

DISCUSSION PAPER SERIES

IZA DP No. 11020

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**Chelsea Murray**

*University of Wollongong*

**Robert Clark**

*Australian National University*

**Silvia Mendolia**

*University of Wollongong and IZA*

**Peter Siminski**

*University of Technology Sydney,  
University of Wollongong and IZA*

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## ABSTRACT

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# Direct Measures of Intergenerational Income Mobility for Australia

We present the first Australian estimates of intergenerational mobility that draw on direct observations of income from two generations. Using panel data for three birth cohorts of young adults from the Household, Income and Labour Dynamics Australia survey, the estimated intergenerational income elasticity is 0.28. Correcting for attenuation bias raises this to 0.41. We estimate the rank correlation to be 0.27. We show that Australia has greater mobility than the US and this is not sensitive to methodological choices. We also show that spousal selection and family structure may be important determinants of income persistence across generations.

**JEL Classification:** J62

**Keywords:** intergenerational mobility, Australia, HILDA

**Corresponding author:**

Peter Siminski  
School of Accounting, Economics, and Finance  
University of Wollongong  
Northfields Avenue  
Wollongong, NSW 2522  
Australia  
E-mail: [siminski@uow.edu.au](mailto:siminski@uow.edu.au)

## 1. Introduction

Equality of opportunity is generally considered as an important goal for society.<sup>1</sup> Australians, in particular, tend to value the principle of egalitarianism, a characteristic that has historically distinguished the Australian community from the class structures typical of more established societies in the United Kingdom and Western Europe (Argy 2006; Leigh 2007). A society is characterised by equality of opportunity if all individuals have the same chances to move up (or down) the social hierarchy, regardless of family background.

Aside from being central to the concept of fairness, there are strong economic motivations for equality of opportunity being a desirable social outcome. As discussed by a number of authors (Argy 2006; D'Addio 2007; Cobb-Clark 2010; OECD 2010), barriers to lifetime achievement can hinder economic efficiency because the aptitudes and abilities of some individuals are more likely to be misallocated or underutilised. Inequality of opportunity can also have implications for social cohesion and society's faith in the political system (Argy 2006; Cobb-Clark 2010). Measuring the extent to which people face equal opportunities is hence of interest to policymakers both on equity grounds and for efficiency reasons.

Due to challenges in defining and measuring opportunities, most studies that seek to quantify equality of opportunity do so indirectly by studying intergenerational mobility (Chetty et al. 2014a).<sup>2</sup> Intergenerational mobility refers to the association between a child's socioeconomic outcomes as an adult and those of his or her parents, and has long been acknowledged as an indicator of the degree of equality of opportunity (Becker & Tomes 1986).

To our knowledge, four previous studies have estimated the extent of intergenerational earnings mobility in Australia (Leigh, 2007; Mendolia and Siminski, 2016; Huang et al., 2016; Fairbrother and Mahadevan, 2016).<sup>3</sup> None have estimated the extent of income

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<sup>1</sup> See, for example, Argy (2006); Vogel (2006); D'Addio (2007); Black and Devereux (2011); Blanden (2013); Jäntti and Jenkins (2015); and Mendolia and Siminski (2017).

<sup>2</sup> While less prevalent than studies of intergenerational mobility, there are studies that attempt to measure equality of opportunity directly; see, for instance, Bourguignon et al. (2007), Lefranc et al. (2008), Checchi and Peragine (2010), and Ferreira and Gignoux (2011).

<sup>3</sup> Intergenerational mobility in Australia has been studied from a sociological perspective for some decades. That literature has studied mobility with respect to occupation and educational outcomes. This literature shows that family background has an important impact on individual chances of success (see for example Radford, 1962; and more recently, Evans and Kelley, 2002). The emergence of high quality panel data from the HILDA Survey has enabled research into intragenerational (year-on-year) mobility. Chesters (2015) examined the role of mature-age education participation in intragenerational earnings and occupational mobility, finding that the

mobility using direct observations of income from two generations. The lack of research in this area may be due to a scarcity of longitudinal datasets suitable for intergenerational studies. These four studies have adopted the approach of imputing parental earnings on the basis of occupation. As highlighted by Leigh (2007), Mendolia and Siminski (2016) and Huang et al. (2016), the use of imputed parental earnings is a rather crude approach. Variation in earnings within a given occupation is not taken into account. In addition, since the imputation procedure is based on information from the child's sample, it assumes that the occupational wage structure is the same in each generation. To the extent that intergenerational mobility is driven by children receiving higher or lower earnings in the same occupation as their parents, or changes in the occupation-earnings structure over time, imputed earnings may be an inaccurate proxy for true parental earnings, resulting in mismeasurement of the IGE. It is also possible that retrospective reports of parental information are subject to measurement error and recall bias, further contributing to measurement error in predicted parental earnings (Wooden & Watson 2000).<sup>4</sup>

The first elasticity estimate for Australia (Leigh, 2007) places Australia as relatively mobile in an international context, given its level of inequality (see Figure 1 in Corak 2013, p. 82). An update to Leigh's study by Mendolia and Siminski (2016), however, finds the level of mobility in Australia to be considerably lower. They follow Leigh's methodology, but pool 12 waves of data (2001–2012), substantially increasing the sample size and reducing sampling variability. Importantly, they show that the elasticity in 2004 (the year for which Leigh's estimate is calculated) is somewhat lower than the IGE for other years in their sample. Their preferred estimate of 0.35 is substantially higher than Leigh's and implies that intergenerational mobility in Australia is not particularly high relative to other OECD countries and is consistent with the level of inequality. Both Leigh's (2007) and Mendolia

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completion of undergraduate and postgraduate degrees has a significant positive effect on occupational prestige and earnings, respectively. Wilkins and Warren (2012) studied year-on-year changes in income ranking between 2001 and 2009. Research on the intergenerational transmission of economic outcomes is more limited. It generally shows that young people from disadvantaged backgrounds are more likely to experience negative income and socio-economic outcomes (Pech and McCoull, 2000; Cobb-Clark et al., 2012).

<sup>4</sup> A common approach to navigate such issues is to calculate a benchmark IGE estimate for the US based on full earnings histories, and a second US estimate using the same approach as used for the country with inferior data. Mendolia & Siminski (2016) use this approach for Australia, following Leigh (2007). They estimate an IGE for Australia (based on imputation), then scaled it up by the ratio between the two US estimates, in order to arrive at a 'corrected' estimate for Australia. A similar technique has been used in Corak (2013) to obtain a number of cross-country comparative estimates used to construct a version of the 'Great Gatsby curve', which plots the IGE against an index of inequality for 13 countries.

and Siminski's (2016) preferred estimates include an adjustment for the downward bias that arises from the imputation of parental earnings.

Huang *et al.* (2016) employ a methodological variation in the use of a random effects model in the second stage IGE estimating equation. As in Mendolia and Siminski (2016), the sample is pooled over multiple waves of data (2001–2013). Their preferred IGE estimate range of 0.24–0.28, which does not adjust for bias due to imputation of parental earnings, is slightly higher than Mendolia and Siminski's (2016) unadjusted estimate of 0.23. Another recent Australian study, by Fairbrother and Mahadevan (2016), applies the same method as Mendolia and Siminski (2016) and relies on 13 waves of HILDA Survey data. Their father-son elasticity estimate of 0.20 (which also does not include an adjustment for the bias due to imputation) is somewhat lower than the unadjusted estimates in the two other recent studies.<sup>5</sup>

Our paper makes a number of contributions to the Australian literature. It presents the first estimates of intergenerational mobility for Australia that are based on directly observed income for parents and their children. We assert that HILDA is now mature enough for such a study, albeit with some caveats and adjustments. We pay particular attention to the extent of life-cycle biases, and attenuation due to measurement error. Secondly, the use of direct income measures facilitates comparisons of elasticities based on various household and individual measures of income and earnings. Such comparisons reveal that household-level dynamics, such as spousal selection and family structure, may be important determinants of the persistence of income across generations in Australia. Finally, while previous studies from Australia focus primarily on the IGE and to a lesser extent the IGC and transition matrices, we also present the first Australian estimates of the rank correlation. Consistent with other studies, we find the rank correlation to be more stable with respect to minor methodological choices and less prone to the types of bias that affect IGE estimates. We estimate a number of comparable measures of mobility for Australia and the US (PSID) and find that Australia is more mobile than the US, regardless of which approach is used.

