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Sociology As Social Science and Cameral Sociology:
Some Further Thoughts

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FURTHER THOUGHTS

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Abstract

Boudon has suggested that one can identify four 'major and permanent' ideal-types of sociology: expressive sociology, critical sociology, cameral sociology and – 'the sociology that really matters' - cognitive or scientific sociology. The relationships between scientific sociology, or sociology as social science (SSS), and the other three types are further examined. It is argued while tension or indeed conflict must inevitably arise as between SSS and expressive and critical sociology, there is little to prevent the relationship with cameral sociology being one of co-operation and complementarity. In the light of this argument, two further issues raised by Boudon are considered: those of how sociological problems should be constituted and of how progress in sociology should be understood - and furthered. It is held that in regard to both these issues SSS will benefit from close ties with cameral sociology.

Introduction

In the inaugural lecture of the European Academy of Sociology series, Raymond Boudon suggested (Boudon, 2002) that one could identify four 'major and permanent' ideal-types of sociology: namely, aesthetic or expressive sociology, committed or critical sociology, descriptive or cameral sociology and then - 'the sociology that really matters' - cognitive or scientific sociology. I find this typology attractive, and my own conception of 'the sociology that really matters' is essentially the same as Boudon's. In this lecture I shall therefore start out from the basis he provides and pursue two main objectives. First, I shall consider how scientific sociology or sociology as social science (SSS) should be seen as relating to the three other types that Boudon distinguishes; and, in this regard, my more specific concern will be to argue that a 'special relationship' should be recognised between SSS and cameral sociology: that is, sociology aimed at informing and guiding public policy.[1] Second, and in the light of this argument, I shall seek to throw further light on two other questions that Boudon himself raised: those of how sociological *problems* are, or should be, constituted and of how *progress* in sociology might be claimed and promoted.

SSS and the three other sociologies

Boudon accepts, as a matter of fact, that sociology is 'a house of many mansions'. However, he avoids an easy pluralism through the clear privilege that he attaches to scientific sociology. I would like to say something more in this connection, and, to begin with, as regards the relations between SSS and expressive and critical sociology, respectively. These relations must, I believe, be ones of tension, if not conflict, on account, ultimately, of quite divergent objectives and methodological positions.

The primary goal of SSS is the explanation of what would otherwise be 'opaque' social phenomena (Boudon, 2002: 373; cf. Goldthorpe, 2000: ch. 1). But expressive sociology is concerned not with the opaque but rather with what is in some way apparent to, and felt by, 'lay members' of societies in the course of their everyday lives. And expressive sociology then seeks to persuade its readers not by the force of its explanatory logic but, rather, by appealing to their personal experience, by giving them at least the sense of understanding this experience more deeply, and by thus evoking a positive emotional response from them. In these respects, literary skill - as well perhaps as an opportunistic ability to catch the *Zeitgeist* - will be of much greater relevance to an author's success than, will say, expertise in techniques of social research or analytical power.[2]

In critical sociology - in the sense, that is, of the Frankfurt School - some attempt at explaining social phenomena may perhaps figure. But this is ultimately subordinate to the attempt to establish, through a process of 'reflection', the objective validity of a particular *normative* position, whether one expressed in moral or political terms. This point is well brought out by the

distinction, often made by proponents of critical sociology, following Habermas (1972), between the 'technical interests' of empirical-analytical social science and the 'emancipatory interests' of a sociology that refuses to separate positive and normative issues but that still claims to constitute a distinctive form of *knowledge*.[3]

From both these quarters, moreover, SSS comes under implicit, and indeed often quite explicit, attack. From the side of expressive sociology, SSS is accused of a 'scientism' that leaves it arid and remote from human concerns, yet still unable to fulfil its high cognitive ambitions (as a recent example, see Flyvbjerg, 2001). From the side of critical sociology, and somewhat contradictorily, SSS is accused of a 'technicism' that readily allows the knowledge it produces to be applied in the service of power - to be used, say, as the basis of mystification, manipulation and control (see, for example, Marcuse, 1964).

