# Intergenerational Mobility and Sample Selection in Short Panels

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## ABSTRACT

Using data from the first eleven waves of the BHPS, this paper measures the extent of the selection bias induced by adulthood and coresidence conditions --- bias that is expected to be severe in short panels --- on measures of intergenerational mobility in occupational prestige. We try to limit the impact of other selection biases, such as those induced by labour market restrictions that are typically imposed in intergenerational mobility studies, by using different measures of socioeconomic status that account for missing labour market information. We stress four main results. First, there is evidence of an underestimation of the true intergenerational elasticity, the extent of which ranges between 10 and 25 percent. Second, the proposed methods used to correct for the selection bias seem to be unable to attenuate it, except for the propensity score weighting procedure, which performs well in most circumstances. This result is confirmed both under the assumption of missing-at-random data as well as under the assumption of not-missing-at-random data. Third, the two previous sets of results (direction and extent of the bias, and differential abilities to correct for it) are also robust when we account for measurement error. Fourth, restricting the sample to a period shorter than the eleven waves under analysis leads to a severe sample selection bias. In the cases when the analysis is limited to four waves, this bias may range from 27 to 80 percent.

*Key Words*: Sample selection; Censored data; Panel data; Intergenerational mobility; Occupational prestige.

*JEL classification codes*: C23; C24; J24; J62.

# 1 Introduction

Opinion among economists about the extent of intergenerational mobility has markedly changed in the last twenty years. Twenty years ago, the general view — based primarily on data from the United States — was that there was little persistence in economic status across generations (Becker and Tomes, 1986). A more recent wave of studies in the United States and several other industrialized countries has questioned that view, and found evidence suggestive of far less mobile societies than was earlier believed.[1] Two important problems, which marred early studies, have been underlined. First, the samples used in most analyses were nonrepresentative. In particular, they tended to be of small size and refer to highly homogeneous groups of the population of children and parents (e.g., individuals were from a specific region or city or they were twins). Second, long-run permanent economic status was poorly measured. Most of the early studies used single-year or other short-run measures of economic status, generally proxied by earnings or income.

To limit such problems, the more recent literature uses longitudinal household data from national probability samples. These data tend to avoid sample homogeneity (but may still lead to relatively small estimating samples). Moreover, if they have a long enough time series component, they allow researchers to compute better measures of long-run status, which are typically obtained after averaging data over several years. For example, using five-year averages, Solon (1992) and Zimmerman (1992) estimate intergenerational correlations that are about twice as large as those found in the earlier research surveyed by Becker and Tomes (1986). Using even more years of data, the estimates reported in Mazumder (2001) reveal intergenerational correlations that are about 50 percent greater than those reported in the Solon's and Zimmerman's studies. Recent research has also considerably improved our understanding of the mechanisms that link one generation to the next. Several studies have modeled and estimated the effect on intergenerational mobility of family background (Shea, 2000), neighbourhood influences (Page and Solon, 2003) and assortative mating (Chadwick and Solon, 2002).

However, researchers have virtually ignored one important issue that may plague all intergenerational correlation estimates, namely the issue of selection. The extent of intergenerational mobility is usually measured by estimating a relationship between a measure

[1] See Solon (1999) for a comprehensive review of this more recent literature.

of son's or daughter's economic status (e.g., earnings or income) and the same measure of economic status for his or her parent(s). Common practice has been to exclude all records of data where parents or children report no earnings or income (because, for example, they were unemployed at the time of the survey). Two exceptions are the studies by Couch and Lillard (1998) and Minicozzi (2003). Both conclude that there exists an important role for assumptions on labour market selection in identifying intergenerational income mobility, but their evidence is mixed. Couch and Lillard assign one dollar of income to individuals who have a valid report of no earnings, and find that more selected samples lead to higher correlations between sons' and fathers' incomes (i.e., *less* mobility). Minicozzi uses a different method and estimates different Manski-type bounds around children's income. Contrary to Couch and Lillard, she finds that dropping both unemployed and part-time employed sons leads to a *higher* degree of mobility than if part-time employed sons had been included.

In intergenerational mobility analyses, however, there may be also selection problems that are induced by restrictions not driven by unemployment, part-time employment or lack of labour market information. In the panel data usually used in such analyses, parents and children must be found living in the same household for some time during the panel years and followed over time. Information on children's or parents' status may be censored, for example, by non-ignorable attrition or by the fact that the time series component of longitudinal datasets is not long enough. The aim of this paper is precisely to assess how severe the problem of short panels can be for the estimation of intergenerational mobility between sons' and fathers' socioeconomic status. In short panels, in fact, the choice of the child generation is typically constrained by the trade-off between younger and older children. The former group is likely to be a random sample of children who coreside with their parents but their observed socioeconomic status is almost certainly a noisy measure of long-run status (the condition underlying the choice of this group is what we call *adulthood condition*). The latter group will have better measures of status but its inclusion may bias the sample towards children who coreside with their parents at late ages (this imposes what we call *coresidence condition*).

To address the problem induced by these two conditions we introduce a new methodology which we apply to a short panel using the first eleven waves of the British Household Panel Survey (BHPS), covering the period 1991-2001.[2] All BHPS respondents aged 16 or more are

[2]In this paper we specifically focus on (and model) this double selection problem only, and abstract from

asked to report the occupation of their parents when they were aged 14. This information is based on retrospective questioning (and therefore may suffer from recall error), but it relies neither on the adulthood condition nor on the coresidence condition. Using a continuous index of occupational prestige — which relates strongly to labour income — we then estimate intergenerational elasticities in occupational prestige that are free of selection bias. However, by linking children to parents over the eleven years of the panel and imposing both adulthood and coresidence conditions, we obtain a new selected subsample of son-father pairs. For this subsample, we again estimate intergenerational elasticities. Comparing these elasticities to those previously estimated provides us with a direct measure of the extent of the selection bias we are interested in. We then evaluate two approaches that correct for this potential bias. The first belongs to the general class of Heckman-type sample selection corrections. The second is within the class of models based on propensity score estimation, which we estimate using procedures based on inverse propensity score weighting and on dummies for different levels of the propensity score. Finally we conduct some sensitivity analyses to assess whether the elasticities estimated on the selected sample are robust to varying the length of the panel.

The rest of the paper is organised as follows. Section 2 describes the issue of selection we are concerned with in this study and casts our contribution within the relevant literature. Section 3 presents the data source, the estimating samples, our measures of socioeconomic status, and the other variables used in estimation. Section 4 discusses our methodology to gauge the extent of selection bias in our sample and to correct for it. Section 5 reports our results and shows a number of sensitivity checks. Section 6 concludes.

# 2 The Issue

In analysing the extent of intergenerational mobility, researchers have used several different measures of long-run socioeconomic status (e.g., educational attainment, labour income, hourly wages and occupational indices). Each measure has advantages and disadvantages.[3]

the selection issues related to labour market conditions, attrition or nonresponse. However, we will try to account for the issues related to nonresponse and/or out-of-labour-market conditions in ways similar to those used by Couch and Lillard (1998). An empirical analysis of intergenerational mobility where all these different types of selection are jointly modelled and fully accounted for bears investigation in future work.

[3]For a review, see the discussions in Atkinson, *et al.* (1983), Bowles and Gintis (2002) and Erikson and Goldthorpe (2002).

Because of restrictions imposed by our data, we will use an index of occupational prestige computed according to the technique proposed by Goldthorpe and Hope (1974), which is widely used especially in the sociological literature.[4] There are also several popular methods of examining intergenerational correlations in socioeconomic status. In this paper, we perform our analysis using a log-linear regression model which assumes that the log of a son's permanent socioeconomic status in family $i$, denoted by $y_i$, can be expressed as a linear function of his fathers log permanent socioeconomic status, $x_i$, according to

$$y_i = \alpha + \beta x_i + u_i, \tag{1}$$

where $\beta$ is our parameter of interest that denotes the intergenerational elasticity of son's status with respect to father's status; $\alpha$ is the intercept term that represents the change in status common to the son's generation; and $u_i$ is a random disturbance. Assuming (for simplicity and momentarily) that the underlying variances in father's and son's status are equal (so that $\beta$ coincides with the correlation between father's and son's status), a value of $\beta = 1$ indicates a situation of complete immobility, whereby (apart from the influence of $u_i$) the sons' position in their status distribution is fully determined by their fathers' position. A value of $\beta = 0$ instead indicates a situation of complete mobility or regression to the mean, whereby the child's position is completely independent of his father's; and with intermediate values of $\beta$ between 0 and 1, the status distribution still regresses to the mean, but at a rate that decreases the higher is $\beta$.[5]

If $y_i$ and $x_i$ are observed for all sons and fathers in the sample, then ordinary least squares (OLS) estimation will produce a consistent estimate of $\beta$. If, however, either $y_i$ or $x_i$ are observed only for some father-son pairs because of selection, the OLS estimate of $\beta$ obtained from these censored data is inconsistent. There are a number of reasons why information on $x$-$y$ pairs may be censored. First, observations where a parent or a child have no required status information are usually deliberately dropped from the analysis.

[4] Goldthorpe and Hope (1974) suggest that the scale which results from their occupational prestige grading exercise should not be viewed as a grading of social status *stricto sensu*, i.e., as tapping some underlying structure of social relations of "deference, acceptance and derogation" (p. 10). It should instead be viewed as "a judgement which is indicative of what might be called the 'general goodness' or ... the 'general desirability' of occupations" (pp. 11-12). Ermisch and Francesconi (2003) present a measurement model that shows the conditions under which the estimates based on the Hope-Goldthorpe index are consistent estimates of parameters similar to those in the economic literature.

[5] It is possible of course to have a negative $\beta$ (i.e., reversal). In these circumstances, fathers with status above (or below) the mean will have sons with below (or above) average status levels.

For example, by excluding fathers and sons that were not in full-time jobs (i.e., working on average 30 hours per week at least 30 weeks per year), Zimmerman (1992) implicitly assumes exogenous selection into full-time employment. Solon (1992), Dearden *et al.* (1997), Couch and Dunn (1997) and Chadwick and Solon (2002), among others, exclude cases in which income observations are nonpositive, and thus implicitly assume exogenous selection into employment. These assumptions are not consistent with standard economic results according to which selection into the labour force or into full-time employment is likely to be correlated with potential earnings (Heckman, 1979; Vella, 1998).[6]

Second, information on $x$-$y$ pairs may be censored because information on child's or father's status is missing (item nonresponse) or because one of the two individuals is not in the sample (unit nonresponse). By dropping father-child observations in which at least one has missing status information or has attrited out of the survey under analysis, most studies implicitly assume that nonresponse is random or that attrition is ignorable.[7] Again, these assumptions run counter to the findings that nonresponse is likely to be systematically correlated with observable and unobservable characteristics, or that estimates of intergenerational relationships are likely to be affected by differential attrition (Fitzgerald *et al.*, 1998).

