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Moving Towards Estimating Sons' 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. They also suffer from sample selection issues and further bias driven by spells out of work. We consider these issues together for UK data, the National Child Development Study and British Cohort Study, for the first time. When all three biases are considered, our best estimate of lifetime intergenerational economic persistence in the UK is 0.43 for children born in 1970. Whilst we argue that this is the best available estimate to date, we discuss why there is good reason to believe that this is still a lower bound, owing to residual attenuation bias.

I. 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 has become a focus of extensive policy debates and government initiatives. This renewed focus has been strongly influenced by emerging research findings, with evidence suggesting that the level of mobility in the UK is low by international standards (Blanden, 2013; Corak, 2013; Jerrim and Macmillan, 2015) and declined over time (Blanden *et al.*, 2004; Blanden, Gregg and Machin, 2005).¹ Black and Devereux

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¹Social 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, Gregg and Macmillan, 2013).

(2011) and Jäntti and Jenkins (2013) provide a recent review of this and related literatures internationally.

Whilst the early literature on intergenerational mobility focused on the association between fathers' and sons' 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, 2013, for a full discussion). There are two slightly different focuses here. One is to consider intergenerational mobility as the differences between lifetime incomes or earnings across two complete generations of parents and children. The other assesses the extent to which a child's adult outcomes mirror their childhood circumstances. In this second setting, ideally the degree of intergenerational mobility would be measured as the association between the socio-economic status of parents throughout a person's childhood and their lifetime earnings as an adult. Both approaches are very data-intensive but the second less so. Indeed the former cannot be realistically attempted without access to administrative register data linked across generations which is unavailable in the UK. Our paper therefore attempts to estimate the lifetime intergenerational economic mobility of a birth cohort of sons, focusing on the childhood circumstances and adult destinations of children.

These intensive data requirements mean that the literature on intergenerational economic mobility often approximates lifetime concepts with point in time measures or short periods. This produces three substantive biases that have been addressed within this literature. The first two, attenuation bias and lifecycle bias, have received more attention and have been shown to have significant impacts on the estimation of intergenerational persistence when using point in time measures. 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 full-childhood income (see e.g. Mazumder, 2005).

An alternative, more problematic, 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 used by Dearden, Machin and Reed (1997) for the UK, who use the 1958 birth cohort with sons' earnings measured at age 33 and suggest that attenuation bias is substantial enough to move the estimated intergenerational elasticity (IGE) from 0.24 to the region of 0.55, although the authors note that this is likely to be an upper bound. This approach has similarities with the two-sample two-stage technique, when family income or fathers' earnings are unobserved but other characteristics such as parental education and occupation are (Ermisch and Nicoletti, 2007; Jerrim, Choi and Rodriguez, 2014).

Jenkins (1987) drew attention to the second issue of lifecycle bias, based on the generalized 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 this bias comparing current earnings with lifetime earnings and show that the lifetime IGE can be consistently estimated by using earnings at a specific age where the measurement error in current earnings is approximately classical. The specific age is shown by Böhlmark and Lindquist (2006) to not be stable across gender, cohorts or

countries.² Nybom and Stuhler (2011) argue that the validity of this approach rests on an assumption that does not hold in Swedish data. Therefore estimates based on earnings measured at a point in time in the second generation generate a bias of somewhat uncertain magnitude.

We use longitudinal data to explicitly consider these two biases for the first time in the UK. Dearden *et al.* (1997) are the only UK study to previously consider the effect of attenuation bias on estimates of intergenerational persistence and were restricted to using the problematic two stage approach. We present the first estimates averaging across observations. No study has yet explicitly considered the role of lifecycle bias on estimates of intergenerational economic mobility in the UK.

Moving towards lifetime estimates of sons' earnings enables us to consider a third important issue for the first time for the UK: the impact of spells out of work. Previous UK estimates based on point in time measures have excluded those who have zero earnings at the time of observation. 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, unsurprisingly, disproportionately drawn from poorer family backgrounds (Macmillan, 2014). This issue has been discussed in a number of papers in the US (see Couch and Lillard, 1998; Mazumder, 2005; Dahl and DeLeire, 2008; Chetty *et al.*, 2014a; Mitnik *et al.*, 2015, for an important recent contribution) but has not been applied to the UK context to date.

