CENTER FOR LABOR ECONOMICS
UNIVERSITY OF CALIFORNIA, BERKELEY
WORKING PAPER NO. 34

Earnings Mobility in the US: A New Look at Intergenerational Inequality

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March 2001

Abstract

This study uses a new data set which merges the Social Security earnings histories of parents and children in the 1984 Survey of Income and Program Participation to measure the intergenerational elasticity in earnings in the United States. Earlier studies that found an intergenerational elasticity of 0.4 have typically used only five year averages of fathers' earnings to measure fathers' permanent earnings. However, dynamic earnings models that allow for serial correlation in transitory shocks to earnings imply that using such a short time span may lead to estimates that are biased down by as much as fifty percent. Indeed, by using many more years of fathers' earnings than earlier studies, the intergenerational elasticity between fathers and sons is estimated to be 0.6 or higher implying significantly less mobility in the U.S. than previous research indicated. The elasticity in earnings between fathers and daughters is of a similar magnitude. The evidence also suggests that family income might have an even larger effect than parents' earnings on children's future labor market success. Intergenerational mobility is lower for families with low net worth, offering some empirical support for theoretical models that predict differences due to borrowing constraints. Some evidence of lower economic mobility among blacks is found but the results are not conclusive.

I am grateful to David Card, David Levine, Ken Chay, John DiNardo, Michael Reich, Nada Eissa, Mike Clune and seminar participants at Illinois, The Chicago Fed, Cornell and UMass for their helpful advice and comments. I greatly appreciated the help of Andrew Hildreth, Julia Lane and Susan Grad in helping me gain access to the data. The help of staff at the Social Security Administration, and especially Minh Hyunh, is also gratefully acknowledged. I would also like to thank the Center for Labor Economics at UC Berkeley for financial support.

I. Introduction

How economically mobile is America? Is the United States best described as an equal opportunity society where individuals succeed or fail based on their talents and ability, or is future success largely determined by circumstances at birth? Is there an economic underclass that is essentially trapped in poverty for generations? The answers to these questions undoubtedly have a strong bearing on whether America should be viewed as an "equal" society and whether policies are needed to address long-term inequities. Despite the obvious importance of economic mobility as a basis for public policy, economists have devoted more attention to measuring and analyzing cross-sectional inequality, and have only recently begun to make strides in understanding the dynamics of inequality among families over generations.

In recent years a growing body of research has used the regression coefficient relating a son's income to his father's as a summary measure of the degree of intergenerational mobility in society.[1] A high intergenerational elasticity is indicative of a rigid society, since a person's place in the income distribution is largely determined by the circumstances they are born into. In contrast, a low correlation in economic status across generations is indicative of a mobile society in which an individual's lifetime income is largely independent of his or her family background.

Even with an empirical estimate of the intergenerational elasticity in income in hand, it is not entirely obvious how to assess its importance in characterizing mobility. One way to illustrate the significance of this measure is to imagine what it implies about the evolution of the black-white wage gap in the United States. An intergenerational regression coefficient of 0.2, for instance, implies that only 20 percent of any income gap would remain after a generation (25 years). Using this logic, the black-white weekly wage differential that stood at about 25 percent for men of age 25 to 40 in 1980[2] would be reduced to just 5 percent by 2005 for similarly aged

[1] Recent studies include Solon (1992), Zimmerman (1992), Altonji and Dunn (1991), Peters (1992), Shea (1997), Mulligan (1997) and Corak and Heisz (1999). A full survey can be found in Solon (1999).
[2] See Smith and Welch (1989).

men if all other shocks were ignored.[3] If instead, the intergenerational coefficient was 0.6, then the black-white wage gap would still be a sizable 15 percent in 2005.