Third, many longitudinal studies typically restrict their analysis to children from specific birth cohorts, say, born between years $b_1$ and $b_2$, $b_1 < b_2$ (e.g., Solon, 1992; Couch and Dunn, 1997; Bjorklund and Jantti, 1997; Chadwick and Solon, 2002; Minicozzi, 2003). The restriction on $b_2$ (which we label *adulthood condition*) is motivated by the need to assure that children's socioeconomic status is observed as far out as possible on their life cycle (when they are "adult") so that their observed status can be taken as a reliable measure of long-run permanent status.[8] This need has driven researchers to choose a relatively low $b_2$. The restriction on $b_1$ (which we label *coresidence condition*) is instead motivated by the need to avoid overrepresenting children who left home at later ages. If there are unobserved factors affecting children's later socioeconomic status (through $u_i$ in (1)) that also influence children's chances of living (or "coresiding") with their parents, then the OLS estimate of

[6]This is the type of selection examined by Couch and Lillard (1998) and Minicozzi (2003).

[7]Selection based on the availability of other relevant variables for children and parents (e.g., age, hours of work, education) will induce similar problems. See Couch and Lillard (1998) for a review of studies that used sample selection criteria on such variables.

[8]Clearly the selection generated by the adulthood condition and the selection induced by the child's labour market conditions described above are closely related. Our empirical analysis will in part address this issue.

$\beta$ will suffer of sample selection bias. So the need to limit this bias has led researchers to choose a relatively high $b_1$. This double choice produces the selection problem which this paper focuses on.

In longitudinal birth cohort datasets that follow individuals born in one specific week over time (such as the National Child Development Study used by Dearden *et al.* (1997)), the problem of choosing $b_1$ and $b_2$ does not exist since $b_1$ and $b_2$ are equal and coincide with the year of the start of the survey, $b_0$. The drawbacks of such data sources however are that intergenerational analyses can be done only after a relatively long period of time since the surveys started (which may lead to serious attrition bias), and generalisations to other (younger or older) cohorts are not straightforward. In nationally-representative family-based longitudinal data (such as the Panel Study of Income Dynamics used by Solon (1992) or the German Socioeconomic Panel used by Couch and Dunn (1997)), the problem exists and may be serious, especially when $b_1 < b_2 \leq b_0$. For example, in Solon (1992) $b_1$=1951 so that children are aged at most 17 in 1968 (i.e., $b_0$, the first year of the survey analysed by Solon) to address the coresidence condition, and $b_2$=1959 so that children are aged at least 25 in 1984 (the last year of observation) to address the adulthood condition. Apart from attrition issues, this problem disappears if the panel data have a long enough time series component, so that $b_2 \geq b_1 \geq b_0$. But for analyses based on short panels, such as that used in this paper, the problem is potentially severe. Our two main objectives therefore are (a) to quantify the extent to which standard estimates of $\beta$ obtained from short panels are affected by this bias, and (b) to evaluate alternative econometric techniques to alleviate its effects. Before explaining the methodology to pursue such objectives, in the next section we describe the data used in our empirical application.

# 3 Data

The data we use are from the first eleven waves of the British Household Panel Survey (BHPS) collected over the period 1991-2001. Since Autumn 1991 the BHPS has annually interviewed a representative sample of about 5,500 households covering more than 10,000 individuals. All adults and children in the first wave are designated as original sample members. On-going representativeness of the non-immigrant population has been maintained by using a "following rule" typical of household panel surveys: at the second and subsequent waves, all original sample members are followed (even if they moved house or if their households split up). Personal interviews are collected, at approximately one-year intervals, for all adult members of all households containing either an original sample member, or an individual born to an original sample member.[9] The sample therefore remains broadly representative of the population of Britain as it changes over time.[10] From the BHPS, we select three different samples and employ various measures of the occupational status variable for sons and fathers, with the double aim of gauging the extent of selection bias of interest here and of attenuating the measurement error problem inherent in all intergenerational studies. We now turn to describe samples and variables.

## 3.1 Samples

Our main analysis is restricted to 2691 men (or sons) born between 1966 and 1985, who have at least one valid interview over the panel period under study. This represents our Full Sample. The BHPS asks all adult respondents aged 16 or more to provide information about their parents' occupations when they (the respondents) were aged 14, and releases data on an index of occupational prestige introduced by Goldthorpe and Hope (1974). This index ranges from 5 to 95, with greater values indicating higher occupational prestige, and it is highly correlated with earnings.[11] Between 1991 and 2001, the BHPS data indicate a correlation between gross monthly earnings and the Hope-Goldthorpe (HG) index of 0.70 for men and 0.75 for women. Because the position of individuals in the occupational hierarchy is relatively stable over the life cycle, the HG scale is also likely to be an adequate measure of people's permanent socio-economic status (Nickell, 1982; Ermisch and Francesconi, 2003).

All the individuals in the Full Sample who could be successfully matched to their father are part of our second sample, which we refer to as Restricted Sample. There are 1114 of such father-son pairs. Differently from the Full Sample, this imposes stringent adulthood

[9] Individuals are defined as "adult" (and are therefore interviewed) from their sixteenth birthday onwards.

[10] Of the individuals interviewed in 1991, 88 percent were re-interviewed in wave 2 (1992). The wave-on-wave response rates from the third wave onwards have been consistently above 95 percent. See Taylor (2003) for a full description of the dataset. Detailed information on the BHPS can also be obtained at <http://www.iser.essex.ac.uk/bhps/doc>. The households from the European Community Household Panel subsample (followed since the seventh wave in 1997), those from the Scotland and Wales booster subsamples (added to the BHPS in the ninth wave) and those from the Northern Ireland booster subsample (which started in wave 11) are excluded from our analysis.

[11] Phelps Brown (1977) reports a strong log-linear relationship between median gross weekly earnings and the HG score, with a rise of 1 unit in the index being associated with an increase of 1.031 percent in earnings. Nickell (1982) finds a correlation between the HG score and the average hourly earnings of 0.85.

and coresidence conditions.[12] Individuals born in 1966 were aged 25 in the first year of the panel (1991) and 36 in the last (2001): they could have lived with their parents at any age between those two years. With a median home-leaving age of about 23-24 (Ermisch and Di Salvo, 1997; Ermisch and Francesconi, 2000), coresidence at such ages means that the Restricted Sample overrepresents sons who left home at late ages. At the other extreme, individuals born in 1985 were aged 16 in 2001 (the last year of analysis): although they are likely to be a random sample of young people living with their parents, their HG index is arguably a noisy measure of long-run status. The comparison of the intergenerational elasticities obtained from the Full Sample and the Restricted Sample will provide us with a measure of the extent of the selection in short panels, under the maintained assumption that Full Sample estimates do not suffer from selection bias.

As discussed in the Introduction, one of the issues emphasised by recent empirical studies is the lack of reliable measures of fathers' long-run permanent status. In this respect our dataset is no exception (see the next subsection). To gauge the impact of the related bias, we thus analyse a third sample (of fathers). In the second (1992) and eleventh (2001) waves of the BHPS, adult respondents were asked to provide information on all their children regardless of where they lived. This, which we call Supplemental Sample, is composed of 1434 men whose sons were born between 1966 and 1985 (the same year-of-birth selection used in the Full Sample). This should provide us with a random sample of fathers with sons born in those years.[13] As measurement error corrections would typically require information on the variance of fathers' permanent status (Zimmerman, 1992), the Supplemental Sample allows us to estimate any moment of distribution of fathers' HG scores, both unconditional and conditional on son's age.

## 3.2 Socioeconomic Status Variables

We use two alternative measures of sons' socioeconomic status. Assuming exogenous selection into the labour market, the first measure (labelled $HG_1^s$) is given by the average HG score over all waves after excluding the cases with missing status information either because

[12] Although the date-of-birth restrictions imposed on the Full Sample are the same as those imposed here for comparability purposes, they could be relaxed and allow us to concentrate only on prime-age men in employment.

[13] As opposed to the Full Sample, however, the Supplemental Sample does not contain information on sons' HG scores, except for those cases in which — as in the Restricted Sample — father and son were observed to live in the same household at least once over the sample period.

the son does not work or because his information is genuinely missing.[14] The second measure ($HG_2^s$) is given by the average HG score over all waves after replacing the cases in which the son is not in paid employment (except for those who are in full-time education) with the minimum HG score observed in the whole sample.[15] Table 1 shows that the mean values of $HG_1^s$ and $HG_2^s$ are 43 and 41 respectively for sons in the Full Sample, and 40 and 39 for sons in the Restricted Sample. Regardless of the sample, therefore, $HG_2^s$ is smaller than $HG_1^s$ (as we replaced the cases with missing prestige information with the minimum HG observed), but their differences are not statistically significant. Imputed values, in fact, amount only to 9 percent in the case of the Full Sample and 5 percent in the case of the Restricted Sample.

As mentioned above, one of the major difficulties in estimating intergenerational elasticities abides in the fact that father's status, $x_i$, is measured with error. The key problem is the lack of direct measures of permanent status. In the case of the Full Sample, in particular, the BHPS provides us with only one single-year measure of fathers' occupational prestige (when sons were aged 14).[16] Although the HG index is an arguably good proxy for long run status, a single-year measure may still be tainted by transitory fluctuations in fathers' careers. In addition, the BHPS elicits this information by asking respondents to report their parents' occupation when they were aged 14. The retrospective questioning of children to obtain data on parents may of course generate recall errors.[17] Both types of errors (due to measurement and recall) may be such that the variance of observed status is greater than the variance of permanent status, leading the OLS estimate of $\beta$ in (1) to be biased downward.[18] Estimation of (1) is further complicated by the fact that fathers' and sons' occupations may refer to different points in their life cycle. Although this age variation could also bias the

[14]Several studies have argued that averaging status data over time reduces the impact of the transitory component of the status variable (thus reducing the potential of errors-in-variables bias) and yields a more accurate measure of permanent status. See, among others, Solon (1992), Zimmerman (1992), Dearden *et al.* (1997), and Mazumder (2001).

[15]This minimum value does not refer to the minimum score in the individual's work history (because this information is not available), but to the overall minimum score observed in the Full Sample. For a similar treatment of non-random selection into the labour force, see Couch and Lillard (1998).

[16]In the case of the Restricted Sample, instead, multiple measures of fathers' HG scores are available in principle. For that sample, however, there are still measurement problems in that fathers and sons are observed at different points in their life cycle (see below). Clearly, the Restricted Sample is expected to suffer from the selection bias discussed in Section 2.

[17]Again, our Restricted Sample does not have to face this problem as fathers and sons report independently their own occupational information.

