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Moving Towards Estimating Lifetime Intergenerational Economic Mobility in the UK

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Abstract

Estimates of intergenerational economic mobility that use point in time measures of income and earnings suffer from lifecycle and attenuation bias. We consider these issues for the National Child Development Study (NCDS) and British Cohort Study (BCS) for the first time, highlighting how common methods used to deal with these biases do not eradicate these issues. To attempt to overcome this, we offer the first estimates of lifetime intergenerational economic mobility for the UK. In doing so, we discuss a third potential bias, regularly ignored in the literature, driven by spells out of work. When all three biases are considered, our best estimate of lifetime intergenerational economic persistence in the UK is 0.43, significantly higher than previously thought. We discuss why there is good reason to believe that this is still a lower bound.

JEL classification: I20, J62, J24

Keywords: Intergenerational mobility, measurement, income inequality

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1. Introduction

Over the last decade or so there has been a major resurgence in research exploring the extent of intergenerational persistence in inequalities. In the UK in particular, intergenerational economic mobility, commonly described as social mobility in the media and by the government, has become a focus of extensive policy debates and government initiatives. This renewed focus on social mobility has been strongly influenced by emerging research findings, with evidence suggesting that the level of mobility in the UK is low by international standards (Corak, 2013, Blanden, 2013). International comparisons of mobility has also spawned what Alan Krueger, former Chair of the Council of Economic Advisers at the White House, has dubbed the "Great Gatsby Curve" which documents the pervasive correlation between income inequality within a generation and the intergenerational persistence in inequality. Looking across time, Blanden et. al. (2004, 2005) found that income mobility levels had fallen in the UK, using two British cohorts, the National Child Development Survey (NCDS) birth cohort of 1958 and the British Cohort Study (BCS) birth cohort of 1970[4]. Black and Deveraux (2011) and Jännti and Jenkins (2013) provide a recent review of this and related literatures internationally.

Whilst the literature on intergenerational mobility began by focusing on the association between fathers' lifetime earnings and sons' lifetime earnings, recent studies have moved towards a family-based focus concerning the association between parental resources in childhood and sons' (and indeed daughters') adult economic outcomes (see Jäntti and Jenkins for a full discussion, 2013). In this setting, ideally the degree of intergenerational mobility within a country would be measured as the association between the socio-economic status (SES) of parents throughout a person's childhood and their lifetime earnings as an adult. As such it represents the extent to which adult outcomes mirror childhood circumstances and is an indicator of the persistence of inequality across generations. This is extremely data intensive as it requires a person's entire childhood and working life to be observed. The literature on intergenerational economic mobility therefore approximates lifetime intergenerational

[4] Social class mobility has remained constant over time (Erikson and Goldthorpe, 2010). Differences across income and class measures of mobility are likely driven by an increase in within-class inequality (Blanden et. al., 2013).

mobility with point in time measures. These then are used as proxy indicators for family resources throughout childhood and lifetime earnings.

Within this developing literature two substantive biases have been highlighted that have been shown to have significant impacts on the estimation of intergenerational persistence when using point in time proxy measures: attenuation bias and life cycle bias. Solon (1992) and Zimmerman (1992) noted the existence of attenuation bias in estimates of intergenerational mobility driven by measurement error and transitory variation in incomes measured at a point in time in the parents' generation. The common approach to address this bias is to average over repeat measures in the parents' generation, moving towards a measure of childhood income (see for example Mazumder, 2005). The second approach has been to undertake a two stage process where current income is regressed on parental characteristics, such as education and occupation, which are predictors of longer-term income variation across families. This is the approach used by Dearden et al. (1997) for the UK and has similarities with the two sample two stage approach when family income or fathers earnings are unobserved but characteristics such as education and occupation are observed (Nicoletti and Ermish, 2007, Jerrim et. al., 2014).

Jenkins (1987) drew attention to the issue of lifecycle bias based on the generalised errors in variables model by exploring the relationship between point in time and lifetime earnings. Haider and Solon (2006), Grawe (2006) and Böhlmark and Lindquist (2006) explore the bias comparing current earnings with lifetime earnings although they do not go as far as directly estimating lifetime intergenerational persistence. These studies report that intergenerational elasticities (IGEs) can be consistently estimated by using current earnings at a specific age where current earnings closely predicts lifetime earnings. The specific age is shown by Böhlmark and Lindquist to not be stable across gender, cohorts or countries[5]. More recently, Nyborn and Stuhler (2011) show the validity of this approach rests on an assumption that does not hold in Swedish data.

We use the rich birth cohort studies available in the UK to consider these two biases for the first time in this context. Dearden, Machin and Reed (1997) are the only UK study to previously consider the likely impact of attenuation bias on estimates of intergenerational persistence. Given the timing of the study they were restricted to

[5] Bohlmark and Lindquist (2006) estimate that this bias is zero at age 36 for Sweden and 38 for the US.

using the problematic two-stage approach rather than the more commonly used averaging across income measures. We present the first estimates using this less problematic way of dealing with measurement error for the first time. No studies have yet explicitly considered the likely role of lifecycle bias on estimates of intergenerational economic mobility in the UK, beyond using measures based on data from ages where the bias is likely to be low based on international studies.

The implication of these existing biases, which are likely to be variable across populations and time, is that estimates of IGEs based on point in time measures as proxies for lifetime measures are likely to understate current estimates of mobility. Moving to estimating the IGE using actual lifetime measures of earnings is important to understand the full extent of persistence in inequalities across generations. We are the first to move towards estimating lifetime intergenerational economic mobility in the UK. More widely, only Nybom and Stuhler (2011) using Swedish administrative tax record data consider lifetime measures in both generations. Dahl and DeLeire (2008) and Mazumder (2005) and Chetty et al. (2014) for the US and Gregg et al. (2013) for Swedish data explore lifetime earnings/income measures for the parent generation only.

Moving towards lifetime estimates of sons' earnings enables us to consider an important issue for the first time in the intergenerational literature: the impact of spells out of work. Previous estimates based on point in time measures have excluded those who have zero earnings at the time of observation. Those who are out of work are therefore excluded from many of the estimates of IGEs so well known in the literature. Yet when considering lifetime earnings, periods out of work clearly matter. Since the mid-1970s employment rates of working age men in the UK, at cyclical peaks, have fallen from around 95% to 80% meaning that for recent cohorts, periods of non-employment will be materially important to lifetime earnings. Those who experience substantial periods out of work are, perhaps unsurprisingly, disproportionately drawn from those with poorer family backgrounds. We show that the exclusion of workless individuals from point in time measures of earnings creates a small bias due to sample selection. More importantly, we also show that including periods out of work in a measure of lifetime earnings highlights a materially important third source of bias when estimating IGEs using point in time proxy measures.

When taking the three measurement issues combined, we find that raw estimates of intergenerational economic mobility understate persistence across generations with an order of magnitude of 25 percentage points based on sons' earnings reported in their early 30s (as in Blanden et al. 2004). The extent of intergenerational economic mobility in the UK has been substantially understated to date. In the next section we lay out our modelling approach in more detail and in section 3 we discuss our data. Section 4 presents our results before we end with some brief conclusions.

2. Methodology

The ideal estimate of the intergenerational elasticity (IGE) as a measure of persistence would measure the relationship between the log of lifetime earnings of an individual in adulthood ($y_i^{son^*}$) and the log of earnings of the father or income of the parents of the individual throughout childhood ($y_i^{parent^*}$) as shown in (1).

$$y_i^{son^*} = \alpha + \beta y_i^{parent^*} + u_i \tag{1}$$

In an OLS regression, the estimated coefficient $\hat{\beta}$ therefore gives the IGE or the association between parental resources during childhood and the individual's lifetime adult earnings.

Conceptually the joint distribution of parents' and children's incomes can be separated into two components: (1) the joint distribution of parents' and children's ranks, formally known as the copula of the distribution, and (2) the marginal distributions of parent's and children's incomes (Chetty et al. 2014). The marginal distributions reflect the degree of inequality within each generation, typically measured by the Gini coefficient. The standard IGE combines the marginal and joint distributions, capturing both the extent of re-ranking across generations (whether the children who came from rich families are still the richest in adulthood) and the spread of the income distributions between the rich and the poor (inequality). If the income distributions are represented by a ladder, re-ranking describes people switching rungs on the ladder and inequality describes how far apart the rungs of the ladder are. A number of alternative measures of mobility including rank-rank coefficients and the quintile transition matrices present estimates of mobility based purely on the re-ranking of individuals across generations, ignoring the inequality component.

We estimate rank-rank coefficients as in (2) to contrast estimates of the extent of re-ranking across generations to the combined IGE where inequality across income distributions is also considered. We can therefore assess the extent to which our measures are driven by the ordering of individuals within the joint distribution or the extent of inequality within the marginal distributions.

$$(Rank\ y_i^{son^*}) = \alpha + \beta'(Rank\ y_i^{parent^*}) + u_i \tag{2}$$

Combining the evidence on both measures therefore offers useful information about what is driving changes in mobility by comparing the extent of re-sorting that occurs across generations and the extent to which inequalities are passed across generations.

