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Parental Income and Children’s Well-being and Future Success:
An Analysis of the SIPP matched to SSA Earnings Data*

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1. Introduction

A vast literature in the social sciences has studied the association between parental income and children’s outcomes to establish the importance of parental economic resources on children’s well-being. One limitation of nearly all of these studies is the lack of availability of parental earnings histories over long periods of time for a very large and representative sample of families in the US. It is well established that the bias in using single year measures of parental income to proxy for long-run income can be sizable and can vary by parental age (Solon, 1992; Mazumder, 2005; Haider and Solon, 2006). Similarly, few studies have been able to distinguish the relative importance of parental income obtained in specific periods of the life course of the child. A growing literature has shown that there are critical periods in childhood development where material resources may be especially valuable (e.g. Cunha and Heckman, 2007, Almond and Currie, 2010). This paper addresses these issues by assembling a rich intergenerational dataset containing measures of parental income taken over many years and at various points of the life course of the child.

An even bigger challenge is to convincingly demonstrate that the statistical associations between parental income and children’s outcomes truly reflect causal processes and are therefore amenable to policy interventions. This paper like most of the preceding literature settles for providing descriptive estimates that may nonetheless prove informative for future research and provide a better backdrop for policy discussions. Improving our understanding of the true association between parental resources and children’s outcomes may still be useful until we are able to obtain convincing estimates of causal effects. For example, larger estimates for particular outcomes or at particular stages of the lifecycle may provide important suggestive results.

In order to construct an intergenerational sample I pool families from the 1984, 1990-1993, 1996, 2001 and 2004 SIPP panels. Each of these SIPP samples were matched to earnings histories contained in
SSA administrative earning records. I use the administrative data to construct long-term time averages of
parents’ earnings.

There are two distinct parts of the analysis. In the first part, I use these time averages of parent
earnings to estimate the association between parent income and childhood well-being. I use SIPP topical
modules on Children’s Well-Being, Functional Limitations and Disability, Health Status and Utilization
of Health Care and Extended Measures of Well Being to obtain a broad set of measures related to
childhood health and well-being. In the second part of the analysis, the earnings of the children as adults
are the main focus of the analysis and I show the extent to which the importance of parent income differs
over the life-cycle of the child.

2. Background

Parent Income and Child Well-Being

There is an enormous literature that discusses the many potential determinants of childhood well-
being. This paper focuses on the role of just one of these factors, parent income, and in particular,
parental labor market earnings.\footnote{In this section I use “income” for ease of exposition but all of the empirical estimates concern labor market earnings.} It has been well established that parent income is clearly amongst the
most important if not the most important determinant of child well-being (Brooks-Gunn and Duncan, 1997). Given the critical role of parent income it is critical that it be well measured and its effects
estimated as accurately as possible.

Issues related to the measurement of parental income have played an important role in the
development of the literature on intergenerational economic mobility. Researchers have typically
estimated a regression of children’s log income on parent’s log income. The regression coefficient also
known as the intergenerational elasticity, measures the degree of persistence in income and one minus this
coefficient has been used to infer the degree of intergenerational mobility. The first set of estimates of
this regression typically used only single year measures of income in each generation producing estimates of the intergenerational elasticity of income of around 0.2. This suggested that there was substantial mobility in the U.S. and that on average income differences between families would be wiped out within three generations (Becker and Tomes, 1986).

The central idea that changed the consensus view of intergenerational mobility stems from Milton Friedman’s (1957) insight that economic behavior is more strongly related to permanent income than transitory income. Bowles (1972) was perhaps the first to apply this idea in the context of intergenerational mobility by suggesting that the transitory income fluctuations could bias down estimates of the degree of intergenerational persistence that were based on just using a single year of income. Solon (1989, 1992) demonstrated the bias more formally and showed how even short multi-year averages of income could dramatically reduce such bias. Solon’s (1992) study substantially revised the consensus view of the intergenerational elasticity in income from 0.2 to 0.4 or possibly higher.

Building on Solon’s work, Mazumder (2005) argued that persistent transitory fluctuations could lead to non-negligible bias in even short term averages of parent income. Using both simulations and actual estimates based on a new intergenerational sample derived from the 1984 SIPP matched to social security earnings records, Mazumder argued that estimates of the intergenerational elasticity in the US may be as high as 0.6. This would suggest that earnings differences would take several more generations to be eliminated. Simulations from Mazumder (2005) also provide estimates of the reliability ratio for multi-year averages of income when the explanatory variable of interest is permanent income. These show that coefficients using a single year of parent income are biased down by about 50 percent and that even 5 year averages are biased down by about 30 percent.

