The Uses of History in Sociology: A Reply

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John H. Goldthorpe

The uses of history in sociology: a reply*

INTRODUCTION

The form of my reply to the preceding four papers is conditioned by the large variation that they show in their content and quality. Hart and Mann for the most part simply fail to engage with the main thrust of my original arguments. The strategy of their responses appears to be based, first, on the gross mis-statement of these arguments and, secondly, on seeking to shift attention from the issues I raised by launching largely irrelevant attacks on my own work. I have therefore to begin with one section on ‘Misrepresentations’ followed by another on ‘Diversions’. Bryant also starts from a misapprehension of my position and devotes a good deal of his paper to establishing claims, the bearing of which on my position is not altogether apparent to me. Finally, though, Mouzelis, in the shortest piece of the four, does focus consistently on the central questions that I addressed, and I am grateful to him for some assurance that I am still capable of writing comprehensible English. I can then, fortunately, have a third section, mainly devoted to Mouzelis’s paper and to the latter part of Bryant’s, which is headed ‘The Real Issues’. I may add that I am further grateful to Mouzelis, and to Bryant, for conducting a vigorous debate without, however, allowing personal animus to intrude.

MISREPRESENTATIONS

(i) History, sociology, historical sociology and macrosociology

The starting point of my (1991) paper (Goldthorpe 1991: from this point on all page references to this paper will appear in square brackets) was with the claim Abrams (1980:x), following Giddens (1979: 230) that history and sociology ‘are and always have been the same thing’ — that neither logically nor methodologically can any meaningful line of division be drawn between them. This claim I sought to challenge. There was still force, I suggested, in the
distinction between idiographic and nomothetic disciplines, if understood as one of emphasis rather than of principle. But my main concern was to show that history and sociology necessarily differed in the nature of the evidence that they used or, more precisely, in the ways in which this evidence came into being.

For Mann, it then appears, my attempt to deny that history and sociology are 'the same thing' can only imply that I seek to establish 'a massive separation' between them (51). For example, Mann asserts that I accept the view 'that history is the study of the particular' (40, emphasis in original); that I wish to regard sociology as being limited to 'the study of the present' (38; and cf. also Hart, 21); and that I insist that 'only the professional historian has access to the relics' (43). All this is nonsense — as anyone who goes back to my paper will find. I emphasize that historians cannot hope 'to produce descriptions that are free of general ideas about social action, process and structure' [212]; I set out the circumstances in which I believe that sociologists have of necessity to resort to historical research [216]; and I make no suggestion whatever that relics should be the sole preserve of the historian. My argument concerning the differences in evidence available to historians and sociologists is that the latter have the possibility of engaging in research that itself generates evidence, 'over and above' [214], not as distinct from, that of exploiting relics. Sociologists, I quite explicitly state, 'can, and often do, draw on relics as evidence, in just the same way as historians' [214]. Furthermore, I discuss at some length the work of Michael Anderson (1971) and Gordon Marshall (1980) precisely as instances where a recourse to historical research by sociologists was required — since what Mann would call the 'analysis of origins' was involved.

I must also refer to this discussion in response to Bryant's contention that, although I raise 'several serious objections to the current revival of historical modes of analysis in our discipline' (3), 'the case against historical sociology' is in the end not made (15). Of course it is not made — because it was never my intention to do so. How could it be, when I invoke the studies of Anderson and Marshall 'as leading examples of how in fact historical sociology should be conceived and executed' [217]? To be sure, I go on to criticize what I refer to as 'a kind of historical sociology clearly different to that represented by the work of Anderson and Marshall' [219] — that is, 'grand historical sociology'. But to discriminate within historical sociology is scarcely to reject it.

If Bryant elides the distinction between 'grand historical sociology' and historical sociology tout court, Mann, it should be noted, makes a yet more blatant attempt at argument by pre-emptive definition: that is, in claiming that the tradition of grand historical sociology is what 'Americans call' macrosociology (39). Americans — and not only they — do of course call various other things macrosociology as well, and
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with no less justification. But Mann thus paves the way for his finest moment. While contending – quite groundlessly – that I advocate 'a massive separation' between history and sociology, he then (46) refers to the several points in my paper where I am indeed concerned to point out the necessary interrelations between the two disciplines, and triumphantly announces that ‘Goldthorpe concedes the entire case for macro-sociology’! Confronted with argument in this Malice-in-Blunderland style, it is difficult to conceive of any rational response. Perhaps stating the obvious will suffice. One can deny that history and sociology are ‘the same thing’ but, even while underlining their differences as intellectual enterprises, still appreciate the close connections that must exist between them. And one can be less than impressed by the pretensions of grand historical sociology but still admire and support other forms of historical sociology and of macrosociology alike.

(ii) Research strategies and methods

Having observed that sociologists are able to generate their own data, rather than being reliant on the evidence of relics, I then went on to claim that they are thus ‘in a position of advantage that should not be disregarded or lightly thrown away’ ([214] emphasis in original): sociologists should turn to history ‘only with good reasons and in full awareness of the limitations that they will thereby face.’ The studies of Anderson and Marshall served to illustrate historical research by sociologists which, as well as being necessary, given the questions they wished to treat, was also of value on account of 'their sensitivity to just what manner of inferences the relics available to them can, and cannot, sustain' [226]. However, I took in contrast the work of Kai Erikson (1966) as an instance of a recourse to history that appeared misguided. The hypotheses of interest to Erikson were ones that he himself regarded as having a high level of generality; and thus, it would seem, they could have been examined as appropriately, and at the same time more rigourously, in the context of contemporary rather than seventeenth-century American society.

In other words, my concern here was to put forward an argument about sociologists’ research strategies – and one which, as I maintain further below, none of my critics appears ready to challenge directly. Instead, Hart and also Bryant choose to impute to me a different argument which I did not make and which indeed I sought – unavailingly – to disown: that is, the argument that the methods of ‘sociological’ research, as compared to those of ‘historical’ research, are unproblematic and possess an ‘inherent superiority’ (Bryant: 7).

