Moving Towards Estimating Lifetime Intergenerational Economic Mobility in the UK

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Abstract

This paper makes two significant contributions to the current literature on intergenerational economic mobility in the UK using 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. We consider the role of life-cycle bias and measurement error for the first time in relation to point-in-time estimates of mobility. We also estimate lifetime intergenerational economic mobility in the UK for the first time, highlighting an additional bias driven by those who experience spells out of work. We show that each bias has contributed to understating previous estimates of intergenerational economic mobility in the UK.

When all biases are taken into account, it is likely that previous point in time estimates of intergenerational mobility in the UK understated intergenerational economic mobility by 0.18-0.26, or over 45%. The more realistic figure for intergenerational economic mobility in the BCS is around 0.51: 51% of inequalities in sons earnings are transmitted across generations in the UK. Hence the problem of intergenerational economic mobility in the UK is far worse than initially thought.

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JEL Classification: J62, D31, J64

1. Introduction

Ideally the degree of intergenerational mobility within a country would be measured by the association between the socio-economic status (SES) of parents throughout a person's childhood and their life-time earnings as an adult. As such it represents the extent to which adult outcomes mirror childhood circumstances and are an indicator of equality of opportunity. For this reason, this is currently a highly topical area in the UK where the notion of 'opportunity for all' has been cited as a goal by all three major political parties as a central social policy goal. This is extremely data intensive as it requires a person's entire childhood and working life to be observed. The existing literature on intergenerational economic mobility, therefore approximates this life-time intergenerational mobility with point in time measures.

Within the literature on the measurement of intergenerational mobility, two biases have been highlighted that are likely to have significant impacts on the estimation of intergenerational persistence when using point in time measures of childhood family income and a person's adult earnings: life-cycle bias and attenuation bias. Haider and Solon (2006), Grawe (2006) and Bohlmark and Lindquist (2006) drew attention to the issue of life-cycle bias in the US and Sweden where estimates of intergenerational persistence are biased to the extent that point in time earnings is correlated with life-time earnings. These studies show that estimates of intergenerational mobility are downward biased when earnings are measured early in a person's labour market career. 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 commonest approach to address this bias is to average over repeat measures in the parents' generation and thus the movement here is toward a longer-term income. In terms of earnings for the second generation, the literature to has sought to say at what age is the bias zero (at age 36 and 44 in Bohlmark and Lindquist) rather than to assess for observed life-time earnings.

In the UK, much of the evidence to date has focused on comparisons and drivers of income persistence rather than the measurement of persistence. Blanden et. al. (2004, 2005) found that intergenerational income persistence increased over time – intergenerational mobility declined for a cohort born in 1970 compared to a cohort born in 1958. Blanden et. al. (2007) found that this can be largely accounted for by the increasing association between family income and educational attainment in the latter cohort. None of these studies explicitly consider these measurement issues. Dearden Machin and Reed (1997) are the only UK study to consider the likely impact of attenuation bias on estimates of intergenerational persistence. Given the data constraints at the time of writing, they only considered this for the eldest birth cohort born in 1958. No studies to date have yet considered the likely role of life-cycle bias on estimates of intergenerational economic mobility in the UK. So here there has been no move toward using life-time measures in either generation, rather just assessing the likely bias from point in time income.

The UK has a series of rich birth cohort studies which follow children born in time window for the rest of their lives. The first two of these are now sufficiently mature to document the biases described above (only in the second can we observe multiple measures of parental income) and move toward measures of life-time intergenerational mobility. In doing so we identify a third bias, not yet considered in any literature both in the UK or internationally, driven by missing data from spells out of work. Those who experience substantial periods out of work are 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 important though is that including periods out of work in a measure of life-time earnings results in a materially important third source of bias. 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. Hence the problem of intergenerational economic mobility in the UK is far worse than initially thought.

In the next section we discuss the related literature on measuring intergenerational economic mobility. In section 3 we lay out our modelling approach in more detail and in section 4 we discuss our data. Section 5 presents our results before we end with some brief conclusions.

2. Related literature

For economists, the ideal measure of mobility persistence within a country is measured as the earnings or income for the cohort member over their entire adulthood and the parental income during the cohort members' entire childhood. This assesses the relationship between lifetime or permanent childhood income and lifetime adult earnings. Data is therefore required for the parents' income in the cohort members' childhood and for the cohort member's own earnings later in life. These data requirements are fairly exacting and have led to early estimates of intergenerational persistence in the UK using point in time measures of parental and children's adult earnings. Sometimes fathers earnings is considered rather than childhood incomes which would increase the data requirements for life-time measures further.

