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Michael Mann

In praise of macro-sociology: a reply to Goldthorpe¹

In the pages of this *Journal* John Goldthorpe recently (1991) attacked what he called 'grand historical sociology' (GHS) and argued that its methods and data were far inferior to those of sociologists who study the present. He named me as one of the GHS cast, if only in a walk-on role. I have to confess I find his polemic rather elusive. He is abusive yet poorly informed about GHS while being obsequious toward historians, for him the true custodians of the past.² The payoff from his attack is also feeble: merely a warning that historical evidence, what he calls 'the relics', constitute rather imperfect evidence. Yes, indeed. None the less, it is worth responding to his attack, principally to remind sociologists of how essential grand macro-sociology is to our discipline.

HISTORY AND SOCIOLOGY

(a) *Definitions*

Are there different subject-matters for history and sociology? Goldthorpe says 'yes'. He stresses the differences between sociological and historical terrains and methods and prefers that each cobbler sticks to his/her last. For him sociology is the study of present society, while history is (uncontentiously) the study of the past. But his stress on their differences is undercut by his recommendation that both disciplines pursue an empiricist methodology.

I define sociology differently, as the science of society – regardless of tense. By 'science' I mean systematic knowledge, the attempt to use a systematic methodology to generate a generalized form of knowledge. 'History' does not necessarily pursue such a form of knowledge, though it may. This renders the two academic disciplines neither identical nor fundamentally different: they differ but overlap.³ Their routine practices differ more since (especially in Britain) historians are trained in the idiographic, the particular and the context, sociologists in general concepts and in the scientific method. Yet there is no *necessary* connection between the past and the idiographic and between the present and the nomothetic.

However, there are differences between methods appropriate for studying past and present. Goldthorpe emphasizes that whereas study of the past (history) depends entirely on sources which are 'finite and incomplete relics', the study of the present (sociology) may actually create new, fuller, better data through active fieldwork. Though this has some truth, I would qualify it in two ways. First, sociologists of the present cannot go out and create whatever data they like. Many institutions – especially those frequented by the powerful – remain closed to them. Try going with tape-recorder and interview schedule to the Bundesbank! The survey has a democratic bias: it gives credence to the experience of ordinary individuals and weighs them equally. Adding up the sum of individual responses will not reveal the structures of society, since it neglects power relations. In the future surveys will be supplemented with memoirs and institutional archives not yet available to us. Second, created data bring their own problems. They interpose the mind and practices of the sociologist between data and reader. If the sociologist has also transformed 'raw' data into scales, equations etc., these transformations also stand between the recording of reality and the reader. Moreover, such sociologists are subject to a vice almost unknown among those who study the past – 'dataphilia', the love of the data one has created and then transformed. For dataphiles their measures come to stand for reality itself – as we see in contemporary sociology journals. I later discuss Goldthorpe's own dataphilia.

(b) Defending grand historical sociologists

If sociologists could derive from the present abstract propositions applicable to all societies, and they preferred the data-quality of surveys of the present, they could ignore the past altogether. Indeed, most sociologists do study the present, though their motive is rather simpler, to understand their own society by the most economical route. Actually, only a few sociologists believe they can generate abstract propositions about social relationships that are independent of time and place. 'Rational choice theorists' are one such group, and two of them have recently criticized the same GHS cast (especially Skocpol and myself) from the opposite wing to Goldthorpe, arguing that we should couch our hypotheses in abstract, context-less terms.⁴ Goldthorpe and I would seem to agree that such a level of abstraction is impossible. Context matters – the most we can attain is generalization about macro-contexts like 'advanced capitalism', 'the modern nation-state', 'the nuclear family' etc. These are the stuff of what we normally call general theory in sociology.

Yet – as we all learned as students – most general theory in sociology emerged from attempts to understand the appearance and development of capitalism and industrialism in the West. It is thus rooted in

grand historical sociology. From Marx, Durkheim and Weber, through Parsons, Aron, Marshall and Bendix to current sociologists like Giddens, Lipset or Skocpol, most sociological theorists have used as their data-base the present-and-past-combined of western society (sometimes compared with other civilizations, and sometimes – as with Parsons or Giddens – blended with more abstract endeavours). Americans call this tradition *macro-sociology*, the term I shall use here. Macro-sociology is what the cast named by Goldthorpe attempt to do, and it is what most of the greatest sociologists have always done.

Macro-sociologists use the past in three main ways.

(i) An *historical-causal* analysis of origins considers the conditions which gave rise to modern institutions relevant to understanding their present nature and likely persistence. Durkheim's study of religion is a classic example, Ingham's (1984) study of British capitalism and Moore's *Social Origins of Dictatorship and Democracy* (1969) more recent ones.

