



The Uses of History in Sociology: Reflections on Some Recent Tendencies

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The uses of history in sociology: reflections on some recent tendencies*

ABSTRACT

This paper questions the now widely held view that no meaningful distinctions are to be drawn between the disciplines of history and sociology. It is argued that one – highly consequential – difference concerns the nature of the evidence on which historians and sociologists typically rely or, more precisely, the way in which this evidence comes into being. This argument is developed and illustrated with reference to various examples of sociologists resorting to historical research and the difficulties they have encountered; and further in the context of a critique of ‘grand historical sociology’ whose practitioners have so far failed to provide their work with any adequate methodological basis.

I

To take up again the question of the uses of history in sociology may well appear regressive. For to do so implies, of course, making a distinction *between* history and sociology which would now be widely regarded as untenable. Thus, for example, Philip Abrams, in his highly influential book, *Historical Sociology*, has advanced the argument that since ‘history and sociology are and always have been the same thing’, any discussion of the relationship of one to the other must be misguided; and Abrams in turn quotes Giddens to the effect that ‘There simply are no logical or even methodological distinctions between the social sciences and history – appropriately conceived’.¹

As Abrams is indeed aware, the position he adopts is in sharp contrast with that which would have been most common among sociologists two decades or so previously. At this earlier time, sociologists were for the most part anxious to differentiate their concerns from those of historians. For example, much use was made of the distinction between ‘idiographic’ and ‘nomothetic’ disciplines. History was idiographic: historians sought to *particularise* through the

description of singular, unique phenomena. Sociology was nomothetic: sociologists sought to *generalise* through formulating theories that applied to categories of phenomena.² However, all this was in the period before the British sociological community (anticipating Sir Keith Joseph) lost its nerve over the idea of ‘social science’ – before, that is, the so-called ‘reaction against positivism’ of the late 1960s and 1970s created a new mood in which political radicalism went together with intellectual conservatism.

My first contribution to the debate on ‘history and sociology’ dates back to this prelapsarian time, and was in fact a *critique* of the idiographic-nomothetic distinction.³ My remarks were not especially well received by either historians or sociologists, and this present contribution may, I fear, prove similarly uncongenial. For what I would now think important is that attempts, such as that of Abrams and Giddens, to present history and sociology as being one and indistinguishable should be strongly resisted.⁴

To avoid, if possible, being misunderstood, let me stress that I do not seek here to re-establish the idiographic-nomothetic distinction, or at least not as one of principle. I do not believe, for example, that sociologists can ever hope to produce theories that are of an entirely transhistorical kind; nor that historians can ever hope to produce descriptions that are free of general ideas about social action, process and structure. However, good grounds do still remain for refusing to accept the position that *any* distinction drawn between history and sociology must be meaningless.

To begin with, I would argue that the idiographic-nomothetic distinction is still pertinent if taken as one not of principle but of *emphasis*. Historians do – quite rightly – regard it as important that dates and places should be attached to the arguments they advance as precisely as possible; as E. P. Thompson has aptly remarked, ‘the discipline of history is above all a discipline of context’.⁵ Sociologists – no less rightly – believe that they are achieving something if the time and space co-ordinates over which their arguments apply can be widened. And from this one use of history in sociology is immediately suggested. History may serve as, so to speak, a ‘residual category’ for sociology, marking the point at which sociologists, in invoking ‘history’, thereby curb their impulse to generalise or, in other words, to explain sociologically, and accept the role of the specific and of the contingent as framing – that is, as providing both the setting *and* the limits – of their own analyses.⁶

However, it is not on such issues that I wish here to concentrate. My aim is rather to focus attention on another major difference between history and sociology which has, I believe, been much neglected but which carries far-reaching implications for sociological practice. This difference concerns the nature of the evidence that the two disciplines use or, more precisely, the way in which this evidence comes into being.⁷

As a trainee historian at University College London in the 1950s, I underwent a standard catechism on method, which began with the question: what is a historical fact? The answer that had to be given was: a historical fact is an inference from the relics. This answer struck me then – and still strikes me – as the best that can be given, and as one of considerable significance. What the answer underlines is the obvious, but still highly consequential, point that we can only know the past on the basis of what has physically survived from the past: that is, on the basis of the relics – or of what may be alternatively described as the residues, deposits or traces – of the past.⁸

These relics are of very different kinds. They may, for example, be simply natural remains, such as bones or excrement; or again, artefacts, such as tools, weapons, buildings or works of art. But of most general importance are what one might call ‘objectified communications’: that is, communications in some written form and, especially, ‘documents’ of all kinds. Whatever their nature, it is these relics, and only these relics, that are the source of our knowledge about the past. Statements about the past – historical ‘facts’ – are inferences from the relics, and can have no other basis. In short: no relics, no history.

So far as the practice of history is concerned, there are two points about relics that it seems important to recognise: first, they are *finite* and, second, they are *incomplete*. The relics that exist are just a limited selection of all that could have survived – a sample, so to speak, of a total universe of relics, where, however, neither the properties of the universe nor of the sample are, or can be, known.⁹ The relics of a given period may diminish, by being physically destroyed, but they cannot increase.

It is true of course that not all the relics that exist at any one time are known about. Historians have always the possibility of discovering ‘new’ relics, of adding to the known stock: and it is indeed an important part of their *métier* to do so. It is also true that from any set of relics, the inferences that can be made are *infinite*. The ‘facts’ that the relics yield will tend to increase with the questions that historians put to them and, in turn, with the range of the problems they address and with the development of their techniques of inquiry. However, none of this alters the situation that the relics themselves, in a physical sense – what is *there* to be discovered and interrogated – are finite and are, to repeat, a selection, and probably only a quite small and unrepresentative selection, of all that could have survived. It must therefore be the case that limitations on the possibilities of historical knowledge exist simply because it is knowledge of the past – because it is knowledge dependent on relics. There are things about the past that never can be known simply because the relics that would have been essential to knowing them did not in fact survive.