Blanden, Gregg and Machin (2005) use two British cohorts, the National Child Development Survey (NCDS) birth cohort of 1958 and the British Cohort Study (BCS) birth cohort of 1970 to assess the extent to which intergenerational mobility had shifted over time. Estimating intergenerational persistence based on point in time income measures in childhood when sons are age 16 and point in time earnings measures in adulthood when the sons are age 33/30, they find that mobility in the UK has decreased across this period: the intergenerational elasticity of earnings with respect to parental income, increased from 0.205 to 0.291. This indicates that the level of intergenerational mobility persistence has risen: the UK became less mobile across the cohorts. These results have been widely reported and conflict with a notion that opportunities were more equal now than in the past.

Haider and Solon (2006), Grawe (2006) and Bohlmark and Lindquist (2006) use data from the US and Sweden to illustrate that measures of earnings at age 30 are likely to understate lifetime earnings in both countries. In each study, lifetime earnings, measured as an average across the entire life-cycle, is regressed on current point in time earnings to illustrate the relationship between current and lifetime earnings. Hence the life-time earnings are not calculated and directly related to parental circumstances, just the bias from using point in time estimates is assessed. So if individuals earnings are observed too early in their life-cycle, typically before age 38 in the US and age 35 in Sweden, then current labour market earnings fall significantly below lifetime earnings. This is likely driven by the returns to education not yet being realised in the labour market for those with the highest levels of education and, given that education is socially graded, this will understate true lifetime differences in earnings for those form different background. This approach reduces the extensiveness of data required, calculating the correlation between earnings at any age and life-time earnings from separate data and potentially an earlier cohort. Previous estimates of intergenerational persistence in the UK are therefore likely to understate lifetime economic persistence due to life-cycle bias. However, this relationship is not necessarily constant across countries or cohorts and hence there is value in observing the true relationship where possible.

Solon (1992) and Zimmerman (1992) first drew attention to the issue of measurement error within the intergenerational mobility literature when attempting to estimate correlations in income across generations for the United States. Both studies illustrate that measures of income in childhood at a point in time can lead to attenuation bias in estimates of intergenerational persistence as these measures are affected by measurement error and transitory variation in incomes. Previous estimates of intergenerational persistence in the UK are therefore also likely to understate lifetime economic persistence due to this attenuation bias. Dearden, Machin and Reed (1997) attempted to correct for this issue in the earlier 1958 birth cohort using a two-stage least squares approach and found substantially higher estimates of intergenerational persistence, in the region of 0.55, although this is likely to be an upper bound due to the assumptions required for the technique used.

In both of these literatures cited above, concerning life-cycle and attenuation biases, the data used has been based on averages of measures of annual earnings of fathers and sons over several years where earnings is positive. So that years with no earnings are excluded from the calculations. Yet when considering life-time earnings, periods out of work clearly matter but for point in time measures including zeros will radically alter the variance of the measures used. This will potentially alter the life-cycle bias adjustment through the relationship between point in time earnings, with zeros, and life-time earnings. In addition increased variance in the fathers earnings is likely to increase the concerns about attenuation bias. Hence there is value in utilising data where all three potential biases can be assessed simultaneously.

3. Methodology

The standard economic literature, the ideal estimate of intergenerational persistence would measure the lifetime earnings of an individual in adulthood ($y_i^{son^*}$) and the earnings of the father or income of the parents of the individual across their entire childhood ($y_i^{parent^*}$).

$$y_i^{son^*} = \alpha + \beta y_i^{parent^*} + u_i \quad (1)$$

In an OLS regression, the estimated coefficient $\hat{\beta}$ therefore gives the intergenerational elasticity or the association between parental resources during childhood and the individual's adult earnings. If the estimated association is zero then there is full intergenerational mobility; the income of the parents has no influence on the child's future income. If the association is one, there is complete immobility; childhood resources fully determine adult resources.