Another way to highlight the importance of the intergenerational elasticity is to think about what it implies about the long-term effects of public policy. Assuming an intergenerational elasticity of 0.6 implies that the full effect of a policy intervention that improves the income of a group in one generation by 10 percent will be to raise their income by 25 percent over all future generations. On the other hand, if the intergenerational elasticity was just 0.2, the full effect would only be 12.5 percent. Returning to the black-white wage gap, Krueger (1995) has argued that the effects of racial segregation of schools on blacks born during the 1920s was to reduce their income by 21 percent. Assuming an intergenerational elasticity of 0.6 this suggests that the offspring of this cohort, those born in the 1940s, have earnings 12 percent lower due simply to the segregation of their *parents'* schools. Policies such as affirmative action are arguably more justified as a remedy for past injustice if the intergenerational elasticity in earnings is relatively high.

The results from several studies from the 1990s (e.g. Solon 1992, Zimmerman 1992) have pointed to an intergenerational elasticity in the U.S. of about 0.4, a figure twice as high as what researchers had previously thought and suggestive of a far less mobile society than was earlier believed.[4] The key explanation for the relatively higher estimate of the intergenerational elasticity in the most recent studies is that these studies regress children's earnings or incomes on their parents' *permanent* income, rather than their incomes in a single year. All of the recent studies on the U.S., however, come from just two surveys, the Panel Study on Income Dynamics (PSID) and the National Longitudinal Study (NLS), both of which have relatively small sample

[3] A number of factors such as skill-biased technical change or declining unionism could affect each group differently and temporarily widen the gap further. This example also assumes a common elasticity for both groups and no other group-specific effects.
[4] Solon (1999) presents a summary of findings of other studies with similar results.

sizes, and suffer from considerable attrition when constructing intergenerational samples.[5] In addition, because of data limitations, researchers using these data must estimate fathers permanent earnings using only a few years of earnings. This is a problem because the existing literature on earnings dynamics shows that transitory shocks in earnings are highly serially correlated. Thus, even five years of income data is not enough to form an error-free estimate of permanent income. Indeed using estimates of earnings models in the literature, estimates of the intergenerational coefficient based on only five-year averages of fathers' earnings may be biased down by as much as fifty percent. This implies that the true intergenerational elasticity may be closer to 0.6. Coming from a different perspective, Couch and Lillard (1998) have argued that previous results have been based on restrictive sample selection rules that may be unwarranted and may have biased the results *upward*.

This study uses a new data source, the 1984 Survey of Income and Program Participation (SIPP) matched to Social Security Administration's Summary Earnings Records (SER) to produce new estimates of the transmission of income across generations. Although this data set has some limitations it provides the long-term earnings histories for both parents and children without any problem of sample attrition. In addition, the data provides significantly larger samples and richer measures of income and wealth for the parents.

The key result of this paper is that the intergenerational elasticity in earnings between fathers and sons is estimated to be 0.6 or higher, a figure substantially above previous estimates and indicative of a relatively immobile society. The higher estimate is due to the availability of many more years of earnings data on fathers which eliminates the substantial downward bias stemming from transitory shocks to earnings that exists in previous studies. Indeed, the results when fathers' permanent earnings are based on shorter time horizons closely track the findings from previous research.

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[5] For example, in the samples that use five year averages of fathers' earnings, Solon (1992) using the PSID has only 290 father-son pairs while Zimmerman (1992) using the NLS has only 192.

This study also generates a number of new findings concerning the persistence of earnings across generations. The intergenerational elasticity between fathers and daughters is similar to that found between fathers and sons. The fathers-daughter relationship has received scant attention in most of the existing literature on intergenerational mobility.[6] Using family income rather than parent earnings leads to higher estimates of the intergenerational elasticity when both are measured over a short time horizon. This provides further evidence that previous estimates of intergenerational mobility that were based on short-term averages of fathers earnings may have understated the degree of intergenerational persistence in economic status. This study also presents strong evidence in favor of theoretical models that emphasize borrowing constraints as a source of intergenerational inequality (Becker and Tomes 1986, Mulligan 1997). Using detailed information on wealth from the SIPP, the intergenerational elasticity is estimated to be significantly higher for families with low net worth and is negligible for those in the top quartile of net worth. These results suggest that policies that target borrowing constrained families may play an important role in reducing inequality over the long-term. The estimates in this study also show a higher intergenerational elasticity among black families than white families, particularly when both parents' earnings are included. The findings, however, are not precise enough to justify a strong conclusion. A methodological contribution of this study is that careful attention is paid to sample selection rules to address Couch and Lillard's (1998) criticism that past studies may have inappropriately dropped observations if sons or fathers report zero earnings. In this study various exclusion rules are used to analyze the effects of including years of zero earnings for both fathers and their children, and the results are not sensitive to these variations.