For the most part, however, this key insight regarding the importance of averaging parental income over many years has not been utilized by social scientists for analyzing most other outcomes. An exception is a recent paper by Rothstein and Wozny (2009) who show that much more of the black-white
test score gap can be explained when using long-term averages of parent income than using just one year. This substantially revises the estimates of Fryer and Levitt (2006) concerning the amount of the black-white test score gap that is unexplained.

One important reason why researchers have not explored the role of long-term averages is simply due to data considerations. Outside of the PSID, few data sources contain panel data on parental income.\(^2\) A limitation of the PSID is that sample sizes can be relatively small. This limitation is amplified if one is concerned about the representativeness of the sample due to ongoing attrition since the beginning of the sample in 1968.\(^3\)

A more fundamental issue is whether the explanatory variable of interest in a particular study ought to be permanent income or current income. In some cases this may be clear from theoretical considerations and may depend on the plausibility of the existence of borrowing constraints. In many other cases where researchers are interested in purely reduced form or descriptive statistical associations (as is the case with intergenerational mobility), this may come down to a judgment call. In the case of understanding the importance of parental income on childhood well-being there is a strong case to be made that the object of interest is income received during the childhood of the child (perhaps under the assumption that borrowing constraints exist for some households). In this case it may be optimal to use a short-time average (e.g. 5 to 10 years) of parent income both because we are interested in income during this period and because of the desirable properties of time averages in reducing bias. Given the potential important changes in interpretation of existing socioeconomic gaps or income gradients there is a strong case for at a minimum, exploring how estimates are altered when using multi-year averages of parental income.

The Timing of Parental Income

\(^2\) An exception is the Children of the NLSY, where income from the parents can be linked from the NLSY79.

\(^3\) An additional issue is raised if one uses data from the SEO (oversample of poorer households) component of the survey where there were problems with the implementation of the initial sampling scheme (Brown 1996).
In recent years there has been an explosion of interest in the importance of conditions early in life on long term outcomes. Much of this has stemmed from insights from the literature on the developmental origins of adult health and disease. In the economics literature, recent work on human capital formation has emphasized the importance of early life influences on cognitive and non-cognitive skill development (Cunha and Heckman, 2007). It is natural therefore, to consider whether the parental income received in particular stages of the life cycle of the child may be particularly important for children’s long-term outcomes. One working assumption is that borrowing constraints affect at least some families so that they are unable to borrow funds from their expected future earnings to finance investment in their children. In any case, it would certainly be valuable to estimate some descriptive statistics. For example, if one were to find no significant differences over the life course of the child this would provide some suggestive evidence in support of the notion that borrowing constraints are not empirically relevant.

A few studies have attempted to identify the importance of parental income in particular periods of childhood (e.g. Case, Lubotsky and Paxson, 2002; Brooks-Gunn et al, 1997). These studies have in most cases been limited by small samples and typically do not have the power to reject differences in the effects by the age of the child even when differences in point estimates may be qualitatively large. A more fundamental concern is that these studies may not have adequately concerned issues concerning life cycle biases (e.g. Haider and Solon, 2005). If for example, income is better measured at some periods of the parent lifecycle than others (Mazumder, 2001) then unless fertility patterns are constant over the life cycle (which they clearly aren’t) then this could mechanically affect estimates of the effect of parental income by child age.

3. Data and Methodology

match rates are high in most years and that selection is not a major concern.\footnote{I am in the process of writing a technical memo that has not yet gone through Census disclosure that will document this in greater detail. Mazumder (2005) has previously shown that the match rate between the 1984 SIPP panel and the SER data is around 90 percent and that correcting for selection based on inverse probability weighting has little effect on intergenerational regressions.} The SER data covers annual earnings over the period from 1951 to 2007, while the DER data is available from 1978 onward.