In turn, I would then argue, it is essential that proponents of SSS should seek actively to respond to such attacks; and, further, to devote more time and energy to counter-critique - in particular, of a methodological kind and focused on the knowledge-claims that expressive and critical sociology make, which are in fact often of a very weakly grounded kind.[4] In other words, SSS should be upheld as a clear alternative to these rival versions of sociology. Otherwise, adverse effects can be expected to follow (some, I believe, are already visible) both for the image and reputation of sociology in the eyes of our colleagues in other social and human sciences and for sociology's influence in public affairs.[5]

I turn now to the relation between SSS and cameral sociology; and, in this case, I would argue, an entirely different situation obtains. While the ideal-typical distinction between these two sociologies remains valid, there would seem little reason why, in actuality, any serious tension, let alone conflict, need here arise. To the contrary, the historical, and, one might say, the natural relation of SSS and cameral sociology is one of co-operation and mutual support. Two main, though not the only, ways in which this relation is expressed are the following.

First, cameral sociology, in performing its primarily descriptive functions, provides SSS with vast reserves of more or less systematic data as a basis for what Merton (1987) has called 'establishing the phenomena': that is, for establishing that the various, more or less complex social regularities that it is the concern of SSS to explain do in fact exist in the form that is supposed. And in this regard, I would add, we should not neglect the extent to which description itself, and especially when based on relatively sophisticated forms of observation, can promote theory. The history of the natural sciences provides numerous examples. Think only of the revolution in cosmological theory that followed in response to Hubble's observation - itself, it seems, little motivated by theory - that all galaxies appear to be moving away from us, and that the relationship between the speed at which they are moving and their distance from Earth is linear.[6]

Boudon cited the work of Le Play (1855/1877-9) as a notable example of cameral sociology. I would myself place yet greater stress on the work of the now often forgotten figures who established national censuses and systems

of registration data; and, further, on that of the cameralist pioneers of the social survey, especially of the sample survey, such as Kiaer and Bowley (cf. Bulmer, Bales and Sklar eds., 1991) who had, incidentally, to fight a tough battle against those still upholding the Le Playiste 'monographic' method (Goldthorpe, 2000: ch. 4).

The sample survey, as in effect an invention of cameral sociology, provides a classic illustration of the maxim that 'New instruments make new science' (cf. Crump, 2001). Before the sample survey came into being, the main resource for sociologists in their efforts to identify and analyse social regularities were historical and ethnological studies, drawn on, as it were, at second-hand. The sample survey provided an important alternative resource - more restricted, of course, in its possible range of coverage but capable in most respects of providing more detailed and reliable data. SSS is today, I would believe, just as inconceivable without the sample survey and its products as astronomy would be without the telescope or biology without the microscope.

Secondly, SSS has, in turn, the possibility of making a major input to cameral sociology; and, in particular, in so far as the latter extends beyond its basic descriptive function, as it now increasingly does, in order to deal with questions of policy design and evaluation. If the aim is to assess *ex ante* how likely an envisaged policy is to achieve its goals, and with what other unintended, and possibly unwanted, consequences, then theory, implicit if not explicit, is inevitably involved. And it is important, I believe - and not just on public relations grounds - that proponents of SSS should be ready to show,

wherever they see opportunity, that in providing relevant theory they have something of value to offer.

Karl Popper pointed out (1957: Part III) that 'piecemeal' (as opposed to 'Utopian') social engineering can be seen as setting up a kind of experiment, or 'quasi-experiment', that can play a major role in the growth of social scientific knowledge. Given that theory must in some form enter into the design, and perhaps implementation, of a policy, then the empirical evaluation of the policy *ex post* can serve as a revealing test of the theory - or, as Popper puts it (1957: 67), as a means of acquiring knowledge by 'comparing the results expected with the results achieved'. In other words, there are good intellectual, as well as more pragmatic, reasons why scientific sociologists should be prepared to 'get their hands dirty' through some involvement in social engineering.