(ii) A quest for *variation* seeks to understand modern institutions like capitalism or the nation-state or patriarchy by comparing them with social groups with other types of economic, political or gender institutions. Weber's comparative analysis of religions is a classic example, the 1970s Marxian 'mode of production' wave and some macro-feminism (e.g. Lerner 1986) more recent ones.

(iii) A more *abstract-comparative* macro-sociology, less tied to understanding the present, analyses the past to test more general propositions about human communities. This has been typically a late twentieth-century quest: for example, the search for 'last instance' power relations existing between political elites and dominant economic classes. The recent research on this question is voluminous, covering such topics as nobles and monarchs in early modern Europe (Lachmann 1990), Bismarckian welfare legislation (Steinmetz 1990), New Deal legislation⁵ and US foreign and military policy since the 1930s (Krasner 1978, Domhoff 1990, Hooks 1991). Most uses of data files like those of the Human Relations Area Files or 'The Correlates of War' Project have a similar abstract-comparative purpose.

Now Goldthorpe appears to be arguing that modern sociology has so improved its techniques of analysis that it can depart from all three quests. I will show that this is not so, using the example of his own work. But I do not require that most, or even many, sociologists pursue such quests (I am no imperialist *within* sociology), only that some good ones do (for it is a difficult enterprise, requiring command of both large quantities of information and theoretical rigour). Yet *none* of these three methods, *pace* Goldthorpe, is much interested in the particularities of history. Evidence from the past is instead essential to their systematizing drive. Goldthorpe accepts the currently dominant view among British historians that history *is* the study

of the particular. *But the past is intrinsically no more particular than the present.* A sociology confined to the particularities of our present or the past would be parochial and methodologically inadequate for all our grander theoretical projects. Thus we *have to* throw away the advantages (and disadvantages) of creative fieldwork in the present over part of the terrain from which we generate macro-sociology. That is my defensive position in support of grand historical sociology. I now move onto the attack.

(c) *Attacking historians*

Sociologists actually have a *second* advantage over most current historians, but this one is usable on the past. It is grounded not in the nature of their respective objects of enquiry, but in their professional training. Sociological training always includes substantial immersion in *theory* about how societies work – the problem of order, the relationship between beliefs, actions and structure, materialism versus idealism, the relations between ethnicity, classes, churches/sects, states etc. etc. It backs this up empirically with immersion in comparative and historical sociology and anthropology. Sociologists are expected to acquire some knowledge of how a large diversity of societies have operated. The training of historians – especially in Britain – rarely includes either theory or comparative social structure. But we sociologists can insert a given social group amid a much broader range of the ways humans have organized their relations with each other.

Naturally historians do have general theories, but they rarely parade them on the page. Their implicit theories usually reflect the conventional wisdom of the writer's time and place. In their writings on the English Civil War (Goldthorpe's principal example) the politics of most of the contending historians show through clearly – right up to Jonathan Clark, whose 'sophistication' is commended by Goldthorpe. I would hope that most sociologists' theories were a little subtler than these politically-driven agendas.

Thus on the very terrain where historians operate – interpreting the relics – macro-sociology enjoys a superiority. I have read a very large number of historians' books and articles. I have great respect for their skills with the relics and for their intuitive feel for their period, both of which are far greater than mine. I do not wish to bite one of the hands that feed me. But I find most historians far less sophisticated when generalizing about the societies they are studying. I also find annoying the commitment of mediocre historians to a 'pointilliste' methodology of piling on the quotations and the particularities without trying to arrange their evidence in more systematic ways.⁶

Of course, the label 'historian' covers a multitude of practices, and some historical fields show greater interest in theories of social

structure – for example, classicists, sinologists and Islamicists. Many archeologists show as much interest in social theory as do sociologists. Archeology is, of course, the discipline *most* confined by ‘finite and incomplete relics’. But most leading archeologists do not settle for the rather passive empiricism – sticking entirely to limited relics – Goldthorpe recommends. They try to make sense of their relics by putting them within the broader framework of meaning provided by sociological and anthropological theory and by the comparative sociology of early and later societies. The study of the European Bronze Age or the study of pre-Columbian Meso-America now provide analyses of class, status and political power rivalling what we professional sociologists produce (they read us – perhaps we should read them).

But compared to most historians macro-sociologists have far more experience in dealing with theories. We also have a rather broader view of social and historical possibilities. Historians tend to specialize in a confined period and – to a very disturbing extent – in the territories constituted by a single contemporary nation-state. Few know much about the very broad range of societies studied by their peers, still less about the broader range known to sociologists and anthropologists. Few know much about the process of human development. In short, most know little about the past.