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 sons' lifetime earnings highlights a materially important third source of bias from estimates of IGEs based on point in time proxy measures, as a result of mis-measurement of lifetime earnings.

When taking the three measurement issues together, we find that estimates of intergenerational economic mobility understate persistence across generations by 14 percentage points based on sons' earnings reported in their early 30s (Blanden *et al.*, 2004). The extent of intergenerational economic mobility in the UK has been overstated to date. Our preferred measure suggests the IGE is 0.43 for sons born in 1970.³ We argue that this is still likely to be an understatement as we can only average over two years of parental income in childhood. Simulations from administrative data (Annual Survey of Hours and Earnings [ASHE]) in the UK suggest that this estimate is around 80% of the true estimate, which would be closer to 0.55.

In the next section, we lay out our modelling approach in more detail and in section III we discuss our data. Section IV presents our results before we end with some brief conclusions.

² Böhlmarm and Lindquist (2006) estimate that this measurement error is approximately classical at age 36 for Sweden and 38 for the US.

³ Note that this estimate is very close to that found in the seminal work by Tony Atkinson and colleagues, using the York Study in Atkinson *et al.*, (1983).

II. Methodology

An idealized estimate of the IGE as a measure of persistence of economic outcomes would measure the relationship between the log of lifetime earnings of an individual in adulthood ($y_i^{\text{son}^*}$) and the log of earnings of the father or income of the parents of the individual throughout childhood ($y_i^{\text{parent}^*}$) as shown in equation (1).

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

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: the joint distribution of parents' and children's ranks, formally known as the copula of the distribution, and the marginal distributions of parent's and children's incomes, reflecting the degree of inequality within each generation (Chetty *et al.*, 2014a). The standard IGE combines the marginal and joint distributions, capturing both the extent of re-ranking across generations and the spread of the income distributions. 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. We also estimate rank-rank coefficients as in equation (2) to contrast estimates of the extent of re-ranking across generations to the combined IGE.

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

While we prefer to focus on the IGE as this gives the most complete picture of the differences in lifetime earnings across family backgrounds, considering both measures offers some useful insight into the nature of each bias. By comparing across the measures as we attempt to reduce each bias, we can evaluate whether this is re-ordering individuals within the distribution or changing the overall variance of the distribution.⁴ As discussed below, the rank-based measure is likely to be less sensitive to measurement issues and therefore may be preferred if point in time measures are all that is available.

Point-in-time estimates of intergenerational economic mobility

Due to the stringent data requirements to measure intergenerational persistence in incomes, much of the previous literature has focused on point in time measures. Instead of observing the desired measure of parental income across childhood ($y_i^{\text{parent}^*}$) or sons' lifetime earnings ($y_i^{\text{son}^*}$) we observe point in time proxies for these (y_{it}^{parent} , y_{it}^{son}) which deviate from the preferred measures through an error term which captures both reporting errors and short-term transitory fluctuations.

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

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

⁴ Although it is noted that there may be factors that affect both the variance of incomes and the ranking of individuals.

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 of those from more affluent families compared to those from more deprived families. This will therefore lead to us understating the true IGE. Focusing on sons' earnings for notational simplicity (although lifecycle bias affects both generations) a measure of sons' earnings at a point in time varies from the lifetime earnings across the lifecycle by some parameter, λ_t from equation (5).

$$y_{it}^{\text{son}} = \lambda_t y_i^{\text{son}^*} + \varepsilon_{it} \quad (5)$$

Assuming no error in the parental income variable, we estimate

$$y_{it}^{\text{son}} = \alpha + \beta y_i^{\text{parent}^*} + e_{it} \quad (6)$$

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

$$\begin{aligned} \text{plim} \hat{\beta} &= \frac{\text{Cov}(\beta y_i^{p^*} + e_{it}, y_i^{p^*})}{\text{Var}(y_i^{p^*})} \text{ and } e_{it} = \lambda_t (\beta y_i^{p^*} + u_i) + \varepsilon_{it} - \beta y_i^{p^*} \text{ so} \\ \text{plim} \hat{\beta} &= \lambda_t \beta + \text{Corr}(y_i^{p^*}, \varepsilon_{it}) \frac{\sigma_{\varepsilon_{it}}}{\sigma_{y^{p^*}}} \end{aligned} \quad (7)$$

When $\lambda_t = 1$, the measurement error is approximately classical and therefore $\hat{\beta}$ provides a consistent estimate of β assuming that $\text{Corr}(y_i^{p^*}, \varepsilon_{it}) = 0$.