[18]Because our Supplemental Sample is not affected by such errors, it should give us an idea of how serious this problem might be for the Full Sample.

estimates of intergenerational mobility, no general conclusion on the direction of this bias can be reached (Jenkins, 1987).[19]

To address at least part of such problems we use four different measures of fathers' socioeconomic status. The first uses the single-year measure reported by sons, which identifies fathers' occupational prestige when their sons were aged 14, and excludes all cases with missing information (because either the son refused to answer, or he did not know his father's occupation, or his father did not work when he was 14).[20] This is the measure available for the Full Sample, $HG_1^f$. For all sons in the Full Sample who do not report the occupational information on their fathers but could be successfully matched to them, we replace the missing values on paternal occupational prestige with the HG scores that are available for fathers and average them over all available waves (as long as they are nonmissing). Of course, this replacement can be done only for those father-son pairs that are in the Restricted Sample. We denote this measure $HG_2^f$. Our third measure, $HG_3^f$, replaces the missing values of $HG_2^f$ with the minimum score observed in the entire Full Sample.[21] The last measure, $HG_4^f$, is exactly as the previous one, except that it replaces the missing values of $HG_2^f$ with the median score in the Full Sample. The assumption behind $HG_3^f$ is that the missing values are for fathers with low permanent incomes. This is justified by the fact that the majority of such cases are missing due to inapplicability (that is, fathers who are likely to be unemployed or out of the labour market). The $HG_4^f$ measure instead assumes that the missing values refer to fathers who are randomly spread over the entire distribution of occupational prestige. We replace them with the median which minimises the mean absolute errors and is robust to outliers.[22]

In the case of the Supplemental Sample we compute two measures of fathers' HG scores. The first, $HG_{SS}^f$, is the index obtained from the father in the second or eleventh wave when

[19]Using American and Canadian data, Grawe (2003) documents that income variance increases as father's income is measured later in the life cycle, and this leads to lower estimates of the intergenerational elasticity. On the other hand, as sons become older, estimates of income persistence are higher. Evidence for other countries, however, is not yet available.

[20]Different fathers have different ages at their sons' birth. Thus, fathers' status is likely to be measured at different points of fathers' working careers. In our analysis we cannot account for this, because father's age is not available.

[21]The rationale for this recoding exercise is the same as that adopted in the case of sons (see above).

[22]One of the replacement methods used by Minicozzi (2003) is similar to our $HG_3^f$, when she replaces missing values in annual labour income with \$2,000. Differently from Couch and Lillard (1998) who use this method to compute a selection-corrected point estimate of the intergenerational elasticity, Minicozzi (2003) uses it to estimate the worst-case lower bound of the elasticity.

he is asked to report information about all his children. The second, which we denote $\overline{HG}^f_{SS}$ is the average of the HG scores across all waves in which the father is observed. If this information is missing, we replace it with the occupational prestige reported for the most recent job. Both averaging across waves and replacing missing data are meant to reduce the problems induced by missing or inapplicable cases and measurement error (see subsection 5.2.3).

Table 1 shows that the distributions of $HG^f_1$, $HG^f_2$ and $HG^f_4$ are relatively similar in the Full and Restricted Samples, with means ranging between 47.6 and 49.9 and standard deviations ranging between 12.3 and 15.7. The mean prestige scores in the Supplemental Sample are remarkably close to the scores in the Full Sample, which suggests that measurement and recall errors affecting the status measures in the Full Sample may not be substantial. In addition, we never reject the hypothesis that the mean HG scores and their standard deviations in the Full and Restricted Samples differ for sons or for fathers, even at significance levels as high as 10 percent. The story is different for $HG^f_3$. For this measure we find lower values in the Restricted Sample at about 46 points, and especially in the Full Sample with a mean score of about 38.6 points. These two values differ at any statistical level, and, within sample, each of them also differs from the other three measures, $HG^f_1$, $HG^f_2$, and $HG^f_4$. Table 1 shows that the imputations underlying $HG^f_3$ and $HG^f_4$ lead to 12- and 54-percent bigger samples in the case of the Restricted and Full Samples respectively as compared to the analysis based on $HG^f_2$. Replacing a large proportion of missing/inapplicable cases (particularly in the Full Sample) may alleviate one problem of selection but may introduce another problem related to how the imputation is performed. This trade-off must be kept in mind while interpreting our results, particularly those involving $HG^f_3$.

## 3.3 Other Variables

Table 1 lists the summary statistics of the other variables used in the analysis. As in several other studies, the model for son's status in equation (1) is extended to include son's age and its square (Solon, 1992; Zimmerman, 1992; Dearden *et al.*, 1997; Couch and Dunn, 1997; Corak and Heisz, 1999). Although the age range of 16 to 34 years is the same in both samples, the mean age of sons in the Full Sample is 23.2, about 2 years greater than in the Restricted Sample. We do not have information on fathers' age in the Full Sample, but in the Restricted Sample their mean age is 48.2, while it is 46.9 in the Supplemental Sample, whose higher standard deviation reflects its greater heterogeneity.

The next section will illustrate our methods to account for sample selection biases. A number of other variables are used to model the probability of observing fathers and sons living in the same household at least once during the sample period. These are reported in Table 1. Approximately 94 percent of the sons in both samples are white. The average year of birth for sons in the Full Sample was 1974, compared with 1976 for those in the Restricted Sample. Religious attendance is another factor that is believed to have a deep effect on the likelihood of young people's leaving parental home (Cherlin, 1992). Although religious views on family formation are varied, strong religious beliefs are one cultural source of ideas that encourages the maintenance of traditional values (Wilcox, 2002). For each of the three religious denominations considered here (Catholic, Protestant, and other religions), "attendance" is defined as attending religious services at least once a month.[23] In both samples, less than 20 percent of the young men are religiously active, more than 40 percent have no religious affiliation and the rest have an affiliation but do not attend services regularly.

Many studies of household formation have underlined the importance of the price of housing (e.g., Haurin *et al.*, 1994; Ermisch, 1999). House prices indeed can affect the likelihood of observing fathers and sons living together, and so they may determine the selection into the Restricted Sample.[24] In Table 1, the (log) average prices of housing are relatively sim-

[23]The "Protestant" group includes: Church of England (Anglican), Church of Scotland, Free Presbyterian, Episcopalian, Methodist, Baptist, Congregationalist, and other Christian denominations. The "other religions" include: Muslim, Hindu, Jewish, Sikh, and other non-Christian denominations. The omitted category includes those with no religious affiliation as well as those who have a religious affiliation but attend religious services only infrequently. Distinguishing between such two groups does not change our results.

[24]The price of housing is an ambiguous concept when there are different housing tenures, non-neutral tax treatment of them, and probable imperfections in financial markets (Ermisch and Di Salvo, 1997; Ermisch, 1999). In Britain in 2002, nearly 90 percent of households are either owner-occupiers (68 percent) or "social tenants" (22 percent). The latter primarily includes households who rent their dwelling from local authorities. Social housing is not allocated by price, but by administrative procedures, which give priority to families with children and the elderly. While only a small proportion of all households rent from private landlords, it is a relatively important sector for young people leaving their parental home, being the destination of 45 percent of all departures and 33 percent of departures among those who are not full-time students. Owner-occupation is the destination for 56 percent of non-student departures. Information on rents in the private market is not available in the BHPS, and there are barriers to entry into social rental housing for young people. Our measure of the price of housing is the same as that used by Ermisch (1999), and is given by the average "mix-adjusted" house price relative to the retail price index in any given year for the region in which a person resided in that year. It adjusts for changes in the mix of the size and type of house (e.g., detached, semi-detached, flat, etc.), but does not adjust for quality change. This measure is likely to capture a large proportion of the variation in a measure of the annual "user cost of housing" for owner-occupiers, because mortgage and income tax rates are set nationally and relative house prices show much larger variation over time than these. It also could be viewed as an indicator of housing market conditions, in both rental and

ilar in the Full and Restricted Samples, with their corresponding levels being about 65,000 and 63,000 respectively. These averages mask large differences across regions and over time. Eight regional dichotomous variables are included in the selection model for the standard regions of Great Britain.[25] These are the same regions used to assign annual house prices, and so identification of the effects of the price of housing on the probability of not living apart from parents is based on the differences in patterns of house price changes among regions. Table 1 shows that the regional distributions of sons are similar across samples.

# 4 Methods

The first objective of our analysis is to determine the impact of sample selection assumptions on intergenerational mobility estimates. We begin by estimating an extended (still very parsimonious) specification of equation (1) on the Full and Restricted Samples, namely:

$$y_i = \alpha + \beta x_i + A_i'\gamma + \epsilon_i, \tag{2}$$

where $A_i$ is a vector containing the son's age and age squared, and $\epsilon_i$ is the new error term. Comparing then the estimates of $\beta$ obtained from the two samples provides us with a measure of the extent of the bias. We will perform this comparison by using each of the two HG score measures for sons alternatively with each of the three measures for fathers.

After having established the magnitude of the bias (if any), we consider different estimation methods to correct for it. For this purpose, we apply five different methods.[26] The first method is based on a maximum likelihood (ML) estimation of the main equation (2) jointly with the following selection equation:

$$r_i^* = Z_i'\Theta + v_i, \tag{3}$$

with

$$\begin{aligned} r_i &= 1 \text{ if } \quad r_i^* > 0 \\ r_i &= 0 \text{ otherwise,} \end{aligned} \tag{4}$$

owner-occupied markets. For individuals in the Full Sample who could not be matched with their fathers, the price of housing refers to the price observed in the first wave they were in the panel. For those who coreside with their fathers (and therefore, all sons in the Restricted Sample), this variable is measured at the last wave they were observed living together.

[25]The regions are Greater London (which is our base category), South East, South West, East Anglia and East and West Midlands, North West (including Yorkshire and Humberside), Rest of the North, Wales, and Scotland.

[26]For a comprehensive review, see Vella (1998).

where $r_i^*$ is a latent variable with associated indicator function $r_i$ that takes value 1 if the father's status, $x_i$, is observed, and 0 if $x_i$ is missing; $Z_i$ is a vector of explanatory variables (possibly including $A_i$) that determine the probability of observing a son living with his father during the survey years and are assumed for simplicity to be observed for all individuals; $v_i$ is a zero-mean error term with $E(\epsilon_i \mid v_i) \neq 0$; and $\Theta$ is a conformable vector of parameters to be estimated. Under the assumption that $\epsilon_i$ and $v_i$ are independently and identically distributed $N(0, \Sigma)$, with $\Sigma$ being a full-rank variance-covariance matrix, and $(\epsilon_i, v_i)$ independent of $Z_i$ and $x_i$, it is straightforward to estimate all the parameters in (2) and (3) using a standard ML procedure.

