CHAPTER ONE

Introduction to Volume II

The essays brought together in Volume I of this book were concerned with the present state of sociology. This state I had to regard as one of general intellectual disarray—although with some more encouraging developments being very recently discernible. In line with such an assessment, the essays were of two kinds: critical and programmatic. Taken together, these essays form in effect an extended prolegomenon to those of Volume II; and, thus, a brief recapitulation of their main themes may here be helpful.

FROM VOLUME I TO VOLUME II

The critical essays of Volume I focus on basic methodological difficulties that, I believe, arise with certain current styles of sociological work. I single out what I label as 'grand' historical sociology (to which may be assimilated 'grand' treatments of globalisation as representing, say, 'epochal transformation'), case-oriented macrosociology, and sociological ethnography. These are all styles of sociology that involve processes of data collection and analysis that are primarily qualitative in character; and it might then be thought that my aim in these essays is to launch an all-out attack on such qualitative research. This is not in fact the case. Rather, I try to establish three specific points.

First, I seek to demonstrate that the qualitative styles of sociology in question do face methodological problems—and ones that their proponents have to a large extent neglected. These are problems ultimately of the reliability and validity of the kinds of data on which chief reliance is placed and of the ways in which such data are used in processes of theory construction and evaluation. In their essentials, such problems are also encountered in quantitative work. But while quantitative sociologists have, over time, evolved various techniques, grounded in statistical theory, through which these problems can at all events be assessed and managed, even if never entirely resolved, sociologists committed to qualitative work have been far more inclined simply to disregard or discount them or to beg off from the technical efforts for which their treatment calls.

Second, insofar as qualitative sociologists are in fact ready to recognise the methodological difficulties that confront them, they tend, I try to show, to look for ways of coping with them that would in some sense be distinctive to their own style of work, and that would in particular allow them to avoid any complicity in 'positivist' (read statistical) approaches. However, the special procedures that are suggested are not well elaborated or codified, they often lack transparency, and, insofar as they are made explicit, seem open to rather obvious objections. Furthermore, and quite ironically, they appear in various respects to lead their proponents into positions that are in fact of an *ur*-positivist kind. This occurs, for example, when grand historical sociologists seek to justify their empirical reliance on secondary sources by envisaging historical facts not as—often challengeable—inferences from incomplete and possibly biased primary sources but rather as discrete and stable 'items' that can be excerpted and reassembled at will as the building bricks of their wide-ranging inductivist constructions; or again when case-oriented macrosociologists or sociological ethnographers seek to overcome difficulties of generalising from insufficient or possibly quite unrepresentative data by resorting to entirely deterministic logical methods of analysis or by invoking theory that, supposedly, allows certain knowledge of lawlike relations.

Third, I underwrite the argument previously made by King, Keohane, and Verba (1994) that all styles of social research, whether qualitative or quantitative in character, have to respect a similar logic of inference: that is, a similar logic governing the way in which one may properly move from data about the world to claims that go beyond the data, whether in a descriptive or an explanatory mode, or, one might say, a logic that governs the interplay of evidence and argument. Such a logic can in fact be regarded as applying to all forms of science and scholarship. It does not dictate any particular set of research strategies or techniques but has rather to be expressed through methodologies elaborated in ways that are found appropriate to different subject-matter areas and substantive issues. Sociology in its more quantitative forms uses methods that are grounded in advances made in the statistical analysis of observational data from the later nineteenth century onwards—which can be understood as extensions of the logic of inference as expressed in the experimental methods of the natural sciences. And what, then, I wish chiefly to maintain is that it can be fairly required of practitioners of more qualitative forms of sociology that they too should attempt further extensions of this logic that they would regard as appropriate to their particular research fields and interests. As I recognise in the Introduction to Volume I and elsewhere, there are at the present time welcome indications that movements in this direction are indeed occurring—although ones still contested by those who would rather see qualitative sociology grounded in some form of relativist epistemology or who believe that it should be judged more by literary than by scientific standards.