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 gender (Böhlmark and Lindquist, 2006). In addition, Nybom and Stuhler (2011) use Swedish tax record data to question whether the assumption $\text{Corr}(y_i^{p^*}, \varepsilon_{it}) = 0$ holds. They suggest that small deviations cause inconsistent estimates of $\hat{\beta}$ 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 across the life course. This provides direct evidence on the shape of the relationship as individuals' age.

Measurement error

The second substantive measurement issue discussed in previous research is that at any point in time, family income 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 estimate

$$y_i^{\text{son}^*} = \alpha + \beta y_{it}^{\text{parent}} + e_{it} \quad (8)$$

Assuming this measurement error is classical as is typical in this literature,⁵ our estimate $\hat{\beta}$ therefore varies from the true β as:

⁵ See Blanden *et al.* (2013) for a discussion of non-classical measurement error in this context.

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

so

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

The OLS estimate therefore gives a lower bound estimate of the true IGE. We apply the method suggested by Solon (1992) of using average income over time to minimize the attenuation bias caused by classical measurement error.

Mazumder (2005) illustrates that the more observations of childhood income available, the more limited the effect of measurement error and transitory shocks on the estimated IGE. Gregg *et al.* (2013) explore this using Swedish administrative data. They find that using just two income observations during childhood measured six years apart, hence breaking the serial correlation in transitory variation across adjacent income observations, can deliver an estimate that is 80% of the true IGE, using income measured across the entire childhood. We can further explore this issue using the administrative ASHE data to see the likely extent of measurement error in UK data. We consider multiple earnings observations for a group of males of a similar age and time to those fathers observed in our data (see Appendix S1 for further details). Comparing a full childhood measure of earnings to an average measure using two observations, six years apart, produces an estimated attenuation bias of 20%. This suggests that the IGE that we estimate in our data is around 80% of the true estimate.

Sons' 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 near lifetime intergenerational economic mobility for sons in the UK for the first time. We estimate as close to equation (1) as we have ever been in the UK. In doing so, we highlight a major restriction of previous research: the exclusion of individuals that are workless. This workless bias has two components. The first is sample selection, where our estimates are not representative of the whole population because they exclude individuals who are found towards the bottom of the income distribution. The second is a methodological issue regarding what to assign to those who are workless as a replacement value for their earnings during periods out of work. We consider both issues separately in our analysis.

A handful of papers, mainly from the US, deal with workless spells in different ways. Some studies, for example, Minicozzi (2003) and Francesconi and Nicoletti (2006), predict the potential earnings of workless individuals using complex methods such as making assumptions on earnings bounds, selection models, and simulations of the present value of lifetime income based on estimated earnings-generating processes. Jäntti *et al.* (2006) deal with zero earners by replacing reported zero earnings with data from other years in which the workers had positive earnings. Other studies simply assign different values to the zero earners to be able to compute the standard IGE (Mazumder, 2005; Chetty *et al.*, 2014a,b; Mitnik *et al.*, 2015). Drewianka and Mercan (2009) and Mazumder (2005) explore how

IGE estimates change when imputing both fathers' and sons' zero earnings. The majority of these studies agree that the IGE tends to be higher when workless spells are included.

When measuring sons' lifetime earnings, employment and earnings are important in their own right, not just as sources of income. There is therefore value in showing both sons' lifetime earnings including zero earnings (observed employment shocks to earnings) and sons' 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. This is often how short spells out of work are considered in data that employ a measure of annual earnings. Yet this may not be a true representation of the individual's available resources and significantly increases inequality in the earnings distribution. If this approach is applied when a person is out of work for a whole year then it also requires a value to be given to attain a defined value when logged (often 1 as is the case here). For these reasons, we prefer the second method, earnings replacement, which imputes the average benefit level available at the time of the workless spell. 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.⁶

Note that while our IGE estimates will be sensitive to this choice, rank based measures are more robust to alternative specifications as they are scale invariant. Mitnik *et al.* (2015) also present an alternative Poisson pseudo max likelihood estimator, which provides stable estimates regardless of the choice of value to assign to workless spells.