Let me give an example to illustrate both aspects of the relation between SSS and cameral sociology that I have discussed. In Britain by the 1990s it had become clear from both official statistics and academic research (e.g. Heath and Clifford, 1990; Kerckhoff and Trott, 1993) that the large-scale expansion and reform of education over the post-war years had not brought about any sizable reduction in social class differentials in educational attainment. And further research, drawing on the British series of multi-purpose birth-cohort studies, points to another important conclusion: that an essential stability characterises *secondary* as well as *primary* educational stratification, to use Boudon's (1974) distinction. That is to say, marked differentials persist not only in the demonstrated academic ability of children of different class

backgrounds but, further, in the educational choices that are made by these children *even when ability is held constant.*[7]

These empirical results have then prompted various theoretical efforts aimed at their explanation. Richard Breen and I, building on the earlier work of Boudon (1974), have developed a model of educational choice (Goldthorpe, 1996; Breen and Goldthorpe, 1997; Breen, 2001) which accounts for the apparently lower aspirations of working-class children in terms of what we call 'equal relative risk aversion'. Under our model, working-class children are seen as facing *greater* risks than children of more advantaged backgrounds when they take up more ambitious educational options - the costs of failure are likely to be heavier - and therefore as acting quite rationally in requiring of themselves, on average, a *higher* level of ability - and thus a stronger assurance of success - than do more advantaged children before they embark on such options.

Of late, this problem of persisting class differentials in education has come to the political forefront in Britain in the context of a fierce debate over the financing of higher education. There is some consensus that students should pay more of the costs of their university education, but sharp division over the way in which they should do so and over the likely effects of different methods of payment on the numbers seeking university entry and their class composition. The Breen-Goldthorpe model does in fact lead to some clear expectations in this regard: (i) that any attempt to reduce class differentials in higher education will be undermined to the extent that costs are 'front-loaded' - as, for example, by a system of loans repayable on immediately on

graduation, since working-class caution in educational choice will be increased by a reluctance to incur debt; and (ii) that a preferable approach would therefore be through the 'end-loading' of costs – as, for example, through a graduate tax payable only when and if a certain level of earnings has been attained. The model, and research that has lent empirical support to it (notably Callender and Jackson, 2003)[8], have played a part in the debate; and present government proposals - for a 'hybrid' scheme involving means-tested grants and deferred-repayment loans - do reveal at least a greater awareness than previously of issues of risk aversion in differing class contexts. Moreover, whatever scheme is eventually introduced, close monitoring of its effects, intended and unintended, should provide results of major theoretical interest.

This example brings out, then, the possibility for an interplay between SSS and cameral sociology that is beneficial to both. And underlying this possibility is the fact that these two types of sociology share, if not the same concerns, then at all events a commitment to the same logic of inference or understanding of the relation of evidence and argument. It is this, moreover, that chiefly sets them apart from both expressive and critical sociology.

From this further consideration of Boudon's four types of sociology, I now move on to what I would see as the in part related questions of how we are to identify sociological problems and of how we are to recognise and encourage progress in sociology.

Sociological problems

As regards sociological problems, I have already suggested that these in general arise when a social regularity of some kind is observed and 'established' but is at the same time opaque. Such regularities may be found in rates of individual action, in patterns of social interaction, in sequences of events, in institutional forms and configurations etc. And opacity is present when we cannot readily provide a causal or 'generative' account of a regularity: that is, cannot explain how it comes to be as it is, how it is sustained and how in turn it might be disrupted. SSS is concerned with developing theories that can explain regularities of interest and thus, as Boudon puts it (2002), 'dissolve the puzzling character' of the phenomenon.

There is, however, an important point that needs to be added here and which could, in short, be stated as follows. While all sociological problems will entail puzzlement, the mere fact that you or I, individually, may find some social phenomenon to be puzzling is not in itself *sufficient* grounds for claiming that a serious sociological *problem* exists.

