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Progress in Sociology: The Case of Social Mobility Research

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**PROGRESS IN SOCIOLOGY: THE CASE OF SOCIAL MOBILITY  
RESEARCH**

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DRAFT, OCTOBER, 2003, COMMENTS WELCOME.

## INTRODUCTION

The issue I start from is that of whether sociology progresses. Is it possible to demonstrate a cumulative growth in sociological knowledge and understanding? Of late, sociologists themselves have often given negative, or at best uncertain, answers to this question.

There are, for example, those who believe that it is mistaken in principle to expect progress in sociology. An extreme, 'post-modernist' view is that scientific progress is in general an illusion; all knowledge is in fact 'locally' conditioned and, thus, relative. But a somewhat more plausible claim is that while progress can, and does, occur in the natural sciences, the social sciences are a quite different kind of undertaking in which the possibility of progress is far more doubtful, and especially as regards theoretical knowledge.

Thus, authors such as Bryant (1995: 4-6) and Flyvbjerg (2001: 25-37) have contended that all attempts in sociology to develop 'theory proper' - i.e. theory in the sense of the natural sciences that aims at steadily growing explanatory power across a range of empirically established phenomena - are doomed to failure. For these authors, the underlying problem is ontological in character. In Bryant's words, for theory proper to be possible in sociology, human societies would 'have to be constituted differently from the way they are'. The argument sustaining this position is not easily followed. It starts from the observation that while the natural sciences deal with physical entities, the social sciences are concerned with 'self-reflecting humans' (Flyvbjerg, 2001: 32) who 'construct' their own social world. On this basis, two further claims

are then made - with, so far as I can see, no very adequate logical or empirical backing: first, that since sociologists are part of the 'reality' they study, their concepts are necessarily dependent upon, and in constant interaction with, those that are embedded in the everyday lives of 'lay' actors; and, second, that all propositions made by sociologists that aim at generality will thus be rendered inherently unstable, since such propositions will in fact need to change in response to changes in lay actors' own understandings and interpretations of their social worlds - and including those that are prompted by the practice of sociology itself .<sup>1</sup>

However, despite the unsatisfactory nature of the argument, the conclusion to which it supposedly leads is clear enough. Sociology has to be an essentially hermeneutic discipline whose practitioners, like lay members of society themselves, can aim only at interpretation, not explanation, of the social world, and thus at producing not science, at least on anything like the natural science model, but rather 'discourse' of, perhaps, some moral and pragmatic significance.<sup>2</sup>

Another group of authors can be identified who are less concerned with *a priori* arguments than with the apparent *fact* that progress in sociology has, so far at least, not matched that achieved in the natural sciences. Thus, Cole (1994; cf. also 1992) maintains that while in sociology and the natural sciences alike disputes about what counts as knowledge are quite common on the 'periphery' - where science is, as it were, in the making - sociology is then far less successful than the natural sciences in the transfer of knowledge from the periphery to the 'core': that is, to a growing body of knowledge that is

generally accepted as valid and important. Cole sees two main difficulties facing sociology in this regard. The first is again one that relates to the phenomena with which sociology deals: whether or not because of fundamental ontological differences, these phenomena are more *mutable*, over time and space, history and culture, than those dealt with by the natural sciences. The second difficulty is that sociologists, in choosing the problems they study, are far more likely than natural scientists to be influenced by personal interests, values and ideologies than by purely cognitive considerations - including that of how 'do-able' a particular research project is.

Rule (1997) takes up similar questions to Cole but focuses on the way in which sociological theory, rather than developing systematically, both prompted and constrained by research, tends more to follow intellectual taste and fashion. In sociology building on what went before does not carry the same kudos as being *à la mode*. In the end, though, Rule is somewhat more optimistic than Cole. 'Theory for coping' - i.e. theory that tries to provide some understanding of how societies objectively form and constrain the experience of their members - shows, he believes, more evidence of progress than 'theory as expression', which seems chiefly to be a reflection of the experiential reality of those individuals who produce it.

In what follows, I address issues of the kind raised by the authors I have referred to, though taking the second group a good deal more seriously than the first. My argument is therefore far more empirical than philosophical or methodological in character and, moreover, relates to just one area of

sociological work: i.e. the study of social mobility. My purpose will be to show that in this case at least progress in sociology can, in some meaningful sense, be documented - which is in itself enough to undermine the more extreme 'impossibilist' position, without need to enter further into its obscure foundations. I accept that important differences exist between the natural and the social sciences, broadly on the lines indicated by Cole. But these I take as being differences of degree, not of kind, that do not require or justify any kind of intellectual *apartheid*. I also accept that what may be true of social mobility research need not - indeed does not - hold good for other areas of sociology, and, in conclusion, I therefore make some suggestions as to why this might be so.

Social mobility research is of course a field in which Robert Erikson has made a major contribution - and in which I have had the good fortune and great pleasure to work for long with him. If, in what follows, the advances that I claim in this field are often linked to Robert's name, then that will be fitting enough on this occasion. I am, however, uncomfortably aware that I might at times appear to be creating glory in the reflection of which I can myself bask. By way of offsetting this possibility, I would here note - and consistently with my general theme - a distinctive feature of achievement in science, as distinct, say, from the arts: that is, that while such achievement may be associated with the names of particular individuals, it is in a more fundamental sense *collective* in character. The best indication of this is that if any truly scientific achievement had not been made by X or Y, then, sooner or later, it would have been made by Z - in contrast to an achievement such as, say, *Hamlet*, which, if it had not been written by Shakespeare, would not have been written

at all (cf. Wolpert, 1992). In so far, then, as I am correct in holding that, in social mobility research, sociology can show something recognisable as scientific progress, all 'individual' contributions will need to be understood as grounded in, and as expressions of, an enterprise in which many others were integrally involved.

