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The Correlation of Wealth Between Parents and Children in Australia

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Abstract

We present the first estimates of intergenerational wealth mobility for Australia. The rank correlation is 0.253, compared to 0.306 for the United States using comparable methods. This correlation varies greatly by child age when wealth is observed, from 0.1 before age 30, to 0.5 after age 40. This sharp increase with age is stronger than for other countries, is not explained by sample selection bias and is not specific to particular types of wealth. We also argue that neither income mobility nor wealth mobility, as operationalised in empirical work, align neatly with the wealth concept in the Becker & Tomes framework.

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JEL CLASSIFICATION
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1. Introduction

The extent to which children's outcomes echo their parents' outcomes is closely related to ideals about equality of opportunity (Corak 2013). If children's outcomes are strongly correlated with parents' outcomes, then economic or social mobility is low. Such correlations have been studied for decades by scholars from various disciplines. Sociologists have focused on mobility in social class, occupation and education. Australian examples include Marks and McMillan (2003), Redmond et al. (2014) and Chesters (2015). Economists have mainly focused on earnings and income in empirical work. Recent empirical work on income mobility includes Chetty et al. (2014, 2017), Deutscher and Mazumder (2020) and Kennedy and Siminski (2021).

The theoretical foundation for the economic approach to intergenerational mobility comes from Becker and Tomes (1979, 1986). In their models, intergenerational persistence of economic outcomes is driven by endowments (shared characteristics of parents and children) and parental investments into their children. Parents decide how much to invest in children according to a utility function based on arguments related to own consumption and child's wealth (Becker and Tomes 1979) or child utility (Becker and Tomes 1986).[1] Becker and Tomes (1979) discuss how wealth can be converted to 'permanent income' streams. Consequently, a large literature cites the Becker & Tomes framework to justify a focus on permanent income. Beginning with Solon (1992), the main emphasis in empirical work has been to accurately estimate permanent-income correlations when income is observed over relatively short periods. This work focuses on issues such as attenuation bias and lifecycle bias. The general conclusion from this literature is that income measured over several years around midlife is a good proxy for permanent income. Such measures yield approximately unbiased estimates of permanent income mobility (Grawe 2006; Haider and Solon 2006; Nybom and Stuhler 2016, 2017).

A somewhat neglected issue, however, is whether income (permanent or otherwise), indeed captures the wealth (or utility) concepts in the Becker & Tomes frameworks. Income, as captured in surveys and in administrative data, is far from a complete measure of the economic resources that children command. It typically excludes gifts and transfers from parents. It usually excludes noncash income, including the imputed rental value of owner-occupied housing (Saunders and Siminski 2005). Capital gains are often excluded, especially when they are unrealised. Furthermore, bequests from parents usually come later than midlife. Consequently, the link between observed income (even if measured at midlife) and the theoretical benchmark of (lifetime) wealth is far from perfect. This is particularly so if direct transfers (inter vivos and bequests) are quantitatively important; or if parental transfers contribute to home purchase, rather than to human capital or other assets that generate cash income. Income is hence a noisy signal of lifetime wealth, implying that intergenerational correlation of income is likely to be lower than corresponding correlations of lifetime wealth (Boserup et al. 2017 make a similar point).

Why then, does empirical work focus on income mobility, rather than on wealth mobility directly? Wealth inequality has increased since the 1980s in many countries (Alvaredo et al. 2018; Katic and Leigh 2016). Public consciousness of wealth accumulation and bequests as key drivers in the evolution of inequality is particularly strong (Piketty 2011; Piketty and Zucman 2015). Interest in bequests and wealth transfers has hence gained momentum in recent years (Kopczuk 2013; Boserup et al. 2018; Productivity Commission 2021). But there have been relatively few empirical studies on intergenerational wealth mobility. Charles and Hurst (2003) is the best-known early work to directly estimate intergenerational wealth mobility, for the United States. Other notable examples include Adermon et al. (2018), who use Swedish data to study multigenerational wealth correlations and the central role of inheritances; Arrondel (2013) who study intergenerational correlations of wealth alongside risk and discounting preferences; Clark and Cummins (2015), who study long-run intergenerational wealth persistence in England, linking administrative data sets using rare surnames; Kubota (2017), who estimate intergenerational wealth persistence in Japan, exploring the roles of income, educational level, bequests and preferences; and Pfeffer and Killewald (2018), who study multigenerational wealth persistence in the United States. Becker and Tomes (1986) and Charles and Hurst (2003) both cite earlier empirical work on wealth mobility, which mostly draws on small and unrepresentative samples.

A likely reason for the few studies is that ‘wealth’ measured at a point in time is quite different to the concept of wealth that underpins the Becker & Tomes framework. Wealth, or more correctly ‘net worth’, at a single point in time is a narrower measure of economic wellbeing. It is a function of earnings and other income over the life course, as well as consumption and savings paths. It is a function of transfers, especially from parents (bequests and inter vivos transfers). All of these factors evolve considerably over the life course, and hence the age at which wealth is observed (for both generations) is likely to be critical. Clarity on this issue is of first-order importance, especially for studies that draw on relatively short-run panels of linked intergenerational wealth data.

We take the stance that neither income mobility nor wealth mobility, as operationalised in empirical work, align neatly with the wealth concept in the Becker & Tomes framework. They can instead be seen as complementary, imperfect indicators of the intergenerational persistence of economic wellbeing. Despite their limitations, they are particularly useful when used in comparative work, when these parameters are estimated for various countries, or over time, using comparable methods.

This paper presents the first estimates of intergenerational wealth correlations in Australia. We draw on data from the Household, Income and Labour Dynamics, Australia (HILDA) panel survey. In HILDA, parents can be linked to their adult children, but only if they lived in the same household in the first wave. Whilst HILDA is a high-quality dataset, its main limitation for this study is its length. Wealth was first measured in Wave 2 (2002), and most recently in 2018. We therefore pay particular attention to the implications of the short panel length for our analysis.

Our first (and main) approach is based on the pioneering work of Charles and Hurst (2003), whose data were characterised by similar limitations to ours. We estimate the intergenerational wealth correlation to be 0.253, controlling for child and parental age. We then conduct a comparable analysis using the US Panel Study of Income Dynamics (PSID). Whilst previous studies also used PSID to estimate wealth mobility (Charles and Hurst 2003; Pfeffer and Killewald 2018), we use a sample selection approach that closely resembles what we do with HILDA. This allows us to more confidently gauge how the wealth correlation in Australia compares to that of other countries. Our estimated correlation is 0.306 in PSID, clearly higher than our Australian estimate.