Historians, we may then say, are concerned with *finding* their evidence from among a stock of relics. In contrast – and this is the

difference I want to stress – sociologists have open to them a possibility that is largely denied to historians. While sociologists can, and often do, draw on relics as evidence, in just the same way as historians, they can, in addition *generate* evidence. This is of course what they are doing when they engage in ‘fieldwork’. They are producing, as a basis for inferences, materials *that did not exist before*.¹⁰ And it is, I would argue, such generated evidence, rather than evidence in the form of relics – in other words, evidence that is ‘invented’ rather than evidence that is discovered – that constitutes the main empirical foundations of modern sociology.

The immediate reason for this difference in the way in which historical and sociological evidence comes into being is obvious: historians work ‘in the past’, while sociologists *can also* work ‘in the present’. However, behind this immediate reason lies the difference of emphasis that I earlier referred to: sociologists do not seek to tie their arguments to specific time and space coordinates so much as to test the extent of their generality. Thus, if a sociologist develops a theory intended to apply, say, to all industrial societies, it will be only sensible at all events to *begin* the examination of this theory through research conducted in contemporary rather than in past industrial societies; and hence through research which permits the generation of evidence rather than imposing a reliance upon relics.

If, then, there is here, as I would wish to maintain, a major difference between history and sociology as forms of disciplined inquiry, what follows from it for the uses of history in sociology? The main implication is, I believe, clear enough. Because sociologists have the possibility of producing their own evidence – over and above that of exploiting relics – *they are in a position of advantage that should not be disregarded or lightly thrown away*. In other words, sociologists should not readily and unthinkingly turn to history: they should do so, rather, only with good reasons and in full awareness of the limitations that they will thereby face.

Here again I am, I suspect, in some danger of being misunderstood. Let me therefore at once add that I do not in any way seek to suggest that sociology is in some sense a ‘superior’ discipline to history: rather, I am concerned to bring out just how difficult history is – since, as will later emerge, I believe that some sociologists have clearly failed to appreciate this. Nor do I suppose that generated evidence, in contrast to that in the form of relics, is unproblematic. I am well aware that it too must always be critically viewed as regards its completeness as well as its reliability and validity, and indeed that in these latter respects special problems result precisely from the processes of generation. However, what I do wish to emphasise are the very real advantages that are gained where the nature and extent of available evidence is not restricted by the mere accidents of physical survival; where, moreover, the collection of evidence can be ‘designed’ so as to meet the

specific requirements of the inquiry in hand; and where questions of the quality of evidence can always be addressed, as they arise, by generating yet further evidence through which to check and test the original.¹¹

II

To develop these arguments, I now turn to particular cases. To begin with, it may be helpful if I give an example of what I would regard as a mistaken – one might say, perverse – recourse to history on the part of a sociologist. I take here Kai Erikson's book, *Wayward Puritans*, which is a study of social deviance within the seventeenth-century Puritan community of Massachusetts Bay.

In his Preface, Erikson states his aims clearly. He begins with certain hypotheses about social deviance drawn from a Durkheimian position, and he aims to examine two hypotheses in particular: first, that some amount of deviance is functional for a community in helping it to define its moral and social boundaries, and thus in preserving its stability; and second, that, because of this functionality, deviance within any community will tend to be at a fairly constant level over time. Erikson then proposes to take Massachusetts Bay as a case-study. 'The purpose of the following study', he writes

is to use the Puritan community as a setting in which to examine several ideas about deviant behaviour. In this sense the subject matter of the book is primarily sociological, even though the data found in most of its pages are historical . . .

And, he goes on

The data presented here have *not* been gathered in order to throw new light on the Puritan community in New England but to add something to our understanding of deviant behaviour in general, and thus the Puritan experience in America has been treated in these pages as an example of human life everywhere.¹²

Judged in the light of this statement, *Wayward Puritans* is, I would argue, a failure – and indeed a necessary failure – because of its reliance on historical materials. The hypotheses that Erikson starts from are not seriously examined, and could not be, simply because Erikson does not have the evidence needed for this among the relics at his disposal.

Thus, as regards the first hypothesis, on the functionality of deviance, Erikson draws largely on court records, indicating the response of the authorities to antinomianism, Quakerism and alleged witchcraft. But he has little evidence of how *the community at large*, as distinct from the authorities, reacted to such deviance or, for that

matter, to its treatment by the authorities. In other words, he has no adequate basis on which to determine whether, in consequence of the deviance he refers to, there was, or was not, a stronger definition of the moral and social boundaries of the community. So far as popular perceptions and evaluations are concerned, he is without means of access.

Likewise, in treating the second hypothesis, on the constant level of deviance, Erikson has to rely on official crime statistics, which, for well-known reasons, give only a very uncertain indication of the actual level of social deviance, and are influenced in their trend by a variety of other factors. However, unlike the sociologist of deviance working in contemporary society, Erikson cannot investigate in any detail the processes through which the official statistics were constituted, nor can he collect data of his own which could provide alternative estimates – as, say, through some form of ‘victim survey’.

To be sure, the hypotheses that Erikson addresses are not ones that would be easily tested under any circumstances. But, given that they derive from a theory that pretends to a very high level of generality, there is all the more reason to ask why Erikson should impose upon himself the limitations that must follow from choosing a historical case. Why should he deny himself the possibility of being able to generate his own evidence, to his own design, and under conditions in which problems of reliability and validity could best be grappled with? Any sociologist, I would maintain, who is concerned with a theory that *can* be tested in the present should so test it, in the first place; for it is, in all probability, in this way that it can be tested most rigorously.¹³

I would now like to move on to consider cases where the recourse of sociologists to history *would* appear to have the good reasons which, I earlier maintained, should always be present. Here my aim is to illustrate what such reasons might be, but also – when they are acted upon – the difficulties that may be expected.

Sociologists, one might think, will most obviously need to turn to history where their interests lie in social change. However, it should be kept in mind that a recourse to the past – or, that is, to the relics thereof – is not the only means through which such interests may be pursued: life-course, cohort or panel studies, for example, are all ways of studying social change on the basis of evidence that is, or has been, collected in the present. Sociologists, I would argue, are compelled into historical research only where their concern is with social change that is in fact historically defined: that is, with change not over some analytically specified length of time – such as, say, ‘the life-cycle’ or ‘two generations’ – but with change over a period of past time that has dates (even if not very precise ones) and that is related to a particular place. Sociologists have a legitimate, and necessary, concern with such historically defined social change because, as I have earlier suggested, they wish to know how widely over time and space their theories and hypotheses might apply.¹⁴

One illustration of what I have in mind here is provided by Michael Anderson's book, *Family Structure in Nineteenth Century Lancashire*. Anderson is concerned with the hypothesis that in the process of industrialisation, pre-existing forms of 'extended' family and kinship relations are disrupted. Specifically, he is interested in whether or not this hypothesis holds good in the British case – that of the 'first industrial nation'. Thus, to pursue this issue, Anderson aims to examine just what was happening to kinship relations in Britain at the time when, and in the place where, the 'take-off' into industrialism is classically located. In contrast, then, with Erikson, Anderson has a quite clear rationale for turning to historical research.