Point-in-time estimates of intergenerational economic mobility

Due to data limitations, much of the previous literature in the UK and elsewhere has estimated intergenerational mobility based on measures of parental income in childhood and adult earnings observed at one point in time. Therefore instead of observing the desired measure of parental income across childhood ($y_i^{parent^*}$) or sons' lifetime earnings ($y_i^{son^*}$) we observe point in time proxies for these (y_{it}^{parent}, y_{it}^{son}) which deviate from the lifetime concepts through an error term which captures both reporting errors and short-term transitory fluctuations.

$$y_{it}^{parent} = y_i^{parent^*} + \epsilon_{it} \tag{3}$$

$$y_{it}^{son} = y_i^{son^*} + \varepsilon_{it} \tag{4}$$

Lifecycle bias

A substantive measurement issue, highlighted by Jenkins (1987), is that there is considerable heterogeneity in earnings trajectories over the lifecycle which vary by family background. Haider and Solon (2006), Grawe (2006) and Böhlmark and Lindquist (2006) show that if earnings are measured too early in the lifecycle, current earnings will understate true lifetime earnings. This will therefore lead to us understating the true IGE. Focusing on the sons earnings for notational simplicity (although lifecycle bias affects both measures) a measure of son's earnings at a point in time varies from the lifetime earnings across the lifecycle by some parameter, λ_t

$$y_{it}^{son} = \lambda_t y_i^{son^*} + \varepsilon_{it} \tag{5}$$

Assuming no error in the parental income variable, we estimate

$$y_{it}^{son} = \alpha + \beta\, y_i^{parent\ ^*} + e_{it} \tag{6}$$

Our estimate $\hat{\beta}$ therefore varies from the true β as:

$$plim\hat{\beta} = \frac{Cov(\beta y_i^{p^*} + e_{it}, y_i^{p^*})}{Var\,(y_i^{p^*})} \text{ and } e_{it} = \lambda_t\left(\beta y_i^{p^*} + u_i\right) + \varepsilon_{it} - \beta y_i^{p^*} \text{so}$$

$$plim\hat{\beta} = \lambda_t\beta + \mathrm{Corr}(y_i^{p^*}, \varepsilon_{it})\, \sigma_{\varepsilon_{it}} / \sigma y_i^{p^*} \tag{7}$$

When $\lambda_t = 1$, $\hat{\beta}$ provides a consistent estimate of β provided that Corr($y_i^{p^*}$, ε_{it}) = 0, which is assumed in the aforementioned studies.

An important point to note here is that λ_t is a population estimate that is related to the shape of age-earnings profiles which are likely to vary across country, cohort and time (Böhlmark and Lindquist, 2006). Further, Nybom and Stuhler (2011) use Swedish tax record data to show that the assumption Corr($y_i^{p^*}$, ε_{it}) = 0 does not hold and even small deviations renders the estimated $\hat{\beta}$ inconsistent at the point where $\lambda_t = 1$.

Due to these issues with proxying lifetime earnings with a point in time measure we take the approach of estimating intergenerational persistence directly at various points across the lifecycle to show how the estimated $\hat{\beta}$ evolves. This provides direct evidence on the shape of the relationship as individuals' age for two cohorts of data in the UK for the first time.

Measurement error

The second substantive measurement issue discussed in previous research is that at any point in time family income measure is likely to be measured with error and includes unobserved transitory shocks as shown in equation (3) (see Solon, 1992, Zimmerman, 1992). In this setting, assuming no error in the sons' earnings measure we therefore estimate

$$y_i^{son^*} = \alpha + \beta y_{it}^{parent} + e_{it} \tag{8}$$

Assuming this measurement error is classical as is typical in this literature[6], our estimate $\hat{\beta}$ therefore varies from the true β as:

$$plim\hat{\beta} = \frac{Cov(y_{it}^{p}, \beta y_{it}^{p} + e_{it} - \beta \epsilon_{it})}{Var\,(y_i^{p^*})} \text{ so}$$

$$plim\hat{\beta} = \beta \frac{\sigma_{y^p}^2}{\sigma_{y^p}^2 + \sigma_\epsilon^2} \tag{9}$$

The OLS estimate therefore gives a lower bound estimate of the true IGE. Solon (1992) introduced the idea of using average income across a number of observations to minimise, although not eradicate, the attenuation bias caused by classical measurement error. Mazumder (2005) illustrates that the higher the number of income observations available, the more likely that the measurement error is eradicated. Gregg et. al. (2013) explore this using Swedish administrative data and find that estimates using fewer income observations can deliver up to 80% of the true IGE if the income observations are measured a number of years apart, hence breaking the serial correlation in error across adjacent income observations. Using a more recent cohort of data with two observations of family income measured six years apart, we can apply this method of reducing attenuation bias to UK data for the first time, avoiding the issues with the two stage method used previously and discussed in detail in Dearden, Machin and Reed (1997).

Both measurement issues have two aspects that can be conceptually separated and developed analytically. Measurement error and lifecycle bias will reflect both positional inaccuracy and scale mis-measurement. Using our description of income distributions as a ladder, positional inaccuracy relates to people being placed on the wrong rungs on the ladder and scale mis-measurement relates to wrongly measuring how far apart the rungs of the ladder are. Taking lifecycle bias as an example, if we observe earnings before a person has realised the full returns to their education, this can lead to placing them lower in the distribution than will occur some years later when their earnings have matured: positional inaccuracy. In addition the scale of

[6] See Blanden et. al. (2013) for a discussion of non-classical measurement error in this context.

earnings gaps between the less and better educated will be understated: scale mis-measurement. The same applies to measurement error or transitory income shocks.

The alternative estimation approach we adopt utilising rank based estimation removes the issue of scale mis-measurement (inequality) from the picture and just leaves the positional accuracy concern. By comparing the IGE regression coefficients to the rank-rank coefficients throughout our analysis we can therefore comment on the relative effects of scale measurement and positional accuracy from both types of bias. A priori we would expect the two biases discussed here to be smaller in magnitude in the rank based measure, especially lifecycle bias which we expect to be driven by the earnings gaps between high and low educated individuals rather than the rank ordering.

Lifetime intergenerational economic mobility

Given the issues discussed with point in time measures of incomes, a central contribution of this paper is to attempt to estimate lifetime intergenerational economic mobility in the UK for the first time. We therefore estimate as close to equation (1) as we have ever been in the UK, estimating the association between the log of lifetime earnings in the second generation and the log of parental resources in childhood by taking an average across earnings measures across individuals' lifetimes (details in section 3 below). In doing so, we highlight a major restriction of previous research: the inability to capture mobility trends for individuals that are workless[7].

Previous literature using the UK birth cohorts finds a causal impact of youth unemployment spells on wages and employment twenty years later (Gregg, 2001 and Gregg and Tominey, 2005). Macmillan (2014) illustrates that workless spells are not random across generations – individuals who experience spells out of work are more likely to come from workless (and therefore more disadvantaged) backgrounds. Previous estimates of the IGE based on point in time earnings are likely to be further understating intergenerational persistence then by excluding those who we do not observe earnings for at a point in time because they are out of work due to this third, often unmentioned, workless bias.

[7] Indeed, estimates of life-cycle bias based on current vs. lifetime earnings in the existing literature also exclude individuals who have zero earnings in any given year (although we note that individuals who are out of work for part of the year will be included in the US and Scandinavian analyses as annual earnings is widely used).

When measuring lifetime earnings, we therefore consider the implications of including periods out of work as genuine income shocks to the cohort member. A methodological issue is what to assign those who are workless as a replacement value for their earnings during period out of work. We compare and contrast three alternative methods here: zero earnings, welfare benefits as earnings replacement and wages foregone.

Through the IGE we are trying to estimate differences in lifetime opportunities. Here then employment and earnings are important in their own right, not just as sources of income. There is therefore value in showing both lifetime earnings including zero earnings (observed employment shocks to earnings) and lifetime earnings where benefits replace earnings (a resource-based measure). Measuring spells out of work as zero earnings represents the true earnings value received by those who are out of work. Yet this may not be a true representation of the individual's available resources and significantly increases inequality in the earnings distribution. It is therefore likely to overstate the true impact of worklessness on lifetime earnings for the IGE as this is sensitive to inequality.

The second method, earnings replacement, imputes the average benefit level available at the time of the workless spell. This is our preferred measure as this is more representative of available resources and mirrors the measure of resources (family income) used in the first generation. This may of course overstate family resources if not everyone who is out of work claims benefits and understate family resources if those who are out of work are claiming a more generous benefit such as disability allowance.

The third method, wages foregone, represents the earnings that we might expect the individual to earn if they were in employment. This is estimated using a selection model which predicts labour market participation based on self-reported health and previous labour market attachment (see Data Appendix). This method is likely to understate the true impact of worklessness on lifetime earnings or incomes as this measures the lifetime potential earnings if the person was always in work rather than actual realised outcomes.

3. Data

We use the two mature British birth cohort studies: the National Child Development Study (NCDS) born in 1958 and the British Cohort Study (BCS) born in 1970. The NCDS obtained data at birth and ages 7, 11, 16, 23, 33, 42, 46 and 50 for children born in Great Britain in a week in March 1958. The BCS originally included all those born in Great Britain in a week in April 1970. Information was obtained about the sample members and their families at birth and at ages 5, 10, 16, 26, 30, 34, 38 and 42. Both cohorts began with around 18,000 children.

Parental income

For the purpose of our study, we need to observe the resources of parents and sons across generations. We focus on sons in this paper, consistent with the vast majority of existing literature, to avoid issues of female labour market participation. We do note that as we move to focus on lifetime earnings including spells out of the labour market, this methodology clearly takes us towards a future study of intergenerational mobility for females. Parental income data is available at age 16 in both of the birth cohort studies. In the NCDS the data is banded for net mother's earnings, net father's earnings and net other income, with an average of the midpoints of all three categories used as a final broadly continuous measure. In the BCS, parental income before taxes and deductions is derived from banded data. We generate a continuous income variable by fitting a Singh-Maddala distribution (1976) to the eleven bands of data using maximum likelihood estimation. This is particularly helpful in allocating an expected value for those in the open top category. A transformation is implemented to the bands from gross to net using information from the Family Expenditure Survey (FES) of 1986 for comparability with the NCDS measure and a child benefit level is imputed based on the observed number of children in the household at age 16. These measures have been used on a number of occasions and a great deal of work has been done already to test their robustness and comparability (see Appendix B, Blanden, Gregg and Macmillan, 2011).

A repeat of income data for another period is not available in the NCDS but is available at age 10 in the BCS and so averaged income from two periods can be constructed for this cohort (a log of the average is taken). As at 16, a continuous measure of family income is derived using a Singh-Maddala distribution on the banded data (seven bands in total). Income at 10 is transformed from gross to net using the Family Expenditure Survey (FES) of 1980 and a child benefit level is imputed based on the number of children in the household. Income is deflated to 2000 prices for each measure. If income is missing in one period it is imputed based on income in the other period and differences in the social class, employment status, housing tenure and family composition across the two periods (see Data Appendix for further details). In our final sample we observe at least one income observation for all individuals and two income observations for 47% of our final sample.