The paper proceeds as follows: Section II describes the measurement issues involved in studies of intergenerational income mobility. In particular, this section demonstrates how the measures of permanent income in the existing literature that use averages over just five years can

[6] This is probably due, in part, to the fact that marriage (coupled with higher average earnings for men) weakens the reliability of daughters' earnings or income as a measure of economic standing.

substantially underestimate the intergenerational elasticity. In Section III the construction of the matched dataset is explained and a number of strategies are outlined to deal with some shortcomings in the data. Section IV presents the methodology used in the study and describes the main results. In addition, a variety of alternative approaches are presented that deal with possible criticisms of the research. Section V presents extensions of the research. This includes an analysis of the effects of family income on children's earnings, how borrowing constraints might influence the intergenerational transmission of inequality and results concerning differences in mobility by race. Section VI concludes.

II. Measurement Issues

There is a long tradition dating back to Sir Francis Galton in 1877 that has examined the rate of regression to the mean of different characteristics across generations. The basic methodology has been to use an autoregressive model to calculate the correlations of these characteristics between parents and children. The first major theoretical model to analyze the transmission of income across generations was by Becker and Tomes (1979). They proposed a utility maximizing framework in which parents choose between current consumption and investment in their children's' human capital. Under a set of simplifying assumptions they derived a straightforward result that son's income is a linear function of father's income. A major contribution of their model was their emphasis on human capital as a primary channel by which income inequality is transmitted. On the other hand, as Goldberger (1989) pointed out, their theoretical model does not offer any more empirical content than the earlier "mechanical" approaches to studying income transmission, since it leads to the same statistical model.

In any case, empirical studies have typically used the following regression model to measure the intergenerational elasticity between fathers and sons:

$$(1) \qquad y_{1i} = \alpha + \rho y_{0i} + \beta_1 Age_{0i} + \beta_2 Age^2_{0i} + \beta_3 Age_{1i} + \beta_4 Age^2_{1i} + \varepsilon_i$$

Here y_{1i} represents a measure such as the log of annual income of the son in family *i*, while y_{0i} is the corresponding measure for the father. The only additional right hand side variables that are generally included are age and age squared, in order to account for the effects of the lifetime profile of earnings for both the father and son. Other covariates have generally not been included in these studies since the goal of the research has been to obtain a summary measure of all the factors related to income that are transmitted over generations.[7] Ordinary Least Squares (OLS) is

[7] Therefore, ρ should not be given a *causal* interpretation.

generally used to estimate the equation and the coefficient of interest, of course, is ρ, which measures the intergenerational elasticity.[8]

As might be expected, the first datasets that contained detailed intergenerational information on income used relatively obscure samples.[9] These studies found the correlation to be less than 0.2. On the basis of these results and other international studies, Gary Becker in his 1988 address to the American Economics Association, asserted that “In all these countries, low earnings as well as high earnings are not strongly transmitted from fathers to sons…”.[10]

As carefully documented by Solon (1989, 1992), there are three basic problems with these early studies, all of which have the effect of understating the true parameter estimate.[11] These are illustrated in the following statistical framework:

$$y_{0is} = y_{0i} + w_{0is} + v_{0is} \tag{2}$$

$$y_{1it} = y_{1i} + w_{1it} + v_{1it} \tag{3}$$

$$y_{1i} = \rho y_{0i} + \varepsilon \tag{4}$$

In this setup, y_{0is} represents the father's log earnings in year s, while y_{1it} is the earnings of his son in year t.[12] Equation 2 breaks down the father's earnings in a particular year into three components: a permanent component that reflects the true long-term earnings capacity, y_{0i}; a component that captures any transitory shocks that might affect that particular year's earnings, w_{0is}; and finally, a term that captures any errors due simply to mismeasurement such as an