We then examine life-course variation in the estimates. We find that wealth correlations are considerably smaller when wealth is measured at younger ages of the child (about 0.1), increasing to 0.5 when wealth is measured around middle-age. Through supplementary analysis, we confirm this is not driven by sample selection bias affecting older children. Overall there is strong evidence that the age at which wealth is measured is an important factor in wealth correlations for Australia. This relationship between the wealth correlations and the child age at wealth measurement is stronger than has been observed for other countries. We also show that this pattern is not driven by particular types of wealth, such as property wealth or financial wealth.

The remainder of the paper is structured as follows. Section 2 describes the data and presents descriptive statistics. Section 3 presents a non-parametric analysis of wealth mobility, while Section 4 presents the main estimates of intergenerational rank correlations. Section 5 addresses life-course considerations. Section 6 concludes.

2. Data

We draw on data from the HILDA Survey (Release 19). To provide a cross-country comparison, we also use US data from the PSID.

HILDA is a longitudinal study of around 17,000 individuals in Australia, commencing in 2001. Respondents are interviewed annually, with data currently available to 2020. HILDA's initial sample in 2001 is nationally representative, and household members identified in Wave 1 of the study are followed indefinitely. Children who were teenagers in Wave 1 are now aged in their 30s.

2.1 Measuring Wealth in HILDA

Wealth data are collected every four years in HILDA, starting in wave 2002 and subsequently in 2006, 2010, 2014 and 2018. ‘Wealth’ is net worth of the household, equal to assets minus debts. Wealth is measured at the household level in HILDA, as many items that can be classified as asset or debts are shared amongst the household, such as the value of the family home. The wealth module in HILDA is detailed, with data being collected separately on many components of wealth, as outlined for example in Section 4.21 of Summerfield et al. (2015). For observations with incomplete or missing observations, an imputation procedure is used, as also described in Summerfield et al. (2015).[2]

We match parents (whose wealth was observed in 2002 and 2006) with their own adult children (whose wealth was observed in 2018). We create a parental wealth measure equal to the average of wealth in 2002 and 2006 (Wave 2 and Wave 6).[3] In both of these waves, the average of each parent's household wealth was used if the child could be matched with both parents. Otherwise, the wealth of the single matched parent is used. Ideally, an average wealth measure of children would also be preferred, but this is limited by the length of the HILDA survey. Since wealth is measured on a household level, children who are still living with their parent(s) in 2018 are also excluded, because identifying the child's share of the household's net worth is not practical. By excluding those who were still living with their parents in 2018, larger proportions of those in the younger age groups are dropped. However, the number of observations excluded because the child and parent were living together is relatively small, as shown in Table A1. Wealth is expressed in March 2019 prices, using the consumer price index (Australian Bureau of Statistics [ABS] 2020).

2.2 Sample Selection

The relatively short HILDA panel allows for observed parent and child wealth to be 16 years apart, at most. This means that child wealth is measured at younger ages than parent wealth, regardless of sample selection decisions. Further, children can only be matched to their parents if they lived together in at least one wave. This presents challenges for sample selection decisions. One option is to include as many matched children as possible, by including children from older cohorts in the estimation sample. To be included in the sample, such older children must have lived with their parents at a relatively old age, say in their 20s or 30s. This allows child wealth to be observed at older ages, but it also risks bias due to a non-representative sample, since such children may be different to the majority of children who leave home much earlier. Another option is to choose a much younger and smaller, but more representative, sample. For such a sample, child wealth has only been observed up to ages around mid-30s. This is quite young, as wealth accumulates greatly over the life course.

We show results using both of these approaches. We also carefully scrutinise potential bias due to non-representativeness, as well as examining the role of child age at which wealth was observed. As we will show, age is a major factor in the results, while bias from non-representative samples in the first approach seems to be minor. We therefore treat the first approach as our main approach, which has the added benefit of closely resembling the approach of Charles and Hurst (2003), who faced similar data limitations. For this approach, we include all matched children who were aged between 25 and 65 when their wealth was observed (Wave 18).[4] Parents above the age of 65 at the time their wealth was observed were also excluded as retirement might affect wealth accumulation. 1,867 child–parent pairs are included in the sample for main analysis.

To construct this sample, we first match children in Wave 1 to their parent(s). We match all children between the age of 8 and 48 in Wave 1 (which corresponds to age 25 to 65 in Wave 18) with their parents observed in the same year.[5] As per the discussion above, the match rate of children to their parents is much lower for older children as they were less likely to live with their parents in Wave 1 (see Table A1). Indeed, no children aged over 41 in Wave 1 were in the estimation sample.

2.3 Summary Statistics

Summary statistics for key variables of the HILDA sample are reported in Table 1. The average age for children at 2018 is 31.97, whilst the average age for parents is 46.90 at the time their wealth was observed.[6] Wealth for both child and parents are positively skewed, with mean wealth significantly greater than the median wealth. As parents

Table 1 Descriptive Statistics for Main Estimation Sample (Household, Income and Labour Dynamics in Australia [HILDA])

Variable	*Child (2018)*	*Parent (2002, 2006)*
Age	31.97 (5.48)	46.90 (6.91)
Percentile of wealth		
20th	24,030.4	172,098.5
40th	114,537.3	450,615.7
60th	297,916.6	755,897.2
80th	646,097.6	1,283,628.9
Median wealth	196,634	578,742.5
Mean wealth	453,3328 (864,026.4)	927,319.1 (1,261,992)
Total household income	130,649.4 (122,810)	134,423.1 (97,828)
Highest education level		
Year 11 or below	12.91%	20.89%
Year 12	20.73%	8.57%
Post-school certificate or diploma	31.66%	39.31%
Bachelor degree	21.42%	14.68%
Postgraduate degree	13.28%	16.55%

Note: 1,867 child–parent pairs are included in the sample. Imputed household wealth is included. All wealth and income variables are adjusted to 2019 prices. Parental age, wealth and income are the average across 2002 and 2006 for the father and mother. The highest level of education between father and mother across 2002 and 2006 is shown. Standard deviations are shown in parentheses.

were generally older than children, parental wealth was in general higher than child wealth. Inflation-adjusted household income for parents is similar to that of children. Table A1 also reports the children's age distribution in the HILDA sample. The highest age of children at 2001 was 41 in the main estimation sample, after exclusions due to missing data, loss to follow-up, and co-residence of children and parents in 2018.