Goldthorpe is for live-and-let-live with the historians, each group of empiricists keeping to demarcated terrains (that is the way an upstart sociologist – an expert in social mobility to boot – makes his way in Oxford). I prefer to take further the imperialism of Comte and Spencer. I cannot merely use the ‘facts’ which ‘underlabourer’ historians produce for my theories. Goldthorpe’s base (history)/superstructure (GHS) model argues that we must defer to historians’ superior knowledge of the facts – we must accept their accounts of the relics as ‘entire accounts’. If they disagree he thinks we have ‘a major problem’: ‘*which* secondary account should be accepted?’ (1991: 223). I fail to understand this. 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 historians’ theories). Historians’ facts are mediated by the (normally implicit and commonsensical) social theories of their own time and by the requirements that the academic historian contribute to current scholarly debates. The latter are much like the academic pressures on most of us – not merely to support conventional wisdom, to position ourselves in relation to Professor A and B or School A and B, to boldly declare a revisionism etc. etc. So macro-sociologists read Professor A *and* Professor B, School A *and* B, to see how they introduce different facts or how they interpret the same facts differently, to recognize from whence their interpretations derive, to make judgments about

sociological plausibility based on other times and places, to ignore a loudly-declared revisionism which to the outsider seems mere detail, to make consequent judgments about facticity. We reject part of *all* their accounts.

EPISTEMOLOGY AND METHOD

Beneath all this may lurk an epistemological divide. Goldthorpe writes as an empiricist.⁷ His position would seem to presuppose that the facts are independent of our perceptions, to be obdurate and unchanging. My own position is more Kantian, a kind of 'as if' positivism. We must act as if this were true, since there is no stopping short of relativism if we do not. We must present the facts as we see them, engage with the contrary facts produced by others and demonstrate to the satisfaction of third parties that our perception generates more explanatory and predictive power. I also accept the sociological conclusion that Weber drew from Kant: that facts are only interpretable, and can only be grouped into sociologically-significant categories, in terms of meaning-systems. For a sociologist theory and data perennially enmesh and correct one another. Both empiricist historians and sociologists neglect this to their great cost. We need theories to make sense of, and to systematically organize, our data (and we need good data to evaluate our theories).

Thus the methodology of macro-sociologists differs considerably from that of most historians today, but it differs little from that of other sociologists. We construct hypotheses from theoretical hunches, consult alternative hypotheses, refine our own after initial skirmishes with data, and set out our refined theory more explicitly than is common among historians. We attempt to operationalize the variables involved, ensuring adequate variation on the critical variables, and we try to collect relatively systematic data, hating mere illustration, preferring quantitative data if we can get our hands on them. Like sociologists in any field, we do not stick rigidly to the 'hypothetico-deductive sequence', nor do we necessarily present our research findings in this form. The more general the questions asked, the more difficult is the research task, the more approximate become the indicators, the more spotty becomes the coverage of the variance. Very often we must guess or admit ignorance. That is true of many of the most important macro-issues, whether we are attempting to understand secretive finance capital in the present or feudal states in the past, whether generalizing about the preconditions of western capitalism or the viability of modern socialism.⁸ Our reach far exceeds our grasp, but we reach toward an understanding which is essentially social-scientific.

And if we still feel too dependent on the primary research of others,

we can take one further step. If we remain dissatisfied with historians' accounts and cannot decide among them, we *can* go to the relics ourselves. Goldthorpe insists only the professional historian has access to the relics. This is not true. Not all primary material is inaccessible to us, nor do we all leave it entirely alone.

MACRO-SOCIOLOGICAL PRACTICE

I briefly examine macro-sociological practice. Four recent sociological usages of the French Revolution offer a typical range of recent work. Skocpol's now-classic study of modern revolutions (1979) depends almost entirely on secondary accounts. Runciman (1983) and Goldstone (1991) also relied on secondary sources. Yet my own study (1993: Chap. 6) also makes use of primary sources – *memoires* and other published works by revolutionaries and counter-revolutionaries. Nor do Skocpol nor Runciman argue much with their sources (though both side with some historians against others). Yet Goldstone often argues that the secondary sources provide unbelievable or inconsistent accounts, especially on demographic and economic matters, and he extensively recalculates the quantitative data provided by historians. I also recalculate, both state budgets and the social and cultural backgrounds of the revolutionaries. None of us read as deeply as a specialist historian of the French Revolution. But we had broader theoretical agendas and our hunches about the Revolution derived from those. Our theories enabled us to focus a narrow but powerful searchlight on the Revolution, finding patterns in the data to which historians had not been sensitive, and finding inconsistencies or implausibilities in their accounts. And macro-practitioners are often forced either into a creative use of secondary data or into primary research because theory leads us to ask questions of the data which historians have not asked.

