A REAPPRAISAL OF SOCIAL MOBILITY IN BRITAIN*[1]

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Abstract *Social Mobility in Britain* has been a key work for theories of mobility and social stratification, but its basic data on the occupation of fathers and sons is open to question. Arguing from evidence (mainly from the Census) about occupational transition and differential fertility, this paper suggests that the 1949 study appears to have an implausible number of middle-class fathers. When this critique is related to the peculiarities of the data already separately reported by others such as Ridge, Hope, and Noble, a strong case can be made for the rejection of the Glass findings. It follows that the conventional sociological wisdom that Britain has a low rate of mobility must be reconsidered, and also that those theories of stratification which have drawn too uncritically on *Social Mobility in Britain* must now be re-examined.

FOR OVER twenty years British sociology of stratification has been profoundly influenced by the LSE study published as *Social Mobility in Britain* (Glass, 1954). Central contributors to subsequent writings on class, such as Bottomore, Westergaard, Miliband, and Parkin, draw both directly and indirectly on the evidence of a rigid, relatively closed, and stable society which the book presented.[2] As a result, the Glass study is not just a historical description of mobility patterns in the first half of this century: rather, it has become an integral, if sometimes implicit, part of the continued debate about class structure and formation, and so retains a greater immediacy than some other, more recent, empirical studies.

It is often the case with such seminal works that any technical defects or limitations are quickly overlooked and soon conveniently forgotten. Glass himself is at great pains to point out a number of unusual features of his results, and later writers (mainly in the field of social mobility rather than class theory *per se*) have echoed his observations. However, very little has been done to consider what significance such features might have. For example it is almost a commonplace that the LSE mobility tables are unusually symmetrical, or that class differentials in fertility rates produce a biased distribution of fathers' occupations—both points originally made by Glass. Other peculiarities have been reported by Ridge and Hope during the recent phase of national mobility studies. But as yet, these problems have not collectively received the attention which they merit; the intention of this paper is to remedy this, and thereby to raise doubts about the uses to which the Glass data have been put. The present authors contend that there are major factual inaccuracies and logical improbabilities in the LSE study which call for explanation, and for which no adequate explanation currently exists. These difficulties in turn bring

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into question those theories of class which have drawn too uncritically on *Social Mobility in Britain*.

The starting-point of this reappraisal is the core of all mobility analysis, the mobility table. One of the general characteristics of mobility tables showing respondents' occupations tabulated by their fathers' occupations is that the distribution of occupations for the respondents is different from that of their fathers. The dominant pattern of such tables is that fewer of the fathers have middle-class occupations than do the respondents (or sons), and conversely more fathers appear to be in working-class occupations. This pattern is associated with greater over-all upward occupational mobility than downward mobility, despite varying levels of inherited advantage and self-recruitment, and it occurs because these are tables for twentieth-century industrial societies.

There are two major historically specific reasons why the fathers/sons distributions differ in this respect; changes in occupational structure and differences in fertility. The thesis of occupation transition, that industrial societies manifest a trend towards increasing skill levels, with more professional, technical, and white-collar workers, and relatively fewer unskilled manual workers, implies a steady expansion of the more desirable middle-class occupations. This necessitates a flow of workers into those new occupations which expand the middle-class sector. In other words, the sons of working-class families are less likely to inherit their fathers' jobs because relatively speaking, the working-class sector is contracting while the middle-class sector is expanding, thereby making it easier to enter (Payne, 1977). This process is reinforced by educational policies designed to produce a workforce with the necessary skills for further vocational specialization. The result is structural mobility, which is increasingly seen as the major component of mobility (Hauser, 1975a, 1975b).

In the mainland UK for example, professional, technical, supervisory, and routine white-collar workers increased from about 3,900,000 in 1921 to over 6,900,000 in 1971, and have come to occupy 45 per cent of all jobs compared with 30 per cent, as Table 1 shows.

Blue-collar workers, despite an absolute increase in numbers between 1921 and 1951, have been in relative decline since the First World War, and absolute decline since 1951. Their proportion of the classified work-force has fallen from 70 per cent to under 55 per cent (excluding armed forces).

This pattern is reflected in the mobility tables for Scotland, although it is distorted by other effects (particularly differential fertility) discussed below, and does not show up so dramatically. A comparison of fathers' and sons' generations for the 1975 Scottish Mobility Study sample shows 34·5 per cent and 46·2 per cent middle class respectively, and 65·5 per cent and 53·8 per cent working class. A similar regrouping of the Blau and Duncan data gives the middle class (categories 1–7) 29·5 per cent fathers and 39·6 per cent sons (excluding 'farmers'), with the working class 70·5 per cent fathers and 60·4 per cent sons. (Blau and Duncan, 1967: 496.)

Table 1

*Occupational transition in Scotland, England, and Wales in 1921, 1951, and 1971: economically active males, aged 20–64**

Source: adapted from Census Occupational Tables (for details see Payne, *et al.*, 1975).

		1921	%	1951	%	1971	%
'White-collar' occupations: SEG 1–6; 8; 12–14 (incl. farmers)	Scotland	419,641	28·3	480,729	31·2	560,890	39·1
	England and Wales	3,519,704	30·0	4,706,004	35·1	6,386,250	45·8
	Total	3,939,345	29·8	5,186,733	34·7	6,947,140	45·2
'Blue-collar' occupations: SEG 7; 9–11; 15	Scotland	1,063,192	71·7	1,060,221	68·8	872,040	60·9
	England and Wales	8,225,290	70·0	8,713,168	64·9	7,566,240	54·2
	Total	9,288,482	70·2	9,773,389	65·3	8,438,280	54·8
Totals (excluding armed forces and 17 (not known))	Scotland	1,482,833	100	1,540,950	100	1,432,930	100
	England and Wales	11,744,994	100	13,419,172	100	13,952,490	100
	Total	13,227,827	100	14,960,122	100	15,385,420	100

* Throughout this paper, data is presented for crude categories such as 'White-collar' or 'Manual' workers. This is unsatisfactory, but to achieve comparability over the several studies it is unavoidable.

The second historically specific process which gives rise to differences between the fathers' and sons' distributions as seen in mobility tables is differential fertility. Broadly speaking, working-class fathers each have more children (and therefore of course more sons) than do middle-class fathers. A sample of the sons would find more who said their fathers were working class, because there would be a greater chance of working-class sons being sampled. This would lead to an over-estimate of the proportion of working-class fathers (Glass, 1954: 191).