A second illustration is provided by Gordon Marshall's book, *Presbyteries and Profits*. Marshall is concerned with the 'Weber thesis' – that a connection exists between the secular ethic of ascetic Protestantism and 'the spirit of capitalism'. In the long-standing debate on this thesis, the case of Scotland has several times been suggested as a critical one, in that, in the early modern period, Scotland had a great deal of ascetic Protestantism – that is, Calvinism – yet showed little in the way of capitalist development. Marshall's aim is then to re-examine the Scottish case for the period from around 1560 down to the Act of Union of 1707. Marshall points out that Weber himself always emphasised that his argument on the role of the Protestant ethic in the emergence of modern capitalism was intended to apply *only to the early stages* of this process: once a predominantly capitalist economy was established, its own exigencies – in the workplace and market – would themselves compel behaviour generally consistent with the 'spirit of capitalism' without need of help from religion. Again, then, Marshall, like Anderson, has obviously good grounds for his recourse to history.

Now before proceeding further, I should make it clear that I have the highest regard for the two studies to which I have just referred. Both make signal contributions to the questions they address; and, for me, they stand as leading examples of how in fact historical sociology should be conceived and conducted. I say this because I want now to go on to emphasise the severe limitations to which the analyses of both authors are subject: *not* because of their deficiencies as sociologists, but simply because of the fact that they were forced into using historical evidence – forced into a reliance on relics – rather than being able to generate their own evidence within a contemporary society.

The relics on which Anderson chiefly relies are the original enumerators' books for the censuses of 1841, 1851 and 1861. On this basis, he can reconstruct household composition according to age, sex and kinship relations, and he can also to some extent examine the residential propinquity of kin. But this still leaves him a long way short of adequate evidence on the part actually played by kinship in the lives of the people he is studying and on the meanings of kinship for them. He attempts to fill out the essentially demographic data that he has

from the enumerators' books by material from contemporary accounts. But these would, I fear, have at best to be categorised as 'casual empiricism' and at worst as local gossip or travellers' tales. Titles such as *Walks in South Lancashire and on its Borders*, *A Visit to Lancashire in December 1862*, and *Lancashire Sketches* give the flavour.

Anderson is in fact entirely frank about the problem he faces. 'It must of course be stressed', he writes, 'that just because interaction with kin occurred it is no necessary indication that kinship was important. The real test, which is quite impossible in any precise way in historical work, would be to examine the extent to which kinship was given preference over other relational contacts (and the reasons for this preference), and the extent to which contacts with kin fulfilled functions which were not adequately met if kin did not provide them'.¹⁵

The point I want to make here would perhaps best be brought out if one were to compare Anderson's study of kinship with one carried out in contemporary society – let us say, for example, Claud Fischer's study of kinship and of other 'primary' relations in present-day San Francisco, *To Dwell Among Friends*.¹⁶ The only conclusion could be that the latter is greatly superior in the range and quality of data on which it draws, and in turn in the rigour and refinement of the analyses it can offer. And this point is, of course, not that Fischer is a better sociologist than Anderson but that he has an enormous advantage over Anderson in being able to generate his own data rather than having to rely on whatever relics might happen to be extant.

Turning to Marshall, one finds that he has problems essentially the same as those of Anderson. One of Marshall's main concerns is that Weber's position should be correctly understood – following the vulgarisations of Robertson, Tawney, Samuelson and other critics; and in this respect Marshall makes two main points. First, Weber was not so much concerned with official Calvinist doctrine on economic activity as with the consequences of *being* a believing Calvinist for the individual's conduct of everyday life – consequences which the individual might not even fully realise. In other words, Weber's thesis was ultimately not about theology but subculture and psychology. Secondly, Weber's argument was that the Protestant ethic was a necessary, but not a sufficient cause of the emergence of modern capitalism; there were necessary 'material' factors also – such as access to physical resources and to markets, the availability of capital and credit etc.

Thus, Marshall argues, in evaluating the Weber thesis, it is not enough to look simply for some overt association between theology, on the one hand, and the development of capitalist enterprise on the other. What is required is more subtle. It is evidence that believing Calvinists, on account of their acceptance of a Calvinist world-view, were distinctively oriented to work in a regular, disciplined way, to

pursue economic gain rationally, and to accumulate rather than to consume extravagantly – so that, *if* other conditions were *also* met, capitalist enterprise would then flourish.

Marshall's position here is, I believe, entirely sound. But it leads him to problems of evidence that he can in fact never satisfactorily overcome – despite his diligence in searching out new sources and his ingenuity in using known ones. And the basic difficulty is that relics from which inferences can systematically be made about the orientations to work and to money of early modern Scots are very few and far between.

In other words, what is crucially lacking – just as it was lacking for Anderson and indeed for Erikson – is material from which inferences might be made, with some assurance of representativeness, about the *patterns of social action* that are of interest within particular collectivities. As Clubb has observed, the data from which historians work only rarely allow access to the subjective orientations of actors *en masse*, and inferences made in this respect from actual behaviour tend always to be question-begging.¹⁷ And Marshall, it should be said, like Anderson, sees the difficulty clearly enough. He acknowledges that it may well be that 'the kind of data required in order to establish the ethos in which seventeenth-century Scottish business enterprises were run simply does not exist' – or, at least, not in sufficient quantity to allow one to test empirically whether Calvinism did indeed have the effect on mundane conduct that Weber ascribed to it.¹⁸

III

Let me at this point recapitulate. I have argued that history and sociology differ perhaps most consequentially in the nature of the evidence on which they rely, and that this difference has major implications for the use of history in sociology. I have presented a case of what, from this standpoint, must be seen as a perverse recourse to history on the part of a sociologist; and I have now discussed two further cases where, in contrast, such a recourse was justifiable, indeed necessary, given the issues addressed, but where, none the less, serious difficulties arise because of the inadequacy of the relics as a basis for treating these issues.