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[8] If the variance in log income (or earnings) is the same for both generations then ρ is also the intergenerational correlation. The intergenerational correlation has been emphasized in the sociology literature on intergenerational mobility. The two measures are roughly comparable even if the variance in income differs substantially across generations as shown by Solon (1992).

[9] For example, Behrman and Taubman (1985) was based on a sample of white male twins who served in the armed forces. Sewell and Hauser (1975) used a sample of high school seniors in Wisconsin who were no longer in school seven years later.

[10] See Becker (1988).

[11] Bowles (1972) first pointed out the problems with using single year measures of income as a proxy for permanent income.

[12] For simplicity, earnings are assumed to be measured as deviations from the mean

inaccurate report of earnings, v_{0is}.[13] Equation 3 performs the analogous decomposition for the son.

Equation 4 simply restates equation 1 except that it omits age effects for simplicity, and demonstrates that the relationship of interest is between the father's *permanent* earnings and the son's *permanent* earnings. In actuality, researchers with access to only one year's measure of the father and son's earnings will not be able to estimate (3) but instead, will regress the father's *measured* earnings from a single year on the son's *measured* earnings also from a single year. If we assume that the transitory shocks and the measurement error are independent of the true permanent earnings, then the estimate of ρ, $\hat{\rho}$, will be biased downwards by an attenuation factor. It is easily shown that:

$$(5) \qquad \text{plim } \hat{\rho} = \rho\lambda,$$

$$\text{where } \lambda = \left(\frac{\sigma_{y0}{}^{2}}{\sigma_{y0}^{2} + \sigma_{w0}^{2} + \sigma_{v0}^{2}} \right),$$

is an "attenuation" coefficient" arising from the mismeasurement of permanent income. The sources of downward bias can now be clearly seen. First, by using a single year of data on father's earnings, noise is generated through σ^2_{w0} term, the variance of transitory fluctuations. This can, in fact, reduce the parameter estimate significantly.[14] Second, there is bias due to measurement error in the father's earnings, which is captured by σ^2_{v0}, the variance of the measurement error term. Finally, many of the studies use relatively homogeneous samples of fathers, which has the effect of reducing the "signal" in the data because σ^2_{y0}, is relatively low. Unless the use of a homogeneous sample also happens to reduces the noise, the downward bias will be exacerbated.

[13] For the moment, both the transitory component and the measurement error component are viewed as white noise.

Several studies in the early 1990s used either the Panel Study of Income Dynamics (PSID) or the National Longitudinal Surveys (NLS) —longitudinal datasets that were nationally representative and allowed for multiple year measurements—to address these problems.[15] By averaging the father's income over several years they were able to reduce the bias from transitory income shocks and measurement error.[16] The results in nearly all cases were significantly higher than the 0.2 coefficient from the early literature and instead pointed to an intergenerational correlation of around 0.4.

These studies, however, overlooked one important factor. Despite averaging fathers' earnings over a few years, there still might be considerable attenuation bias due to persistence in transitory income. It is well established from many error-component models of long-term earnings profiles that the transitory component of income is highly serially correlated.[17] The theoretical implications of this on past econometric results that used multiyear averages can best be seen by extending the statistical framework to incorporate serial correlation in the transitory component.[18] Specifically if we model w_{0is}, the transitory component of earnings as a stationary autoregressive process,

$$w_{0is} = \delta w_{0is-1} + \xi_{is} \qquad (6)$$

where δ represents the autoregressive parameter, then the attenuation coefficient when averaging over T years, λ_T, can be expressed as follows:

$$\lambda_T = \frac{\sigma_{y0}^2}{\sigma_{y0}^2 + \frac{1}{T}\alpha\sigma_w^2 + \frac{1}{T}\sigma_v^2} \qquad (7)$$

