Social class mortality differentials: artefact, selection or life circumstances?

A J FOX¹, P O GOLDBLATT², AND D R JONES¹

From the Social Statistics Research Unit,¹ The City University, Northampton Square, London EC1V 0HB and Office of Population Censuses and Surveys², St. Catherine's House, Kingsway, London WC2B 6JP

SUMMARY Data from 10 years follow up of mortality in the OPCS Longitudinal Study are used to relate deaths of men in 1976–81 to their social class as recorded by the 1971 census. Explanations of social class mortality differentials are critically reviewed in the light of these new data. The similarity between the class differentials observed for men aged 15–64 years in this study and those reported in the 1970–2 Decennial Supplement on Occupational Mortality indicate that the published gradients were not in fact grossly distorted by numerator denominator biases. Distortions to gradients observed in the early years of the longitudinal study and ascribed to selective health related mobility out of employment from the principal social classes to the permanently sick had largely worn off after five years of follow up. Sharp gradients at ages over 75 years, similar to those at younger ages, suggest that, for men aged over 50 years, selective health related mobility between social classes does not contribute to differentials in mortality.

Background Analyses of mortality differentials in Decennial Supplements on Occupational Mortality (see, for example, the latest report¹) have traditionally provided the main source of data in the discussion of health inequalities by social class. For example, the recent review of inequalities conducted by a Department of Health and Social Security Research Working Group under the chairmanship of Sir Douglas Black² derived much of its evidence from this source. Deficiencies in the data for assessing the exact magnitude of differentials and the changing patterns over time, and for identifying causal factors, were evident from their analysis. The method of deriving occupational mortality statistics suffers from major numerator denominator biases because the occupation reported at the registration of a death is not necessarily that reported at census. At the same time this method produces only cross sectional data which cannot be used to shed light on the influences of social mobility. These defects in traditional data, recognised in the earliest analyses over 100 years ago³, have meant that even though they were unable to indicate the importance of such explanations, decennial supplements have regularly warned about the influences of artefacts,⁴ namely, classification changes and numerator denominator biases, and selection effects,⁵ namely, selective movement between classes of the sick and the healthy. While the Black Report gave little credence to these explanations,⁶ critics have argued, mainly on theoretical grounds,⁷ that their role in perpetuating differentials is underestimated.

When, in 1973, the Office of Population Censuses and Surveys (OPCS) announced its intention to conduct a longitudinal study of a 1% sample of the population of England and Wales, one of the stated purposes was to construct a data source free of numerator denominator biases (see Cohort Studies; New Developments⁸ p 6). This sample, of people born on any of four birthdays during the year, was initially selected from the 1971 census. So far, for each person in the sample, vital events between 1971 and 1981 censuses have been incorporated into the data set.

Data on mortality in the first five years of this study have been compared with those obtained by the traditional cross sectional approach in an earlier report.⁹ This analysis highlighted the distortions of social class gradients to be found at the inception of prospective studies. A sizeable fraction of men aged 55–64 years were classified by the census as having an "inadequately described occupation" or as being "unoccupied". The 1970 Classification of Occupations allocated people to social classes on the basis of their occupation and their status within an
occupation. Those people who were "inadequately described" or who were "unoccupied" were not allocated to a social class. Since these groups included men who were permanently sick, their mortality was particularly high. Their exclusion from social classes resulted in a less marked mortality gradient from class I to class V in the longitudinal study than in the decennial supplement.

As was described in the main report of mortality in 1971–5, the prospective approach permits analysis of the change in mortality by duration of follow up. When discussing the impact of health related mobility, it was pointed out that the effect of the exclusion of the permanently sick from social classes I to V would be expected to wear off with increased duration of follow up and that, as a consequence, the differential from classes I and V would widen in later years of the study.

Failure to classify the permanently sick (and others) to one of social classes I to V is an example of health related selection, in this case primarily representing selection out of the labour force into unemployment or health related retirement. While this is clearly the most dramatic form of selection, it is often suggested that mobility between social classes, in particular, drift down the social scale with increased disability and ill health or, perhaps, positive health enabling people to move up the social scale could, in principle, be a major factor in explaining the wide social class gradients observed in mortality. Such arguments suggest that the gradient by social class, say, ten years before death, or even at birth, would provide a more appropriate measure of socioeconomic differentials which would consequently be narrower than that by social class at death.