There are two aspects to using SER records that raise potential issues. The first is that some individuals who are working are not covered by the social security system and their earnings will be recorded as zero. Second, earnings in the SER data are censored at the maximum level of earnings subject to the social security tax. While in principle the DER data is not subject to either of these problems, an examination of the data shows that the DER data actually shows higher rates of non-coverage than the SER data. Since the non-coverage patterns are different in the two datasets, I take the maximum of earnings in a year between the SER and DER to minimize the bias due to non-coverage. In the second part of the analysis I only use the SER data and exclude those in the non-covered sector because the DER data is only available from 1978 onward. To deal with the top-coding problem in the SER I impute earnings among the topcoded for each year starting in 1961 by using the March CPS and calculating the mean earnings among those above the topcode by cells defined by race, sex and education.

For both parts of the study the sample consists of children who were 0 to 20 years old and who were co-resident with at least one parent at the time of their first interview.\footnote{Due to a coding error the current results inadvertently exclude many single parent families. Correcting for this only makes most estimates larger. Future versions of this paper will correct this coding error.} The sample was also restricted to children whose parents were between 15 and 45 years old when the child was born. For the first part of the analysis, I progressively average parent earnings over 1, 3, 5 and 7 years. The earnings of the father are used if his earnings were positive for all 7 of the years. If not, I use the mother’s earnings if her earnings were positive in all 7 years. If neither had positive earnings in all 7 years, the family was dropped from the sample. The time period covered by the averages ends in the year prior to the interview in which the observation is used. For example, if an outcome was taken from an interview that occurred in
October 1992, the one-year average would use 1991 income, the three-year average would use 1989 through 1991 income, the five-year average would use 1987 through 1991 income and the seven-year average would use 1985 through 1991 income. Consequently, time averages were generated for each specific outcome depending on the SIPP panel used and the year of the interview.

For the child well-being analysis I organize the outcomes into five distinct groups. Summary statistics are shown in Table 1. The first group is a set of general health outcomes. This includes: 1) an indicator for a physical, learning or mental condition that limits schoolwork which is asked of children 5 or older; 2) an indicator for a physical, learning or mental condition that limits child behavior asked of children younger than 5; 3) an indicator for poor health (based on health status rated on a 1 to 5 scale where 1 is excellent and 5 is poor) that combines health status reported in the Children’s Wellbeing Topical Module and health status reported in the Functional Limitations and Disability Topical module; 4) an indicator for spending the night in a hospital in the last year; 5) the number of nights spent in the hospital in the last year; and 6) the number of days in the last four months that illness or injury kept the individual in bed for at least half the day.

The second set of outcomes deal with health care utilization and include: 1) the number of times the child talked to a doctor in the last year; 2) the number of dentist visits in the last year and 3) an indicator for using prescription drugs daily. The third group of outcomes is also health related and includes three anthropomorphic measures of children below the age of 5. These include: 1) a weight-for-height Z-score; 2) a weight-for-age Z-score; and 3) a height-for-age Z-score. All three measures are calculated using 2 or 3-month age bins separately for males and females.

The fourth group of outcomes examines a range of childhood educational measures. These include: 1) the number of times the child changed schools; 2) an indicator for having repeated a grade; 3) an indicator for having been suspended or expelled; 4) an indicator for having received special education services; and 5) an indicator for having a learning disability.
The fifth and final set of outcomes examines a range of measures related to home environment and family resources. These include: 1) the number of times a child was read stories in the last week; 2) an indicator for whether the child had ever been in day care; 3) an indicator for not being able to meet basic needs (food, rent, utilities, etc.) at some point in the last year; 4) an indicator for the family not having enough food in the last four months; 5) a count of the number of days without enough food or money to buy food in the last month; and finally 6) an indicator for a family member skipping a doctor visit when he or she needed to go. The last four outcomes are estimated using one observation per family.

All of the outcomes (denoted by $y_i$) are multiplied by 100 for convenience in displaying and interpreting results. This is mostly useful for the indicator variables so that the coefficients can be interpreted as percentage point effects. For each regression, I include a basic set of covariates (“Basic Controls”) which consist of indicators for survey year, child age when the outcome was measured, race, ethnicity, gender, state of residence and an indicator for using father’s earnings. A more extensive set of controls (“Added Controls”) includes all of the basic controls and adds parent education, parent health status and parent age. For the regressions dealing with health I have also added a third set of controls which also include separate indicators for the father, mother and child having private health insurance. These controls are contained in the vector $X_i$. The analysis does not include survey weights but estimates (not shown) are very similar when I include them.\(^6\) The main object of interest is $\gamma$ in the equation below.