The remainder of the paper is structured as follows: Sections 2 and 3 describe data and methods. The main results are presented in Section 4. Section 5 presents a detailed assessment of potential sources of bias. Section 6 presents a comparative assessment of mobility in Australia and the US, and Section 7 concludes.

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<sup>5</sup> The primary aim of this paper is to investigate transmission mechanisms that can potentially explain differences in elasticity estimates across gender and levels of parental education attainment, as opposed to producing an elasticity estimate suitable for international comparisons (Fairbrother & Mahadevan 2016).

## 2. Data

We use 15 waves of the Household Income and Labour Dynamics Australia (HILDA) Survey, which is a nationally representative panel survey initiated in 2001. The survey collects information about economic and subjective well-being, labour market dynamics and family dynamics from in-scope respondents on an annual basis, via face-to-face interviews and self-completed questionnaires (Summerfield et al. 2016).

The sample construction and variable definitions in this study closely follow the approach adopted by Chetty et al.'s (2014b) influential US study. This decision was made for transparency and because the challenges of working with a short panel are shared by the two studies. Chetty et al. drew on federal income tax records spanning 1996–2012.

While of comparable length, there are several differences between the tax data used by Chetty et al. (2014b)'s and the HILDA survey data. Chetty et al. (2014b)'s tax data cover the entire population, while HILDA is a nationally representative sample; hence, sample sizes in Chetty et al. (2014b)'s study are much larger. Second, definitions of income and other variables used in each dataset are not exactly comparable; measurement errors are likely to take a different form in each dataset; and attrition and missing observations are significant issues with the HILDA Survey data that are not addressed in Chetty et al. (2014b)'s study. Finally, the HILDA Survey dataset is two years shorter than the population tax data, meaning that children's incomes are observed at different ages in this study than in Chetty et al. (2014b)'s study. Therefore we do not treat results from the two studies as comparable.<sup>6</sup>

The following sub-sections describe how the analysis sample is constructed and summarise key variable definitions, highlighting any departures from Chetty et al. (2014b)'s approach.

### 2.1 Sample Construction

Following Chetty et al. (2014b), parents are identified as the first individuals recorded as the child's mother or father, irrespective of whether the reported mother or father subsequently

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<sup>6</sup> Instead, we draw on the US Panel Study of Income Dynamics PSID to construct comparable estimates between countries in Section 5.

changes to a different individual.<sup>7</sup> A child who initially reports having two parents and later reports having only one parent is considered matched to two parents over the entire period. In the case that a child's parents separate and the child resides in two households, this rule allows for both parents' incomes to be considered even if only one parent is recorded as a co-resident in the HILDA Survey. Conversely, where a child is initially matched to one parent and is later matched to two parents, they will be considered a single-parent child until the second parent joins the household, at which point both individuals are regarded as the child's parents.

We again follow Chetty et al. (2014b) in selecting three birth cohorts to construct the sample of children. The children in these birth cohorts were aged between 15 and 17 in the first wave of data (i.e. are born between 1984 and 1986). There is a trade-off between lifecycle bias and sample selection bias inherent in the choice of birth cohorts—using cohorts born in earlier years allows income to be observed when children are older, thus reducing left-side lifecycle bias. Since children begin to move out of the parental household in their late teens, however, using earlier cohorts increases the likelihood of over-sampling children who stay at home later in life, potentially compromising the representativeness of the sample. Chetty et al. (2014b) limit their analysis to children aged 16 and younger in the first year of the sample because the percentage of children matched to parents drops sharply for earlier cohorts. In the HILDA Survey data, in contrast, the percentage of children matched to at least one parent remains very high (around 95 percent) for children who are 17 years old and younger in 2001, falling to 85 percent for the next oldest cohort. Including the 1984–1986 birth cohorts thus represents a compromise between observing the children's income as close as possible to mid-life, while mitigating potential sample selection bias.

The analysis sample consists of 489 parent-child pairs, which represent 56.6 percent of the children born in 1984–1986 who participated in the survey in 2001. Whilst such attrition is consistent with reported HILDA retention rates, attrition is a threat to the validity of a study such as ours. Appendix A considers attrition in detail, and makes comparisons of the observable characteristics of the estimation sample and the full 2001 sample.

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<sup>7</sup> Subsequent changes to the reported mother or father are ignored for simplicity. Only 1.2 percent of children in the core sample report a different mother or father between 2001 and 2005 (when parental income is measured) to the individual first identified as their mother or father.



## 2.2 Variable Definitions

This section defines the key variables used to measure intergenerational mobility. All monetary variables are measured in 2011 dollars, adjusted for inflation using the consumer price index (CPI).

Various measures of household and individual income are available in the HILDA Survey. Following Chetty et al. (2014b), the primary measure used in this study is household financial year gross total income, which is the sum across all household members of financial year market income, private transfers, Australian and foreign pensions and benefits and irregular income (Summerfield et al. 2016). For comparative purposes, intergenerational mobility is also calculated with respect to other income measures, namely: hourly wages and salary; financial year gross wages and salary; financial year gross total income; household financial year regular private income; household financial year disposable total income; and equivalised household income. Definitions of these income measures can be found in Appendix B. All income measures are subject to weighted top-coding, which substitutes an average value for all observations that are equal to or exceed a given threshold (Summerfield et al. 2016).

The following sub-sections describe how the household total income of parents and children were constructed in more detail. Equivalent procedures were used to construct the other income measures.

### 2.2.1 Parent Income

We follow Chetty et al. (2014b) for parent and child income definitions. Annual parental income is computed as a single parent's household income if one parent is identified in a given year, or the mean of the mother's and father's household income if two parents are identified.<sup>8</sup> The overall parental income variable is then taken to be the mean of non-missing annual parental income observations over the five years from 2001 to 2005, including observations of zero income.<sup>9</sup>

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<sup>8</sup> As highlighted by Chetty et al. (2014b), household measures of income increase with co-residence of parents and do not account for the size of the household. To assess whether these features generate bias, results are also presented using individual measures of income and equivalised household income.

<sup>9</sup> Missing values due to item non-response are imputed in the HILDA Survey data (Wilkins & Warren 2012), and hence the number of individuals with missing income values is relatively small—approximately 12 percent of the sample of parents and 7.5 percent of the sample of children have a missing income value in at least one year.

An advantage of averaging both parents' household incomes over the simpler method of using the child's household income is that it allows both parents' incomes to be considered if they separate after the commencement of the HILDA Survey. Following Chetty et al. (2014b), the earliest years of data available are used to construct the parental income variable in order to best reflect the economic resources accessible to a child while they are growing up. Given that the median parent age in the sample at 2001 is 44, the earliest years of income data are also the closest to midlife for the majority of parents, which is the age at which right-side lifecycle bias is likely to have the smallest influence. Averaging over five years of income reduces the effects of transitory fluctuations in income, thereby decreasing measurement error in the explanatory variable (Solon 1992).

### **2.2.2 Child Income**

Child income is constructed similarly to parental income, with annual household income averaged over the last two years in the dataset (2014 and 2015). The most recent years of data available are used so that child's income is measured at as late an age as possible (between 28 and 31 years of age for the analysis sample, depending on the birth cohort), when observed income is more representative of lifetime income (refer to Section 3.3.2 for further discussion). Chetty et al. (2014b) do not explicitly justify the choice to average over two years. But averaging across years reduces the variance of measurement error in observed income, which is a central component of left-side lifecycle bias (Nybom & Stuhler 2016b). It also allows for the inclusion of children who have zero or missing income observations in one year.

Table 1 shows descriptive statistics for the estimation sample used in the main analysis.