2.4 US Data: PSID

To generate comparable estimates for the United States, we also construct a sample with PSID using an approach that mirrors our approach with HILDA. To account for any differences due to time effects or sampling process, we attempt to construct a sample from PSID that is similar to the length and time of HILDA.

PSID commenced in 1968 in the United States and consisted of an initial sample of close to 5,000 families, including a nationally representative random sample, and an over-sample of low-income families with a head aged under 60 years. Individuals in the initial sample and their descendants were followed up annually until 1997 and biannually after that. Following Mendolia and Siminski (2016), and many other studies, the additional 1997 and 1999 Latino immigrant samples were included in our analysis but the low-income oversample in the initial 1968 sample was excluded from the analysis. Children and parents are linked using prospective matching with the Family Identification Mapping System provided by the online PSID data centre (Insolera and Mushtaq 2021).

To construct a sample from PSID that is comparable to HILDA, only children interviewed in 2001 who were living with their parents are included. Children's wealth in PSID is observed in 2017, and parental wealth is in 2001 and 2005, which in each case is one year earlier than HILDA. Similar to HILDA, wealth is reported as a household measurement in PSID, so children living with their parents in 2017 are dropped from the sample. The child–parent matching rate, as well as the number of observations for each age group in the PSID sample, are reported in Table A2.[7]

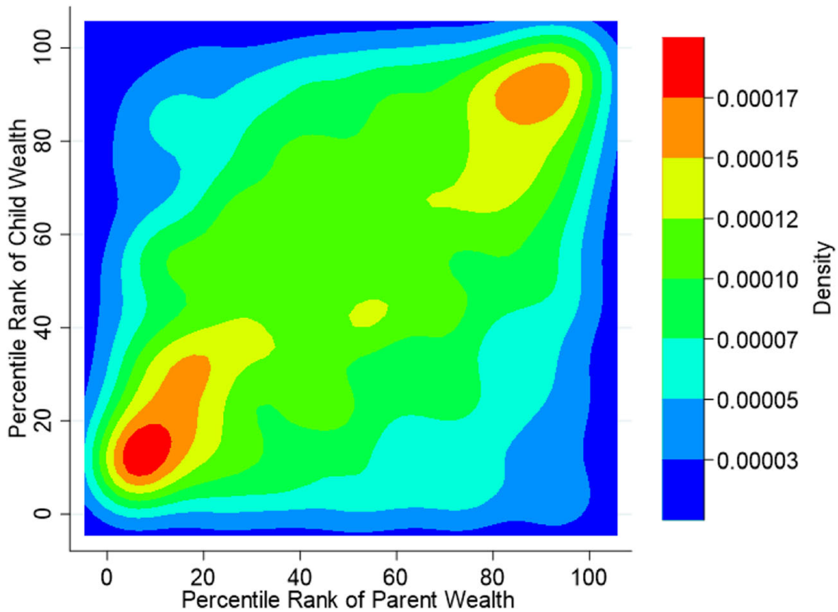
3. Non-Parametric Analysis

We first examine the relationship between child wealth and parental wealth using non-parametric methods.

3.1 Distribution of Child–Parent Wealth

A bivariate joint density plot (heat map) between percentile rank of parental and child wealth is shown in Figure 1.[8] The main feature of the plot is the high density at the bottom-left of the plot. This indicates that children whose parents are at the lower end of the wealth distribution are likely to be at the

Figure 1 Bivariate Density Plot Between Percentile Rank of Child Wealth and Parental Wealth



This figure is a bivariate density plot for the joint distribution of child wealth percentile and parent wealth percentile. The sample is restricted to children aged 25–64 at 2018 (when their wealth was observed), who were living with one or more parents in 2001, and no longer living with parents in 2018.

lower end of the wealth distribution themselves. There is a similar (but weaker) peak in the density at the upper end of the distribution. The lowest densities are at the top-left and the bottom-right, suggesting that large movements (from the bottom to the top of the distribution, or vice versa) are rare.

3.2 Transition Matrix

We now present a transition matrix, which shows the proportion of children in each quintile of the child wealth distribution, by quintile of the parent wealth distribution. This time, we control for age, since it is highly correlated with wealth (Jappelli 1999; Kapteyn et al. 2005; Lim and Zeng 2016). Following Charles and Hurst (2003), for each generation, log wealth is regressed on a quadratic function of age. We run this regression separately for parents (controlling for a quadratic of parent age) and again for children (controlling for a quadratic function of child age). Where both parents' age is observed, we use their average age. The residuals from each regression are used to rank adults and children into quintiles, after observations showing zero or negative wealth are reassigned to the bottom of the distribution to ensure that all observations from the sample are included (8.2 per cent of children and 2.6 per cent of parents have zero or negative wealth in the main estimation sample). If we redraw the density plot (Figure 1) using these age-adjusted wealth measures, the pattern is qualitatively similar, but with lower peaks.

The transition matrix for parental and child age-adjusted wealth is shown in Table 2. Each column represents the quintile of age-adjusted parental wealth. Quintile 1 is the lowest, whilst Quintile 5 corresponds to the highest level of wealth. The rows represents quintiles of age-adjusted child wealth. Each entry in Table 2 represents the conditional probability of the child having wealth in a particular quintile, conditional on the child's parents' wealth quintile.

Table 2 Intergenerational Transition Matrix: Age-Adjusted Quintiles of Log Wealth

Child age-adjusted log wealth quintile (2018)	*Parental age-adjusted log wealth quintile (2002/06)*				
	1	*2*	*3*	*4*	*5*
1 (lowest)	35	20	16	14	16
2	25	23	21	17	13
3	18	21	23	22	17
4	11	22	22	23	22
5 (highest)	12	14	19	24	32
Total	100	100	100	100	100

Note: Each column of the table shows the percentage of children in each quintile of the child age-adjusted log wealth distribution, conditional on parent age-adjusted wealth quintile. The percentage in each column sums to 100%.

If parent wealth and child wealth were uncorrelated, the conditional probability in each quintile would be uniform at 20 per cent. At the other extreme, if there is a perfect correlation between parental and child wealth, one would expect the diagonal of the transition matrix to be 100 per cent, with zeros in all other off-diagonal conditional probability. It is apparent from the table that children with parents' wealth in the top or bottom quintile are much more likely to remain in the same wealth quintile as their parents (35 per cent in the bottom quintile and 31 per cent in the top quintile). For children with parents in the second and third quintiles, the conditional probability of being in the top quintile is much lower than being in the middle quintile.

Observations from the transition matrix are congruent with the joint density plot in Figure 1, where children with parents with wealth in the highest quintile and the lowest quintile are more likely to stay at the same wealth quintile themselves.