Not all historians had ignored the geopolitical-fiscal causes of the Revolution.⁹ But Skocpol was the first to produce a more general theory of revolution as the joint outcome of pressures put on states by geopolitics and agrarian class conflict. Nor had historians ignored the contingent aspects of the Revolution, so stressed by Runciman. Yet his general theory of evolutionary, reformist and revolutionary change (1989: esp. 361–7) allows him to distinguish more clearly those old regime practices which were structurally-doomed from those which fell more contingently in the Revolution. Their accounts come closest to a more generous version of Goldthorpe's base/superstructure model, in which historians provide the data, sociologists the theory. Goldstone also started from theory, his rather original attempt to interpret revolutions in neo-Malthusian terms. Though I cannot quite agree with this theory, it led Goldstone to quite original historical

analyses: on eighteenth-century France, for example, he gives the best analysis to date of the relations between the economy and state finances (showing that no sector of the French economy was very 'backward', France just had too much agriculture, and that the main fiscal weakness was the Crown's inability to tax high-profit sectors). Goldstone does not generalize 'above' the historians' facts, as in the base-superstructure model: he has changed historians' and sociologists' beliefs about the nature of those facts (as one prominent historian of the Revolution, William Doyle, has noted).

Thus between 'primary' and 'secondary' research lies borderline territory where we may derive original data from 'secondary' sources. I use my own research for further illustration.

The main relics for researching English medieval state finances are the bi-annual Exchequer Rolls. Each, when unrolled, is the length of a cricket pitch, written in the clerks' own coded form of Latin. I realized at once that I was not a medieval scholar! But, thankfully, scholars over the last hundred years have produced monographs containing simple tabular presentations of revenue and expenditure for various periods. Reading their footnotes carefully, noting their methodological remarks, sometimes recalculating their estimates, I could construct (1984, 1986) the first available seven century time-series for English state finances. I had to accept part but not all of what historians told me. I had to accept that they (and not I) could read the original script and transcribe and tabulate it accurately. I also had to accept some combination of their various methods of data categorizations. But that is all. I was unconcerned with their interpretations of the politics of the day, unconcerned with their theories of kingship, Church-State relations or the rise of Parliament. I did not have to accept their research as an entire account.

When I arrived at eighteenth-century and nineteenth-century statistical sources, the task was much easier, since these were actually 'published' for official use in languages which are intelligible to me. I still needed some guidance from expert historians, but my final data-set on the finances (and manpower) of five states over this period is, I believe, the best available in the literature (Mann 1993: Chaps 11, 14 and Appendices). It results from the very fact that I am not a specialist, and so was able to ask questions of the data inspired by other countries and centuries and by sociological theory.

Another example comes from my current research. The age at which most leading Russian revolutionaries were first known to have been engaged in illegal political activities is available through published collections of biographies.¹⁰ I provisionally calculate the median age among the leading 64 male Bolsheviks to have been seventeen (the 4 females were almost seven years older). Thus we must look at the high schools, at apprenticeships and at other juvenilia for better understanding of Bolshevism (since the median age among the

Socialist Revolutionaries was 22, to understand them we must also turn to the universities). And the Bolsheviks' occupational careers – an important part of the attempt by historians to establish their 'class background' – turns out to be substantially contaminated by their adolescent political activities and by police harassment. This empirical line of enquiry is in part (though not entirely) original. Yet this research is largely from secondary sources.

It is in fact common for macro-sociologists to contribute original empirical findings whether or not they base this on large quantities of primary research.¹¹ Of course, macro-sociology is not brimming over with primary research. But nor is the sociology of the present. Sociology journals (especially in the USA) are full of secondary analyses of data collected by government agencies or polling firms. Few sociologists nowadays do their own interviews. Since they undertake more data transformations, it becomes more difficult to use their 'raw' data in secondary analysis. A tiny number of those doing secondary analysis return to the interview schedules of the primary studies (as Goldthorpe recommends). A rather larger minority get what are termed 'clean' copies of the data-tapes – and this is Goldthorpe's own more normal practice in mobility research. That is fine – he, like us, can run to the primary research materials if challenged. But supposedly ideal empiricist methods are rare sociological practice.

When many data transformations are undertaken, social reality may lie farther away from the researcher than it does in macro-sociology. And where large data-sets are treated as 'complete' and 'clean', and where computers are employed to do many elegant transformations, sociologists may be seduced into dataphilia and forget the reality out there. In macro-sociology evidence is limited yet we know something about what happened next. This means that what we are trying to explain constantly looms in front of us as a finished jig-saw for which we know we only have some of the pieces. We may be tempted toward teleology but at least we cannot love our data.