The second method follows the parametric two-step (TS) estimation procedure suggested by Heckman (1979), which, like the previous method, imposes a bivariate normal distribution on the error terms, $\epsilon_i$ and $v_i$. The idea here is that in the conditional expectation of (2), after normalising $\sigma_v$ to 1, the term $E(\epsilon_i \mid Z_i, r_i = 1)$ equals $\sigma_{\epsilon v}\left(\frac{\phi(Z_i'\Theta)}{\Phi(Z_i'\Theta)}\right)$, where $\phi(\cdot)$ and $\Phi(\cdot)$ denote the probability density and cumulative distribution functions of the standard normal distribution, and the term in parentheses is known as the inverse Mill's ratio. Therefore $E(\epsilon_i \mid Z_i, r_i = 1)$ is different from zero as long as the error terms in (2) and (3) are correlated, that is, $\sigma_{\epsilon v} \neq 0$. The TS method is first to obtain an estimate of the inverse Mill's ratio using a probit model, and then obtain a consistent estimate of $\beta$ (and the other parameters in (2)) by OLS.[27] Without imposing any exclusion restriction, both ML and TS methods will identify our model (2)-(4) via functional form (i.e., through the nonlinearity implied by the error structure). However, in the analysis below we introduce exclusion restrictions, with our selection equation including the same variables in (2) plus dummy variables for region of residence, religious attendance and race, and the regional house price index.[28]

A criticism of the ML and TS methods is their reliance on distributional assumptions on $\epsilon_i$ and $v_i$ (Lee, 1984; Bera *et al.*, 1984). In particular, bivariate normality implies that the relationship between the error terms is linear. Our third method tries to test for departures from normality by including terms that capture systematic deviations from linearity in the disturbances. This method again involves a two-step procedure, in which the first step is identical to that of TS. The second step estimates the conditional expectation of (2), but

[27]Following Heckman (1979), we compute consistent estimates of the variance-covariance matrix of $\beta$ and $\gamma$ in the second step by accounting for the heteroskedasticity of the error terms.

[28]Similar restrictions are used in Vella (1998). Clearly, for our exclusion restrictions to be valid, we need region, race and the house price index: (a) to be predictive of $r_i$; (b) to assign the realisation of $r_i$ randomly across families given the other observables; and (c) to be exogenous to $\epsilon_i$.

— rather than including the first-step estimate of the inverse Mill's ratio as an additional regressor — it includes a polynomial expression that is proportional to the inverse Mill's ratio and takes the form

$$E(\epsilon_i \,|\, Z_i, r_i) = \left[\frac{\phi(Z_i'\Theta)}{\Phi(Z_i'\Theta)}\right] \sum_{j=0}^{J} (Z_i'\Theta)^j,$$

where $J$ is the order of the polynomial. If $J = 0$, this expression equals the inverse Mill's ratio of the TS method. But coefficients of the terms for $j = 1, ..., J$ that are statistically different from zero lead to reject the hypothesis that the disturbances are bivariate normal. In our application $J$=4. We refer to this method as TSN, to underline its two-step nature and the fact that it tests for nonnormality.

Our last two methods rely less on distributional assumptions by imposing an index restriction according to which $E(\epsilon_i \,|\, Z_i, r_i = 1) = g(Z_i'\Theta) = h(Pr(r_i = 1 \,|\, Z_i))$, where both $g$ and $h$ are unknown functions, and $Pr(r_i = 1 \,|\, Z_i) = \Phi(Z_i'\Theta)$ is the propensity score. The fourth method follows a two-step propensity score stratification (PSS) procedure by slightly modifying the estimator proposed by Cosslett (1991). In the first step we estimate the selection model parametrically via probit (rather than nonparametrically as suggested by Cosslett). In the second step, we estimate our main equation (2) while approximating the selection correction, $g(\cdot)$, by $J$ indicator variables, $d_j = \{\mathrm{I}(Pr(r_i = 1 \,|\, Z_i) \in j)\}$, where $j = 1, ..., J$ is one of the $J$ intervals that partition the [0,1] support of the propensity score.[29] In our analysis, we use three alternative specifications of the PSS method. In one, we consider four dummy variables indicating whether the predicted propensity score is in the bottom quartile, or between the 25th percentile and the median, or between the median and the 75th percentile, or in the top quartile. In another, we consider ten dummy variables that partition the predicted propensity score distribution in deciles. In the last, we partition the [0,1] support of the propensity score in equally spaced intervals controlling for the balancing score property.[30]

Finally, our fifth method applies the propensity score weighting (PSW) procedure recently

[29] Although similar to ours, the procedure by Cosslett (1991) suggests a partition of the support of $Z'\Theta$ instead of the support $[0, 1]$ of the propensity score.

[30] This property requires that the distribution of the variables $Z$ does not differ between sons who are observed living with their fathers and sons who are not, given that the propensity score lies in each of the intervals which partition the [0-1] support. If the balancing score property is not satisfied for a specific interval, we split it into disjoint smaller subintervals, until the property is satisfied. See Rosembaum and Rubin (1983) for further details.

used in Robins and Rotnitzky (1995), Robins *et al.* (1995), and Abowd *et al.* (2001). This is again a two-step method with the first step requiring the usual estimation of the selection model via probit. In the second step, we estimate the main equation (2) using weighted least squares with weights given by the inverse propensity scores obtained from the first step.

A key issue in selection problems has to do with the way in which the selection process itself is believed to be generated. If the probability that $r_i = 1$ is independent on $\{y_i, x_i, Z_i\}$, then the data are *missing completely at random*, the missing data problem can be ignored, and we can estimate equation (2) using the subsample of father-child pairs with observations on both $y_i$ and $x_i$ (and other regressors). If instead $r_i$ is not independent of $y_i$, but, conditional on $y_i$ and $Z_i$, it is uncorrelated to $x_i$, the data are said to be *missing at random* (MAR), and consistent estimates of $\beta$ can be obtained from (2) using the subsample of individuals with complete observations and considering the missing covariate problem as if it were a missing dependent variable problem (and, thus, applying the five correction methods outlined above).

If $r_i$ is independent neither of $y_i$ nor of $x_i$, the data are *not missing at random* (NMAR), and consistent estimates of $\beta$ can be obtained only by inverting (2) into

$$x_i = a + by_i + A_i'c + \xi_i, \tag{5}$$

which allows us to consider the problem of the missing $x_i$ as a standard missing dependent variable problem and to apply again our five correction methods straightforwardly.[31] In the reverse-OLS regressions (5), $a$, $b$, and $c$ are parameters, and $\xi_i$ is an error term. Since our parameter of interest is $\beta$ in equation (2), this can be recovered from (5) through

$$\widehat{\beta} = \widehat{b}\Big(\frac{\sigma^2_{\tilde{y},\tilde{y}}}{\sigma^2_{\tilde{x},\tilde{x}}}\Big), \tag{6}$$

where $\sigma^2_{\tilde{k},\tilde{k}}$ is the variance of $\tilde{k}$, $\tilde{k} = \tilde{y}, \tilde{x}$ , and $\tilde{k}$ is the residual of the regression of $k$ on $A$. Now, $y_i$ and $A_i$ are observed for all father-son pairs, so the estimation of $\sigma_{\tilde{y},\tilde{y}}$ is unproblematic. However, $x_i$ is observed only for father-child pairs for whom both the adulthood and the coresidence conditions hold, the possibility of missing $x_i$'s must be accounted for in estimating $\sigma_{\tilde{x},\tilde{x}}$. For this purpose, the information contained in the Supplemental Sample is crucial. Specifically, for all fathers in that sample, we regress the log of their HG index on a constant,

[31] It is worthwhile noticing that we estimated one specification of the selection model (3)-(4) in relation to (2) and another specification in relation to (5). In the first, the $Z_i$ vector excludes both $y_i$ (obviously because it is the dependent variable) and $x_i$ (because we impose MAR). In the second specification, we exclude $x_i$ (since it is the dependent variable in (5)) but we include $y_i$.

their son's age and age squared, and use the residuals of this regression to compute $\sigma_{\tilde{x},\tilde{x}}$. Therefore, for each of the five methods described above (ML, TS, TSN, PSS, and PSW), we will present two sets of estimates of $\beta$, one set that assumes data missing at random and another set that assumes data not missing at random.

Table 2 summarises the assumptions imposed by each of the methods described in this section to obtain a consistent estimate of the intergenerational correlation parameter.[32] For example, assumption A1, which is needed for the OLS estimator of $\beta$ in (2) to be consistent, requires $y_i$ to be independent of $r_i$ conditional on $x_i$ and $A_i$. This means that the residuals in (2) must not depend on the selection process. However, such a dependency could be driven either by unobserved variables — in which case the ML, TS, TSN, and PSS methods and their corresponding assumptions will apply — or only by observed variables that are excluded from the main equation (2). In this latter case, assumption A5 becomes relevant.[33] Finally, assumption A3 identifies our notion of MAR, which naturally will be relaxed when we estimate our models under the hypothesis of NMAR data.

# 5 Results

## 5.1 The Extent of Selection Bias

Table 3 reports the estimated intergenerational elasticities using eight different combinations of the son's and father's status variables for the Full and Restricted Samples computed under the assumption that $y_i$ is independent of $r_i$ conditional on all the explanatory variables in (2). The estimates from the Full Sample range between 0.08 and 0.23, while those from the Restricted Sample range between 0.11 and 0.20.[34] The last column of Table 3 reveals that the gap between the two sets of estimates is statistically significant at conventional levels, except for the case in which $y_i$ is proxied by $HG_2^s$ and $x_i$ by $HG_1^f$ (second row).[35]

[32] The notation "$\perp\!\!\!\perp$" indicates statistical independence.

[33] A similar reasoning is valid when the reverse-regression model (5) is used.

[34] In spite of being smaller than the elasticities reported in many recent studies that use earnings or income as measures of status (Solon, 1999), our estimates are close to those shown in Atkinson *et al.* (1983) for Britain, when they use *net* family incomes rather than earnings as their variables of interest. They are also close to those reported in Blanden *et al.* (2004), where the log of children's earnings are regressed on the log of parental income, and to those reported in Ermisch and Francesconi (2004), who also use HG scores.

[35] Using a probit model in which $r_i$ is determined by $y_i$, $x_i$ and $A_i$, we can also check if assumption A1 holds by testing whether the coefficient on $y_i$ is significantly different from zero. Differently from the Chow test shown in Table 3, this test does not require linearity in the intergenerational mobility equation, but imposes a parametric probability model for the selection process. The results are reported in Appendix Table A.2

Taking the values from the Full Sample as benchmark estimates free of selection bias, we notice that the direction of the bias depends on whether we use $HG_3^f$ as a measure of father's status. As discussed in subsection 3.2, this allows for larger sample sizes but at the cost of replacing more than 50 percent of the missing observations on $x_i$ with the minimum observed HG score. When $HG_3^f$ is used, the elasticity estimates from the Restricted Sample are always greater than those from the Full Sample. This is consistent with the findings by Couch and Lillard (1998). The difference is small but still significant in one case (row 6 of Table 3), and very large, of the order of 46 percent, in the other case (row 5). However, this overestimation is likely to be the result of the imputation procedure used to construct $HG_3^f$. In the other six cases (i.e., when $HG_1^s$, $HG_2^s$, $HG_1^f$, $HG_2^f$, and $HG_4^f$ are used), instead, we always find that the Restricted Sample leads to an underestimation of the true intergenerational elasticity, which is in line with the results obtained in a different context by Minicozzi (2003). The magnitude of the bias varies from moderate (between 12 and 14 percent in the first two rows) to large (about 25 percent in rows 3-4 and 7-8). Both the direction of the bias and its extent therefore depend on how socioeconomic status is measured.[36] Biases of the order of 25 percent are arguably large. But even those found in the first two rows of Table 3 are sizeable, in the sense that they are likely to be consequential to mobility. This is because of our measure of socioeconomic status. In general, values of $\beta$ of 0.18 or 0.19 suggest patterns of intergenerational mobility that are relatively similar to those implied by values of about 0.22 or 0.23. The sons' (conditional) probability of staying in or moving to different points of the HG scores distribution varies by 1 or 2 percentage points as long as their father's prestige does not lie at the extremes of his distribution. However, if father's prestige is at the extremes (e.g., bottom and top deciles), the differences in probability are larger (of the order of 3-4 percentage points) and such differences may underpin substantially different occupations and

(panel (a)). In five out of eight cases assumption A1 is rejected (rows one, three, and five). This indicates that selection issues are relevant. Importantly, in only two cases (rows four and six), this test contradicts the results from the Chow test. Notice also that the normality assumption imposed by the probit specification is always rejected except when $HG_1^s$ and $HG_2^f$ and when $HG_1^s$ and $HG_4^f$ are used (see panel (h)).