**Table 1: Sample Statistics**

	<b>Mean</b>	<b>S.D.</b>	<b>Min.</b>	<b>Max.</b>
<i>Children</i>				
Household Total Income (\$)	109,549.90	75,091.77	7,380.07	771,093.30
Household Private Income (\$)	101,886.30	74,826.41	234.52	749,464.40
Household Disposable Income (\$)	89,264.38	52,168.00	7,380.07	473,468.90
Equivalised Household Income (\$)	65,250.03	38,673.64	7,380.07	308,437.30
Individual Hourly Earnings (\$)	28.78	12.70	1.20	114.47
Individual Earnings (\$)	52,707.00	30,339.70	60.98	214,274.00
Individual Total Income (\$)	54,054.27	32,440.92	79.74	214,274.00
Number of people in household in 2015	2.64	1.23	1	7
Proportion co-residing with a parent in 2014 or 2015	.179	.383		
<i>Parents</i>				
Household Total Income (\$)	89,470.53	59,740.13	14,102.20	517,426.20
Household Private Income (\$)	80,881.04	61,932.82	72.80	485,658.40
Household Disposable Income (\$)	70,567.72	39,347.20	14,102.20	343,140.20
Equivalised Household Income (\$)	37,965.63	23,448.49	9,561.78	200,161.60
Individual Hourly Earnings (\$)	23.00	19.57	2.52	340.01
Individual Earnings (\$)	35,632.53	23,529.59	80.00	151,625.80
Individual Total Income (\$)	42,558.90	26,923.18	59,71.60	212,106.00
Number of people in household in 2001	4.38	1.44	2	11
Mean of parents' age in 2001	44.47	5.27	32.50	61.00

*Notes:* this table shows summary statistics for the main estimation sample, which consists of 489 children born in 1984 and 1986 who were matched to parents in the first (2001) wave of HILDA and had valid income measures in at least one of the last two available waves (2014 and 2015).

### 3. Methods and Key Sources of Bias

#### 3.1 Intergenerational Elasticity

The most widely used indicator of economic mobility is the intergenerational elasticity (IGE), which measures the expected percentage difference in child’s income (or earnings) for a one-percent difference in parents’ income (or earnings) (D’Addio 2007). The IGE is estimated using the following regression:

$$y_{0i} = \alpha + \beta y_{1i} + \varphi_1 A_{0i} + \varphi_2 A_{0i}^2 + \psi_1 A_{1i} + \psi_2 A_{1i}^2 + \omega G_{0i} + \varepsilon_i, \quad (1)$$

Where  $y_{0i}$  is the logarithm of child’s household income,  $y_{1i}$  is the corresponding measure for the child’s parents, and control variables are included to account for variation in child’s age,  $A_{0i}$ , parents’ age,  $A_{1i}$ , and child’s gender,  $G_{0i}$ .<sup>10</sup> The least-squares estimate of the slope coefficient,  $\beta$ , is the parameter of interest. Age controls are included to account for differences in mean income over age, since incomes are measured over a range of ages for both parents and children (Nybom & Stuhler 2016a). The gender dummy variable accounts for the earnings gap between men and women (Mazumder 2005a). Children and parents with zero or negative average income are necessarily excluded from the IGE analysis due to the log-log specification.

Consistent with much of the existing literature, standard errors are corrected for within-family correlation by clustering at the household level (see for example Mazumder 2005a, 2015; Yuan 2017). We do not apply sampling weights.<sup>11</sup>

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<sup>10</sup> Parents’ age is calculated as the average of the mother’s and father’s age where two parents are identified.

<sup>11</sup> Given that the analysis sample includes some children who responded to the HILDA Survey in 2014 but not in 2015 (and vice versa), the HILDA data do not include sample weights that are well suited for our analysis. The use of paired longitudinal weights that correct for non-response in 2015 (2014) would exclude those who participated in the survey in 2014 (2015) only. We get similar results, however, if we apply weights which correct for non-response in 2015. For most income measures, the weighted estimates of the IGE and rank correlation are slightly smaller than the unweighted results, but are qualitatively similar.

### 3.2 Rank Correlation

While the IGE has a long history in the intergenerational mobility literature, recent studies increasingly rely on rank-based measures of mobility.<sup>12</sup> The rank correlation (or Spearman rank correlation) measures the association between the parents' position in the lifetime earnings distribution and their children's position in the earnings distribution. Rank correlations are calculated using the equation

$$R_{0i} = \alpha + \rho^S R_{1i} + \psi_1 A_{1i} + \psi_2 A_{1i}^2 + \omega G_{0i} + \varepsilon_i, \quad (2)$$

Where  $\rho^S$  is the rank correlation,  $R_{0i}$  represents the child's percentile rank in the child income distribution,  $R_{1i}$  represents the parents' percentile rank in the parent income distribution, and other variables are as previously defined. Following Chetty et al. (2014b), children's percentile ranks are defined based on their position in the distribution of child incomes within their birth cohorts, in order to control for changes in income over age.<sup>13</sup> As described in the following sections, sample sizes in this study are small, and hence dividing the sample into birth cohorts may result in measurement error in ranks. Given that age variation is controlled for in the definition of child rank, child age controls are not included in the estimating equation. Parents' percentile ranks are measured as their position in the distribution of all parental incomes in the analysis sample, and a quadratic in parents' age is included in the estimating equation.<sup>14</sup> As for the IGE, a gender dummy variable is also added.

An important difference between the specification of the IGE and rank correlation is that the rank correlation allows for the inclusion of parents and children with zero or negative average income. This has little impact when estimates are based on household measures of income, but is more influential when using narrower definitions of income, such as labour market earnings.

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<sup>12</sup> Studies estimating the rank correlation include Chetty et al. (2014a, 2014b), Dahl and DeLeire (2008), and Mazumder (2015) for the United States; Corak et al. (2014) for Canada, Sweden and the United States; Chen et al. (2017) for Canada; Nybom and Stuhler (2016b) for Sweden; and Gregg et al. (2017) for the United Kingdom. To the author's knowledge, there have been no studies that estimate the rank correlation using Australian data.

<sup>13</sup> In the case of ties, the rank is defined as the mean rank for individuals at the same income level. For example, if 10 percent of the sample has zero income, then all of those households would be assigned a percentile rank of five.

<sup>14</sup> An alternative method of controlling for age variation in parental income is to rank parents within age categories. This approach yields similar rank correlation estimates to the baseline specification that ranks parents across the entire parental income distribution (estimates for household income are 0.266 and 0.273 for each method, respectively).

### 3.3 Key Sources of Bias

Ideally, one would use measures of lifetime income for both generations to estimate (1) and (2). This is rarely the case in practice, with researchers being forced to use short run approximations. A substantial literature has recognised that this leads to downward bias of the IGE, particularly due to attenuation bias as well as life-cycle bias. A newer literature suggests that the rank correlation is more robust to these issues. A comprehensive review is beyond the scope of this paper, but we discuss the key issues here.

#### 3.3.1 Attenuation Bias

Bowles (1972) was the first to recognise the importance of the errors-in-variables problem in measuring intergenerational mobility. Along with subsequent studies by Atkinson (1981) and Solon (1989, 1992), Bowles demonstrates that the use of single-year earnings observations severely underestimates the IGE due to transitory fluctuations in earnings. Solon (1992) also emphasises the role of unrepresentative samples in contributing to attenuation bias.

Pioneering work by Peters (1992), Solon (1992) and Zimmerman (1992) demonstrated that estimates of the IGE could be improved by averaging over three to five years of parental earnings. Averaging over multiple earnings observations reduces attenuation bias by increasing the signal-to-noise ratio of observed earnings. Overall, studies into classical errors-in-variables bias point to averaging over multiple years of parental earnings observations as one method to reduce attenuation bias, however it is likely that many years of earnings data are required to eliminate this source of bias (Mazumder, 2005a).

Another line of research finds evidence of association between the size of classical attenuation bias and the age at which short-run proxies of parents' earnings are observed. Baker and Solon (2003) and Mazumder (2005a) use large earnings datasets for men from Canada and the United States to show that the variance of the transitory component of observed earnings,  $Var(u_{1i})$ , follows a U-shaped pattern over the lifecycle, reaching its lowest point around age 40 (see Figure 2 in Mazumder 2005a, p. 240). This implies that measuring parental earnings at particularly early or particularly late stages of the lifecycle, where earnings observations are noisiest, may lead to further attenuation bias. The fact that transitory noise is so high for older men poses challenges for estimating the IGE in short samples, where typically earnings are only available for children at young ages and for parents at older ages.

