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ABSTRACT

The Correlation of Wealth between Parents and Children in Australia*

We present the first estimates of intergenerational wealth correlation for Australia, using HILDA. The rank correlation varies greatly by child age when wealth is observed, from 0.1 before age 30, to 0.5 after age 40. Most children in our estimation sample are young. For these children overall, the estimate is 0.253. Our comparable estimate for the USA is 0.306. Wealth correlations are difficult to interpret and not well grounded in theory. We therefore also implement Boserup et al.'s (2017) framework to estimate the intergenerational correlation of lifetime resources, but conclude that HILDA is not yet mature enough for this task.

JEL Classification: D31, J62, H00

Keywords: intergenerational mobility, wealth mobility, Australia

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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 focussed on mobility in social class, occupation and education.[1] Economists have mainly focussed on earnings and income in empirical work.[2]

The theoretical foundation for the economic approach to intergenerational mobility comes from Becker & 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 whose arguments are consumption, and child's lifetime wealth (Becker & Tomes, 1979) or child utility (Becker & Tomes, 1986).[3] As discussed by Becker & Tomes (1979), lifetime wealth is conceptually equivalent to 'permanent income'. 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 – addressing issues such as attenuation bias and lifecycle bias.[4]

A somewhat neglected issue, however, is that income captured in surveys and in administrative data is far from complete. It typically excludes gifts and transfers from parents. It usually excludes noncash income, including the imputed rental value of owner-occupied housing (Saunders & 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 cash income (even if measured at midlife) and the theoretical benchmark of lifetime wealth is far from perfect. This is particularly so

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[1] Australian examples include Marks and McMillan (2003), Redmond et al. (2014), Chesters (2015).

[2] Recent empirical work on income mobility includes Chetty et al. (2014, 2017), Deutscher & Mazumder (2020) and Kennedy & Siminski (2021).

[3] In Becker & 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.

[4] The general conclusion from this literature is that income measured over several years around mid-life is a good proxy for permanent income. Such measures yield approximately unbiased estimates of permanent income mobility (Grawe, 2006; Haider & Solon, 2006; Nybom & Stuhler, 2016; 2017).

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.

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 (Alverado et al., 2018; Katic & Leigh, 2016). Public consciousness of wealth accumulation and bequests as key drivers in the evolution of inequality is particularly strong (Piketty, 2011; Piketty & Zucman, 2015). Interest in bequests and wealth transfers has hence gained momentum in recent years (Kopczuk, 2013; Boserup et al., 2018). But there have been relatively few empirical studies on intergenerational wealth mobility. Charles and Hurst (2003) is the best known early work which directly estimated intergenerational wealth mobility, for the USA.[5]

One reason for the few studies is that 'wealth' measured at a point in time is quite different to the concept of lifetime wealth that underpins Becker & Tomes' framework. Wealth, or more correctly 'net worth', at a single point in time has many determinants (Boserup et al., 2017). It is a function of earnings and other income, 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 which draw on relatively short-run panels of linked intergenerational wealth data.

Equally importantly, wealth correlations are difficult to interpret without further theoretical underpinning. Boserup et al. (2017) provide a promising framework for interpreting wealth correlations. They refer to 'lifetime resources' as the concept of primary interest. Lifetime resources are the sum of lifetime income and transfers from parents. They show that permanent income correlations underestimate the correlation of lifetime resources. They also prove that estimated wealth correlations equal the correlation in lifetime resources under certain assumptions, if one controls for the permanent income of both generations.[6]

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

[5] Other notable examples are Adermon et al. (2018), Arrondel (2013), Clark & Cummins (2015), Kubota (2017), and Pfeffer and Killewald (2018). Becker & Tomes (1986) and Charles & Hurst (2003) both cite earlier empirical work on wealth mobility, which mostly draws on small and unrepresentative samples.

[6] We discuss these assumptions in Section 6.

was first measured in wave 2 (2002), and most recently in 2018. We therefore pay particular attention to this implications of the short panel length for our analysis.

Our first (and main) approach is based on the pioneering work of Charles & 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 turn attention to life-course variation in the estimates.[7] 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.