Thus, Hart launches an attack on my ‘complacent presentation’ (23) in which, she claims, in the course of a ‘eulogy of tailor-made data’, ‘problems of validity and reliability, the “disciplinary catechisms” of
a sociological training, are not even mentioned' (23, emphasis in original). This is, however, just more nonsense. I plainly state [214] that 'generated evidence', in the same way as that derived from relics, 'must always be critically viewed as regards its completeness as well as its reliability and validity'; and I indeed add that 'in these latter respects special problems result precisely from the processes of generation.'

Bryant's complaint (7) is that I attempt to draw up a 'balance sheet' between historical and sociological forms of inquiry with 'all methodological credits accruing on the sociological side of the ledger'. (Note that it is Bryant, not I, who here introduces the adjectives 'historical' and 'sociological' to refer to past- and present-oriented research, respectively.) Again, though, I must reply that this is a charge that is not—and cannot be—sustained. I would, as it happens, mostly accept Bryant's observations on the particular appeal of relics as evidence; and, as indicated above, I do recognize the kinds of problem typically associated with data deriving from survey and other kinds of field research. Indeed, in view of Bryant's total disparagement of 'standard sociological data' (8 and ns. 8–12)—which is at least two decades stale—my own position might well be regarded as more 'balanced' than his.

However, the aim of my paper was not to evaluate different methods of research in abstracto. It was, to repeat, to argue that sociologists are in a position of advantage—not 'superiority'—when they are able to treat a research issue on the basis of data collected specifically for the purpose (even though there may be attendant problems) rather than having to rely entirely on the happenstance of relics (even though these may have some distinctive merits).

DIVERSIONS

(i) Empiricism and 'dataphilia'

Mann's main diversionary strategy is to accuse me of 'empiricism' and 'dataphilia'. He gives only a sketchy indication of what he means by empiricism and then adds to the uncertainty by admitting (n. 7) that he is not after all sure whether I am an empiricist or not. However, the question one must ask is: just what relevance does all this have for my paper and specifically, since this is what seems chiefly to disturb Mann, for my charge that in the work of grand historical sociologists the links between evidence and argument are both tenuous and arbitrary? The answer is, so far as I can see, 'not a lot'. But, so as not to appear to be avoiding issues, I might at least say the following.

If the distinguishing trait of an empiricist is the supposition that 'the facts are independent of our perceptions' (Mann: 42—1 I assume he
really means 'conceptions'), then I am not one. If, however, an empiricist is one who believes that data (facts from a certain conceptual standpoint) are essential both to evaluating theories and also to establishing the *explananda* to which theories are, presumably, to be addressed, then I accept the label. And in so far as this position entails taking matters of data quality very seriously, I am happy to be labelled as a 'dataphiliac' also. Since Mann chooses to bring my recent work with Robert Erikson (1992) into the argument, I may elaborate with reference to this.

If Mann had read this work at all seriously (readers should be warned that his account of it is garbled throughout), he would have noted that there is in fact an extensive discussion (ch. 2) of the question of the 'conceptual context' within which social mobility should be studied. Our decision to take up a class structural, rather than a hierarchical, context - a decision we then implement through our class schema - is basic to what follows. Our data on mobility are constituted through this schema, with particular attention being given to questions of reliability, especially in regard to cross-national comparability. The data are then used, on the one hand, to test a range of extant theories and, on the other, to bring out new regularities for which new explanations are required. Mann contends (48) that all we end up with is a pattern of 'no results'; but, yet again, this seems to reflect his belief that if only his misrepresentation is big and barefaced enough, he may get away with it. Patterns of absolute class mobility rates, it is true, show little regularity, whether viewed over time or across nations, and have thus, we suggest, to be accounted for more in historical than sociological terms. But, in contrast, patterns of relative rates display a rather remarkable degree of temporal constancy and also tend to show a large commonality across both nations and their constituent subpopulations, as defined by region, ethnicity or gender (see Erikson and Goldthorpe 1992: chs. 3, 5, 6 and 7 esp.). That it allows such regularities to be revealed is, we believe, the best justification for our conceptual approach; and the regularities are, incidentally, ones which, despite his protestations, call into question much of Mann's standard rhetoric (e.g. 46–51) - echoed by Hart (28–9) - on questions of class, nation states, gender relations etc.²

The point of Mann's lengthy 'warnings to empiricists' is, one presumes, to suggest that macrosociologists such as Erikson and myself should learn from grand historical sociologists in matters of concept formation, the relating of concepts to data and in turn of data to theories. If so, I must, for my own part, return thanks for his consideration and proceed regardless. I might, however, add that while in my original paper my prime concern was with the quality of data in grand historical sociology, other critics have focused precisely on inadequacies in conceptualization and in the linking of data and theory in explanatory strategies. See, for example, in the first respect,

Grounded theory and women’s voting

While Mann attempts to create a diversion through an attack on ‘empiricism’, Hart seeks the same effect through a celebration of ‘grounded theory’. In so far as this is of any relevance to the issues raised in my critique of grand historical sociology, it is not, I suspect, in a way that Hart envisaged: that is, it serves to undermine the response made by Mann, and also Bryant, to the effect that ‘theory’ can help in resolving the problems of evidence which I sought to stress. This point I will return to later.

Here, I would simply observe that whether or not sociologists are proponents of grounded theory does little to affect the position of advantage which, I argued, they enjoy in being able to create data rather than being restricted to the evidence of relics. Indeed, in so far as theory is developed by allowing ‘the sociological intelligence’ to ‘roam’ over a wide range of data, as Hart would have it (26), then not to be dependent on data that is conditioned by mere accidents of survival would seem to be all the more important. Nor will it do for Hart to claim (23) the use of ‘unobtrusive or less obtrusive methods of gathering evidence’ just for her side of the argument (cf. also Bryant, 8–11). While it is true that archival work does not obtrude greatly on the dead, all non-obtrusive methods of an observational kind can, of course, be applied only in the present.