Sons' earnings

In the second generation, comparable earnings information for the cohort members is available in the NCDS at age 23, 33, 42, 46 and 50 and in the BCS at age 26, 30, 34, 38 and 42. Questions were asked on the individuals' gross pay and the length of their pay period and comparable monthly measures were calculated from this information. We can therefore observe monthly earnings for the NCDS cohort at various points in time almost across their entire working lives (average age 38). For the BCS cohort we can observe monthly earnings at various points in time across two thirds of their working lives (average age 34). Earnings are deflated to 2000 prices for each observation and the log of this is taken for our measures of earnings at each point in the lifecycle that the cohort members are observed.

To measure lifetime earnings an average is taken across all observed earnings periods and then a log of the average is used as our measure of lifetime earnings. If earnings are missing in any period due to attrition we impute earnings using the approach outlined in Wooldridge (1995) and Semykina and Wooldridge (2010). This panel imputation method predicts earnings based on their earnings in other periods and the observed education level of the cohort member, interacted with time to account for lifecycle bias (see Data Appendix). This has very little impact on our estimates of lifetime intergenerational economic mobility and allows us to increase our sample. Dichotomous imputation variables are included for each observed earnings period to indicate whether the information is observed or imputed.

Given the differential spacing of the earnings observations in the NCDS[8] we impute a linear trajectory for each month between earnings observations in both cohorts before taking an average across all months, essentially creating a weighted

[8] (23-33 = 10 year apart, 33-42 = 9 years apart, 42-46, 46-50= 4 years apart).

average of observed lifetime earnings. We consider three measures of lifetime earnings: the most complete measure of lifetime earnings available in our data from age 23-50 in the NCDS and two comparable measures in the NCDS and BCS from age 26-42.

To account for those without earnings due to periods out of work information from monthly work history data, available in the NCDS and BCS from 16-50 and 16-42 respectively, is combined with our measure of monthly earnings. If the individual is observed as workless in any given month, their earnings for that month are replaced with a workless value. As discussed in section 2, three alternative values are assigned to those who are observed out of work in any given month: zero, earnings replacement or wages foregone. Earnings replacement is calculated based on the average level of job seekers allowance, income support and incapacity benefits received by cohort members at 42 and 46 in the NCDS and 30 and 34 in the BCS. This is adjusted for inflation and assigned whether the individual claimed any benefit or not[9]. Wages foregone are estimated using a selection model where self-reported health at the time that the cohort member is observed as workless and the previous four years of labour market participation are used to model selection (further details in the Data Appendix).

Sample restrictions

For the point in time estimates of intergenerational economic mobility, considering issues of lifecycle and attenuation bias, the sample is restricted as in previous studies to all sons with earnings who are employed but not self-employed, with parental income reported at age 16. When we consider measurement error in the BCS, this restriction is relaxed to those with at least one parental income observation at 10 or 16. The implications of observing either compared to one period of parental income in the BCS are considered in the next section.

Various sample restrictions are explored in the results for estimates of lifetime intergenerational economic mobility. An individual must have at least one income observation in childhood and be observed in the monthly work history data for at least five years to be included in our analysis. If individuals are workless for less than two years, or out of work for two years but in employment for the majority of time

[9] Our imputed values are very close to the average income replacement rates reported by the Institute for Fiscal Studies (IFS).

observed, they must have at least one earnings observation to be included in the sample. If individuals are workless for over two years and are out of work for the majority of time observed (proportion of time workless > 60%) they are not required to have any earnings observations. This last group of individuals are not included in the analysis until the final stage when we consider those who are always workless.

These sample constraints restrict the available sample to 3452 in the NCDS and 4312 in the BCS. The representativeness of our final sample is discussed in detail in the Data Appendix.

4. Results

Point in time estimates of intergenerational economic mobility across the lifecycle

We start by exploring trends in lifecycle bias in the UK for the first time by documenting the profile of point in time estimates of the IGE as sons' age. Table 1 presents the IGE estimates from OLS regressions of log earnings at various points across the lifecycle of sons on the log of parental income at age 16 in the NCDS and BCS. The estimates at 33 in the NCDS and 30 in the BCS replicate those found in Blanden et. al. (2004) which suggested that mobility across time has declined in the UK or alternatively the IGE, the persistence of inequality across generations, has increased over time. The lower panel of the table also reports coefficients from the rank of earnings regressed on the rank of family income, removing any differences in variation (inequality) between the two measures.

Focusing on the NCDS who we observe up until age 50 currently, the IGE starts very low at age 23, at 0.042, rising rapidly to 0.205 by age 33, then increasing to 0.291 by age 42 before declining again to 0.259 at age 46 and 0.224 at age 50 (as illustrated in Figure 1). In the BCS (lower panel Table 1 and middle line on Figure 1) we can see a similar pattern emerging across the life-cycle with estimated intergenerational persistence increasing from 0.203 at age 26 to 0.291 at 30, 0.324 at 34, 0.385 at 38 and 0.395 at 42. Note that at any given age the estimated persistence in the BCS cohort is significantly different from that in the NCDS at the 5% level (higher persistence, lower mobility). At the two most comparable pairs of ages, 33 and 42 in the NCDS and 34 and 42 in the BCS, the intergenerational elasticity is 0.119 and 0.106 percentage points higher in the later cohort.

The shape of the trajectories of the IGE supports the idea that their evolution is driven by the realisation of returns to education, which will be socially graded, as the shape broadly mirrors the relationship between current and lifetime earnings for the US, presented in Böhlmark and Lindquist (2006). Figure 1 illustrates that the rate of increasing persistence across the period is very similar in the NCDS and BCS suggesting that the age-earnings trajectories are similar across cohorts in the UK, unlike the pattern found in Sweden (Böhlmark and Lindquist, 2006, Gregg et. al., 2013). If the UK is closer to Sweden (US) in terms of the relationship between current and lifetime earnings, the estimated IGE at age 36 (38) is likely to be a close approximation of lifetime mobility while estimated IGE at age 42 will overstate this.

When considering the estimated rank-rank coefficients, which remove scale measurement issues, these show a similar, although less pronounced, pattern across the lifecycle. Figure 2 plots these trajectories for both the NCDS and BCS. In both cohorts, the rank based coefficient rises sharply to age 30/33 and then rises only gradually, peaking at 42. This suggests that lifecycle bias is driven mainly by scale mis-measurement (the mis-measurement of earnings gaps between better and less well educated individuals) rather than positional accuracy concerns.

We next move on to consider the impact of attenuation bias on our estimates of intergenerational mobility in the UK, presenting results taking an average across incomes in the later BCS cohort for the first time. Table 2 presents estimates using average parental income at 10 and 16 rather than income at 16 in the BCS, to minimise the impact of attenuation bias driven by measurement error and transitory shocks to incomes. Income is only observed at one point in time in the NCDS and therefore comparable estimates cannot be computed for this cohort. There are two issues to consider when estimating across a longer window of parental incomes: the impact of averaging income for those who we observe income and earnings for in Table 1 and the impact of adding additional individuals who do not report an income at 16 (the measure used for comparability with the NCDS) but who we do observe information for at age 10.

Panel A of Table 2 estimates intergenerational persistence for the same samples as in Table 1 (those who we observe income for at 16) averaging across the two periods if income is available at age 10 and imputing an income at age 10 if not (12% of parents of cohort members with reported income at 16 do not at 10, see Data Appendix for further discussion). The estimated intergenerational elasticity increases across all ages but the magnitude increases with age by 0.02 percentage points at age 26, 0.04 at age 30, 0.07 at age 34, 0.09 at age 38 and 0.11 at age 42. The attenuation bias is increasing across the lifecycle. Note that the rank-rank coefficients are very similar to those seen in Panel B of Table 1 indicating that any issues of measurement error and transitory shocks in the measure of parental income at 16 are causing scale mis-measurement issues rather than positional inaccuracy within the distribution of income.

Panel B of Table 2 introduces additional sample members for whom parental resources are observed at age 10 but not at age 16. The introduction of these additional sample members increases the sample size considerably (41% of our final sample report income at 10 but not at 16) but changes the estimated IGEs and rank coefficients very little. The estimated IGE moves by, at most, 2 percentage points across the five ages that sons' earnings are observed in the BCS. Increasing the sample to include individuals who do not report income at 16 is therefore not biasing the estimates of intergenerational persistence in any consistent way. The lifecycle movements in these estimates are also shown as the upper line in Figure 1 and Figure 2 for comparison. As seen in Panel A, the rank-rank coefficients are very close to those observed when using a point in time measure of parental income at age 16.

Note that by averaging across two periods we are not fully dealing with issues of attenuation bias. Gregg et. al. (2013) used Swedish data to measure the likely attenuation bias left in estimated IGEs when averaging across two observations, six year apart, compared to averaging across the entire childhood of the son. They found that the estimates in Table 2 were likely to represent around 80% of the total estimated intergenerational persistence if parental income were observed in every year across childhood. If this also holds for the UK, where measurement error from survey data might be thought to be larger than in Swedish administrative data, then these estimates likely still understate the true IGE by at least 20% (or 0.1 at age 38).

Lifetime intergenerational economic mobility

To minimise the impact of both lifecycle and attenuation bias on our estimated IGEs, we move towards lifetime measures of both parental income and sons' earnings for the first time in the UK, using average earnings across the lifecycle for sons and where possible, average incomes in childhood for parents. For the remainder of the analysis we consider four samples: the most complete measure of lifetime intergenerational economic mobility based on earnings at 23-50 in the NCDS and parental income at 16, two comparable samples based on earnings at 26-42 and parental income at 16 in the NCDS and BCS and a sample which minimises attenuation bias based on earnings at 26-42 and average parental income at 10 and 16 in the BCS.