To end with, however, I would like to move on from these instances of sociologists resorting to history in the pursuit of quite specific problems to consider – with my initial argument still in mind – a whole *genre* of sociology which is in fact *dependent upon history in its very conception*. I refer here to a kind of historical sociology clearly different to that represented by the work of Anderson or Marshall, and which has two main distinguishing features. First, it resorts to history because it addresses very large themes, which typically involve the

tracing out of long-term 'developmental' processes or patterns or the making of comparisons across a wide range of historical societies or even civilisations. And secondly, it is based largely or entirely not on inferences from relics but rather on 'history' in the sense of what historians have written – or, in other words, not on primary but on secondary, or yet more derivative, sources.

The idea that sociologists might proceed by taking the results of historical research as their main empirical resource in developing wide-ranging generalisations and theories is not of course a new one. It was in fact a nineteenth-century commonplace. Its plainest expression was perhaps provided by Herbert Spencer when he wrote that, for him, sociology stood to works of history 'much as a vast building stands related to the heaps of stones and bricks around it', and further that 'the highest office which the historian can discharge is that of so narrating the lives of nations, as to furnish materials for a Comparative Sociology.'¹⁹

From the end of the nineteenth century, this understanding of the relationship between history and sociology met with severe criticism and rather rapidly lost support. Historians had indeed never taken kindly to the idea that they should serve as some kind of intellectual under-labourers; and sociologists became increasingly interested in developing their own methods of data collection.²⁰ However, in more recent times, a notable revival of what might be called 'grand historical sociology' has occurred. This was led by the appearance in 1966 of Barrington Moore's *The Social Origins of Dictatorship and Democracy*, and then consolidated in the USA by the subsequent work of Immanuel Wallerstein and Theda Skocpol, and in this country by that of Perry Anderson, with other authors such as John Hall and Michael Mann following in the wake.²¹ What I would now wish to argue is that the practice of these authors does in fact raise again all the difficulties inherent in Spencer's programme, and that the use of history in sociology as exemplified in their work is problematic in a far more fundamental way than in any of the studies earlier considered.

The authors in question would certainly not wish to represent their position in terms similar to those of Spencer. They would rather incline to the idea that history and sociology are one and indivisible; and, instead of viewing historians *de haut en bas*, they would surely wish to include them in the joint enterprise as equal partners.²² None the less, the fact remains that grand historical sociology in its twentieth-century form, just as in its nineteenth, takes secondary historical sources as its evidential basis, and must therefore encounter the methodological difficulties that are entailed – even though its exponents have thus far shown little readiness to address, or even acknowledge, them.

The root of their predicament is richly ironical. The revival of grand historical sociology can be seen as one expression of the

'reaction against positivism' within the sociological community to which I referred at the start; and yet its practitioners' own *modus operandi* – the use they seek to make of secondary sources – must depend upon what is an essentially positivistic conception of *historiography* – to which they would, I suspect, be reluctant to give any explicit support.

The catechism that I was put through as an undergraduate had a clear objective. It was to prompt a rejection of the view that the past – or at least certain well-documented aspects of the past, such as 'high' politics – could in principle be reconstructed, fact by fact, so that the distinction between history in the sense of what actually happened in the past and history in the sense of what is written about the past might be elided. Against this 'positivist' conception of historiography – as it was indeed labelled²³ – it was urged upon us that historical facts could not be cognitively established as a collection of well-defined items or entities, each independent of the rest, which, when taken together, would then dictate a specific and definitive version of the past. Rather, historical facts should be recognised as no more than 'inferences from the relics'; and inferences which had always to be weighted, so to speak, according to the security of their grounding, which were often interdependent – that is, stood or fell together – and which were of course at all times open to restatement, whether radically or through the most subtle changes of nuance.

Now, to repeat, I very much doubt if grand historical sociologists would wish to take up the defence of positivist historiography as against this latter view. But it is difficult to see how, *in practice*, they can avoid *assuming* an essentially positivist position. For even if the procedures they follow in producing their sociology do not actually require the elision of the two senses of history, they still cannot afford to recognise a too indeterminate relation between them.

Grand historical sociologists have to treat the facts, or indeed concatenations of facts or entire 'accounts', that they find in secondary sources *as if they were* relatively discrete and stable entities that can be 'excerpted' and then brought together in order that some larger design may be realised. In anti-positivist vein, Carl Becker has expressly warned that historical facts should *not* be thought of as possessing 'solidity', 'definite shape' or 'clear persistent outline', and that it is therefore especially inapt to liken them to building materials of any kind.²⁴ But the very procedures of grand historical sociologists push them back, willy-nilly, to Spencer's idea of using the stones and bricks of history to construct the great sociological edifice – and constructional metaphors do indeed reappear. Thus, for example, one finds Skocpol remarking 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'.²⁵

However, I would then wish to respond that the constructions that result are likely to be dangerously unsound. In particular, I would argue that in grand historical sociology the links, that are claimed, or supposed, between evidence and argument tend to be both *tenuous* and *arbitrary* to a quite unacceptable degree.

As regards the first charge, it is, I would suggest, instructive to consider some fairly specific argument advanced by a grand historical sociologist, and to note the 'authorities' that are invoked as providing its factual basis; then, to work back from these citations – through perhaps other intermediate sources that are involved – until one comes to direct references to relics of some kind. What, I believe, one will typically find is that the trail is longer and harder to follow than one might have expected, and that, not infrequently, it reaches no very satisfactory end.