\[
(1) \quad y_i = \gamma \text{Inc}_i + \beta X_i + \epsilon_i
\]

The second part of the analysis examines differences in the effects of parental income over the life course of the child by examining children’s earnings as adults as an outcome. To gain some further insight into the issue of timing of income I also examine two other outcomes for which we may have some a priori judgments about which periods of the lifecycle of the child might matter most. The first is the probability of college enrollment after completing high school. If borrowing constraints bind for

\(^6\) Survey weights can help correct for oversampling of poorer households in more recent SIPP and adjust for attrition bias. Future drafts of the paper will incorporate weights.
some families then we might expect parental income received just prior to entering college to matter most. The second experiment is to consider health status during young adulthood. Here the thought is that health early in life might matter for determining long-run health and so perhaps, parental income received early in life might matter most.

This part of the analysis utilizes four samples. The first two samples were used when the outcome of interest was son’s earnings between 2003 and 2007. I excluded daughters because a major limitation of the administrative data is that I have no information on labor supply. The more conservative sample included 4477 sons born between 1964 and 1975, so the youngest sons were 28 in 2003. The less conservative sample included 9506 sons who were born between 1964 and 1980, so the youngest sons were 23 in 2003. The third sample was selected to analyze the impact of parental income across a child’s life cycle on the probability of enrolling in college in the first year after high school and included 1174 sons who entered the 12th grade after the 1st wave of the SIPP and at least two years before the end of their SIPP panel. The final sample included 4627 sons who were at least 15 at the time of the Functional Limitations and Disability topical module interview.

The parent income measures were mapped from calendar year to child age. For this exercise, I only used SER income data because the shorter length of the DER series (which only begins in 1978) is too restrictive for the cohorts available. The income measures linked to child age were then used to generate separate time averages of parent income covering the ages: -3 to -1, 0 to 5, 6 to 11, 12 to 17 and 23 to 28. Father’s SER income was used in all of the averages if he had positive earnings in 14 of the 21 years beginning three years before his child was born and ending when his child was 17. The natural log of the time averages was used in the analysis. We used mother’s income if she had positive income in 14 of the 21 years and the father did not. To be included in the analysis, one of a child’s parents was

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7 I have found in previous attempts to run intergenerational regressions with daughters using administrative data that there are major differences in the results when compared to analogous estimates from survey data where one can account for labor supply.
required to have positive earnings in 14 of the 21 years. Summary statistics for all of the earnings measures for each sample are shown in Table 7.

Son’s earnings between 2003 and 2007 were calculated using the same methodology as the parental income measures in the first part of the analysis. That is, for each year the greater of the SER and DER value was used. Only sons with at least one year of positive earnings were included in the sample. The denominator of the average used the number of years with positive earnings.

I generated a measure of college enrollment by looking at all students who entered the 12th grade after the start of their SIPP panel (to minimize the chance that they were repeating the grade) and at least two years before the end of their panel. I then generated an indicator for whether they enrolled in college in the year after they completed the 12th grade or, for students who spent over a year in the 12th grade, in the year after the year in which they entered the 12th grade. I measured health using the health status measure rated on a 1 to 5 scale by the respondent taken from the Functional Limitations and Disability Topical Module. I used this health status measure to code an indicator for having poor health (health status equal to 4 or 5). For each of the outcomes, I then run regressions of the form:

\[
y_i = \gamma_1 \text{Inc}_{3\text{mo}-1} + \gamma_2 \text{Inc}_{6\text{mo}5} + \gamma_3 \text{Inc}_{6\text{mo}11} + \gamma_4 \text{Inc}_{12\text{mo}17} + \gamma_5 \text{Inc}_{23\text{mo}28} + \beta X_i + \epsilon_i
\]

The vector of Xs in all regressions of sons earnings include: a quadratic in the age of the son; a quartic in the age of the parent; parent education; parent health status; and indicators for the use of father’s earnings, the SIPP panel, black, other race, hispanic and state of residence. In some specifications I also include parent earnings from the period when the child was between the ages of 23 and 28. This, in some sense, controls for permanent income and uses earnings from a period in which parental investments are not expected to matter as an additional test. Other important specifications also include interactions of each bin of parent income with a quadratic in parent age-40 and/or interactions of each bin of parents’ income with a quadratic in children’s age minus 35. This allows for controls for possible life cycle bias (Haider and Solon, 2006, Lee and Solon, 2009) and the estimates for the gammas now reflect the effects for
parents at age 40 and for kids at age 35. Finally, in regressions using college enrollment or health as an outcome I revert to the covariates used in part 1 of the analysis where I have a basic set of controls and an added set of controls that add parent education and parent health status.