As we move to consider lifetime earnings we must deal with an issue largely ignored in the literature to date: individuals experiencing workless spells during their lifetime. Up until now previous studies that use point in time estimates of intergenerational persistence only present IGEs for those who are in work, effectively ignoring the sub-group of the population who are out of work at the time of measurement. However spells out of work among sons will not be randomly allocated across the parental income distribution.

Table 3 shows the distribution of workless spells in our data and how this varies by family income in childhood and average lifetime earnings in adulthood. As can be seen from Panel A, across all four samples the majority of individuals in our data are always employed (60% over the window 23-50 in the NCDS and 70-90% over the shorter window 26-42) although this varies across the lifecycle with more workless spells at the beginning and end of the periods as illustrated by the difference between samples 1 and 2 from the NCDS (this is consistent with lifecycle bias in workless experiences illustrated in Macmillan, 2014). A minor proportion of the sample (4-15%) experiences extended periods of worklessness (greater than two years) over their lifetime and a very small proportion (1-3%) are never in work.

Panels B and C summarise the average family incomes in childhood and average lifetime labour market earnings of those who always work compared to those experiencing varying degrees of worklessness. Those who always work are from families with higher parental income in childhood than those who experience workless spells and these individuals also earn more on average in the labour market across their lifetime. An individual who is never out of work in the NCDS is from a family with £329 income a week on average and earns £542 per week on average in adulthood from 23-50. If we compare this to an individual who is out of work for over 2 years from 23-50, their family income is £296 per week and they earn £348 per week on average when in work. In the BCS individuals who are never in work from 26-42 are from families that have incomes at 16 that are 30% lower than individuals who always work. Patterns of lifetime earnings are similar in terms of workless experience across the two cohorts.

Given that workless experiences are not random in terms of family background or later labour market outcomes, we might expect any estimated IGE to vary based on the sample of individuals that we consider. As noted, previous estimates of IGEs typically only focus on those who are in work. We begin by presenting estimates of lifetime intergenerational economic mobility in the UK for a sample of individuals who are always in work before introducing those who spent spells of time out of work over the observed period. We are not adjusting earnings for periods out of work, at this stage, but rather looking at issues of sample selection by workless experiences for those who report earnings in other periods. These individuals have earnings at least once across the ages observed in the respective cohorts but will be missing from various point in time estimates of intergenerational mobility if they are out of work at those specific ages. For now, those who are always out of work are excluded from the analysis.

The top row of Panel A, Table 4 shows the first estimates of lifetime intergenerational economic mobility based on lifetime earnings for the UK. In the NCDS this estimate is 0.18 in both the longer and shorter windows. If the pattern is repeated in the BCS cohort we can assume that our BCS estimate of 0.30 is getting close to a lifetime estimate. Addressing attenuation bias by using averaged family income in the BCS raises the IGE to 0.37.

Introducing individuals with less than two years of workless spells over the period observed increases intergenerational persistence by around 1 percentage point in the NCDS but makes very little difference in the BCS. Including those who are out of work for over two years increases the intergenerational elasticity by a further 1-2.5 percentage points. Therefore overall, restricting the sample of individuals for whom we estimate intergenerational mobility for in previous point in time estimates to those who are in always in work attenuates our estimated IGE by around 0.01-0.03.

Panel B of Table 4 presents the estimated rank-rank coefficients. These follow a similar pattern to that seen in the estimated IGEs but of smaller magnitudes. Here the addition of those who spent spells of time out of work attenuates the rank-rank coefficient by 0.01 in the NCDS and has little effect in the BCS. In line with the general pattern in results shown so far, any bias from restricting the sample to only those in employment is driven by scale mis-measurement rather than re-ranking of individuals in each generation.

This analysis does not yet include periods of worklessness in the measure of lifetime earnings used and therefore in the estimated IGE. Table 5 moves to including periods of worklessness in our measures of average lifetime earnings. As discussed in sections 2 and 3, this can be done in a number of ways. We present estimated IGEs (Panel A) and rank-rank coefficients (Panel B) for three alternative measures of worklessness: zero earnings, earnings replacement from imputed benefits and wages foregone through spells out of work.

The first row in each panel of Table 5 replicates the final rows of Panel A and B in Table 4, showing the IGEs and rank-rank coefficients for the whole sample when periods of worklessness are ignored. Including workless spells in our lifetime earnings measure, first treating periods out of work as zero earnings, increases our IGE estimate by 0.04. If we alternatively use the value of earnings replacement benefits, our preferred measure, this increases the IGE by 0.02.

If we use the wages foregone our IGE estimates are very similar to those when we ignore spells out of work. As discussed in our methodology section, there is reason to believe that this will be a lower bound estimate as it treats those who actually experience spells out of work as if they were in work, albeit recognising their selection in terms of a lower wage rate. Spells out of work have wider reaching consequences for opportunities than just lowering the individuals' potential wage.

Our estimated IGE is therefore further attenuated by ignoring these spells out of work in our measure of average lifetime earnings. Depending on which measures of earnings replacement are preferred, the IGE estimates are attenuated by a range of 0 to a further 0.04. The rank-rank coefficients (Panel B) are essentially unchanged regardless of which value we choose to use for workless spells. This suggests that the bias from the exclusion of spells out of work is driven entirely by mis-measurement in the scale of earnings inequality.

Finally, we introduce to our analysis those individuals who are out of work for the entire period that they are observed. These individuals are from considerably more disadvantaged families than those who are always in work as seen from Table 3 and have no actual earnings in adulthood. Table 6 replicates Table 5 including the additional individuals who are always workless in the analysis using our three alternative measures. The value used to assign spells out of work is particularly important with the inclusion of these individuals. If we use zero earnings, the IGE rises by 70% to 0.36 in the NCDS (both samples) and 0.52 in the BCS based on income at 16 only and 0.65 when income is averaged over ages 10 and 16. This is driven by the fact that the standard deviation of sons earnings more than doubles using this measure. If we instead use earnings replacement benefits, the increase is more modest with estimates of the elasticity rising by around 20% compared to estimates where we ignore periods of worklessness. As seen in Table 5, imputing a foregone wage with adjustment for non-participation looks broadly similar to treating people as always working.

As in Table 5, the rank-rank coefficients remain unchanged across all measures of worklessness, indicating that observed changes in the IGE are again driven by the scale of the earnings measures rather than any re-ranking (these individuals are at the bottom of any distribution of income and earnings).

Focusing on our preferred measure of earnings replacement from imputed benefits, the estimated IGE taking into account all three potential biases is 0.43. There is reason to believe that this is a lower bound estimate however. As discussed, our approach for adjusting for attenuation bias, averaging of family income at ages 10 and 16, is only partial. Based on Swedish data, where full family income in childhood is observable this estimate would be around 80% of the true IGE. So adjusting for this would place the IGE for the UK at around 0.54: 54% of lifetime inequality being passed across generations. This is also based on an approximation of lifetime earnings captured over ages 26 through to 42. In the earlier generation widening this window to 23 through to 50 makes almost no difference but of course this might change in the later generation as they age. More importantly, these estimates do not yet account for differences in male labour market participation as they approach retirement age.

5. Conclusions and discussion

This paper has made three significant contributions to the current literature on intergenerational economic mobility for the UK. First, we have explicitly considered the role of lifecycle bias and attenuation bias for the first time in relation to point in time estimates of mobility. Second, and most significantly, we have estimated lifetime intergenerational economic mobility in the UK for the first time, highlighting an additional bias driven by those who experience spells out of work to be considered in this context, which is the third substantive contribution.

Our results suggest that our previous estimates of intergenerational economic mobility in the UK are likely to have understated the true extent of the problem. Lifecycle bias is shown to have led to an understatement of the IGE by around 0.05-0.07 in the measures produced Blanden et. al. (2004) and attenuation bias, due to measurement error and transitory shocks, leads to an additional understatement of the IGE by 0.08-0.10. The exclusion of workless individuals and accounting for spells out of work in measures of lifetime earnings has led to a further understating of the IGE by around 0.05.

While our final estimates suggest that lifetime intergenerational economic mobility is currently around 0.43 in the BCS, there are two reasons to believe that this estimate still understates true levels of intergenerational persistence. First, taking an average of incomes across only two periods, albeit six years apart hence minimising any auto-correlation, will not completely eradicate attenuation bias. When we account for the likely size of this bias the most realistic figure for intergenerational persistence in economic inequality in the BCS cohort is around 0.54: 54% of inequalities in parental incomes are transmitted across generations in the UK. Second, both cohorts are unable to yet inform us on patterns of labour market exit as individuals enter retirement age. Future NCDS and BCS data releases will provide a more complete picture of labour market participation in later years.

Moving towards lifetime measures of intergenerational economic mobility removes some of the problematic assumptions that exist within the current literature on lifecycle bias and starts to provide a true picture of persistence in inequalities across generations. The evidence presented suggests that, in addition to lifecycle bias and attenuation biases, studies measuring intergenerational economic mobility should consider the role of workless spells in their analysis, including both the sample selection that this causes and how best to include these individuals in terms of their economic resources.

An alternative approach to dealing with these measurement issues is to focus on mobility measures that exclusively focus on the extent of re-ranking of incomes across generations. These are shown to be far less susceptible to these measurement issues. Of course the downside of focusing purely on these rank-rank measures is that we lose the scale measurement across generations, or the extent of inequality, which is undoubtedly an important part of the story of intergenerational mobility across time and countries and plays an important role in public policy discussion.