## **PROGRESS IN SOCIAL MOBILITY RESEARCH**

Discussion of the possibility or actuality of progress in sociology has tended to centre on theory. However, while a special importance may indeed attach to theory, questions of progress in other respects need not, and should not, be neglected. I therefore consider progress in social mobility research under the heads of data, concepts and analysis, and empirical findings before coming to theory. In no case, I must stress, do I try to provide anything like a comprehensive account of developments over the half-century or more in which the study of social mobility has formed an identifiable specialism within sociology; nor do I focus on the 'leading edge' concerns of the present day. I concentrate, rather, on what seem to me to be the clearest instances of progress in the sense of developments leading to the growth of 'core' knowledge in Cole's sense.

### ***Data***

If one examines the first general treatise on social mobility, that of Sorokin (1927/ 1959), one finds that the data on which the author was able to draw

were both limited and at the same time highly heterogeneous. They consisted for the most part of the assessments of historians of rates and patterns of different forms of mobility in various societies of the past - usually based on quite fragmentary sources - supplemented by a miscellany of more contemporary studies, most often ones concerned with what would now be called 'elite recruitment': i.e. studies of mobility *into* such fairly restricted social groups as millionaires, industrialists, political leaders and 'men of genius'.

Taking this situation as baseline, it would then seem evident enough that very substantial progress as regards data has been made, and on two main fronts: *coverage* and *quality*.

In the case of coverage, the key developments could be listed as follows.

- (i) The growing availability, at least across economically more advanced societies, of data on social mobility derived from surveys representative of national populations. The pioneering study here was that directed by Glass in Great Britain in 1947 (Glass, ed., 1954) which produced a wide range of information relevant to the intergenerational mobility of adult men and women and also detailed family and employment histories.<sup>3</sup>
- (ii) The replication over time of such nationally-based mobility studies or the collection of data relevant to mobility in relatively frequent 'general-purpose' national surveys. In this way, substantial data-sets of 'repeated cross-sectional' format have been built up, some now extending over three or four decades, that provide a sound basis for establishing societal trends in mobility

- (iii) The supplementation of data on mobility from one-off or repeated cross-sectional inquiries (and from retrospective life-history studies) by data from prospective 'panel' or 'birth-cohort' studies. Such studies allow an alternative perspective on mobility trends and also provide the most appropriate kind of data for testing theories of the causal processes that underlie mobility rates and patterns.

In the case of data quality, the following advances could be singled out.

- (i) Improvements in the coding of data relevant to mobility, in particular occupational and educational data. In Glass's study, for example, occupational data were 'category' coded to the very informally constructed Hall-Jones prestige scale - with demonstrably low reliability. In later studies, the 'index' coding of occupational data to relatively well specified scales or classifications has become standard.
- (ii) Tests of the accuracy of information reported in interviews and of the overall degree of reliability of data, taking into account both recall and recording as well as coding error. Such tests, while giving generally satisfactory results, have also identified instances where reliability is likely to be lowest - e.g. respondents' reports on their own, or their parents', occupations in the fairly distant past (cf. Hope *et al.*, 1986; Breen and Jonsson, 1997).
- (iii) Improvements in the comparability of data used in cross-national studies of mobility. In early studies of this kind in the tradition of Lipset and Zetterberg (1956), comparability was sought, though not

very successfully, simply through the collapsing of the coding categories used in different national inquiries to some 'lowest common denominator'. In later work, however, occupational scales specifically devised for cross-national research (e.g. Treiman, 1977; Ganzeboom and Treiman, 1996) have been used; or original data have been re-coded to widely applicable - and by now widely employed - classifications, notably the CASMIN class schema (Erikson and Goldthorpe, 1992) and the CASMIN educational classification (König *et al.*, 1988; Brauns and Steinmann, 1999).

Those who believe that nothing recognisable as scientific progress occurs, or should indeed be expected to occur, in sociology would probably regard advances of the kind noted above as being 'merely technical' in character and of little intellectual significance. But such a view would reveal a rather basic misunderstanding of science and, in particular, of the way in which, as the history of the natural sciences well illustrates, progress is in fact often directly dependent upon developments in observational methods that allow new and better data to be produced. The maxim that 'New instruments make new science' is well-founded (Crump, 2001). Moreover, as Steuer (2002) has argued, one of ways in which the social sciences could be thought to differ - in degree - from the natural sciences is that, partly on account of the greater mutability of the phenomena with which they deal, they tend to call for a 'more painstaking and ingenious uncovering of the facts'. Thus, for the social sciences, improvements in techniques of data collection will often represent a distinctive challenge and, insofar as they are brought about, an achievement of corresponding importance.

### ***Concepts and Analysis***

In social mobility research the question of the *conceptual context* within which mobility should be studied has from the first received a good deal of attention. Should mobility be defined, observed and measured in terms of, say, a hierarchy of occupational prestige or socioeconomic status or, alternatively, of positions identified within an occupational or class structure (compare e.g. Glass, ed., 1954 and Svalastoga, 1959 with Carlsson, 1958).

However, it is not in the resolution of conceptual issues of this kind that progress is to be looked for - i.e. in the sense of a steady movement towards 'the one best way'. In fact, while divisions remain among researchers about which approach is, overall, the most revealing, there is by now fairly wide agreement, first, that different approaches are more or less appropriate to different problems; and, second, that the empirical findings that emerge within alternative conceptual contexts differ in their detail rather than in their more salient and consequential features.<sup>4</sup> In other words, a clear indication is here provided that concepts and their provenance are less crucial in constructing sociological knowledge than those who deny the possibility of a scientific sociology would seem to suppose, and that more important are the *propositions* to which concepts, as applied in research, actually give rise (cf. Popper, 1976: 21-8 esp; 1994: ch. 2.).