The best development of this point that I know of is that by Merton (1959), starting from a critique of comments made on the nature of sociological problems by the precocious Ralf Dahrendorf (1958). This author, Merton observes (1959: xi), 'presents a disarmingly simple formula: the sociologist has only to select one or another social fact, declare himself puzzled by it, and then ask "Why is it so?"' It is certainly the case, Merton concedes, that 'as Piaget has shown and as any parent can testify, young children delight in asking "Why" about much of what they see'. But, he witheringly adds, 'though

the biblical promise may on occasion be fulfilled, we cannot assume that a little child will ordinarily show the way to adult scientists.'[9]

Merton's argument then is that in problem-finding in sociology, what he calls 'originating questions' have always to be followed up by a consideration of the 'rationale of the question': that is, by a consideration of what will be the implications of a successful resolution of the problem that is posed. 'The rationale', Merton writes (1959: xix), 'states what will [then] happen to other parts of knowledge and practice … In this way, it helps distinguish between the scientifically consequential and the scientifically trivial question. In short, the rationale states "the case for" the question in the court of scientific opinion.'

Merton is fully aware, it should be said, of the part that may played in the growth of knowledge by 'idle curiosity'. But, consistently with his general view of science as a collective and public activity (1973), he still maintains that idle curiosity can serve as its own justification 'only for a time'. If the investigator's questions - and answers - are to be more than a private hobby, then, sooner or later, a convincing rationale for them has to be provided to others in the discipline.

It is then at this juncture that my previous discussion of the relation between SSS and cameral sociology becomes relevant. The rationale for a problem may relate, Merton states, to knowledge *or* practice: that is, to the value of knowledge for its own sake or to values other than that of knowledge itself but to which the application of knowledge can contribute: for example, values such as material welfare, security, comfort and pleasure. And while Merton

recognises that in any particular field the prevailing 'balance' of pure and applied research may be a matter for concern, his main emphasis is on their ultimate complementarity, and indeed on the fact that often in sociology a well-defined problem will prove to have what he calls a 'double rationale' (1959: xxi-ii). Its pursuit will be justifiable in respect of pure and applied considerations alike - as with the problem of educational differentials and educational choice to which I previously referred.

I would strongly underwrite Merton's position here; and I would see the close relation between SSS and cameral sociology, for which I have argued, as helping to ensure that the problems that practitioners of SSS address are in fact 'consequential' rather than 'trivial' ones. In some instances, it will of course be possible to provide a compelling rationale for a problem entirely in terms of its importance for the 'internal' scientific development of sociology. But being able to show that problems are *also* of applied importance is a further indication of their non-triviality. And in this way some prophylactic may be provided against the tendency in SSS that today I find most worrying - and that renders SSS especially vulnerable to attack from exponents of evaluative and critical sociology. That is, a tendency towards what I would call 'sociological dandyism': a preoccupation with models, whether statistical or theoretical, on account more of their intrinsic elegance, refinement and subtlety than of what can be shown to follow from their sociological use that is of major substantive relevance, whether from the standpoint of pure or applied interests.

Moreover, there is another argument to be made in this regard - and one that in fact conveniently links the present issue to that of progress in sociology. There is, I would suggest, a significant, even if not an absolute, difference between the social and the natural sciences in the relation of pure and applied research, and, in turn, a similar difference in the way in which problems come to be formulated. These differences derive from the far greater *mutability* of social than of natural phenomena (cf. Cole, 1994).

Natural scientists can reasonably suppose that the world with which they are concerned is, in its essentials, the same as that which confronted their predecessors; and it is accumulating knowledge of these unchanging essentials that is then applied in many different contexts. As sociologists, however, we are faced, even in our own professional lifetimes, with the emergence of phenomena that appear quite new in themselves. And we cannot safely assume that we can deal adequately with these new phenomena simply by updating or otherwise adapting the theories of an earlier time. Consider, for example, the recent rapid changes that have occurred in most modern societies in patterns of family formation and dissolution. Can we simply accept that theories of the family and of marital relations formed in the earlier or mid-twentieth century will still *mutatis mutandis* be of use today?