Consider the following example: suppose there were only two classes, *A* and *B*, of equal size, and the population remained unchanged over two generations; but class *A* fathers had on average one child each (0·5 sons per father), while class *B* fathers each had on average three children (1·5 sons per father). Regardless of the occupations of the sons, the distribution for eight fathers would be:

Fathers	*A*	*A*	*A*	*A*		*B*	*B*	*B*	*B*		
Birth Rate	0·5	0·5	0·5	0·5		1·5	1·5	1·5	1·5		
Sons		*a*		*a*	*b*	*b*	*b*	*b*	*b*	*b*	

Six out of eight sons (the *b*s) would say that their fathers were class *B*, and only two out of eight (the *a*s) would declare class *A*. Therefore our estimate of the fathers' occupational distribution on the basis of the sons' answers would not be

50:50 but 25:75 (for further discussion, see Allan and Bytheway, 1973). As a concrete illustration, the class fertility for women married between 1900 and 1909 was 2·64 for R-G Class I, ranging to 4·17 for R-G Class V. Marriages contracted between 1927 and 1931 (and enumerated twenty to twenty-four years later) ranged from 1·88 to 3·18 for the same classes (Carr-Saunders *et al.* 1958: 25). To some extent these differences in crude averages are 'damped down' by infant mortality, so that 1·88 becomes 1·78 while 3·18 becomes 2·74, (MOH, 1956: 233) and the rate for sons becomes thereby approximately 0·89 (below full self-replacement) and 1·37. Although the manual/non-manual 'differential has been remarkably constant, in relative terms, over the early part of this century' at around 1·9 and 2·7 live born children (Carr-Saunders *et al.*, 1958: 24) the absolute rate has changed, particularly in more recent years. Each generation covers a long period both of changing rates of fertility, and of a changing occupational—and therefore R-G class—structure. It is therefore intended that these fertility rates should serve mainly as an illustration. During the first half of this century the *maximum* effect would be that for every 100 non-manual fathers reported by a sample of sons, the actual distribution in the fathers' generation was nearer 110, while for every 100 manual fathers reported, the real figure would have been nearer 75. Thus in mobility tables the apparent difference between fathers' and sons' occupational distributions under-estimates the number of middle-class fathers and over-estimates the number of working-class fathers, so that the already present effects of occupational transition are exaggerated. The two processes are complementary.

The effects of these processes should be evident in the following distributions of fathers and sons taken from the LSE study: the dotted line indicates the middle-class/working-class boundary.

Table 2

Occuptional distributions of fathers and sons in Social Mobility in Britain
Source: Glass, 1954: 180–3

	Father's occupation as reported by son	Son's occupation as reported by son
1. Professionals	129	103
2. Managerial/Executive	150	159
3. Higher non-manual/Supervisory	345	330
4. Lower non-manual/Supervisory	518	459
5. Routine non-manual/Skilled manual	1,510	1,429
6. Semi-skilled manual	458	593
7. Unskilled manual	387	424
Totals	3,497	3,497

Table 2 is not a mobility table: it says nothing about how pairs of fathers and sons fit into particular classes, because the two columns are discrete. It will be immediately apparent that the two distributions do *not* differ greatly from one another, and that the difference between them in six out of seven classes is not as expected. Classes 1, 3, and 4 of the middle-class sector show *more* fathers than sons, while working classes 5, 6, and 7 all show *fewer* fathers than sons.

But it is known that these figures distort the real distribution because of differential fertility, so that the actual situation is one in which there are even more middle-class occupations in the father's generation, and even fewer working-class occupations. So the full extent of the difference is under-estimated. The Glass mobility table appears to refer to a society at a time when the middle class has contracted and the working class expanded, against the thesis of occupational transition.

However, it can be calculated from census data that for at least the last thirty years of the period covered by Table 2 there was a 16 per cent expansion of the middle class (standardizing for population size[3]) and there is no reason to assume that for the earlier part of the period (going back because of the older fathers to, say, the middle of the nineteenth century) there was no growth whatsoever of the middle classes. Thus it can be concluded that on the one hand there is census evidence of *an expansion of at least 16 per cent*, while on the other the Glass data show *a contraction of around 17 per cent*.[4] Even if the exact magnitude of these percentages is in doubt, it therefore seems reasonable to raise questions about the validity of the sample data.

It should be recognized that Glass himself is aware of these problems and attempts to deal with them, although with only partial success. Indeed, his explanations may have unwittingly contributed to later misinterpretations of his position, because he stresses the stability of the occupational structure, and suggests that differential fertility is a phenomenon the impact of which is minimized by falling mainly in the recent past. Neither of these lines of argument can be accepted without further review of the census figures for the relevant years.

Taking differential fertility first, the central question appears to be at what historical point did it become a significant factor? In view of the little attention that it received from later commentators, Glass's discussion of this appears to have been taken as implying that the class fertility differential is not too important, because of the late appearance of the differential, and the countervailing effect of mortality: as he says,

> Mortality is correlated negatively with social status, but so is fertility, so that the two factors will tend to counteract one another. It is unlikely, however, that they will completely cancel each other. Having regard for the historical development of social status differences in fertility, it is more probable that, relatively, the bias towards the representation of 'manual' fathers will be greater on the more recent than the earlier cohorts. (Glass, 1954:191)

But as Glass has since pointed out, fertility differentials rose from the marriages of the 1870s (when the ratio of manual to non-manual fertility was around 1·25)

to those in the inter-war years (when at its peak the ratio was around 1·40); and adjustment for mortality only very partially counteracts the differential (see Glass, 1969: 44, and Rollet and Jones, 1972). It follows that the differences in fertility effect between the early and late cohorts covers more of the sample than some later commentators have taken Glass to mean. It certainly can be traced back to the 1870s, as various writers have shown (Banks, 1954; Hawthorn, 1970; Thompson, 1970).