[14] Solon (1992), in reviewing previous estimates, concludes that this factor can attenuate the coefficient by 30 percent or more.
[15] Solon (1999) identifies 15 different studies using these surveys. Probably the most widely cited are Altonji and Dunn (1991), Solon (1992) and Zimmerman (1992).
[16] Additional techniques such as instrumental variables were also used, though these estimates often introduced a positive bias and could only provide an upper bound estimate.
[17] Some examples are Lillard and Willis (1978), MacCurdy (1982), Card (1994) and Hyslop (2000).
[18] While both Solon (1992) and Zimmerman (1992) present formulas on the bias when incorporating serial correlation in the transitory component, they do not pursue the implications of this on their results.

$$\text{where, } \alpha = 1 + 2\delta \left\{ \frac{T - \left[\frac{(1-\delta^T)}{(1-\delta)} \right]}{T(1-\delta)} \right\}$$

In the absence of serial correlation in transitory fluctuations (i.e. $\delta = 0$), the coefficient $\alpha = 1$ in equation (7), and it is clear that averaging lowers the noise relative to the signal. With serial correlation, however, the α term creates an offsetting factor. Indeed, the larger δ is, the larger the overall attenuation bias will be. In order to get a sense of the possible implications, I conducted some simulations using plausible values for δ, and for the fraction of total variance in one year's earnings that is due to transitory factors, permanent factors and measurement error. Using one set of estimates for these parameters from a recent study by Hyslop (2000), the attenuation coefficient when averaging earnings over five years is .660.[19] A set of simulation results is shown in Appendix Table A1 where the value of δ is either 0.5 or 0.8 under three different assumptions about the breakdown in the variance of single-year earnings. These results suggests that estimates of the intergenerational elasticity of 0.4 using five year averages may still be biased down by fifty percent.

This bias may be further compounded by greater homogeneity due to sample attrition. Solon (1992) for example, uses less than 60 percent of the original cohort of sons and acknowledges evidence of greater homogeneity in the resulting sample. The father's age in most of these studies is also quite high since the datasets do not allow researchers to link earnings *backwards* in time from the starting point of the longitudinal survey. Although the age of the father is controlled for in most studies, there may be advantages to using younger fathers.[20]

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[19] This assumes that $\delta = 0.8$, that share of the variance in earnings accounted by permanent factors is 0.5, by transitory factors is 0.3, and by measurement error is 0.2. These are also precisely the same estimates found by Card (1994).

[20] Gordon (1984) and Baker and Solon (1999) have found that the transitory variance follows a "U-shape" over the life-cycle suggesting that measures taken say, around age 40 may have less attenuation bias than those taken at age 50.

III. Data Issues

Overview of SIPP and SER

This analysis uses the 1984 Survey of Income and Program Participation (SIPP) matched to Social Security Administration's (SSA) summary earnings records (SER). The SIPP is designed to provide detailed information on income and participation in government assistance programs. The 1984 SIPP was a nationally representative longitudinal survey that started with over 50,000 individuals in nearly 20,000 households. In the 1984 SIPP there was no oversample of low income households as was done in later SIPP years. The survey was designed to follow individuals and families over a two and a half year period. The survey began in October 1983 and continued until July 1986 covering the period from June 1983 to June 1986. Interviews took place every four months and the "core" questionnaire covers three major topics: labor force and government program participation; earnings and employment; and amounts of income received. In addition, in each wave of interviews after the first interview, there was a "topical module" that asked more detailed questions on a particular topic of interest.[21]

Respondents were also asked to provide the social security number of their family members with a guarantee of confidentiality. The Social Security Administration subsequently tried to match those individuals who entered the SIPP in one of the first three waves to their summary earning records (SER). The resulting file contains the individual's SIPP identifiers along with annual taxable earnings from 1951 to 1998. An additional set of variables is also available in the file including date of birth, sex, race, self-employment status, agricultural status, military status and a number of variables related to social security coverage. It should also be noted that the SER file can only be used to gather information on *earnings* and not other forms of

[21] Topical modules that could be relevant for this study are education, work history, assets and liabilities, child care, real estate property, migration history and household relationships.

income (e.g., asset income and transfers) that are available in the SIPP and may have been used in previous studies on income mobility.