Where conceptual progress may properly be sought, and indeed found, in mobility research is where it is associated with the solution of specific analytical problems, and ones that arise however the context of mobility may

be understood. In this regard, progress can most readily be seen in the analysis of intergenerational mobility (or, more generally, of mobility envisaged as transitions between 'origins' and 'destinations' rather than in a complete life-course perspective).<sup>5</sup>

In the work of Glass and others of his generation, the main basis for the analysis of mobility was a contingency table - now known as the 'standard mobility table' - in which individuals' origins, as categorised according to status or class, were crossed with their destinations, categorised in a similar way. Analysis then consisted of various operations performed on counts in the internal and marginal cells of the table. In such analysis, two major problems arose, in dealing with each of which later researchers can claim to have achieved genuine advances in 'ways of sthinking' about social mobility.

- (i) While the standard mobility table provided an appropriate basis for the calculation of percentage 'outflow' and 'inflow' rates of mobility, it was not easy to adapt it so as to bring into the analysis factors of likely importance in mediating mobility - for example, education. Progress in this respect, was, however, made, chiefly under the leadership of Duncan (Duncan and Hodge, 1963; Blau and Duncan, 1967), through the adoption of a regression approach. Destination became the dependent variable, while origin was an independent explanatory variable taken together with education and whatever other intervening variables might be deemed of interest. Moreover, in so far as these latter variables could be placed in some likely temporal sequence, path-analytic techniques could be used in order

to 'decompose' the gross correlation of origins and destinations into a direct and a series of indirect effects. In this way, then, the resolution of a technical difficulty went together with a fairly radical conceptual reorientation. The relationship between origins and destinations was no longer treated simply in terms of 'mobility' but was rather seen as the outcome of a *process* of 'status attainment' (since Duncan and his associates worked chiefly with a scoring or ranking of occupations on a socioeconomic status scale).

- (ii) The standard mobility table also served as a basis for efforts to separate out the effects on observed (e.g. percentage) mobility rates of, on the one hand, differences between the two marginal distributions of the table (seen as the source of 'structural' mobility) and, on the other, the pattern of net association prevailing between origins and destinations (seen as the source of 'exchange' mobility). However, efforts in this direction remained unsatisfactory until a new approach via loglinear modelling was introduced by Hauser (Hauser *et al.* 1975; Hauser, 1978). This allowed patterns of origin-destination association within the mobility table to be analysed in a 'margin-insensitive' way; and, at the same time, led to a progressive shift away from the old idea of total mobility being made up of structural and exchange components to the more viable and revealing distinction between absolute and relative mobility rates (Goldthorpe, 1980/1987; Erikson and Goldthorpe, 1992).

It would be true to say that the two developments noted above had certain divergent aspects. The status attainment approach, while facilitating multivariate analysis, was most readily implemented with continuous variables - such as status scores, years of education etc. - and by making the (heroic) assumption of linear and additive effects. In contrast, loglinear modeling, while remaining largely bivariate, made possible detailed analyses of mobility tables, organized on the basis of class or occupational categories, through which 'endogenous mobility regimes', could be identified: i.e. persisting patterns of relative rates or, alternatively, varying levels of (net) association for different origin-destination transitions.

However, in recent years further progress has been made in integrating these two approaches and combining their strengths. Most importantly, the possibility has been developed (Logan, 1983; Breen, 1994) of reformulating loglinear models for the grouped data of standard mobility tables as multinomial logistic regression models for individual-level data, in which a range of other variables, whether categorical or continuous, can be included. Analyses that follow this strategy (e.g. Hendrickx and Ganzeboom, 1998; Breen and Goldthorpe, 1999, 2001) are in fact now producing new empirical findings, especially in regard to the role of education in mobility processes, that have in turn important theoretical implications, as will seen in the sections to follow.<sup>6</sup>

Again, then, the advances here reviewed cannot be dismissed as of only technical interest. What is indicated is that it is in grappling with, and solving, technical problems, rather than through *lucubrations de chambre*, that

conceptual advances of real consequence are most likely to be made. To claim that the possibility of progress in sociology is compromised by the dependence of its concepts on those of lay members of society is in fact to overlook the capacity of working sociologists to form concepts of their own that are distinct from those of lay members and that, as the next section in particular will show, are crucially involved in the growth of knowledge of a kind that lay members, in the course of their everyday lives, have little need, as well as little opportunity, to acquire.

### ***Empirical findings***

In this regard, progress is best demonstrated by the establishment, in an increasingly refined form, of a series of empirical regularities that extend across a relatively wide range of institutional and cultural contexts. Such progress has been most marked in the study of intergenerational mobility - again as in the case of conceptual and analytical developments - and has indeed occurred in close conjunction with the latter. Of the findings in question, the following could be reckoned most important.