Glass also recognizes the occupational basis of the discrepant father/son pattern, but while accepting that it appears unusual, he goes on to argue that it represents a genuine change in the social structure of Britain. He writes that the data

> suggest a slight decline in the opportunities for high status over time, a decline which appears in the data for the subjects' fathers as well as for the subjects themselves . . . the most likely conclusion is that there was no important change between 1911 and 1941 in the proportion of 'non-manual' employment for the [fathers] concerned . . . the increase in the proportions of 'manual' occupations—and therefore of occupations of relatively low rank in the prestige hierarchy—as the more recent decades of birth are approached, is genuine. (Glass, 1954: 190, 192–4)

Thus Glass accepts his data as being valid, even if 'somewhat unexpected' and requiring some justification. He argues that his finding of a contraction of middle-class opportunity is not necessarily

> in conflict with the known fact that certain specific types of white collar occupations have greatly expanded over the past fifty years. It would mean, however, that other occupations of comparable status have contracted to an even greater extent. And also that the expanded opportunities in certain white collar occupations have been taken over by women. (Glass, 1954: 190)

But he does not elaborate on which specific occupations he has in mind, and he does not relate his findings to the thesis of occupational transition—although of course the state of such theories in the 1950s was less advanced than the present day. Instead, he uses the 1951 census data as a comparison—or as Ridge has noted in the context of discussing peculiarities of brother/subject/ father differences, the original investigators 'seem to have been somewhat surprised, and attempted by manipulations of census statistics to show that the same trend can be observed on a status scale other than that of Hall and Jones' (Ridge, 1974:91). Glass and his colleagues present two tables of comparison with the 1951 census, one for the fathers and one for the subjects. Since the argument is essentially the same for both, we shall deal mainly with the latter, because the data on the fathers are a poor estimate of true occupational distribution owing to the differential fertility factor.

Glass claims that the occupational structure has led to an increase in categories 4 to 7 inclusive ('manual') in his sample, from 78·7 per cent for the oldest ten-year cohort, to 89 per cent for the youngest cohort, with a consistent trend for each cohort in between. He compares this with 1951 data from the census showing that the R-G's social classes 3 to 5 inclusive (again, 'manual') have similar scores of 78·4 per cent to 87·7 per cent for the same cohorts. This is shown in Table 3.

Table 3

Proportions of 'manual' respondents in four cohorts as reported in Social Mobility in Britain

Source: adapted from Glass, 1954, Tables 1 and 12: 181 and 194.

Cohort born in	Glass sample Total	Glass sample Percentage 'manual' (Cats. 4–7)	1951 Census Percentage 'manual' (R–G 3–5)
1890–1899	540	78·7	78·4
1900–1909	751	79·7	79·2
1910–1919	772	83·1	81·5
1920–1929	755	89·0	87·7
Over all	3,497	83·0	81·3*

* Not given in Glass, but calculated from *Census* 1951, Occupational Tables, No. 17 and 18, for proportion of civilian population aged 20–59 in 1951 who were in classes 3 to 5 inclusive.

It is Glass's argument that the smaller representation of older men in the 'manual' sector indicates a shift in the underlying occupational structure, towards the manual sector. In other words, the older men entered occupations when there were more 'non-manual' ones available—so fewer are in 'manual' occupations now—whereas the younger men were competing for a contracted supply of 'non-manual' jobs, and so were forced to appear in greater numbers as 'manual' employees.

A re-examination of the Census data, this time including 1921, 1931, and 1951, has been carried out as part of the Scottish Mobility Study, and this can be adapted to show approximately the same categorization scheme (Payne *et al.*, 1975). The general level of the dichotomy is confirmed but its pattern over time for economically active males does not uphold Glass's position.

Table 4

Changes in proportions of 'manual' and 'non-manual' economically active men aged 20–64 in Scotland, England, and Wales, 1921, 1931, and 1951

Source: see Table 1.

	'Manual' (SEG's 6–12, 15)			'Non-manual' (SEG's 1–5, 13, 14)			Totals (excluding armed forces, etc.)		
	England and Wales	Scotland	Combined	England and Wales	Scotland	Combined	England and Wales	Scotland	Combined
1921	86·8	86·2	86·7	13·2	13·8	13·3	11,744,964	1,480,834	13,225,798
1931	88·1	87·7	88·1	11·9	12·3	11·9	13,247,333	1,542,253	14,789,586
1951	84·8	85·1	84·8	15·2	14·9	15·2	13,419,178	1,540,784	14,959,962

These figures, taken direct from the censuses for three different time points, indicate that the 'manual' sector *fell by 2 per cent* between 1921 and 1951, despite

the important fluctuation in 1931 due to the Depression, whereas Glass's figures, based on the cohorts of a single census, give the impression of a monotonic 9·3 per cent increase over all and 8·5 per cent for the three youngest cohorts. The use of three time points and censuses is more reliable than an estimate based on cohorts a single time point. The findings from the three censuses match those of Bain *et al.* who, using a slightly different categorization scheme on the occupational population of Britain, also report that 1921 has a lower proportion of manual occupations than in 1931, but higher by around 2 per cent than in 1951. In addition, 1911 shows an even higher figure than for 1921, albeit by less than 1 per cent (Bain *et al.*, 1972: 113). Bendix, again using a different categorization, finds the same pattern and extends the trend of a declining 'manual' sector back to 1907 (Bendix, 1969: 248). Other writers on evidence of varying reliability have also supported the view that the long-term trend has been for a contraction of the manual sector, not the expansion Glass has claimed (see Payne, 1977).

This interpretation is supported by the distribution of a sample of Scots born between 1910 and 1929. These men are the contemporaries of Glass's two youngest, 1910–19, and 1920–9 cohorts. While perfect correspondence is not to be expected (sampling error, national difference, coding compatibility) the direction of the difference between the two sets of figures, taken for caution's sake at a crude level, suggests that very similar men may be proportionately less 'manual' at a later stage in their careers. 16·9 per cent of the Glass 1910 cohort were in non-manual jobs (categories 1–3, Glass, 1954: 186): interviewed in 1975 the Scottish 1910 cohort reported 23·6 per cent currently in non-manual jobs. Glass's (younger) 1920 cohort were only 11·0 per cent non-manual in 1949: 27·5 per cent of their Scottish contemporaries by 1975 were holding non-manual jobs. If one accepts for a moment that both the Glass and the Scottish data on respondents themselves are reliable, then we are left with a career effect which helps to explain the difference between the reported levels of non-manual employment. It is not unreasonable to suggest that age and hence *intra*-generational 'career' mobility takes sufficient men out of the manual sector through promotions to produce the cohort illusion which Glass takes for the real occupational structure. In other words, each successively younger cohort *is* more manual because the men are younger, and *not* because the occupational structure has changed.