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Table 1: Life-cycle bias in estimates of the intergenerational income elasticity (IGE) and Rank Coefficient in the UK

NCDS								
Age of earnings	23			33		42	46	50
β	0.042			0.205		0.291	0.259	0.224
	(.020)			(.026)		(.034)	(.026)	(.039)
Rank-rank	0.065			0.199		0.218	0.183	0.175
coefficient	(.024)			(.021)		(.021)	(.024)	(.024)
SD inc	*0.397*			*0.379*		*0.390*	*0.383*	*0.383*
SD earns	*0.334*			*0.464*		*0.633*	*0.568*	*0.612*
N	*1803*			*2161*		*2213*	*1653*	*1709*
BCS								
Age of earnings		26	30	34	38	42		
β		0.203	0.291	0.324	0.385	0.397		
		(.023)	(.022)	(.027)	(.031)	(.033)		
Rank-rank		0.258	0.305	0.322	0.337	0.338		
coefficient		(.026)	(.021)	(.023)	(.027)	(.024)		
SD inc		*0.480*	*0.479*	*0.476*	*0.487*	*0.486*		
SD earns		*0.418*	*0.475*	*0.534*	*0.554*	*0.649*		
N		*1416*	*1976*	*1691*	*1265*	*1596*		

Standard errors in parenthesis

Table 2: The impact of measurement error on estimates of the intergenerational income elasticity and Rank Coefficient in the BCS averaging income at 10 and 16

Panel A: Imputing income at 10 if missing					
Age of earnings	26	30	34	38	42
β	0.225	0.345	0.396	0.478	0.506
	(.027)	(.026)	(.032)	(.037)	(.039)
Rank-rank coefficient	0.242 (.026)	0.306 (.023)	0.331 (.025)	0.343 (.028)	0.347 (.025)
SD inc.	*0.422*	*0.419*	*0.422*	*0.420*	*0.421*
N	*1416*	*1976*	*1691*	*1265*	*1596*
Panel B: Imputing income at 10 or 16 if missing					
Age of earnings	26	30	34	38	42
β	0.227	0.366	0.420	0.468	0.497
	(.022)	(.022)	(.031)	(.031)	(.032)
Rank-rank coefficient	0.235 (.020)	0.301 (.017)	0.319 (.019)	0.323 (.021)	0.318 (.019)
SD inc.	*0.389*	*0.383*	*0.385*	*0.386*	*0.387*
N	*2364*	*3340*	*2806*	*2080*	*2685*

Standard errors in parenthesis. Dummy included if income is imputed.

Table 3: Frequency of worklessness across the life-cycle and by family background

Panel A: Frequency of sample (%)				
Cohort:	NCDS	NCDS	BCS	BCS
Earnings life cycle period:	23-50	26-42	26-42	26-42
Family income observed at:	16	16	16	10/16
Time spent workless				
None	60.4	69.7	87.1	86.3
<2 years	23.5	18.1	5.5	5.6
2+ years	14.5	10.6	4.3	4.8
All	1.5	1.5	3.1	3.3
Total	100.0	100.0	100.0	100.0
N	*3453*	*3453*	*2543*	*4312*
Panel B: Average weekly family income (2001 £s)				
None	328.96	328.28	350.28	322.93
<2 years	317.11	313.13	321.24	297.31
2+ years	296.35	289.45	275.57	270.85
All	269.00	269.00	245.93	246.54
N	*3453*	*3453*	*2543*	*4312*
Panel C: Average weekly earnings (2001 £s)				
None	542.05	532.54	517.24	510.03
<2 years	490.39	464.24	432.71	411.60
2+ years	347.87	332.07	331.09	314.78
All	0.00	0.00	0.00	0.00
N	*3453*	*3453*	*2543*	*4312*

Family income figures differ slightly in columns 1 and 2 as the proportion of people in each cell changes as workless period definitions change across periods of lifecycle considered.

Table 4: Lifetime estimates of the IGE and Rank Coefficient in the UK with no adjustment for periods out of work by lifetime workless experiences – cumulative samples described in the first three rows of Table 3.

Panel A: Intergenerational elasticities (β)				
Cohort:	NCDS	NCDS	BCS	BCS
Earnings life cycle period:	23-50	26-42	26-42	26-42
Family income observed at:	16	16	16	10/16
Time spent workless				
None	0.178 (.025)	0.183 (.023)	0.298 (.021)	0.372 (.020)
SD earns	*0.456*	*0.456*	*0.475*	*0.478*
N	*2085*	*2408*	*2214*	*3723*
<2 years	0.188 (.022)	0.190 (.022)	0.299 (.020)	0.371 (.020)
SD earns	*0.463*	*0.467*	*0.483*	*0.486*
N	*2898*	*3034*	*2355*	*3963*
2+ years	0.212 (.021)	0.207 (.021)	0.302 (.020)	0.383 (.020)
SD earns	*0.488*	*0.489*	*0.491*	*0.497*
N	*3400*	*3400*	*2464*	*4170*
Panel B: Rank-rank coefficient				
None	0.180 (.021)	0.188 (.020)	0.307 (.021)	0.300 (.016)
N	*2085*	*2408*	*2214*	*3723*
<2 years	0.188 (.018)	0.190 (.018)	0.305 (.020)	0.295 (.016)
N	*2898*	*3034*	*2355*	*3963*
2+ years	0.194 (.017)	0.192 (.017)	0.306 (.020)	0.300 (.015)
N	*3400*	*3400*	*2464*	*4170*

Standard errors in parenthesis. Dummy included where earnings are imputed at each age. The standard deviation of earnings and apply to the corresponding cells in both panel A and B. They are not repeated in Panel B for this reason.

Table 5: Lifetime estimates of the IGE and Rank Coefficient in the UK with alternative adjustments for periods of worklessness in the measure of lifetime earnings – excluding those who are nearly always out of work (i.e, row 4 in Table 3)

Panel A: Intergenerational elasticities (β)				
Cohort:	NCDS	NCDS	BCS	BCS
Earnings life cycle period:	23-50	26-42	26-42	26-42
Family income observed at:	16	16	16	10/16
Ignoring workless spells	0.212 (.021)	0.207 (.021)	0.302 (.020)	0.383 (.020)
SD earns	*0.488*	*0.489*	*0.491*	*0.497*
Including workless spells as:				
Zero earnings	0.255 (.025)	0.255 (.026)	0.343 (.028)	0.425 (.028)
SD earns	*0.594*	*0.618*	*0.670*	*0.687*
Imputed benefits	0.232 (.022)	0.230 (.023)	0.320 (.021)	0.398 (.021)
SD earns	*0.522*	*0.530*	*0.515*	*0.523*
Wages foregone (selection)	0.217 (.021)	0.210 (.021)	0.305 (.020)	0.386 (.020)
SD earns	*0.496*	*0.492*	*0.495*	*0.501*
N	*3400*	*3400*	*2464*	*4170*
Panel B: Rank-rank coefficient				
Ignoring workless spells	0.194 (.017)	0.192 (.017)	0.306 (.020)	0.300 (.015)
Zero earnings	0.194 (.017)	0.194 (.016)	0.308 (.020)	0.298 (.015)
Imputed benefits	0.194 (.016)	0.195 (.016)	0.308 (.020)	0.298 (.015)
Wages foregone (selection)	0.192 (.017)	0.191 (.017)	0.307 (.020)	0.300 (.015)
N	*3400*	*3400*	*2464*	*4170*

Standard errors in parenthesis. Dummies included where earnings are imputed at each age. The standard deviation of earnings applies to the corresponding cells in both panel A and B. They are not repeated in Panel B for this reason.

Table 6: Lifetime estimates of the intergenerational income elasticity and partial correlation in the UK, including those who are nearly always workless – so adds in sample described in row 4 of Table 3

Panel A: Intergenerational elasticities (β)				
Cohort:	NCDS	NCDS	BCS	BCS
Earnings life cycle period:	23-50	26-42	26-42	26-42
Family income observed at:	16	16	16	10/16
Ignoring workless spells	0.212 (.021)	0.207 (.021)	0.302 (.020)	0.383 (.020)
SD earns	*0.488*	*0.489*	*0.491*	*0.497*
Including workless spells as:				
Zero earnings	0.363 (.045)	0.366 (.046)	0.523 (.056)	0.654 (.056)
SD earns	*1.091*	*1.103*	*1.458*	*1.494*
Imputed benefits	0.252 (.023)	0.251 (.024)	0.345 (.022)	0.430 (.022)
SD earns	*0.564*	*0.572*	*0.577*	*0.584*
Wages foregone (selection)	0.222 (.021)	0.215 (.021)	0.310 (.020)	0.392 (.020)
SD earns	*0.503*	*0.496*	*0.504*	*0.508*
N	*3453*	*3453*	*2543*	*4312*
Panel B: Rank-rank coefficient				
Ignoring workless spells	0.194 (.017)	0.192 (.017)	0.306 (.020)	0.300 (.015)
Zero earnings	0.195 (.016)	0.196 (.016)	0.306 (.019)	0.297 (.015)
Imputed benefits	0.195 (.016)	0.196 (.016)	0.306 (.019)	0.297 (.015)
Wages foregone (selection)	0.194 (.016)	0.193 (.016)	0.306 (.019)	0.300 (.015)
N	*3453*	*3453*	*2543*	*4312*

Standard errors in parenthesis. Dummies included where earnings are imputed at each age. The standard deviation of earnings applies to the corresponding cells in both panel A and B. They are not repeated in Panel B for this reason.