4. Results

Child Well-Being

I start by presenting the results concerning children’s health outcomes in Table 2. The first entry in the table, -1.049 shows the coefficient on parental income where the outcome is a health condition that prevents school work. The standard error is about 0.18 and the effect is significant at the 1 percent level. The point estimate suggests that a doubling of parental income would lead to a reduction in the probability of such a health condition by a little more than a full percentage point. Since the mean rate of such health conditions is about 5 percent, this would be about a 20 percent effect size evaluated at the mean. This would essentially be what the typical researcher would estimate when using only the current income available in the survey. Moving across the row the next three columns shows how the effect changes as the length of the time average is increased. In column (4), when I use a 7 year time average, a doubling of income now reduces the probability of a schoolwork limiting health condition by about 1.5 percentage points. Essentially moving from a single year of parent income to a 7 year average raises the coefficient by 42 percent.

Columns (5) through (8) use the same lengths of time averages but now include the added set of covariates on parental characteristics. This sharply reduces the point estimates. For example, when using a 7 year average (column 8), a doubling of parental income now reduces the probability of a health condition by 1.1 percentage points. The difference between using a single year and a seven year average, however, remains substantial. In fact, the coefficient on using a seven year average is now 60 percent higher than the coefficient on a single year of income. Finally, in column (9) I continue to use a 7 year average but now include a set of indicators for health insurance coverage. This further reduces the effect
of doubling parent income to about 1 percentage point. Whether one wants to condition on health insurance coverage depends on the question of interest. Since one reason why parental income might matter for health is precisely because it enables one to access health insurance, it may be of interest to know the full effect of income unconditional on insurance coverage. For that reason, I focus on column (8) as a preferred specification, though the results in column (9) are certainly of interest and are suggestive of the potential role of health insurance access.

The next row shows the analogous effects on the probability of a health condition that affects the behaviors of children at or below the age of 5. Here I find negative point estimates that are smaller in absolute value than the results from the previous outcome, however, the incidence rates are much lower for this outcome. The general pattern of much larger effects for longer time averages is also apparent. However, because the samples for this more limited age range are considerably smaller (N=2811), the standard errors are considerably larger and the effects are not statistically significant at conventional levels. Nevertheless, despite the lack of precision, the point estimates imply very large effects of close to 50 percent evaluated at the sample mean.

I next turn to the outcome of having reported poor health status for one’s child. The results in column (4) using a seven year time average suggests that a doubling of parent income would reduce the probability of poor health among children by more than half a percentage point, an effect that is significant at the 5 percent level. Using only a single year of income would lead to a coefficient only one-third the size. However, moving to columns (5) through (9), it is clear that adding controls for parental status entirely removes the effects of parent income. While a lack of a finding is in some sense a “negative result” it provides some helpful information. It suggests that the effects of parent income on the incidence of poor health may simply proxy for other parental characteristics like health status. However, for other outcomes where the results do not go away, it may be suggestive that parent income is not simply serving as a proxy for other parent characteristics.
For the probability of staying overnight in a hospital, the results are fairly robust. All of the coefficients on the time averages of parent income are negative and significant, at a minimum, at the 10 percent level. The results are similar even when controlling for covariates (columns 5 to 8) and even when I include health insurance status (column 9). A doubling of parent income reduces the probability that a child will stay overnight in a hospital by about 0.3 to 0.4 percentage points an implies an effect size of about 15 to 20 percent evaluated at the mean. If we focus on the preferred specification in column (8), we also find negative coefficients that are significant at the 10 percent level for the number of days spent in a hospital and the number of days that illness kept the child in bed at least half the day. The effect sizes from these outcomes range from 15 to 30 percent.

In Table 3, I turn to health care utilization outcomes. I find that for all three outcomes using a 7-year average leads to substantially higher point estimates on the effect of parental income than using a single year. The coefficients increase from anywhere between 15 percent to 44 percent. Notably, controlling for parent characteristics reduces, but does not eliminate, the effects which are all generally highly statistically significant. Using the column (8) results, the effect sizes for these outcomes, evaluated at the mean, range from 6 percent to 12 percent.

