610), with reference to the *BJS* comments, that this is why my essay has led to reactions 'that have been so defensive and so nearly, in some cases, hysterical'. For unless some intellectually satisfying solution can be provided, 'the whole field is vulnerable'.

Lustick goes on to suggest (1996: 613–15) that at least the beginnings of a solution might be found if researchers dependent on secondary historical materials were to be more explicitly concerned with 'patterns within historiography', as distinct from 'patterns within History', and ready to treat each possible secondary source as a 'data-point' that is subject to error, whether random or biased. I would regard this as a proposal that should certainly be taken further. To do so would at all events serve to bring discussion of the methodology of grand historical sociology under the general rubric of the logic of inference. I would, however, note that the problem of selection bias does in fact occur at two different levels, as Lustick appears at one point to acknowledge: that is, not only at the level of the researcher's choice of secondary sources but further at the deeper level of the 'natural selection' of primary sources during the passage of time. And the extreme, though not uncommon, case that here arises—I give examples in my essay—is where relics from the past of the kind that would be necessary for making certain inferences have simply not survived to any adequate extent.

In the second critical essay, 'Current Issues in Comparative Macrosociology', I seek primarily to question the idea that certain methodological problems that are well known to arise in quantitative work—what I label as the small N, the Galton, and the black box problems—can in fact be avoided or more readily handled via a qualitative, case-oriented approach. These problems, I argue, are in fact met with in qualitative just as much as in quantitative research, and the distinctive methods supposedly available to the case-oriented approach not only fail to resolve them but also—like the standard procedures of grand historical sociology—often carry wider implications of a rather surprising kind. Most seriously, perhaps, attempts to overcome the small N problem (the problem of 'too many variables and not enough cases') by resorting to 'logical' as opposed to statistical methods of analysis must rest on the—strongly positivist—assumption that the social world is deterministic rather than probabilistic in character and can moreover be studied as such: a probabilistic approach is not required even on account of the problems of uncertainty or error that inevitably arise in all processes of data collection.

The essay was published in its original form in a special number of *Comparative Social Research* (vol. 16, 1997), together with comments by Abbott, Goldstone, Ragin, and Rueschemeyer and Stephens, Teune, and Tilly, to which I replied (1997). More than one of these commentators linked my criticisms of qualitative case studies to those previously made by King, Keohane, and Verba (1994) and also by Lieberson (1992, 1994), and thus construed them as part of a concerted attack on this style of research in sociology and political science (cf. also Adams, Clemens, and Orloff, 2005: 24–25). However, what is by now much better appreciated is that what motivates criticism of the kind in question is not a hostility to qualitative research as such but, rather, a commitment to the view that there is 'one logic of inference' (King, Keohane, and Verba, 1994: ix), to which the particular methodological procedures followed in qualitative and quantitative research must *alike* be subject. As I have earlier remarked, the increasing acceptance of this position among leading proponents of case-oriented research is a notable and welcome development.

Furthermore, from both the initial and later reactions to my essay (see esp. Collier, Brady, and Seawright, 2004: 254–55), it is evident that a large measure of agreement exists on at least one major issue. It is common ground that once empirical regularities have been established, theory is then required as a basis for explaining these regularities: that is, in order to overcome the black box problem of otherwise merely 'mindless' correlations and associations. And to this it may be added that the idea of 'process tracing' as a means of determining possible causal relations, which appears widely favoured among case-oriented researchers, has some obvious similarities to that of mechanism-based explanation to which I previously referred.