Goldthorpe's main target for attack is a book relying wholly on secondary analysis, Barrington Moore's *Dictatorship and Democracy* (1969) – not on his more research-oriented *Injustice* (1978). He could have discussed Hall's *Powers and Liberties* (1985) or Giddens' *The Nation-State and Violence* (1985). These are all grand-generalizing books, depending on historians' secondary accounts. But they are also profoundly theoretical books and so also depend on the classical theories of Marx, Weber, Adam Smith, the body of subsequent macro-sociology and the general state of modern western self-reflection. Since their authors all have something original to say, they blend theory and data to depart from the points of view of specialists and theorists alike.

Actually, Goldthorpe only snipes at Moore, carefully staying right

away from substantive arguments. He refers only to Moore's partial use of sources on the English Civil War, apparently unaware his criticisms were made years ago (though with greater generosity of spirit) by the very macro-practioners he attacks (e.g. Skocpol 1973; for a review of criticisms, see Smith 1983). Twenty-six years after the first publication of *Dictatorship and Democracy*, we can clearly see its weaknesses – not only in terms of subsequent historical scholarship but also in terms of the limitations of Moore's theoretical orientation (a kind of 'Marxian liberalism'). Moore did not fully prove his case. But the grandeur of his enterprise and the importance of his conclusions – an *explanation* of why our civilization has generated both democracy and dictatorship (what could be more relevant to us?) – resonated among a whole generation of sociologists and historians. His conclusions provided testable propositions about the preconditions of democracy. Some of the most important have stood up well to a battery of subsequent testing.¹² Of how many works of 1966 sociology could one say that? Are there *any*?

This leads me to a puzzle I have with Goldthorpe's attack. What is its *point*? What is the *general* significance of attacking Moore for selective use of sources on the English Civil War? There is none since Goldthorpe concedes the entire case for macro-sociology. He says that 'sociologists can never "escape" from history' (1991: 225), that our theories can never be 'of an entirely transhistorical kind' (1991: 212), that we 'are compelled into historical research' if we are concerned with social change 'over a period of past time which has dates . . . and that is related to a particular place' (1991: 216). Obviously 'time' refers not just to dates like '1789' but also to broader temporal processes like 'the rise of modern society' or 'the development of industrial (or post-industrial) society'. Equally, some 'places' are very big ones – 'Britain', 'Europe', 'the West', 'the global community'. Thus sociology which is neither parochial nor concerned to generate wholly abstract propositions about the nature of any human community must include the study of the past in its framework. Since Goldthorpe says he accepts all this, what is left of his polemic? All that remains is his warning of the dangers lurking amid the relics. I thank him for that warning, since he may only be trying to help. Let me try to reciprocate with warnings of my own – since empiricism lames sociologists as it does historians.

THREE WARNINGS FOR EMPIRICISTS

(a) *Think macro-sociologically*

If we do not think in macro-sociological terms, then our empirical work will reproduce the conventional theories of our own socialization

experiences. A generation before either Goldthorpe or I had the dubiously enriching experience of confronting some of the major economic, ideological, military and political dislocations of the twentieth century. Thus its macro-sociology was intellectually ambitious, aimed at major world issues (Moore is a good example). But post-war consensus then lulled the next generation into thinking that the temporary arrangements and ideologies of its own nation-state were eternal. This is evident, for example, in British writing on social stratification: the facticity of occupational class relations within a single nation-state remains largely unquestioned, while the institutionalized competition of bourgeois and social democratic parties still dominate the ways such classes are analysed. Thus the authors of the major recent British study of social stratification (Marshall *et al.* 1988) – in many ways an excellent study – asked many questions (or rather the polling firm it hired asked many questions) about occupations, class identity and voting intentions, but few questions about other sources of social identity or stratification. As Peter Saunders has remarked, if you ask questions only about class, then what you get out are classes.

But stratification and classes concern matters besides occupations. Sociologists like myself became aware that the post-war consensus was blinding us to matters of nationalism and internationalism, authoritarianism and democracy, war and peace, and to the peculiarities of states. Probably far more became sensitized to matters of gender. We have mostly turned to such topics not just because of their intrinsic importance, but also because they directly impinge upon social stratification. The quest returned us to more macro traditions. We have brought substantial findings into sociology on all these matters – on the causes of capitalist take-off, of democracy, of patriarchy, of the rise and decline of civilizations and nations, and the changing nature and continued importance of states, militarism, ethnicity and gender today.¹³

These diverse quests contain a major theoretical contribution: an emphasis on the *contingency* of social structures. I mean by this not indeterminacy but rather (*pace* Durkheim) that social facts are not things. Their existence and shape as ‘facts’ actually depend on a whole web of other social arrangements whose shapes and inter-relations are changing. Thus any particular arrangement of classes depends, for example, not merely on structures we generally term as ‘economic’ but also on familial, geographical, political and other structures, all of which are changing and which are also changing the shape of classes (and vice-versa). For example, classes used to be treated largely within the confines of nation-states. Now, thanks to macro-sociology (and to changes in global capitalism), we know they are variably (across times, spaces and classes) ‘national’, ‘international’ (or ‘nationalist’) and ‘transnational’. As others have noted

(e.g. Rose 1992), and as I argue in the next section, they are also variably gendered.