Indeed Glass himself presents conflicting evidence. In a footnote discussing Bowley's work and updating it, he shows a small *increase* in 'non-manual' male occupations which he does not explain beyond reference to 'important elements of non-comparability' (Glass, 1954: 193). And while Glass also claims to find an expansion of the 'manual' sector for three groups each aged 45–54 in the 1911 to 1931 Census (Glass, 1954: 191–2), this evidence is not only bedevilled by comparison problems, but confuses age equivalence with functional career equivalence.

That is to say, entry to an occupational sector (such as foreman or manager) is not always at an identical age for all men at all historical periods, even if entry into

the 'non-manual' sector always on average increases with age for any one cohort over time. This is because of institutional changes, such as educational reforms and the growth of credentialism, and historical events such as wars which affect men of different ages in different ways (for some, career prospects may be improved by the death of rivals or seniors, but in turn that accelerated group constitute a greater block to promotion for the next youngest cohort). The occupational situation of any cohort is the product of three major factors:

(a) its unique historic location, which no other cohort can ever share;
(b) its stage of career cycle, that is to say, its seniority, which other younger cohorts will in turn occupy; and
(c) the changing occupational structure (with its expansion of non-manual opportunities) which applies to all cohorts, albeit more to the youngest cohort in the process of training and recruitment as this group is the most 'flexible'.

The inter-play of these factors is complex, but Glass has interpreted the data on the three 45–54-year-old cohorts, and the data in Table 3, solely in terms of the last of these factors. He has mistaken what may be changes in access to certain jobs at different career stages for changes in the overall structure. By the same token, the evidence for the Scottish labour force quoted above must be regarded with caution because the increase in the non-manual proportion is not only due to seniority, as we implied but is also due in part to changes of occupational structure.[5]

Because of these observations about changes in the occupational structure, age-effects, and differential fertility, Glass's interpretation of the problems apparent in his data cannot be accepted as a solution. The problems persist. Because gross mobility rates are necessarily dependent on the marginal distributions, *Social Mobility in Britain* provides an unreliable estimate of intergenerational occupational mobility, which in turn will require the revision of those of our theories of class relations in Britain which derive from the 1949 study.

If this is accepted, it is still an open question as to what went wrong with the LSE inquiry. The sample design is not obviously faulty, the response rate of 9,296 out of 12,294 (75·9 per cent) is respectable and the age/marital status comparison with the Registrar-General's estimates suggests that the achieved interviews provided an adequate representation of the population. Glass says that while there is some small bias in the age and marriage composition, it is doubtful if this is serious enough to affect the analysis, in part because of the use of cohorts eliminates the over-weighting of some age groups: The other variables—

> fertility and attainment of secondary education—do not appear to have been affected to any considerable extent. . . . In sum, therefore, though the sample is by no means perfect, the bias involved is not likely to be crucial and is to a substantial extent counteracted by the method of analysis. (Glass, 1954: 92)

Indeed, as indicated below, the occupational representativeness of the respondents is not what is in question, since again, though not perfect, the bias is not great and

it does not provide an explanation for the deviant fathers' distribution. By the same token, an over-representation of older age groups and married men does not explain the fathers' pattern. It would be an unfortunate chance if a sample which is unexceptional on five variables should be widely deviant on a sixth which in turn we might expect to be related to at least some of those five.

A second possibility is a class differential in attrition rates: if migration and war casualties selected disproportionately for the sons of working-class fathers, then the residual population would appear to have an excess of middle-class fathers, because only the sons of the middle class would remain to be sampled. If this were a major effect, it would impinge most on the young men who fought the 1914–18 war, and who later participated in the great emigration of the late 1920s; that is to say, men born between 1890 and 1899. This cohort is only 75 per cent of the size of the other complete cohorts (Glass, 1954: 90), and so if there are significant differential attrition rates, this cohort should demonstrate them, assuming that some general process of adjustment has not since intervened.

In the first place, cohort 1890–9 does not show a marked excess of non-manual respondents, a necessary condition if its working-class population has suffered differential attrition. Its proportion is 21·3 per cent, only 0·9 per cent more than the next youngest generation. When allowance is made for some career affect, this does not support the idea of excessive working-class casualties. Secondly, although the cohort does have a higher proportion of non-manual fathers (20·6 per cent in categories 1, 2, and 3, while the others score 16·3, 17·4, and 14·6 per cent in descending order of age: however, the pre-1890 age group also shows an excess at 21·6 per cent) its net contribution of fathers is small because it is a smaller cohort and the excess of non-manual fathers cannot be attributed to any excess of non-manual sons. Thirdly, the marginal totals for the study mean that some downward mobility was inevitable—but within the differential attrition model, there is no immediate explanation of why the working-class sons of middle-class fathers were not equally at risk as working-class sons of working-class fathers—so sharply reducing the chances of the downward mobile of getting into Glass's sample. We are still left with the lack of fit between the fathers' and sons' distribution in a sample which adequately represents the population from which the *respondents* were drawn. Furthermore, another study only two years later reports 26·6 per cent non-manual fathers and 34·3 per cent non-manual sons (Benjamin, 1958: 266).

We would therefore wish to suggest that there is something seriously wrong with Glass's data, most probably in the fathers' occupations. The source of error may be something to do with the respondents' accuracy or veracity; it may lie in the interviewing technique; it may be a coding problem, or a combination of any of these. There is now no way to tell, although the discussion below reviews some further possibilities. But in the absence of some explanation which can *also* restore confidence in the data, it must be strongly urged that conclusions based on the mobility rates reported by Glass be held in abeyance.

This sceptical approach to the Glass data is not the position adopted by others writing since the father/son peculiarity became known. Perhaps it is the very recognition of the fact that the fathers' distribution does not represent a population in the normal sense that has discouraged them giving the matter further consideration. The international comparative sociologists (Bendix and Lipset, 1959; Miller, 1960; Svalastoga, 1965; Fox and Miller, 1966; Cutright, 1968; etc.) have all accepted Glass without question—as one would expect, given their general orientation towards grand comparative exercises and their lack of interest in cultural and historical variations (Payne, 1973). Bibby remarks on the similarity of the two generations, but uses the data as the cornerstone for his discussion of mobility measures (Bibby, 1975: 125). Duncan-Jones notes that 'it is well known that this table has rather a regular pattern' (Duncan-Jones, 1972: 195): again, he uses the data in his exposition (both Bibby and Duncan-Jones employ the Miller version of the Table 2 data (Miller, 1960)). It is slightly ironic that the development of the various coefficients and methods of analysis have used concrete examples drawn from a table that is so untypical of mobility tables generally.