IGE compared to rank-rank measures

All three measurement issues have two aspects that can be conceptually separated and developed analytically. Measurement error, lifecycle bias and bias from workless spells 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 realized 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 earnings gaps between the less and better educated will be understated: scale mis-measurement.

The alternative estimation approach we adopt utilizing rank-based estimation removes the issue of scale mis-measurement leaving just the positional accuracy concern. Rank-rank measures are therefore not attenuated in the usual classical manner but can still be affected by non-classical measurement error that affects both the parents' and the sons' measures. While these measures are less sensitive to error in the tails of the distribution, they can be more sensitive to non-classical measurement error around the middle of the distribution. These measures are also likely to be less sensitive to lifecycle bias, particularly at older ages, as the spread of incomes becomes a central driving force rather than position

⁶ In an earlier version of this paper (Gregg, Macmillan and Vittori, 2014), we also use a sample selection model. The results from this method are very similar to treating people as always working.

(Nybom and Stuhler, 2015). We would also expect these measures to be less sensitive to issues of workless spells in the data as they remove questions about what value to assign to those who are out of work.

By comparing the IGE regression coefficients to the rank-rank coefficients throughout our analysis we can therefore comment on the relative effects of scale mis-measurement and positional inaccuracy from each bias. A priori we would expect the three biases discussed here to be smaller in magnitude in the rank based measure.

III. 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 note that as we move to focus on lifetime earnings including spells out of the labour market, this methodology takes us towards a future study of intergenerational mobility for women.

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.⁷ 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

⁷We have explored the implications of measurement error from individuals' choosing a band rather than giving an actual value in the cohort studies by using administrative data and find this to be minimal. Further details are available in Appendix S1.

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 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 Appendix S1 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. Our results are robust to restricting our sample to only those with observed income in both periods (see Footnote 13 for more details).

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 across almost their entire working lives (23–50). For the BCS cohort we can observe monthly earnings at various points in time across two thirds of their working lives (26–42). 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 Appendix S1). 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 NCDS⁸ 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 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 II, two alternative values are assigned to those who

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

are observed out of work in any given month: zero⁹ and earnings replacement. 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.¹⁰

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 16.¹¹ 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 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 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 nearly always workless. These sample constraints restrict the available sample to 3,453 in the NCDS and 4,312 in the BCS. The representativeness of our final sample is discussed in detail in Appendix S1.

IV. 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 has declined over time in the UK or alternatively 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.

⁹The value of £1 is assigned to allow us to take the logarithm of earnings.

¹⁰Our imputed values are very close to the average income replacement rates reported by the Institute for Fiscal Studies.

¹¹Consistent with previous studies (see the Appendix B of Blanden, Gregg and Macmillan, 2011, for discussion).

TABLE 1

Lifecycle bias in estimates of the intergenerational income elasticity (IGE) and Rank Coefficient in the UK

| <i>NCDS</i> | | | | | |
|------------------------|------------------|------------------|------------------|------------------|------------------|
| <i>Age of earnings</i> | 23 | 33 | 42 | 46 | 50 |
| β | 0.042 (0.020) | 0.205 (0.026) | 0.291 (0.034) | 0.259 (0.026) | 0.224 (0.039) |
| Rank-rank coefficient | 0.065 (0.024) | 0.199 (0.021) | 0.218 (0.021) | 0.183 (0.024) | 0.175 (0.024) |
| <i>SD inc</i> | 0.397 | 0.379 | 0.390 | 0.383 | 0.383 |
| <i>SD earns</i> | 0.334 | 0.464 | 0.633 | 0.568 | 0.612 |
| <i>N</i> | 1,803 | 2,161 | 2,213 | 1,653 | 1,709 |
| <i>BCS</i> | | | | | |
| <i>Age of earnings</i> | 26 | 30 | 34 | 38 | 42 |
| β | 0.203 (0.023) | 0.291 (0.022) | 0.324 (0.027) | 0.385 (0.031) | 0.397 (0.033) |
| Rank-rank coefficient | 0.258 (0.026) | 0.305 (0.021) | 0.322 (0.023) | 0.337 (0.027) | 0.338 (0.024) |
| <i>SD inc</i> | 0.480 | 0.479 | 0.476 | 0.487 | 0.486 |
| <i>SD earns</i> | 0.418 | 0.475 | 0.534 | 0.554 | 0.649 |
| <i>N</i> | 1,416 | 1,976 | 1,691 | 1,265 | 1,596 |

Note: Standard errors in parenthesis. BCS, British Cohort Study; NCDS, National Child Development Study.