Next, we adopt Boserup et al.'s (2017) suggestions. Their theoretical results imply that wealth should be measured at the same age for both parents and children. But they also show that for Denmark, the estimates are quite similar if child wealth is measured at earlier ages instead. Given the short length of HILDA, we are forced to use child wealth that is observed at a relatively young age for most children. Unlike Boserup et al. (2017), however, we continue to find our results to be quite sensitive to child age – with much higher correlations at older ages. We also find our results to be sensitive to the version of permanent income that we control for. We therefore cannot confidently interpret the results as revealing the correlation in lifetime resources for Australia.

The remainder of the paper is structured as follows. Section 2 describes the data and presents descriptive statistics. Section 3 presents non-parametric of wealth mobility, while section 4 presents the main estimates of intergenerational rank correlations. Section 5 addresses life-course considerations. Section 6 presents our attempt to implement Boserup et al.'s (2017) approach, and Section 7 concludes.

[7] The concept of 'lifecycle bias' is central in the income mobility literature. This is because correlation of lifetime income is usually the parameter of interest in income mobility work. In contrast, there is no established corresponding concept in the wealth mobility literature. With the recent exception of Boserup et al. (2017), the wealth mobility literature does not refer to a target parameter that is analogous to the correlation of lifetime incomes.

2 Data

We draw on data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey (Release 19). To provide a cross-country comparison, we also use US data from the Panel Study of Income Dynamics (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 2019. 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 in their mid-late thirties.

2.1 Measuring wealth in HILDA

Wealth data are collected every 4 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[8].

We create a parental wealth measure equal to the average of wealth in 2002 and 2006 (Wave 2 and Wave 6).[9] 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

[8] For details on the items used in HILDA to calculate household net worth, see Section 4.23 of Summerfield et al. (2015). For observations with incomplete or missing wealth observations, imputed wealth is used, provided with the original data, also outlined in Summerfield et al. (2015).

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

is not practical.[10] Wealth is expressed in March 2019 prices, using the consumer price index (ABS. 2020).

Throughout the analysis, we do not use weights. However, the key results are not sensitive to the use of weights, as will be shown.[11]

3.2 Sample selection

We draw on Waves 1-19 of HILDA, with wealth measured at waves 2, 6, 10, 14 and 18. For our main analysis, we follow Charles and Hurst (2003), by including children aged between 25 and 65 when their wealth was observed (Wave 18). 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.

Similar to Murray et al. (2018), we first match children in Wave 1 to their parent(s). With panel survey data, like HILDA and PSID, a child can only be matched to their parent if they are living in the same household as their parents in at least one wave. For convenience, we match all children between the age of 8 and 48 in 2001 (which corresponds to age 25 to 65 in 2018) observed in HILDA with their parents observed in the same year[12]. The match rate of children to their parents is lower for older children as they were less likely to reside with their parents in Wave 1 (see Table A1). This may introduce selection bias as the sample of matched child-parents are less representative of population. It also means that the average age of children is relatively young, as will be described.

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[10] 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.

[11] Where we refer to weighted results, the weight used is the child's longitudinal paired (wave 1: wave 18) enumerated person weight (wlear).

[12] Whilst all children who are born/adopted to the family are followed up subsequently in HILDA, any new child born after 2001 would still be under the age of 18 when they were surveyed in 2018, hence excluded from the study sample. It is likely that some older children who were not residing with their parents in 2001 moved (back) into the parent's household in subsequent years. However, HILDA's following rules state that such children would only remain in the study population when living in the same household as core sample members. Our estimation sample excludes children who were living with their parents in Wave 18. Therefore, all children who were not living with their parents in Wave 1 would be excluded from the analysis, either through the HILDA following rules, of by our sample selection rules.

Since HILDA is still a relatively short panel, child and parent wealth are only observed at most 16 years apart. Children are at a younger age when their wealth is measured when compared to the age when parents' wealth is measured.

3.3 US Data: Panel Study of Income Dynamics

To generate comparable estimates for the US, 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 US and consisted of an initial sample of close to 5000 families, including a nationally representative random sample, and an oversample 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 & 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.

3.4 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.[13] Wealth for both child and parents are positively skewed, with mean wealth significantly greater than the median wealth. As parents were generally older than children, parental wealth was in

[13] 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).

general higher than child wealth. Interestingly, price adjusted household income for parents was quite 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.