In Table 4, I examine the effects of parental income on the height and weight of children under the age of 5. The first outcome examined is the z-score of weight for height which is sometimes used to classify “wasting” or malnutrition for low levels. Of course, high levels can also be indicative of potential problems such as Type II diabetes. In the first four columns, I find a small negative effect of between 0.02 and 0.04 standard deviations from a unit increase in log parental income that is statistically insignificant. Once I control for covariates this effect is virtually eliminated. Similarly no consistent, let alone, statistically significant effect is found for weight for age. Interestingly, a small positive and statistically significant effect of about 0.06 to 0.08 standard deviations is found for height for age but this effect does not rise with the length of the time average. More importantly, the effect is sharply reduced and is statistically insignificant once I control for other parental characteristics. On the other hand, given
the relatively small sample it maybe that there is a small positive effect of parental income that cannot be uncovered with this data.

The fourth set of measures of child well being deal with educational outcomes and the results are presented in Table 5. I find a negative effect on the number of school changes that rises with the length of the parental income time average. Using a 7-year average with the baseline controls yields a marginally significant effect. However, this effect is no longer statistically significant when adding additional parental characteristics. Nonetheless, the imprecisely estimated coefficient is suggestive of an effect size of about 9 percent evaluated at the mean. The effects are highly robust for the next three outcomes. The specification in column (8) suggests that doubling parent income reduces the likelihood of repeating a grade by 2.6 percentage points (39 percent effect size), reduces suspensions by 1.6 percentage points (22 percent effect size) and reduces placement in special education classes by 1.4 percentage points (16 percent effect size). Further, these results are a powerful example of how lengthening the time averages of parent income can dramatically alter the size of the estimated effects. I find that the estimated coefficients are between 70 and 90 percent larger when using a 7-year average than when using current year income. For learning disability, I find that some statistically insignificant income effects appear when using only the baseline controls but that these are completely removed when controls for parental characteristics are added.

The estimates for the final group of measures on home environment and family resources are shown in Table 6. The results are a mixed bag. I find that for three of the outcomes, number of times read stories, ever in daycare, and days without food, there are highly significant effects when I use the baseline controls that are dramatically reduced and no longer significant once I further control for parental characteristics. However, for the other outcomes, inability to meet basic needs, food inadequacy and skipped doctor visits, the results are highly robust. The implied effect sizes for these outcomes are 55 percent, 62 percent and 39 percent, respectively.
Even though it may seem obvious that parental income will be strongly associated with two of the outcomes (inability to meet basic needs and food inadequacy) since these outcomes essentially reflect the availability of parental resources, it is important to point out that once again, the 7 year time averages yield significantly higher coefficients than current income. For example for food inadequacy, the effect size is nearly 60 percent higher when using a 7-year average than when using current income. So even for these most basic indicators of well-being it is clear that the longer time averages matter.

Timing of Parental Income

I now turn to examining the effects of parental income received at particular points in the lifecycle of sons. Panel A of Table 8 shows the estimates for a sample of sons born between 1964 and 1975, for whom earnings are measured when they are at least 28. The first column shows the estimates for a relatively simple specification that includes no corrections for life cycle bias and does not control for parent income earned during the child’s adult years. The results suggest that no significant effects are found for the period prior to the birth of the child nor are any effects found for the first 5 years of life. Significantly positive effects are estimated, however for the age ranges of 6 to 11 and 12 to 17. For example, a doubling of parental income during the period when the child is between the ages of 12 and 17 would raise sons’ earnings by about 10 percent. The analogous effect for earnings at ages 6 to 11 is 7 percent.

Column (2) adds in the controls to address possible life cycle bias due to the age at which parental income is measured. As might be expected, this has the effect of raising the coefficients. For example, I now estimate a 13 percent effect on parent income received during the ages of 12 to 17. The estimates in column (3) include a correction for life cycle bias in the sons generation (but not the parents). This adjustment leads to a higher coefficient for the age 6 to 11 bin (0.14) than for the age 12 to 17 bin (0.10). Interestingly, this has no effect on income earned during the ages of 0 to 5. Finally, in column (4) when
both life cycle adjustments are implemented simultaneously, the effects are almost identical for both 6 to 11 (0.134) and 12 to 17 (0.136).