As well as urging ‘the virtues of grounding theory in data’, Hart seeks also to demonstrate the dangers that may arise with ‘tailor-made’ data: that is, those collected with the specific aim of examining a particular theory or related hypothesis. Where data collection is focused in this way, Hart claims, inquiry tends to be too restrictive and data are ‘all too often polluted by the theoretical terms of the investigation’ (22). But, again, it is not apparent just how Hart’s argument is intended to connect with that of my paper. It will of course always be possible, and I in no way sought to deny, that what one sociologist regards as a data-set well designed for its purpose, another will regard as defective because particular issues are neglected, variables omitted etc. – just as, one might add, what one sociologist believes is theory well grounded in data, another might consider quite fanciful. But such issues are the stuff of everyday
sociological debate; and if there is any implication at all for present concerns, it can only be that these issues will be the more *decidable* when data that bear specifically on them can in fact be obtained. Let me illustrate with the case that Hart chooses to raise.

Hart contends that the *Affluent Worker* studies show the limitations of tailor-made data in that, as a consequence of preconceived theory and also of 'masculinist' bias, the wives of the workers interviewed were not asked any question about their own political partisanship. This, Hart believes, was a particularly grave omission in view of the fact that 'the female vote had determined the outcome of the three successive general elections' (25) that preceded the research. I would reply as follows. That a higher proportion of women than of men voted Conservative in the post-war elections was indeed well known at the time when the *Affluent Worker* study was being designed. But what was also recognized was a rather obvious point that Hart manages to overlook: that from such a crude empirical fact alone, a *gender* effect on voting cannot be inferred. Nothing more may be involved than the effect on vote of other individual attributes, themselves associated with sex, such as age (or generation) or class. In other words, more women might vote Conservative than men not because of their gender *per se* but simply because more women than men belong to particular age-groups or classes. Decisions made in designing the interview schedules for the *Affluent Worker* study clearly reflected this latter interpretation: Hart completely disregards it. So who is right?

For general elections prior to that of 1964, there are not, so far as I am aware, any extant data that could support the kind of multivariate analysis that is here called for: the relics appear inadequate to our purposes. But the 1964 election, which is that closest in time to the *Affluent Worker* interviews (October, 1962 – February 1964), was followed by the first-ever British General Election Survey; and this does provide an appropriate data-set. Analysis then shows that, at least for the period of the *Affluent Worker* research, Hart's entire argument is without foundation. There is *no* tendency for women to be more Conservative than men, once age and class are controlled. Thus, in the case of working-class women aged 21 to 40 – the relevant group for comparison with the wives of our Luton respondents – it actually turns out that *fewer* of those voting supported the Conservatives than did their male counterparts.

The data of the 1964 survey have long been in the public domain. Why did Hart not use them? We might than have been spared the repetition of a quite erroneous tirade already contained in her paper 'Gender and Class in Britain' (1989). However, the question I would regard as more important is the following. How often can data-sets that are *not* in some large degree 'tailor-made', but are rather constituted from the happenstance of relics, permit the testing of rival hypotheses in the general manner illustrated above, where the relative
influence of several different factors on social action across nationally defined populations is the crucial issue?

EXPLANATION AND ‘THE ONTOLOGY OF THE SOCIAL’

The second section of Bryant’s paper comprises a discussion of problems of explanation and of the idiographic-nomothetic distinction – although, as he at one point acknowledges, these were explicitly not ones on which my attention centred. What Bryant has to say in this respect, I find far from compelling. I would certainly be ready to make the case against a historical sociology with the kind of philosophical underpinnings and programme that he would wish to lay down. However, so far as the paper I actually wrote is concerned, the only way in which I can see that Bryant’s discussion could bear on it is if he wishes to challenge my argument about sociologists’ choice of research strategy: that is, if he wishes to deny that the choice of whether to work in the present or in the past does in fact exist – since the ‘ontology of the social’ is such that all sociological problems worthy of consideration are ones that require historical treatment. Thus, methods of data collection applicable only in the present would ipso facto be vitiated, and sociologists would have no option but to commit themselves entirely to research of a relic-dependent kind.

Whether or not Bryant does want to make such a claim is not entirely clear to me. But since the relevance of what he writes to my paper must remain obscure unless he intends to reach some such conclusion, I may make one comment on the assumption that this is the case.

If Bryant is insisting that social phenomena can be meaningfully studied only when taken as, so to speak, embedded in their specific historical contexts, he is in effect rejecting a distinction that I sought to make [216] between historical and analytical time. But consider what this must then mean. It would, to begin with, preclude sociologists from studying such temporal effects as those of life-cycle phase or of generational or cohort membership if considered in any degree abstractly: reference would have always to be made to particular life-courses, generations or cohorts, with places and dates attached. It would not be proper, even in principle, to consider these effects per se; that is, as distinct from effects that are indeed quite ‘period’ or ‘place’ specific – and this despite the fact that such analysis has been routinely, but still most illuminatingly, undertaken in regard to a range of phenomena such as political partisanship, social mobility, labour market activity, family formation and so on. Furthermore, any comparative analysis in these respects would also be ruled out, since, of course, all comparison must entail abstraction at some level or other. Does Bryant really wish to embrace an ‘ontology of the social’ that carries these implications? If
so, a lack of explicitness in his presentation is perhaps understandable. It was just such an ambivalent stance that I had in mind when I referred in my paper [212] to a 'loss of nerve' over the very idea of social science.

THE REAL ISSUES

(i) History, sociology and sociologists' research strategies

To repeat, the argument with which my paper began was that (i) pace Abrams and Giddens, history and sociology are not 'the same thing'; (ii) that one important way in which they differ is in the nature of the evidence on which historians and sociologists draw; and (iii) that sociologists, in being able to generate their own data in the present, rather than being restricted to the relics of the past, are in a position of advantage that should be recognized and not given up without good reason. On the first of these points, I would note that none of my critics is ready explicitly to defend the view taken by Abrams and Giddens, and that Mouzelis indeed joins me (31) in rejecting it. C'est déjà quelque chose. Again, I do not find any basic objection to my second point, which is perhaps scarcely surprising since it is essentially a factual one that would be difficult to controvert. It would then seem that it is on the third point that disagreement centres.

Here, the main line of reply followed by my critics (see e.g. Bryant 7–10, Hart, 22–3) is to accept that in some respects data created on the basis of field work will indeed offer advantages over evidence derived from relics, while, however, insisting that in other respects just the reverse will hold. In turn, then, I am accused of a lack of balance in my evaluation of these two methodologies.