- (i) Endogenous mobility regimes - or patterns of relative mobility rates - show a high degree of temporal stability within national societies, often remaining only little changed over many decades (Erikson and Goldthorpe, 1992; Wong, 1994). Statistical models postulating constant relative rates over such time periods typically misclassify less than 5 per cent of all cases in standard mobility tables. The change that does occur is mostly non-directional in character - i.e.

not uniformly towards more or less equal relative rates but where it is directional, it is more often towards more equal relative rates or, that is, towards increased rather than decreased fluidity (Erikson and Goldthorpe, 1992; Breen, ed., forthcoming). However, in most national societies such increases in fluidity turn out to be limited to a particular period or birth cohort rather than being continuous.<sup>7</sup>

- (ii) Endogenous mobility regimes also show a notable degree of cross-national commonality, at least so far as the overall pattern of relative rates is concerned (Featherman *et al.*, 1975; Grusky and Hauser, 1984; Erikson and Goldthorpe, 1992: chs. 4 and 5). Claims of national 'exceptionalism' as regards unusually high (or low) levels of social fluidity or 'openness' are thus called into question. At the same time, though, the significant cross-national differences in endogenous mobility regimes that do exist can be more readily related to nationally-specific institutional or cultural factors or historical circumstances than to more general societal processes, such as industrialisation or modernisation, or to types of political regime (Erikson, 1990; Wong, 1990; Erikson and Goldthorpe, 1992; Rijken, 1999).<sup>8</sup>
- (iii) As a corollary of (i) and (ii), variation in absolute mobility rates, which is often substantial both over time and cross-nationally, has to be attributed overwhelmingly to structural effects: i.e. to shifts in the distributions of populations over the levels of status or the

occupational groups or classes in relation to which mobility is defined.

- (iv) In all modern societies the most important factor mediating intergenerational mobility is individuals' educational attainment; other relevant individual characteristics, such as IQ or motivation, appear to operate to a large extent via educational attainment. However, no society has yet become a true 'meritocracy' in the sense that individuals' social origins and destinations are statistically independent once education - or IQ, motivation or other 'merit' variables - are controlled: a 'direct' effect of origins persists. (Marshall *et al.*, 1997; Breen and Goldthorpe (1999, 2001). Moreover, the importance of education in mediating mobility does not steadily increase over time. On the one hand, the association between origins and educational attainment (controlling for the direct effects of educational expansion) weakens, if at all, only slowly (Blossfeld and Shavit eds. 1993); and, on the other hand, several recent national studies indicate that the association between education and destinations is itself now showing a tendency to weaken rather than to strengthen (Breen, ed., forthcoming).
- (v) In the associations between origins, education and destinations, a significant interaction effect is regularly present (contrary to the assumption of linear and additive effects in standard path-analytic models). The association between origins and destinations tends to

be weaker at higher educational levels than at lower (Hout, 1988; Breen and Jonsson, 2003; Vallet, forthcoming); or, in an alternative interpretation, the association between educational attainment and destinations tends to be weaker for individuals of more advantaged origins (Guzzo, 2002; cf. also Ishida *et al.*, 1995).

To repeat, what is represented here is a set of fairly well established empirical regularities - not the expression of 'iron laws' of social mobility. Though in fact extensive, these regularities can then be expected to have their temporal and spatial limits. However, in the present context, there are two further features of the findings in question that call for emphasis.

First, these findings cannot be regarded as obvious or 'only to be expected', either in their already demonstrated range of applicability or in their actual substance. Indeed, they have in many respects been found surprising, even implausible - contrary to the claim of Giddens (1987: 19-21; 70-1), as invoked by the 'impossibilists', that since all sociology must be 'parasitic' on lay concepts, its findings are always likely to appear 'banal'. For sociologists, a clear theoretical - i.e. explanatory - challenge is then posed. Second, far from these findings in some way deriving from the lay sociology of the members of the societies to which they relate, they are ones that could not conceivably have been arrived at *other than through* the specific procedures of sociologists studying social mobility. That is to say, what they *are* entirely dependent on are developments in techniques of data collection and in concepts and analysis of the kinds that I have previously noted.<sup>9</sup>

## ***Theory***

As I earlier observed, discussion of the possibility of progress in sociology has centred on theory. While this appears unduly restrictive and, at least in the case of mobility research, would lead to the quite unjustifiable neglect of progress in other respects, it is clear that the question of whether theoretical advance can be achieved in sociology is one of particular importance.

Early mobility research has to be seen as oriented far more to socio-political than to theoretical concerns. After Sorokin's somewhat *ad hoc* efforts at synthesis, it was not in fact until the 1960s that any further attempt was made to bring empirical findings under the aegis of a theoretical position. However, this attempt was then a highly significant one. Social mobility became one of the main topics in relation to which the currently dominant form of sociological theory - macrosociological functionalism - was actually applied. Suggestive but relatively brief passages in the work of leading proponents of such theory, notably Parsons (e.g. 1960), were taken up and developed both by authors concerned with the analysis of industrial and post-industrial societies, such as Kerr and his associates (1960/1973) and Bell (1973, 1980), and also by a number of specialist mobility researchers, most importantly Blau and Duncan (1967) and Treiman (1970).

Basic to the functionalist theory were the supposed 'exigencies' of modern social systems and in particular, so far as social mobility was concerned, the requirement for human potentialities or 'resources' to be exploited as fully as possible wherever within the social structure they might happen to be located. This requirement was seen as prompting, on the one hand, the progressive

expansion and reform of educational institutions in the interests of a greater equality of educational opportunity; and, on the other, the growing importance of educational qualifications as the basis for selection in employment. In turn, as principles of 'achievement' or 'universalism' thus superseded those of 'ascription' or 'particularism', mobility regimes would be transformed: the association between social origins and destinations would steadily weaken. Or, as Bell would have it (1973: 30), modern societies were destined in their functional 'logic' to become, convergently, education-based meritocracies characterised by their 'openness' and rising levels of social fluidity.