It is then often the case that awareness of such new phenomena first comes about through 'cameralist' concerns - simply because governments or their agencies are called upon to respond to the changing circumstances. And in turn sociologists' engagement with these concerns and with socio-political

problems deriving from them may well place them on the theoretical frontier. In other words, we cannot in the social sciences make the equation of 'fundamental' research with pure research as readily as in the natural sciences. Or, to put the same point somewhat differently, in the social sciences far more than in the natural sciences problems that are of a quite fundamental kind may be posed *exogenously*, that is, by the development of the phenomena studied, as well as *endogenously*, that is, by the development of science itself.

Progress in sociology

I come now to the question of progress in sociology. As Boudon notes (2002), the apparent failure of sociology to make progress - to produce cumulative knowledge - has often been seized upon by critics, and there are indeed those within sociology who would accept this failure and take it to indicate that sociology should not even try to be 'scientific' (e.g. Bryant, 1995; Flyvbjerg, 2001).

Now I would clearly join with Boudon in believing that sociology *can* claim to have progressed and that it is capable of progressing further as a social science (Goldthorpe, 2003a). However, what might be asked is whether, in arguing for a close relation between SSS and cameral sociology, I do not make it harder to defend this position. For Boudon observes (2002: 375) that in so far as a 'cameralist orientation' becomes dominant in sociology, it will be more difficult to maintain its cumulative character. Cameral sociology - as I have just myself suggested - is required to switch its focus from topic to topic

according to their ranking on the socio-political agenda, and with little concern for long-term continuity in research or theory.

I would then certainly agree that Boudon here identifies a real danger. And I would in fact add another. When sociologists become involved with cameralist concerns there is always the risk that they are tempted to take up problems that, for the present at least, are simply beyond their cognitive reach. Thus, they fail to produce adequate answers and the reputation of sociology as social science is damaged. Natural scientists, perhaps because knowledge in their fields is generally better structured, are more ready and able than sociologists to tell politicians and administrators at the outset what is, and what is not, currently feasible in research and application.

None the less, despite these dangers, I would still believe that the claim of progress in SSS is one that, on balance, close ties with cameral sociology will help to support rather than undermine.

To begin with, there can be little doubt that sociology has made progress in its methods of enquiry: in particular, in techniques of data collection and data analysis, grounded in the theory of probability. And in these respects, a co-operative relationship between SSS and cameral sociology would seem, on the historical record, to have done clearly more good than harm - and conspicuously so in the case of data collection. There is, moreover, much scope for further co-operation, beneficial to SSS, in such areas as, say, the establishment of national and international data archives, the design of censuses and use of census material, and the creation of closer linkages between census, survey and registration data.[10] Interestingly, technical

progress in sociology has been *least* evident in more qualitative types of research - those, for example, of a historical or ethnographic character - where inputs from cameral sociology have been few.

We should not, I believe, play down the importance of technical advances. None the less, I recognise that when the question of progress in sociology is raised, it is in fact on the extent of *theoretical* progress that debate centres. Has sociology demonstrated an increase in its capacity not just to describe but also to explain social phenomena? And here, I would suggest, the difficulty that comes to the fore is that raised by the relatively high degree of mutability of social phenomena to which I earlier referred. As Cole has put it (1994: 139), sociologists, in attempting to explain the phenomena that concern them, are shooting at a moving target; and, he adds, 'a target that frequently has changed or disappeared by the time the bullet arrives'.

The main way that has been proposed of overcoming this difficulty is to raise sociological theory to a higher level of generality, or abstraction, than that at which mutability is most evident. But, as various critics (Cole included) have pointed out, attempts in this direction have not been impressive. Either the theory elaborated has amounted to little more than a conceptual scheme (as, say, with Parsons); or has been at such a high level of generality as to verge on the tautologous and untestable (as, say, with Homans). However, an alternative approach has of late been formulated by a number authors (see e.g. Hedström and Swedberg, eds, 1998; Blossfeld and Prein, eds., 1998) which seems to me to have far greater potential. That is, to seek to provide explanations for social phenomena not by subsuming them under general,

covering, laws but rather by giving an account of the causal processes or 'mechanisms' by which they are produced.