For example, in *Social Origins of Dictatorship and Democracy*, Moore spends several pages reviewing aspects of English economic history over the late medieval and early modern periods, and then concludes as follows:

In the light of this general background there would seem to be little reason to question the thesis that commercially minded elements among the landed upper classes, and to a lesser extent among the yeomen, were among the main forces opposing the King and royal attempts to preserve the old order, and therefore an important cause, though not the only one, that produced the Civil War.²⁶

However, if one actually examines the sources that Moore cites, both before and after this passage, the grounding of his argument is very far from apparent. Indeed, it is quite unclear just what is the evidence, at the level of relics, in the light of which there would be 'little reason to question' the thesis that Moore advances. In the 'authorities' referred to – the main ones are Tawney's *Agrarian Problems of the Sixteenth Century*, his essay on 'The Rise of the Gentry' and Campbell's *The English Yeoman* – there is in fact remarkably little 'evidence' bearing in any direct way on the crucial link that Moore seeks to establish between economic position and political action.²⁷ And such as there is cannot be regarded as evidence in the sense that relics themselves are evidence or, for that matter, the data of a social survey are evidence. Rather, what one has are series of inferences, often complex and indeed often quite speculative, which are drawn from relics that are manifestly incomplete, almost certainly unrepresentative, and in various other ways problematic – as the authors in question are very well aware. In other words, such 'facts' as are here available cannot be understood as separate, well-defined 'modules', easily carried off for sociological construction purposes, but would be better regarded simply as strands in heavily tangled, yet still often rather weak skeins of interpretation.

In effect, then, what grand historical sociologists seem to me to be

generally doing is not developing an argument on the basis of evidence – in the manner of ‘primary’ historians or again of sociologists working on their ‘own’ research data – but rather, engaging in interpretation that is of, at least, a second-order kind: that is, in interpretation of interpretations of, perhaps, interpretations. And in consequence, I would maintain, the connection between the claims they make about the past and relics that could conceivably serve as warrant for these claims is often – as in the passage from Moore that I have quoted – quite impossibly loose. Following the practices that are here illustrated, history must indeed become, in Froude’s words, ‘a child’s box of letters with which we can spell an word we please’.²⁸

As regards my second charge, that of arbitrariness, the idea of historiography as a matter of inferences from relics that are finite and incomplete is again directly relevant. It follows from this that historians working on the same topic, and indeed on the same relics, may quite reasonably come to quite different conclusions – as of course they may for other reasons too. But it further follows that there may be little or no possibility of their differences ever being resolved – because the relics that would be necessary to settle the disputed issues simply do not exist. For grand historical sociologists, this then raises a major problem: where historians disagree, and may have perhaps to remain in disagreement, *which* secondary account should be accepted? By what criteria should the grand historical sociologist opt for one of two, or more, conflicting interpretations?

Thus, to return to Moore and his treatment of the economic and social origins of the English Civil War, the question one may ask is: why, on this notoriously controversial matter, and one plagued by a lack of relevant evidence, does Moore choose largely to follow what has come to be thought of (not altogether fairly) as the ‘Tawney’ interpretation rather than any of its rivals? By the time Moore was writing, it should be said, the idea that the ‘rising’, commercially oriented gentry were key actors in the parliamentary opposition to the King and his defeat in the Civil War was in fact fast losing ground among English historians, both to interpretations that gave the leading role to other socio-economic groupings and, more importantly, to ones that questioned whether political allegiance in the Civil War period had any close association at all with economic position and interest.²⁹

The answer to the question I have posed is, I believe, as obvious as it is unsatisfactory. Moore favours the interpretation that fits best with his overall thesis of the ‘three routes to modernity’; in other words, that which allows the English Civil War to be seen as an instance of a successful ‘bourgeois revolution’. However, he still fails to present any serious case for this choice. Supportive sources simply receive accolades, such as ‘excellent analysis’ or ‘unsurpassed account’, while less congenial ones are disparaged as ‘conservative historiography’.³⁰

This clearly will not do. But if mere tendentiousness is not the solution, what is? In the end, of course, any rational way of evaluating a secondary source must involve some judgment on the inferences made from the primary sources – that is, from the relics. But once this is recognised, the methodological bind in which grand historical sociologists find themselves becomes only more apparent. Their large designs mean, they tell us, that they cannot themselves be expected to work directly from the relics but must rely on the studies of specialist authorities. However, they are then either forced into positivistic assumptions concerning the ‘hardness’ and ‘solidity’ – and also the ‘transportability’ – of the evidence that these works can yield; or, if they accept that what these sources provide is no more than rival complexes of inference and interpretation, then they must explain how they propose to choose among them *without knowledge of the primary sources*.³¹

Since I have been so critical of the methodological basis of grand historical sociology, I should, before finishing, consider what its exponents have themselves had to say on the matter. In fact, as I have already implied, they have said remarkably little. Methodological issues tend to be raised, if at all, in the early pages of their books, but then only to be dealt with in a quite perfunctory – and unconvincing – manner.³² However, there is one statement by Skocpol, from the concluding chapter of the collection she edited, *Vision and Method in Historical Sociology*, which is of interest in several respects.

Skocpol writes as follows

Because wide-ranging comparisons are so often crucial for analytic historical sociologists, they are more likely to use secondary sources of evidence than those who apply models to, or develop interpretations of, single cases. . . . From the point of view of historical sociology, . . . a dogmatic insistence on redoing primary research for every investigation would be disastrous; it would rule out most comparative-historical research. If a topic is too big for purely primary research – *and if excellent studies by specialists are already available in some profusion* – secondary sources are appropriate as the basic source of evidence for a given study. Using them is not different from survey analysts reworking the results of previous surveys rather than asking all questions anew . . .³³

I would note, first of all, about this passage how clearly it shows the pressure that bears on grand historical sociologists to move towards the positivistic, Spencerian programme – ‘excellent’ historical studies by specialists can be ‘the basic source of evidence’ for the wide-ranging sociologist. And also revealing is the reference to ‘redoing the primary research’ – as if it were apparent that the same result as before would necessarily emerge.

Secondly, I would point out that Skocpol is quite mistaken in the

analogy she seeks to draw with survey-based research. The 'secondary analysis' of survey data to which she refers is different from the grand historical sociologist's use of secondary sources, precisely because it *does* entail going back to the 'relics': that is, at least to the original data-tapes and perhaps also to the original questionnaires or interview schedules. And it is then these materials that serve the secondary analyst as evidence – *not* the interpretations of the original analyst, which may be, and indeed often are, disputed. Thus, a closer parallel would be between the secondary analyst of surveys and the historian who again works through and reinterprets a body of source materials discovered and initially analysed by a predecessor.