Matching Issues

This matched file potentially allows for intergenerational analysis of families where children were living with their parents between June 1983 and June 1984 and where the children had social security numbers that were provided to SIPP interviewers. That level of information would allow for a link between parent information in the 1984 SIPP to children's earnings as adults up to 1998. In order to go a step further and also access the full social security earnings history for the parents, it is necessary that the parents also provided social security numbers. Therefore an analysis of both children's and parents' full history of earnings requires that *both* be successfully matched to their SER earnings.

The universe selected for analysis in this study includes children born between 1963 and 1968 who were coresident with either or both parents or living away from home while at college during the first wave of the 1984 SIPP (June-September 1983). The implied age range was limited to those 15 or older in 1983 because of the poor match rate for younger children. This is apparent in Figure 1 which shows the match rate by age.[22] This lower bound on age also ensures that the sons and daughters are at least 27 years old when their earnings are observed in the years 1995 through 1998. The sample was also restricted to those who were age 20 or under in 1983 to ensure that the sample did not over-represent those who stayed at home until a late age.[23] The possible selection biases that could result from these rules are addressed in Section IV.

[22] In the early 1980s social security numbers were not nearly as universal among children as they are today. The key factors that determined whether someone would have a social security number were if he or she worked, had a bank account, owned stocks or received any form of government assistance. An econometric analysis of whether a 15 to 20 year old in the SIPP was matched to the SER showed all of these factors to be significant. This suggests that the sample used here over-represents both poor and rich households. Weighting the sample by the inverse of the probability of being matched has a minor effect on the results as shown in section IV.

[23] Earlier studies such as Solon (1992) and Zimmerman (1992) have used 18 years of age as an upper age cutoff for kids living at home. In the SIPP, however, sons and daughters living away while attending

There are also some difficulties in creating the parent-child matches using the SIPP. One problem is that the topical module of the 1984 SIPP which explicitly describes family relationships, does not take place until the eighth wave (January to March 1986) at which point the sample size significantly declines.[24] Therefore, in order to use the most representative sample, that is free of any attrition bias, the sons and daughters must be matched to their father in the first wave. The task is also complicated by the fact that children are usually directly linked only to their mother (when one exists in the household). The child can then be linked through the mother, to the mother's spouse. This, of course, may result in matching sons to their stepfathers rather than their biological fathers. The existing literature has largely not been concerned about whether the matches are to the biological father, arguing that what is being investigated is the broad effect of family background and not just genetic influences.

There are a total of 4072 child-parent pairs in which both the child and at least one parent are successfully matched to the SER file, representing an overall match rate of 87 percent.[25] In 3158 cases, sons or daughter and their fathers are both successfully matched to their earnings records. Of these, 1663 represent father-son pairs while 1495 cases are father-daughter pairs. An alternative approach is to use SIPP income or earnings data from 1984 and 1985 for the parents instead of matching them to the SER file. A major drawback, however, is that because of attrition, budget cutbacks and nonresponse to earnings questions, there is a much smaller sample with complete SIPP income data —only 912 father-son pairs and 809 father-daughter pairs.

SER Data Problems

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college were considered living at home and are included. The percent of 19 and 20 year olds still living at home or at college in the 1984 SIPP is over 70 percent.

[24] During the eighth wave only 3 of the 4 rotation groups were interviewed. In addition, approximately 3000 households were dropped from the SIPP in early 1985.

[25] The match rate within the pairs are as follows: fathers alone are matched at a 93.5 percent rate, mothers alone are matched at a 93.2 percent rate, sons alone are matched at a 88.8 percent rate and daughters alone are matched at a 88.2 percent rate.