Table 1 Descriptive Statistics for Main Estimation Sample (HILDA)

Variable	**Child (2018)**	**Parent (2002, 2006)**
Age	31.97	46.90
	(5.48)	(6.91)
Percentile of wealth:		
▪ 20^{th}	24,030.4	172,098.5
▪ 40^{th}	114,537.3	450,615.7
▪ 60th	297,916.6	755,897.2
▪ 80^{th}	646,097.6	1,283,628.9
Median wealth	196,634	578,742.5
Mean wealth	453,3328	927,319.1
	(864,026.4)	(1,261,992)
Total household income	130,649.4	134,423.1
	(122,810)	(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 %

Notes: 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.

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. [14] 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 lower end of the wealth distribution themselves. There is a simlar (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 is highly correlated with wealth (Jappelli, 1999; Kapteyn et al., 2005; Lim & Zeng, 2016). Following Charles and Hurst (2003), for each generation, log wealth is regressed on a quadratic function of age. The residual is kept and observations with zero or negative wealth are reassigned to the bottom of the distribution to ensure that all observations from the sample are included.[15,16]

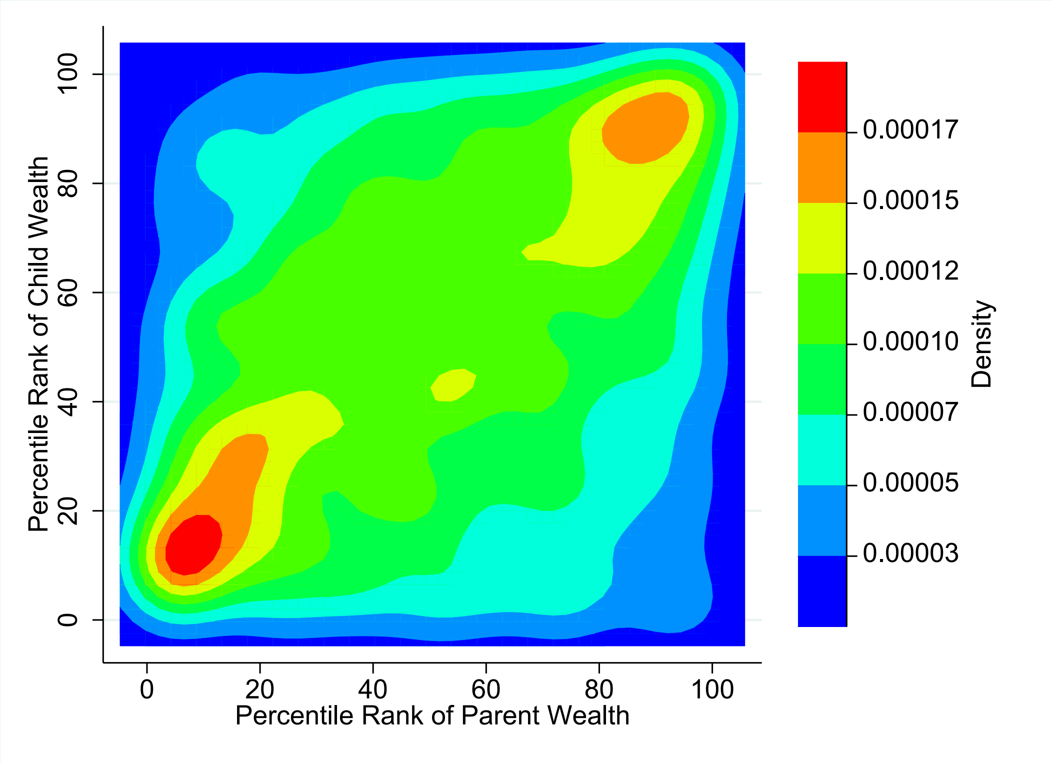
The transition matrix for parental and child age-adjusted wealth is shown in Table 2. Each column represents the quintile of age-adjusted parental wealth. Quintile 1 is the lowest, whilst quintile 5 corresponds to the highest level of wealth. The rows represents quintiles of age-adjusted child wealth. 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.