One difficulty with this response, as I have already sought to show, is that it is largely directed against an argument that I did not in fact advance - that 'sociological' research possesses some kind of inherent superiority over 'historical' research. But, further, in failing to get beyond generalities, my critics do not engage with the claims that I did seek to press regarding sociologists' research strategies: that where a substantive problem can be treated through research undertaken in the present, it will usually be preferable to exploit this possibility; and, correspondingly, that where a recourse to history is made - and even though this may be necessitated by the nature of the problem in hand - it must then be accepted that certain, and perhaps quite crucial, questions may have to be left without any adequate answer, simply on account of the limitations of the relics available.

Precisely because my concern was not with different methodologies considered in the abstract but with their application in specific instances of sociological inquiry, I developed these claims with
reference to several illustrative cases. And for my critics to take issue with me directly, it would then be necessary for them to give some attention to these cases. They would, for example, have to show that Erikson, in seeking to test his hypotheses on the functionality of deviance, was not, after all, ill-advised to engage in research dependent on relics that happened to have survived from seventeenth-century New England; or that Anderson and Marshall misled themselves, as well as me, in supposing that there were important aspects of kinship in nineteenth-century Lancashire or of economic life in early modern Scotland that were inaccessible to them. Or, at very least, my critics would have to argue that the cases I considered were unrepresentative or in some other way unfairly chosen. However, what is remarkable is that nothing at all of this kind is attempted.

One can indeed maintain that evidence constituted from relics has its positive as well as negative features relative to evidence that is derived from fieldwork. As I have said, I broadly agree with Bryant's remarks on this matter. But what Bryant along with my other critics would appear to overlook, and what the cases I discussed well bring out, is that no guarantee exists that for any given sociological problem investigated in the past -- and especially for any that turns on the patterns of social action to be attributed to a relatively large collectivity -- relics of the kind necessary to address this problem effectively will in fact be found. Whatever the advantages of relics as evidence may be, these advantages can of course only be realized in so far as relics relevant to a particular issue are there to be examined.

(ii) Grand historical sociology and positivist historiography

Grand historical sociologists, I argued, proceed -- and must proceed -- by supposing that the results of primary historical research constitute a body of empirical findings in which it is then possible for them to ground their own, more ambitious, projects. In Skocpol's words (1979: xiv), primary historical research serves as their 'basic source of evidence'. But, I went on to suggest, grand historical sociologists become in this way committed to a conception of historiography that is of an essentially positivistic kind. In a historiography that has escaped from positivist assumptions, historical facts may be understood as simply 'inferences from relics', which are made with very varying degrees of security and which, moreover, typically represent highly interdependent elements within complex, and inevitably contestable, interpretive schemes. In principle, grand historical sociologists may themselves favour such an understanding. But, in practice, their methodology requires that they must, willy-nilly, adopt a positivist stance: that is, in treating the facts -- or the concatenations of facts or entire 'accounts' -- that they find in the works of historians as if they were an assemblage of relatively discrete and stable entities, bearing a
similarly well-attested relationship to ‘the past’, from which they can then, in ‘scissors-and-paste’ fashion, select items to be combined, reordered and marshalled in support of their macrosociological endeavours.

In what I take to be some kind of reply, Mann seeks to maintain (43) that it is in fact ‘common’ for grand historical sociologists to engage in primary historical research as well as using secondary sources. In so far as this might be the case, then of course my critique would not apply. But Mann is here being not a little disingenuous. He knows as well as I do that the contribution to primary research of those whom I labelled grand historical sociologists is just about negligible. He himself acknowledges that the works of Moore, Skocpol and Hall to which I refer are ones founded entirely upon secondary sources – and he could have included those of Perry Anderson and of Wallerstein as well. It was, moreover, part of my definition of ‘grand historical sociology’ that it was essentially reliant upon ‘“history” in the sense of what historians have written’ [1991: 220] – since I took seriously Skocpol’s statement to this effect (as quoted again above) and also her further remark (1984: 382) that it would indeed be ‘disastrous’ if primary ‘studies by specialists’ could not be exploited in this way.4

A more substantial response comes from Mouzelis. The idea that sociologists might proceed by taking the results of historical research as their main empirical resource was, I observed, a nineteenth-century commonplace, the plainest expression of which was provided by Herbert Spencer. From the end of the nineteenth century, Spencer’s work and its underlying methodology came under increasing attack from historians and sociologists alike, and before long was effectively discredited. But, I argued, the practice of the grand historical sociologists of the present day raises again all the difficulties that were revealed in Spencer’s programme. Mouzelis objects (31–2) that the comparison that I make here is ‘unfair and misleading’. Grand historical sociologists differ significantly from Spencer in that they are not concerned with propounding general laws of social development – which historical cases serve merely to illustrate – and in that their use of historical materials shows a far greater sensitivity to context.

I may note, to begin with, that the posturing engaged in by Mann does not do much to help Mouzelis’s case. Mann proudly proclaims (41) that he stands in line of sociological descent from Spencer – and Comte – and that he is indeed ready to go beyond even their ‘imperialism’ in rendering historians’ accounts subordinate to his own theoretical insights. His eagerness to attack historians for their intellectual deficiencies (40–2), even while living off them, could scarcely be more Spencerian. (I suspect that sooner or later he will meet his Maitland.) Mouzelis, I would then suggest, is rather too generous in attributing to grand historical sociologists in general a degree of modesty and restraint that not all in fact possess.
More seriously, though, I do not see that on the crucial issue of the use of secondary historical sources as evidence, Mouzelis’s attempted defence of grand historical sociologists is successfully carried through. Even if the latter do, in the main, differ from Spencer in their theoretical ambitions, it still remains to be explained just why this should, in itself, better enable them to overcome the methodological problems with which I was concerned. Mouzelis here contents himself with the argument that because grand historical sociologists are not working with highly abstract theoretical schemes, they can be attentive to the specificities of the historical materials that they use, rather than simply ‘looting’ the historical record in order to fill up a series of ‘empty boxes’. But this does not really meet the main point of my critique.