### 3.3.2 Lifecycle Bias

Building on work by Jenkins (1987), the next generation of literature has considered non-classical measurement error on both sides of the regression equation. While the classical errors-in-variables model assumes that measurement error is purely random, such that only errors in the explanatory variables affect consistency, in practice measurement error tends to exhibit non-randomness related to age-earnings profiles. Björklund (1993), for example, shows that the association between current and permanent earnings exhibits a strong lifecycle pattern for males in Sweden, implying that one-period earnings may be a better proxy for lifetime earnings at some ages than others. Applying this insight to the estimation of IGEs, Reville (1995) and Grawe (2006) find that estimates of the IGE rise as the age at which sons' earnings are observed is increased from the twenties to the late thirties. Solon's (1999) survey of the intergenerational mobility literature confirms this pattern—studies relying on earnings observations from early in sons' careers yield the smallest IGE estimates. Evidence of lifecycle variation in measurement error on the right-hand side has also been highlighted by Grawe (2006) and Nilsen et al. (2008), who demonstrate that father-son IGE estimates fall with father's age.

While earlier studies recognised the need to account for these lifecycle patterns, it was generally assumed that including age controls in the classical errors-in-variables model would suffice (Stuhler 2010). As explained by Haider and Solon (2006), however, age control variables merely adjust for the average effect of age on earnings; heterogeneous variation *around* the mean growth rate is not captured. Vogel (2006) illustrates how this variation can generate lifecycle bias in IGE estimates by noting that highly educated workers tend to have steeper earnings profiles than the population mean growth rate. Given that many intergenerational mobility studies observe earnings at earlier years for children and later years for parents, lifetime children's earnings will be underestimated and lifetime parents' earnings will be overestimated for highly educated individuals (and vice versa for lowly educated individuals). If educational achievement is correlated within families, and if higher education tends to lead to higher lifetime earnings, then the IGE is biased downward by non-classical measurement error to a greater extent than implied by the classical errors-in-variables model with independent measurement errors.

Haider and Solon (2006) argue that estimates of the IGE will be subject only to the classical attenuation bias if earnings are measured around the middle of the lifecycle for both parents and children.

## 4. Baseline Results

This section presents baseline (unadjusted) estimates of IGE and rank correlations for various income definitions.

### 4.1 Intergenerational Elasticity

In the baseline analysis, parents' income is measured as mean income from 2001 to 2005, while child income is averaged over 2014 and 2015 when children are approximately 30 years old.

The (raw) baseline IGE estimates are presented in Table 2. Following Chetty et al. (2014b), the preferred income measure is household total income, which yields an IGE estimate of 0.282. We argue in Section 5 that this estimate is likely to be attenuated by approximately 30 percent (Mazumder, 2005a), leading to a bias-adjusted IGE estimate of 0.409. This result implies that around 41 percent of the income difference between two households in the parents' generation will persist into the children's generation, on average (Mazumder, 2005b). An alternative interpretation is that for children who grow up in households with incomes above or below the mean, the child's household income is expected to be 59 percent closer to the mean than their parents' household income.

IGE estimates with respect to other measures of individual and household income are provided in rows (2)–(6) of Table 2. Amongst the household measures of income (rows (1)–(4)), the IGE is reasonably robust to the definition of income used, varying by no more than 10 percent around the estimate of 0.282 for household total income.

Equivalised household income (provided in row (4)) adjusts disposable household income for the number of individuals in the household. It is intended to provide a more direct measure of material living standards than unadjusted income by accounting for the 'needs' of the household. This study applies the OECD 'modified scale', first proposed by Hagenaars et al. (1994), to calculate the equivalent number of household members. The IGE estimate with respect to equivalised household income, 0.311, is larger than that for unadjusted household income. This result is suggestive of intergenerational persistence in household structure (Chadwick & Solon 2002). If the intention of a study is to examine the intergenerational transmission of living standards, then this result also implies that the use of unadjusted household income may bias IGE estimates downwards.



Consistent with existing studies (for example Chadwick & Solon 2002; Beller & Hout 2006), the IGE estimates for individual income measures (rows (5)–(7)) are considerably lower than those using household measures. The IGE of individual total income is 35 percent smaller than the IGE of household total income, implying that mobility is substantially lower once family-level dynamics such as assortative mating and intrahousehold division of labour are accounted for (Torche 2015). While an in-depth study of assortative mating would require the estimation of elasticities of own income with respect to the spouse’s parents’ income, as in Chadwick and Solon (2002) and Hirvonen (2008), the fact that the IGE of household income is substantially higher than that of individual income is indicative of an important role for spousal selection in the intergenerational transmission of economic wellbeing.

The difference between household income and individual income IGE estimates may also be partially attributable to the likelihood of children living with a partner (Mitnik et al. 2015). If children from high-income backgrounds are more likely to live with a partner in adulthood than children from lower-income backgrounds, then the disparity between the household incomes of children from poor and rich backgrounds is amplified. This would have the effect of increasing the persistence of household income between generations. The characteristics of the estimation sample appear to support this theory: for children whose parents’ household income was in the top half of the parental income distribution, 67 percent reported co-residing with a partner in 2014 or 2015, compared to 55 percent of children who grew up in households in the bottom half of the parental income distribution. When the sample is restricted to the 61 percent of children who co-reside with a partner in 2014 or 2015, the IGE estimate drops to 0.235. That is, accounting for the probability of living with a partner in adulthood explains almost 50 percent of the difference between the IGE of household income and individual income. These results are consistent with Mitnik et al.’s (2015) US study.

The ‘partner-probability’ theory would seem to imply that the IGE of equivalised household income should be lower than that for total household income, as the equivalised measure should control for income differences that arise through larger household sizes. However there the relative propensity of individuals from high- and low-income backgrounds to have children is also a factor. For two households with the same total income, a single-parent household with two or more children will have a lower equivalised income than a household of two adults and no children (since a second adult is weighted at 0.5 and two children are weighted at 0.6 under the modified OECD equivalence scale). Thus, even if individuals from high-income backgrounds are more likely to have larger households through partnering, the effect on the equivalised income elasticity may be offset (or even outweighed)

by a higher probability of individuals from low-income backgrounds to have more children. The following statistics from the analysis sample are consistent with this hypothesis: for children from the top 50 percent and bottom 50 percent of the parental income distribution, respectively, the mean number of children is 0.476 and 0.628; the percentage of households with two or more children is 14.5 and 19.7; and the proportion of single-parent households is 7.4 and 17.8.

The IGE of hourly earnings, 0.096, is considerably smaller than the other estimates. Hourly earnings are sometimes viewed as the most direct measure of the transmission of earnings power between generations, as it measures the value of one unit of labour in the market, abstracting from labour supply decisions (Causa & Johansson 2010). Interpreted in this way, the hourly earnings IGE of 0.096 could be indicative of a relatively low rate of persistence of labour earnings potential between generations. This estimate is also considerably lower than those from studies that have used the imputation approach. But if we use the same estimate sample as our main analysis, but with Mendolia & Siminski's (2016) imputation model, the IGE increases considerably to 0.23. This is the same as Mendolia & Siminski's estimate, despite major differences in the samples used by the two studies. We think that the most likely explanation for our very low IGE for hourly earnings is that parental hourly earnings are subject to major measurement error, leading to severe attenuation bias. Hourly earnings are derived as a ratio of two self-reported variables. It is likely that measurement error in both earnings and hours of work causes the ratio between the two to be especially noisy, which may aggravate attenuation bias from right-side measurement error (Duncan & Hill 1985).<sup>15</sup>

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<sup>15</sup> Hourly earnings information is not obtained directly from survey respondents, but is instead calculated using directly observed weekly earnings and weekly hours of work. Summerfield et al. (2016) acknowledge that some respondents report low wages and salaries with high hours and vice versa, resulting in 'odd' cases when deriving hourly wage variables.

**Table 2: IGE Estimates by Income Measure**

<b>Income Measure</b>	<b><math>\beta</math></b>	<b>s.e.</b>	<b>N</b>
Household Total Income	0.282	0.049	487
Household Private Income	0.286	0.056	475
Household Disposable Income	0.277	0.053	487
Equivalised Household Income	0.311	0.048	487
Individual Hourly Earnings	0.096	0.039	364
Individual Earnings	0.199	0.050	400
Individual Total Income	0.185	0.053	486

*Notes:* This table shows IGE estimates (column (1)), standard errors (column (2)) and sample sizes (column (3)) for different measures of income. Estimates are based on the analysis sample and income definitions for parents and children as described in Section 3.2.