[14] 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 random number drawn from a uniform distribution between -$1 and $1 to the wealth measure to avoid having unequal numbers of observation in each percentile. The resulting graph is similar with or without the random number added on.

[15] 8.2% of children and 2.6% of parents have zero or negative wealth in the main estimation sample.

[16] If we redraw the density plot (Figure 1) using these age-adjusted wealth measures, the pattern is qualitatively similar, but with lower peaks.

Figure 1 Bivariate density plot between percentile rank of child wealth and parental wealth



Note: 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.

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

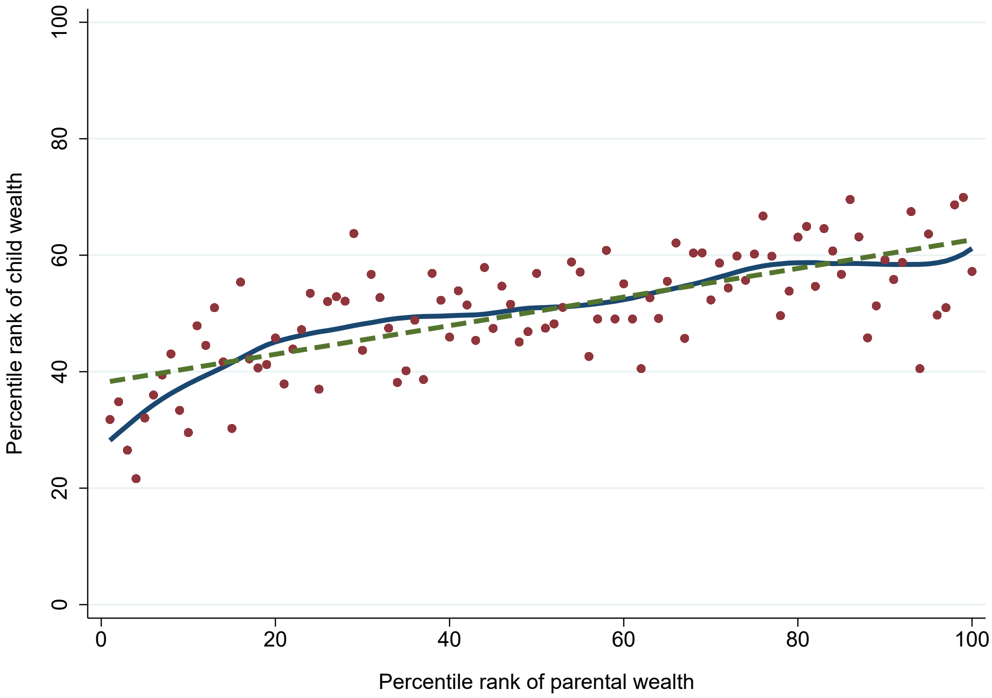
Note: Each column of the table shows the percentage of children in each quintile of the child age-adjusted log wealth distribution, conditional on parent age-adjusted wealth quintile. The percentage in each column sums to 100%.

If parent wealth and child wealth were uncorrelated, the conditional probability in each quintile would be uniform at 20%. On 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%, 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% in the bottom quintile and 31% 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 fitt. 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.

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.

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:

$$rankW_c = \alpha + \beta\, rankW_p + Age_c + Age_c^2 + Age_p + Age_p^2 + \epsilon_c$$

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.[17]

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^{***}	0.332^{***}	0.212^{***}	0.306^{***}	0.338^{***}
	(0.0246)	(0.0241)	(0.0517)	(0.0269)	(0.0207)
R^2	0.260	0.110	0.097	0.132	0.182
N	1,867	1,867	397	1,552	2,458

Notes: This table shows comparable estimates of wealth rank-correlations for Australia and the USA. 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. * p < 0.05, ** p < 0.01, *** p < 0.001

[17] 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.

Column 1 of Table 3 shows the baseline intergenerational wealth correlation estimate to be 0.253.[18] 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% 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, who 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 focussing on the oldest possible cohorts. The resulting estimate (0.212) is smaller than the baseline 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 USA (Leigh, 2007; Mendolia & Siminski, 2016, Murray et al., 2018, Deutscher & Mazumder, 2020). Our estimate with PSID is lower than Pfeffer and Killewald's (2018), whose ranged from 0.32 to 0.39. A likely explanation is the sample selection procedure we've 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).[19]

[18] 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.