For example, Mouzelis can rightly observe that Moore (1966) seeks to explain different ‘routes to modernity’ not in terms of general developmental laws but of the strategies pursued by different collective actors in particular historical circumstances. However, this in no way controverts my claim that, in his use of secondary sources, Moore shows little recognition of the complex and inherently uncertain processes of inference and interpretation that historical scholarship entails, and in turn of the fact that the material on which he draws cannot be treated as evidence in the same way that primary historical sources (i.e. relics) are evidence or, for that matter, the data that are produced through sociological fieldwork. What is in this regard especially revealing, as I sought to show, is Moore’s attitude in his treatment of the English Civil War (or, I could have added, of the French Revolution) towards ‘revisionist’ writing – which has of course to be seen as the very stuff of any post-positivist historiography. Moore in fact takes up the same stance as that adopted by Mann when the latter asserts (41–2) that ‘revisionism’ can be more or less ignored if, in the judgment of the theoretically sophisticated macrosociologist who stands above the squabbles of the historians, it simply reflects hidden ideologies or professional rivalry or is in any event ‘mere detail’. Or, one might alternatively wish to say – if it appears unduly disturbing to the seemingly ‘solid’ historical evidence in which the macrosociologist’s theory had previously been ‘grounded’.

I would further add that Mouzelis fails to see that a concern to propose and illustrate abstract developmental laws is not the only source of a positivistic attitude towards historical facts. Thus, as Burawoy (1989: 773–4) has observed, such an attitude is also powerfully promoted by the strategies of ‘analytic induction’ which Skocpol, for example, has represented as a key resource for comparative macrosociology (Skocpol 1979: 36–7; 1984: 378–81; Skocpol and Somers 1980: 183–4; cf. also Rueschemeyer 1991: 32–4). Such strategies imply that, as Burawoy puts it, ‘historical patterns have their own voice’, and there is then a reluctance to recognize that the facts that
make up these patterns are already interpretations and, quite possibly, disputed ones. For Skocpol, 'the facts have a certain obviousness that they don't for historians' and she pays relatively little attention to the controversies that rage around them. Nor is this accidental: 'She is forced into this blindness in order to get her induction machine off the ground.'

(iii) Evidence and argument in grand historical sociology

The inadequate appreciation shown by grand historical sociologists of the difficulties that arise in their use of secondary sources then resulted, I maintained [222], in serious weaknesses in their work in the linkage between evidence and argument: this was often tenuous and arbitrary to a quite unacceptable degree. This charge was indeed at the core of the critique of grand historical sociology with which my paper ended. But while it seems that it is this critique that has chiefly prompted the foregoing responses, only Mouzelis concentrates on addressing its main thrust directly.

As regards the tenuous nature of the linkage between evidence and argument, Mouzelis concedes (33) that grand historical sociologists’ dependence on secondary sources ‘is obviously a disadvantage’. However, he then goes on to contend that ‘we live in an imperfect world’, that all methodologies have their problems and, further, that there are in any event ‘a variety of ways of minimising the risks’ entailed in writing historical sociology without the use of primary historical evidence. In fact, in what follows, Mouzelis indicates just one such way (33–4): that is, the encouragement of ‘a dialectical process’ of scholarly communication and debate through which the arguments of grand historical sociologists can be constantly reexamined and, if necessary, refined and qualified. Thus, Mouzelis suggests, my own objections to Moore’s interpretation of the English Civil War should be seen as a ‘moment’ in such a process rather than as an argument for ‘abandoning’ the kind of historical sociology that Moore’s work exemplifies.

In reply, I would first wish to emphasize – and I address this remark also to Hart and Mann – that nowhere in my paper did I claim the right to say that grand historical sociology should be ‘abandoned’: only the right to criticize it, in as radical a way as I see fit, and even if that should prove damaging to the amour propre of some of its exponents. Mouzelis is ready to grant me that right and indeed to encourage me to use it. But I have then to observe that he does not appear to appreciate how far, on the matter in question, my criticism actually goes.

As Mouzelis notes, I see the linkage between the arguments of grand historical sociologists and their secondary, or yet more derivative, evidence as being unduly tenuous because they are in effect
engaged in interpretations of interpretations of, perhaps, interpretations. However, I also stress that the initial interpretations involved, that is, those of the historians using primary sources, may well be themselves of a highly tentative if not speculative nature—and openly presented as such—simply because of the limitations of these sources. In neglecting this further point, Mouzelis then mistakes the chief intent of my criticism of Moore—as also does Mann when he accuses me (45–6) of only sniping at Moore and of ‘carefully staying right away from substantive arguments’. My primary concern was not in fact with the substantive issue of whether Moore’s thesis about the social bases of political divisions in the English Civil War is correct or not, but rather with the methodological issue of the basis on which this thesis is advanced. What I sought to emphasize was the enormous gap that exists between the actual content of the secondary works on which Moore relies—and even if one accepts here Bryant’s distinction between reportage and interpretation—and the argument that Moore seeks to build upon these works. Moreover, I was not (as Mann seeks to imply) engaging in some vendetta directed specifically against Moore. I make it clear [228, n. 21] that I would see my critique as being equally applicable to the treatment of the English Civil War to be found in Wallerstein (1974) or in Anderson (1974); and further still [229–30, n. 23], that I would question whether any ‘social change’ interpretation of the Civil War—even, let us say, one known by the Recording Angel to be valid—could in fact be adequately sustained by the relics from seventeenth-century England that have been left available to us (cf. my quotation [n. 23] from Hexter 1961).

It is, incidentally, Mouzelis’s failure to recognize my critique of Moore as being methodological rather than substantive that lies behind the question he raises of how I can claim, without myself having a knowledge of the relevant primary sources, that the thesis of ‘the rise of the gentry’ was already losing ground at the time when Moore wrote. As it happens, I once did have some knowledge of these sources, but that is beside the point. For my argument [223] referred not to the actualities of English society in the sixteenth and seventeenth centuries but to the state of opinion among historians in the mid-twentieth. And what I asked was why—on what grounds—did Moore still opt largely to accept the thesis in question when ‘revisionists’ were casting doubt upon the evidence for it and its supposed political implications.