This attempt to show functionalist theory actually at work had much to commend it. It clearly brought out the basic form that functionalist explanations take; it provided a reasonable basis for understanding the findings of mobility research up to, say, the end of the 1960s; and, above all, it led to a series of propositions that were eminently open to test in subsequent research. In the 1970s functionalism in general lost its dominance, largely as a result of shifts in ideology and intellectual fashion (thus well illustrating both Cole's and Rule's arguments earlier noted). However, its specific application in the case of social mobility was in fact chiefly undermined by the accumulation of empirical findings that were scarcely consistent with it. As indicated above, both the dependence of educational attainment on social origins and endogenous mobility regimes in themselves have shown a marked resistance to change, and no universal and consistent tendency towards greater social fluidity has been revealed across societies as their industrialisation or modernisation proceeds.<sup>10</sup> Furthermore, even in those national cases where an increase in 'openness' can be demonstrated, there is

often additional evidence - for example, of little if any increase in the role of education in mediating mobility - that points to other causal factors being at work than those that the functionalist theory would propose (see e.g. on France, Vallet, forthcoming; and on Sweden, Jonsson and Erikson, forthcoming).<sup>11</sup>

In recent years, therefore, new theoretical efforts have been made that, in contrast to those of the 1960s, are directed as much towards explaining continuity as change in mobility rates and patterns and in the social processes that underlie them. Moreover, as well as being more relevantly oriented towards the main body of empirical findings, these efforts can be seen as progressive in at least two further respects.

- (i) While functionalist theory sought a 'macro-to-macro' explanation of mobility rates and patterns in terms of the exigencies of social systems, the aim is now at 'micro-to-macro' explanation in terms of the aggregate outcomes, intended or unintended, of the intended, purposive or planful actions of individuals (cf. Coleman, 1990: ch. 1). The application of functionalist theory to the case of social mobility clearly exposed its lack of adequate microfoundations, as reflected in particular in its inability to explain just why changes *should* come about on the lines needed to meet system exigencies.<sup>12</sup> The new approach, starting from individual action and the idea of 'choice under constraint', has so far been chiefly directed towards explanations of persisting differentials in educational attainment, following Boudon's (1974) seminal distinction between

'primary' and 'secondary' processes of educational stratification (e.g. Breen and Goldthorpe, 1997; Breen, 2001; Jonsson and Erikson, 2000; Hilmert and Jacob, 2003).<sup>13</sup> However, extensions of this approach have been made to the explanation of mobility rates and patterns themselves in terms of the 'mobility strategies' of individuals and families (Goldthorpe, 2000); and, also in this connection, attention is now being given to the hitherto neglected role of employers and their strategies of recruitment, promotion etc. (Jackson *et al.*, forthcoming).

- (ii) The theory of action that underlies the new approach is of a fairly explicit kind, namely, rational action theory in one form or another, ranging from standard microeconomic theory to versions embodying less restrictive conceptions of rationality. In this way, any resort to shifting, *ad hoc* assumptions is discouraged and, more substantively, the possibility, at least, is created of arriving at 'more final' explanations than those that can be reached via theories of action that end simply with 'black-box' appeals to differences in (sub)cultural values or social norms - i.e. that do not seek to explain either the content of values and norms or the compliance of individuals with them (cf. Boudon, 1998, 2003). At the same time, explanatory efforts grounded in rational action theory preserve the merit of functionalist explanations in being open to test, and attempts at the empirical assessment of those noted above are now being made (e.g. Schizzerotto, 1997; Davies *et al.*, 2002; Becker,

2003; Breen and Yaish, 2003) with, so far, reasonably encouraging results.

In both these ways, I would then hold, an advance has been made at least in the *form* of the theory that is applied in mobility research; and the promise that follows of better explanations for the main empirical regularities calling for explanation is already in some part being realised.

In the light of arguments of the kind I noted at the start, such a claim of theoretical progress would seem most likely to be challenged in regard to issues of generality. Authors such as Bryant and Flyvbjerg would, presumably, wish to argue that even if sociologists may rely on their own concepts in establishing *explananda*, any explanation that is grounded in a theory of action must involve some representation of actors' concepts - of *their* understandings and interpretations of the situations in which they act - that must in turn expose sociologists' explanations to instability: i.e. make them subject to change as actors' understandings and interpretations change (and, perhaps, in response to sociologists' previous accounts). Consequently, contextual specificity must prevail over generality. Likewise, if more moderately, Cole might maintain that even if *explananda* in the form of empirical regularities extensive in time and space can be determined, these will still have *some* institutional and cultural limits, and so too, therefore, will the generative processes to which explanations of these regularities refer; further, what appear to be the same phenomena may in any case prove to be generated in different ways where different institutional and cultural conditions hold.

My reply to such objections would comprise two, related points. First, it is possible to accept, as I would, that, in principle, general theory in sociology is always likely to be problematic - chiefly for reasons given by Cole - while, however, still insisting that it is what happens *in practice* that really matters. In other words, the crucial questions are those of *how far* empirical regularities can in fact be established that extend across a range of different societal contexts and of *how far* explanations of these regularities, of some consistent theoretical provenance, can be advanced and stand up to test. It is by reference to considerations such as these that my claim of theoretical progress in social mobility research is made. Ultimately, all theory in sociology may have to be 'middle-range'. None the less, and as Cole does indeed appear to recognise (1994: 152), middle-range theory is still theory. Furthermore, middle-range is a matter of degree, and one obvious way in which theories at this level might progress is precisely through their integration and the development of their domains of application.<sup>14</sup>