## 4.2 Rank Correlations

Rank correlation estimates using the baseline specification rules described are presented in Table 3. For household total income, the rank correlation is 0.273, which is close to the corresponding IGE estimate of 0.282. This result implies that a 10 percentile increase in parents' position is associated with their child being 2.7 percentiles higher in their respective income distributions.

As discussed in the previous section, private, total and disposable income differ in terms of the level of government intervention in income redistribution: private income is not affected by public redistribution, while total income incorporates public transfers and disposable income reflects the effects of the tax system. The rank correlation of total household income is approximately 15 percent lower than for private income (0.273 compared to 0.323, respectively), and the rank correlation of disposable income is an additional 2.5 percent smaller than the total income correlation. This may suggest that income redistribution, particularly in the form of public transfers, plays a moderate role in improving intergenerational mobility in ranks. In contrast, the inclusion of public transfers and taxes reduces the IGE estimate by only three percent. It is not obvious as to why income redistribution would have a larger impact on rank mobility than log income mobility. As for the IGE, adjusting for household size raises the rank correlation to 0.299, indicating that family structure is correlated across generations.

The disparity between household and individual measures of income is somewhat smaller in rank correlations than in log income elasticities, although household measures still imply lower levels of mobility than individual measures. The rank correlation estimates for individual and household total income are 0.214 and 0.273 respectively, while the corresponding IGE estimates are 0.185 and 0.282. It was proposed in the previous section that the difference in mobility for household and individual incomes may be partly attributable to children from high-income backgrounds being more likely to co-reside with a partner in adulthood than children from low-income backgrounds. If both this hypothesis and the theory of assortative mating hold, then a smaller discrepancy in rank correlations than the IGE is to be expected. If high-income individuals are more likely to live with a partner, and that partner tends to also have high income capacity, then this would result in greater household income inequality (which, by construction, increases the IGE), but has a less pronounced impact on ranks.

Interestingly, the rank correlation of individual earnings, 0.262, is closer in magnitude to the correlations with respect to household measures, and is substantially higher than the IGE of individual earnings (0.199). This could arise due to the inclusion of observations of zero average earnings in the rank correlation (of which there are 89), which are necessarily excluded from the IGE analysis due to its log-log specification. An alternative explanation is that the rank correlation is mechanically larger than the IGE simply by virtue of measuring different concepts of intergenerational mobility; however, given that the rank correlation of individual income is much closer in magnitude to the corresponding IGE estimate (0.214 and 0.185, respectively), it seems more plausible that the inclusion of zero earnings observations is primarily responsible for the larger earnings rank correlation estimate. The result that earnings persistence is stronger with the inclusion of zeros implies that mobility is lower amongst individuals with no labour market earnings, and hence that the IGE of earnings is biased downwards due to the exclusion of zeros.

It is surprising that in this study, the persistence of earnings is considerably higher than that of income, with rank correlation estimates of 0.262 and 0.214, respectively. Earnings are comprised of wages and salaries, while income also includes business income, investment income, private and public transfers and irregular income such as redundancies (Summerfield et al. 2016). Potential explanations for the results from this study being inconsistent with past literature are that, since children's income is measured during their late twenties and early thirties, self-employed children may not yet have started their own business, or the accrual of wealth and investment income may be a more important source of income later in the

lifecycle for children from affluent backgrounds (Chen et al. 2017). The IGE estimate is also larger for earnings than for individual income; however the difference is less marked.

**Table 3: Rank Correlation Estimates by Income Measure**

<b>Income Measure</b>	$\rho^S$	<b>s.e.</b>	<b>N</b>
Household Total Income	0.273	0.046	489
Household Private Income	0.323	0.045	489
Household Disposable Income	0.266	0.046	489
Equivalised Household Income	0.299	0.045	489
Individual Hourly Earnings	0.151	0.053	380
Individual Earnings	0.262	0.045	489
Individual Income	0.214	0.043	489

*Notes:* This table shows rank correlation estimates (column (1)), standard errors (column (2)) and sample sizes (column (3)) for different measures of income. Estimates are based on the analysis sample and income definitions for parents and children as described in Section 3.2. Definitions of the income measures are provided in 0.

## 5 Assessing Attenuation and Life-Cycle Bias

### 5.1 IGE

It is widely recognised that measurement error and variations in income over the lifecycle can generate bias in the estimation of intergenerational mobility. Given that the income measures in this study rely on short-term averages of parental income and observations of children's income early in their lifecycle, it is anticipated that the mobility estimates presented in Table 2 are affected by attenuation and lifecycle bias to some degree. As discussed in the previous sections, both forms of bias are expected to have an attenuating effect, such that the estimates should overstate the level of intergenerational mobility. While it is not possible to exactly quantify the impact of attenuation and lifecycle bias on the IGE estimates with the available data, this section provides a limited investigation of the impacts of these biases on the IGE estimates and an indication of the effectiveness of methods adopted in this study to reduce bias.

Panel A of Figure 1 evaluates the sensitivity of the IGE to attenuation bias resulting from measurement error in parental income. This study follows the common approach of averaging over multiple years of parental income to obtain a measure of income that is less affected by transitory shocks. The estimates presented in Panel A show the impact of increasing the number of years over which parental income is averaged from one through to five (the number of years used to construct the baseline IGE estimates in Table 2), with the year range centred at 2003 (see Chetty et al., 2014b and Mazumder, 2015 for an interesting discussion of the issue).

Increasing the time average from one year to three years results in a 22 percent rise in the estimated IGE, from 0.210 to 0.257. Moving from a three- to a five-year average increases the IGE by an additional 18 percent. Overall, averaging over five years of parental income reduces attenuation bias by approximately 44 percent relative to using one parental income observation. These results indicate that attenuation bias has a substantial impact on the IGE estimates presented in this study, and it is likely that averaging over five years only partially accounts for this bias. It therefore seems highly likely that the raw estimate of 0.282 understates the true IGE value.

Clearly it is not possible to determine the exact extent to which the estimate is attenuated; however, comparison to benchmarks from other studies may provide an approximation of the magnitude of the bias. Using a simulation that calculates the attenuation factor for short-run proxies of lifetime earnings, Mazumder (2005a) finds that mobility estimates based on five-year averages are biased downwards by approximately 30 percent.<sup>16</sup> If the assumptions underpinning this result apply equally to household income, this implies that the unbiased IGE estimate would be 0.409 (applying a correction factor of  $1/0.69$ ). Given that household income may be a less error-prone measure than individual earnings (Mazumder 2005a), however, it is likely that 30 percent represents an upper bound for the effect of attenuation bias.

As explained above, lifecycle-related measurement error limits the capacity of increasing time averages to reduce right-side attenuation bias, given the data restrictions in this study. Income measurements may become noisier later in the lifecycle as individuals approach retirement and reduce their labour supply or drop out of the labour force. Mazumder (2001) finds that the variance of the transitory component of men's earnings in the United States is

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<sup>16</sup> Assuming that half of the variance of current earnings observations is due to the permanent component of earnings, and assuming that the transitory component of earnings follow a first-order autoregressive process with an autocorrelation coefficient of 0.5.

lowest around age 40 but rises rapidly from the late forties into the late fifties. Although household income may be less affected by noise related to labour force participation than earnings, there is still expected to be a considerable lifecycle effect for household income. Approximately 80 percent of parents in the sample are over age 40 in 2001, implying that parental income observations beyond 2005 would entail substantial lifecycle-related measurement error for the majority of the sample.

The second panel in Figure 1 evaluates the impact of left-side lifecycle bias on the IGE estimate by varying the age at which the child's income is measured. The lifecycle robustness test used in this study maintains the same child birth cohorts (i.e. 1984–1986) and parent income definition (i.e. averaged over 2001–2005) as the baseline estimates, instead varying the age at which child's income is observed from 25 to 29.<sup>17</sup> For each estimate, child incomes are measured in different calendar years depending on their birth year.