Most analyses of inequalities in health have concentrated on differentials between the Registrar General's social classes simply because these data, however crude and inadequate, constitute the most readily available national series. As earlier analyses of the longitudinal study have demonstrated, several dimensions to class differentials in mortality can now be explored. In particular, aspects of education, housing, and household possessions can now be studied, each of which is, to some degree, related to wealth and income. The concentration on occupationally based social class in this paper reflects the opportunity afforded to us by deaths occurring in the period 1976–81 to evaluate critically previous findings and hypotheses from this study concerning social class differentials. It does not indicate that we do not recognise that these other dimensions are equally worthy of further investigation; indeed, it is our intention to return to these issues in later projects. While this paper concentrates on differentials in male mortality, we are also looking at female mortality and will report this work later.

**Methods**

Social class in both periods is that defined on the basis of the occupation and employment status as reported in the 1971 census and assigned using the 1970 Classification of Occupations. Expected deaths are calculated in the standard method of prospective studies; person-years-at-risk by age for men in each social class are multiplied by age specific death rates for all men in the longitudinal study. The latter were calculated by dividing all deaths (in each five year age group) during the periods 1971–5 and 1976–81 respectively by the corresponding person-years-at-risk. A fuller account of this method is given in the report of mortality in the period 1971–5. When annual figures are shown in this article, expected deaths are calculated using corresponding annual age specific death rates for the entire longitudinal study sample.

Further editing of the original data and the addition of late death notifications means that the data for 1971–5 presented here are slightly different from those previously published.

Trends in the annual standardised mortality ratios (SMRs) for each social class with time between 1971 and 1981 have been investigated fitting regression models equivalent to the assumption that the observed deaths \( O_{ij} \) in the \( i \)th social class (\( i = 1, 2 \), corresponding to classes I, II, IIIN, IIIM, IV, V) inadequately described and unoccupied) in that part of the \( j \)th calendar year in the risk period (\( j = 1, \ldots, 11 \)) are independent Poisson variables whose means satisfy

\[
\log E(0_{ij}) = \log (E_{ij}) + \text{constant} + \alpha_i + \beta_j x
\]

where \( E_{ij} \) is the number of deaths expected in the \( i-j \)th category (see above) and \( x \) is the time in years from the 1971 census to the mid-point of the \( j \)th time interval. The model was fitted using the GLIM package and hence \( \alpha_1 \) is set to zero. This model ignores autocorrelation between observations in successive time intervals, and cautious interpretation of the parameter estimates so derived is thus necessary. It should also be noted that the model does not assume a natural ordering of the social class categories.

Trends with age groups were investigated in a similar model using data for the six principal social class categories (\( i = 1, \ldots, 6 \): social classes I, II, IIIN, IIIM, IV, V) and nine age groups: 45–49 (\( j = 1 \)), 50–54 (\( j = 2 \)), \ldots 80–84 (\( j = 8 \)), 85 and over (\( j = 9 \)). The variable \( x \) takes the value of the mid point of each age range (\( j = 1, \ldots, 8 \)) and a value of 90–0 in the final age group.
Change in mortality with duration of follow up

Overall mortality of men aged 15–64 years at death by social class is summarised in Table 1 and fig 1 for the years 1971–5 and 1976–81 separately. Several features are of interest in view of the comments made above about the contrasting findings in earlier analyses of this source and the decennial supplement analysis. The most striking change in SMR is that observed for the unoccupied, which fell from 299 in 1971–5 to 213 in 1976–81. The effect of this fall in SMR on mortality differentials for those men who were allocated to a social class is coupled with an effect of the ageing of “unoccupied” men beyond age 64 years to reduce substantially the fraction of deaths contributed by this group. In 1971–5 more than 10% of deaths at ages 15–64 years were to men who were “unoccupied” at the 1971 census. This fraction was reduced to little over 5% by 1976–81.