We now turn to the correlation between child and parental wealth. Figure 2 plots the average percentile rank of child wealth for each percentile rank in parental wealth, with a linear fitted line between the child and parental percentile ranks. A non-parametric fitted curve is also plotted, and it largely overlaps with the linear fitted line. There is slight nonlinearity at the bottom quintile, where the slope of the fitted curve is steeper than the linear fit. This suggests those in the bottom quintile may have less upward mobility than the rest of the population. Overall, the relationship between the rank of child and parental wealth is close to linear.

4. Rank Correlations

We now turn to the correlations between parent and children wealth rank, controlling for age. The following equation is estimated using OLS, with standard errors clustered at the household that the child–parent pair reside in 2001:

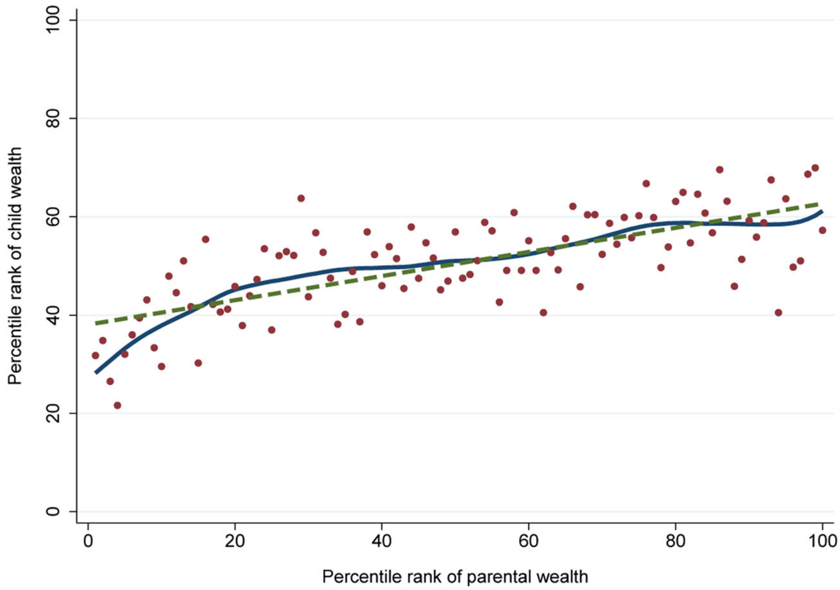
$$\text{rank}W_c = \alpha + \beta\,\text{rank}W_p + \gamma_1\text{Age}_c + \gamma_2\text{Age}_c^2 + \gamma_3\text{Age}_p + \gamma_4\text{Age}_p^2 + \epsilon_c$$

The percentile rank of wealth for each child (c) is regressed on the percentile rank of the wealth of their parent(s) (p), controlling for quadratics in parent and child age. The parameter of interest is β.[9]

Column (1) of Table 3 shows the baseline intergenerational wealth correlation estimate to be 0.253.[10] This suggests that a one percentile increase in parental wealth is associated with a 0.253 percentile increase in child wealth. This estimate is similar to the slope of the linear fit in Figure 2. Column (2) shows the estimated correlation without controlling for child or parental age. This much higher estimate of 0.332, shows that age accounts for 24 per cent of the raw wealth correlation, confirming that age is an important factor.

In Column (3), we re-estimate the correlation after limiting the sample to child cohorts who were aged 15–17 in Wave 1. This is the same restriction applied by Murray et al. (2018), who studied income mobility, and in turn based their approach on Chetty et al. (2014). This approach minimises potential selection bias, since almost all 15–17-year olds were living with their parents in Wave 1, whilst also focusing on the oldest possible cohorts where this is the case. The resulting estimate (0.212) is smaller than the baseline

Figure 2 Mean Child Wealth Percentile Rank by Parent Wealth Percentile



Note: Each point on the scatter plot represent the average percentile rank of child wealth for the observations within a percentile of parental wealth. Child and parental wealth were age-adjusted. The dashed line a linear fit. The solid curve is fitted using local linear regression with Epanechnikov kernel and bandwidth of 6.552.

Table 3 Estimated Intergenerational Wealth Rank Correlations

	HILDA			*PSID*	
	(1) *Baseline*	*(2)* *no age controls*	*(3)* *Children aged 15–17 in 2001*	*(4)* *Children living with parent in 2001*	*(5)* *All matched children*
Intergenerational wealth correlation	0.253*** (0.0246)	0.332*** (0.0241)	0.212*** (0.0517)	0.306*** (0.0269)	0.338*** (0.0207)
R^2	0.260	0.110	0.097	0.132	0.182
N	1,867	1,867	397	1,552	2,458

Note: This table shows comparable estimates of wealth rank-correlations for Australia and the United States. For both HILDA and PSID, the sample is restricted to children aged 25–64 when their wealth was observed (2018 for HILDA, 2017 for PSID), who were not living with a parent at that time, but were living with one or more parents in 2001. The exceptions are Columns (3) and (5). Column (3) shows estimates from a restricted sample, which corresponds with some precedents in the income mobility literature. Column (5) includes a broader PSID sample, which includes any children that could be matched with parent(s), not only those living together in 2001. Parent wealth is the average of wealth observed in 2002 and 2006 (HILDA), or 2001 and 2005 (PSID). Standard errors in parentheses are clustered on the 2001 household ID.

Abbreviations: HILDA, Household, Income and Labour Dynamics in Australia; PSID, Panel Study of Income Dynamics.

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$.

estimate, but subject to a large standard error. One interpretation is that intergenerational wealth persistence is lower than income persistence, since Murray et al. report a rank correlation of 0.27 for the same cohorts. However, this comparison is affected by major life cycle considerations, which are not fully understood for wealth, as we discuss in the next section.

The corresponding estimate using the PSID sample is reported in Column (4). The correlation (0.306) in PSID is higher than in HILDA. This is consistent with similar comparative work on relative income mobility, which has consistently found greater mobility in Australia than in the United States (Leigh 2007; Mendolia and Siminski 2016, Murray et al. 2018, Deutscher and Mazumder 2020). Our estimate with PSID is lower than that of Pfeffer and Killewald (2018), which ranged from 0.32 to 0.39 using child wealth at 2013 and parental wealth at 1984. A likely explanation is the sample selection procedure we have adopted to mirror our main analysis with HILDA data. The average age of children and parents in our PSID sample is consequently younger than in Pfeffer and Killewald. To verify this, we re-estimate the intergenerational wealth correlation with an extended sample in PSID, which includes any child–parent pair that was matched in PSID but need not be residing in the same household in 2001. The resulting estimate of 0.338 using this extended PSID sample is reported in Column (5).