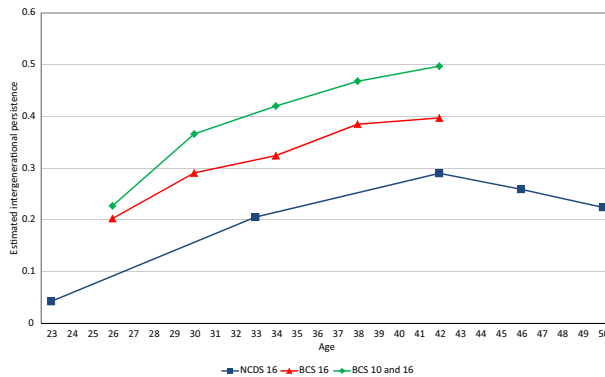


Figure 1 Lifecycle bias in estimates of the intergenerational income elasticity and partial correlation in the UK

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 and 0.291 by age 42 before declining to 0.259 at age 46 and 0.224 at age 50 (as illustrated in Figure 1).¹² In the BCS

¹² Given that differences are not statistically significant we would not emphasize a decline. However, the literature on the bias due to lifecycle effects for other countries shows a similar age profile for estimates of the correlation between point in time earnings at each age and lifetime earnings (see Böhlmarm and Lindquist, 2006; Haider and Solon, 2006).

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

| <i>Age of earnings</i> | <i>26</i> | <i>30</i> | <i>34</i> | <i>38</i> | <i>42</i> |
|--|------------------|------------------|------------------|------------------|------------------|
| <i>Panel A: Imputing income at 10 if missing</i> | | | | | |
| β | 0.225 (0.027) | 0.345 (0.026) | 0.396 (0.032) | 0.478 (0.037) | 0.506 (0.039) |
| Rank-rank coefficient | 0.242 (0.026) | 0.306 (0.023) | 0.331 (0.025) | 0.343 (0.028) | 0.347 (0.025) |
| <i>SD inc.</i> | 0.422 | 0.419 | 0.422 | 0.420 | 0.421 |
| <i>N</i> | 1,416 | 1,976 | 1,691 | 1,265 | 1,596 |
| <i>Panel B: Imputing income at 10 or 16 if missing</i> | | | | | |
| β | 0.227 (0.022) | 0.366 (0.022) | 0.420 (0.031) | 0.468 (0.031) | 0.497 (0.032) |
| Rank-rank coefficient | 0.235 (0.020) | 0.301 (0.017) | 0.319 (0.019) | 0.323 (0.021) | 0.318 (0.018) |
| <i>SD inc.</i> | 0.389 | 0.383 | 0.385 | 0.386 | 0.387 |
| <i>N</i> | 2,364 | 3,340 | 2,806 | 2,080 | 2,685 |

Note: Standard errors in parenthesis. Dummy variable included if income is imputed. BCS, British Cohort Study.

(lower panel Table 1 and middle line on Figure 1) we can see a similar pattern emerging across the lifecycle with estimated intergenerational persistence increasing from 0.203 at age 26 to 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 IGE is 0.119 and 0.106 percentage points higher in the later cohort. 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).

When considering the estimated rank-rank coefficients, which attempts to remove scale measurement issues, these show a similar, although less pronounced, pattern across the lifecycle, as expected. In both cohorts, the rank based coefficient rises sharply to age 30/33 and then continues to increase gradually, peaking at 42. This suggests that lifecycle bias is indeed driven mainly by scale (the mis-measurement of earnings gaps between better and less well educated individuals) rather than positional accuracy issues. Therefore estimates based on earnings observed relatively early in the lifecycle, such as those in Chetty *et al.* (2014a), benefit from focusing on the rank-based measure rather than the IGE as these estimates are more reliable from early 30s.