There is no distinct pattern in the estimates shown in Panel B. If left-side lifecycle bias did affect the estimates in the manner predicted by previous research, there would be a positive relationship between the value of the IGE estimate and the age at which child's income is measured. Instead, the estimates are relatively sensitive to child's age, but with no consistent pattern. A potential explanation for this result is that lifecycle bias does not have as significant an effect on the IGE as has been found for other countries. Given the apparent sensitivity of the estimates presented in Panel B to child's age, though, a more likely explanation is that the sample size is not large enough to identify a strong lifecycle pattern in the IGE estimates.<sup>18</sup> This would be an informative area for future research as the HILDA Survey panel matures, or as other datasets become available that offer the possibility of analysing larger sample sizes.

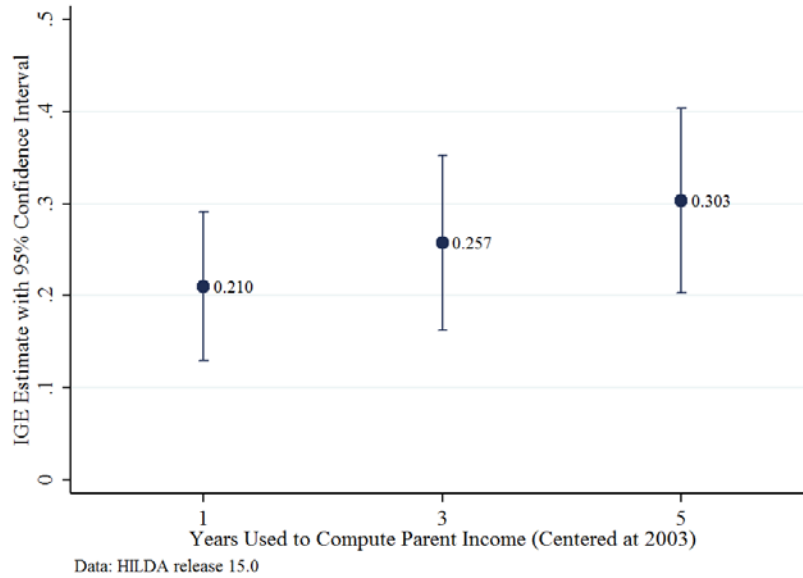
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<sup>17</sup> The analysis sample is held constant, as for the attenuation bias robustness test. This means that a child's income must be observed at each age from 25 to 29 in order to be included in the analysis.

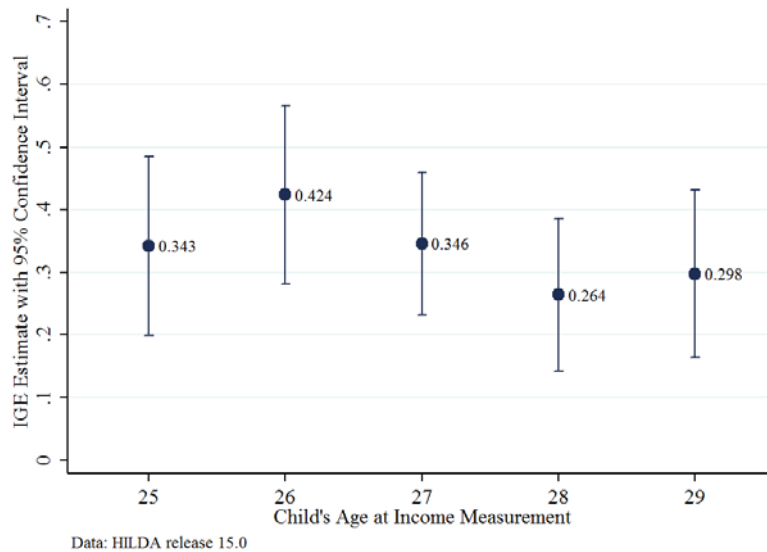
<sup>18</sup> Due to the constant sample restriction (see previous footnote), the sample size is somewhat smaller for the estimates in the lifecycle bias robustness test (329 parent-child pairs) than for the baseline estimates (489 parent-child pairs).

**Figure 1: Impact of Attenuation and Lifecycle Bias on IGE Estimates**

**A. Attenuation Bias: IGE Estimates by Number of Years Used to Compute Parent Income**



**B. Lifecycle Bias: IGE Estimates by Age of Child**



*Notes:* This figure evaluates the impact on the estimated IGE of household income of changes in the number of years used to measure parents' income (Panel A) and changes in the age at which child income is measured (Panel B). The figure is based on the analysis sample described in Section 3.1 (i.e. child birth cohorts 1984–1986). In Panel A, the first point uses parent income data from 2003 only to define log parent income; the second point uses mean parent income from 2002–2004; and the third point uses mean parent income from 2001–2005. In Panel B, each point uses child income measured at each age from 25 through to 29, meaning that the calendar year in which income is measured for each estimate varies depending on the birth cohort. The estimation sample is held constant across all estimates in both panels.

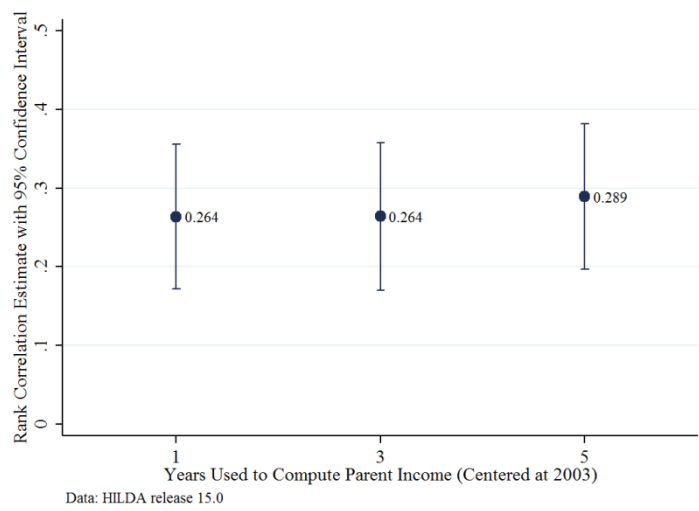


## 5.2 Rank Correlations

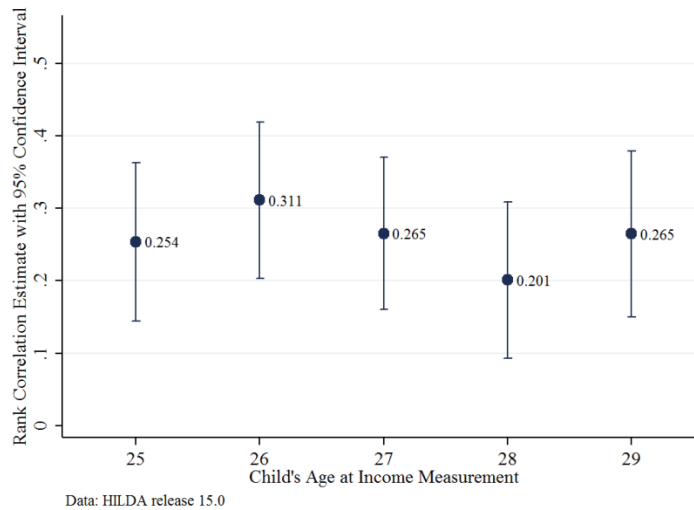
Figure 2 repeats the tests for attenuation and lifecycle bias, this time for the rank correlation. Panel A indicates that the rank correlation is reasonably robust to attenuation bias. The estimate increases by approximately nine percent when the number of years over which parents' income is averaged is raised from one to five, a much smaller change than that seen for the IGE, which increases by more than 40 percent under the same robustness test. Several recent studies also find the rank correlation to be more robust than the IGE to this form of bias (see for example Mazumder 2015; Nybom & Stuhler 2016a; Gregg et al. 2017). These studies also find that the rank correlation is more stable than the IGE over the age at which child's income is measured. In our study, however, the test for lifecycle bias is inconclusive about the impact of this form of bias. Like the IGE, the rank correlation is reasonably sensitive to the age at which children's income is measured, ranging between 0.201 and 0.311. But there is no clear pattern in these fluctuations that would indicate whether they are driven by a systematic effect of lifecycle bias, or simply sampling variability. As for the IGE, more extensive analysis of the influence of attenuation and lifecycle bias on the rank correlation would be a useful area of future research, either as the HILDA Survey matures or when other datasets become available in Australia that are better suited to this objective.