This attenuation of a selection effect implies that differences in mortality between social classes in the study between 1976 and 1981 are less affected by the problem of the unoccupied than they were shortly after the 1971 census. As was predicted from analysis of the time trend in 1971 to 1975 (Fox and Goldblatt pages 187 and 203), the social class gradient becomes much clearer in the extended period of follow up. The difference between class I and class V widens markedly, and progressive differentials between classes start to appear more strongly.

Figure 2 shows the annual changes in SMR by social class in order to provide a guide as to whether or not the gradient is continuing to widen. Although the figure gives the impression that classes I and V are continuing to diverge over the period 1976–81, the annual SMRs are based on small numbers of deaths, particularly that for 1981 which covers only the period January to 5 April. Table 2 gives parameter estimates and standard errors fitting the model specified above to these figures. The specified model fits the data moderately well, as a deviance of 66.0 on 72 degrees of freedom indicates. The null model \( \alpha_i = \beta_i = 0, i = 1, \ldots, 8 \) yields a deviance of 745.4 on 87 degrees of freedom. The slope estimates, \( \beta_i \), in Table 2 confirm the impression given by fig 2 that the SMRs for social class I are declining with duration of follow up and those for classes II, III, IV, and V show tendencies of various magnitudes to rise with duration of follow up. The model also confirms the marked decline in SMR for the unoccupied, noted earlier.

Mortality in 1976–81 by age

While the longitudinal study has advantages over the decennial supplement analysis, for example, in terms of numerator denominator biases, it suffers severely because many fewer deaths are included in the analysis. Table 1 is based on analysis of just over 8000 male deaths between 1971 and 1981 at ages 15–64 years as compared with the 273000 considered in the decennial supplement. However, difficulty in obtaining information in the census on
the previous occupation of men over retirement age has restricted traditional analysis of social class differences to those observed at working ages. At older ages the numerator denominator biases become unmanageable. Because it relies only on information from one source, the longitudinal study allows us to look at mortality differences beyond retirement age with some confidence, particularly as the duration of follow up increases.

Table 2 Parameter estimates for the model of time trends within social class

<table>
<thead>
<tr>
<th>Social class</th>
<th>$i$</th>
<th>$\alpha_i$</th>
<th>Standard error ($\alpha_i$)</th>
<th>$\beta_i$</th>
<th>Standard error ($\beta_i$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td>0</td>
<td>-0.037</td>
<td>0.166</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>I</td>
<td>1</td>
<td>0</td>
<td>0.129</td>
<td>-0.058</td>
<td>0.022</td>
</tr>
<tr>
<td>II</td>
<td>2</td>
<td>-0.218</td>
<td>0.136</td>
<td>0.019</td>
<td>0.012</td>
</tr>
<tr>
<td>III Non manual</td>
<td>3</td>
<td>-0.076</td>
<td>0.136</td>
<td>0.019</td>
<td>0.012</td>
</tr>
<tr>
<td>III Manual</td>
<td>4</td>
<td>-0.083</td>
<td>0.123</td>
<td>0.011</td>
<td>0.007</td>
</tr>
<tr>
<td>IV</td>
<td>5</td>
<td>-0.033</td>
<td>0.127</td>
<td>0.020</td>
<td>0.009</td>
</tr>
<tr>
<td>V</td>
<td>6</td>
<td>0.135</td>
<td>0.137</td>
<td>0.028</td>
<td>0.026</td>
</tr>
<tr>
<td>Inadequately described</td>
<td>7</td>
<td>0.056</td>
<td>0.186</td>
<td>0.028</td>
<td>0.026</td>
</tr>
<tr>
<td>Unoccupied</td>
<td>8</td>
<td>1.26</td>
<td>0.134</td>
<td>-0.067</td>
<td>0.014</td>
</tr>
</tbody>
</table>

Fig 2 Mortality of men aged 15–64 by social class and year of death

Table 3 and fig 3 compare mortality by social class at ages 65–74 years and 75 years and over with that at younger ages. This is restricted to the period 1976–81 in order to allow for the selection effect of the unoccupied to wear off. At older ages this unoccupied group is less important to the analysis than at younger ages. Although more deaths to unoccupied men occurred over age 65 years than at younger ages, the deaths of unoccupied men accounted for only 4% of deaths over age 65 years. The decline in importance of this category with age is expected because the cohort effect of health selection is expected to wear off with time. At the same time people who had been permanently sick for a number of years before the census, but who were over retirement age at census, may have tended to report themselves as retired rather than permanently sick. For the retired the census coders would seek a last occupation (which they did not do for the permanently sick). In this way men who were sick at these older ages would be included in the social class analysis.