We next 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 minimize 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 we cannot present comparable estimates 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 sample as 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 who report income at 16 do not report income at 10, see Appendix S1 for further details). Comparing the estimates from Panel B of Table 1 to those from Panel A of Table 2, the estimated IGE correcting for measurement error is larger at all ages and increasing with age: 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 from measurement error is large and 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.¹³ The lifecycle movements in these estimates are also shown as the upper line in Figure 1 for comparison. As seen in Panel B of Tables 1 and 2, the rank-rank coefficients are very close to those observed when using a point in time measure of parental income at age 16. This is to be expected given that rank-based measures are less affected by error in the tails of the distribution where averaging is likely to be more important.

Note that by averaging across two periods we are not fully dealing with issues of attenuation bias. Analysis of the administrative ASHE data for the UK suggests that the estimates in Table 2 are likely to represent around 80% of the total estimated IGE if parental income were observed in every year across childhood (see Appendix S1 for full details). Therefore these estimates likely still understate the true IGE by at least 20% (or 0.1 at age 38).

¹³ Our estimates are very stable if we restrict our sample to only those who we observe parental income for at both 10 and 16. For example, the estimated IGE at age 34 and 38 are 0.408 and 0.519 respectively and the rank-rank coefficients are 0.296 and 0.332 for this sample (full results available from the authors on request). This suggests that it is the reduction in measurement error and transitory variation brought about by averaging that are driving our results, rather than our imputation method and the resulting increase in sample.

TABLE 3
Frequency of worklessness across the lifecycle and by family background

| <i>Cohort:</i> | <i>NCDS</i> | <i>NCDS</i> | <i>BCS</i> | <i>BCS</i> |
|--|-------------|-------------|------------|------------|
| <i>Earnings life cycle period:</i> | 23–50 | 26–42 | 26–42 | 26–42 |
| <i>Family income observed at:</i> | 16 | 16 | 16 | 10/16 |
| <i>Time spent workless</i> | | | | |
| <i>Panel A: Frequency of sample (%)</i> | | | | |
| 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 |
| <i>N</i> | 3,453 | 3,453 | 2,543 | 4,312 |
| <i>Time spent workless</i> | | | | |
| <i>Panel B: Average weekly family income (2000 £s)</i> | | | | |
| 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 |
| <i>N</i> | 3,453 | 3,453 | 2,543 | 4,312 |
| <i>Time spent workless</i> | | | | |
| <i>Panel C: Average weekly earnings (2000 £s)</i> | | | | |
| 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 |
| <i>N</i> | 3,453 | 3,453 | 2,543 | 4,312 |

Notes: 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. BCS, British Cohort Study; NCDS, National Child Development Study.

Sons' lifetime intergenerational economic mobility

To minimize the impact of both lifecycle and attenuation bias on our estimated IGEs, we move towards lifetime measures of 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 of sons 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 minimizes attenuation bias based on earnings at 26–42 and average parental income at 10 and 16 in the BCS.

As we move to consider sons' lifetime earnings we must deal with spells out of work. 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 (consistent with lifecycle bias in workless experiences, 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 almost never in work.

Panels B and C summarize 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 to 50. If we compare this to an individual who is out of work for over two years from 23 to 50, their family income in childhood 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 to 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. We begin by presenting estimates of sons' 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 estimates of sons' lifetime intergenerational economic mobility based on sons' (near) 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 current estimate of 0.30 is getting close to a lifetime estimate. Addressing attenuation bias by using averaged family income rather than income at a point in time 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 IGE by a further 1–2.5 percentage points. Overall, restricting the sample of individuals for whom we estimate intergenerational mobility for in previous point in time estimates to those who are 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