**Figure 2: Robustness of Rank Correlation Estimates to Attenuation and Lifecycle Bias**

**A. Attenuation Bias: Rank Correlation Estimates by Number of Years Used to Compute Parent Income**



**B. Lifecycle Bias: Rank Correlation Estimates by Age of Child**



*Notes:* This figure evaluates the robustness of the estimated rank correlation of household income to changes in the number of years used to measure parents' income and income rank (Panel A) and changes in the age at which child income is measured (Panel B). The figure is based on the analysis sample described in Section 3.1 (i.e. child birth cohorts 1984–1986). In Panel A, the first point uses parent income data from 2003 only to define parent income; the second point uses mean parent income from 2002–2004; and the third point uses mean parent income from 2001–2005. In Panel B, each point uses child income measured at each age from 25 through to 29, meaning that the calendar year in which income is measured for each estimate varies depending on the birth cohort. The estimation sample is held constant across each of the estimates in both panels.

## 6. Comparisons to the US

Given the relatively short length of the HILDA panel, the guiding principle for our analysis to date has been to follow the lead of a recent prominent study that drew on a panel of similar length (Chetty et al., 2014b). However, our results are not comparable to that study, primarily because we use survey data and they use administrative data. In this section, we seek to construct comparable estimates for Australia and the US, drawing also on the Panel Survey of Income Dynamics (PSID), an American household panel survey. We estimate intergenerational elasticities, as well as rank correlations for both countries, drawing on the Cross-National Equivalence File (CNEF) version of each dataset.

Our guiding principle in this analysis is again to follow Chetty et al. (2014b)'s approach, whilst navigating the additional complications that result from differences between HILDA and PSID. The first complication is that PSID CNEF data are currently available only up to 2013. So we shift the PSID analysis window back two years in all respects, including the birth cohort years. This ensures that child age at time of (own and parental) income observation is the same as in HILDA, whilst still using the full length of the HILDA panel. The second complication is that PSID waves occur every two years, rather than every year as in HILDA. In our main HILDA analysis, we followed Chetty et al. (2014b) by averaging child income over the last two years of the panel. Since we cannot do this in PSID, we show results from two alternative measures of child income: income in the last year only, and the average over the last observation and the one from two years earlier.<sup>19</sup> Further, instead of averaging parental income over the first 5 years in the data window, we take the average of parental income in the first wave, and the waves that occurred two and four years later. Of course we do this for both HILDA and PSID. This introduces attenuation bias, but it does so for both countries.

We focus primarily on disposable (after tax and transfer) income, rather than gross income. Chetty et al.'s (2014b) focus on gross income seems to be a choice of convenience, justified by the argument that government transfers lead to few rank-reversals which hence should not affect rank correlations. Since we are equally interested in IGEs, and since the

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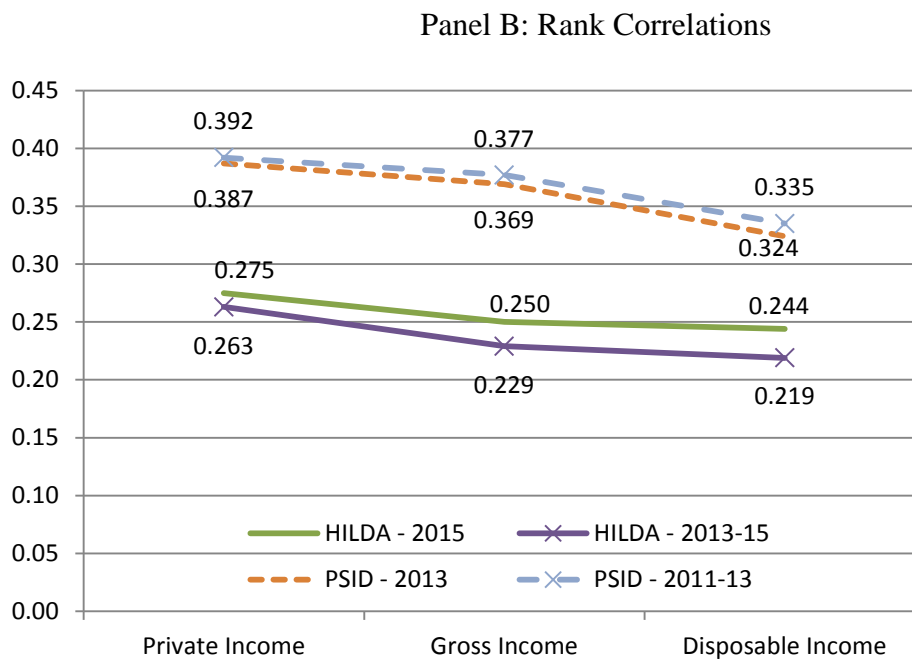
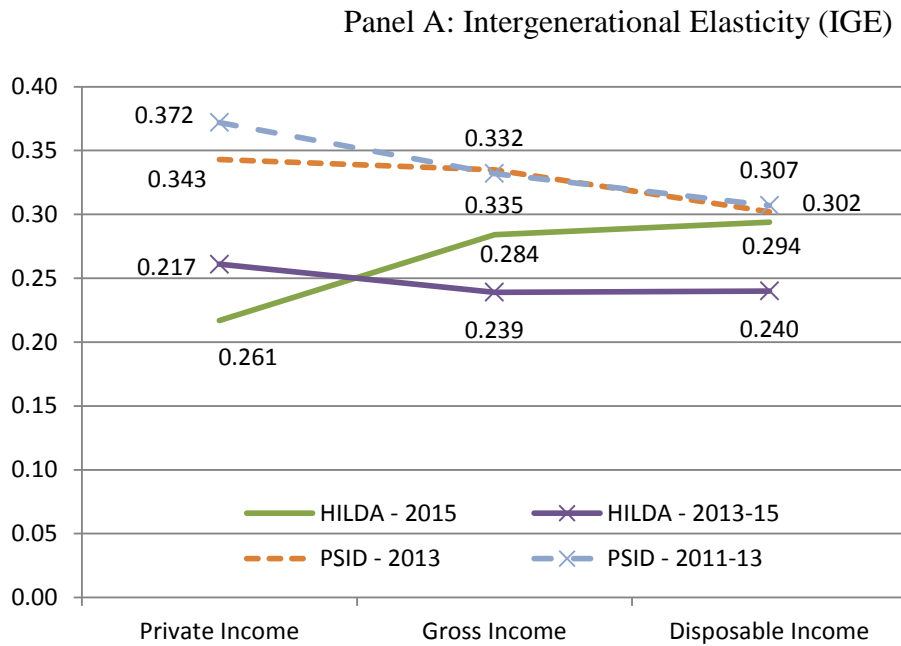
<sup>19</sup> We do not see clear grounds by which one of these child income versions should be preferred to the other. The average across two years should reduce noise in the dependent variable and hence improve precision. On the other hand, it uses data from two-years earlier in the life course, increasing life-cycle bias. Nevertheless, both options seem valid for the comparative analysis.

transfer systems of both countries should not be abstracted from, we prefer disposable income. But we also show results for gross and private income.

The results are summarised in Figure 3. The top panel shows IGEs and the lower panel shows rank correlations. Each panel shows 12 separate estimates, one for each combination of income type (private, gross or disposable), child income period (single year, or 2 year average) and survey (HILDA or PSID). This figure has several key features. The first is that the rank correlation approach produces estimates that are considerably less sensitive than the IGE to variations in the specification. Chetty et al (2014b) and others have also noted the greater stability of the rank approach.

The second key feature of Figure 3 is that the estimates are always larger for PSID than for HILDA, regardless of the approach used. This provides strong evidence that the extent of mobility is greater in Australia. Using disposable income, the IGE is 28% higher in PSID than in HILDA when the 2-year average child income is used, or 3% with the single-year measure. The rank correlations suggest a greater difference between countries. The rank correlation is 53% higher in PSID than HILDA using the 2-year average child income, and 33% higher using single-year child income.