The problem of those men with inadequately described occupation, although numerically of increasing importance at older ages, has less impact than at younger ages because the mortality of this group is nearer to that of the whole population at the same age. Again, this is as would be expected since many respondents at these older ages reported that they were retired (in response to the census question about economic position) but subsequently on the census schedule failed to give an occupation. This latter failure appears to have occurred as a result of a genuine misunderstanding of the census instructions and, in mortality terms at least, was unrelated to the health of the respondent.

The data presented in table 3 and fig 3 are the first reliable estimates of social class gradients in England and Wales at these older ages. The impression given is that, even if there is some narrowing of the class gradients with age, there remains a marked difference in the oldest age group. This is borne out by the age specific mortality ratios presented in fig 4.
Social class mortality differentials: artefact, selection or life circumstances?

Fig 3 Mortality of men in 1976–81 by social class and broad age groups

and the model fitted to these data, which is summarised in table 4. As with the model fitted to the time trends for each social class, the model in table 4 gives a satisfactory fit to the data. The deviance of 39.3 on 42 degrees of freedom compares with a deviance of 258.5 on 53 degrees of freedom in the null model. Much of the reduction in deviance is achieved by fitting a model with only social class terms (deviance 50.5 on 48 degrees of freedom). The parameters \( \beta_i \), which represent class specific trends in SMR with age, support the impression given by fig 4 of no clear trends with increasing age, except for men in social class IIIM, for whom the mortality ratios are higher at older ages.

Discussion

These new data on deaths of men at ages 15–64 years in the period 1976–81 from the OPCS Longitudinal

Study confirm earlier estimates of mortality differentials by social class from the 1970–72 Decennial Supplement on Occupational Mortality and those from mortality in the period 1971–5 from the longitudinal study. In particular, the effects of selection out of the workforce, noted in the early period of follow up, are found to have diminished

Table 3 Mortality of males in 1976–81 by social class in 1971 and age at death

<table>
<thead>
<tr>
<th>Social class in 1971</th>
<th>15–64</th>
<th>65–74</th>
<th>75 and over</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Obs</td>
<td>Exp</td>
<td>SMR</td>
</tr>
<tr>
<td>I</td>
<td>128</td>
<td>1938</td>
<td>66</td>
</tr>
<tr>
<td>II</td>
<td>625</td>
<td>808-3</td>
<td>77</td>
</tr>
<tr>
<td>IIIN</td>
<td>450</td>
<td>429-3</td>
<td>105</td>
</tr>
<tr>
<td>IIIM</td>
<td>1435</td>
<td>1501-0</td>
<td>96</td>
</tr>
<tr>
<td>IV</td>
<td>774</td>
<td>710-6</td>
<td>109</td>
</tr>
<tr>
<td>V</td>
<td>358</td>
<td>289-1</td>
<td>124</td>
</tr>
<tr>
<td>Armed Forces</td>
<td>20</td>
<td>21-8</td>
<td>92</td>
</tr>
<tr>
<td>Inadequately described</td>
<td>86</td>
<td>45-1</td>
<td>191</td>
</tr>
<tr>
<td>Unoccupied</td>
<td>231</td>
<td>108-3</td>
<td>213</td>
</tr>
<tr>
<td>Total</td>
<td>4107</td>
<td>4107-4</td>
<td>100</td>
</tr>
</tbody>
</table>
substantially. While the linear models fitted in table 2 suggest that differences between social classes may continue to widen with increased duration of follow up, the data on which this model was based may fit alternative models equally well. In particular, after the widening differential during this initial period, we might now expect to see a continuing flattening of the slopes with time for each class as the follow up period is increased. It is still too early in the study to predict when such changes will become negligible from year to year.