In columns (5) through (8) I now also include parent earnings when the child was between the ages of 23 and 28. In the most basic specification (5), this turns out to have the largest coefficient. However, once I include life cycle controls for the parent (columns 6 and 8) the effect is no longer statistically significant as the standard errors begin to blow up. This is because there is a high degree of collinearity between income earned at these ages and the interactions of parent income with the quadratic in parent age. If we think of column (8) as the most preferred specification, there is some suggestive evidence that the largest effects of parental income appear to be during the ages of 6 to 11. However, I would urge caution in interpreting the point estimates too strongly both because of the lack of precision and also because the parental earnings are drawn from the SER where topcoding is high prior to the 1980s and where it is not totally clear how effective the imputation strategy is.

The bottom panel of Table 8 shows an analogous set of results when broadening the sample to include more recent cohorts whose earnings are used when they are as young as 23. This enlarges the sample considerably but reduces the size of all of the estimates –possibly because of the younger age of sons in the 2003-2007 period. The same basic pattern of results emerges providing suggestive evidence that income in the first 5 years of life appears to matter much less than income earned during the ages of 6 to 11.

Finally, in Table 9 I examine how the timing of parental income affects college enrollment and health status measured in adolescence or early adulthood. The top panel shows the results for college enrollment. Here the strongest and most consistent effect is found for earnings when the child is between the ages of 6 to 11. A doubling of income during this period is associated with a 7 percentage point increase in the likelihood of college enrollment or about a 17 percent size effect evaluated at the sample mean. Perhaps surprisingly, the effect of income during the ages of 12 to 17 appears to be less important
and there is perhaps insufficient precision to detect a statistically significant effect once one controls for parent characteristics. Still, the point estimate for income earned during the ages of 12 to 17 in the preferred specification in column (4) suggests that a doubling of parent income would increase enrollment by 4 percentage points or about 10 percent. In Panel B, it is apparent that given the low incidence of poor health for adolescents (2.2 percent) that I have insufficient data to estimate statistically significant effects. For example, the point estimates suggest that a doubling of income received when children are between the ages of 6 to 11 would lower the incidence of poor health by about 0.5 percentage points which is actually a quite sizable effect but one that cannot be precisely estimated. We do not find evidence that income received between the ages of 0 and 5 appear to matter, however. In future work it might be worth exploring the extent to which the larger than expected coefficients for the -3 to -1 age range may be driven by the year prior to birth which could capture the early part of the in utero period.

Overall, the results of the timing exercise across the three outcomes appear to provide somewhat consistent, though only suggestive, evidence that income during the ages of 6 to 11 are especially important for children’s long-run socioeconomic success.

5. Conclusion

This analysis constructs a unique intergenerational dataset which is able to better estimate the effects of parental income on a rich set of measures of child health and well-being than most previous studies. By assembling long earnings histories for parents I can reduce the measurement error inherent in using only current year income as an explanatory variable. For several critical outcomes I show that 7-year averages of parent income lead to estimates of effect sizes that are substantially higher than are obtained using only a single year of income data. I further show that some (but not all) of these outcomes are also robust to including a rich set of covariates including parental characteristics that are often absent in many cross-sectional datasets.
I further use the long time spans of parental earnings to estimate the relative importance of income received during particular periods of childhood. I find some preliminary suggestive evidence that income received during the child ages of 6 to 11 are particularly important in explaining long-run economic success. An important contribution of the analysis is that I am able to take into account potential lifecycle biases that could otherwise lead to biased results.
Bibliography


Table 1: Summary Statistics of Measures of Child Well-Being

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Table 2: Effects of Parental Income on Children’s Health

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Notes: The “Basic Controls” includes indicators for survey year, child age, race, ethnicity, gender, state and use of Father's vs. Mother's earnings. The set of “Added Controls” adds parent education, parent health status and parent age to the basic controls. Column (9) also adds separate indicators for the father, mother and child having private health insurance.
Table 3: Effects of Parental Income on Health Care Use

<table>
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Notes: The “Basic Controls” includes indicators for survey year, child age, race, ethnicity, gender, state and use of Father’s vs. Mother’s earnings. The set of “Added Controls” adds parent education, parent health status and parent age to the basic controls. Column (9) also adds separate indicators for the father, mother and child having private health insurance.
Table 4: Effects of Parental Income on Physical Characteristics