Figure 3: Comparisons of Intergenerational Mobility Estimates between Australia (HILDA) and the US (PSID)



Notes: This figure shows various comparable estimates of intergenerational income mobility for Australia (HILDA) and the US (PSID), using the CNEF versions of the data. Panel A shows intergenerational elasticity estimates and Panel B shows rank correlations. In HILDA, the sample is defined in the same way as the main analysis, with two modifications: Parental income is averaged over waves 1, 3 and 5, to match the 2-year gap between waves in HILDA; for the same reason, child income is either the 2015 measure, or the average of income measured in 2013 and 2015. PSID uses the same definitions and sample selection criteria, but shifted two years earlier in all respects including the birth cohort selection criteria, because the CNEF version of PSID is currently available only up to 2013.

## 7 Conclusion and Discussion

The empirical results suggest that a substantial proportion of economic advantage is passed from parents to children in Australia. The baseline parent-child income elasticity estimate is 0.282 (before correcting for any bias). The elasticity estimate is found to be subject to attenuation bias arising from the use of short-run approximations of parental income. Applying a crude adjustment for the potential amount of attenuation bias, the elasticity rises to 0.409, indicating that there is much less mobility once measurement error in the explanatory variable is accounted for. This implies that approximately 41 percent of differences in household income in the parents' generation persist into the children's generation.

While not directly comparable to existing estimates for Australia, which rely on hourly earnings and father-son comparisons, this result is broadly in line with the most recent mobility estimates for Australia (Huang et al. 2016; Mendolia & Siminski 2016). We do not find evidence of systematic variation in the IGE by age at which child's income is measured, although our statistical power is limited. But if the evidence on lifecycle bias from other countries applies to Australia, then even our attenuation bias-adjusted elasticity estimate underestimates income persistence.

Elasticities of household measures of income are found to be considerably higher than those for individual measures, possibly because children who grow up in higher income households are more likely to live with a partner as adults or because spousal selection results in children with similar earnings capacity partnering with each other. After adjusting the income measure for household size, the elasticity of equivalised income rises, which may reflect the observation that individuals from lower-income backgrounds have a higher propensity to have children and have more children on average than their peers from higher-income backgrounds. Overall, these results suggest that household-level dynamics, including the probability of living with a partner, partner's earnings capacity (conditional on living with a partner), and the number of children in the household, can have a significant influence on the persistence of material living standards. As such, studies that seek to measure the transmission of economic wellbeing may need to consider broader measures of income that account for differences in family structure.

The rank correlation of household income is estimated to be 0.273. Consistent with existing studies, this mobility index is found to be substantially more robust to attenuation

bias than the IGE, although it is expected that the estimate is still biased downwards to some extent. As for the IGE, the extent to which this estimate is subject to lifecycle bias is unclear.

Our comparative analysis suggests that Australia is more mobile than the US and this is consistent across a number of alternate approaches and measures. The most robust results are for rank correlations. These are much higher for the US (33% to 53% higher than for Australia using household disposable income). The gap is smaller for the IGE (3% to 28% higher for the US).

Finally, obtaining precise and accurate estimates of intergenerational mobility can only inform a relatively narrow understanding of equality of opportunity in Australia. Insights into the underlying drivers of mobility, gained either through the analysis of transmission mechanisms, or by comparing rates of mobility across subgroups in society, are as important to the formation of policies targeted at improving access to opportunity as the point estimates which form the focus of this study.

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## Appendix A: Attrition and Representativeness

Table A1 provides sample sizes by birth cohort and for the overall analysis sample. The analysis sample consists of 489 parent-child pairs, which represent 56.6 percent of the children born in 1984–1986 who participated in the survey in 2001. The majority of the reduction in sample size is attributable to attrition between 2001 and 2014–2015 rather than to an inability to match children to parents. Retention rates in this study are consistent with the reported HILDA Survey re-interview rates (Summerfield et al. 2015, 2016).

**Table A1: Sample Sizes by Child’s Birth Cohort**

Year of birth	Survey respondent in 2001	Survey respondent in 2001, matched to at least one parent		Survey respondent in 2001 and at least one of 2014 or 2015, matched to at least one parent	
	N	N	%	N	%
1984	314	296	94.3	167	56.4
1985	274	261	95.3	140	53.6
1986	310	307	99.0	182	59.3
1987	315	312	99.0	165	52.9
1988	321	316	98.4	201	63.6
1989	321	318	99.1	200	62.9
Analysis sample: 1984–1986	898	864	96.2	489	56.6
Extended sample: 1984–1989	1,855	1,810	97.6	1,055	58.3

*Notes:* Column (1) provides the number of HILDA Survey respondents in 2001 by birth year. Columns (2) and (3) show the number and percentage of those who report co-residing with at least one parent between 2001 and 2005. Column (4) provides the number of survey respondents in 2001 who are also survey respondents in 2014 and/or 2015 (when child income is measured), to whom a parent can be matched. Column (5) shows column (4) as a percentage of column (2).<sup>20</sup>

Survey attrition can affect sample representativeness if it is non-random, and may bias results if the pattern of attrition is correlated with parents’ or children’s incomes (Solon 1992; Jäntti

<sup>20</sup> For ease of sample construction, the sample was restricted to those children who responded to the HILDA Survey in 2001. This restriction does not exclude any potential sample members from the analysis—there are no individuals who did not participate in the 2001 survey who (1) participated in at least one of the 2014 and 2015 surveys, (2) are matched to at least one parent, and (3) are born between 1984 and 1986.

& Jenkins 2015). In Table A2, we compare characteristics of potential in-sample respondents (those born between 1984 and 1986 matched to at least one parent who participated in the HILDA Survey in 2001) to characteristics of the analysis sample and differences in characteristics between the two samples are small and do not strongly indicate that attrition has negatively impacted the representativeness of the analysis sample.

**Table A2: Comparison of Analysis Sample Characteristics to Potential Sample**

<b>Variable</b>	<b>Analysis Sample</b>	<b>Potential Sample</b>
Mean parental household income (Standard deviation)	\$88,723.79 (\$60,934.55)	\$85,440.07 (\$60,842.63)
Percentage of mothers who are unemployed	9.8	8.2
Percentage of fathers who are unemployed	5.1	3.9
Percentage of mothers with a Bachelor degree or higher	18.6	17.8
Percentage of fathers with a Bachelor degree or higher	18.2	17.6
Percentage of mothers who did not complete year 12	46.7	45.5
Percentage of fathers who did not complete year 12	24.1	22.1

*Notes:* Row (1) provides the mean (and standard deviation, in parentheses) of parental household income, calculated as described in Section 3.4. Rows (2) and (3) show the percentage of mothers and fathers, respectively, who were unemployed in at least one year between 2001 and 2005. Parental educational attainment is given in rows (4) to (7). Column (1) provides sample statistics for children included in the final analysis sample; column (2) provides sample statistics for children born between 1984 and 1986 who responded to the HILDA Survey in 2001 and are matched to at least one parent between 2001 and 2005.

## Appendix B: Income Variable Definitions

Table B1 provides definitions for the income measures used in the empirical analysis. Definitions are taken from Summerfield et al. (2016) and Hagenaaars et al. (1994).

**Table B1: Income Variable Definitions**

<b>Income Variable</b>	<b>Definition</b>
Household Total Income	Household financial year gross total income. The sum across all household members of financial year market income (wages and salaries, business income, investment income and regular private pension income), private transfers, Australian and foreign pensions and benefits and irregular income.
Household Private Income	Household financial year regular private income. The sum of household financial year regular market income (wages and salary, business income, investment income and regular private pension income) and regular private transfers.
Household Disposable Income	Household financial year disposable regular income. The sum across all household members of financial year gross regular income (financial year regular income from private and public sources, excluding irregular income) minus taxes on financial year gross regular income.
Equivalised Household Income	Household disposable income divided by the modified OECD equivalence scale. The equivalence scale assigns a value of 1 to the first person in the household, a value of 0.5 for each other household member aged 15 years and over, and a value of 0.3 for each child under 15.
Individual Hourly Earnings	Hourly wage rate for all jobs. Current weekly gross wages and salaries for all jobs divided by combined hours per week usually worked in all jobs, for employed respondents.
Individual Earnings	Gross last financial year (July 1 to June 30) wages and salaries. If the respondent could provide their income after deductions were taken out, then this was used to estimate their gross income by applying the tax scale.
Individual Total Income	Financial year gross total income. Comprises financial year gross regular income from all private and public sources plus financial year gross irregular income.