In the programmatic essays of Volume I, I then aim to outline, and to give a rationale for, the kind of sociology that I would wish to see emerge as a new mainstream—to serve not as a basis of exclusion but rather as an exemplar of shared standards, in relation to which methodological discussion and debate can be carried on expressive of a genuine pluralism rather than of a merely convenient, 'anything goes' counterfeit. I am naturally influenced here by what I would judge to be, on the one hand, those respects in which sociology has thus far been most successful and, on the other hand, those in which potential for the future is most apparent.

On the side of research, I believe that the most notable achievements have been made in quantitative work: in particular, in the demonstration, through the analysis of large-scale, usually survey-based, data-sets, of a wide range of empirical regularities, often extensive in time and space, that were hitherto unrecognised or only inadequately described. I thus maintain that such work will, and should, remain central to sociology, and that continuing progress can be expected as survey research becomes more diverse in its designs, as quantitative techniques become more powerful and refined, and as data collection and analysis alike become more specifically adapted to sociological concerns.

On the side of theory, it is not possible to claim any similar record of achievement. However, encouraging developments can, I believe, be observed in the revival, following on the apparently final collapse of functionalist thinking, of what Boudon (1987) calls the 'individualistic tradition' in sociology; and also in a reassertion of the idea that theory does not exist, as it were, for its own sake but must serve the primary task of providing a basis for explanation. In particular, I see major potential in the expression of the individualistic tradition via rational action theory—used as a more general term than rational choice theory—and in the context of what I describe as mechanism-based theorising: that is, theorising that attempts to explain phenomena of interest not by subsuming them under general, covering laws but rather by identifying the causal processes, or mechanisms, through which they are generated and sustained and perhaps changed or disrupted.

The programmatic essays have therefore two main concerns. First, they aim to bring out the essential complementarity of the quantitative analysis of large-scale data-sets and rational action theory. The quantitative analysis of data (QAD) can provide increasingly sophisticated descriptions of empirical social regularities but is not in itself capable of also providing explanations of them. Causal accounts cannot be simply cranked out of statistical analyses; a theoretical input is also needed. And rational action theory (RAT) appears as an especially apt basis for the specification of the patterns of action and interaction out of which relatively large-scale social regularities are produced. Moreover, through the development of such generative models and their subsequent testing by means of further empirical research, the explanatory potential of the underlying theory can be evaluated, and especially insofar as it can be set in competition with that of other, rival theoretical approaches. In this way, then, as I seek to show, the possibility is created of overcoming what I refer to as 'the scandal of sociology', the manifest lack of integration of research and theory, and thus of achieving an intellectually more coherent discipline.

The second concern of the programmatic essays of Volume I is then with trying to clarify RAT, as I would wish to understand it, and to remove misconceptions that appear widespread and persistent about such theory and, more generally, about the individualistic tradition within sociology, of which RAT is one major expression. I aim to show that it is a mistake to equate RAT simply with those versions of it that are most commonly found in economics, and in turn to regard RAT, and the principle of methodological individualism that it embodies, as alien and threatening to the very nature of the sociological enterprise. Rather, RAT in different versions and the individualistic tradition itself have deep roots in the history of sociology. A form of RAT developed so as to meet the particular requirements of presentday sociology—around the idea of subjective and bounded, rather than objective and infinite rationality—offers, I suggest, a means of overcoming the long-standing opposition between the explanation and the interpretation of social action and also the best prospects for making headway in regard to various other basic problems of sociological theory.

Turning now to the present volume, the essays I include again fall into two kinds: six that I label as illustrative and the two final essays that I label as retrospective. In the remainder of this introductory chapter I discuss these two sets of essays in turn, beginning in each case with some general observations that aim to show their continuity with those of Volume I and then going on to comment on the essays separately so as to bring out their more specific contexts and motivations.