[36]Irrespective of the sample (and especially in the Full Sample), the estimates obtained with the $HG_3^f$ measure are always the lowest. In line with the interpretation suggested by Minicozzi (2003), these could be seen as lower bounds of $\beta$. For sons, however, when we use $HG_2^s$ — which similarly to $HG_3^f$ replaces missing score values with the sample minimum — the evidence is mixed. In some cases, the intergenerational elasticity is smaller when it is computed with $HG_2^s$ than when it is computed with $HG_1^s$ (e.g., rows 1 and 2), in others it is greater (e.g., rows 3-4). This warrants some caution in interpreting the results obtained with $HG_2^s$ as worst-case lower bounds of $\beta$. Indeed, if the sons with missing values have fathers at the bottom of the distribution, the selection-corrected estimates using $HG_2^s$ could be greater than the true value of $\beta$.

earnings. For example, almost 90 percent of fathers in the top decile are managers (across all industrial sectors) and professionals (e.g., engineers, architects, university professors, medical doctors, solicitors and chartered accountants). This is true only for 58 percent of fathers in the ninth decile of the HG score distribution (which lies between the 80th and the 90th percentiles). Those occupational differences are reflected in substantial pay differentials. For example, the 2001 average monthly earnings is 2,100 for fathers in the ninth decile, while fathers in the top decile earn 2,500 per month, approximately 15 percent more. In sum, a downward bias of up to 25 percent in intergenerational elasticities based on occupational prestige measures is likely to provide us with a different picture of social mobility even if the "true" value of the elasticity is low.

## 5.2 Correcting for Sample Selection Bias

The results for the (probit) selection model are reported in Appendix Table A.1. We show three specifications. Specification [1] is relevant for equation (2), specifications [2] and [3] pertain to model (5) but use two different measures of $y_i$, $HG_1^s$ and $HG_2^s$ respectively. Our estimates confirm a number of previous results (e.g., Haurin *et al.*, 1994; Ermisch and Di Salvo, 1997). Older sons are less likely to be matched with their fathers, and so are young Black (Carribean or African) men (albeit this is not statistically significant). Men of Indian ethnic origin are instead more likely to be observed living with their fathers than otherwise identical White men. This is also the case for young men who regularly attend religious services (but only if they are non-Christians). Individuals who live in Greater London are more likely to coreside with their fathers in any of the panel years as compared to individuals in other parts of the country. Higher house prices significantly reduce the probability of observing father-son pairs, suggesting a lower probability of coresidence in the panel in times or areas with relatively higher house prices. This is in contrast with the findings reported in other studies (e.g., Ermisch, 1999), although such studies were typically interested in the household formation process rather than the coresidence of fathers and sons (indeed a number of sons who could not be matched with their fathers in our sample are found to live with their mothers).

### 5.2.1 MAR Data

We now turn to see how well the methods described in Section 4 reduce the sample selection bias that affects $\beta$ obtained from the Restricted Sample. Table 4 shows these results under the assumption of MAR data.[37] For expositional convenience, the first two columns of Table 4 report again the elasticities shown in Table 3 for all the possible combinations of $HG^s$ and $HG^f$. All correction methods seem to be unable to attenuate the bias, except for the PSW method.[38] Excluding the cases in which $HG_3^f$ is used (when the PSW procedure aggravates the upward bias in $\beta$ induced in the Restricted Sample), in all other cases this method performs very well. For example, in rows 3-4 (where the bias is 25 and 24 percent respectively), the PSW-corrected values of $\beta$ are 0.16 and 0.17 respectively, or 12 and 6 percent smaller than the corresponding true values in the Full Sample.

The PSW method is valid when assumptions A3 and A5 are satisfied (see Table 2). The tests reported in Appendix Table A.2 panel (c) reveal that, for all models that exclude $HG_3^f$ and one case with $HG_4^f$, assumption A3 cannot be rejected at conventional levels of statistical significance. Assumption A5 instead appears to hold in two cases only (rows two and four in panel (e)). Unsurprisingly, the fact that the correction methods other than PSW perform poorly is confirmed by the rejection of A4 regardless of the measures of socioeconomic status (panel (d)). Taken together, therefore, these results indicate that the selection of interest here is primarily driven by observables.

### 5.2.2 NMAR Data

The procedure we adopt to obtain consistent estimates of $\beta$ when the data are NMAR is: (i) to estimate the reverse regression (5); and (ii) to multiply the $b$ coefficient by the ratio in variances as shown in (6). The first two columns of Table 5 report the estimates of $b$ for the Full and Restricted Samples respectively. Typically, these estimates (in either sample)

[37]We conducted a number of tests on the significance of the inverse Mill's ratio in the TS method, the polynomial terms in the TSN procedure, and the correction dummy variables in the PSS estimation. In all cases, we cannot reject the hypothesis that the coefficients of such variables are zero. However, as the Chow tests reported in Table 3 reveal, these tests seem to be not powerful enough to detect significant differences in intergenerational elasticities.

[38]Here and in the subsequent correction exercises, the results on the PSS method refer only to the case in which the correction term is approximated by four dummy variables that partition the predicted propensity score distribution in quartiles. The results obtained when ten dummy variables (corresponding to deciles) are included as well as the results that control for the balancing score are not shown for convenience, but are similar to those reported. They can be obtained from the authors.

are greater than those found for $\beta$ in Table 3. This is especially evident in the case of the regressions that use $HG_3^f$ as the measure of father's status. Given the link between $b$ and $\beta$, such a result implies that the variability in status among fathers is greater than that among sons (see also the standard deviations of $HG^s$ and $HG^f$ in Table 1). This relationship is plausible if the variance in status increases over the life cycle (Grawe, 2003).[39] Comparing the estimates of the two samples in Table 5, we find that the Restricted Sample produces a greater $b$ in three cases out of eight (rows 2, 5, and 8). However, as revealed in the Chow tests reported in the third column of the table, the differences are significant only for the estimates in the rows 5 and 6, which use $HG_3^f$.[40]

The next two columns of Table 5 show the estimates of the variance in father's status, $\sigma^2_{\tilde{x},\tilde{x}}$, computed on the Full and Supplemental Samples, respectively.[41] We cannot reject the hypothesis that $\sigma^2_{\tilde{x},\tilde{x}}$ under the Full Sample is equal to $\sigma^2_{\tilde{x},\tilde{x}}$ under the Supplemental Sample for the first measure of father's status, $HG_1^f$. In the other cases, however, the two variances are significantly different at standard levels.

The intergenerational elasticities implied by the estimates of $b$ and $\sigma_{\tilde{x},\tilde{x}}$ according to expression (6) are in Table 6. The first column shows the values of $\beta$ found when $b$ and both $\sigma^2_{\tilde{x},\tilde{x}}$ and $\sigma^2_{\tilde{y},\tilde{y}}$ are from the Full Sample. This produces the same estimates of $\beta$ reported in Table 3, which — in what follows — are again taken as our new benchmark. The values in the second and third columns differ by the estimates of $b$ (which are computed on the Full and Restricted Samples, respectively), but use the same ratio of variances, namely the ratio between the variance in son's status computed on the Full Sample and the variance in father's status computed on the Supplemental Sample.

Using $HG_3^f$ yields large overestimates of $\beta$ (see rows 5 and 6 in Table 6). As discussed earlier, this is likely to be the result of our imputation of missing observations on $x_i$. For the other measures, the Restricted Sample underestimates the true value of $\beta$, with the bias ranging from moderate (about 10 percent in the first row) to large (about 34 and 41 percent

[39]This however is not the case when we use $HG_4^f$, whose dispersion is generally lower than that observed for the other paternal measures of occupational prestige.

[40]With the exception of rows two and eight, the test for the validity of assumption A2 leads to the same result (see Appendix Table A.2, panel (b)).

[41]Notice that the variances for the Full Sample are identical each time we use the same measure of father's status, while those for the Supplemental Sample never change because they are based on the same measure of status. We do not report the estimates of the variance for the Restricted Sample, because they will yield the same estimates of $\beta$ as those shown in Table 4. We also do not report the estimates of the variance in son's status, $\sigma^2_{\tilde{y},\tilde{y}}$, since $y_i$ and the other relevant variables in (5) are observed for all father-son pairs.

in rows 3 and 7, respectively), except for the case when $HG_2^s$ and $HG_1^f$ are used. This last case represents the only striking departure from the values reported in Table 3, and implies an upward bias of about 11 percent. Thus, $\beta$ is biased and in general likely to be underestimated, at least if one disregards our more problematic measure of father's status, $HG_3^f$.

Table 6 shows that the PSW method corrects for the bias in all cases except when $HG_2^s$ and $HG_1^f$ are used (row 2) and when $HG_3^f$ is used (rows 5-6). Interestingly, the cases in which the PSW method performs well are also the models for which assumption A7 is not rejected, though only marginally when $HG_2^s$ and $HG_4^f$ are used (see Appendix Table A2). The other methods perform less satisfactorily, perhaps because their underlying assumption A6 is always rejected. These findings broadly confirm those obtained above under the hypothesis of MAR data, and again stress the importance of selection on observables.