Given that the mobility coefficient measures the association between two distributions spread decades apart it can be useful to adjust the coefficient by the variance of the distributions so as to control for changes in inequality across this time period. This measure, known as the partial correlation, therefore gives us a scale invariant measure of intergenerational persistence.

$$r = \hat{\beta}\left(\frac{SD^{y^{parent^*}}}{SD^{y^{son^*}}}\right) \quad (2)$$

Point-in-time estimates of intergenerational economic mobility

Previous literature in the UK has estimated the intergenerational elasticity (1) and the partial correlation (2) using measures of parental income in childhood and adult earnings observed at one point in time. Therefore both measures are measured with error.

$$y_{it}^{parent} = y_i^{parent^*} + \epsilon_{it} \quad (3)$$

$$y_{it}^{son} = y_i^{son^*} + \varepsilon_{it} \quad (4)$$

As discussed a substantive measurement issue, highlighted by Haider and Solon (2006), Grawe (2006) and Bohlmark and Lindquist (2006) is that age-earnings profiles are steeper for individuals with more human capital. If earnings are measured too early in the life-cycle, the differences between those with different levels of human capital will not yet be realised in terms of earnings. As levels of human capital vary by family resources, early measures of earnings will understate the true intergenerational persistence across generations. Focusing on the sons earnings for notational simplicity (although life-cycle bias affects both measures) a measure of sons earnings at a point in time varies from the lifetime earnings across the lifecycle by some coefficient, $\hat{\lambda}_t$.

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

Assuming no error in the parental income variable, we estimate

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

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

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

$$plim\hat{\beta} = \lambda_t \beta \tag{7}$$

An important point to note here is that the relationship between returns to education and educational inequality vary both across country and across time. Life-cycle bias is also therefore likely to vary across countries and across cohorts. Indeed, when previous studies have used current and lifetime earnings to estimate $\hat{\lambda}_t$ this is found to vary by age across countries and cohorts. We take an alternative approach, estimating intergenerational persistence directly at various points across the life-cycle. This provides direct evidence on the shape relationship as individuals' age for two cohorts of data in the UK for the first time.

A further issue to consider in the measurement of mobility is that any 'point in time' family income measures are likely to be measured with error and include unobserved transitory shocks as shown in equation (3). In this setting, assuming no error in the sons earnings measure we therefore estimate

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

Our estimate of $\hat{\beta}$ 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 estimates therefore give a lower bound of the true estimates of beta. Solon (1992) introduced the idea of using average income across a number of observations to minimise, although it is noted not eradicate, the downward bias due to measurement error. Using a more recent cohort of data with two observations of family income, we can apply this method of dealing with attenuation bias to UK data for the first time, minimising the issues with the 2SLS method used previously in Dearden, Machin and Reed (1997).

These issues of measurement have two aspects that can be conceptually separated and developed analytically. Measurement error and life cycle biases will reflect both positional inaccuracy and scale measurement. If earnings distributions are represented by a ladder, positional inaccuracy relates to people switching rungs on the ladder and scale measurement relates to how far apart the rungs of the ladder are.

Observing earnings when a person has not yet realised the full returns from education can lead to placing them lower in the distribution than will occur some years later when their earnings have matured: individuals changing positions on the ladder. In addition the scale of earnings gaps between the less and better educated will be understated, which means the variance or inequality dimension is mis-measured: the distance between the rungs of the ladder will be under- or over- stated. The same applies to measurement error or transitory income shocks.

Standardisation of income and earnings, reflected in the partial correlation coefficient, removes the issue of scale measurement (inequality) from the picture and just leaves the positional accuracy concern. By comparing the regression coefficients to the standardised partial correlations throughout our analysis we can therefore comment on the relative affects of scale measurement and positional accuracy from both types of bias.

Lifetime intergenerational economic mobility

A central contribution of this new research is to estimate lifetime intergenerational economic mobility in the UK for the first time. The second contribution of this paper is therefore to move away from the point-in-time estimates of intergenerational economic mobility to take a view of earnings in the second generation across the life-cycle by averaging earnings across individuals' lifetimes (details in section 4 below).

In doing so, we highlight a major restriction of previous research: the inability to capture mobility trends for individuals that are workless or have no observable wage.

Previous literature by Gregg and Tominey (2005) on the effects of unemployment on future unemployment within the same generation finds a causal impact of youth unemployment spells on wages twenty years later. Macmillan (2014) illustrates that workless spells are not random – individuals who experience spells out of work are more likely to come from more disadvantaged backgrounds. By excluding those who we do not observe earnings for at a point in time because they are out of work, we are likely to be further understating intergenerational persistence due to this third, regularly unmentioned, workless bias.