THE ILLUSTRATIVE ESSAYS

The six illustrative essays follow on directly from the programmatic essays of Volume I: that is to say, they are intended to show what the kind of sociology that I there envisage and advocate might look like in practice, and in particular to indicate how the closer integration of research and theory that I believe such a sociology makes possible can actually be realised. However, while the essays have this broad purpose, they do in fact relate to one particular research field in sociology: that of social stratification and mobility, in which my own expertise and experience chiefly lie. I naturally hope that they will be of interest to other specialists in this field, and that they will not prove too off-putting to sociologists with different interests. The latter will no doubt wish to ask themselves whether similar illustrative material could be derived from their own research fields—and to consider the implications, positive or negative as they may see them, of the answers they arrive at. The essays, as will be discovered, vary a good deal in their form and content, but they do, I hope, achieve a certain unity in the three following respects.

First, each essay aims to bring out, though with differing emphases, the potential for the interplay between research and theory that I would see as following from the three-phase schema discussed at various points in Volume I (see ch. 9 esp.). This comprises:

1. establishing social phenomena to be explained in terms of the empirical regularities through which they are manifest,

2. hypothesising processes or mechanisms at the level of individual action adequate to generate these regularities, and then
3. testing the validity of the explanations thus advanced by means of further empirical enquiry.

Second, and again following on arguments advanced in Volume I (see esp. chs. 6 and 7), those features of social stratification and mobility that are taken as phenomena to be explained are ones established primarily through the quantitative analysis of large-scale data-sets, and the generative processes that are suggested as adequate to their explanation are ones couched in terms of rational action theory.[1] At the same time, though, RAT is taken as only a special, if privileged, theory of action and is thus always seen as being in competition with other theories of action or of 'social behaviour'. And further the possibility is kept open of other kinds of empirical inquiry than QAD being used in the testing of theoretical explanations.

Third, the regularities empirically established in the field of social stratification and mobility on which theoretical attention primarily focuses are ones of a particular kind. They are regularities, of a macrosocial character, that take the form of relative constancies over time and commonalities across sociocultural contexts rather than regularities expressing variation in the form of secular trends or systematic cross-societal or cross-cultural differences.

I might add that while this last feature is to some extent a matter of accident—in the recent past the more notable regularities demonstrated in research in social stratification and mobility have been ones of relative constancy and commonality—the accident is rather fortunate. There is advantage to be gained from concentrating on regularities of the kind in question for reasons that have been well set out by Lieberson (1987: 99–107). To begin with, the reliance of much quantitative analysis on regression methods creates the danger that the study of variation, of one kind or another, is privileged simply because it is variation that this statistical technology is designed to handle. Variation in the phenomenon of interest is presupposed and is then accounted for in terms of independent or 'explanatory' variables. But in the case of phenomena that are characterised by little variation, their treatment via such methods is problematic, and they may therefore be neglected.[2] Moreover, it is entirely possible that the processes or mechanisms that underlie the very presence of some phenomenon are different from those that underlie such variation as it may display. And, as Lieberson argues, we may then be seriously misled if we try to deal with causes of variation without first having an adequate appreciation of 'fundamental' causes—that is, the causes of the phenomenon per se. Thus, to take examples from what is to follow, we need to have some theoretical understanding of why an association exists and persists between children's class origins and their educational attainment or between their class origins and the class positions that they themselves eventually attain *before* we attempt to explain why—should this appear to be the case—this association is strengthening or weakening over time or differs in its strength or pattern from one society or culture to another.[3]

The illustrative essays are then chiefly concerned with aspects of the interplay of research and theory in a particular context of enquiry. If they are found lacking in some desirable features of the essay form, such as elegance of composition and a sense of completeness, this may reflect, I would like to think, not just my own inadequacies in writing expository prose but also the inherent messiness of what one finds on, as it were, the edge of any attempt at bringing research and theory together. Uncertainties can and do arise over the precise nature of what is to be explained, over just what the explanatory theory advanced does and does not claim, over what exactly would count as corroborating or disconfirming evidence, and so on. I have not tried to cover up such uncertainties or the difficulties to which they give rise. Many issues are left open and problems unresolved, and the attentive reader will no doubt find more weak spots and loose ends in the arguments put forward than I have myself appreciated. I can only plead in mitigation that the essays are essentially concerned with giving an account of sociology in the making—a process that is in its nature open-ended and provisional.