For men dying at ages over 65 years the present analysis extends the analysis of SMRs beyond the scope of previous data. In view of earlier comments on national data, it is perhaps surprising that at ages after retirement mortality gradients in 1976–81 are almost as steep as those found in the later stages of normal working life. For example, at ages over 75 years men allocated to social class V have a more than 50% higher death rate than men in social class I.

For the majority of men aged over 65 years at census, the social class recorded here represents their main occupation before retirement. Hence the existence of such steep gradients in these older age groups sheds some important light on arguments about the influence of selective mobility between social classes. The argument that a drift down the social scale may occur during periods of ill health cannot be extended with any force to people over age 75 years at death who would have retired 10 or more years before death. Differentials at these oldest ages must therefore be attributed to their socioeconomic environments, lifestyles, and circumstances in the period leading up to their deaths, not to their changing social class as a result of ill health. Prospective studies of people exposed to particular occupational hazards also indicate that direct effects of occupation have greater impact on SMRs at younger than at older ages (see, for example, Case et al).

These comments about the mortality of men at older ages can also now be extended to men towards the end of the working age range. The data presented here, relating deaths in 1976–81 to social class in 1971, suggest that the mortality gradient by social class five to 10 years before death is similar both to that by social class at death (published in the 1970–72 Decennial Supplement on Occupational Mortality) and to the gradients presented here for men at older ages. This again suggests that mortality gradients at these ages, although subject to short term effects of health related exclusion from the social class classification, are little influenced by health related mobility between social classes. Far more important are the effects of accumulated life experiences.

Where then does this leave arguments about the influence of inter class mobility and its effects on mortality differentials? First, we should emphasise that most of the literature on inter class mobility and its relation to health, both applied and theoretical, has concentrated on mobility at younger ages than are considered here. These studies focus on the changes between generations, comparing father's class with own class, or at the earliest stages of working life, comparing first job with job at age 30, say. These are ages when there is a much higher degree of mobility than at older ages. Some of this mobility is undoubtedly health related, as is particularly well illustrated by the Aberdeen study of pregnancy outcome in relation to own and father's social class. Also, the effects of any health related selection at these ages will be magnified beyond the level implied by the numerical size of those changing class because a small proportion of people at these ages are unhealthy. However, as we argued in our analysis of mortality in 1971–5 (see Fox and Goldblatt, Chapter 11), such selection effects would be expected to wear off with time if they reflected only health at the time of selection.

Other mechanisms by which health selection at younger ages could have a major influence on mortality at older ages can be hypothesised. For example, it might be suggested that factors which influenced health related behaviour after selected mobility were the same as those that influenced the direction of this mobility. As an illustration, under such a model, people who smoked or had poor diets

Table 4 Parameter estimates for the model of trends in SMR with age group within social class

<table>
<thead>
<tr>
<th>Social class</th>
<th>i</th>
<th>$\alpha_i$</th>
<th>Standard error ($\alpha_i$)</th>
<th>$\beta_i$</th>
<th>Standard error ($\beta_i$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Constant</td>
<td></td>
<td>-0.635</td>
<td>0.328</td>
<td></td>
<td></td>
</tr>
<tr>
<td>I</td>
<td>1</td>
<td>0</td>
<td></td>
<td>-0.0039</td>
<td>0.0045</td>
</tr>
<tr>
<td>II</td>
<td>2</td>
<td>0.253</td>
<td>0.357</td>
<td>-0.0024</td>
<td>0.0019</td>
</tr>
<tr>
<td>III Non manual</td>
<td>3</td>
<td>0.859</td>
<td>0.374</td>
<td>-0.0041</td>
<td>0.0025</td>
</tr>
<tr>
<td>III Manual</td>
<td>4</td>
<td>0.412</td>
<td>0.343</td>
<td>-0.0033</td>
<td>0.0014</td>
</tr>
<tr>
<td>IV</td>
<td>5</td>
<td>0.734</td>
<td>0.354</td>
<td>-0.0004</td>
<td>0.0018</td>
</tr>
<tr>
<td>V</td>
<td>6</td>
<td>0.896</td>
<td>0.382</td>
<td>-0.0017</td>
<td>0.0026</td>
</tr>
</tbody>
</table>
Social class mortality differentials: artefact, selection or life circumstances?

might be more likely to be downwardly mobile; their dietary or smoking behaviour would, moreover, remain constant at least until such time as it had seriously and irrevocably affected their health.