5. Life-Course Considerations

We now consider how wealth correlations vary by age of child at the time wealth is observed. This is particularly important for our study, since children who are matched to parents in our data are generally young when their wealth is observed. We know of two previous studies that used panel data long enough to address this issue thoroughly. Pfeffer and Killwewald (2018) showed results between ages 25 and 64 for the United States; Boserup et al. (2017) for ages 20 and 44 for Denmark. Both found the correlation to increase modestly from mid-20s onwards. Boserup et al. (2017) also documented declining correlations from age 20–27.

Figure 3 (Panel A) shows results by child age at the time child wealth was observed. Each point is for a 4-year cohort group, except for the far-right point, which is for all children aged 40–64, since there are few such children in the sample. This figure includes younger children than the main analysis (from 20 years of age), to enable comparisons with Boserup et al. (2017), who observed particularly high wealth correlations for children in their early 20s.

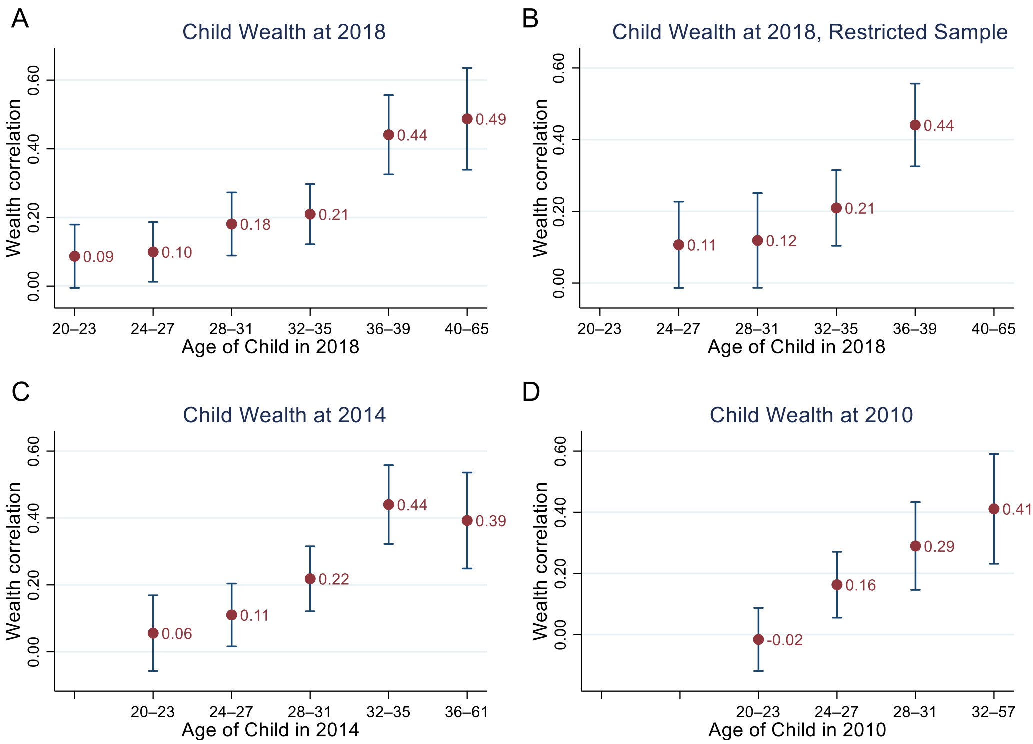
Figure 3 (Panel A) shows a clear positive relationship between age of child and the estimated wealth correlation. The correlation is lowest for children aged 20–23 at just 0.09, and highest for those aged 40–64, at 0.49.[11] This relationship between age and the correlation is considerably stronger than observed for other countries in earlier work. It therefore warrants further scrutiny.

5.1 Age or Selection Bias?

The estimated correlations in Figure 3 (Panel A) are particularly high for children aged 36 and over at Wave 18. To be included in the estimation sample, they must have lived with their parent(s) in Wave 1, when they were aged 19 or over. As shown in Table A1, much smaller percentages of children aged 19 and over were living with their parents, compared to younger children. Such children may be different to others. It seems plausible that parents may invest more in these children compared to children who leave home earlier. Indeed, co-residence is itself an important component of parental support. See Cobb-Clark and Gørgens (2014), who examine the nature of parental support in the context of intergenerational mobility in Australia. They find that disadvantaged young people are less likely to receive parental support, in terms of either financial transfers or co-residence. Also, children still at home are more likely to be in tertiary education, with a greater gradient on income/wealth over lifetime.

To explore this potential source of bias, we re-estimate the correlations for the younger cohorts, after restricting the sample to children who lived with their parents for longer. The

Figure 3 Estimated Wealth Rank Correlations by Age of Child



Note: This figure shows estimated rank correlations by age of child, using a similar approach used for the baseline results shown in Table 3. For Panel A, the estimation sample includes children aged 20–65, but otherwise follows the same sample selection procedure as the baseline analysis. Panel B shows results from a smaller sample, restricted to children who were living with parent(s) at age 19–22, thereby mimicking the sample selection criteria for the 36–39-year old group in Panel A. Panels C and D show wealth correlations for the same birth cohorts as the main analysis, but with wealth measured earlier (2014, and 2010, respectively).

idea is to impose a consistent set of sample selection rules across cohorts, making comparisons between cohorts less problematic. If sample selection bias is the driver of the results in Panel A, we should not observe an age gradient in the results generated with these restricted samples.

Specifically, the inclusion rule for each group in this restricted sample is to be living with parent(s) at age 19–22, which is the same as required (by necessity) for the 36–39-year old group in the main analysis. For example, children in the cohort aged 32–35 at Wave 18 are now only included if they were living with their parent(s) in Wave 5. Similarly, for the child cohort aged 28–31 in Wave 18, only those living with parent(s) in Wave 9 were included, while members of the 24–27-year-old cohort were retained only if living with parent(s) in Wave 13. For most cohort groups, this reduces the sample by around 50 per cent. The exception is the youngest cohort (aged 20–23 in Wave 18), who is excluded from this analysis. For that group, children would only meet the selection rules if they lived with parents in Wave 17, but not in Wave 18, leaving a very small and uninteresting sample. Note that we use the same wealth measures here as we do in the main analysis.