At the same time, though, I do not wish here to take up a too defensive position. Even if the essays do reveal the more seamy underside of work in progress, they also, I submit, provide clear enough evidence that a sociology of the kind that they are intended to illustrate is in fact capable of actually *achieving progress,* and not just in extending our empirical knowledge of social phenomena but in developing theoretical understanding as well. In a thoughtful contribution, Cole (1994) has argued that while on the periphery or 'research frontier' there is little difference in the way in which the social and the natural sciences proceed, sociology, at least, falls behind the natural sciences in its ability to convert new knowledge produced on the—often untidy and disputed—frontier into 'core' knowledge that is generally accepted as valid. It would be difficult to deny the descriptive force of this argument. But, as I think Cole would agree, the problem is not entirely insurmountable: sociology is not, by the very nature of its subject matter, prevented from producing cumulative knowledge, as the 'impossibilists' to whom I refer in the introductory chapter to Volume I would suppose.[4] And, as I have sought to show more fully elsewhere (Goldthorpe, 2005; and cf. Hout and DiPrete, forthcoming), the study of social stratification, and of social mobility in particular, can count as one area in which new knowledge has been steadily gained *and* has, in some part, been formed into what is recognisable as core knowledge.

One last general comment may be made, with American readers chiefly in mind. Although I have not, I hope, neglected relevant American literature, a good deal of the work that has influenced my own, and with which I engage, is European. The essays may then serve the further purpose of increasing transatlantic awareness of what have, I believe, been significant, even if still minoritarian, developments in European sociology over recent decades. On the one hand, the style of micro-to-macro, and primarily RAT-based, explanation that is highlighted, while of course having important American origins, especially in the work of Coleman (1990), would seem of late to have been pursued with greater enthusiasm and effect in Europe than in the United States. And, on the other hand, a substantial part of the quantitative empirical work on which I draw is of a cross-national comparative kind, the growth of which in Europe has in various ways been promoted by post-1989 political events and is today underpinned by levels of funding for both research and organisational support that American colleagues may well find enviable.[5]

These developments are, moreover, being matched by an expanding professional infrastructure. The European Consortium for Sociological Research, established since 1991, has a present membership of over 50 research institutes and university departments with active research centres, holds regular conferences on comparative European sociology, and sponsors the *European Sociological Review*. In addition, the European Academy of Sociology, founded in 2000, under the presidency of Raymond Boudon, has a strong representation of scholars with a general commitment to the individualistic tradition of sociological analysis and an interest in sociological applications of RAT. Through these bodies in particular, an increasingly favourable context is being provided for the advancement of sociology as social science, in the interests of which the illustrative essays are written.[6]

Considering these essays in more detail, the first three could be said to form a closely related trio. They are all concerned with regularities that have been demonstrated in social class differentials in educational attainment in modern societies and with the explanation of these regularities.

The first essay, 'Class Analysis and the Reorientation of Class Theory: The Case of Persisting Differentials in Educational Attainment', begins with an attempt to locate the problem of educational differentials in a larger context. I argue that until recently class theory, whether Marxist or liberal in inspiration, has been preoccupied with the dynamics of class and in turn, and rather strangely, with developments that have not in fact taken place: in the Marxist case, with class formation, intensifying class conflict, and the emergence of working-class revolutionary politics; in the liberal case with class decomposition and the emergence of a 'classless' form of society. Somewhat more relevantly, I suggest, the central *explananda* of class theory could rather be seen as various well-established regularities that point to the *stability* of class in modern societies or, at all events, to the powerful resistance to change that class relations and associated inequalities in life chances and differences in patterns of social action would appear to display.