In this case, the aim is not, so to speak, to 'rise above' mutability but rather to accept it and, by the same token, to accept the ever-present possibility that the theoretical accounts that are given of particular phenomena will be limited in the range of their applicability, and that new phenomena may thus require new accounts. As Elster has argued (1998), revealing an underlying causal mechanism *does* provide an explanation in so far as the mechanism is *more general* than the phenomenon it is seen as producing; but, at the same time, an explanation in terms of a mechanism cannot be *as general* as that which would be provided by a covering law.

I would, however, further agree with Elster when he goes on to say that although in sociology we may resort to mechanism-based explanations *faute de mieux*, we should not be unduly put off by this - we should not allow the best to be the enemy of the good.[11] What I would want to add is the following. In seeking mechanism-based explanations of social phenomena, we should always recognise a *twofold* theoretical task: first, to specify an adequate and a testable mechanism; and, second, to take up the question of its 'domain of application'. If we attend to this second requirement, along with the first, we can hope to make progress through theoretical statements that are at all events of *increasing* generality. That is, by working, as it were, 'bottom-up', and seeking to integrate what Coleman (1964) called 'sometimes-true' theories, rather than by trying to impose covering laws 'top-down'.

In this regard again, I would then suggest, SSS will benefit in so far as its practitioners show a readiness to engage with issues of cameral sociology. Through such an engagement, they will be called upon to confront problems that arise exogenously rather than endogenously, to use my earlier distinction, and will in this way be subject to two constraints or disciplines that I would view as salutary.

First, undue tendencies towards the 'cherry-picking' of problems will be limited - or, at all events, exposed: that is, tendencies towards choosing problems more because they seem likely to display the explanatory power of some favoured generative model rather than because they do have their own independent and compelling rationale. The requirement will be to move, as it were, from *explananda* to *explanantia*, from effects to causes - which, following Popper, I would take to be the way of science - rather than to go in the reverse direction.

Second, theoretical models will more often come to be deployed in 'data-rich' rather than 'data-poor' contexts. This means that they will face relatively stringent empirical tests of their validity - and including perhaps from the 'quasi-experiments' of policy evaluation - rather than being able to survive largely through inadequacies or uncertainties in such relevant evidence as is available. In this way, serious explanatory efforts may be more readily distinguished from hopeful 'just-so stories' - or, perhaps, 'never-true' theories; and what I earlier referred to as sociological dandyism should thus be further discouraged.

Conclusions

My summary remarks can be brief. The four ideal-types identified by Boudon are an apposite way of mapping the terrain of contemporary sociology. But this, we must recognise, is a contested terrain. And those of us who agree with Boudon that scientific sociology, or SSS, is 'the sociology that really matters' must be ready to stand up for what we believe in rather than taking refuge in an easy - and ultimately spurious - pluralism. However, we need to be clear just where the more and the less serious lines of division lie.

The great rift, I have argued, falls between SSS, on the one hand, and expressive and critical sociology, on the other. Fundamental differences here arise over both objectives *and* methodology - over what sociology can and should seek to achieve and over how sociological work is to be conducted. In contrast, while cameral sociology can certainly be differentiated from SSS in its typical concerns, it does not diverge from SSS in any way or to any extent that would entail fundamental conflict. To the contrary, there seems no major obstacle to SSS and cameral sociology operating in a co-operative and complementary fashion.

Viewing the matter from the standpoint of SSS, a close relationship with cameral sociology offers important pragmatic advantages. SSS can gain the opportunity to show its own relevance to public debate and to the formation of public policy, and in turn reinforce its claim on public resources. But such a relationship can be the source of important intellectual advantages also: in leading practitioners of SSS to confront 'consequential' problems that they might otherwise avoid or simply miss; in keeping them *au fait* with the

technical advances, especially in data-collection, that cameral sociology has historically pioneered; and in helping to expose the programme of theoretical progress through mechanism-based explanations to the kinds of empirical challenge that it will need to face, and overcome, as the condition of its success.