[19] Pfeffer and Kilewald (2018) used child wealth from 2013 and parental wealth from 1984.

5 Life-Course Considerations

We now consider how wealth correlations vary by age of child at 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 & Killwewald (2018) showed results between ages 25-64 for the US; Boserup et al. (2017) for ages 20-44 for Denmark. Both found the correlation to increase from mid-20s onwards. Boserup et al. (2017) also documented declining correlations from age 20-27.

Figure 3 shows results by child age at the time child wealth was observed. Each point is for a four-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 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.[20] 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 are particularly high for children aged 36 and over. To be included in the estimation sample, they must have lived with their parent(s) in wave 1, when they were aged 19 and over. As shown in Table A1, a much smaller percentage 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

[20] 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.

earlier.[21] 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. 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 this these restricted samples. Specifically, the inclusion rule for each group in this restricted sample is to be living with parents at age 19-22, which is the same as for the 36-39 year old group in Panel A. For most cohorts groups, this reduces the sample by around 50%. The exception is the youngest group, who is excluded from this analysis. For that group, children would only be included if they lived with parents in wave 17, but not in wave 18, leaving a very small, and uninteresting sample.

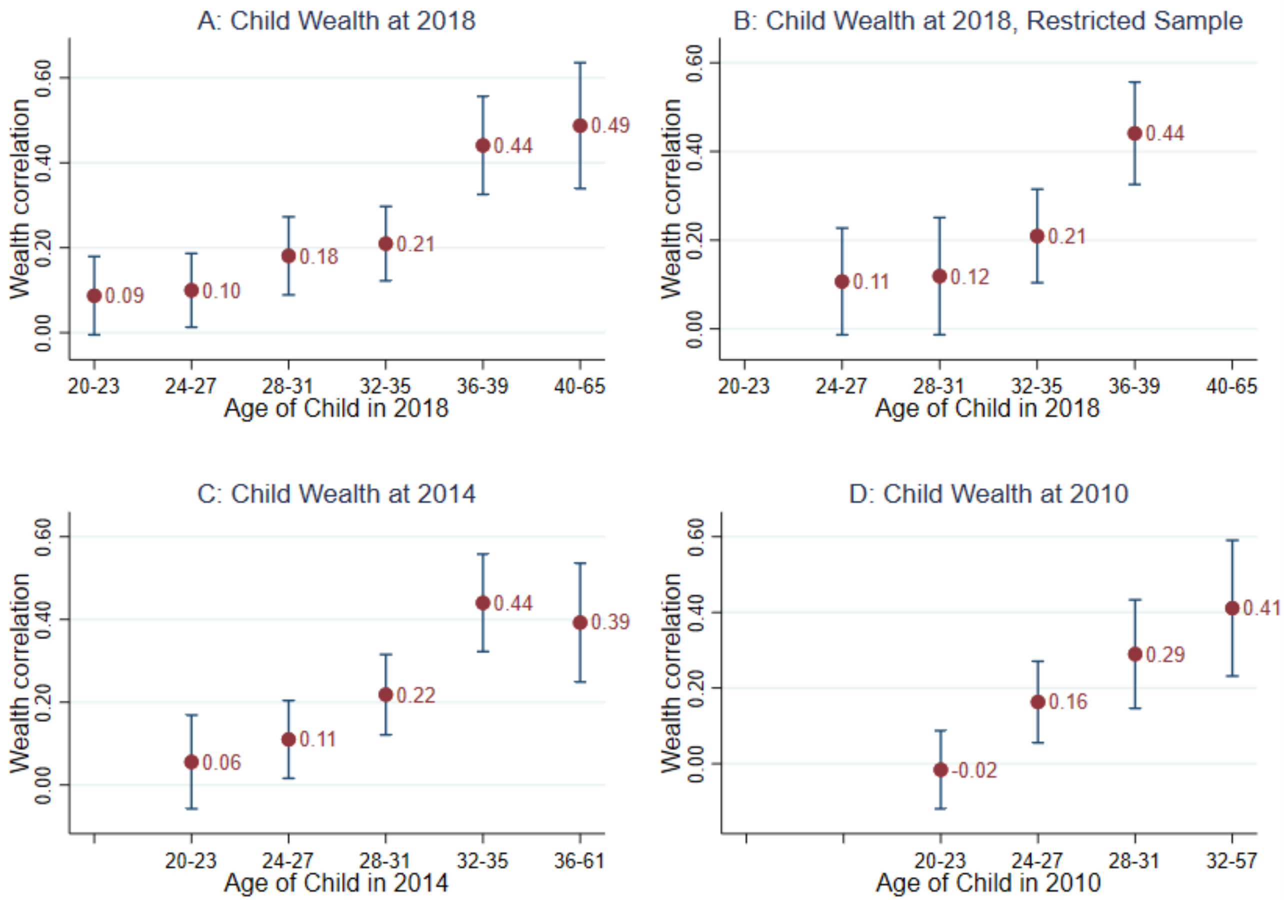
The results for this restricted samples are shown in Panel B. For most cohort groups, the estimates are similar to those for the full sample. They do not support the hypothesis that selection bias contributes to the strong relationship between age and the wealth correlation in Panel A.