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

| <i>Cohort:</i> | <i>NCDS</i> | <i>NCDS</i> | <i>BCS</i> | <i>BCS</i> |
|---|------------------|------------------|------------------|------------------|
| <i>Earnings life cycle period:</i> | 23–50 | 26–42 | 26–42 | 26–42 |
| <i>Family income observed at:</i> | 16 | 16 | 16 | 10/16 |
| <i>Time spent workless</i> | | | | |
| <i>Panel A: Intergenerational elasticities (β)</i> | | | | |
| None | 0.178 (0.025) | 0.183 (0.023) | 0.298 (0.021) | 0.372 (0.020) |
| <i>SD earns</i> | 0.456 | 0.456 | 0.475 | 0.478 |
| <i>N</i> | 2,085 | 2,408 | 2,214 | 3,723 |
| <2 years | 0.188 (0.022) | 0.190 (0.022) | 0.299 (0.020) | 0.371 (0.020) |
| <i>SD earns</i> | 0.463 | 0.467 | 0.483 | 0.486 |
| <i>N</i> | 2,898 | 3,034 | 2,355 | 3,963 |
| 2+ years | 0.212 (0.021) | 0.207 (0.021) | 0.302 (0.020) | 0.383 (0.020) |
| <i>SD earns</i> | 0.488 | 0.489 | 0.491 | 0.497 |
| <i>N</i> | 3,400 | 3,400 | 2,464 | 4,170 |
| <i>Time spent workless</i> | | | | |
| <i>Panel B: Rank-rank coefficient</i> | | | | |
| None | 0.180 (0.021) | 0.188 (0.020) | 0.307 (0.021) | 0.300 (0.016) |
| <i>N</i> | 2,085 | 2,408 | 2,214 | 3,723 |
| <2 years | 0.188 (0.018) | 0.190 (0.018) | 0.305 (0.020) | 0.295 (0.016) |
| <i>N</i> | 2,898 | 3,034 | 2,355 | 3,963 |
| 2+ years | 0.194 (0.017) | 0.192 (0.017) | 0.306 (0.020) | 0.300 (0.015) |
| <i>N</i> | 3,400 | 3,400 | 2,464 | 4,170 |

Notes: 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 Panel B. They are not repeated in Panel B for this reason. BCS, British Cohort Study; IGE, intergenerational elasticity; NCDS, National Child Development Study.

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 sons' lifetime earnings used and therefore in the estimated IGE. Table 5 moves to including periods of worklessness in our measures of sons' average lifetime earnings. As discussed in sections II and III, this can be done in a number of ways. We present estimated IGEs (Panel A) and rank-rank coefficients (Panel B) for two alternative measures of worklessness: zero earnings and earnings replacement from imputed welfare benefits.

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)

| 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 |
| <i>Panel A: Intergenerational elasticities (β)</i> | | | | |
| Ignoring workless spells | 0.212 (0.021) | 0.207 (0.021) | 0.302 (0.020) | 0.383 (0.020) |
| <i>SD earns</i> | 0.488 | 0.489 | 0.491 | 0.497 |
| Including workless spells as: | | | | |
| Zero earnings | 0.255 (0.025) | 0.255 (0.026) | 0.343 (0.028) | 0.425 (0.028) |
| <i>SD earns</i> | 0.594 | 0.618 | 0.670 | 0.687 |
| Imputed benefits | 0.232 (0.022) | 0.230 (0.023) | 0.320 (0.021) | 0.398 (0.021) |
| <i>SD earns</i> | 0.522 | 0.530 | 0.515 | 0.523 |
| <i>N</i> | 3,400 | 3,400 | 2,464 | 4,170 |
| <i>Panel B: Rank-rank coefficient</i> | | | | |
| Ignoring workless spells | 0.194 (0.017) | 0.192 (0.017) | 0.306 (0.020) | 0.300 (0.015) |
| Zero earnings | 0.194 (0.017) | 0.194 (0.016) | 0.308 (0.020) | 0.298 (0.015) |
| Imputed benefits | 0.194 (0.016) | 0.195 (0.016) | 0.308 (0.020) | 0.298 (0.015) |
| <i>N</i> | 3,400 | 3,400 | 2,464 | 4,170 |

Notes: 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 Panel B. They are not repeated in Panel B for this reason. BCS, British Cohort Study; IGE, intergenerational elasticity; NCDS, National Child Development Study.

The first row in each panel of Table 5 replicates the final rows of Panels 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. Our estimated IGE is therefore further attenuated by ignoring these spells out of work in our measure of average lifetime earnings. The rank-rank coefficients (Panel B) are essentially unchanged regardless of which value we choose to use for workless spells. This confirms 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