I then take continuing class differentials in educational attainment, despite a general expansion of educational provision and increases in overall levels of educational qualification, as providing a major example of such resistance to change—in contrast, for example, to the very rapid decline, if not reversal, of gender differentials previously favouring males. I go on to outline a RAT-based account adequate to explain the degree of persistence of class differentials: that is, an account that shows how this aggregate outcome results from central tendencies in educational decision-making by children and their families that can be understood as rational, given their differing class situations and the nature of the opportunities and constraints that characterise these situations. I further indicate how this account may be appropriately extended to one of the more obvious deviant cases, that of Sweden, where a decline in class differentials in educational attainment over a fairly lengthy period has in fact been demonstrated.

In the second essay, 'Explaining Educational Differentials: Towards a Formal Rational Action Theory', which is coauthored with Richard Breen, the theoretical argument of the first essay is developed and, as the title indicates, is given a more formal, mathematical expression. Such formalisation is still rather rare in sociology and is here undertaken in a largely experimental spirit. Formalisation can, however, undoubtedly serve to bring out the full implications of a theoretical argument and thus to increase both the coherence with which it is stated and the extent to which it becomes open to empirical test. In the present case, for example, we were helped to see, and to spell out, more clearly the crucial part played in our explanation of persisting class differentials by two ideas: (1) that some degree of perceived risk attaches to children continuing in education rather than leaving or, more generally, in making more rather than less educationally ambitious choices, and (2) that this risk tends to be greater in the case of children from less rather than more advantaged class backgrounds, although *equal relative* risk aversion can be supposed in regard to the common goal of avoiding downward social mobility.

The general explanation of how educational differentials are created, sustained, and in some cases reduced that is put forward in these two essays has attracted an encouraging amount of attention, both in the form of critical discussion of the type of theory involved—that is, RAT—and, more important, in the form of attempts to test the explanation through further empirical enquiry. In the third essay, 'The Theory Evaluated: Commentaries and Research', which is published here for the first time, I aim to review these differing responses and to assess their significance. As regards RAT, I remain convinced that, where based on the idea of subjective and bounded rationality, this is in fact the type of theory that can most appropriately be pursued. It allows educational choice to be seen as action guided by perceived costs and benefits in the context of given opportunities and constraints, rather than as merely socioculturally conditioned behaviour, while at the same time not supposing the infinitely rational expectations of the standard economics treatment of educational choice. As regards the specific claims of the theory that Breen and I have developed, I conclude, first, that a good deal of evidence has been produced that is consistent with the operation of the key mechanism of risk aversion that is invoked, but, second, that what is so far lacking is evidence of a more direct kind that it is indeed this mechanism that is crucially at work. In this connection, I note the problem of an adequate research methodology for reliably establishing individuals' goals and expectations—for example, about what level of employment they aim to achieve and about what level of education they see as being necessary for this. I suggest that further progress in evaluating the theory may depend on how far this problem, to which there would seem analogues in many other contexts, can be overcome.

The fourth illustrative essay, 'Social Class and the Differentiation of Employment Contracts', may seem to mark a rather abrupt change of focus. However, it relates to those preceding it in the following way. The 'class schema' from which the essay starts out has become widely used in research in the field of social stratification and mobility, including in studies of class differentials in educational attainment, and has, I believe, both empirical and theoretical advantages in these respects over classifications or scales of 'socioeconomic status'. The understanding of class that informs the schema is in fact directly reflected in the explanation of educational differentials that Breen and I advance: in particular, in our stress on the importance of class differences not just in current levels of income but also in security and stability of income and in long-term income prospects—which the class schema can be shown to reflect.