Second, such a position can be adopted without implying any decisive methodological discontinuity between sociology as a social science and the natural sciences. Those who are most concerned to assert such a discontinuity tend to operate with a restricted and indeed quite old-fashioned idea of the form that theoretical explanation takes in the natural sciences. This they see as explanation that operates by subsuming phenomena under 'covering laws' of a universal and deterministic kind - the success of which is indicated by the possibility of prediction. Since in sociology such (successful) covering laws are rare or non-existent, it is then taken to follow that sociology cannot aim to be 'scientific'.<sup>15</sup> However, it has for some time been recognised

that explanation in the natural sciences does not always conform to the covering law model. Often, and especially in the biological sciences, explanation is concerned, rather, with determining causal processes or mechanisms, operating at 'deeper' levels than that at which the phenomena of interest are observed (cf. Cox, 1992) - on, in fact, essentially similar lines to those noted above in the search for micro-level explanations for regularities in mobility rates and patterns. Although in the natural sciences such causal processes are typically established with much greater theoretical coherence and cogency than, so far at least, in sociology, it is still the case that successful explanation in this mode may not allow for prediction (as, say, in evolutionary biology) or that, because of the mutability of phenomena, may never achieve complete generality (as, say, in ecology). Again, then, the point to be stressed is that the differences that arise between - at least some of - the natural sciences and sociology are ones of degree that in no way warrant qualitative distinctions.<sup>16</sup>

### **WHY HAS SOCIAL MOBILITY RESEARCH MADE PROGRESS ?**

On the basis of the foregoing, I would then wish to maintain that in social mobility research over the last half-century or more recognisable progress *has* been made across a wide front - and that the 'impossibilist' position is thus undermined. More and better data have been assembled; ways of conceptualising and analysing mobility rates, patterns and process have steadily improved; empirical knowledge has increased, including knowledge of a growing number of hitherto unsuspected and wide-ranging regularities; and

the theoretical task of providing explanations of these regularities is now being taken up in more promising, and already to some extent more productive ways than before.

To be sure, there are in social mobility research, as in any other field of enquiry, many unresolved problems, areas of uncertainty if not confusion, and issues of contention. But this, I would argue, applies chiefly - to return to Cole's distinction - to the 'periphery', to the situation on the research frontier, while, contrary to Cole's expectation for sociology at large, a body of generally accepted 'core' knowledge has in fact been established. The strongest support for this claim is to be found in several surveys of the field written over the recent past by individuals whose positions on the periphery are by no means identical (e.g. Kurz and Müller, 1987; Ganzeboom *et al.*, 1991; Treiman and Ganzeboom, 2000; Hout, 2002). Not surprisingly, these surveys reveal frequent, and sometimes quite sharp, differences of view. However, these mostly concern questions that are accepted as being still 'open', and in fact make sense only in the context of a broader consensus that also clearly emerges - and within which, I believe, my own assessment of progress in the field would fall.

As I remarked at the outset, I would not wish to argue that the situation that exists in social mobility research is general throughout sociology. To the contrary, I suspect that for many other fields the case for progress would be far more difficult to make; and, in particular, that Cole's point about the limited transfer of knowledge from periphery to core would prove much harder to

deny. This being so, I conclude with some thoughts on the conditions that may have specially favoured social mobility research.

First of all, a feature of the institutional context of such research should be noted. Although actually carried out in many different kinds of national institution, ranging from university departments to central statistical bureaux, social mobility research has, virtually from its origins, been significantly - and, I would think, uniquely - influenced by international exchanges and collaboration, chiefly under the aegis the Research Committee on Social Stratification and Mobility ('RC 28') of the International Sociological Association. RC 28 was founded in 1951 and has been in continuous existence ever since apart from one short hiatus (1969-71). From the 1970s it has regularly convened on a twice-yearly basis with a membership that comprises a very high proportion of all sociologists who are active in the field.

On the basis of RC 28, a research tradition has been created, now extending over several generations, through which a relatively large collectivity of sociologists has given its attention to a set of fairly well defined problems *in a sustained manner*. Again, there are few, if any, parallel cases. Sociologists have not in general been very good at 'sticking with' problems, largely, it seems, because of their undue susceptibility to both ideology and intellectual fashion which authors such as Cole and Rule rightly see as inimical to cognitive advance. Critics of social mobility research (e.g. Miller, 1998) have contended that its concerns have been too narrowly focused. But these critics overlook the possibility that, especially given sociology's still rather modest resources, human and material, there may well be great advantage in any

field of research in concentrating these resources on the treatment of a limited number of central problems, and then on working out systematically from these (cf. Treiman and Ganzeboom, 2000), rather than adopting a 'spreadshot' approach that could be excessively responsive to transient, non-cognitive influences.

It is, moreover, the international character of the collective effort of RC 28 that has itself helped to protect the possibility of progress against the distractions of ideology and fashion. The range of ideological positions represented within the Committee has always been wide. Most notably, from the early 1970s to the break-up of the Soviet bloc sociologists from this region played a prominent role in its work, and it was therefore a condition of the Committee's viability that members should be ready to distinguish between ideological and scientific issues and to find ways of discussing the latter that were acceptable, and profitable, across the ideological spectrum. Their success in this regard is indicated by the fact of the committee's long-term survival, although some self-deselection did no doubt occur in the case of those who were committed to positions that would not allow the problem of ideology to be thus 'neutralised'.

The international composition of RC 28 also diffused the impact of fashion on mobility research. What is in sociological vogue at any one time tends to vary a good deal across national societies or geographical regions. Thus, in an international context, what might be represented from any one quarter as the *dernier cri* is always likely to meet with scepticism from others, and bandwagon effects are inhibited. RC 28 has in fact remained remarkably free

from the influence of the successive waves of intellectual fashion - from, say, structuralist Marxism, via radical feminism to post-modernism - that have washed over much of sociology. In turn, and more positively, what might be called an 'international style' of sociology has been encouraged that is capable of transcending more local, and passing, enthusiasms.