Noble is one of the few commentators who has criticized the LSE Study; however, despite doubts about the sample, he does not argue for the rejection of Glass's findings (Noble, 1972, 1975). He suggests that the 1949 study, 'which is correctly indicating little change in occupational structure, but, simultaneously, over-representing non-manual workers among the respondents, may also seriously over-estimate the non-manual element in the generation their fathers represent' (Noble: 1972, 428). In other words, he attributes the excess of non-manual fathers to an excess of non-manual sons in the sample. However, this involves a number of misinterpretations. Firstly, Noble's statement is predicated on the assumption that Glass correctly indicates little occupational change, whereas the actual change, as indicated above, is very different from the picture Glass presents. Secondly, to what extent does Glass over-represent the non-manual sons? Noble gives four different figures for the non-manual proportion in 1951: 26·4, 38·4, 32, and 37 per cent. The first is for all males, the second is for household heads (both taken from the 1951 Census of Great Britain), the third is from Benjamin's study (Benjamin, 1958), and the fourth is Miller's classes 1 to 3a, taken from Miller's account (which, despite Miller's claim, refers to England and Wales only). Noble uses the difference between the first and last of these figures as an indication that Glass's sample is unsatisfactory.

However, the correct comparison with the 37 per cent is the age-adjusted English and Welsh civilian population figure for R-G's 1951 classes 1, 2, and 3, less the skilled workers, SEG 10: this gives 35·5 per cent as the census estimate, or an error of 1·5 per cent (Census 1951, Occupation Tables, 148–9, Nos. 17 and 18). The earlier discussion of 'non-manual' by Glass gave the sample as 17 per cent 'non-manual', as against the census estimate of 18·1 per cent (Table 2). So that while the sample is not perfect, its small error lies mainly in its shortage of upper middle

class, and its excess of lower-middle-class representation. But the extent of the over-representation is not as great as Noble implies, and the larger part of it appears in the classes which are most open to imprecisions of coding.[6]

Even if there is an over-representation of non-manual sons, along the general lines of Noble's argument, this only relates to an excess of non-manual fathers provided that one accepts that there are high self-recruitment rates. But beliefs about these rates are of course largely based on Glass's work, so that an element of tautology creeps in. If we accept the figure of 1·5 per cent excess of non-manuals, this appears to be equivalent to saying that about fifty-four cases in categories 1–5a should really be manual workers. But none of these could be removed from categories 1 to 3, because these are already in deficit (by about 1·1 per cent in the under-60s' part of the sample) and indeed require an *addition* of, say, around sixty more non-manuals. The net effect is that the upper three non-manual categories (1 to 3) have 594 cases and should have 654, while the lower two non-manual categories 4 and 5a, have 701 and should be 587, for an over-all non-manual total of 1,341. But if we assume that adjustment to non-manual sons changes the number of non-manual fathers by one for every son (i.e. assuming perfect self-recruitment at this dichotomous level), then while categories 4 and 5a would contribute fewer middle-class fathers, categories 1, 2, 3 would contribute more; the fifty-four cases which are moved would 'take with them' their fathers, so that we would still have exactly the same excess of fathers, proportionate to the number of sons. If one assumes, more realistically, a moderate degree of self-recruitment then the excess of non-manual fathers in class 1, 2, and 3 *increases* absolutely although not relatively, while the excess in 4 and 5a becomes relatively greater (since removing one son removes less than one father). Conversely the new replacement manual sons add less than their own number to the manual fathers, which worsens the shortage of manual fathers because the sons' total is increased more than the fathers'. This effect will be limited by the fact that there is less downward than upward mobility in industrial society, so that adding manual sons adds mainly to the manual fathers whereas any adjustment to the non-manual sons has some effect on manual fathers. Again, the impact of adjustment in one category depends on its size and pattern of recruitment. We are therefore unable to accept Noble's explanation of the discrepancies in the fathers' and sons' distributions.

Noble's original comments about errors in the sample develop from his view of age and career effects. He argues that household-heads have a higher social-class rating than 'all males' (26·4 and 38·4 per cent non-manual in 1951), and because men with sons are more likely to be household-heads, than 'all sons', there should be more fathers in the non-manual category than sons (Noble, 1972: 427). It is for this reason that he is less worried by the over-representation of non-manual fathers. However, since Glass is talking about men aged twenty or over, 'all sons' are closer to marriage age and household-headship than 'all males', the latter including males 15–19. So if Noble is correct, 'all sons' should occupy the middle ground between

household-heads and 'all males'. It has already been shown that on the categories 1–5a definition, 35·5 per cent of 'sons' over twenty years old are non-manual; this compares with 36·0 per cent for all males and 37·0 per cent for household-heads (Marsh, 1965: 200). (37·0 per cent is chosen in preference to Noble's 38·4 per cent in order that all three sets of figures are calculated in an identical way). Thus, firstly, the difference between household-heads and males is not as great as Noble suggests (1·5 or at most 2·9 per cent on Noble's figure) and secondly, the 'all males' are *more* 'non-manual' than their older 'all sons', so suggesting that the age/household-head effect is more complicated than it initially appears. Furthermore, an age effect on this scale would show up *if* the occupational structure was nearly stable, which contrary to Noble's view, it was not. Certainly in subsequent studies the age effect disappears, due to structural change and differential fertility—which he does not discuss. So although he is aware of the peculiarities of the 1949 data, his assumption about structural continuity and career effects prevent him from recognizing the importance of the fathers' over-representation.

Similarly, while in a 1972 paper Ridge and Macdonald have commented on the way that the pre-dating fathers' distribution resembles the later sons' distribution (Ridge and Macdonald, 1972: 142), they continue to use the 1949 data in a search for mobility trends (and Ridge has carried out further analysis more recently; see below). As they say, their tables for 1949, 1951 (Benjamin, 1958), 1962 (Runciman, 1966), and 1963 (Butler and Stokes, 1969) show no clear patterns, and they hesitate to draw conclusions about trends from the evidence of those four tables. Having considered the lack of comparability due to different time-points for the fathers' jobs, due to non-response, and the option of 'concocting a tale' to fit the discrepancies and finding in none of these solutions a satisfactory answer, they raise the possibility of eliminating one table. But because the criteria for selecting 'a table to discard are unclear' (Ridge and Macdonald, 1972: 142) they leave the question open. The implication of their elimination idea is that the Glass data is the odd-man-out: without it the sons' non-manual proportions would be 34·3, 35·6, and 37·5 per cent for 1951, 1962, and 1963 respectively. The fathers' non-manual proportions would be 26·6, 25·6, and 29·5 per cent (although this latter comparison is only approximate—see Ridge and Macdonald, 1972: 142 and 146–7). These figures are closer to a consistent pattern and the three 'retained' time-points all clearly show the excess of non-manual sons over non-manual fathers. Thus whereas Ridge and Macdonald hesitate to discard the 1949 table for want of criteria, the present authors would suggest that this paper provides sufficient grounds for the elimination of the LSE data to be made.