The results for this restricted sample are shown in Panel B. For most cohort groups, the estimates are similar to those for the full sample. They do not support the hypothesis that selection bias contributes to the strong relationship between age and the wealth correlation in Panel A.

We further explore the role of age in Panels C and D. Here, we use the same sample selection procedure to the main analysis, except with child wealth observed at earlier years—2014 for Panel C and 2010 for Panel D. Both panels show similar patterns of increasing correlations across cohorts. Combined with Panel A, these results also provide further evidence that the observed patterns are not explained by selection bias. In particular, a comparison of Panels A and D shows that for every cohort the correlation is considerably larger when using child wealth observed at 2018, compared with child wealth observed at 2010. For the cohort aged 20–23 in 2010, the estimated correlation is −0.02 for 2010 (Panel D), and 0.18 for 2018 (Panel A). For the other three cohorts where we are able to make this comparison, the estimated correlations are also higher in 2018, by 0.05, 0.15 and 0.08, respectively.

Overall, the results show a strong positive relationship between child age and the wealth correlation. This relationship appears stronger than observed for other countries in earlier work. On the available evidence, this relationship is not driven by the potential selection bias associated with linking older children with parents in HILDA.

5.2 Different Types of Wealth

We now consider whether the very strong role of child age is confined to particular types of wealth. In particular, whether property wealth or financial wealth may be driving the results.

Figure 4 Exploring Different Types of Wealth

Note: This figure explores whether the strong role of child age in wealth correlation estimates is driven by particular types of wealth. Panel A is the same as Panel A in Figure 3. In Panels B, C and D, the child wealth measure excludes property wealth, financial wealth, and ‘other’ wealth, respectively. These are mutually exclusive categories, which together account for all observed wealth.

However, no particular type of wealth seems to drive these results, as we show in Figure 4, and discuss below.

Property wealth accounts for almost half (48 per cent) of Wave 18 child wealth in the estimation sample. Almost three quarters (74 per cent) of such property wealth is held in owner-occupied homes. Figure 4 (Panel B) shows additional wealth correlations by age, this time recalculated after excluding child property wealth. These are generally similar to the main results, which are shown again in Panel A for ease of comparison.

Financial wealth also accounts for almost half (45 per cent) of child wealth. More than half (53 per cent) of their financial wealth is held as superannuation. The results are again similar when financial wealth is excluded. This is shown in Panel C. For completeness, Panel D shows results when ‘other’ wealth (which includes motor vehicles, net business assets and collectibles) is excluded. For the Panel D estimates, child wealth is hence treated as the sum of property wealth and financial wealth. The results are again similar to the other panels.

The results in Figure 4 show that the strong role of child age is not specific to particular types of wealth. This suggests that it is not driven mainly by institutional details such as Australian property markets, or by the superannuation system.

6. Conclusion

We have presented the first estimates of intergenerational wealth mobility for Australia, using HILDA. The estimated intergenerational rank correlation of wealth is 0.253. This is lower than our comparable estimate for the United States (0.306), generated with PSID and a sample selection procedure that closely follows our main analysis. This comparison is consistent with studies of earnings mobility and income mobility, which have also found lower correlations (more mobility) in Australia than in the United States (Leigh 2007; Mendolia and Siminski 2016; Murray et al. 2018; Deutscher and Mazumder 2020).

Since HILDA is still a relatively short panel survey, most of the children in the sample were relatively young when their wealth was observed. We therefore place particular emphasis on life-course considerations. Such emphasis is warranted because the correlations are highly dependent on child age, more so than has been found for other countries. These correlations vary from about 0.1 for children in their twenties, increasingly steadily to 0.5 when children are 40–64. This does not seem to be explained by selection bias due to difficulties with child–parent matching for older age groups, nor are they driven by particular categories of child wealth. These intriguing findings will be worth exploring further as HILDA continues to mature.

Studies of wealth mobility are currently not well grounded in theoretical frameworks such as the canonical work of Becker and Tomes (1979, 1986). We see the work of Boserup et al. (2017) as the most promising move towards bridging this gap. In the Working Paper version of this paper (Siminski and Wu, 2021), we attempted to use their framework and suggestions to explore the correlation of lifetime resources. However, we concluded that HILDA is not yet mature enough to achieve this successfully, mainly due to the life-course considerations mentioned above. We hope that future work on wealth mobility continues to explore its theoretical groundings.

Future research could also explore the drivers of intergenerational wealth correlations in Australia. Much of the international literature on mechanisms of wealth transmission has taken a decomposition approach. Typically, additional variables (such as income or education) are controlled for, to see how much this ‘explains’ (reduces) the wealth correlation. Since wealth correlations are associations and not causal parameters, such a mediation approach is generally problematic (Mendolia and Siminski 2017). These challenges are most clearly articulated, and most successfully navigated, by Fagereng et al. (2021). Their approach exploits quasi-random assignment of Korean-born adoptees to Norwegian families, thereby abstracting from genetic drivers.[12] They conclude that direct transfers are the most important observed mediator for wealth transmission. Child education, income and financial literacy were the other mediators considered. But we do not know whether these findings generalise to other countries, where institutions differ, or indeed at older child ages. Future research could seek to confirm whether direct transfers are the most important mechanism in Australia.[13] The role of such transfers in assisting children to buy homes seems particularly important to explore.[14]

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Endnotes

1. In Becker and Tomes (1986), parent utility is equal to the discounted sum of utilities from consumption of all descendants, assuming all generations have the same utility function. In Becker et al. (2018), parent utility is a function of consumption and expected (lifetime) resources of children.

2. Our wealth correlation estimates may be subject to attenuation bias if there is reporting error in parental wealth. We do not believe this is likely to be a major issue, given the careful wealth data collection process. The results in Table A3 support this. They show that estimates derived using parental wealth observed at one period of time (2002 or 2006) are very similar to the main estimates, for which the average of parental wealth at 2002 and 2006 is used.

3. Average parental wealth is used to reduce measurement error due to temporal shocks or reporting error. Whilst wealth may by more stable over time than income, following the literature on income measurement, temporal fluctuation in the measurement of wealth/income may occur due to unexpected shocks (Brenner 2010).

4. In practice, the oldest child included in the estimation sample under these sample selection rules was aged 58 when their wealth was observed.