Figure 1: Life-cycle bias in estimates of the intergenerational income elasticity and partial correlation in the UK

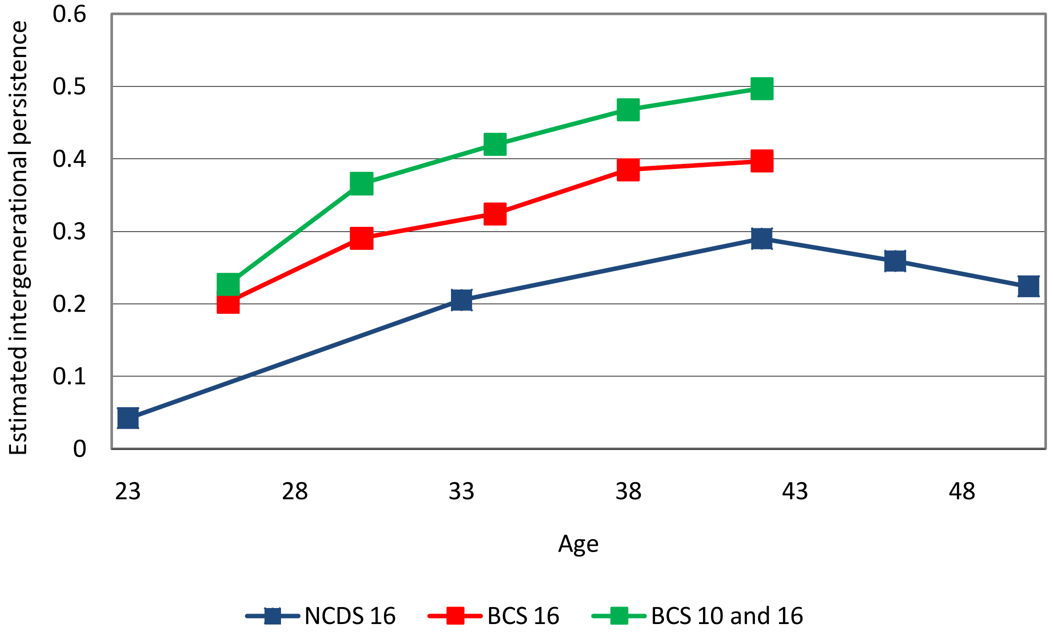
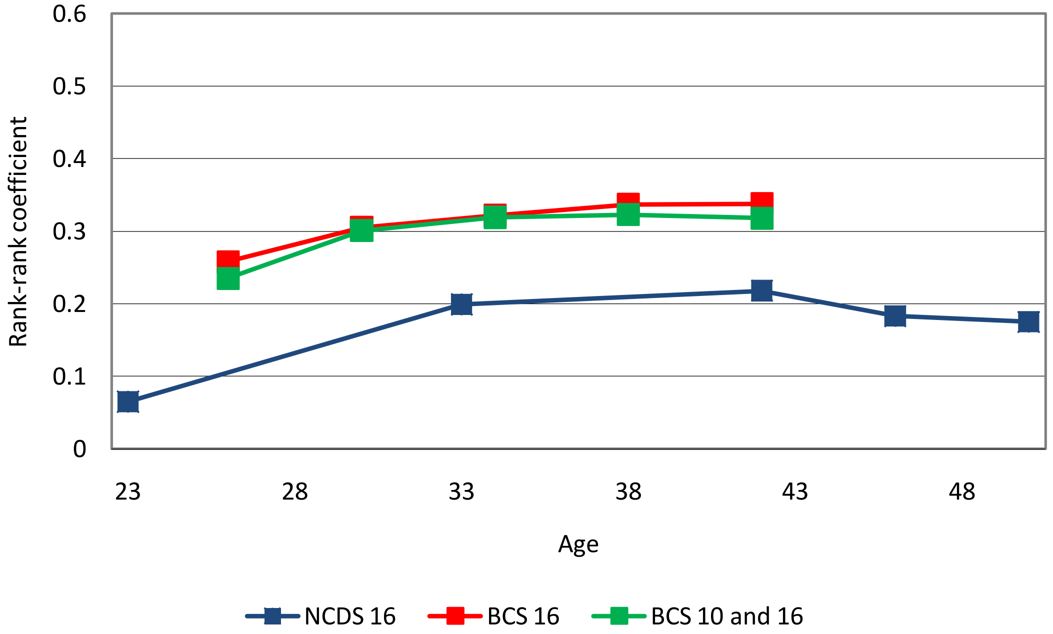


Figure 2: Life-cycle bias in estimates of the intergenerational rank-rank coefficient in the UK



Data Appendix

In this section we offer some descriptive analysis of our sample and we describe in detail the imputation methods adopted to deal with missing income and earnings.

Sample selection

When estimating lifetime earnings we take an average across all observed earnings periods and then a log of the average is used. The most complete measure for the NCDS is that obtained across age 23-50 and two comparable measures are estimated in the NCDS and BCS from age 26-42. Various restrictions are adopted in our sample. First of all an individual must have at least one income observation in childhood and be observed in the monthly work history data for at least five years to be included in our analysis. If individuals are workless for less than two years, they must have at least one earnings observation to be included in the sample. If individuals are workless for over two years, the same restriction applies unless they are out of work for the majority of time observed (proportion of time workless > 60%) in which case they are not required to have any earnings observations. These individuals are not included in the analysis until the final stage when individuals who are always workless are brought into the analysis so that we end up with a sample of 3452 individuals for the NCDS and of 4312 individuals for the BCS.

We start by providing some descriptive statistics related to our sample and to the sample we have excluded from our estimates of lifetime intergenerational mobility. This can be explored from the perspective of either generation. Table DA1 refers to parental characteristics: parental age, father's social class and education attainments for both parents. The table shows that in both cohorts, there are significant differences in the average parental age at birth of the child for those included and excluded from our sample although the direction of the bias varies across cohorts. Information on father's social class, observed at age 11 in the NCDS and the age 10 in the BCS[10], is very similar across the two samples, included and excluded, except for a slightly underrepresentation of self-employed in both cohorts

[10] The schema used is a 7-category variable (1-non skilled manual, 2-skilled manual, 3-lower grade technician, 4-self employed, 5-routine non manual, 6-lower grade manager, 7-professional), in line with those used to provide the headline results in sociology (Erikson and Goldthorpe, 2010) which is derived from the information on Socio-Economic Group available in the datasets.

and a slight over representation of professional fathers and a slight under representation of non-skilled manual in the BCS. This is consistent with the commonly observed pattern of attrition from those in lower classes in the latter cohort. The direction of the bias in other categories is not consistent across the cohorts.

Parental education is classified into 4 categories: with 1 equating to less than O levels, 2 to O levels/GCSEs, 3 to A levels and 4 to higher education. Our BCS and NCDS samples are mostly characterized by both parents with a low level of education. Indeed, the majority do not have O levels/ GCSEs: around 60 percent of the fathers and almost half of the mothers in the NCDS and around 60 percent of both parents in the BCS. This reflects the fact that these individuals would have been educated before the 1972 raising of the school leaving age to 16 to coincide with the first set of formal qualifications. The distribution across education levels is very similar between the included and excluded sample for the NCDS, for both mothers and fathers. As seen with social class, in the BCS there is a slightly higher proportion of those with A levels or above for both mothers and fathers with a slightly lower proportion with no O levels suggesting a slight sample selection in favour of more affluent responses.

Table DA2 shows descriptive statistics for child's characteristics: IQ, reading and maths scores. The measures are standardised to mean zero. The table indicates that our included sample is characterized by sons with a higher performance along the three dimensions: general ability, reading and maths in both cohorts compared to those excluded from our sample.

Overall then our sample over-represents more affluent parents and better performing children in the latter BCS cohort. In the NCDS cohort, while the parents look very similar in terms of observed characteristics to those who are not included in our analysis, the children are clearly outperforming those who are excluded from our analysis.

Imputation

Our approach to modelling requires imputation for missing values for family income and sons earnings as an adult where we observe at least one other non-missing observation.

Parental income at 10 and 16

We start with the parents' generation which is more straightforward. In the NCDS there is only one observation of family income and hence there is no imputation undertaken. For the BCS cohort two observations are available at ages 10 and 16. Of our final sample of 4312 individuals in the BCS cohort, 47% report income at both 10 and 16, 41% report income at 10 but not 16 and 12% report income at 16 but not at 10. For the 47% who report income at both 10 and 16, an average of income is taken across both periods. For the remaining 53%, income is imputed for the period that it is not observed based on income in the other period and changes in social class, employment status, housing tenure and lone parent status across the two periods. An average is then taken across the observed and imputed income for each individual and dummy variables are included in our estimation to indicate when income is observed or imputed.

Table DA3 illustrates the average incomes for those who report incomes at 10 and 16 and the imputed incomes for those missing incomes in the respective periods. The incomes at 10 and 16, deflated to 2000 prices, are very similar when reported with an average monthly parental income of £1439.56 at 10 and £1482.44 at 16. For the 12% who report income at 16 but not 10, the imputed value of income used at age 10 is quite far below this average, £773.29 a month, suggesting that these individuals have low income at age 16 and are likely to have suffered shocks to their employment, class, housing tenure or lone parent status across this period. For the 41% who report income at 10 but not at 16, the average imputed value is around 20% lower than the average reported income at 16, again indicating that these individuals are from slightly lower income families at 10 and may have suffered shocks to their employment, class, tenure or family structure over the period.

Imputed earnings when missing and wages foregone for periods out of work

When attempting to measure earnings across the lifecycle we face the problem of missing wages. Individuals do not report wages for two reasons:

1) due to attrition or item-non-response in the survey data (individuals report they are employed but do not report earnings or they have missing wages and do not give info on their employment status)
2) due to a period of worklessness.

By combining monthly work history data with monthly earnings observations, we can identify which of the two groups' individuals belong to at each point in time.

For the first group we impute their earnings using information on earnings in other periods and the observed education level of the cohort member interacted with time to account for lifecycle bias. For the second group, we use three alternative imputation methods: zero, income replacement or wages foregone. The zero earnings simply consists in replacing the missing wages with a zero wage, income replacement is calculated based on the average level of job seekers allowance, income support and incapacity benefits received by cohort members at 42 and 46 in the NCDS and 30 and 34 in the BCS. This is adjusted for inflation and assigned whether the individual claimed any benefit or not. Finally, wages foregone are estimated using the panel imputation method proposed by Wooldridge (1995) which allows us to control for unobserved heterogeneity and selection in one common framework.

Wage imputation

To impute wages for our first group of interest, those missing earnings due to attrition or item non-response, observed wages w_{it} are modelled as a function of education and time periods by interacting the four education categories (1 =< o-levels, 2= o-levels, 3= a-levels, 4=degree+) with the five time periods corresponding to the age at which the son is interviewed: respectively, ages 23, 33, 42, 46 and 50 in the NCDS and ages 23, 30, 34, 38 and 42 in the BCS,

$$w_{it} = \beta + \theta_{jt} \sum_{j=1}^{4} \sum_{t=1}^{5} educ_{ij} * \tau_t + \alpha_i + \varepsilon_{it} \qquad (1)$$

where α_i is the individual-specific unobserved effects and ε_{it} is the unobserved random disturbance. The predicted mean wages from (1) will give us a measure of imputed lifetime potential wage.