In the course of reconsidering the Glass data, both Keith Hope and John Ridge have also discovered certain peculiarities for which they attempt to produce *ad hoc* explanations. In his paper 'Trends in the openness of British Society in the Present Century' Hope examines the evidence of changes in the mobility processes in Britain by comparing the 1949 LSE findings with those of the 1972 Oxford inquiry.

As he remarks, 'Before drawing any conclusions from a comparison between two studies about the respects in which they differ it is necessary to ensure that they do not differ in respects in which they should be the same' (Hope, 1975: 10). To this end he compares the overlapping parts of the two samples, or as he terms it the 'splice'. Setting aside the problem of attrition, the younger 'Glass' men and the older 'Oxford' men are both samples from the same population. By taking the job in the Oxford inquiry which falls nearest in time to the 1949 inquiry (job ten years after entering the labour force) Hope is able to replicate, within limits set by the passage of time, part of the Glass study. Rather surprisingly he discovers that the product moment correlation for the father's last job to son's job in the 1949 study is 0·47, while the comparable figure for the Oxford inquiry is only 0·36. The marginals of the two tables diverge fairly substantially (see Table 5) which may be attributed to a combination of class-differential attrition, failure to replicate the Hall-Jones scale accurately and normal sample variation. In fact these marginals do not explain the different correlations.

Table 5

*Distribution of respondents by Hall-Jones occupational class in the 1949 and 1972 samples, and the 1951 census**

Source: K. Hope, *Trends in the openness of British Society*, Part II, Table 1, and Census 1951 England and Wales Occuption Tables, Tables 17 and 18 (by interpolation).

Hall-Jones classes	1972		1949		Census 1951 Men aged 22–41
1	4·9		2·7		
2	4·3	15·7	3·4	13·7	17
3	6·5		7·6		
4	9·5		11·1		18
5	38·9		48·2		40·8
6	30·8		15·9		16
7	5·2		11·2		8·3

* The census is partitioned in keeping with Glass *et al.*, *Social Mobility in Britain*, p. 191, and D. C. Marsh, *The Changing Social Structure of England and Wales 1871–1961*, pp. 199–201, so as to approximate to the Hall-Jones categories.

Using a technique developed by Goodman and others (Goodman, 1969), Hope is able to show that there are 'significant differences between the two inquiries in the nature of the relations between fathers' and sons' occupational levels, even after differences in the marginal distributions have been taken into account' (Hope, 1975: 13). He also constructs a table which illustrates the extent to which the values of the various cells of the two tables would have to be modified to bring their exchange mobility into line. From this it is clear that Glass has *an excess of self-recruited respondents in all categories* of the scale when compared with the Oxford data. What sort

of explanation can we find for these differences? Hope concentrates his attention on the respects in which the attempt to replicate might have failed. He suggests the following possibilities:

(a) Differential effects of mortality and migration
(b) Failure to replicate the Hall-Jones coding successfully
(c) Unreliable recall of occupations at a considerable remove of time by the Oxford sample
(d) The fact that the job reported by the Oxford men was not actually in 1949 but spread about that time

The last possibility is easiest to dismiss, because Hope is able to demonstrate conclusively that the explanation does not rest there. None of the remaining explanations seem plausible, because although they might explain differences, they do not explain why *self-recruitment* should be higher in the 1949 inquiry. So that on point (a) it is a little difficult to imagine what sort of social process could lead to a disproportionate survival of self-recruiters at all socio-economic levels, although the association between self-recruitment, traditional occupations (and proprietorship), and propensity to migrate might repay further attention. Again, even if one were to assume that the Oxford team were singularly inept in attempting to reproduce the Hall-Jones scale there is no immediately apparent reason why this should exaggerate the extent of social mobility (the self-recruitment effect extends throughout the scale and cannot therefore be due to the simple mis-location of some particularly crucial group). In fact John Ridge provides evidence of an infelicity in the original Glass coding which ought to work in the opposite direction and dilute the intergenerational association for the Glass study relative to the later Oxford work. (Ridge, 1974: 92.) Similarly, until Hope's study of the reliability of survey data becomes available there seems to be no way of settling conclusively the effect of recall problems on occupational reporting.[7]

In the meantime 'the two studies remain stubbornly divergent however one tries to bring them into line' (Hope, 1975:49). As attempts to explore possible sources of inaccuracy in the Oxford retrospective data have failed to cast light on the problem, Hope concludes that 'it seems reasonable to place greater reliance on the results of the later inquiry rather on those of the earlier study' (Hope, 1975: 38). The clear implication of this is that in some as yet unidentified way Glass overestimated the degree of occupational inheritance, in addition to his over-estimate of non-manual fathers as established above.

The point of departure for John Ridge is somewhat different. He is examining the total patterns of inheritance of status within the family in the Glass sample by considering those cases where the respondent had two or more brothers. Given that the respondent is in each case a randomly selected 'brother' himself, one would intuitively expect that the correlations between the respondent and each brother (arranged by birth order) would be roughly equal, and would be in line with the

correlations among the other brothers. The correlation between respondent and his father, and the brothers with their fathers should also be roughly equal. As displayed in Table 6, the results observed depart from this expectation.

Table 6

Married men in England and Wales with two or more married brothers for whom occupations were reported in the Social Mobility in Britain *survey*

Source: Mobility in Britain Reconsidered (J. Ridge, 1976: 96).