In this study, the use of SER data introduces three key concerns. The first is that although instances of zero annual earnings may reflect non-working, they could also be due to employment in a job that is not covered by social security.[26] Although about 90 percent of jobs in the U.S. are now covered, in the early 1980s the figure was somewhat lower. However, even 10 percent of the sample incorrectly classified as zeroes presents a significant problem if regression results are sensitive to sample selection rules around zero earnings. A second problem is that because earnings are only taxed for Social Security up to the taxable maximum for the year, the SER file "topcodes" earnings at this cutoff. This is further compounded by the fact that there have been large changes in the real value of the taxable maximum over the last forty years resulting in large changes in the fraction of the sample who are topcoded, as shown in Figure 2.

Finally, even among those with positive earnings, a large number of individuals have both covered and non-covered earnings.[27] This is illustrated in Figure 3 which uses the full sample of adults in the 1984 SIPP-SER and plots SER earnings on the x-axis and SIPP earnings on the y-axis. If there was random reporting error, the graph would show a random scattering of points around the forty-five degree line. Instead, there is a large fraction of people who report dramatically higher earnings than are actually taxed for social security. Each of these three issues may present econometric problems not only because they affect the dependent variable, children's adult earnings, but because they also affect the *independent variable*, parents' earnings. Because of the differences in available information for the sons and daughters compared to their parents, each of these issues must be addressed separately for the two groups.

Data Solutions: Children's Earnings

Distinguishing instances of zero earnings from non-covered earnings for the sons and daughters is perhaps the most difficult issue of all. The problem is that there are no available data

[26]Many federal, state and local government workers are not covered by social security. In addition, workers in the underground economy or workers in certain occupations are paid outside of the tax system.

on hours worked for the children in the sample as of the late 1990s. Approximately 12 percent of the sons and 21 percent of daughters had zero covered earnings in 1996.

What turns out to be useful, however is the use of a different dataset, the 1996 SIPP-SER, which matches a completely different set of individuals in the 1996 SIPP to their social security earnings. It also has precisely the same variables as the 1984 SIPP-SER file. Looking at the same cohort using the 1996 SIPP-SER dataset permits a further breakdown of those with zero earnings, into "non-covered" and "non-workers". In this sample, about 57 percent of the men with zero SER earnings were employed for the full year and are classified as non-covered while 32 percent worked for only zero to two months of the year and are called non-workers.[28] Those working in the non-covered sector are primarily government workers or self-employed. The comparable rates for the daughters were 21 percent and 71 percent, respectively. These numbers suggest two conclusions. First, most of those with zero SER earnings are either non-workers or full-time workers in the uncovered sector. Only about 10 percent of zero earners fall in the gray area of having zero earnings and working part-year. Second, the problem of non-covered workers is particularly important for men.

One strategy to deal with the problem of non-covered workers is to estimate probit models on the 1996 SIPP-SER dataset for members of the 1963-1968 cohort to predict the probability that individuals with zero earnings will have actually worked a full year as a function of all available information contained in the 1996 SER file as well as any basic demographic information that can be determined by adolescence.[29] This function is then used on the sample of sons and daughters from the 1984 SIPP-SER to obtain predicted probabilities for each individual

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[27] This may be due to tax avoidance or as a result of having more than one job.

[28] The universe is restricted to those who remained in the 1996 SIPP through the end of 1996. Those who are considered employed for the whole year may have worked for 10 to 12 months. Because of the rotation group structure of the SIPP some individuals may have only joined the survey starting in February or March of 1996.

[29] For the most part, survey information from the 1996 SIPP is deliberately omitted from this analysis since such information is obviously unavailable for the sample of sons and daughters from the 1984 SIPP. The exceptions are some basic demographic information and whether individuals ever attended a college. For

that they were not covered. A second probit is also run to predict the likelihood that someone with zero earnings worked no more than two months. The estimated function is then applied to the sons and daughters to obtain a second set of predicted probabilities. Each of these two probits are run separately for men and women, using data for both 1996 and 1997.[30] The estimates from the probit models are then combined in order to classify each son or daughter as either a non-worker or as non-covered for each year.[31] Those identified as non-covered can then either be dropped from the analysis or their earnings may be imputed using the mean level of log earnings for the group from the 1996 SIPP. Similarly, those identified as non-employed are then assigned the mean level of log earnings for those who worked between zero to two months.