Table DA4 compares the average monthly wages for non-missing earnings (first row) with that obtained by predicting earnings using the observed education level of the cohort member interacted with time to account for life-cycle bias (second row). The third row summarises the average monthly earnings obtained using the life cycle estimate for the missing earnings. The number missing earnings are then shown in the last row.

Average monthly wages of lifecycle imputed and actual earnings (first two rows) appear to be similar across different ages and cohorts and almost equivalent in some cases (at age 26 in the BCS or at age 33 in the NCDS). The lifecycle imputed earnings are consistently lower than the actual estimates. The small discrepancy between the two measures suggests that the lifecycle estimate can be considered a good proxy of actual earnings. Imputed lifecycle earnings for the missing and non-missing samples do not show a clear pattern of differential bias across different ages: comparing the lifetime averages shown in the last column, the average of the lifecycle imputed for the non-missing earnings is lower than for the missing earnings in the NCDS while the opposite is true for the BCS. It is also worth noting that the gap between the two measures is larger at later ages, from age 42 in the NCDS and from age 38 in the BCS with the lifecycle measure of the missing earnings ranked lower. This is signalling that those not reporting wages are characterized by lower education and possibly lower returns to education that tend to fully materialize around the age of 40.

Modelling selection: Wages foregone

For our second group of interest, individuals with missing wages who are observed to be out of work, we use an alternative approach. As discussed in the main paper, we examine three alternative measures of earnings when individuals are observed as workless: zero earnings, earnings replacement or wages foregone. The third of these, wages foregone, attempts to estimate the potential wage of the individual who is observed to be out of work, if they had been in work taking into account selection decisions regarding labour market participation.

In a panel setting one could possibly correct for sample selection using fixed effects estimator but estimates are inconsistent if the decision to participate in the labour market is not random or not fully captured by the observable variables. There may be some time-variant factors that cannot be identified by an individual fixed effect and that are going to influence wages through the error term. Heckman (1979) developed an estimator to deal with this source of bias for cross-sectional data. Later Wooldridge (1995) proposed an imputation method that can be used in a panel setting thus allowing to control for heterogeneity and selection in one common framework (see e.g. D'Addio et al., 2002; Dustmann and Rochina-Barrachina, 2007; Jackle and

Himmler, 2010; Semykina and Wooldridge, 2010). We use the Wooldridge approach to estimate wages foregone.

We first build a selection equation which describes a person's decision to participate in the labour market where s_{it}^{*} indicates the latent propensity to work and 1[.] is an indicator function that is equal to one when the individual participates in the labour market and zero otherwise:

$$s_{it}^{*} = z_{it}\gamma_1 + k_i + u_{it}\ ;\ s_{it} = 1[s_{it}^{*} > 0] \quad (2)$$

z_{it} is our vector of explanatory variables which includes, along with the education dummies from the wage equation (1), some elements that are likely to drive selection. We model selection using dummy variables for self-reported health status at the time of the interview and information on workless periods in the previous 4 years. k_i represents unobserved characteristics that are fixed over time and u_{it} are the individual-specific unobserved disturbances.

Participation in the labour market is defined as being in employment at the time of the interview. Following Mundlak (1978), Chamberlain (1984), and Wooldridge (1995) the time-invariant effects are assumed to be linked with z_{it} through a linear function of k_i on the time averages of z_{it} (denoted with $\bar{z}_i$) and an orthogonal error term a_i which exhibits no variation over time and is independent of z_i and u_{it} :

$$k_i = \bar{z}_i + a_i \quad (3)$$

hence equation (2) can be rewritten as follows:

$$s_{it}^{*} = \gamma_0 + z_{it}\gamma_1 + \bar{z}_i\gamma_2 + v_{it}, \quad (4)$$

with the composite error term $v_{it} = u_{it} + a_i$ being independent from z_{it} and normally distributed with zero mean and variance σ^2 .

The procedure proposed by Wooldridge (1995) that allows correcting for selection bias consists first in running a probit of s_{it}^{*} on z_i and $\bar{z}_i$ for each t (equation 4) and saving $\hat{\lambda}_{it}$ the Inverse Mills Ratios (IMRs), $\lambda_{it} = \lambda(\bar{z}_i\gamma_1)$. The wages foregone, taking into account the selection into the labour market, is obtained from the estimate of a new wage equation of the selected sample (equation 5) where the IMRs λ_{it} obtained from the selection equation are included for each time period (as suggested in Wooldrige, 1995) along with the regressors included in equation 4 (where x_{it} stands for the interaction between education and time dummies).

$$w_{it} = x_{it}\psi_1 + \xi_t\lambda_{it} + \eta_i + e_{it} \qquad (5)$$

Note that we assume different coefficients for λ_{it} in each time period by including interaction terms of the IMRs interacted with time dummies. The potential wage is obtained from equation (5) using a within-group estimator and computing standard errors robust to heteroskedasticity. A preliminary test for the presence of selection bias is also performed: the "variable addition test". This is tested by testing the joint significance of the IMRs obtained from the sample selection probit for each time periods in the wage equation.

Table DA5 describes the variables used for the selection and wage equation, while estimates of the selection equation (4) for each time period for the NCDS and BCS are respectively presented in Tables DA6 and DA7. Table DA8 shows results obtained from estimating the wage equation (5). As we can see in Tables DA6 and DA7, having poor health and having experienced time out of work in the previous 4 years do negatively affect selection into the labour market. In the wage equation (Table DA8) the null hypothesis of the Wald test that the 5 selections effects are jointly equal to zero is rejected both for the NCDS and BCS meaning that selection is an issue that needs to be accounted for in our estimates. Moreover negative lambdas interacted to each time period are almost consistently negative meaning that the estimates of intergenerational mobility obtained without taking into account selection into the labour force would be downward biased.

In Table DA9 we compare average monthly earnings using different imputation methods for those individuals who have missing earnings due to workless spells. The top panel refers to the NCDS while the bottom panel displays results for the BCS. The first row shows average monthly wages obtained by imputing the missing earnings due to worklessness using the lifecycle imputation above, ignoring any selection issue, using only information on education over the lifecycle. The second row shows monthly average earnings calculated using the Wooldridge sample correction method to estimate wages foregone. Finally, the last row shows average monthly wages obtained from the same sample imputing their earnings using information on earnings replacement (benefits).

As we would expect, the average monthly wages based on the benefit measure show are lower than the imputed wages ignoring periods of time out of work and the wages foregone estimates. Our estimated monthly wages using wages foregone and lifecycle imputation are similar at ages 23 and 33 for the NCDS and at ages 26 and 30 for the BCS while they tend to differ much more above the age of 40 in the NCDS and from age 38 onward in the BCS and consistently in the two cohorts when looking at the lifetime average. Indeed selection seems to pull down wages compared to standard lifecycle measures starting from the age of 40. This is when human capital returns are realized, as discussed above. The divergence between lifecycle and wages forgone might indicate that selection tends to have stronger negative effects on earnings at later ages when the negative effects of poor health and previous unemployment spells are most likely to emerge. Indeed, individuals with missing wages due to workless spells exhibit a poorer health status compared to those who work and also appear to have experienced a larger proportion of time out of work in the previous 4 years.

Table DA1: Description of parental characteristics for those included and excluded from our lifetime analysis. Figures show proportions of each sample excluding those with missing data.

	NCDS IN SAMPLE	OUT OF SAMPLE	cross-sample Difference	**BCS** IN SAMPLE	OUT OF SAMPLE	cross-sample difference
	Mean	Mean		Mean	Mean	
parental average age	38.47	38.97	-0.50**	32.35	28.07	4.28***
			(0.21)			(0.36)
father's social class	Freq	Freq		Freq	Freq	
1-non skilled manual	21.60	20.56	1.04	13.52	16.43	-2.91**
			(0.99)			(0.92)
2-skilled manual	34.08	30.16	3.92***	26.77	27.83	1.06
			(1.14)			(1.15)
3-lower grade technician	6.53	6.00	0.53	10.27	9.77	0.50
			(0.59)			(0.78)
4-self employed	3.20	7.84	-4.64***	9.27	12.28	3.01***
			(0.57)			(0.79)
5-routine non manual	10.50	8.84	1.66**	6.20	5.26	0.94
			(0.72)			(0.61)
6-lower grade manager	14.13	16.12	-1.99**	18.64	16.11	2.53**
			(0.88)			(0.99)
7-professional	9.96	10.49	-0.53	15.33	12.32	3.01***
			(0.74)			(0.90)
father's education						
1- less than O-levels	60.28	59.54	0.74	64.59	69.72	-5.13***
			(1.32)			(1.13)

2-O-levels/GCSEs	18.76	17.79	0.97	14.21	13.35	0.86
			(1.05)			(0.83)
3-A-levels	13.10	14.30	-1.20	11.32	9.80	1.52**
			(0.93)			(0.74)
4-higher education	7.85	8.36	-0.51	9.88	7.14	2.74***
			(0.74)			(0.68)
mother's education	Freq	Freq		Freq	Freq	
1- less than O-levels	49.54	48.45	1.09	63.29	69.65	-6.36***
			(1.33)			(1.12)
2-O-levels/GCSEs	29.80	29.65	0.15	17.40	15.41	1.99**
			(1.22)			(0.88)
3-A-levels	15.21	16.65	-1.44	12.76	9.99	2.77***
			(0.97)			(0.75)
4-higher education	5.44	5.26	0.18	6.54	4.95	1.59**
			(0.60)			(0.55)

Note: Two-sample t tests and two-sample tests of proportion are performed to check the differences in means and frequencies between the included and excluded samples.
*** Indicates significance at the 99% confidence level, ** is significant at the 95% confidence level, and * indicates a 90% confidence level. Standard errors in parentheses.