Panels C and D show further correlations by age, this time with child wealth observed at earlier years – 2014 for Panel C and 2010 for panel D. Both panels show similar patterns of increasing correlations with age. 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 at 2018 than at 2010. For the cohort aged 20-23 in 2010, the estimated correlation is -0.02 for 2010, and 0.18 for 2018. For the other three cohorts where we are able to make this comparison, the estimated correlation is 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.

[21] Indeed co-residence is itself an important component of parental support. See Cobb-Clark & 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.

Figure 3 Estimated Wealth Rank Correlations by Age of Child in HILDA



Notes: 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 living with parents 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, with wealth measured earlier (2014, and 2010, respectively)

6 The Correlation of Lifetime Resources

In all of the results shown above, we have followed the majority of the literature by estimating correlations of household wealth. As we have emphasised, such correlations are difficult to interpret without further theoretical underpinning. Wealth (net worth) at a point in time is highly dependent on the age at which it is measured, and it may not be indicative of a person's lifetime resources. Nevertheless, that is the standard approach used internationally to study intergenerational wealth correlations. We now depart from that approach, and assess whether it is currently feasible to instead estimate the correlation of lifetime resources using HILDA.

Boserup et al. (2017) present a promising theoretical framework through which to study wealth correlations. They position 'lifetime resources' as the concept of primary interest. Lifetime resources are the sum of lifetime income and transfers from parents. If child utility is a function of consumption (and other factors), then lifetime resources are more relevant to utility than lifetime income. They show that estimated wealth correlations equal the correlation in lifetime resources under certain conditions, and arguably strong assumptions. The main assumptions are that both generations follow the same life course paths of consumption, savings, and investments/transfers to children. The conditions are: (i) wealth can be measured at any age, but must be measured at the same age for parents and children; (ii) one must control for the permanent income of both children and parents in the regression.