Here, then, the burden of my reply to Mouzelis is that the tenuous connection between argument and evidence in grand historical sociology may often not be remediable by any amount of the criticism and debate that he would propose. That is, because the tenuity derives not so much from the fallibility of particular authors as from the ineluctable circumstance that there are aspects of the past—and including ones that we may suppose to be of major importance—that
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are, and will in all probability remain, largely beyond our cognitive grasp. It is in this respect that my critique of grand historical sociology connects directly with my concern earlier in my paper to bring out the ultimate dependence of what can be achieved in historical research upon the mere happenstance of relics. And this dependence is a fact that Mouzelis's bland observation that all methodologies have their problems does not adequately comprehend. Nor can its significance be diminished by his strictures (35) against a methodological perfectionism that 'fetishises relics'. I do not know what Mouzelis means by this last phrase and suspect that it may be no more than a rhetorical flourish; but, in any event, it in no way alters the situation that it is relics, and only relics, that can provide our knowledge of the past.

I would again stress that I do not in any of the foregoing seek grounds for proposing an end to grand historical sociology. It leads me, rather, to suggest a way in which its practitioners could greatly improve their credibility. That is, if they were to show themselves more ready to acknowledge the methodological practices developed by historians, or indeed by sociologists who do work with primary sources (cf. Scott 1990), in seeking to understand the limits to historical knowledge and to recognize them where they arise. They might then be less inclined to advance arguments which, in clearly exceeding these limits, may very well not be right but, by the same token, have the dubious attraction of being protected from ever being shown to be wrong.

As regards arbitrariness in grand historical sociologists' use of evidence, I saw this as chiefly occurring where the secondary works on which they rely are in serious conflict. If historians disagree (and may perhaps have to remain in disagreement because the kinds of relics necessary to settle disputed issues do not exist), how do grand historical sociologists decide which account they will accept – or, I could have added, how they will 'mediate' between different accounts? Their actual practice, I argued, did not point to any satisfactory answer to this question. What was most apparent was a tendency, in some instances quite blatant, for authors simply to accept the authority of those accounts most congenial to their own arguments, while ignoring or disparaging others.5

Mouzelis again does not seek to deny that a methodological problem here exists. However, again too, he is ready to propose a solution, and of a quite simple kind: what is required is that, in drawing on secondary works as evidence, grand historical sociologists should show greater 'self-discipline and detachment' (34). With such an improvement in attitude, reinforced by the processes of communication and criticism to which he has previously referred, there is no reason, Mouzelis believes, why the enterprise should not continue with sound scholarly and social-scientific credentials.
But this will not do. Self-discipline and detachment are indeed admirable qualities, and I would obviously agree with Mouzelis that it is highly desirable that grand historical sociologists (along, no doubt, with everybody else) should display them more fully. However, I would have thought it by now well understood that the methodological basis of scholarly or scientific inquiry of whatever kind is not to be looked for merely in the personal attributes of its practitioners but must rather lie in some normative structure that provides the context—the procedures, criteria etc.—that meaningful and progressive argument requires.  

Mouzelis's position is, in this respect, retrograde from that taken up by Skocpol a decade ago. As I noted in my paper [225], Skocpol (1984: 382) explicitly recognized the need for 'clear, consensual rules and procedures for the valid use of secondary sources as evidence' and saw that 'varying historiographical interpretations' constituted one obvious problem to be addressed. A question to which Mouzelis (and my other critics) might well then have directed their attention—but in fact disregard—is that of why the methodological development that Skocpol called for has not to any appreciable extent been realized. The lack of progress confirms me in my view that grand historical sociologists do here face a very severe difficulty: namely, that of providing grounds for the critical evaluation of secondary works which do not demand a knowledge of the primary sources from out of which these works are written.

Mouzelis apart, my critics take up the issues I raised of argument and evidence in grand historical sociology in only a rather limited way; and, so far as I can see, only one further response emerges that calls for serious comment. This is best expressed by Bryant—Mann offers a less argued though more extravagant version—and is to the effect that the methodological problems to which I drew attention are in fact ones that can be overcome by a resort to 'theory'.

For Bryant (13), historical works are 'woven from two distinguishable strands: what might be called reportage, on the one hand, and interpretation on the other.' Thus, it is possible for grand historical sociologists, in using secondary works as evidence, to concentrate as far as possible on their elements of reportage—the 'facts' of which, Bryant holds, possess 'stability' (14). However, where conflicting interpretations of these facts do arise, the question of how the grand historical sociologist should decide between them has an 'obvious' answer: that is, 'by means of theory and comparative evidence' (14). Mann, it seems, would go further. Not only can theory be used in such adjudication between interpretations but, in addition, to modify the 'facts' themselves. (What, one may ask, happens here to Bryant's 'stability'?). Mann tells us (41) that 'if I am sceptical about historians' facts, I have other sources of information (my theories based on broader knowledge about how societies operate, and my knowledge of
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historians' theories).’ In using secondary works, he is therefore ready ‘to make judgments about sociological plausibility based on other times and places’ (41–2) which may override the authors' own views; and it is then such judgments that allow him to ignore or reject ‘revisionism’ or indeed, one supposes, any other aspect of historians' accounts.

These are remarkable claims, and the first question that has to be asked about them is evident enough: just what is this theory – about 'how societies operate' – which is called upon to do such crucial work? Bryant and Mann are extraordinarily coy. They have, apparently, access to the kind of theory for which sociologists have been looking since sociology began, and yet they are reluctant to tell us about it – or indeed to give us even a single illustration of it being used in the ways they propose. Since, for whatever reasons (Is it all a secret?, Could it just be a lot of bluff?), we are not provided with the illumination that we might expect, it is not easy to know how further to proceed. However, the following at least might be said.

First, an appeal to theory to help decide between empirically-grounded, but still conflicting, interpretations or indeed to question particular ‘facts’ could be an acceptable methodological move. However, for this to be so, the theory would need to be of a quite powerful kind. It would have to permit the demonstration that one interpretation followed rigourously from it, while others did not; or, in the case of dubious ‘facts’, that these were so contrary to otherwise well-supported expectations that some error – of observation, recording etc. – might reasonably be supposed. And, of course, in addition to having substantial confirmation, the theory would also need to be a rather general one, the applicability of which to the circumstances in question could safely be claimed.