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, income replacement and wages foregone. The first, zero earnings, represents the true earnings value received by those who are out of work. While this is representative of their true earnings, this significantly increases inequality in the earnings distribution and may not be a true representation of the individual's available resources. It is therefore likely to overstate the true impact of worklessness on lifetime earnings. The second method, income replacement, imputes the average benefit level available at the time of the workless spell. This is our preferred measure of earnings replacement as this is more representative of resources and mirrors the measure of resources in the first generation. The third method, wages foregone, are estimated using a Wooldridge panel selection method (see Data Appendix) which combined information on wages in other periods with a selection equation, using self-reported health to model labour market participation. This method is likely to understate the true impact of worklessness on lifetime earnings.

Individuals may also be missing from our estimates due to attrition or item-non-response in the survey data used to measure mobility in the UK. We further analyse the impact of this on intergenerational estimates by imputing earnings and income for these individuals using the Wooldridge panel imputation method (see Data Appendix) although this has little impact on our estimates of lifetime intergenerational economic mobility.

4. Data

As in previous studies of intergenerational economic mobility in the UK, we use the two mature British birth cohort studies: the National Child Development Study (NCDS) born in 195 and the British Cohort Study (BCS) born in 1970. Both cohorts began with around 18,000 children. Blanden, Gregg and Macmillan (2013) find no significant impact of attrition between the two cohorts on the characteristics of the families during childhood. The National Child Development Study (NCDS) obtained data at birth and ages 7, 11, 16, 23, 33, 42, 46 and 50 for children born in a week in March 1958. The BCS originally included all those born in Great Britain between 4th and 11th April 1970. Information was obtained about the sample members and their families at birth and at ages 5, 10, 16, 26, 30, 34 and 38.

For the purpose of our study, we need to observe the resources of parents and sons across generations. Parental income data is available at age 16 in both of the British 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. A transformation is implemented to the bands from gross to net using information from the Family Expenditure Survey (FES) of 1986 for comparability. We generate continuous income variables by fitting a Singh-Maddala distribution (1976) to the data using maximum likelihood estimation. This is particularly helpful in allocating an expected value for those in the open top category. 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 Data Appendix, Blanden, Gregg and Macmillan, 2013).

A repeat of income data for another period is not available in the NCDS but is at age 10 in the BCS cohort and so averaged income from two periods can be constructed for this cohort. 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.

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 and 38. 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 the NCDS cohort almost across their entire working lives (average age 38) while the BCS cohort can be observed across two thirds of their working lives (average age 32).

For the point-in-time estimates of intergenerational economic mobility, accounting for life-cycle bias and attenuation bias, the sample is restricted as in previous studies to all sons with earnings observations at a point in time who are observed as employed (and not self-employed) with at least one parental income observation at age 16 in the NCDS and at 10 or 16 in the BCS. The implications for observing both compared to one period of parental income in the BCS are considered in the next section.

To measure lifetime earnings an average is taken across all observed earnings periods. If earnings are missing in any period due to item-non response (individual reports they are employed but does not report earnings), they are imputed based on earnings in other periods and the observed education level of the cohort member (interacted with time to account for life-cycle bias). Dichotomous imputation variables are included for each observed earnings period to indicate whether the information is observed or imputed (further details in the Data Appendix). Given the differential spacing of the earnings measures in the NCDS (10 year apart, then 9 years apart, then 4 years apart) for both cohorts we impute a linear trajectory for each month between earnings observations, creating a weighted 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 26-38.

To account for those without earnings due to periods out of work, monthly work history data, available in the NCDS and BCS from 16-50 and 16-38 respectively is combined with the monthly earnings observations. If the individual is observed as workless in any given month, their earnings trajectory for that month is replaced with a workless value. As discussed in the methodology section, three alternative values are assigned to those who are observed out of work in any given month: zero, income replacement or wages foregone. 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. Wages foregone are estimated based on earnings in other periods, the observed education level of the cohort member and self-reported health at the time that the cohort member is observed as workless to model participation (further details in the Data Appendix).

Various sample restrictions are explored in the results for estimates of lifetime earnings. An individual must have at least one income observation in childhood and be observed in the monthly work history data to be included in the 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 (>60%) in which case they are not required to have any earnings observations. These individuals are not included in the analysis until the final stages when workless periods are considered.