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<td>Weight for Height z-score</td>
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<td>1707 1707 1707 1707</td>
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<tr>
<td>Weight for Age z-score</td>
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Notes: The “Basic Controls” includes indicators for survey year, child age, race, ethnicity, gender, state and use of Father's vs. Mother's earnings. The set of “Added Controls” adds parent education, parent health status and parent age to the basic controls.
Table S: Effects of Parental Income on Children’s Educational Outcomes

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Notes: The “Basic Controls” includes indicators for survey year, child age, race, ethnicity, gender, state and use of Father’s vs. Mother’s earnings. The set of “Added Controls” adds parent education, parent health status and parent age to the basic controls. Column (9) also adds separate indicators for the father, mother and child having private health insurance.
Table 6: Effects of Parental Income on Home Environment and Family Resources

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Notes: The “Basic Controls” includes indicators for survey year, child age, race, ethnicity, gender, state and use of Father’s vs. Mother’s earnings. The set of “Added Controls” adds parent education, parent health status and parent age to the basic controls.
Table 7: Summary Statistics of Samples Used to Estimate the Timing of Parental Income

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<th>Outcome</th>
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<tr>
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Table 8: Effects of the Timing of Parental Income on Children's Earnings

Dependent Variable: Log Average Earnings Between 2003 and 2007

Panel A: Males Born Between 1964 and 1975

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<th>Effect of Parent Inc. at Child Ages of</th>
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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
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<td>-0.033</td>
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<td>[0.024]</td>
<td>[0.035]</td>
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<td>[0.036]</td>
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<td>-0.021</td>
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<td>0.015</td>
<td>-0.038</td>
<td>-0.031</td>
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<td>[0.050]</td>
<td>[0.048]</td>
<td>[0.060]</td>
<td>[0.037]</td>
<td>[0.050]</td>
<td>[0.047]</td>
<td>[0.060]</td>
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<tr>
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<td>0.08</td>
<td>0.137</td>
<td>0.134</td>
<td>0.064</td>
<td>0.068</td>
<td>0.13</td>
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<td>0.103</td>
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<td>[0.034]**</td>
<td>[0.038]**</td>
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<td>[0.078]</td>
<td>[0.018]**</td>
<td>[0.078]</td>
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</tbody>
</table>

| Observations                          | 4477   | 4477   | 4477   | 4477   | 4477   | 4477   | 4477   | 4477   |
| Child LC Adj.                         | No     | No     | Yes    | Yes    | No     | No     | Yes    | Yes    |
| Parent LC Adj.                        | No     | Yes    | No     | Yes    | No     | No     | Yes    | Yes    |

Panel B: Males Born Between 1964 and 1980

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<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
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<tbody>
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<td>-3 to -1</td>
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<td>0.019</td>
<td>0.027</td>
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<td>[0.017]</td>
<td>[0.022]</td>
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<td>[0.024]</td>
</tr>
<tr>
<td>0 to 5</td>
<td>0.02</td>
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<td>-0.007</td>
<td>-0.022</td>
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<td>[0.038]**</td>
<td>[0.041]**</td>
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<td>0.061</td>
<td>0.052</td>
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</table>

| Observations                          | 9506   | 9506   | 9506   | 9506   | 9506   | 9506   | 9506   | 9506   |
| Child LC Adj.                         | No     | No     | Yes    | Yes    | No     | No     | Yes    | Yes    |
| Parent LC Adj.                        | No     | Yes    | No     | Yes    | No     | Yes    | No     | Yes    |
Table 9: Effects of the Timing of Parental Income on College Enrollment and Health

Panel A: Dependent Variable is College Enrollment\*100

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<th>(3)</th>
<th>(4)</th>
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<td></td>
</tr>
<tr>
<td>-3 to -1</td>
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</tr>
<tr>
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<td>[0.021]</td>
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<td>[0.043]</td>
</tr>
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<td>0.075</td>
<td>0.071</td>
</tr>
<tr>
<td></td>
<td>[0.038]*</td>
<td>[0.036]*</td>
<td>[0.038]**</td>
<td>[0.036]*</td>
</tr>
<tr>
<td>12 to 17</td>
<td>0.083</td>
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<td>0.073</td>
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Observations: 1174
Baseline Controls: Y Y Y Y
Added Controls: N Y N Y

Panel B: Dependent Variable is Poor Health Status\*100

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Observations: 4627
Baseline Controls: Y Y Y Y
Added Controls: N Y N Y