The conceptual basis of the schema is the definition of class positions in terms of employment relations, but on practical grounds, it is actually implemented in research through information on individuals' employment status and occupation. The question does then arise of how far, when thus implemented, the schema captures those differences in employment relations that, conceptually, it is supposed to capture—or, more technically, the question of its criterion validity. In fact, empirical analyses that have been made to test the schema in this regard, especially in connection with its adoption as the basis of a new official social classification for the UK, have given generally encouraging results (Rose and O'Reilly, eds., 1997; Rose and O'Reilly, 1998; Rose and Pevalin, eds., 2003; Rose, Pevalin, and O'Reilly, 2005). What this means, therefore, is that a fairly systematic association exists between individuals' employment status and occupation, on the one hand, and, on the other, the kind of employment relations in which they are involved as indicated by form of payment, perquisites, control of working time, employment security, promotion opportunities, and so on. And in turn, therefore, the further question of evident sociological interest can be raised of why this should be so. Why, especially in the case of employees (as distinct from self-employed persons), should those in different occupational groupings have their employment regulated in such differing ways?

I suggest an explanation of this empirical regularity—that is, in effect an explanation of why classes exist—in terms of employers' rationally motivated attempts to deal with problems of the employment contract as these arise in the case of employees engaged in different kinds of work and, specifically, with the problems of work monitoring and human asset specificity. Although, ideally, employers might wish to reduce all employment contracts to simple money-for-effort spot contracts—or in effect to 'commodify' labour—these problems mean, I argue, that approximations to spot contracts are likely to meet the needs of organisational effectiveness only with rather basic forms of labour, and that in the case of professional and managerial employees especially contracts with a quite different rationale are typically required. This leads therefore to the prediction that the differentiation of employment contracts will continue on its present pattern to a far greater extent than much fashionable discussion of 'the future of work' would suggest. In developing a RAT-based explanation in this case, I am more influenced than elsewhere by current theory in economics, but chiefly of a kind that shows notable divergences from orthodox utility theory and in ways that in fact bring it closer to RAT in the form that I would see as especially appropriate for sociology.

The next essay, 'Class Analysis: New Versions and Their Problems', does indeed represent something of a diversion but, I hope, a worthwhile one. It can be read as my response to other recent attempts to reformulate class analysis that have been made in addition to, and in part in critique of, my own, although out of very similar concerns for the closer integration of research and theory. In particular, I seek here to develop relatively brief comments that I have earlier offered on the work of Aage Sørensen and David Grusky (Goldthorpe, 2000, 2002a). While not Marxists themselves, both these authors regard class analysis as having more specifically Marxist origins and objectives, and, for this reason, as currently facing more severe challenges, than I would myself be ready to accept. Consequently, their proposals for the renewal of class analysis—advanced from sometimes apparently similar but in fact quite divergent 'neo-Ricardian' and 'neo-Durkheimian' positions, respectively—are, in my view, unnecessarily radical. Sørensen would wish to make the prime focus of class analysis the study of conflict among social collectivities over differing kinds of rent, while Grusky urges that the level at which analysis is undertaken should be 'ratcheted down' from that of 'aggregate classes', the mere constructs of sociologists, to that of specific occupational groupings, meaningful to their members and thus a far more likely basis for the formation of real sociocultural entities and for collective action of any kind.

The research programmes that follow from Sørensen's and Grusky's proposals have, I believe, significant potential. Rent-seeking activity on the part of different collectivities is indeed widespread in modern societies and has been neglected by sociologists—and, among rent-seeking collectivities, occupational groupings figure very prominently. Furthermore, some occupations no doubt do represent sociocultural entities exerting a powerful influence over their members' social identities and patterns of action, and we need to know more about which they are and whether, overall, such 'occupational communities' are becoming more or less important. However, while I would see these programmes as providing valuable complements to class analysis as more conventionally understood, I aim in my essay to show that they cannot serve as substitutes for it. I argue, on conceptual and empirical grounds, that under the new versions that Sørensen and Grusky envisage, class analysis would in effect be virtually displaced from the field of macrosociology. The very idea of a class structure becomes problematic; serious difficulties in turn arise in studying class effects as opposed to occupational and other more sectional effects—and including the effects of class origins on class destinations or, that is, intergenerational class mobility; and such class action as may occur at a societal level rather than in more localised contexts cannot be adequately accommodated.