(b) Avoid dataphilia

If we ignore this fluidity, we reify our concepts and measures. Let me instance Goldthorpe's own mobility research. This is undeniably impressive. If one were tempted to claim that the more precise methods of contemporary sociology have rendered obsolete the traditional quests of macro-sociology, one might instance Goldthorpe's massive compilation of secondary analysis of surveys, computer programs, equations and data transformations. Yet its theoretical scope remains rather narrow. Erikson and Goldthorpe's (1992) book tests what they call 'liberal' and 'Marxist' theories of mobility. Liberalism asserts that mobility has increased in linear fashion across advanced capitalism in the twentieth century, Marxism that it has linearly decreased. Though I remember such views, I wonder who adheres to them nowadays. But this project is driven by data, not theory. Its main thrust is methodological: to create a single hierarchical scale of class based on the past occupations of samples interviewed in various countries in the 1970s. They are then able to refute the crude 'liberal' and 'Marxist' theories. They show mobility is something of 'a constant mess', changing but not growing or declining substantially over the mid-twentieth century, with international differences which are neither great nor easily attributable to one transnational trend or to individual national uniquenesses.

Though they produce many interesting detailed results, their overall pattern is essentially one of 'no results'. This leaves us in a quandary. Is society really like this, a bit of a mess? I tend to think it is. But confusion might result because their measure does not create meaningful data. A *very* large number of data transformations occur between interview and published results. It is also difficult to convert an enormous variety of occupations held over fifty years to a single scale or indeed to use it to summarize a lifetime's achievements. Perhaps meaning has evaporated along the way.

Erikson and Goldthorpe tested for this possibility by assessing the performance of their scale compared to alternative occupational scales in predicting variance in two dependent variables, subjective class identity and voting behaviour. Theirs did somewhat better and so must have some meaning, though it explains only a little of the variance. Is asking respondents about which class they are in tapping something really salient in their lives? Is voting the most salient experience in non-sociologists' lives? We might ask whether positions on this scale correlate with more important social experiences – for example with readily-available mortality data. Does each 'class' live longer than the next one lower down – an important and very

'material' test? Or does movement up or down the scale help explain collective behaviour during the twentieth century (the original assumption made by some liberal and Marxist writers)? Erikson and Goldthorpe have caressed their scale, and protected it against its rivals, but they have not used it for these or other significant sociological purposes. Perhaps they will in the future. Yet Goldthorpe's polemic does not augur well. Nor does his first foray with these data into macro-controversy, his dispute with the feminists.

Feminists have argued that Erikson and Goldthorpe's scale is sexist since it measures only the occupations of men (women were first assigned the occupational status of the leading male in their household). Erikson and Goldthorpe responded with a characteristic empirical test: predicting variance in the same two dependent variables from their own male-dominated scale, compared to a scale based on the individual man or woman. Theirs did better. They then made one concession: to base their scale on the highest occupation in the household, regardless of gender (though this is usually held by a man). Then they continued to assign most women's class position by the occupation of the dominant male in their household.

Having made this concession, Erikson and Goldthorpe seem to have the better of this particular argument. *If* we wish to measure class position by a single occupational scale, and *if* we then wish to call movement up and down this scale 'social mobility', theirs may be the best of the alternative measures proffered. But operational purposes should always be subordinated to substantive ones – in this case to understand the relations between class, the household and movement experienced as 'upward' or 'downward'. They have failed to do this. Their obsessive defence of their own scale indicates dataphilia triumphing over macro-sociology.

Every measure has certain costs. This one, being a single continuous scale applied right across the twentieth century, will have problems *if* relations between economic life-chances, occupations and households differ either at different points in the hierarchy or at different points in time, and *if* these differences are relevant to sensed experience of 'upward' or 'downward' movement. Both difficulties arise.

Let me instance some recent trends in the USA. First, since around 1970 individual male incomes have declined in real terms by about 20 per cent, yet household incomes have held steady. The main reasons are that women are working longer and children are staying longer in the parental home. The household composed of men, women and children all working is becoming more economically necessary, while the proportionate economic contribution of the man to the family has been declining for half a century. The 'family wage', that staple of turn-of-the-century collective bargaining, is nearly dead. There is great public consciousness of this in America and debate over whether mass inter-generational downward economic mobility is occurring.