5. Results

Point-in-time estimates of intergenerational economic mobility

Table 1 presents our point-in-time estimates of intergenerational elasticities and partial correlations from OLS regressions of log earnings at various points across the life-cycle 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) indicating that mobility across time has declined in the UK: intergenerational persistence increased over time.

Focusing on the NCDS, who we observe up until age 50 currently and therefore almost have a complete life-cycle perspective of intergenerational mobility for, we can see that intergenerational persistence starts very low at age 23, at 0.042, before steadily climbing to 0.205 by age 33, increasing to 0.291 by age 42 and then declining again to 0.259 at age 46 and 0.224 at age 50. This is likely driven by the realisation of returns to education, evident in the increasing variation in earnings as individuals' age, which will be socially graded. The fact that the partial correlations do not exhibit the same trends across the life-cycle bias further supports the notion that this is driven by scale measurement rather than positional inaccuracy. Life-cycle bias (particularly from age 30 onwards) is not driven by individuals changing positions on the ladder at different points in the life-cycle but rather the distance between the rungs on the ladder being under- or over- stated.

Figure 1 plots the trajectory across the life-cycle. This pattern of the intergenerational elasticity rising until around age 40 and then declining is consistent with what we would predict from the life-cycle bias literature. If the UK is closer to Sweden in terms of the relationship between current and lifetime earnings, the estimate at age 33 is likely to be a close approximation of lifetime mobility while the age 42 estimate overstates this. If the UK is more like the US then the age 42 estimate is likely to be more representative of lifetime intergenerational economic mobility while the age 33 estimate is still too early and therefore understates this. We will return to this discussion in the next section.

In the BCS, we can see a similar pattern emerging across the life-cycle with estimated intergenerational persistence increasing from 0.206 at age 26 to 0.291 at 30, 0.324 at 34 and 0.385 at 38. Note that at any given age the estimated persistence in the BCS cohort is significantly different from that in the NCDS (higher persistence, lower mobility). At the two most comparable ages, 33 in the NCDS and 34 in the BCS, the intergenerational elasticity is 0.119 percentage points higher in the later cohort. Figure 1 illustrates that the rate of increasing persistence across the period is very similar in the BCS compared to the NCDS suggesting that life-cycle bias is similar across cohorts in the UK, unlike the pattern found in Sweden (Bohlmark and Lindquist, 2006, Gregg et. al., 2013).

Table 2 presents estimates from the later UK birth cohort, the BCS, using average parental income at 10 and 16 rather than point-in-time income at 16, 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 slightly 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 but who we do observe information for at age 10.

Panel A estimates intergenerational persistence for 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 (22% of parents of cohort members report an income at 16 but not at 10). The estimated intergenerational elasticity increases across all ages by 0.02 percentage points at age 26, 0.04 at age 30, 0.07 at age 34 and 0.09 at age 38. Note that the partial correlations are very similar in Panel A an Table 1 indicating that any issues of measurement error and transitory shocks dealt with by averaging across these two periods are leading to a mis-measurement of the variance of parental income in Table 1 rather than parents re-ranking within the distribution of income.

Panel B 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 changes the estimated intergenerational elasticities and partial correlations very little, increasing the estimated elasticity by just less than 1 percentage point on average across the four estimates. 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.

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 intergenerational elasticities 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 be represent around 80% of the total estimated intergenerational persistence if parental income were observed in every year across childhood.

Lifetime intergenerational economic mobility

We therefore have a picture of previous estimates of intergenerational persistence in the UK being understated by a magnitude of around 0.05-0.07 due to life-cycle bias, assuming that earnings at age 36 are representative of true lifetime intergenerational economic mobility (if the UK was somewhere between the US and Sweden in terms of life-cycle bias). In addition, previous estimates are understated by a further 0.08-0.10 due to attenuation bias driven by measurement error and transitory shocks in point-in-time observations of parental income. To see how this translates into lifetime economic estimates of mobility, we now consider estimates of intergenerational elasticities and partial correlations using average earnings across the life-cycle for sons and where possible, average incomes in childhood for parents.

As we move to consider lifetime earnings we must deal with the issue of individuals spending time out of work during their lifetime. Up until now, point-in-time estimates of intergenerational persistence ignore spells out of work. Indeed, estimates of life-cycle bias based on current vs. lifetime earnings 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 this analysis). However, as is the case for life-cycle bias, spells out of work will not be randomly allocated across the parental income distribution.