### 5.2.3 Measurement Error

As mentioned in the Introduction, a number of studies have emphasised the role played by measurement error in socioeconomic status in estimating intergenerational mobility. The unavailability of measures of permanent status generally leads to downward-biased elasticities (that is, greater mobility). Assuming a classical measurement error model,[42] one way to attenuate the bias has been to average over repeated observations on father's status (Solon, 1992; Zimmerman, 1992). It is straightforward to show that

$$\beta_{avg} = \beta\Big(\frac{\sigma^2_{\tilde{x},\tilde{x}}}{\sigma^2_{\tilde{x}_{avg},\tilde{x}_{avg}}}\Big), \tag{7}$$

where $\beta$ is the elasticity obtained from model (2), $\sigma^2_{\tilde{x},\tilde{x}}$ is the variance of the residual of the regression of $x_{it}$ on $A_{it}$ for all years $t$ in which fathers are observed, $\sigma^2_{\tilde{x}_{avg},\tilde{x}_{avg}}$ is the variance of the residual of the regression of time averages of $x$ on $A$, and $\beta_{avg}$ is the elasticity obtained from (2) when $x_i$ is replaced by its time average, $x_{i,avg}$. The problem is that in either the Full or Restricted Samples we do not have repeated observations on father's status. To compute the term at the denominator in parentheses in expression (7), therefore, we have to resort to the "external" information contained in the Supplemental Sample. In particular,

[42]This assumes that the measurement errors for sons and fathers are mutually uncorrelated and also uncorrelated with the permanent component of status (see Fuller, 1987).

our measure of interest is the average of the father's HG scores across all waves in which he is observed, $\overline{HG}^f_{SS}$ (see subsection 3.2).

The results that adjust $\beta$ to account for measurement error according to (7) are reported in Table 7. The figures in rows 5 and 6 of the first column (Full Sample) show elasticities that are 2.5 times higher than the estimates of the Full Sample in Table 3. This difference underlines that the $HG^f_3$ measure is likely to be contaminated by substantial measurement error. On the other hand, the first two rows show only marginally greater estimates and the third and fourth rows only marginally smaller estimates than those reported in Table 3. When $HG^f_4$ is used, the elasticities are instead about 37 percent smaller (rows 7 and 8). In this case, this result suggests that the variance of fathers' prestige could be underestimated. In general, however, the HG score measures used here seem to be good proxies of permanent economic status. Apart from such differences, the findings emerged earlier are still valid. We highlight two of such findings. First, using the Restricted Sample leads to underestimate the true intergenerational elasticity (with biases ranging between 12 and 25 percent), except when $HG^f_3$ is used (in which cases we obtain upward-biased estimates). Second, all correction methods perform poorly, in the sense that they are unable to attenuate the selection bias, apart from the PSW procedure for the cases in which we detect downward biases. In some instances, the PSW-corrected estimates reduce the bias by an order of 4 (from 25 percent to 6 percent, see the fourth row in Table 7).

## 5.3 The Effect of Changing the Length of the Panel

The BHPS at present is a short panel to study intergenerational mobility issues as compared to other available longitudinal data sources (e.g., the Panel Study of Income Dynamics and the National Longitudinal Survey in the United States, or the National Child Development Study in Britain). There are however even shorter panels of data for Britain as well as other countries, some of which have been discontinued and thus will never reach a long enough time series component (e.g., the European Household Panel Study that has been collected only eight times). In what follows we present a sensitivity check of our analysis when the length of the sample is restricted to a period shorter than eleven waves. In Table 8 we report the $\beta$ estimates computed using three new samples, namely, the subsamples of sons who coreside with their fathers in at least one wave during the first eight, six, and four waves of the BHPS. For each subsample we compute the measures of occupational prestige using only the information available in the fictitiously shorter panel period (that is, the first eight, six, and four waves, respectively).

Two comments are in order. First, limiting the analysis to a smaller number of waves leads to smaller sample sizes, and this seems to decrease the estimation precision quite substantially. While in the restricted sample based on eight and eleven waves all $\beta$ estimates are significantly different from zero at both 1- and 5-percent significance levels, in the subsamples based on six and four waves there are a few cases in which we cannot reject the assumption of a zero intergenerational elasticity. Second, almost invariably the intergenerational elasticity declines as the length of observation (and analysis) shrinks. In most cases this decline is considerable. This has substantial effects on the underestimation of $\beta$. In fact, while the downward bias ranged from 12 to 25 percent when we used all eleven waves, it ranges from 18 to 52 percent when eight waves are used, and from 27 to almost 80 percent when four waves are used.[43] With very short panels, therefore, the statistical inference on the intergenerational elasticity estimates is problematic and the extent of selection bias greatly exacerbated.

# 6 Conclusion

Using data from the first eleven waves of the BHPS, this paper measures the extent of the selection bias induced by adulthood and coresidence conditions — bias that is expected to be severe in short panels — on measures of intergenerational mobility in occupational prestige. We try to limit the impact of other selection biases, such as those induced by labour market restrictions that are typically imposed in intergenerational mobility studies, by using different measures of socioeconomic status that account for missing labour market information.

We stress four main results. First, there is evidence of an underestimation of the true intergenerational elasticity, although some more noisy measures of (father's) status provide support for upward-biased estimates. The extent of the downward bias is moderate in some cases (of the order of 10-12 percent) and large in others (of the order of 25 percent).

The consequences in terms of intergenerational mobility of such biases are noticeable especially at the extremes of the occupational prestige distribution. Second, the proposed

[43]When only four waves are used, even the analysis with $HG_3^f$ produces a downward bias (of the order of 12 percent) rather than an upward bias as we typically found earlier.

methods used to correct for the selection bias seem to be unable to attenuate it, except for the propensity score weighting procedure, which performs well in most circumstances. In some cases, the PSW-corrected estimates of the intergenerational elasticity reduce the bias by an order of 4 (from 25 percent to 6 percent). This result is confirmed both under the assumption of missing-at-random data as well as under the assumption of not-missing-at-random data. Third, the two previous sets of results (direction and extent of the bias, and differential abilities to correct for it) emerge also when we account for measurement error. Fourth, restricting the sample to a period shorter than the eleven waves under analysis exacerbates the extent of sample selection bias. In the cases when the analysis is limited to four waves, this bias ranges from 27 to 80 percent.

In considering these results, some extensions should be considered for future research. It is possible that the selection process analysed here is stronger in some parts of the occupational prestige distribution than in others. If the strength of the selection process is greater where the status distribution is generally more rigid, namely at its extremes (Corak and Heisz, 1999; Ermisch and Francesconi, 2004), then the magnitude of the selection bias may be greater than what we gauged here. In addition, it would be profitable to consider other correction methods to attenuate the bias (e.g., Manski bounds on quantile regressions). This would provide us with additional tools to address the selection issue and would also allow us to see how generalisable our conclusions are on this matter. Another extension is to account for other selection processes (for instance, the selection into employment) more formally. This would help to reach a fuller characterisation of the extent of the selection bias and identify its different sources more precisely than what we have done in this paper. It would also give us greater credibility to analyse the same selection problems for women. Finally, while the size of the Restricted Sample compares favourable with other studies, a richer data source or a longer panel of data would allow us to check the robustness of our findings and increase the range of hypotheses that could be tested.

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Table 1: Summary statistics by sample

| | Full Sample | | Restricted Sample | | Supplemental Sample | |
|---|---|---|---|---|---|---|
| Variables | Mean (S.D.) | $N$ | Mean (S.D.) | $N$ | Mean (S.D.) | $N$ |
| Hope-Golthorpe scores (sons) | | | | | | |
| $HG_1^s$ | 42.99 (13.33) | 2117 | 40.00 (11.63) | 969 | | |
| $HG_2^s$ | 40.92 (14.47) | 2311 | 38.88 (12.30) | 1022 | | |
| Hope-Golthorpe score (fathers) | | | | | | |
| $HG_1^f$ | 49.63 (15.70) | 1152 | 47.57 (15.14) | 397 | | |
| $HG_2^f$ | 49.91 (15.24) | 1753 | 49.29 (14.72) | 998 | | |
| $HG_3^f$ | 38.62 (19.74) | 2691 | 45.99 (16.98) | 1114 | | |
| $HG_4^f$ | 49.36 (12.32) | 2691 | 49.19 (13.93) | 1114 | | |
| $HG_{SS}^f$ | | | | | 47.80 (15.86) | 1427 |
| $\overline{HG}_{SS}^f$ | | | | | 47.68 (15.01) | 1430 |
| Other characteristics | | | | | | |
| Father's age | | | 48.21 (7.54) | 1062 | 46.93 (8.17) | 1434 |
| House price index (log) | 11.08 (0.26) | 2691 | 11.05 (0.32) | 1114 | | |
| Son's characteristics | | | | | | |
| Age | 23.15 (4.51) | 2691 | 21.30 | 1114 | 19.76 (6.64) | 1434 |
| Year of birth | 1974 (5.32) | 2691 | 1976 (5.20) | 1114 | 1976 (5.80) | 1434 |
| Ethnic origin: | | | | | | |
| White (base) | 0.94 | 2691 | 0.94 | 1114 | | |
| Black | 0.02 | 2691 | 0.01 | 1114 | | |
| Indian | 0.02 | 2691 | 0.02 | 1114 | | |
| Pakistani/Bangladeshi | 0.01 | 2691 | 0.02 | 1114 | | |
| Other | 0.01 | 2691 | 0.01 | 1114 | | |
| Religious attendance: | | | | | | |
| Protestant | 0.12 | 2691 | 0.11 | 1114 | | |
| Catholic | 0.06 | 2691 | 0.05 | 1114 | | |
| Other denomination | 0.02 | 2691 | 0.03 | 1114 | | |
| Region of sons' residence: | | | | | | |
| Greater London (base) | 0.105 | 2691 | 0.996 | 1114 | | |
| Rest of South East | 0.186 | 2691 | 0.193 | 1114 | | |
| South West | 0.086 | 2691 | 0.084 | 1114 | | |
| Anglia and Midlands | 0.237 | 2691 | 0.234 | 1114 | | |
| North West | 0.101 | 2691 | 0.111 | 1114 | | |
| Rest of North | 0.163 | 2691 | 0.170 | 1114 | | |
| Wales | 0.044 | 2691 | 0.048 | 1114 | | |
| Scotland | 0.077 | 2691 | 0.060 | 1114 | | |

*Note* : $N$ indicates the number of sons in the Full and Restricted Samples, and the number of fathers in the Supplemental Sample.

Table 2: Assumptions imposed by different estimators

| Label | Assumption | Estimator | Equation |
|---|---|---|---|
| A1 | $(r \perp\!\!\!\perp y \mid x, A)$ | OLS | (2) |
| A2 | $(r \perp\!\!\!\perp x \mid y, A)$ | OLS | (5) |
| A3 | $(r \perp\!\!\!\perp x \mid y, Z)$ | ML, TS, PSS, TSN, PSW | (2) |
| A4 | $(y \perp\!\!\!\perp z \mid x, A)$ | ML, TS, PSS, TSN | (2) |
| A5 | $(r \perp\!\!\!\perp y \mid x, A, Z)$ | PSW | (2) |
| A6 | $(x \perp\!\!\!\perp z \mid y, A)$ | ML, TS, PSS, TSN | (5) |
| A7 | $(r \perp\!\!\!\perp x \mid y, A, Z)$ | PSW | (5) |

*Note* : OLS=ordinary least squares method; ML=maximum likelihood estimator; TS=parametric two-step estimation procedure; TSN=two-step procedure with nonnormality test; PSS=propensity score stratification procedure; PSW=propensity score weighting procedure.