Although what Bryant and Mann have in mind is, to repeat, left obscure, one may indeed doubt if it is theory capable of meeting such requirements. Apart from there seeming to be rather little theory of this quality around in sociology, Bryant at least has indicated (1992) that he could not even support attempts to produce it. If, then, one seeks to fill in what Bryant and Mann fail to tell us, the only reasonable assumption to make is that the theory they invoke is of the kind that Kiser and Hechter have shown to be chiefly favoured among grand historical sociologists: that is, 'theory' which, as Kiser and Hechter put it (1991: 9), is actually no more than 'typologies, orienting concepts and empirical generalisations' or – to be somewhat more charitable – which is 'grounded theory' as advocated by Hart (26–8) and which Mann (42–3) does indeed appear to endorse.

The distinctive features of such theory could be said to be three: first, it is developed more inductively than deductively – it should 'emerge from the data' (Hart, 26); secondly, rather than aspiring to generality, it seeks to display a particular relevance for the temporal
and spatial contexts within which it was thus developed; and thirdly, it is so formulated as to be as open not to critical empirical test but rather to indefinite refinement in a process in which theory and data are, to use Mann’s word (45), ‘blended’. Now what must be pointed out is that, even if such features of grounded theory should be found attractive in other respects, they are ones that make it especially ill-suited to resolving methodological difficulties in grand historical sociology in the way that Bryant and Mann would envisage.

Thus, in adjudicating between conflicting secondary accounts, just how could grounded theory help – given that it is formed, so to speak, without any deductive backbone? For how could it then be said that one account was more consistent with – that is, was to a greater extent implied by – a grounded theory than was another? And even if such a judgment were made, what in the logic of grounded theory would prevent those who wished still to maintain a rival account from arguing that, instead of this being rejected, existing theory should be modified so that it ‘blended’ just as well with the content of their account as with that of any other? Or again, one may ask, what force can grounded theory have in determining the ‘sociological plausibility’ of historians’ interpretations when viewed in comparative perspective – given that it emphasizes its sensitivity and specificity to context? Why should what is known, on the basis of grounded theory, about one time and place be thought applicable to another? As Kiser and Hechter (1991: 12) aptly observe, the ultimate implication of the ‘historicism’ implicit in theorizing of this kind is that ‘comparative history is an oxymoron’. And finally, of course, the issue arises, and especially when it is historians’ facts as well as interpretations that are in question, of the actual grounds on which the superiority of theory is claimed. If a grand historical sociologist chooses to say, as Mann might be ready to do, that in the light of theory a historian’s account must be corrected, why should it not be the other way around? If grounded theory is so formulated that it is not open to empirical refutation, then how can it claim the degree of empirical confirmation that would be needed to underwrite its use in the way that Mann would wish?

In sum, if it is grounded theory that Bryant and Mann have in mind (and if it is not, they will no doubt become more explicit), then their faith that, in grand historical sociology, theory can help overcome problems of linking argument to evidence would seem quite misplaced. I would rather conclude from their arguments, and those of Hart, that the penchant for theory of this kind that grand historical sociologists have in fact displayed is one source of the difficulties in their work to which I drew attention. A resort to grounded theory, in giving a general license for theory to be accommodated to evidence and, equally, for the choice of (what is taken as) evidence to be accommodated to theory, must militate against the ‘self-discipline’ for which Mouzelis calls and invite the tendentiousness that I illustrated.
And to encourage a yet greater reliance on such theory in the practice of grand historical sociology could then serve only to make this less, rather than more rule-governed than it has been hitherto.9

CONCLUSION

Mann states at one point (46) that he is genuinely puzzled by my attack on grand historical sociology: what, he wishes to know, is its 'general significance'? He might of course have asked Hart, who could have told him (28) that its 'real source' is to be found in my 'heavy investments' in 'old sociological orthodoxies' that are by now 'increasingly obsolete' and 'in constant need of defence and reassertion'. But perhaps he knew all this anyway (cf. 47). If, by way of conclusion, I should try to enlighten him further, I can only say that I took the emergence – or revival – of grand historical sociology seriously as development that had attracted a good deal of favourable attention and comment (though not least from among its own exponents). However, my own reading of the genre led me to the view that it was, at all events as it had so far progressed, a deeply flawed undertaking, primarily because it lacked any well thought out and articulated methodology, and that in the attacks on the practices both of 'mainstream' sociology and of 'conventional' history in which grand historical sociologists often engaged, there was no little straining at gnats while camels were being readily swallowed. Following from a more general discussion of the uses of history in sociology, I therefore sought to develop a critique of grand historical sociology that might serve to redress matters somewhat – even if going against the 'progressive' opinion of the day.

The response that my paper has received encourages me to believe that the effort was worthwhile, for three different reasons. First, my remarks do seem to have hit a number of sensitive targets – the best indication of this coming where, as in the passage from Hart referred to above, counterattacks have had to be launched against the supposed motivation of the critique rather than its substance. Secondly, though, the replies received from Bryant and Mouzelis start from an acceptance of the fact that I raised serious questions that call for serious answers. As will have become apparent, I do not find the particular answers that these authors have to offer at all convincing; but, at any rate, in consequence of our exchanges, the issues a stake should have become a good deal clearer. Thirdly, since I first drafted my paper (at the end of 1985), I have been interested, and encouraged, to note the appearance of a series of other – quite independent – contributions in which the methodological bases of grand historical sociology are also questioned, in different, though complementary, respects from those on which I concentrated. The most important of these further
critiques (Nichols, 1986; Burawoy, 1989; Kiser and Hechter, 1991; Lieberson, 1991) I have referred to in the foregoing. I can therefore have some assurance that I am not alone in continuing to believe that grand historical sociology, despite – or perhaps because of – its large pretensions, still rests on very insecure foundations and thus often provides us with the semblance rather than substance of knowledge of our past. Even if my own observations in this regard are clearly unwelcome to its supporters, I could perhaps end with a plea from Francis Bacon, in the hope that the words of this great progenitor of inductivism might command more favourable attention: ‘God forbid that we should give out a dream of our own imagination for a pattern of the world’.