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

| <i>Cohort:</i> | <i>NCDS</i> | <i>NCDS</i> | <i>BCS</i> | <i>BCS</i> |
|---|------------------|------------------|------------------|------------------|
| <i>Earnings life cycle period:</i> | 23–50 | 26–42 | 26–42 | 26–42 |
| <i>Family income observed at:</i> | 16 | 16 | 16 | 10/16 |
| <i>Panel A: Intergenerational elasticities (β)</i> | | | | |
| Ignoring workless spells | 0.212 (0.021) | 0.207 (0.021) | 0.302 (0.020) | 0.383 (0.020) |
| <i>SD earns</i> | 0.488 | 0.489 | 0.491 | 0.497 |
| Including workless spells as: | | | | |
| Zero earnings | 0.363 (0.045) | 0.366 (0.046) | 0.523 (0.056) | 0.654 (0.056) |
| <i>SD earns</i> | 1.091 | 1.103 | 1.458 | 1.494 |
| Imputed benefits | 0.252 (0.023) | 0.251 (0.024) | 0.345 (0.022) | 0.430 (0.022) |
| <i>SD earns</i> | 0.564 | 0.572 | 0.577 | 0.584 |
| <i>N</i> | 3,453 | 3,453 | 2,543 | 4,312 |
| <i>Panel B: Rank-rank coefficient</i> | | | | |
| Ignoring workless spells | 0.194 (0.017) | 0.192 (0.017) | 0.306 (0.020) | 0.300 (0.015) |
| Zero earnings | 0.195 (0.016) | 0.196 (0.016) | 0.306 (0.019) | 0.297 (0.015) |
| Imputed benefits | 0.195 (0.016) | 0.196 (0.016) | 0.306 (0.019) | 0.297 (0.015) |
| <i>N</i> | 3,453 | 3,453 | 2,543 | 4,312 |

Notes: 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 Panel B. They are not repeated in Panel B for this reason. BCS, British Cohort Study; NCDS, National Child Development Study.

in adulthood. Table 6 replicates Table 5 including the additional individuals who are (nearly) 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 age 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 ignoring periods of worklessness. 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. A doubling of family

income in childhood is associated with an increase in sons' lifetime earnings as an adult of 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. Analysis of UK administrative data suggests that this will give approximately 80% of the true IGE. Adjusting for this would place the IGE for the UK at around 0.55.

V. Conclusions and discussion

This paper has made three significant contributions to the current literature on intergenerational economic mobility for the UK. 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. Perhaps more significantly, we have estimated sons' 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 the widely reported previous estimates of intergenerational economic mobility in the UK, by Blanden *et al.* (2004, 2005), which include authors on this paper, have understated the true extent of the UK mobility problem.¹⁴ Lifecycle bias is shown to have led to an understatement of the IGE by relatively little (around 0.01) compared to those from Blanden *et al.* (2004), even though earnings were observed at relatively early ages in previous studies. Attenuation bias, due to measurement error and transitory shocks, leads to a much more serious understatement of the IGE by 0.08–0.10. The exclusion of workless individuals and accounting for spells out of work in measures of sons' lifetime earnings has led to a further understating of the IGE of around 0.05.

While our final estimates suggest that sons' (near) lifetime intergenerational economic mobility is currently around 0.43 in the BCS, there is good reason to believe that this estimate still understates true levels of intergenerational persistence. Taking an average of parental income across only two periods (albeit six years apart and hence reducing serial correlation) is not likely to completely eradicate attenuation bias. Administrative data from the UK suggests that this estimate would be around 80% of the true IGE based on complete childhood incomes, resulting in an estimated IGE of 0.55.

A further issue is that both cohorts are unable to yet inform us on patterns of labour market exit as individuals approach retirement. While it is difficult to extrapolate as to future trends in the IGE based on later ages, evidence from Macmillan (2014) suggests that more educated individuals exit the labour market at a faster rate as they approach retirement. This would work in the opposite direction to current trends, reducing the lifetime IGE. 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 for sons removes some of the problematic assumptions that exist within the current literature on

¹⁴ Although the trends in changes in mobility over time are very similar here to those reported previously.

lifecycle bias and starts to provide a true picture of lifetime 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. The magnitude of this bias is greater than that of lifecycle bias in UK data and more than half the size of the bias driven by measurement error.

An alternative approach to dealing with these measurement issues is to use measures of mobility 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 and thus make comparisons across time and across countries more reliable in the face of data limitations. 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 the public policy discussion (Corak, 2013; Jerrim and Macmillan, 2015).

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Supporting Information

Additional information on sample selection, measurement error and imputation procedures can be found in the online Appendix:

Appendix S1. Sample selection, measurement error and imputation procedures.