Second, one-fifth of households with children are headed by a single parent, of whom four-fifths are non-working women. Their life-chances are poor (they constitute over a third of those classified as living in poverty). Theirs is profound downward mobility, more maritally than occupationally caused: by separation, divorce or because they never married the father(s).¹⁴

The scale of the second US trend results partly because aid to parents of dependent children is the only important permanent welfare benefit available there. But welfare states play important roles in all countries – as does the growth of divorce and separation. These are general problems for contemporary class analysis. In all of them we see the close and complex interaction of occupation and gender – and of other social identities like ethnicity and citizenship status. The relations between family, occupation and economic hierarchy have varied through time and are still varying. No one-dimensional scale can apply equally across times, places or classes.

Erikson and Goldthorpe (1992: 275–7) argue that gender and class inequalities are not closely connected and that women's experience of mobility differs little from that of men. Their argument is contaminated by two types of dataphilia. First, they label their own occupational scale with the prestigious sociological term, 'class'. Yet class comprises far more than just a constructed hierarchy of occupations. In most traditional conceptions of class occupation entwines with property and other market resources. Second, they are testing for the presence of a single hierarchical effect of gender on their own occupational hierarchy. But 'gender' is also multi-dimensional. Most women still suffer sexual discrimination in employment. Collectively they occupy several 'buffering zones' between occupations that are typically male. If women marry, they share in the (usually greater) resources of their partner. If they have children, they forego considerable income and become more dependent on their men. But if they do not share a household with a man, most experience severe downward mobility and many poverty. But all this varies by ethnicity, occupation (unevenly in the case of women themselves, since professional women but few others have made significant economic progress), by welfare state entitlements, citizen status etc. Thus sex, marital statuses, life-cycle position and household compositions entwine to produce what we label 'gender' effects. Even less than 'class', can it be simplified onto one dimension.

But the finished product, 'class', actually results as *all* these economically-relevant forces entwine – occupation/property/market relations, sex/marital status/household composition, ethnicity, nation etc. These entwinings have varied historically and some seem to be changing rapidly now. If we reify our scales, we miss this. We should analyse all this – applying quantitative methods where we can – instead of merely defending our beloved data. Macro-sociology can help in

this task since it has studied variation through time, as well as place, and has theorized the relations between class, gender, nation, ethnicity etc. It perceives, for example, that the family wage, masculine labour movements, the classic nuclear family (all presupposed by Erikson and Goldthorpe as constant) have been time-and-place specific, not invariant features of advanced societies.

Erikson and Goldthorpe (1992: 226) state their ambition of moving on to study the formation of classes as real collective actors (rather than just operational aggregates of occupations). It is difficult to see how they can do this without consulting the relics. Those already consulting them have situated classes as actors amid changing configurations of national and transnational capitalism, amid nation-states, amid wars over territory and citizenship and amid national, ethnic and gender identities. Classes have *always* come entwined with such phenomena. They are *contingent* in the sense I specified earlier. The goalposts of social structure keep moving. If dataphiles do not take note of all this, their measure will merely reproduce the goalposts of their youth. The future sociologist will come across their massed surveys, secondary analyses and data transformations and discard them, for they asked the wrong questions.

(c) *Get your timing right*

Sociology is a practical discipline, seeking to understand and perhaps even change its own society. It is important we do socially-relevant research. Here I simply marvel at Goldthorpe's timing. For a prominent European sociologist to advocate such a massive separation between the practices of history and sociology *in 1991* is as bizarre as it is appalling. It is important that sociologists, especially current sociology students, do not take him seriously.

Many of the goalposts of modern society have just moved. Thus our concepts are in need of some revision. The collapse of Communism and of the last European Empire, its implications for what feminists have thought of as female 'progress', the resurrection of German power and of the balance between the Atlantic Community and *Mittleuropa*, the resurgence of ethnicity and nationalism, the continuation of 'the Moore problem' of the struggle between democracy and dictatorship, the suggested 'post-modern' fragmentation of classes and nation-states, the faltering of social democracy, the problematic relationship between transnational capitalism and the nation-state in a post-Cold War era – are not these problems of our age which require from us *more* macro-sociology of past and present?¹⁵

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NOTES

1. I would like to thank John Hall and Nicky Hart for their comments on earlier drafts of this paper.

2. Abuse and obsequiousness alternate in his footnote 22, for example. His ignorance of the field is revealed in his statement that GHS is 'Marxisant'. Yet – contrary to his assertion in footnote 21 – Hall, Skocpol and myself refer as much to the structural-functionalist Eisenstadt as to Marxist work. Skocpol has quite relentlessly attacked Marxist theories (in her version of the French Revolution there is virtually no bourgeoisie, in her Russian Revolution virtually no proletariat; her later work on American government seeks to demolish class explanations). She, Runciman, Collins, I and others are often labelled 'neo-Weberians'. John Hall's first love is clearly Adam Smith. Goldstone is clearly attached to Malthus and the virtues of social mobility. And Marxists themselves snipe at Perry Anderson for supposed Weberianism and at Wallerstein for 'Smithianism'. There are few reds under the GHS beds.