Table 3 illustrates the distribution of workless spells in our data and how this varies by family income. 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 26-38 and parental income at 16 in the NCDS and BCS and a sample which minimises attenuation bias based on earnings at 26-38 and average parental income at 10 and 16 in the BCS.

As can be seen from Panel A, across all four samples the majority of individuals in our data are always employed (63%-80%) although this varies across the life-cycle 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 (consistent with Macmillan, 2014). A minor proportion of the sample (4-14%) have extended periods of worklessness, greater than two years, over their lifetime and a small proportion (1-3%) are never in work.

Panels B and C summarise the average family incomes in childhood and labour market earnings of those who are always in work compared to those experiencing varying degrees of worklessness. Those who always work are from families with greater parental income in childhood than those who experience workless spells and also earn more on average in the labour market. An individual who is never out of work in the NCDS is from a family with £328 income a week on average and earns £534 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 just £297 per week and they earn £349 per week on average over the period (not including periods out of work). In the BCS individuals who are never in work from 26-38 are from families that have incomes at 16 that are 30% lower than individuals who always work.

Table 4 presents estimates of lifetime intergenerational economic mobility in the UK based on average earnings across the life-cycle. We begin by presenting estimates of intergenerational persistence 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. These individuals have earnings for at least one period of time but will be missing from various point-in-time estimates of intergenerational mobility if they are out of work. For now, those who are always out of work are still excluded from the analysis.

The top row of Panel A, Table 4 indicates that lifetime economic intergenerational mobility is around 0.17-0.18 in the NCDS cohort for those who are always working and 0.26 in the BCS. If attenuation bias is taken into account by averaging parental income, this increases to 0.33 in the younger cohort. Introducing individuals with less than two years of workless spells over the period increases intergenerational persistence by around 1 percentage point in each sample while introducing those who are out of work for over two years increases the intergenerational elasticity by a further 1.5 percentage points. Restricting the sample of individuals for whom we estimate intergenerational mobility for to those who are in work attenuates our estimated elasticity by around 0.02-0.03.

Panel B of Table 4 shows the estimated partial correlations for lifetime economic intergenerational mobility, adjusting the intergenerational elasticities for distributional differences across generations. The addition of those who spent spells of time out of work shifts the partial correlation by 0.005-0.015 suggesting that around half of the increased elasticity is working through a re-ordering of sons within their distributions (changing places on the rungs of the ladder) once those with workless experiences are included and the other half is working through a better representation of the distance between the ladder rungs (more inequality) once these individuals are included.

The estimates in Table 4 illustrate the bias from restricting the sample to individuals who are in work. When this is expanded to include those who experience spells out of work, the intergenerational elasticity estimates increase across the board. This analysis does not yet include worklessness in the estimated intergenerational elasticity however: it is simply illustrating that those who are out of work typically come from lower income families and have lower earnings when observed in the data. Table 5 moves to including periods of worklessness in our measures of average lifetime earnings. As discussed in the methodology and data sections, this can be done in a number of ways. We present estimated intergenerational elasticities (Panel A) and partial correlations (Panel B) for three alternative measures of worklessness: zero earnings, income replacement (imputed benefits) and wages foregone through spells out of work. Zero earnings is likely to be an upper bound while wages foregone represents a lower bound for our estimates.

The introduction of spells out of work into our measures of average lifetime earnings for those who are in work for at least some of the period observed increases our estimated intergenerational elasticities by a further 0.001-0.05 depending on what measure of earnings replacement is used. The wages foregone estimates using the selection modelling are very similar to the estimates from the final row of Table 4. Using zero earnings increases the estimated elasticities the most by substantially increasing the variation of earnings (and also therefore reduces the partial correlation in Panel B). The income replacement method, our preferred method, suggests that intergenerational elasticities are attenuated by a further 0.01-0.02 on average by ignoring periods of worklessness in estimating intergenerational persistence using average lifetime economic earnings.

Finally, Tables 4 and 5 have so far not included 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 those who are always workless into 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: the range of estimates across the alternative techniques is 0.11 in the NCDS 23-50 to 0.23 in the BCS 26-38 controlling for attenuation bias. These few individuals (N<100 in all samples) substantially alter the estimated intergenerational elasticities. If periods out of work are measured as zero earnings, up to 60% of lifetime earnings are associated with parental resources in childhood in the later BCS cohort when accounting for measurement error. In our preferred measure of income replacement (imputed benefits) the estimated persistence across generations is attenuated further by 0.02-0.04 when individuals who are always workless are excluded from the analysis. In the NCDS, lifetime intergenerational economic mobility is estimated to be 0.24 on average while in the BCS the estimate of lifetime intergenerational economic mobility is 0.32 without accounting for attenuation bias and 0.40 once attenuation bias is taken into account (although not completely eradicated).