Nonetheless, some significant disagreement also remains. Arguments advanced in favour of the qualitative case-oriented approach confirm me in the view I expressed, following various other critics (e.g., Kiser and Hechter, 1991, 1998; Levi, 1997), that this approach is inductivist to a quite excessive degree. Rather than treating the description of the phenomena to be explained, the development of explanatory theory, and then the testing of this theory as methodologically separate phases of the research process, proponents of case studies see it as a virtue that they do in fact disregard any such separation. Thus, Mahoney and Rueschemeyer (2003: 13; cf. also 20–21) stress that, in dealing with small Ns, comparative historical researchers can 'comfortably move back and forth' between their historical data and theory 'in many iterations of analysis as they formulate new concepts, discover novel explanations, and refine preexisting theoretical expectations in light of detailed case evidence'. And likewise Ragin (1997: 30–32 esp.) sees the actual constitution of 'positive' and 'negative' cases—that is, the *explananda* of the analysis—in conjunction with 'the reciprocal clarification of empirical categories and theoretical concepts' as being a central and distinguishing feature of qualitative macrosociology.

The standard objection to proceeding in this extreme inductivist way (cf. King, Keohane, and Verba, 1994: 19–23) is, of course, that if the *explananda*, the theoretical explanation, and the evidence taken as relevant to testing the explanation are all regarded as being open to continuous mutual accommodation, it is then difficult to see how any real progress in evaluating theory can be made. The possibilities for adapting, modifying, or otherwise 'saving' a theory in the face of contrary evidence would appear unlimited.[15]

Ragin (1997: 31) does in fact accept that 'in fairness' to both King and his colleagues and me, it should be recognised that the concerns that lie behind our criticisms are with theory testing rather than with 'concept formation, elaboration and refinement', which, for Ragin, are the prime concerns of case-oriented research. In return, I would have to say that *if* such research has indeed no ambition beyond improving concepts, my grounds for quarrelling with it disappear—although I fail to see why such effort should be put into conceptualisation without then *moving on to* the development of theory per se and its empirical testing. Moreover, it is apparent that Ragin's modesty in this respect would not be shared by Mahoney and Rueschemeyer nor, I would believe, by most other practitioners of this style of research.[16] I would, therefore, wish fully to maintain my critique of its undue inductivism; and I would add that what are represented as instances of the progress that it has achieved (e.g., Mahoney and Rueschemeyer, eds., 2003: part I) are in fact regularly open to question precisely because of the failure to allow the three phases of the research process that I would wish to distinguish an appropriate degree of independence.[17]

The third critical essay, 'Sociological Ethnography Today: Problems and Possibilities', can be seen as closely parallel to the second. I start off from current debates among ethnographers occasioned by the reception of postmodernist ideas. But my main concern is again to question, this time in regard to ethnographic case studies, the effectiveness of methods that can, apparently, transcend any logic of inference and that—as some would see it—offer the possibility of sociological ethnography establishing itself as a radical alternative to positivist (read survey-based and quantitative) forms of research. I argue that, as applied to widely recognised problems of what I call variation within and variation across the locales of ethnography, such methods do not work. The problems in question are again ones ultimately of potential selection bias, and solutions to them are likely to be found only through ethnographers adapting to their own purposes the logic of sampling as this has been developed within the survey tradition. Further, I once more illustrate how efforts to avoid recourse to what is deemed to be positivist methodology can in fact lead to the adoption of *ur*-positivist positions: that is, where attempts to justify generalisations from ethnographies of unknown representativeness turn out to depend on a conception of theory as providing certain knowledge of deterministic, lawlike relations. Finally, though, I suggest that sociological ethnography, in a methodologically enhanced form, could, in some instances, take on an important role in the *testing* of theory, and in particular in testing for the presence of causal mechanisms that are specified at the micro-level of individual action and interaction. In this way, ethnography might be brought into both a complementary and a revealing competitive relationship with survey-based research.