Table 3: Estimated $\beta$ for different measures of status by sample

| Measures of status | Full Sample | $N$ | Restricted Sample | $N$ | Chow Test |
|---|---|---|---|---|---|
| 1. $HG_1^s$ and $HG_1^f$ | 0.225 (0.025) | 1035 | 0.199 (0.039) | 377 | 3.040 [0.017] |
| 2. $HG_2^s$ and $HG_1^f$ | 0.216 (0.029) | 1092 | 0.185 (0.042) | 388 | 2.185 [0.069] |
| 3. $HG_1^s$ and $HG_2^f$ | 0.183 (0.021) | 1533 | 0.137 (0.028) | 875 | 4.386 [0.002] |
| 4. $HG_2^s$ and $HG_2^f$ | 0.185 (0.024) | 1621 | 0.140 (0.031) | 917 | 4.829 [0.000] |
| 5. $HG_1^s$ and $HG_3^f$ | 0.076 (0.012) | 2117 | 0.111 (0.020) | 969 | 7.367 [0.000] |
| 6. $HG_2^s$ and $HG_3^f$ | 0.128 (0.013) | 2311 | 0.130 (0.022) | 1022 | 2.447 [0.044] |
| 7. $HG_1^s$ and $HG_4^f$ | 0.181 (0.022) | 2117 | 0.134 (0.028) | 969 | 4.780 [0.000] |
| 8. $HG_2^s$ and $HG_4^f$ | 0.177 (0.026) | 2311 | 0.135 (0.031) | 1022 | 6.730 [0.000] |

*Note* : $N$ denotes the number of cases (sons and fathers) used in estimation. Standard errors are reported in parentheses. In the last two columns we report the values of the Chow Tests with their corresponding $p$-values in square brackets. The null hypothesis of the Chow Test is that the intergenerational elasticity (net of age and age squared) estimated with the Full Sample is equal to the corresponding intergenerational elasticity estimated with the Restricted Sample.

Table 4: Correcting for sample selection bias under MAR

| | Sample type | | Correction methods | | | | |
|---|---|---|---|---|---|---|---|
| | Full | Restricted | ML | TS | TSN | PSS | PSW |
| 1. $HG_1^s$ and $HG_1^f$ | 0.225 | 0.199 | 0.199 | 0.199 | 0.201 | 0.200 | 0.207 |
| | (0.025) | (0.039) | (0.039) | (0.039) | (0.039) | (0.040) | (0.060) |
| 2. $HG_2^s$ and $HG_1^f$ | 0.216 | 0.185 | 0.185 | 0.185 | 0.187 | 0.182 | 0.203 |
| | (0.029) | (0.042) | (0.042) | (0.042) | (0.042) | (0.042) | (0.064) |
| 3. $HG_1^s$ and $HG_2^f$ | 0.183 | 0.137 | 0.138 | 0.137 | 0.137 | 0.137 | 0.161 |
| | (0.021) | (0.028) | (0.028) | (0.028) | (0.028) | (0.028) | (0.035) |
| 4. $HG_2^s$ and $HG_2^f$ | 0.185 | 0.140 | 0.140 | 0.140 | 0.140 | 0.140 | 0.174 |
| | (0.024) | (0.031) | (0.031) | (0.031) | (0.031) | (0.031) | (0.042) |
| 5. $HG_1^s$ and $HG_3^f$ | 0.076 | 0.111 | 0.110 | 0.111 | 0.109 | 0.111 | 0.125 |
| | (0.012) | (0.020) | (0.020) | (0.020) | (0.021) | (0.021) | (0.025) |
| 6. $HG_2^s$ and $HG_3^f$ | 0.128 | 0.130 | 0.129 | 0.129 | 0.128 | 0.129 | 0.144 |
| | (0.013) | (0.022) | (0.022) | (0.022) | (0.022) | (0.022) | (0.030) |
| 7. $HG_1^s$ and $HG_4^f$ | 0.181 | 0.134 | 0.134 | 0.134 | 0.134 | 0.134 | 0.146 |
| | (0.022) | (0.028) | (0.028) | (0.028) | (0.028) | (0.028) | (0.038) |
| 8. $HG_2^s$ and $HG_4^f$ | 0.177 | 0.135 | 0.136 | 0.136 | 0.136 | 0.136 | 0.151 |
| | (0.026) | (0.031) | (0.031) | (0.031) | (0.031) | (0.031) | (0.046) |

*Note* : Standard errors are reported in parentheses.

Table 5: Estimated $b$ and $\sigma^2_{\tilde{x},\tilde{x}}$ for measures of status by sample

| | $b$ | | | $\sigma^2_{\tilde{x},\tilde{x}}$ | | |
|---|---|---|---|---|---|---|
| | Full Sample | Restricted Sample | Chow Test | Full Sample | Supplemental Sample | Equality Test |
| 1. $HG_1^s$ and $HG_1^f$ | 0.331 | 0.324 | 0.834 | 0.118 | 0.137 | 0.906 |
| | (0.036) | (0.064) | [0.504] | (0.004) | (0.005) | [0.086] |
| 2. $HG_2^s$ and $HG_1^f$ | 0.231 | 0.258 | 1.915 | 0.118 | 0.137 | 0.906 |
| | (0.031) | (0.059) | [0.106] | (0.004) | (0.005) | [0.086] |
| 3. $HG_1^s$ and $HG_2^f$ | 0.257 | 0.192 | 1.848 | 0.110 | 0.137 | 0.845 |
| | (0.030) | (0.040) | [0.117] | (0.004) | (0.005) | [0.001] |
| 4. $HG_2^s$ and $HG_2^f$ | 0.193 | 0.160 | 1.601 | 0.110 | 0.137 | 0.845 |
| | (0.025) | (0.035) | [0.171] | (0.004) | (0.005) | [0.001] |
| 5. $HG_1^s$ and $HG_3^f$ | 0.253 | 0.267 | 111.1 | 0.295 | 0.137 | 0.445 |
| | (0.039) | (0.049) | [0.000] | (0.004) | (0.005) | [0.000] |
| 6. $HG_2^s$ and $HG_3^f$ | 0.293 | 0.252 | 119.3 | 0.295 | 0.137 | 0.445 |
| | (0.031) | (0.043) | [0.000] | (0.004) | (0.005) | [0.000] |
| 7. $HG_1^s$ and $HG_4^f$ | 0.176 | 0.169 | 0.860 | 0.072 | 0.137 | 0.551 |
| | (0.021) | (0.036) | [0.490] | (0.002) | (0.005) | [0.000] |
| 8. $HG_2^s$ and $HG_4^f$ | 0.112 | 0.135 | 2.260 | 0.072 | 0.137 | 0.551 |
| | (0.016) | (0.031) | [0.060] | (0.002) | (0.005) | [0.000] |

*Note* : Standard errors are reported in parentheses. Standard errors for $\sigma^2_{\tilde{x},\tilde{x}}$ are computed using bootstrapping methods with 1,000 replications. The $p$-values of the Chow Test and Equality Test are in square brackets. The null hypothesis of the Chow Test is that the coefficient $b$ (net of age and age squared) estimated with the Full Sample is equal to the corresponding coefficient estimated with the Restricted Sample. The null hypothesis of the Equality Test is that the variance of $\tilde{x}$ computed with Full Sample is equal to the corresponding variance computed with the Supplemental Sample.

Table 6: Correcting for sample selection bias under NMAR

| | Sample type | | | Correction methods[c] | | | | |
|---|---|---|---|---|---|---|---|---|
| | Full/ Full[a] | Full/ Suppl.[b] | Restricted/ Suppl.[c] | ML | TS | TSN | PSS | PSW |
| 1. $HG_1^s$ and $HG_1^f$ | 0.225 | 0.207 | 0.203 | 0.210 | 0.209 | 0.214 | 0.217 | 0.226 |
| | (0.025) | (0.023) | (0.040) | (0.042) | (0.042) | (0.041) | (0.043) | (0.063) |
| 2. $HG_2^s$ and $HG_1^f$ | 0.216 | 0.215 | 0.240 | 0.246 | 0.245 | 0.244 | 0.253 | 0.296 |
| | (0.029) | (0.029) | (0.055) | (0.056) | (0.055) | (0.055) | (0.056) | (0.091) |
| 3. $HG_1^s$ and $HG_2^f$ | 0.183 | 0.161 | 0.120 | 0.124 | 0.124 | 0.121 | 0.125 | 0.157 |
| | (0.021) | (0.019) | (0.025) | (0.027) | (0.026) | (0.026) | (0.027) | (0.032) |
| 4. $HG_2^s$ and $HG_2^f$ | 0.185 | 0.180 | 0.149 | 0.154 | 0.154 | 0.153 | 0.154 | 0.198 |
| | (0.024) | (0.023) | (0.033) | (0.033) | (0.033) | (0.033) | (0.033) | (0.046) |
| 5. $HG_1^s$ and $HG_3^f$ | 0.076 | 0.158 | 0.167 | 0.153 | 0.154 | 0.145 | 0.152 | 0.195 |
| | (0.012) | (0.024) | (0.031) | (0.031) | (0.032) | (0.032) | (0.032) | (0.036) |
| 6. $HG_2^s$ and $HG_3^f$ | 0.128 | 0.273 | 0.234 | 0.234 | 0.234 | 0.232 | 0.231 | 0.261 |
| | (0.013) | (0.029) | (0.040) | (0.040) | (0.040) | (0.040) | (0.040) | (0.053) |
| 7. $HG_1^s$ and $HG_4^f$ | 0.181 | 0.110 | 0.106 | 0.108 | 0.108 | 0.104 | 0.109 | 0.133 |
| | (0.022) | (0.013) | (0.023) | (0.023) | (0.023) | (0.023) | (0.023) | (0.031) |
| 8. $HG_2^s$ and $HG_4^f$ | 0.177 | 0.104 | 0.126 | 0.126 | 0.126 | 0.126 | 0.126 | 0.155 |
| | (0.026) | (0.015) | (0.029) | (0.029) | (0.029) | (0.029) | (0.029) | (0.047) |

*Note* : Standard errors are reported in parentheses. They are computed under the assumption that $\left(\frac{\sigma^2_{\tilde{y},\tilde{y}}}{\sigma^2_{\tilde{x},\tilde{x}}}\right)$ is constant.

[a] Full Sample is used to estimate both $b$ and $\sigma^2_{\tilde{x},\tilde{x}}$.

[b] Full Sample is used to estimate $b$, and Supplemental Sample is used to estimate $\sigma^2_{\tilde{x},\tilde{x}}$.

[c] Restricted Sample is used to estimate $b$, and Supplemental Sample is used to estimate $\sigma^2_{\tilde{x},\tilde{x}}$.