Table DA2: Description of child's characteristics

NCDS	IN SAMPLE Mean	OUT OF SAMPLE Mean	cross-sample difference
ablity test score at 11	0.03	-0.14	0.17*** (0.02)
reading test score at 11	0.10	-0.07	0.17*** (0.02)
maths test score at 11	0.12	-0.04	0.16*** (0.02)
BCS	IN SAMPLE Mean	OUT OF SAMPLE Mean	cross-sample difference
ability test score at 10	0.14	-0.14	0.28*** (0.03)
reading test score at 10	0.07	-0.22	0.29*** (0.03)
maths test score at 10	0.19	-0.14	0.33*** (0.03)

Note: Two-sample t tests to check the differences in means between the included and excluded samples are performed. *** Indicates significance at the 99% confidence level, ** is significant at the 95% confidence level, and * indicates a 90% confidence level.. Standard errors in parentheses.

Table DA3 Actual income versus imputed income for non-missing and missing income

Age of earnings	10	16
Actual income	1439.54	1482.44
Imputed income	773.29	1204.65
Number missing inc	525	1769

Table DA4 Actual earnings versus imputed life cycle earnings for non-missing and all missing earnings

NCDS									
Age of earnings	23			33		42	46	50	Lifetime average
Actual earnings	1196.21			2371.66		2536.18	2629.46	2753.95	2228.58
Life cycle imputation (non-missing earns)	1212.04			2321.12		2240.86	2473.29	2402.15	2075.51
Life cycle imputation (missing earns)	1222.14			2390.33		1940.33	2323.31	2288.42	2105.06
Number missing earns	913			1083		1189	1748	1699	6632
BCS									
Age of earnings		26	30	34	38	42			
Actual earnings		1148.27	1930.04	2381.22	2593.92	2590			2127.1
Life cycle imputation (non-missing earns)		1146.92	1894.56	2278	2503.19	2387.18			2040.16
Life cycle imputation (missing earns)		1092.93	1921	2211.79	2336.97	2262.21			1957.09
Number missing earns		1822	1141	1367	2090	1552			7972

The sample of non-missing earnings refer to all individuals reporting wages, while that for missing earnings refer to all individuals not reporting wages (either workless, or in employment or for which we have no info on employment status.

Table DA5: Description of the variables used to model labour market participation

Variable	Description
Outcome	
Wages	log monthly wages of the sons
Explanatory variables	
Time	dummies in 5 categories: NCDS BCS 1) Age 23 1) Age 26 2) Age 33 2) Age 30 3) Age 42 3) Age 34 4) Age 46 4) Age 38 5) Age 50 5) Age 42
Son's education	dummies in 4 categories: 1) less than O levels 2) O levels 3) A levels 4) Degree or higher
Selection variables	
Health status	dummies in 5 categories: 1) Excellent 2) Very good 3) Good 4) Fair 5) Poor
proportion workless	time spent workless in the previous 4 years

Table DA6: Modelling labour market participation in the NCDS

Age	23	33	42	46	50
EDUCATION					
Less than O levels	0.019	-0.267**	-0.045	0.197	-0.254*
	(0.14)	(0.13)	(0.17)	(0.18)	(0.15)
O levels	0.210	-0.125	-0.136	0.041	-0.097
	(0.14)	(0.13)	(0.16)	(0.16)	(0.14)
A levels	0.083	0.024	0.007	0.140	0.021
	(0.14)	(0.13)	(0.17)	(0.17)	(0.15)
Degree or higher	–	–	–	–	–
HEALTH					
Excellent	0.051	-0.037	0.148	-0.031	-0.120
	(0.07)	(0.09)	(0.12)	(0.14)	(0.12)
Very good	–	–	–	–	–
Good	-0.044	-0.273**	0.044	-0.101	-0.064
	(0.11)	(0.11)	(0.13)	(0.15)	(0.11)
Fair	-0.702**	-0.845***	-0.492**	-0.420*	-0.176
	(0.32)	(0.20)	(0.22)	(0.23)	(0.18)
Poor	0.000	0.000	0.000	-1.031***	-1.538***
	(0.00)	(0.00)	(0.00)	(0.35)	(0.46)
Proportion workless	-2.661***	-2.946***	-2.557***	-1.934***	-4.415***
	(0.15)	(0.18)	(0.18)	(0.26)	(0.28)
Constant	1.357*	2.214***	1.811**	1.974*	4.799*
	(0.56)	(0.59)	(0.65)	(0.98)	(2.08)
N	4298	5543	5532	4578	4718

*p<0.10, **p<0.05, ***p<0.01. Probit estimates of the BCS sample of all males with at least one income observation in childhood and observed in the monthly work history data for at least five years. Proportion workless refers to the proportion of time workless in the last 4 years.Unobserved effects are specified as a linear projection on the (within) means of the regressors.

Table DA7: Modelling labour market participation in the BCS

Age	26	30	34	38	42
EDUCATION					
Less than O levels	0.159*	-0.144	-0.053	-0.145	-0.028
	(0.08)	(0.11)	(0.13)	(0.13)	(0.16)
O levels	0.397***	-0.067	0.006	-0.026	0.242
	(0.08)	(0.10)	(0.12)	(0.12)	(0.16)
A levels	0.292***	-0.091	-0.231	-0.157	0.074
	(0.10)	(0.13)	(0.15)	(0.16)	(0.21)
Degree or higher	–	–	–	–	–
HEALTH					
Excellent	-0.026	-0.017	-0.214*	-0.231*	-0.148
	(0.09)	(0.10)	(0.12)	(0.14)	(0.18)
Very good	–	–	–	–	–
Good	-0.358***	0.036	0.145	0.057	-0.068
	(0.12)	(0.11)	(0.13)	(0.12)	(0.17)
Fair	1.114***	-0.585***	-0.564***	-0.197	0.050
	(0.30)	(0.22)	(0.19)	(0.18)	(0.27)
Poor	0.000	0.000	1.193***	-0.094	-1.111***
	(0.00)	(0.0)	(0.35)	(0.33)	(0.37)
proportion workless	-1.668***	-2.252***	-2.743***	-1.422***	-6.280***
	(0.21)	(0.22)	(0.30)	(0.30)	(0.54)
Constant	1.650***	1.567***	2.193***	0.424	2.642***
	(0.45)	(0.38)	(0.53)	(0.69)	(0.64)
N	3879	5205	4434	4016	4478

*p<0.10, **p<0.05, ***p<0.01. Probit estimates of the BCS sample of all males with at least one income observation in childhood and observed in the monthly work history data for at least five years. Proportion workless refers to the proportion of time workless in the last 4 years.Unobserved effects are specified as a linear projection on the (within) means of the regressors.

Table DA8: Imputed wage models in the NCDS and BCS

	NCDS	BCS
Less than O levels * time 1	-0.478***	-0.628***
	(0.03)	(0.02)
Less than O levels * time 2	0.039	-0.196***
	(0.02)	(0.02)
Less than O levels * time 3	-0.093***	-0.053***
	(0.02)	(0.02)
Less than O levels * time 4	0.000	0.000
	(0.00)	(0.00)
Less than O levels * time 5	0.005	-0.057***
	(0.02)	(0.02)
O levels * time 1	-0.558***	-0.720***
	(0.02)	(0.02)
O levels * time 2	0.041**	-0.227***
	(0.02)	(0.02)
O levels * time 3	-0.070***	-0.071***
	(0.02)	(0.02)
O levels * time 4	0.042**	0.000
	(0.02)	(0.00)
O levels * time 5	0.000	-0.045**
	(0.00)	(0.01)
A levels * time 1	-0.842***	-0.812***
	(0.02)	(0.03)
A levels * time 2	-0.097***	-0.233***
	(0.02)	(0.03)
A levels * time 3	-0.140***	-0.064**
	(0.02)	(0.03)
A levels * time 4	0.000	0.081***
	(0.02)	(0.03)
A levels * time 5	0.000	0.000
	(0.00)	(0.00)
Degree or higher * time 1	-1.226***	-1.074***
	(0.04)	(0.03)
Degree or higher * time 2	-0.145***	-0.340***
	(0.03)	(0.03)
Degree or higher * time 3	-0.085**	-0.098***
	(0.04)	(0.02)
Degree or higher * time 4	0.000	0.000
	(0.00)	(0.00)
Degree or higher * time 5	0.025	0.030
	(0.03)	(0.02)
λ_1	0.049	-0.030

	(0.07)	(0.09)
λ_2	-0.161*	-0.102
	(0.09)	(0.13)
λ_3	-0.466***	-0.128
	(0.09)	(0.12)
λ_4	-0.678***	-0.191*
	(0.11)	(0.11)
λ_5	-0.505***	-0.502***
	(0.07)	(0.16)
Constant	7.725***	7.639***
	(0.01)	(0.01)
Wald test	χ_5^2 =13.30	χ_5^2 =115.9
NT	17778	154757
N	6386	5492

Panel estimates controlling for unobserved individual effects. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01. Wald test on the joint significance of the IMRs are provided. Both in the NCDS and BCS the null hypothesis is rejected. Times 1, 2, 3, 4, 5 refer to ages 23, 33, 42, 46 and 50 in the NCDS and ages 26,30,34,38,42 in the BCS.

Table DA9 Average Imputed wages for individuals with missing wages due to workless spells

NCDS									
Age of earnings	23			33		42	46	50	Lifetime average
Life cycle imputation	1015.77			1600.87		1627.33	1869.11	1904.54	1583.14
Wages foregone	1080.75			1636.58		1152.42	1009.33	1165.58	1211.21
Benefit	162.31			287.30		363.92	403.40	461.51	335.67
BCS									
Age of earnings		26	30	34	38	42			Lifetime average
Life cycle imputation		843.85	1450.19	1660.47	1881.95	1952.18			1493.34
Wages foregone		833.93	1453.07	1616.72	1784.37	1055.54			1303.04
Benefit		311.53	345.04	380.63	444.43	501.38			328.20