In the final illustrative essay, 'Outline of a Theory of Social Mobility', I return to my central concern with wide-ranging social regularities, established through quantitative analysis, and their explanation. A substantial body of research by now exists to show that within modern societies relative rates of intergenerational class mobility have a surprising degree of constancy over time, and also that a large commonality at least in the pattern if not the level of these rates exists across societies. I extend theoretical arguments already introduced in the preceding essays to provide an account of these features of 'endogenous' mobility regimes and also of the part that education plays in mediating mobility. I argue that mobility regimes are conditioned by the nature of the class structures of modern societies and, in particular, by the systematic inequalities in resources that they create, but that regularities in relative mobility rates derive more immediately from the mobility strategies that are typically pursued by individuals of differing class origins. These strategies are understandable as rational adaptations to the opportunities and constraints that characterise different class situations. However, especially in their interaction with the selection policies of employers in regard to different kinds of work, they can, and quite typically do, have the overall effect of maintaining the state of intergenerational class competition for more or less desirable class positions largely unaltered over time.[7]

An implication of this account is, then, that inequality of *opportunity*, as reflected in relative mobility rates, is only likely to show substantial temporal change or cross-national variation in association with corresponding change or variation in inequalities of *condition* among the members of different classes. This implication, I argue, is borne out by the results of research in at least some national cases, such as that of Hungary during the Communist era or of Sweden under social democratic hegemony from the 1930s through to the 1970s, in which clear, if not continuing, shifts towards greater social fluidity followed on significant reductions in class inequalities of condition brought about by political means. In turn, I would expect that insofar as trends towards greater class inequalities in income, as recently evident in the UK, the United States, and elsewhere, persist, and at the same time social welfare provision becomes less redistributive, then instances of significantly *decreasing* social fluidity will become apparent—an expectation that currently emerging research findings would appear to bear out.[8]

THE RETROSPECTIVE ESSAYS

The two long retrospective essays with which Volume II ends are intended as a coda to the book as a whole. They are retrospective in the sense that they seek to answer a particular question that refers to the history of sociology but that is posed, quite explicitly, from the standpoint of the present. This question is one that may indeed have occurred to readers at some stage in this or the preceding volume. I argue, from several different standpoints, in favour of a sociology that attempts to combine the quantitative analysis of social data, and especially of data extensive in time and space, with explanation of the empirical regularities thus revealed through a theory of action, and especially one in which rational action is privileged. But if a sociology in this style has the potential that I would like to suppose for the advancement of sociology as social science, why then has it taken so long for it to emerge or even to be explicitly proposed?

In the history of sociology concerns with furthering the quantitative study of social phenomena and with elaborating and applying a theory of action have in fact been largely pursued in isolation from each other. Is it therefore the case that this historical experience reflects some inherent incompatibility between the two key components of the kind of sociology I would favour that I have simply overlooked? Or has their failure to come together been a matter largely of various unfavourable circumstances, intellectual or institutional, that have tended to recur?

The two essays in which I take up this general question are not then ones of pure historical scholarship but rather examples of 'history with a purpose'—which always carries the danger that present concerns are read back into the past in a quite anachronistic way. I have to suppose that I avoid this danger sufficiently for my question still to be meaningfully addressed.