Thirdly, I would remark that by way of providing a rationale for the methodology of grand historical sociology, Skocpol has little at all to offer. Apart from her – mistaken – *tu quoque* argument directed at survey researchers, all she in fact says is that it would be 'disastrous' for grand historical sociologists if they were to be forced back to primary sources – which is scarcely a way of convincing sceptics.

What is actually of greatest interest is what Skocpol goes on to acknowledge in the paragraph that immediately follows the one from which I quoted: namely, that 'it remains true that comparative historical sociologists have not so far worked out clear, consensual rules and procedures for the valid use of secondary sources as evidence' and further that in this respect 'varying historiographical interpretations' is one obvious problem to be addressed. 'Certain principles', Skocpol believes, 'are likely to emerge as such rules are developed'. But, one must conclude, so far at least, grand historical sociology is *not* significantly rule-governed; its practitioners enjoy a delightful freedom to play 'pick-and-mix' in history's sweetshop.³⁴

IV

To sum up, then, I have argued that the view that history and sociology 'are and always have been the same thing' is mistaken and – dangerously – misleading. Sociology must, it is true, always be a historical discipline; sociologists can never 'escape' from history. It is therefore highly desirable that they should be historically aware – by which I mean, aware of the historical settings and limits that their analyses will necessarily possess, even if they may never be precisely determined. But history and sociology can, and should, still be regarded as significantly different intellectual enterprises. A crucial source of the difference, I have sought to show, lies in the nature of the evidence that the two disciplines use – in the fact that historians have for the most part to rely on evidence that they can discover in the relics of the past, while sociologists have the considerable privilege of being able to generate evidence in the present.

As regards, then, the use of history in sociology, what I have sought to stress is that sociologists should not underestimate, or readily give up, the advantages that they can gain from having evidence that is 'tailor made', whereas historians have usually to 'cut their coats according to their cloth'. Where sociologists are compelled into historical research, by the very logic of their inquiries, then, I have suggested, they must be ready for a harder life – for research typically conducted, as one historian has put it, 'below the data poverty line'.³⁵ They must not only learn new techniques but also to accept new frustrations; in particular, those that come from realising that issues of crucial interest are, and will probably remain, beyond their cognitive reach. Historical sociologists such as Anderson and Marshall have learnt well; and much of what they can in turn teach us stems from their sensitivity to just what manner of inferences the relics available to them, and cannot, sustain. In contrast, grand historical sociologists seem to me to have, so far at least, shied away from the major intellectual challenges that historiography poses, and to have traded implicitly on a conception of it that I doubt if they would wish openly to defend. Until, then, they do meet the challenges before them, and provide a coherent methodology for their work, the question must remain of how far this does possess a real basis in the relics of the past – or merely an illusory one in a scattering of footnotes.

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NOTES

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1. P. Abrams, *Historical Sociology*, Bath, Open Books, 1980, p. x; A. Giddens, *Central Problems in Social Theory*, London, Macmillan, 1979, p. 230.

2. The distinction originates in the *Methodenstreit* in nineteenth-century German universities. For a brief discussion, see R. G. Collingwood, *The Idea of History*,

Oxford, Oxford University Press, 1946, pp. 165–83. An interesting example of its use in the period referred to in the text, with the aim of differentiating yet at the same time showing the complementarity of history and sociology, is R. Bierstedt, 'Toynbee and Sociology', *British Journal of Sociology*, vol. 10, no. 2 (June 1959), pp. 95–104.

3. J. H. Goldthorpe, 'The Relevance of History to Sociology', *Cambridge Opinion*, no. 28, 1962, pp. 26–9.

4. Clearly, my position has in important respects changed since the time of my earlier paper – as a result, I would like to think, of my having had much more experience, whether first- or second-hand, of research into societies both past and present. However, both history and sociology, and the typical orientations of

their practitioners, have also changed. Today, interdisciplinary, or rather a-disciplinary, enthusiasm would seem to me to have gone much too far, at least on the sociological side. And I find it of interest that a similar view has also been taken from the side of history by a distinguished practitioner who is by no means unsympathetic to sociology: see L. Stone, 'History and the Social Sciences in the Twentieth Century' and 'The Revival of Narrative: Reflections on a New Old History' in *The Past and Present Revisited*, London, Routledge, 1987.

5. Quoted in Stone, 'History and the Social Sciences in the Twentieth Century', p. 31.

6. This use of history is that which I have in fact been most concerned in my own work on comparative social mobility. The classic programme for a comparative macro-sociology is that set by A. Przeworski and H. Teune, *The Logic of Comparative Social Inquiry*, New York, Wiley, 1970, which has as its ideal objective 'the replacement of the names of nations with the names of variables'. In so far as, in explaining cross-national variation in social structure or process (e.g. in mobility rates and patterns), the sociologist is forced into invoking institutional or cultural features, or indeed events, as specific features of national histories, then *pro tanto* the Przeworski-Teune programme must fall short of realisation. Cf. R. Erikson and J. H. Goldthorpe, 'Commonality and Variation in Social Fluidity in Industrial Nations. Part I: A Model for Evaluating the "FJH Hypothesis"; Part II: The Model of Core Social Fluidity Applied', *European Sociological Review*, vol. 3, nos. 1 and 2, May and September, 1987, pp. 54-77, 145-66.

7. For pertinent but brief comments by previous authors, see T. H. Marshall, 'Sociology - the Road Ahead' in *Sociology at the Crossroads*, London, Heinemann, 1963, p. 38 esp.; and C. Bell and H. Newby, 'Narcissism or Reflexivity in Modern Sociology', *Polish Sociological Bulletin*, no. 1 1981, pp. 5-19.

8. I was myself put through the catechism by G. J. Renier, a remarkable teacher, whose book *History: its Purpose*

and Method, London, Allen and Unwin, 1950, was our main text and is now unduly neglected. Also influential was Collingwood, *The Idea of History*, especially the Epilegomena.

9. Cf. M. G. Murphey, *Our Knowledge of the Historical Past*, Indianapolis, Bobbs Merrill, 1973; J. M. Clubb, 'The "New" Quantitative History: Social Science or Old Wine in New Bottles?' in J. M. Clubb and K. Scheuch, eds., *Historical Social Research*, Stuttgart, Klett-Cotta, 1980, pp. 13-24.

10. The one instance of which I am aware in which historians likewise generate their evidence is when they engage in 'oral' history. Here too, though, it may be noted that problems of survival, and in turn of representativeness, are of large importance.