6. Conclusions

In this paper we have made two significant contributions to the current literature on intergenerational economic mobility in the UK. We have considered the role of life-cycle bias and measurement error for the first time in relation to point-in-time estimates of mobility. We have also 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. Each bias has contributed to understating previous estimates of intergenerational economic mobility in the UK: life-cycle bias has led us to understate this by 0.05-0.07, attenuation bias due to measurement error and transitory shocks has led us to understate this by 0.08-0.10 and the exclusion of workless individuals and accounting for spells out of work in measures of earnings has led us to understate this by a further 0.05-0.09.

When all biases are taken into account, it is likely that previous point in time estimates of intergenerational mobility in the UK understated intergenerational economic mobility by 0.18-0.26, or over 45%. The more realistic figure for intergenerational economic mobility in the BCS is around 0.51: 51% of inequalities in sons earnings are transmitted across generations in the UK. While Table 6 suggests that lifetime intergenerational economic mobility is around 0.40 in the BCS, there are two reasons to believe that this estimate still understates true levels of intergenerational persistence: the BCS cohort are only observed until age 38 at present and therefore we are only measuring their lifetime intergenerational economic mobility for the first two-thirds of their working lives (average age 32) and in averaging across two periods of family income we are still not completely eradicating attenuation bias. While the NCDS data suggests that lifetime mobility using earnings from 26-38 understates lifetime mobility 23-50 (average age 37) by around 1 percentage point it may be that life-cycle bias differs in the later BCS cohort at later ages. Future BCS data releases will be able to inform this debate. With regards to averaging in childhood, as noted Gregg et. al. (2013) find that averaging across incomes 6 years apart gives estimated elasticities around 80% of those estimated using parental income averaged across the entire childhood.

Our evidence suggests that, in addition to life-cycle 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. Failing to do so could introduce further substantial attenuation biases to estimates of intergenerational economic mobility.

Table 1: Life-cycle bias in estimates of the intergenerational income elasticity and partial correlation 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)
r	0.050			0.166		0.178	0.173	0.139
	(.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)		
r		0.228	0.286	0.282	0.330	0.291		
		(.026)	(.022)	(.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 partial correlation 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)
r	0.214	0.282	0.290	0.342	0.307
	(.026)	(.022)	(.023)	(.026)	(.024)
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)
r	0.204	0.279	0.282	0.318	0.291
	(.020)	(.017)	(.018)	(.021)	(.018)
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	396.35	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*

Table 4: Lifetime estimates of the intergenerational income elasticity and partial correlation in the UK for those with any earnings by lifetime workless experiences – works cumulatively through the first three lines 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: Partial correlations (r)				
None	0.151 (.021)	0.155 (.020)	0.290 (.020)	0.283 (.016)
N	*2085*	*2408*	*2214*	*3723*
<2 years	0.156 (.018)	0.157 (.018)	0.286 (.020)	0.278 (.015)
N	*2898*	*3034*	*2355*	*3963*
2+ years	0.166 (.017)	0.162 (.017)	0.285 (.019)	0.281 (.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 intergenerational income elasticity and partial correlation in the UK for those with any earnings including periods of worklessness in the measure of lifetime earnings – all first three lines of Table 3together

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
Including workless periods 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: Partial correlations (r)				
Zero earnings	0.165 (.016)	0.158 (.016)	0.238 (.019)	0.225 (.015)
Imputed benefits	0.171 (.016)	0.167 (.016)	0.288 (.019)	0.277 (.015)
Wages foregone (selection)	0.167 (.017)	0.164 (.017)	0.285 (.019)	0.280 (.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 always workless – so adds in last line 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
Including workless periods 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: Partial correlations (r)				
Zero earnings	0.128 (.016)	0.128 (.016)	0.167 (.018)	0.159 (.014)
Imputed benefits	0.172 (.016)	0.169 (.016)	0.279 (.018)	0.268 (.014)
Wages foregone (selection)	0.170 (.016)	0.166 (.016)	0.286 (.018)	0.281 (.014)
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