5. Matching using only Wave 1 data does not exclude any potential observations from the estimation sample. Whilst all children who are born/adopted to the family are followed indefinitely in HILDA, any child born after Wave 1 would be under the age of 18 in 2018, and hence excluded from the estimation sample. It is also likely that some older children who were not residing with their parents in Wave 1 moved (back) into the parent's household in subsequent years. However, HILDA's following rules state that such children remain in the study population only whilst living in the same household as core sample members, while our estimation sample excludes children who were living with their parents in Wave 18. Therefore, any children who can only be matched with parents after Wave 1 would be excluded from the analysis, either through HILDA's following rules, or by our sample selection rules.

6. Some other studies of wealth mobility also have considerable parent–child age-gaps at the time when wealth was observed. Charles and Hurst (2003) had a similar age gap, though children and parents were both older. In Arrondel (2013) average child age was 34 whilst average parent age was 59. Parent and child age were similar in Boserup et al. (2017) and Pfeffer and Killewald (2018).

7. Further details on data construction are available from the authors.

8. If multiple observations have the same wealth measure, the number of observations in each percentile rank may not be equal. As a robustness check, we add a random number drawn from a uniform distribution between −\$1 and \$1 to the wealth measure to avoid having unequal numbers of observations in each percentile. The resulting graph is similar with or without the random number added on.

9. This approach follows Adermon et al. (2018), Boserup et al. (2017, 2018) and Pfeffer and Killewald (2018) closely in estimating the rank–rank correlation between child wealth and parental wealth with age controls, as well as related work on income rank correlations.

10. The corresponding estimate is 0.262 (SE = 0.0264) when observations are weighted using longitudinal paired enumerated person weights. The corresponding estimate is 0.248 (SE = 0.0250) if we use an estimate of personal (instead of household) wealth. The measure of personal wealth we use is described in Section 6, and Figure 4 Panel A presents further results for this wealth measure, by age of child.

11. The estimates are similar when observations are weighted using paired longitudinal enumerated person weights. The weighted estimates are 0.07, 0.05, 0.16, 0.19, 0.45, and 0.50 for 20–23, 24–27, 28–31, 32–35, 36–39 and 40+ year old children, respectively. The main results use percentile ranks defined across all children (combined) who are in these age groups, and their parents. An alternate approach is to construct percentile ranks within each child age group. Using such an approach also generates broadly similar estimates: 0.13, 0.11, 0.20, 0.25, 0.48, and 0.46 for 20–23, 24–27, 28–31, 32–35, 36–39, and 40+ year old children, respectively.

12. Even under these conditions, their analysis faced considerable challenges, such as confounding family factors (potentially correlated with parental wealth) and unobserved mediators (potentially correlated with the observed mediators). Their findings suggest that confounding family factors are unlikely to be a large source of bias (because controlling for other observed family factors does little to change the wealth correlation). But the extent of bias due to unobserved correlated mediators is less clear.

13. Recently, the Productivity Commission (2021) estimated that 36 per cent of intergenerational wealth persistence (IWP) among Australians aged 64–74 in 2018 can be attributed to inheritances. They navigate the challenges of HILDA's short panel length by imputing parental income according to observed inheritances received by children (see their Appendix Section B.1). This novel approach is worthy of further development and scrutiny.

14. Such work may be complicated by imperfect data on intergenerational transfers. Data collected to date in HILDA on transfers is ambiguous as to the inclusion of non-cash transfers, and is likely to underestimate their total value. There are plans in place for these items to be modified from 2022 onwards. See also the analysis of HILDA's wealth and transfer data quality conducted by Productivity Commission (2021: Chapter 1).

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Appendix A: Additional Tables

Table A1 Child–Parent Matching and Sample Construction by Child Age in Household, Income and Labour Dynamics in Australia (HILDA)

Age of child observed in 2001	*Children observed in 2001 (N)*	*Children observed in 2001, matched with at least one parent*		*Child–parent pair with valid wealth measures*		*Child–parent pair with valid wealth measures, excluding child residing with parents in 2018*	
		(N)	*(%)*	*(N)*	*(%)*	*(N)*	*(%)*
(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
8	330	327	99.1%	206	62.4%	135	40.9%
9	323	320	99.1%	172	53.3%	134	41.5%
10	367	363	98.9%	207	56.4%	159	43.3%
11	303	299	98.7%	153	50.5%	131	43.2%
12	337	332	98.5%	179	53.1%	157	46.6%
13	318	311	97.8%	148	46.5%	134	42.1%
14	311	309	99.4%	155	49.8%	145	46.6%
15	290	284	97.9%	149	51.4%	129	44.5%
16	305	288	94.4%	159	52.1%	143	46.9%
17	280	245	87.5%	134	47.9%	125	44.6%
18	264	207	78.4%	88	33.3%	82	31.1%
19	268	184	68.7%	84	31.3%	76	28.4%
20	252	143	56.7%	65	25.8%	61	24.2%
21	239	120	50.2%	53	22.2%	49	20.5%
22	242	93	38.4%	44	18.2%	39	16.1%
23	217	71	32.7%	24	11.1%	21	9.7%
24	218	68	31.2%	36	16.5%	29	13.3%
25	260	54	20.8%	28	10.8%	24	9.2%
26	263	44	16.7%	22	8.4%	22	8.4%
27	251	41	16.3%	20	8.0%	16	6.4%
28	282	37	13.1%	15	5.3%	8	2.8%
29	273	21	7.7%	9	3.3%	7	2.6%
30	307	30	9.8%	9	2.9%	7	2.3%
31	308	24	7.8%	7	2.3%	5	1.6%
32	285	26	9.1%	8	2.8%	4	1.4%
33	320	24	7.5%	9	2.8%	7	2.2%
34	288	14	4.9%	6	2.1%	5	1.7%
35	306	21	6.9%	4	1.3%	3	1.0%
36	294	11	3.7%	2	0.7%	2	0.7%
37	351	11	3.1%	1	0.3%	1	0.3%
38	313	15	4.8%	1	0.3%	1	0.3%
39	363	13	3.6%	2	0.6%	2	0.6%
40	325	13	4.0%	3	0.9%	3	0.9%
41	321	13	4.0%	1	0.3%	1	0.3%
42	314	11	3.5%	0	0.0%	0	0.0%
43	318	8	2.5%	0	0.0%	0	0.0%
44	318	3	0.9%	0	0.0%	0	0.0%
45	284	7	2.5%	0	0.0%	0	0.0%
46	283	5	1.8%	0	0.0%	0	0.0%

(Continues)

Table A1 (Continued)

Age of child observed in 2001	Children observed in 2001 (N)	Children observed in 2001, matched with at least one parent (N)	(%)	Child–parent pair with valid wealth measures (N)	(%)	Child–parent pair with valid wealth measures, excluding child residing with parents in 2018 (N)	(%)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
47	291	9	3.1%	0	0.0%	0	0.0%
48	269	4	1.5%	0	0.0%	0	0.0%
Total	12,052	4,423		2,203		1,867	

Note: Column (2) reports the total number of children observed in 2001 by each age in 2001. These children are aged 25–65 in 2018. Column (3) reports the total number of such children that can be matched to at least one parent in 2001. Column (4) is calculated by dividing Column (3) by Column (2). Column (5) reports the number of those child–parent pairs who were followed up in the study and had valid wealth measures. Parents above age 65 were also excluded. Column (6) is calculated by dividing Column (5) by Column (2). Column (7) reports the total number of child–parent pairs that were followed up but excludes observations where children were still living with a parent in 2018. These pairs are excluded as wealth was measured at household level. Column (8) is calculated by dividing Column (7) by Column (2).