This essay has not, so far as I am aware, attracted any published comments of substance. I have, though, received a number of personal communications from sociological ethnographers expressing support for my general position and confirming the need for what critics would wish to label as a 'positivist' turn. One additional point that has been raised in this connection seems to me quite crucial: that is, the importance of ethnographic, and other qualitative, data being as far as possible archived, in the same way as now routinely occurs with survey data, so that they become open to public scrutiny and available for secondary analysis (cf. Corti and Thompson, 2003). Moves in this direction should be facilitated by the extent to which ethnographers now organise their data in a form suitable for computer-assisted analysis. However, resistance to archiving is already evident, including on the—unfortunately—predictable grounds (see, e.g., Parry and Mauthner, 2004) that archival policies and procedures 'are derived from a positivist quantitative model' that does not apply to qualitative material that should be understood as an individual resource and as personal rather than public property. It is difficult to see how such a position can be compatible with the idea of social science; perhaps it is not intended to be.

The fourth critical essay, the most recent in the sequence, is somewhat different in its motivation from the preceding three, although it too has an ultimate methodological concern. I was prompted to write it by what appears to be a growing tendency for social scientists, but especially sociologists, to hold forth on large issues of the day in an ambitious, but often very loose fashion, under the dubious licence of being (or aspiring to be) 'public intellectuals'. I take up one aspect of one such issue—the impact of globalisation on social class—and seek to show that the claims of 'grand' globalisation theorists are empirically ill informed and often have a quite crude and inadequate theoretical basis. Overall, the changes in class inequalities, class structure, and class politics that these theorists associate with an emerging global society are, to judge by more extensive evidence and more rigorous analyses than they acknowledge, far less 'transformational' than they would suppose, and the connection with processes of globalisation is far more problematic. Notions of 'epochal change' of a kind that requires a quite new avant-garde sociology for its comprehension, are not, I argue, to be taken seriously.

As I have maintained elsewhere (Goldthorpe, 2004a), it is indeed important that sociologists show themselves ready to engage with current sociopolitical issues—but, I would argue, as social scientists rather than as public intellectuals who seek authority for their pronouncements more on the basis of *réclame* than of specialist knowledge. To revert to Boudon's point earlier noted, insofar as sociology moves into its expressive or critical modes, it needs to be securely grounded in sociology as social science. It is of interest that of late other authors, including ones more sympathetic than Boudon or I to the idea of the sociologist as public intellectual, have also sought to highlight the problems of sociology and its different audiences that here arise. Thus, for example, Burawoy has argued (2004a: 1609, my emphasis; cf. also 2004b) that 'An effective public or policy sociology is not hostile to, but *depends upon* the professional sociology that lies at the core of our disciplinary field.'[18] I would hope that these problems will become yet more widely debated as the role of sociologists in public life almost inevitably increases.

THE PROGRAMMATIC ESSAYS

The last four essays in this volume I describe as programmatic. Although they take up a number of different substantive issues, their shared purpose is to give some idea of the main elements of the new mainstream sociology that I would wish to see emerge. The essays thus start out from those aspects of research and theory in sociology over recent decades that I would regard as holding most promise, despite the generally unfavourable context for the advancement of sociology as social science that has prevailed.

On the side of research, the most notable achievements have, I believe, been made in quantitative work. New techniques of analysis have been applied to large-scale data-sets deriving from surveys of increasingly diversified and sophisticated design.[19] This has then resulted in the demonstration, in a wide range of substantive fields, of empirical regularities, over both time and space, that were hitherto unrecognised or only inadequately described. On the side of theory, no comparable progress could be claimed. What can, however, be observed is that with the seemingly final collapse of functionalism in both its liberal and its Marxist forms—*fonctionnalisme rose* and *fonctionnalisme noir*—a revival has occurred of what Boudon (1987) calls the 'individualistic' theoretical tradition in sociology. That is, one in which the explanation of social phenomena is sought not in terms of the functional or teleological exigencies of social systems but rather in terms of the conduct of individuals and of its intended and unintended consequences. This revival has been most marked, and, in my view, pursued to best effect, where the emphasis is placed on individual *action* rather than *behaviour* and, further, where the attempt is made to treat action as being in some sense *rational*.[20]

And such an approach has become closely linked with the growing interest in mechanism-based theorising that I earlier noted as a recent encouraging development.