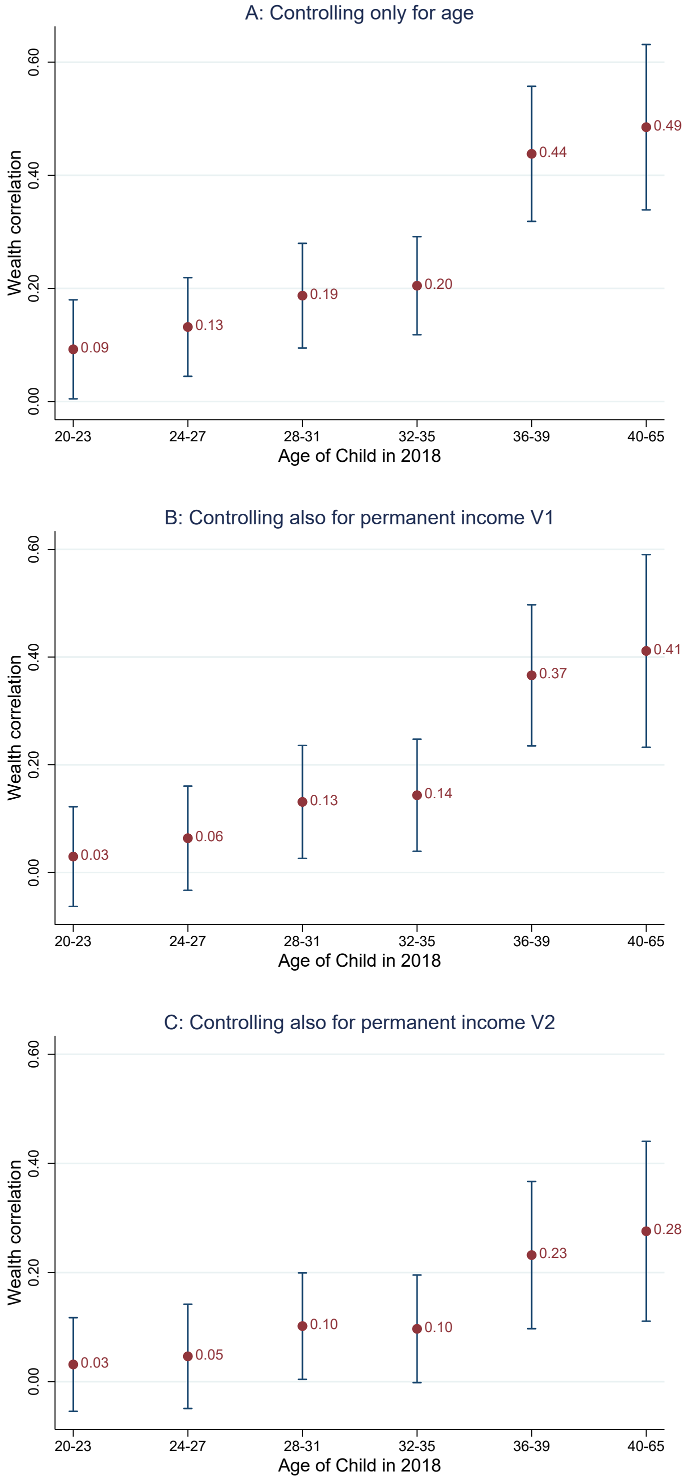
Given HILDA's short length, we do not observe wealth for parents and children at the same age. Further, for most children, we do not observe income during middle age, and hence we may not obtain a reliable proxy for permanent income. However, Boserup et al. also show that for Denmark, the estimates are not sensitive to the age at which children's wealth is observed, from ages 25 to 45. We therefore follow Boserup's approach as closely as possible. We use estimated personal wealth, departing from our focus on household wealth in previous sections of the paper.[22] To approximate permanent income, we average income over three years. For parents, we use the earliest three years available (2001-2003). For children, we use the latest three years available (2017-2019). Many of the children, in particular, are of childbearing age, so it is not clear whether personal income at these ages is a good proxy of permanent income. We therefore construct two versions of this variable. In the first version, we use personal

[22] HILDA only provides a measure of wealth at the household level. We estimate individual wealth by setting it equal to household wealth divided by the number of adults in the household, excluding adults whose relationship in the household is coded as a child of other household members. We continue to use average wealth of parents at 2002 and 2006. In each year, if both parents are present in the data, we use the average wealth of the two parents.

income. In the second version, we use the average income of each child and their spouse, thereby assuming income pooling.

Figure 4 shows the main results, by age at which child wealth is measured. Panel A shows results without controlling for permanent income. These are quite similar to those in Figure 3 Panel A, demonstrating that shifting from household wealth to personal wealth has little effect on the correlations. Panels B and C shows results with linear controls for the permanent income rank of both parents and children. In both versions, each estimate is lower than in Panel A. However, the results are sensitive to the version of permanent income used. Most importantly, unlike Boserup et al.'s results for Denmark, these correlations are highly dependent on the age at which wealth is measured. It is therefore difficult to be confident that the correlations we estimate using these age groups can be treated as valid estimates of the correlation of lifetime resources, as advocated by Boserup et al. (2017).