NOTES

[1] Boudon derives this useful term from Schumpeter's reference (1954: Part II, ch. 3) to 'cameral science' as that aimed at improving the quality of policy-making in the public sphere.

[2] Boudon gives as examples of expressive sociology the work of Erving Goffman or books such as David Riesman's *The Lonely Crowd* (1960) or, from an earlier period, Gustav Le Bon's *Psychologie des foules* (1895). Good examples from more recent times would be Robert Bellah *et al.*, *Habits of the Heart* (1985) - in which the authors express their hope that 'the reader will test what we say against his or her own experience' as a basis for evaluating the book - or the work of Richard Sennett, such as *The Corrosion of Character* (1998).

[3] Crucial here is the claim that it is by some means possible to go beyond empirical-analytical social science 'to determine when theoretical statements grasp invariant regularities of social action as such and when they express ideologically frozen relations of dependence that can in principle be transformed.' (Habermas, 1972: 310).

[4] Attacks on SSS by proponents of expressive and critical sociology have of course often been alike focused on 'positivism'. However, significant differences show up in what 'positivism' is taken to mean and why it is found objectionable, and one of the few common elements in such attacks is a rejection of quantitative methods in sociology, and, it seems, of any kind systematic, reasoned and transparent procedures for data collection and analysis. Consequently, it is often difficult to see any significant difference between the empirical research on which many works of

expressive and critical sociology are based and that which might be carried out by a decent journalist.

[5] As a possible straw in the wind, see Steuer (2002) for an empirically well-founded polemic by an economist, and also my review (Goldthorpe, 2003b).

[6] I owe this particularly striking instance to my colleague, David Hendry.

[7] The fact of significant secondary stratification was first demonstrated by Micklewright (1987) for the 1970s. Meir Yaish, Michelle Jackson and I are currently investigating the situation in subsequent decades. Our results so far indicate little, if any, change.

[8] See also Breen and Yaish (2003) and for the most extensive test of the model so far, on the basis of Danish data, Davies, Heinesen and Holm (2002).

[9] In these secular times, it is perhaps necessary to note that Merton's text is Isaiah, 11, 6.

[10] It has sometimes been argued, with justification, that the census, survey and registration data that form the basis of much cameral sociology, although extensive, are still essentially 'atomistic' and thus of rather little use to sociologists interested in actual social relationships at the micro- or meso-levels. However, in so far as the importance of such relationships to issues of public policy can be demonstrated by sociologists, public authorities will, I believe, become more open to the possibility of collecting more relevant data - as, say, though surveys with 'multi-level' designs or through the linking of conventional population surveys with local studies of social networks, associational structures etc.

[11] As Elster puts it (1998: 49), 'The "plea for mechanisms" is not an argument against lawlike explanations, only against the idea that when such explanations fail - which they usually do - we must fall back on narrative and description.'

REFERENCES

Bellah, R., Madsen, R., Sullivan, W.M., Swidler, A. and Tipton, S.M. (1985) *Habits of the Heart*. University of California Press, Berkeley.

Blossfeld, H.-P. and Prein, G. (1998) (eds.) *Rational Choice Theory and Large-Scale Data Analysis*. Boulder, Col., Westview Press.

Boudon, R. (1974) *Education, Opportunity and Social Inequality*. New York, Wiley.

Boudon, R. (2002) Sociology that Really Matters. *European Sociological Review* **18,** 371-78.

Breen, R. (2001) A Rational Choice Model of Educational Inequality. Instituto Juan March, Madrid. Working Paper 2001/166.

Breen, R. and Goldthorpe, J.H. (1997) Explaining Educational Differentials: Towards a Formal Rational Action Theory. *Rationality and Society* **9**, 275-305.

Breen, R and Yaish, M. (2003) Testing the Breen-Goldthorpe Model of Educational Decision Making. ISA Research Committee 28, New York, August.

Bryant, C.G.A. (1995) *Practical Sociology*. Cambridge, Polity Press.

Bulmer, M., Bales, K. and Sklar, K.K. (1991) *The Social Survey in Historical Perspective, 1880-1940*. Cambridge, Cambridge University Press.