In the first programmatic essay, 'The Quantitative Analysis of Large-Scale Data-Sets and Rational Action Theory: For a Sociological Alliance', my aim is to argue that proponents of these two more promising concerns of contemporary sociology, labelled as QAD and RAT, could with mutual advantage enter into a closer relationship. QAD, I maintain, needs RAT. It is now clear that, as various critics have insisted, statistical techniques, no matter how powerful in revealing social regularities, cannot at the same time be used to crank out causal explanations of these regularities. A theoretical input is essential and on several counts causal narratives grounded in RAT would in this regard seem an especially attractive proposition. Conversely, RAT needs QAD. As critics have also pointed out, if the capacity of RAT to inform effective mechanism-based explanations is to be more convincingly demonstrated than hitherto, it needs to be seen at work in other than apparently handpicked and often 'data-poor' cases. Probabilistic yet wide-ranging regularities of the kind that QAD can establish would therefore appear as highly appropriate *explananda* in relation to which the full range of application of RAT (and at the same time its eventual limits) could be shown up.

This essay first appeared in the *European Sociological Review* (vol. 12, no. 2, 1996) as the lead item in a special number in which various authors considered the prospects for the kind of alliance that I suggest, and it was then reprinted in a collective volume on the same theme (Blossfeld and Prein, eds., 1998). Much commentary on the essay has been positive. I am evidently not alone in believing that through building on the successes of QAD and the potential of RAT, a substantial component of a new disciplinary core for sociology could indeed be created. At the same time, though, I am left under no illusions about the resistance to be overcome if such a project is to make headway—and not only on the part of those who would seek to reject it out of hand as positivism *redivivus*.

Thus, among proponents of QAD there remain those who have difficulty with the idea of taking theory seriously and who would still wish to believe that QAD is able in itself not only to establish empirical regularities but at the same time to provide adequate explanations of them—even if perhaps with the help of a little 'commonsense' interpretation. Likewise, among theorists, whether proponents of RAT or not, there is often a reluctance to accept that they should be ready to take up the explanatory challenges that are posed by descriptive results deriving from QAD, and especially from what is sometimes dismissed as 'merely administrative' research. Thus, Edling (2000: 5–6), in a further comment on my essay, contends that there is 'no general claim on the part of RAT to explain statistical regularities', and would apparently believe that RAT and QAD 'can be connected in a fruitful way' only 'if the collection of quantitative data is guided by a rational choice theoretical framework' in the first place. Edling fails here to distinguish between two different kinds of research: that which would be appropriate to test a RAT-based explanation of some social phenomenon and that which might be necessary to demonstrate the phenomenon for which an explanation is sought. Research of the former kind will obviously need to be 'guided by' RAT, but it would seem quite unduly restrictive and indeed inappropriate to require the same of research of the latter kind.

The second and third programmatic essays follow on from the first and are closely related. In the second, 'Rational Action Theory for Sociology', I start from the observation that RAT comes in fact in a range of different versions, and I attempt to analyse these by reference to three criteria: the strength of the rationality requirements that are imposed, whether the focus is on situational or procedural rationality, and whether the ambition is to provide a theory of action of a special or a general kind. On this basis, I then try to identify in which form RAT would seem to hold out most promise for sociology, and especially for use in conjunction with QAD. I conclude that sociologists are likely to be best served by RAT that draws on a conception of subjective rather than objective rationality and in turn imposes rationality requirements of 'intermediate' strength; that has a strong emphasis on explaining action in terms of its situational rationality (or, in Popper's phrase, its 'situational logic'); and that seeks to be only a special theory of action although one that is still in various ways privileged.