Anyone putting forward such an argument needs to recognise that it is not health that underlies the initial social selection process at younger ages, but health related behaviour, and that this pattern of behaviour must persist into the later stages of life, having been determined by the experiences of childhood or early adulthood. Under the model, experiences and circumstances at older ages may, in general, only serve to reinforce the effect of these early experiences on health related behaviour. We would favour an explanation that recognised a somewhat more variable influence of differentials in the recent experiences and circumstances of individuals on their health related behaviour and hence on mortality. It would recognise both the influences on experience and behaviour that result from downward mobility and the major changes in behaviour that have taken place during this century.

A second mechanism might be put forward to relate mortality at older ages to mobility at younger ages. This would suggest that some types of chronic morbidity at younger ages are severe enough to have an immediate influence on social mobility but do not have a major impact on mortality until considerably older ages. A number of psychiatric conditions could be argued to operate in this way.

It needs to be recognised, however, that such conditions would have to be fairly widespread in the community to have any impact on social class differentials. Most studies of chronic diseases, moreover, indicate raised levels of mortality at every age. The effect of persistently raised levels of mortality among an unhealthy group and their subsequent influence on general mortality can best be illustrated by an example from the longitudinal study. The unoccupied in 1971 were sufficiently large in number in 1971–5 to have a distorting effect on social class mortality gradients. However, in 1976–81 their influence had diminished, in part because their SMRs were lower, but also because their earlier mortality experiences had reduced their numbers.

It would therefore seem improbable to us that incipient ill health could persist in a large part of the community for many years, affecting their job prospects for most of their working lives, failing to diminish the size of the group through early mortality; and yet, at the end of their working lives, resulting in excess mortality sufficient to generate observed social class differentials.

To the extent that they contribute to the interpretation of social class differentials in mortality in the 1970s, therefore, these new data add credence to the gradients published in the decennial supplement and suggest that major explanations for such differentials should not be sought from among artefactual or selection theories. This concurs with the conclusion drawn by the Black Working Group.

These observations, if supported by further data from mortality in the period 1981–5, are of major importance to the interpretation of differentials in the decennial supplement series. By confirming the broad findings of the 1970–2 report, this analysis adds weight to the main focus of the decennial supplement analysis which looks at social class differences by cause of death. We do not suggest that such data will, in the short term, be available from the longitudinal study. However, the observations we have made, for example about health related selection, should encourage those searching for explanations of class differentials for particular causes of death to look to environmental and behavioural factors rather than to social mobility.

A second major limitation of the decennial supplement has resulted from its inappropriateness for making comparisons of mortality differentials over time; this was seen by the Working Party under Sir Douglas Black as one of the main shortcomings of available sources. It is clearly too early to make promises about future comparisons using new data currently being incorporated into the longitudinal study. However, we plan to look at mortality in the 1980s linked to social class in both the 1971 and 1981 census in the same way as we have looked at mortality in the 1970s in relation to 1971 census characteristics alone. This will, for example, allow comparison across generations, at this stage, measured ten years apart. Second, and of greater immediate significance, mobility comparisons of circumstances in 1971 and 1981 are now being made. These will, in the near future, shed important light on the extent and direction of mobility at different ages and, when linked to deaths in the 1980s, to the contribution of health related mobility in the 1970s to mortality in the 1980s. This analysis will build on that which describes the impact of selective migration before the 1971 census on geographic differentials in 1971–5.18

These analyses are part of a collaborative review of the longitudinal study mortality data by OPCS Medical Statistics Division and the Social Statistics Research Unit at City University. The latter are supported in this work by a programme grant from the Medical Research Council.

Crown copyright is reserved.
A J Fox, P O Goldblatt, and D R Jones

References

5 Op Cit 2, 175–8.
6 Op Cit 1, 153–7.