At the same time, the two essays do, I believe, also serve to throw further light on transatlantic differences in the development of sociology—in this case, in development of a long-term kind—that, as will be seen, are of direct relevance to various issues previously discussed in this volume. These differences are in fact especially apparent if due attention is given to the history of research as well as of theory or 'social thought' more broadly, and in particular to the hitherto rather neglected relationship between sociology and one of the major scientific developments of the later nineteenth and earlier twentieth centuries—the probabilistic revolution and the creation of modern statistics. In concentrating on this relationship, and on associated questions of the integration of sociological research and theory, in comparative perspective, the essays may, I hope, lend some support to the efforts of those scholars on both sides of the Atlantic, such as Martin Bulmer, Charles Camic, Anthony Oberschall, and Jennifer Platt, who have produced pioneering historical studies in this area.

In the first of the two essays, 'Sociology and the Probabilistic Revolution, 1830–1930: Explaining an Absent Synthesis', I review selected features of the history of sociology in its formative years in France, England, and Germany. As its title indicates, the main focus of the essay is on the changing relation between emerging sociology and the more or less concurrent probabilistic revolution in scientific thinking. I show that in the initial stages of this revolution sociology represented a research field of major importance, chiefly on account of the work of Quetelet; but that by the time that the revolution culminated, around the turn of the century, in 'the new English statistics', sociology had lost this centrality, even as its own development created a growing need for the kind of theoretical and methodological support that the new statistics could have provided. I conclude, however, that the 'absent synthesis' has to be explained essentially in terms of barriers—of in fact an intellectual more than an institutional character—that were specific to the time and places in question rather than necessary, and of other yet more contingent, difficulties.

Rather ironically in view of the more recent identification of quantitative sociology with positivism, by far the most serious of the intellectual barriers turns out to be the positivistic conception of science, as upheld by Comte and his followers, which was inimical to probabilistic analysis and to the development of a theory of action in equal measure. In some instances, the barrier of positivism was reinforced by that of an undue empiricism—a 'cult of the facts' that recognised little need for theory of any kind; and in others, by persisting confusion over the principle of methodological individualism in sociology and 'psychologism'.

The second essay, 'Statistics and the Theory of Social Action: Failures in the Integration of American Sociology, 1900–1960', then addresses the same question as the first but in a new context. Over the first half of the twentieth century, there was in general a greater readiness in American than in European sociology to exploit the widening possibilities in the analysis of social phenomena that the advance of statistical methods afforded. However, problems clearly remained over the part that quantitative work was to play within the larger sociological enterprise and, in particular, over its relationship to theory. I treat these problems by examining three episodes in the history of American sociology in the period in question that, I believe, are especially illuminating: these relate to sociology at Columbia from 1900 to 1929, at Chicago from 1927 to around 1935, and at Columbia again from 1940 to 1960.

On this basis, I find in fact important similarities with the European situation that I previously considered. Again, I would argue, there was no inherent difficulty in the way of a closer integration of the statistically informed analysis of social data and the attempt to explain the regularities thus revealed from the standpoint of a theory of social action. Rather, movement in this direction was largely frustrated by much the same intellectual barriers as I identified in the European case and by ones that, at least in the case of positivism and empiricism, can be regarded as simply historical variants of the latter. However, it is with psychologism that I see the most serious difficulties as arising, and especially in regard to the development of theory. A concern to ground or indeed embed sociological theory in psychology, whether behavouristic in character or mentalistic in the more indigenous American style of 'the psychology of social life', was not conducive to the reception of a Weberian idea of social action that seeks only minimal psychological foundations. Thus, in America, insofar as a theory of social action—as distinct from one of social behaviour—came to be elaborated, the emphasis was placed on the origins and constitution of action in highly contextualised microsocial situations rather than on the development of accounts of 'central tendencies' in action operating across differing contexts, and thus adequate to explain empirical regularities quantitatively established and analysed at a macrosocial level. The essay ends with the suggestion that the problems of the integration of research and theory that are revealed in the historical episodes examined have by no means been resolved in American sociology today.