N = 583:	Mean No. of brothers = 3·30			
Means	Brother 1	Brother 2	Respondent	
Year of birth	1896·0	1900·3	1901·2	(1)
Hall-Jones code	4·85	4·87	·77	(2)
Children born alive	2·32	2·00	2·10	(3)
Correlations				
Hall-Jones:				
Brother 2	0·443			(4)
Respondent	0·279	0·265		(5)
Father	0·324	0·333	0·439	(6)
Year of birth by number of children	−0·282	−0·264	−0·346	(7)

The correlations between respondent and his brothers are substantially lower than those amongst the brothers. The correlation between respondent and father is considerably higher than that between brothers and father. At the same time, the average status of fathers is 4·73, similar to the respondent's and higher than the brother's. Why should the respondent show more occupational inheritance than his brothers and be more successful at maintaining his status? The answer of course, is that he should not; and there are two possibilities to consider. The first is that the 1949 sample was peculiar in a number of respects; that sampling fluctuations explain the patterns observed. This however seems unlikely as an explanation as it involves arguing that a sample of respondents which has above been taken as a reasonable one in terms of age and occupational spread, appears to contain a high over-representation of middle-class fathers, too high a level of self-recruitment, and a conspiracy of downwardly mobile brothers. This would indeed be an odd and unfortunate sample.[8]

The second and more reasonable position to adopt is to assume that the Glass data was flawed either in the collecting or coding, and hence to seek evidence about the nature of this postulated flaw. Following Ridge's argument, it might be suggested that the brothers are less salient to the respondent than his father and that the information is therefore inaccurate. The trouble with this explanation is that it would also give rise to low 'inter-brother' correlations. Ridge also reports that the variances for brother's status scores are consistently lower than those of respondent.

Neither, it seems, is the result due to the fact that a substantial number of the brothers were in fact dead, and therefore arrested at an earlier career point than the respondents. Indirect evidence on the size of the brothers' families suggests that they are mainly alive and breeding. A second finding which would in any case preempt this explanation is the initially startling fact that there is *nil* correlation between status and age in the Glass sample of respondents (presumably the Hall-Jones scale is too gross, and career development is contained *within* its categories, but see note 5 below). In the area of coding error, an interesting suggestion is made: The brothers' jobs were recorded with less detail. Perhaps for those jobs which may or may not involve skill (i.e. manual jobs for the most part) the less-detailed replies about brothers failed to indicate the level of skill, so that they were consequently down-graded. Unfortunately, this would apply equally to the coding of fathers and is therefore not a satisfactory explanation.

Both Hope and Ridge have been unsuccessful in locating the source of their troubles in either some general area of inaccuracy such as passage of time, fallible memory, sample attenuation (mortality, etc.), 'spread' in the time-points used, or in a data-processing error. Ridge has one further hypothesis to offer, dangerously subversive to the canons of survey research. Perhaps Glass accurately processed and reported the information he received. Perhaps the unlikely pattern of findings occur because the respondents systematically biased their answers. Specifically he suggests that some of the respondents may have deliberately down-graded their brothers as a result of 'sibling rivalry'. The problem here is that once we admit the possibility (indeed, even likelihood on the available evidence) that this group of respondents were so status conscious as to lie, then we are forced to admit the existence of many groups with various potential axes to grind, and the task of survey analysis becomes doubly difficult.

For example, suppose that upwardly mobile men tend to obscure their origins by 'inviting' their fathers to join in their success; what effect would this have? It is fairly easy to show that this would at the same time strengthen the degree of the father/son association *at all levels in the scale* while increasing the proportion of fathers in jobs in the higher classes, and since it might be argued that it would strengthen the father/son correlation while attenuating relationship of the father to his other sons, it might appear that we had gone a long way towards providing a uniform explanation for all the puzzling aspects of the Glass results. We must be cautious however in presenting this speculation as an explanation since there are several features of the data which it does not immediately explain. Firstly we would expect to find that the Oxford 'correct' data would contain relatively fewer non-manual fathers than the Glass data. In fact a cursory inspection of the overlapping parts of the sample suggests that there is a slightly *higher* representation of non-manual fathers in those cohorts of the 1972 study.[9] This however, is not decisive. In both cases we are contrasting respondents at a fairly early stage in their careers with their fathers at a mature stage of career development, and if we are prepared

to assume a fairly strong career affect (together with some occupational transition effect on the fathers' 'last or current job' in the later Oxford study), then these figures may well be correct. Of course we would then be committed to argue that the distortion in the Glass data applied primarily to his older cohorts but this seems to make good sense as we are supposing that it is upwardly mobile men who introduce the error, and they might require time to achieve their own mobility. Being older, they might also be more likely to have an 'old-fashioned' concern with status. A second problem arises concerning the family pattern; we cannot by the above reasoning explain why the correlation between the status of the respondent and his brothers is substantially lower than that between the brothers. Here it seems we would require to introduce additional hypotheses such as Ridge's suggestion of sibling rivalry.

The central point to be made is this; there are a set of factual inaccuracies and logical improbabilities in the original 1949 LSE study which cry out for explanation. As yet, no reasonable parsimonious explanation exists. There are grounds for doubting whether a satisfactory explanation can be based on a simple artefact of the data collection, coding, or analysis; both because of earlier failure to reveal any such effects and because it is hard to envisage what sort of distortion could act to select individuals on the basis of their *mobility experience* in a fairly uniform way throughout the sample. If this is accepted, then it seems that we must either accept a multiple explanation for multiple errors or move into the dangerous terrain mapped by John Ridge. It is clear that we could probably construct an explanation based on systematic distortion on the part of the respondents along the lines indicated above; the problem here is that it stands or falls on the imputation of complex motive and behaviour patterns which however superficially plausible, are unamenable to evidence. Furthermore, even if we are happy to take this course, we are left with one major question: exactly what was it about the Glass study that led to the respondents' falsifications, where other studies seem to show less evidence of such peculiarities. The present authors do not have an answer, and indeed, the intention of this paper is not to explain *why* Glass arrived at his results, but only to show that those results are suspect.

Even if the data *were* accepted uncritically, three features of the Glass sample would still produce a result untypical of other studies. Firstly, Glass's respondents were aged 18 and over, so that although he discards the under-20s, his over-60s number 679 cases, or 19·4 per cent of his sample. This compares with under 12 per cent for studies of men aged 20–64, the model established by Blau and Duncan for subsequent studies. Not only does the Glass sample contain more older men, it compares their current or last occupations with their fathers' current or last occupations, unlike the more recent preference for the father's job when the respondent was 14. These two factors are not self-cancelling, and are compounded by a third, the early date of the LSE work in 1949: historically the Glass data is referring to a very much earlier period, compared with the American or latest British Studies.