Table A2 Child–Parent Matching and Sample Construction by Child Age in PSID

Age of child observed in 2001	Children observed in 2001 (N)	Children observed in 2001, matched with at least one parent (N)	(%)	Child–parent pair with valid wealth measures (N)	(%)	Child–parent pair with valid wealth measures, excluding child residing with parents in 2017 (N)	(%)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
8	227	216	95.2%	141	62.1%	80	35.2%
9	197	181	91.9%	114	57.9%	83	42.1%
10	195	178	91.3%	128	65.6%	100	51.3%
11	223	198	88.8%	134	60.1%	106	47.5%
12	205	189	92.2%	115	56.1%	96	46.8%
13	196	187	95.4%	113	57.7%	98	50.0%
14	206	193	93.7%	128	62.1%	116	56.3%
15	171	164	95.9%	111	64.9%	102	59.6%
16	215	197	91.6%	121	56.3%	107	49.8%
17	223	207	92.8%	139	62.3%	128	57.4%
18	216	198	91.7%	128	59.3%	116	53.7%
19	228	183	80.3%	112	49.1%	102	44.7%
20	224	140	62.5%	92	41.1%	88	39.3%
21	233	127	54.5%	79	33.9%	72	30.9%
22	221	102	46.2%	65	29.4%	60	27.1%
23	208	58	27.9%	30	14.4%	28	13.5%
24	238	54	22.7%	35	14.7%	29	12.2%
25	199	28	14.1%	10	5.0%	8	4.0%
26	216	21	9.7%	12	5.6%	9	4.2%
27	213	19	8.9%	9	4.2%	9	4.2%
28	203	6	3.0%	3	1.5%	2	1.0%
29	218	9	4.1%	1	0.5%	0	0.0%
30	206	9	4.4%	5	2.4%	5	2.4%
31	213	8	3.8%	2	0.9%	1	0.5%
32	168	6	3.6%	0	0.0%	0	0.0%
33	163	4	2.5%	1	0.6%	1	0.6%
34	169	5	3.0%	2	1.2%	2	1.2%

(Continues)

Table A2 (Continued)

Age of child observed in 2001	*Children observed in 2001 (N)*	*Children observed in 2001, matched with at least one parent*		*Child–parent pair with valid wealth measures*		*Child–parent pair with valid wealth measures, excluding child residing with parents in 2017*	
		(N)	*(%)*	*(N)*	*(%)*	*(N)*	*(%)*
(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
35	180	5	2.8%	1	0.6%	0	0.0%
36	186	1	0.5%	1	0.5%	1	0.5%
37	205	6	2.9%	1	0.5%	0	0.0%
38	196	3	1.5%	0	0.0%	0	0.0%
39	228	6	2.6%	1	0.4%	1	0.4%
40	221	2	0.9%	0	0.0%	0	0.0%
41	220	6	2.7%	1	0.5%	1	0.5%
42	241	1	0.4%	1	0.4%	1	0.4%
43	220	8	3.6%	0	0.0%	0	0.0%
44	191	2	1.0%	0	0.0%	0	0.0%
45	219	4	1.8%	0	0.0%	0	0.0%
46	222	2	0.9%	0	0.0%	0	0.0%
47	215	2	0.9%	0	0.0%	0	0.0%
48	213	2	0.9%	0	0.0%	0	0.0%
Total	8,551	2,937		1,836		1,552	

Note: Column (2) reports the total number of children observed in 2001 by each age in 2001 in PSID. Column (3) reports the total number of children observed in 2001 for which at least one parent can be matched in 2001, with the child and parent residing in the same household in 2001. Column (4) is calculated by dividing Column (3) by Column (2). Column (5) reports the number of child–parent pairs that were followed up in the study and had valid wealth measures. Parents that were above age 65 were also excluded. Column (6) is calculated by dividing Column (5) by Column (2). Column (7) reports the total number of child–parent pairs that were followed up but excluded pairs in which children were still living with their parents in 2018. These pairs were excluded as wealth was measured at household level. Column (8) is calculated by dividing Column (7) by Column (2). To examine whether there are systemic differences in parental wealth measurement across years and between parents, we report estimates of intergenerational correlation of wealth using only 2002 or 2006 wealth measurement for mother and father separately in Table A3. The estimates for each group ranges from 0.216 to 0.254, which are similar or slightly lower to the overall estimate of 0.253. As the matching rate of fathers is lower than that of mothers, the sample size of using only fathers' wealth is smaller. Matching rate to parents is also higher in 2002 than in 2006. Overall estimates using fathers' wealth only are lower than that of using mothers' wealth, and the estimates are lower for wealth measurement in 2006 than that in 2002. The lower estimates may be due to the lower matching rate, hence a higher level of attrition. PSID, Panel Study of Income Dynamics.

Table A3 Intergenerational Wealth Correlation Estimates Using Alternative Parental Wealth Measures

Alternate parental wealth measure	*Intergenerational wealth correlation*	*Number of observations*
Average parental wealth between father and mother, 2002 wealth measure only	0.250*** (0.0249)	1,820
Average parental wealth between father and mother, 2006 wealth measure only	0.242*** (0.0249)	1,797
Average wealth across 2002 and 2006, mothers only	0.254*** (0.0245)	1,806
Wealth measure in 2002, mothers only	0.253*** (0.0248)	1,761
Wealth measure in 2006, mothers only	0.235*** (0.0253)	1,721
Average wealth across 2002 and 2006, fathers only	0.226*** (0.0273)	1,496
Wealth measure in 2002, fathers only	0.216*** (0.0276)	1,461
Wealth measure in 2006, fathers only	0.224*** (0.0271)	1,410

Note: Clustered standard errors in parentheses.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.