The general objective that I had in writing this essay was to counter the tendency, widespread among sociologists, to equate RAT with the particular versions of such theory that are most commonly found in economics, and in turn to see RAT as in some way alien and threatening to the very nature of the sociological enterprise. However, from continuing critical responses to RAT, whether or not prompted by my essay, it became evident to me that more needed to be done to try to set right this, and indeed a number of other, mistaken views that persist in relation to RAT: for example, that

RAT has some integral connection with neoliberal political ideology or that it entails an unduly restricted and impoverished view of the human individual or person. Hence, the initial motivation for the third essay, 'Rational Action Theory in Sociology: Misconceptions and Real Problems', which is published here for the first time.

As the title indicates, though, my concern in this essay is not simply with dispelling error. I also set out what I would see as quite fundamental problems that arise with the use of RAT in sociology—but to no less an extent in the pursuit of other theoretical approaches as well. These are, I argue, in the last analysis problems of 'nature and culture' and of 'individual and society' that have been encountered throughout the history of the social sciences or of social thought more generally. But what I further argue is that they are not problems that are open to merely conceptual solutions but rather ones of an ultimately empirical character, our understanding of which will be advanced only by research, and that this will often be research in fields other than that of sociology itself. Moreover, in certain respects, if not in others, advances via research are, I suggest, already being made, and advances from which proponents of RAT in something like the version I would favour can take encouragement.

The fourth and final programmatic essay, 'Causation, Statistics, and Sociology', may appear to be concerned with an issue somewhat removed from those previously considered: that is, that of how the idea of causation is, and might best be, applied in the context of sociological analysis. In fact, this essay can, I hope, help to highlight and integrate certain themes that recur throughout all the preceding essays in this volume.

I distinguish three different understandings of causation deriving chiefly from the work of statisticians. The first, 'causation as robust dependence', is that associated with attempts to make causal inferences through statistical technique alone, without theoretical or other subject-matter input. The second, 'causation as consequential manipulation', is that associated with experimental designs, and especially in applied sciences, where attention centres on assessing the effects of (given) causes—that is, treatments or other interventions—rather than on determining the causes of effects. The first understanding is, I argue, by now outmoded in sociology, and the second, although capable of powerful technical elaboration, is not well suited to a subject in which research must for the most part be nonexperimental and, moreover, in which the concept of action is central. The third understanding of causation that I identify, that of ‘causation as generative process’, I find far more attractive. In this case, the key idea is that advancing a causal explanation of phenomena that are taken to be evident in a set of data means giving an account of some underlying process that would in fact be capable of bringing the phenomena into being: that is, a process operating at a deeper or more micro-level than that at which the relevant data are themselves observed.

This understanding of causation does then fit well with the mechanism-based, micro-to-macro style of sociological explanation that I would generally favour, and I take it as a basis for proposing an approach to causal analysis that would seem especially appropriate for use in the context of a QAD-RAT alliance. This approach can in fact be represented schematically through the three-stage sequencing of the research process that I already indicated in my critique of excessive inductivism in qualitative macrosociology. The first stage is that of establishing the phenomena: that is, of demonstrating the social regularities that constitute the *explananda*, with statistical techniques here being used in an essentially descriptive mode. The second stage is that of hypothesising generative processes at the level of action that have explanatory adequacy and that are of a theoretically grounded kind—as, say, in the form of RAT-based narratives. And the third stage is then that of testing the validity—the actual applicability—of the explanations that are thus advanced, using as wide a range of strategies, direct or indirect, and of research methods and analytical techniques, quantitative or qualitative, as can be effectively brought into play.[21]

This essay has in general been well received (see, e.g., the special number of the *European Sociological Review*, vol. 17, no. 1, 2001) and by statisticians as well as sociologists. In addition to recapitulating some key arguments from the critical and programmatic essays of this volume, I would see it as providing a link between these essays and those intended to illustrate the general position that I have taken up which form the larger part of Volume II. Through such illustration, the importance to sociologists of issues of how they should understand—and implement—the idea of causation in their work may, I hope, become more apparent.