Towards the upper limits, such as a respondent aged 70 in 1949, Glass was dealing with someone whose birth year was 1879, and whose father's birth year was 1845 (taking Glass's estimate of father's mid-child birth at age 34) (Glass, 1954: 191). Such a father would reach the equivalent of retirement age in 1910, still in the era of horse-drawn transport and well before the start of the First World War! His son could easily have worked for over twenty years before the First War, and would be reaching retirement age during World War Two. Of course, at the other end of the age scale the 20-year-old born to a 34-year-old father had worked only since the end of that war, but his father's work-experience would nevertheless start before the previous war—1909: with successively older respondents, the father's work experience (particularly his *entry* into the labour force) becomes increasingly characterized by the inter-war years, and the nineteenth century.

Glass's approach to social mobility is explicitly concerned with both the ability of the middle-class father to pass on his advantaged position, and also the chances of the able working-class child to escape his background and enter a higher-status job (Kreckel, 1972). Thus the implicit causal focus of his study is the father's occupational position (or alternatively, that position's influence on the social context in which the son is born). Since so much of the father's work experience ranges through the second half of the nineteenth, and early twentieth centuries, the mobility Glass reports is strongly bound up in occupational and social processes that had little direct relevance in 1949—let alone 1969. Over two-thirds of Glass's sample have their mobility defined by the occupations of fathers who reached retirement age before the end of the Second War.

Now normal practice would make sociologists hesitant in attributing to the social conditions of the 1960s and 1970s the results of surveys carried out only four years after the end of the last war. But all mobility studies are by nature retrospective and historical, and the LSE study is now more historical than most. It follows that there can be very little justification in uncritically regarding the present mobility processes of Britain (or 'advanced industrial society') as being those reported by Glass. Unfortunately, this is precisely what is still happening, as in for example the work of Raynor (1969: 33–5), Frankel (1969: 161), Goldthorpe (1974: 145–6), Westergaard (1975: 297–302), etc. As Hope has remarked, interpretations of Glass's findings have

> contributed materially to the theoretical debate on British stratification. Indeed, theories of British occupational mobility, so far from being derived from some broad body of speculative sociology, have tended to ground themselves in an agreed reading of the findings of the 1949 inquiry, differing from one another only in the supposed mechanisms and processes which they postulate to explain those findings. (Hope, 1974: 1–2)

While there may have been no alternative study available, continued use of Glass's *Social Mobility in Britain* in anything except a purely *historical* context is indefensible. Indeed, the relevance of the study is now for the history of sociological

ideas, not for the 'sociology of modern Britain', or for stratification analysis, and all the more so in the light of the erroneous numbers of fathers and sons, and the major historical changes in social and occupational structure since 1951.[10]

Notes

1. This paper is based on Working Paper No. 5, Scottish Mobility Study, University of Aberdeen. The Study is financed by SSRC Grant No. HR/2173/1. The authors are grateful for the helpful comments of colleagues, most notably David Glass, Trevor Noble, Robert Moore, and Alan Anderson.
2. For a discussion of this dependence of theory on Glass, see the authors' 'Changes in Occupational Mobility in Scotland', *Scottish Journal of Sociology*, Vol. 1, No. 1, 1976.
3. As Table 1 shows, the relevant population expanded from 13,227,827 in 1921 to 14,960,122 in 1951. Had the middle classes made up 34·7 per cent (the proportion in 1951) of the smaller figure, this would have been 4,590,056, an expansion of 650,711 on 3,939,345 (the number in 1921) or 16·5 per cent.
4. 1,142 sons said their fathers were middle class (Table 2): this figure is weighted by a class fertility factor of 0·9, giving a total of 1,269 middle-class niches in the actual occupational structure of the previous generation. These niches have contracted to 1,051 in the son's generation, as we know directly from the random sample of sons. Hence the contraction of 218, or 17·2 per cent as the upper estimate.
5. Although cohorts generally display little differences between their average status levels, this does not necessarily mean there is no career effect. It may be that older cohorts have benefited from career mobility, but that younger cohorts have benefited disproportionately more from the expansion of the non-manual sector—i.e. structural mobility. This kind of pattern therefore appears as the dominant one in current mobility studies which reflect the expansion of industrial society throughout this century, but see Hauser (1975b: 588–90) for a difference of emphasis.
6. The concentration of over-representation in categories 4 and 5a is important, because they have particular mobility characteristics which ramify throughout the mobility matrix. In general, lower-grade supervisory staff (e.g. foremen) and routine grades of non-manual work (e.g. clerks and shop assistants) are among those categories more likely to be recruited from a wide range of backgrounds (Goldthorpe, 1975: 9–10). In the Scottish Mobility Study, the coding of supervisors and inspectors proved technically difficult and despite detailed checks and recodings, the eventual sample showed about 8 per cent foremen, compared with the 1971 census figure of 5 per cent (although these figures are not for strictly compatible populations and the census figure *should* be marginally lower). Presumably this is a self-inflation effect, and it may be that part of the problem with the Glass data arises from a similar phenomenon.

   Among other very helpful comments on an earlier draft of this paper, Trevor Noble has pointed out the difficulty of allocating SEG 8 (service workers) who should really be split between Hall-Jones categories Va and VI. If SEG 8 is reclassified as manual as Noble has done, then the Glass/Census comparison becomes 37/31·3 per cent, which is more in line with his criticism of the sample.
7. Speculating widely, the very limited occupational 'repertoire' on which most people draw might tend to result in an over-reporting of occupational inheritance where actual clear knowledge was missing.
8. Keith Hope is also able to show that the differences between the mobility rates are consistent throughout the cohorts of both samples, which leads him to argue that they are

different but 'functionally equivalent', and that the difference must therefore lie in some artefact of the survey methods employed rather than in sample differences (which would be unlikely to apply systematically throughout the four cohorts).

9. If we take the comparable parts of the two samples as laid out by Hope (Hope, 1975, Part 2, Table 1) and control for sample differences by reducing the respondents' distribution for the 1972 sample to that of the 1949 study, we find that the 1949 fathers are 16·2 per cent non-manual while the 1972 fathers are 20 per cent non-manual (defined as Hall-Jones Class 1, 2, and 3).
10. It should be emphasized that these criticisms do not necessarily apply to *all* aspects of *Social Mobility in Britain*. The present authors are only concerned with the mobility findings *per se*, and since the sample of respondents appears to be sound, it may well be that other findings (such as Jean Floud's ch. 5 on education) are perfectly valid. We are grateful to one of the editorial readers of *Sociology* for pointing out the need for this clarification.

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