3. Few GHS practitioners are committed, as Goldthorpe asserts, to the view that history and sociology are 'one and indivisible'.

4. Kiser and Hechter (1991) focus on 'state autonomy', using rational choice theory to generate propositions about how all states can maximize their autonomy from civil society. Yet their propositions turn out to be context-specific. For example, their proposition that 'the dependence of rulers varies directly with the mobility of subjects' resources' may work for the relations of modern states with finance capital (mobile) versus industrial capital (mobile), but not for most ancient and medieval states who tended to ally with merchants (mobile) against landowners (fixed) – Eisenstadt (1963) brilliantly analysed such rulers' attempts to generate mobile 'free-floating' resources. Kiser and Hechter also assume that states, personified by state elites, can be treated as actors. This is a dubious proposition (some parts of some states can be so treated, others cannot), from

which Skocpol has moved away and against which I specifically argue (1993: Chap. 3).

5. Here the literature is voluminous and contentious. Contrast, for example, Weir and Skocpol (1985) with Domhoff (1990). Numerous articles in the ASR over the period 1990–92 have continued this debate.

6. Of course, we sociologists can be just as mediocre, but our failings are not these ones.

7. It is difficult to be certain, because his argument is overlaid with bitter asides about the sociological fads of the 1960s and 1970s.

8. Thus some well-known problems of macro-historical method – such as the difficulty of finding enough independent cases to use Mill's 'method of difference' (as advocated by Skocpol) – apply equally to grand issues of the present. Though Goldthorpe asks GHS practitioners to be more explicit about their methods, he is apparently unaware of their voluminous methodological writings – see the review contained in Kiser and Hechter (1991).

9. Behrens (1967) had stressed this, against the predominantly 'social' interpretations of the time.

10. Though a little primary research, conducted with the aid of Ernest Zitzer, helped, especially with the lesser figures.

11. Apart from works already cited, let me instance some of the best recent examples in English. Dandeker (1990) fits his own research into British military history into a broader comparative and theoretical sweep (influenced by Weber, Foucault and Giddens). Fligstein (1990) uses corporate documents and business histories in his 'anti-economistic' theory of the development of the modern corporation. Hamilton's *Who Voted for Hitler?* (1980) exhibits a fine sociological imagination in its use of ecological (voting) data, rigorous research design and creativity in securing variation on indicators (for example, he uses the polling stations at spas and on ocean liners to indicate upper class voting patterns). Hage *et al.* (1989) generated an original data set from the post-1871 fiscal records

of states to test state autonomy theory. Rueschemeyer *et al.* (1992) analyse the preconditions of democracy in comparative and historical perspective, mainly though not entirely with secondary sources. Tilly's latest book (1990) does not present new primary research, but it presupposes decades of such practice. The breadth of his coverage of different types of state also leads him to new conclusions. Wallerstein's (1989) own world-systems theory continues to generate new ways of looking at the works of historians. Watkins (1991) collected a demographic data-set, reinforced with detailed research on French history, to test hypotheses concerning the growth of the modern nation-state. The contributors to Weir *et al.* (1989) mix primary and secondary research to different degrees. We await a forthcoming volume by Skocpol from this project. Note that Domhoff (1990: Chap. 4) has criticized this research project for doing less primary research than he does in establishing which persons were responsible for American social and economic policy. These are all persons whose principal training was in sociology (sometimes combined with political science) – Tilly is a borderline case.

12. Particularly his argument that modern authoritarianism had agrarian roots, among dominant coalitions of large landholders using 'labour-repressive agriculture' in alliance with crowns and state bureaucracies. For this support, together with criticisms, see Stephens (1989).

13. A brief review of some British work can be found in Hall (1989) and Anderson (1990). For a more systematic review of British and American work, see Smith (1990).

14. For a useful recent overview of these trends, concentrating on race, see Hacker (1992).

15. Though I doubt sociology's capacity for prediction at the macro-level, two GHS practitioners did come very close in predicting the demise of the Soviet Union – see Collins (1986) and Goldstone (1993 – though his claims are disputed in the same journal issue by Keddie, Arjomand and Tilly). I, alas, was not so perspicacious.

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