11. Another way of putting much of this is to say, as does Clubb ('The "New" Quantitative History', p. 20) that 'The source materials upon which historians must rely are virtually by definition "process produced"' and that they are, moreover, 'the residual process-produced data that have survived the ravages of time'. Clubb notes that historians occasionally have at their disposal data that were collected for social scientific purposes, and that this is likely to be a more common situation for future historians. However, he then rightly comments that '... we can also imagine that historians in the future will regard these data as no less process-produced in this case by the process of social research as archaically practiced in the mid-twentieth century - and will bemoan the fact that the wrong data were collected, the wrong questions asked, and that underlying assumptions and methods were not better documented.'

12. K. Erikson, *Wayward Puritans*, New York, Wiley, 1966, pp. vii-iii, emphasis in original.

13. Skocpol treats Erikson's intentions as being 'characteristic of historical sociologists who apply general models to history'. See 'Emerging Agendas and Recurrent Strategies in Historical Sociology' in T. Skocpol, (ed.), *Vision and Method in Historical Sociology*, Cambridge, Cambridge University Press, 1984,

p. 364. There can of course be little value in such a procedure unless there are *independent* grounds for believing that the models have some validity. But it should in any event be noted that Erikson himself is clear that his concern is (see text) 'to examine several ideas about deviant behaviour' – for which he does not appear to claim any prior validity.

14. It may also be argued that sociologists have a legitimate recourse to history where their concern is with phenomena such as revolutions, major economic crises, mass panics or crazes etc., which not only happen rather infrequently but are in any event more amenable to investigation in retrospect than as they occur. I am not fully convinced by this argument but, for present purposes, it is not necessary to contest it. Nor do I take up here a concern with history displayed by some sociologists that I would most certainly regard as illegitimate: that is, a concern with 'theorising' history so as, it is hoped, to secure a cognitive grasp on its 'movement' or 'logic'. I have written critically elsewhere on the persistence of such historicism: see e.g. J. H. Goldthorpe, 'Theories of Industrial Society', *Archives Européennes de Sociologie*, vol. 12, no. 2, 1971, pp. 263–88, and 'Intellectuals and the Working Class in Modern Britain', Fuller Memorial Bequest Lecture, University of Essex, 1979.

15. M. Anderson, *Family Structure in Nineteenth Century Lancashire*, Cambridge, Cambridge University Press, 1971, p. 62.

16. C. Fischer, *To Dwell Among Friends*, Chicago, Chicago University Press, 1982.

17. 'The "New" Quantitative History', p. 20.

18. G. Marshall, *Presbyteries and Profits*, Oxford, Oxford University Press, 1980, p. 35.

19. H. Spencer, *An Autobiography*, London, Williams and Norgate, 1904, vol. 2, p. 185 and *Essays on Education*, London, Dent, 1861, new ed. 1911, p. 29.

20. An early but cogent, and, I suspect, highly influential, attack on Spencer by a pre-eminent historian was F. M. Maitland, 'The Body Politic', *Collected Papers*, (ed. H. A. L. Fisher), Cambridge, Cambridge University Press, 1911. Note also Collingwood's critique of the last phase of

'scissors-and-paste' historiography, that of the 'pigeon-holders', whose approach was: 'Very well: let us put together all the facts that are known to historians, look for patterns in them, and then extrapolate these patterns into a theory of universal history.' *The Idea of History*, pp. 263–6. On the sociological side, the late nineteenth and early twentieth centuries saw of course the beginnings in Britain of sample survey methods and a growing interest in other means of data collection. Cf. S. and B. Webb, *Methods of Social Study*, London, London School of Economics, 1932.

21. B. Moore, *The Social Origins of Dictatorship and Democracy*, Harmondsworth, Penguin, 1966; I. Wallerstein, *The Modern World System*, 3 vols., New York, Academic Press, 1974, 1980, 1989; T. Skocpol, *States and Social Revolutions*, Cambridge, Cambridge University Press, 1979; P. Anderson, *Passages from Antiquity to Feudalism*, London, New Left Books, 1974 and *Lineages of the Absolutist State*, London, New Left Books, 1974; J. Hall, *Powers and Liberties*, Harmondsworth, Penguin, 1985; M. Mann, *The Sources of Social Power*, Cambridge, Cambridge University Press, 1986. It might be argued that this 'new wave' of grand historical sociology was in fact led by S. N. Eisenstadt's study, *The Political Systems of Empires*, New York, Free Press, 1963. But Eisenstadt's influence would seem to have been clearly less than that of Moore – chiefly, I suspect, because his highly academic structural functionalism accorded far less well with the prevailing mood of the later 1960s than did the *marxisant* tone and explicitly 'radical' commitment of Moore's work.

22. Thus, for example, in the collection of essays edited by Skocpol, *Vision and Method in Historical Sociology* consideration is given to the work of historians such as Marc Bloch, Charles Tilly and E. P. Thompson alongside that of authors such as Eisenstadt, Moore, Wallerstein and Anderson. Admirers of Bloch, in particular, might well be led to ask 'Que diable allait-il faire dans cette galère?'

23. See, for example, Collingwood, *The Idea of History*, pp. 126–33. Then, as apparently later (cf. E. H. Carr, *What is*

History?, London Macmillan, 1961, ch. 1), the classic expositors of such positivism in historiography were taken to be von Ranke and, in Britain, Lord Acton.

24. C. Becker, 'What are Historical Facts?' in H. Meyerhoff, (ed.), *The Philosophy of History in our Time*, New York, Doubleday, 1955, pp. 120–37.

25. T. Skocpol, *States and Social Revolutions*, p. xiv.

26. B. Moore, *Social Origins of Dictatorship and Democracy*, p. 14.

27. R. H. Tawney, *The Agrarian Problem in the Sixteenth Century*, London, Longmans, 1912, and 'The Rise of the Gentry, 1558–1640', *Economic History Review*, vol. 11, no. 1, 1941, reprinted with a 'Postscript' in E. M. Carus-Wilson, *Essays in Economic History*, London, Arnold, 1954; M. Campbell, *The English Yeoman*, New Haven, Yale University Press, 1942. It must be emphasised that none of these three studies is in fact concerned with the Civil War in any direct way, and that references to it occur only rather incidentally.