Finally, in consequence of the above and also of a strong emphasis on methodological issues, social mobility research in the tradition established within RC28 has been characterised by a more serious concern with the actual 'do-ability' of projects than has prevailed in many other areas of sociology. Criticism of the narrowness of the interests of mobility researchers of the kind previously noted has typically gone together with criticism of their preoccupation with (primarily quantitative) methods. It is held that methods too often determine research problems rather than the other way around. However, critics again fail to see the other side of the argument: that it is easy to set out ambitious, far-reaching programmes for sociological research - but at the same time rather pointless unless the means are available for accomplishing them. As Peter Medawar once aptly observed (1958: 2-3): 'If politics is the art of the possible, research is surely the art of the soluble. Both are immensely practical-minded affairs ... The spectacle of a scientist locked in combat with the forces of ignorance is not an inspiring one if, in the outcome, the scientist is routed.' Such a spectacle is, unfortunately, all-too-familiar in sociology; and social mobility researchers' emphasis on methods reflects the fact that, in their tradition, it is performance not programme that matters. If this means that their achievements have indeed been limited rather than over-arching, they have still been achievements. And for those not totally

bewitched by *discontinuiste* versions of the history of science, it is still the case that progress is made in small steps as well, perhaps, as through revolutionary 'paradigm shifts'.<sup>17</sup>

Since I remain disturbed by the possibility that this paper might be regarded simply as a piece of trumpet-blowing, the conclusion to which the foregoing points is then somewhat reassuring. I earlier argued that scientific progress has in general to be understood as falling to the credit of individuals only in the rather special sense of individuals operating as the - in large part substitutable - agents of a collectivity. On the more specific issue of progress in sociology, I would now want to add that if this is more apparent in social mobility research than elsewhere, this is not, of course, because social mobility researchers are more able, or in any other sense more worthy, than sociologists working in other fields but rather because of the way in which, as a collectivity, they have become socially organised.

To this extent, therefore, I would underwrite the Mertonian position (Merton, 1973) that crucial to the understanding of the success of modern science - of its capacity to advance knowledge - is an understanding of the distinctive institutions through which science as a social activity is conducted and of the guiding norms that these institutions express and sustain.<sup>18</sup> However, as a rational action theorist rather than a functionalist, I cannot here avoid a further question. Why should it be that sociologists concerned with social mobility have been more inclined than others to work within the kind of context in which a cumulative growth of knowledge is favoured - why have they tended more often than others to find that the costs of conforming with the associated

normative constraints have been outweighed by the benefits? This is a question for another occasion; but it is one to which an answer, if it could be provided, would, I believe, throw much light on the present state of sociology.

## NOTES

<sup>1</sup> The 'authority' chiefly invoked in support would appear to be Giddens and, specifically, his thesis (1984, 1987) of the 'double hermeneutic'. This holds not only that all social science concepts must be 'parasitic' on lay concepts but further that, in so far as they then serve to refine or correct the latter, they are absorbed back into social life and its everyday interpretation, thus changing the sociologist's object of study. However, this thesis is itself open to serious challenge, not least on empirical grounds (see further below); and Giddens himself seems quite ambivalent on the implications of the thesis, if accepted, for the relationship between the natural and the social sciences. I would further note that although the authors referred to in the text emphasise their ontological concerns, they do also favour a particular epistemological stance: i.e. anti-foundationalist pragmatism, especially as expressed by Rorty (1980), which entails, among other things, a rejection of the 'correspondence theory' of truth. It may be useful background to my paper if I say that I would broadly adhere to the position set out by Searle (1995) that while there is a category of social or institutional facts that differ from 'brute' facts in being dependent on human agreement or acceptance, and that can therefore be understood as *ontologically subjective*, this does not prevent such 'socially constructed' reality from being treated as *epistemologically objective*. Thus, a version of the correspondence theory of truth is still viable 'as a methodological tool for the investigation of social facts' (Searle, 1995: 200). This would seem to me to be close to the position taken up by Max Weber in the face of earlier efforts at the radical separation of *Geisteswissenschaften* and *Naturwissenschaften*.

<sup>2</sup> Voas (2003) has recently pointed out the dispiritingly limited ambition entailed in this view in critical commentary on Jenkins (2002). It implies in effect that sociology can aspire to be little more than an intellectualised version of what, in British secondary schools in the 1950s, used to be called 'civics' - classes in which, in my experience, were chiefly an occasion for surreptitious reading of the latest sports magazine or trying to arrange dates.

<sup>3</sup> Both in including women and recording life-history data, the Glass study was ahead of its time - but not in a highly productive way. On the one hand, the majority of women covered had very limited or discontinuous employment histories and no acceptable way was apparently found of handling this problem conceptually or analytically. On the other hand, data management methods available at the time could not adequately cope with the volume and complexity of life-histories. Not surprisingly, little use was made of either of these features of the data-set.

<sup>4</sup> This is, of course, what would be expected under the correspondence theory of truth. There may be many different ways of viewing the mountain - more or less revealing for the particular purposes one has in mind - but it is still the same mountain, and different perspectives on it should in principle be reconcilable.

<sup>5</sup> Some forceful criticism has in fact been made of this 'two-point' approach to the analysis of mobility (see esp. Sørensen 1986). However, this has so far had less impact than might have been anticipated for, I think, two reasons. First, because some of the more serious problems that could in principle arise with two-point analyses appear in practice to be often not all that damaging (cf. Tåhlin, 1991); and second, because, while some headway has been made - as, for example, via event history analysis - technical difficulties still remain with the treatment of mobility over multiple points, leading some researchers to recommend a return, at least temporarily, to more descriptive methods, such as those of 'optimal matching' (Halpin and Chan, 1998; Abbott and Tsay, 2000). However, it might be predicted that worklife mobility will be a major 'growth area' over the next decade or so.

<sup>6</sup> One outstanding problem is, however, that of developing a method analogous to path analysis - i.e. that allows for the separation of direct and indirect effects - within the context of logistic regression. So far, the most notable contribution in this regard is Winship and Mare (1983).