(Date accepted: June 1993)

NOTES

* I am indebted to Gordon Marshall for helpful comments on an earlier version of this paper.

1. Thus, Bryant follows Mills and Cicourel in representing survey-based research as typically a matter of collecting information on attitudes or other aspects of ‘respondents’ subjective interpretations’ via questionnaires using ‘pre-selected categories’, after which the material obtained is ‘mechanically scaled’ into quantitative form. Such procedures, Bryant then argues, lead to data that are inherently flawed by the ‘artificiality’ and ‘contamination’ involved (8–9 and ns. 7–12). If Bryant’s characterization were at all an adequate one, I would be inclined to agree with his criticism. But in fact he shows scant awareness of the extent to which, at the present time, data collected via survey methods (i) are concerned not with respondents’ attitudes or even their reported actions but with various of their (or of others’) objective attributes, relationships etc.; (ii) are collected, whether via questionnaire or interview, in unstructured form, after extensive piloting, and are only subsequently coded; (iii) are subject to increasingly sophisticated testing to indicate their degree of reliability and validity; and (iv) can be analysed via powerful statistical techniques without needing to be ‘scaled’ or treated at anything other than a nominal level of measurement. (An illustration of this latter point is, as it happens, provided in note 3 below.)

2. In this connection, two of Mann’s further bizarre misrepresentations may also be noted. First, he describes the class schema we use as comprising ‘a single hierarchical scale’ (48) when we explicitly point out that the schema ‘is not constructed around any single hierarchical principle’ (Erikson and Goldthorpe 1992:44) and emphasize how in this respect it contrasts with occupational status scales. Secondly, he claims that in our work ‘women were assigned the occupational status of the leading male in their household’ (49) when in fact, in addition to an analysis of ‘marital mobility’, we further analyse the mobility of women on the basis of the ‘dominance’ method, according to which the class position of the conjugal family may follow from the employment of either husband or wife and on the basis of women’s own employment (Erikson and Goldthorpe 1992:242–53, 264–75).

3. The analysis entails constructing from the BGES data a four-way table of sex by age (21–40, 41–60, 61 plus) by class
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(service, intermediate, working) by vote (Con, other). Women's own employment is used as the basis of their class allocation. This table, it then turns out, can be fitted rather well ($G^2 = 16.9; \text{df} = 9; p = 0.05; \Delta = 4.3$) by a loglinear model proposing two three-way associations: between age, sex and class and between age, class and vote. And if the model is extended to include the further association between sex and vote, no significant improvement in fit is achieved ($G^2$ falls to only 16.1 for the one degree of freedom lost). Among working-class women aged 21-40, only 22 per cent of those voting reported Conservative support, as compared with 36 per cent of working-class men in the same age group. While this difference should not itself be taken all that seriously, on account of likely sampling error, the analysis reveals clearly enough that, at least by the 1960s, if not before, the 'Conservative woman' had become a mythical creature. Full details of the analysis are available on request.

4. Consistently, in introducing her substantive work, Skocpol (1979: xiv) remarks — and note the Spencerian constructional metaphor — that 'primary research', which the comparativist 'has neither the time nor (all of) the appropriate skills to do', 'necessarily constitutes, in large amounts, the foundation upon which comparative studies are built'. In contrast to Mann, both Bryant (14) and Mouzelis (33) make no bones about the grand historical sociologist's dependence on secondary sources.

5. Mann's seeming response (n. 2) to my charge of tendentiousness in the work of grand historical sociologists is to represent it as one of misplaced anti-Marxist abuse linked with obsequiousness towards historians. In this connection, he refers — bewilderingly — to my n.22, which is of no conceivable relevance. I can only say that here again Mann displays a total disregard for normal standards of argument.

6. Even if the problem of arbitrariness is considered simply at the level of the individual practitioner, another difficulty with Mouzelis's solution is that questions not only of outlook and motivation but also of capacity arise. This is best illustrated in the case of arbitrariness in the use of evidence that derives from linguistic barriers. While some grand historical sociologists are notably better equipped than others with linguistic skills, 'minority' languages are always likely to impose restrictions, which may be seriously damaging. Thus, in Anderson's study (1974) of the rise of 'absolutist' states, Sweden is obviously an important case. But Anderson is forced to base his treatment of this case entirely on non-Swedish works — overwhelmingly in fact, as he acknowledges (1974: 173, n. 1), on the studies of Michael Roberts. Excellent though Roberts' work may be, it still provides only one interpretation of early modern Swedish history; and it is, to say the least, unsatisfactory that in Anderson's account the views of indigenous historians are in effect — and for no good substantive reason — entirely discounted. For an instructive example of how access to a source in a minority language (again Swedish) can serve to call into question a central thesis advanced essentially on the basis of English-language secondary sources, see the critique by Winch (1989) of Weir and Skocpol (1985).

7. Mann at various points (e.g. n.8) seeks to argue that grand historical sociologists have recognized, and indeed have paid substantial attention to, the methodological problems that arise in their work. But the reference he gives in this respect — i.e. to Kiser and Hechter (1991) — is a surprising one, and provides no substance whatever for his case.

8. To avoid any misunderstanding, it should be understood that Kiser and Hechter are here using 'historicism' in a quite different sense to mine in n.14 of my original paper: that is, in what one might call the sense of Meinecke rather than the sense of Popper.

9. Mann seeks to suggest (38 and n. 4) that Kiser and Hechter's criticisms of grand historical sociology and my own come from opposing standpoints. But in fact they converge. Though I think it questionable whether any kind of sociological theory — rational choice theory included — can ever 'escape' from history into total generality, I also believe, as I
said in my original paper, that sociologists should always be seeking to push their theories as far in this respect as they will go. And it is entirely consistent for me to agree with Kiser and Hechter that theory of a more general, deductive character would serve historical sociology better than that now most often favoured. As they pertinently observe (1991:10) 'When data are fragmentary and hard to come by – as often is the case in comparative-historical research – only a theory with high analytical power, and thus low data input requirements, can be tested.'

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