28. J. A. Froude, *Short Studies on Great Subjects*, London, Longmans, 1884, vol. 1, p. 21.

29. An essay important for its catalytic effect was J. H. Hexter, 'Storm over the Gentry', which initially appeared in *Encounter*, no. 10 (1958) and then in an enlarged version in Hexter's *Reappraisals in History*, London, Longmans, 1961. For a more recent critique of 'social change explanations' of the English Civil War – but certainly not one that could be dismissed as sociologically unsophisticated – see J. C. D. Clark, *Revolution and Rebellion*, Cambridge, Cambridge University Press, 1986, ch. 3 esp.

30. See, for example, *Social Origins of Dictatorship and Democracy*, pp. 6, 14 and the Appendix. In the Appendix, 'A Note on Statistics and Conservative Historiography', Moore takes up the difficulties posed for his interpretation of the Civil War by D. Brunton and D. H. Pennington's *Members of the Long Parliament*, London, Allen and Unwin, 1954, which, as Moore notes, led Tawney himself to acknowledge that the division between Royalists and Parliamentarians within the Long Parliament 'had little connection

with diversities of economic interest and social class'. Moore then tries (pp. 511–2) to rework Brunton and Pennington's statistics to save what he takes to be Tawney's thesis against Tawney's own abandonment of it – but succeeds only in providing a nice example of the ecological fallacy.

It might be added here that the treatment of the English Civil War by both Wallerstein and Anderson is no more satisfactory. Wallerstein, who claims that 'contrapuntal controversial work' is a positive advantage for his enterprise (*The Modern World System*, vol. 1, p. 8) reviews a wider range of literature than Moore but by an eirenic *tour de force* still ends up where he wants to be: i.e. able to claim that the English Civil War, though not a direct struggle between classes, none the less resulted from the formation of an agricultural capitalist class which the old aristocracy was forced to accommodate and in part to merge with, thus leading to the early creation in England of a 'national bourgeoisie' (see esp. pp. 256, 269, 282, 297). It must, however, be pointed out that of the 'authorities' whom Wallerstein cites, at least as many would reject this conclusion as would accept it.

Anderson, in contrast, refers to only a very limited number of secondary (or tertiary) sources and then, effectively disregarding all controversy, blandly asserts (*Lineages of the Absolutist State*, p. 142): 'English Absolutism was brought to a crisis by aristocratic particularism and clannic desperation on its periphery; forces that lay historically behind it. But it was felled at the centre by a commercialized gentry, a capitalist city, a commoner artisanate and yeomanry: forces pushing beyond it. Before it could reach the age of maturity, English Absolutism was cut off by a bourgeois revolution.' Once more, it must be emphasised that it is essentially the interpretation of the English Civil War as a 'bourgeois revolution' that has been challenged by 'revisionist' historians over the last two decades or more.

My own judgment would be that the revisionists have indeed succeeded in undermining the supposed evidence for

such an interpretation. But, further, I would doubt that even if there *were* a valid 'social change explanation' of the English Civil War, adequate relics could be found to allow its validity to be demonstrated. What Hexter remarked ('Storm over the Gentry', p. 149) apropos the initial Tawney *versus* Trevor-Roper debate is likely to remain the last word: 'And what such masters of the materials of seventeenth-century history and of historical forensics cannot prove when they set their minds to it, is not likely ever to be proved.'

31. Where historians themselves draw on secondary sources, as for example, in situating their own 'primary' research or in writing 'surveys' of a field, issues of the availability, quality etc. of sources are typically discussed. Moreover, in the latter case at least, and likewise in the writing of textbooks, authors are not under pressure to defend a particular interpretation but can present a review of different positions. Grand historical sociologists, in contrast, usually cannot afford such even-handedness; they need to use – that is, to choose among – secondary sources as evidence for or against a particular thesis. Furthermore, the central theses that are argued for by authors such as Moore, Wallerstein and Anderson are ones which they themselves clearly see as being politically highly consequential, so that questions of how far their use of secondary sources is politically influenced, and of what checks on political bias *they* would believe appropriate, inevitably arise.

32. See, e.g., Moore, *Social Origins of Dictatorship and Democracy*, pp. x–xi; Skocpol, *States and Social Revolutions*, pp. xiv–xv; Anderson, *Passages from Antiquity to Feudalism*, p. 8; Mann, *The Sources of Social Power*, pp. vii–viii, 3–4, 31–2.

33. T. Skocpol, (ed.), 'Emerging Agendas and Recurrent Strategies in Historical Sociology', p. 382.

34. Unlike Skocpol, the other authors earlier cited do not even appear to

recognise the need for a methodology. Their main justification for grand historical sociology would seem to be simply that it gives 'the broad view' and is thus a necessary complement to 'specialists' history. Thus Moore writes (*Social Origins of Dictatorship and Democracy*, p. xi): 'That comparative analysis is no substitute for detailed investigation of specific cases is obvious.' But he goes on: 'Generalizations that are sound resemble a large-scale (*sic*) map of an extended terrain, such as an airplane pilot might use in crossing a continent. Such maps are essential for certain purposes just as more detailed maps are necessary for others.' Moore's cartography inspires no more confidence than his historiography. Assuming that in the above he means 'small-scale' not 'large scale', a small-scale map, useful for an 'extended terrain', is dependent for its accuracy on the detailed surveying from which it is built up. And likewise, as a 'cliometric' and a 'conventional' historian have written together, 'the quality of an historical interpretation is critically dependent on the quality of the details out of which it is spun. Time and again the interpretation of major historical events, sometimes of whole areas, has been transformed by the correction of apparently trivial details . . .' See R. W. Fogel and G. R. Elton, *Which Road to the Past?*, New Haven, Yale University Press, 1983, p. 125.

It should also be said that the methodology of grand historical sociology has attracted little attention from writers concerned with the methodology of the social sciences in general. One essay by Johan Galtung may be noted, though its contribution to practice does not seem large: 'Om makrohistoriens epistemologi og metodologi: en skisse', Nordisk Fagkonferanse for Historik Metodelaere, *Makrohistorie*, Oslo, Universitets-forlaget, 1979.

35. Clubb, 'The "New" Quantitative History', p. 20.