Callender, C. and Jackson, J. (2003) Attitudes to Debt, Further Analyses: School Leavers' and Further Education Students' Attitudes to Debt and their Impact on Participation in Higher Education. London, South Bank University.

Cole, S. (1994) Why Sociology Doesn't Make Progress Like the Natural Sciences. *Sociological Forum* **9**, 133-54.

Coleman, J.S. (1964) *Introduction to Mathematical Sociology*. New York: Free Press.

Crump, T. (2002) *A Brief History of Science as Seen Through the Development of Scientific Instruments*. London: Robinson.

Dahrendorf, R. (1958) Out of Utopia: Toward a Reorientation of Sociological Analysis. *American Journal of Sociology* **64**, 115-27.

Davies, R., Heinesen, E. and Holm, A. (2002) The Relative Risk Aversion Hypothesis. *Journal of Population Economics* **15**, 683-173.

Elster, J. (1998) A Plea for Mechanisms. In Hedström, P. and Swedberg, R. (eds.) *Social Mechanisms*. Cambridge, Cambridge University Press, pp. 45-73.

Flyvbjerg, B. (2001) *Making Social Science Matter*. Cambridge, Cambridge University Press.

Goldthorpe, J.H. (1996) Class Analysis and the Reorientation of Class Theory: the Case of Persisting Differentials in Educational Attainment. *British Journal of Sociology* **47**, 481-505.

Goldthorpe, J.H. (2000) *On Sociology: Numbers, Narratives and the Integration of Research and Theory*. Oxford, Oxford University Press.

Goldthorpe, J.H. (2003a) Progress in Sociology: the Case of Social Mobility Research. Paper prepared for the Council for Working Life and Social Research symposium in honour of Robert Erikson, Stockholm, November.

Goldthorpe, J.H. (2003b) Review of Steuer, M., *The Scientific Study of Society*. *British Journal of Sociology* **54** 000-00.

Habermas, J. (1972) *Knowledge and Human Interests*. London: Heinemann.

Heath, A.F. and Clifford, P. (1990) Class Inequalities in Education in the Twentieth Century. *Journal of the Royal Statistical Society*, series A, **153**, 1-16.

Hedström, P. and Swedberg, R. (eds.) (1998) *Social Mechanisms*. Cambridge, Cambridge University Press.

Kerckhoff, A. C. and Trott, J.M. (1993) Educational Attainment in a Changing Educational System: the Case of England and Wales. In Shavit, Y. and Blossfeld, H.-P. (eds.) *Persistent Inequality: Changing Educational Attainment in Thirteen Countries*. Boulder, Col., Westview Press, pp. 133-53.

Le Bon, G. (1895) *Psychologie des foules*. Paris: Alcan.

Le Play, F. (1855-1877-9) *Les ouvriers européens*. Paris, Imprimerie Impériale.

Marcuse, H. (1964) *One-Dimensional Man*. London: Routledge.

Merton, R.K. (1959) Notes on Problem-Finding in Sociology. In Merton, R.K., Broom, L. and Cottrell, L.S. (eds.) *Sociology Today: Problems and Prospects*. New York, Harper and Row, pp. ix-xxxiv.

Merton, R.K. (1973) *The Sociology of Science*. Chicago: Chicago University Press.

Merton, R. K. (1987) Three Fragments from a Sociologist's Notebook: Establishing the Phenomena, Specified Ignorance and Strategic Research Materials. *Annual Review of Sociology* **13**, 1-28.

Micklewright, J. (1989) Choice at Sixteen. *Economica* 56, 25-39.

Popper, K. (1957) *The Poverty of Historicism*. London: Routledge.

Riesman, D. (1960) *The Lonely Crowd*. New Haven, Conn., Yale University Press.

Schumpeter, J.A. (1954) *History of Economic Analysis*. London: Routledge.

Sennett, R. (1998) *The Corrosion of Character*. New York, Norton.

Steuer, M. (2002) *The Scientific Study of Society*. *British Journal of Sociology*. Boston, Kluwer.