<sup>7</sup> It has also been a frequent finding that little variation in the pattern of relative rates occurs among different sub-populations *within* national societies. For example, few differences show up as between urban and rural areas or among geographical regions, and gender differences are slight - women showing, if anything, a weaker association between origins and destinations than men. Significant intra-societal variation seems most common among ethno-religious communities but even then is typically quite small.

<sup>8</sup> As regards the effects of political regimes, particular interest has attached to the efforts of (some) east-European Communist states in the decades after the second world war to increase social fluidity and create a new 'workers' and peasants' intelligentsia'. Despite the degree of control that these regimes exerted over both educational and employment systems, the degree of their success was still quite limited (see esp. Szelényi, 1998).

<sup>9</sup> Of particular interest here is the application of the concept of 'class' in the study of societies such as Japan in which, according to 'area specialists', it is quite alien. However, if this is so and if sociological analysis is necessarily dependent on lay concepts, then one might expect 'nonsense' results to be produced where mobility research is based on this concept. But in fact the results of such research show the Japanese endogenous mobility regime both to be highly stable and also to follow essentially the 'western' pattern (see Erikson and Goldthorpe, 1992: ch. 10; Ishida, 1995). More generally, it may be noted that a particularly forceful counter-example to arguments such as those of Giddens is the effective use made of the concepts and theories of present-day economics in the study of the economic history of medieval and ancient societies. As Voas pertinently asks (2003), would one really want to study the economy of ancient Babylon only in terms that would have been familiar to Nebuchadnezzar? And one might also ask how in historical sociology the 'double hermeneutic' is in any case supposed to operate.

<sup>10</sup> An argument in favour of such a tendency was advanced by Ganzeboom *et al.* (1989; and cf. also Treiman and Ganzeboom, 2000) but has been subject to strong criticism, including by Jones (1991), Erikson and Goldthorpe (1992) and Wong (1994).

<sup>11</sup> For example, Breen and Luijckx (forthcoming) present evidence for European nations to indicate that a major factor in instances of increasing fluidity in the late twentieth century is a compositional effect resulting from the decline in the number of individuals originating in farm families or the petty bourgeoisie - who are known to have a relatively high propensity for intergenerational immobility.

<sup>12</sup> In other words, the 'feedback loop' problem in functional explanation, as identified by Stinchcombe (1968) and Elster (1979), was fully apparent: i.e. the problem of showing how, if X has an effect Y that is beneficial for the functioning of the system Z, Y in turn maintains X by some feedback loop passing through Z.

<sup>13</sup> The role played by Boudon in arguing for new micro-theoretical effort in social mobility research has been of general importance, although his arguments were initially directed not so much against functionalist explanations as against a form of 'variable sociology' that supposed that causal explanations could be simply cranked out of statistical analysis without need for theory of any kind. See the celebrated debate between Boudon (1976) and Hauser (1976). Such an overestimation of what can be achieved by statistical analysis - most common, it seems, among American sociologists - remains perhaps the main obstacle in the way of further theoretical advance in mobility research.

<sup>14</sup> Cole seems not to consider this possibility. He takes general theory to be chiefly exemplified by the work of such authors as Parsons, Giddens and Alexander, in which, as he rightly argues, the degree of generality sought at the level of concepts undermines explanatory power - and, I would add, in which conceptualisation appears to become an end in itself, superseding that of explanation.

<sup>15</sup> Thus, Flyvbjerg (2001) recurrently takes the failure of sociology to develop theories that allow prediction as being the leading indicator of its qualitative difference from the natural sciences - even while at one point (p. 39) acknowledging that in some natural sciences prediction 'is relatively rare'. Essentially the same line of argument is found in Jenkins (2002). What appears to be neglected here is that prediction in the natural sciences, insofar as it occurs, typically takes place within 'closed' systems - as may be set up in laboratories - and not in the kind of 'open' system with which sociologists have usually to deal. And if, in regard to the latter context, one thinks of forecasting rather than prediction, it is not clear that sociology is always at a disadvantage as compared with the natural sciences. I would be ready to bet against any meteorologist that I would do better as of now (October) at forecasting traffic conditions in central Oxford next Christmas Day than he or she would at forecasting weather conditions.

<sup>16</sup> Such a mode of explanation is now in fact being increasingly proposed, mainly by European sociologists, as one of general relevance and value for sociology. See e.g. Blossfeld and Prein, eds. (1998), Hedström and Swedberg, eds. (1998), Goldthorpe (2000) and Boudon (2002, 2003). For the importance of sociologists looking to biology rather than physics for their models of science, see Lieberson and Flynn (2002).

<sup>17</sup> It is my - highly unfashionable - scepticism regarding the Kuhnian approach to the history and philosophy of science (and my absolute conviction regarding its deleterious effects on sociology) that lead me to speak of a 'tradition' of social mobility research, which can evolve without serious problems of 'incommensurability', rather than a 'paradigm', subject to total overthrow.

<sup>18</sup> Indeed, the specific norms identified by Merton as forming the institutionalised ethos of science could, I believe, all be shown to operate within the RC28 collectivity - with, of course, a normal amount of individual deviance: i.e. universalism - the rejection of the idea that truth-claims in any way depend on the personal or social attributes of those making them; 'communism' - in the sense that all research results, as a product of collaborative activity, should be available in the public domain; 'disinterestedness' - in the sense of a rejection of fraud, deceit, grandstanding etc for personal advantage; organised scepticism - as a methodological mandate; and 'humility' - in the sense of a recognition of working within, and being indebted to, a tradition rather than seeking to create one's own 'system' *de novo*.

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