Table 7: Correcting for sample selection bias under MAR in presence of measurement error

| | Sample type | | Correction methods | | | | |
|---|---|---|---|---|---|---|---|
| | Full | Restricted | ML | TS | TSN | PSS | PSW |
| 1. $HG_1^s$ and $HG_1^f$ | 0.232 | 0.205 | 0.205 | 0.205 | 0.207 | 0.206 | 0.213 |
| | (0.026) | (0.040) | (0.040) | (0.040) | (0.040) | (0.041) | (0.062) |
| 2. $HG_2^s$ and $HG_1^f$ | 0.222 | 0.191 | 0.191 | 0.191 | 0.193 | 0.187 | 0.209 |
| | (0.030) | (0.043) | (0.043) | (0.043) | (0.043) | (0.043) | (0.066) |
| 3. $HG_1^s$ and $HG_2^f$ | 0.177 | 0.133 | 0.133 | 0.133 | 0.133 | 0.133 | 0.156 |
| | (0.020) | (0.027) | (0.027) | (0.027) | (0.027) | (0.027) | (0.034) |
| 4. $HG_2^s$ and $HG_2^f$ | 0.179 | 0.135 | 0.135 | 0.135 | 0.135 | 0.135 | 0.168 |
| | (0.023) | (0.030) | (0.030) | (0.030) | (0.030) | (0.030) | (0.041) |
| 5. $HG_1^s$ and $HG_3^f$ | 0.196 | 0.287 | 0.284 | 0.287 | 0.282 | 0.287 | 0.323 |
| | (0.031) | (0.052) | (0.052) | (0.052) | (0.054) | (0.054) | (0.065) |
| 6. $HG_2^s$ and $HG_3^f$ | 0.331 | 0.336 | 0.333 | 0.333 | 0.331 | 0.333 | 0.372 |
| | (0.034) | (0.057) | (0.057) | (0.057) | (0.057) | (0.057) | (0.078) |
| 7. $HG_1^s$ and $HG_4^f$ | 0.114 | 0.085 | 0.085 | 0.085 | 0.085 | 0.085 | 0.092 |
| | (0.014) | (0.018) | (0.018) | (0.018) | (0.018) | (0.018) | (0.024) |
| 8. $HG_2^s$ and $HG_4^f$ | 0.112 | 0.085 | 0.086 | 0.086 | 0.086 | 0.086 | 0.095 |
| | (0.016) | (0.020) | (0.020) | (0.020) | (0.020) | (0.020) | (0.029) |

*Note* : Standard errors are reported in parentheses. They are computed under the assumption that $\left(\frac{\sigma^2_{\tilde{x},\tilde{x}}}{\sigma^2_{\tilde{x}_{avg},\tilde{x}_{avg}}}\right)$ is constant.

Table 8: Intergenerational elasticities using panels of different length

| | Sample type (11 waves)[a] | | Restricted Samples with: | | |
|---|---|---|---|---|---|
| | Full | Restricted | 8 waves | 6 waves | 4 waves |
| 1. $HG_1^s$ and $HG_1^f$ | 0.225 | 0.199 | 0.158 | 0.112 | 0.091 |
| | | | (0.000) | (0.015) | (0.066) |
| | | | *298* | *282* | *270* |
| 2. $HG_2^s$ and $HG_1^f$ | 0.216 | 0.185 | 0.177 | 0.145 | 0.158 |
| | | | (0.000) | (0.005) | (0.004) |
| | | | *308* | *297* | *287* |
| 3. $HG_1^s$ and $HG_2^f$ | 0.183 | 0.137 | 0.090 | 0.060 | 0.039 |
| | | | (0.009) | (0.096) | (0.345) |
| | | | *612* | *509* | *418* |
| 4. $HG_2^s$ and $HG_2^f$ | 0.185 | 0.140 | 0.104 | 0.097 | 0.078 |
| | | | (0.005) | (0.015) | (0.094) |
| | | | *646* | *542* | *445* |
| 5. $HG_1^s$ and $HG_3^f$ | 0.076 | 0.111 | 0.094 | 0.097 | 0.067 |
| | | | (0.000) | (0.000) | (0.032) |
| | | | *670* | *560* | *458* |
| 6. $HG_2^s$ and $HG_3^f$ | 0.128 | 0.130 | 0.121 | 0.140 | 0.113 |
| | | | (0.000) | (0.000) | (0.001) |
| | | | *713* | *602* | *492* |
| 7. $HG_1^s$ and $HG_4^f$ | 0.181 | 0.134 | 0.087 | 0.055 | 0.037 |
| | | | (0.011) | (0.128) | (0.378) |
| | | | *670* | *560* | *458* |
| 8. $HG_2^s$ and $HG_4^f$ | 0.177 | 0.135 | 0.100 | 0.092 | 0.075 |
| | | | (0.007) | (0.025) | (0.114) |
| | | | *713* | *602* | *492* |

*Note* : Standard errors are reported in parentheses. Figures in italics denote the number of observations.
[a] For simplicity standard errors and number of observations are not reported. They can be found in Table 3.

# A Appendix

## A.1 Selection equation

| Variable | Model 1 Coeff. | Model 1 (S.E.) | Model 2 Coeff. | Model 2 (S.E.) | Model 3 Coeff. | Model 3 (S.E.) |
|---|---|---|---|---|---|---|
| Year of birth: | | | | | | |
| 1966-70 | 0.925 | (0.238) | 1.042 | (0.242) | 1.006 | (0.235) |
| 1971-75 | 1.694 | (0.238) | 1.757 | (0.242) | 1.773 | (0.235) |
| 1981-85 | 1.629 | (0.330) | 1.529 | (0.334) | 1.750 | (0.321) |
| House price (log) | -2.174 | (0.290) | -2.216 | (0.295) | -2.265 | (0.291) |
| Region of residence: | | | | | | |
| Rest of South East | -0.400 | (0.170) | -0.471 | (0.173) | -0.411 | (0.170) |
| South West | -0.693 | (0.215) | -0.833 | (0.221) | -0.784 | (0.216) |
| Anglia and Midlands | -1.161 | (0.224) | -1.298 | (0.230) | -1.282 | (0.226) |
| North West | -1.100 | (0.247) | -1.245 | (0.253) | -1.199 | (0.249) |
| Rest of North | -0.958 | (0.237) | -1.107 | (0.243) | -1.081 | (0.239) |
| Wales | -1.270 | (0.297) | -1.435 | (0.303) | -1.367 | (0.298) |
| Scotland | -1.285 | (0.251) | -1.406 | (0.257) | -1.377 | (0.251) |
| Ethnic origin: | | | | | | |
| Black | -0.476 | (0.412) | -0.568 | (0.414) | -0.555 | (0.399) |
| Indian | 1.155 | (0.394) | 1.139 | (0.387) | 1.198 | (0.390) |
| Pakistani/Bangladeshi | 0.301 | (0.432) | 0.283 | (0.440) | 0.497 | (0.408) |
| Other | -0.426 | (0.460) | -0.378 | (0.475) | -0.353 | (0.420) |
| Religious attendance: | | | | | | |
| Protestant | -0.194 | (0.135) | -0.217 | (0.136) | -0.206 | (0.132) |
| Catholique | -0.272 | (0.187) | -0.248 | (0.189) | -0.214 | (0.186) |
| Other denomination | 1.131 | (0.356) | 1.191 | (0.364) | 1.233 | (0.353) |
| log $HG_1^s$ | | | -0.789 | (0.155) | | |
| log $HG_2^s$ | | | | | -0.412 | (0.128) |
| Constant | 23.355 | (3.343) | 26.829 | (3.479) | 25.878 | (3.413) |
| $N$ | 1035 | | 1035 | | 1092 | |
| $\chi^2$ [$p$-value] | 226.5 [0.000] | | 252.7 [0.000] | | 259.0 [0.000] | |
| Pseudo $R^2$ | 0.167 | | 0.186 | | 0.182 | |

*Note* : Estimates are obtained from probit models. $N$ is the number of sons.

## A.2 Tests on the assumptions imposed by different estimators and normality

| | (a) Assumption A1 | (b) Assumption A2 | (c) Assumption A3 | (d) Assumption A4 |
|---|---|---|---|---|
| $HG_1^s$ and $HG_1^f$ | -2.36 [0.02] | -1.90 [0.06] | -0.97 [0.33] | 2.15 [0.00] |
| $HG_2^s$ and $HG_1^f$ | 0.39 [0.69] | -2.71 [0.01] | -1.48 [0.14] | 2.19 [0.00] |
| $HG_1^s$ and $HG_2^f$ | -2.60 [0.01] | -0.63 [0.53] | 0.07 [0.94] | 1.98 [0.01] |
| $HG_2^s$ and $HG_2^f$ | -0.03 [0.97] | -1.39 [0.17] | -0.64 [0.52] | 1.96 [0.01] |
| $HG_1^s$ and $HG_3^f$ | -5.13 [0.00] | 16.50 [0.00] | 15.56 [0.00] | 3.52 [0.00] |
| $HG_2^s$ and $HG_3^f$ | -1.39 [0.17] | 17.23 [0.00] | 16.28 [0.00] | 3.44 [0.00] |
| $HG_1^s$ and $HG_4^f$ | -2.05 [0.04] | -1.61 [0.11] | -3.66 [0.00] | 3.17 [0.00] |
| $HG_2^s$ and $HG_4^f$ | 2.43 [0.02] | -2.55 [0.01] | 0.29 [0.77] | 3.59 [0.00] |
| | (e) Assumption A5 | (f) Assumption A6 | (g) Assumption A7 | (h) Normality |
| $HG_1^s$ and $HG_1^f$ | -2.96 [0.00] | 1.68 [0.04] | -1.48 [0.14] | 16.02 [0.00] |
| $HG_2^s$ and $HG_1^f$ | -0.41 [0.68] | 1.95 [0.01] | -2.14 [0.03] | 13.47 [0.00] |
| $HG_1^s$ and $HG_2^f$ | -2.76 [0.01] | 2.15 [0.00] | -0.15 [0.88] | 5.51 [0.06] |
| $HG_2^s$ and $HG_2^f$ | -0.25 [0.80] | 2.36 [0.00] | -0.89 [0.37] | 7.13 [0.03] |
| $HG_1^s$ and $HG_3^f$ | -4.97 [0.00] | 2.56 [0.00] | 16.69 [0.00] | 14.76 [0.00] |
| $HG_2^s$ and $HG_3^f$ | -1.29 [0.20] | 2.83 [0.00] | 17.30 [0.00] | 28.15 [0.00] |
| $HG_1^s$ and $HG_4^f$ | -1.97 [0.05] | 1.92 [0.01] | -1.18 [0.24] | 4.40 [0.11] |
| $HG_2^s$ and $HG_4^f$ | 2.32 [0.02] | 2.05 [0.01] | -2.20 [0.03] | 7.43 [0.02] |

*Note* : $p$-values of the tests are in square brackets. For the definition of each assumption and the estimator they apply to, see Table 2. The test on normality (panel (h)) applies to a selection model in which $r_i$ is determined by $y_i$, $x_i$, and $A_i$. If the probit selection model includes $Z$ among its explanatory variables, normality is never rejected.