Figure 4 Alternate Estimates of Wealth Rank Correlations by Age of Child in HILDA



Notes: This figure shows alternate estimates of intergenerational personal wealth rank correlations by age of child. The approach follows the suggestions of Boserup et al. (2017), which draws on wealth correlations to infer the correlation of lifetime resources. It uses estimates of personal wealth. For Panels B and C, the regressions control linearly for the percentile rank of permanent income for both children and parents. For Panel B, permanent income is estimated using personal income, averaged over three years. For Panel C, permanent income is defined differently, assuming that income is shared equally between partners in each year.

7 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 USA (0.306), generated with PSID and a sample selection procedure which closely follows our main analysis. This comparison is consistent with comparative studies of earnings mobility and income mobility, which have also found lower correlations (more mobility) in Australia than in the USA (Leigh, 2007; Mendolia & Siminski, 2016; Murray et al., 2018, Deutscher & 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. Our analysis reveals that 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 20s, 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. However, this will become clearer as HILDA continues to mature.

Unlike studies of income mobility, studies of wealth mobility are currently not well grounded in theoretical frameworks such as the canonical Becker & Tomes (1979, 1986). We see the work of Boserup et al. (2017) as the most promising movement towards bridging this gap. We have attempted to use their framework and suggestions to explore the correlation of lifetime resources. However we conclude that HILDA is not yet mature enough to 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 & 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.[23] They conclude that direct transfers are the most

[23] 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

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. The role of such transfers in assisting children to buy homes seems particularly important to explore.[24]

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.

[24] Such work may be complicated by imperfect data on parental transfers. For example the HILDA data items explicitly exclude non-cash transfers. Other gifts – such as residential property, cars, direct payment of rent or university fees are excluded. Similarly, the inheritance data excludes the value of residential property and other forms of non-cash wealth.

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Table A1 Child-parent matching and sample construction by child age in HILDA

Age of child observed in 2001	Child observed in 2001 (N)	Child observed in 2001, matched with at least one parent (N)	(%)	Child-parent pair with valid wealth measure (N)	(%)	Child-parent pair with valid wealth measure, excluding child residing with parents in 2018 (N)	(%)
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%
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 1 reports the total number of child observed in 2001 by each age in 2001. This correspond to the child aged 25 to 65 in 2018. Column 2 reports the total number of child observed in 2001 that at least one parent can be matched in 2001. Column 3 is calculated by dividing column 2 by column 1. Column 4 reports the number of child-parent pair that was followed up in the study and had valid wealth measure. Parents that were above age 65 was also excluded. Column 5 is calculated by dividing column 4 by column 1. Column 6 reports the total number of child-parent pair that was 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 7 is calculated by dividing column 6 by column 1.

Table A2 Child-parent matching and sample construction by child age in PSID

Age of child observed in 2001	Child observed in 2001 (N)	Child observed in 2001, matched with at least one parent (N)	(%)	Child-parent pair with valid wealth measure (N)	(%)	Child-parent pair with valid wealth measure, excluding child residing with parents in 2017 (N)	(%)
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%
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	8551	2937		1836		1552	

Note: Column 1 reports the total number of child observed in 2001 by each age in 2001 in PSID. Column 2 reports the total number of child observed in 2001 that at least one parent can be matched in 2001, with the child and parent reside in the same household in 2001. Column 3 is calculated by dividing column 2 by column 1. Column 4 reports the number of child-parent pair that was followed up in the study and had valid wealth measure. Parents that were above age 65 was also excluded. Column 5 is calculated by dividing column 4 by column 1. Column 6 reports the total number of child-parent pair that was 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 7 is calculated by dividing column 6 by column 1.

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 father's 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.

Table A3 Intergenerational wealth correlation estimate 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)	1820
Average parental wealth between father and mother, 2006 wealth measure only	0.242^{***} (0.0249)	1797
Average wealth across 2002 and 2006, mothers only	0.254^{***} (0.0245)	1806
Wealth measure in 2002, mothers only	0.253^{***} (0.0248)	1761
Wealth measure in 2006, mothers only	0.235^{***} (0.0253)	1721
Average wealth across 2002 and 2006, fathers only	0.226^{***} (0.0273)	1496
Wealth measure in 2002, fathers only	0.216^{***} (0.0276)	1461
Wealth measure in 2006, fathers only	0.224^{***} (0.0271)	1410

Notes: Clustered standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$