The results of the two probits for men in 1996 are shown in Appendix Table A2.[32] Among the key variables that are significant are: having attended college; the number of years of zero earnings during the late 1990s; total lifetime covered earnings; annual earnings in specific years; a flag indicating an active earnings discrepancy;[33] being 29 years old; never having positive covered earnings; being Mexican and being self-employed interacted with 1995 earnings. The fit of these models is quite high as measured by the Pseudo R^2. The within sample forecasting record is also very impressive. For men in 1996, over 90 percent of the true classifications of non-covered and non-workers were correctly predicted. In terms of the *entire sample* of the cohort of men in the 1996 SIPP-SER, this implies that less than 1 percent of the sample was incorrectly classified. The error in forecasting women's status is higher and implies that about 3 percent of the sample is incorrectly classified. While it is impossible to know how well this model predicts the correct classification of earnings for the sons and daughters in the

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the sons and daughters, data on whether they ever attended a college over the period of the 1984 SIPP (June 1983- June 1986) can then be exploited.

[30] These are the only two full years from the 1996 SIPP that are currently available for analysis

[31] Specifically, individuals are classified based on the category in which they have a higher predicted probability. This is equivalent to assigning them based on the sign of the difference in predicted probabilities.

[32] Results for women and for 1997 are available on request.

[33] These are cases where an individual has contested what they believe to be inaccurate reports of their earnings with SSA and where the dispute has not yet been resolved.

1984 SIPP, the low forecast errors in the 1996 SIPP sample suggest that we can have a high level of confidence in the results.

The second problem, topcoding at the taxable maximum, is much easier to handle for the children than for the parents. For example, only 6 percent of the sons and 2 percent of the daughters were topcoded in 1996. There are two approaches that are used to address this problem. The first is to estimate tobit models rather than Ordinary Least Squares (OLS) as will be discussed in the next section. The second approach is to use the 1996 SIPP-SER to impute earnings for those topcoded in 1995 through 1998.[34] Results using both approaches will be shown in section IV.

The implications of the fact that some children will have both covered and non-covered earnings are not entirely clear. Essentially, it means that for a fraction of the children, observed earnings from the SER will under-represent actual earnings. To the extent that this measurement error in the dependent variable is random it will not bias the intergenerational elasticity coefficient although it will enlarge the standard errors. On the other hand, if this error is correlated with *fathers' earnings*, then the results would be biased. One way that this could arise is if fathers who are non-covered, typically have lower earnings and also have children who are likely to have non-covered earnings and hence, lower observed earnings. In this case the bias would be upwards.[35] While there is evidence that there is a positive intergenerational correlation in self-employment status (Dunn & Holtz-Eakin (1996)) it is not clear that this translates into a sizable correlation in overall non-covered status. In addition, there is evidence that the distribution of earnings among the self-employed is similar to the overall population. The problem might be more significant if the same form of measurement error exists for *fathers'*

[34] Specifically, the mean value of SIPP earnings of those in the cohort with SIPP earnings above the social security taxable maximum was calculated for 1996 and 1997. There was no significant difference between the imputed values for men and women. The 1995 imputation value simply used the 1996 value converted to 1995 dollars using the CPI. Similarly, the 1998 value used the 1997 inflation adjusted value.

earnings. In this case the measurement error in children's earnings may be correlated with *measured* fathers' earnings. Measurement error in fathers' earnings will be addressed shortly. In any event, there is no simple way to solve the measurement error problem for the dependent variable given the lack of direct survey data on the children in their adult years.

Data Solutions: Parent Earnings

Some of the problems with using social security earnings data are considerably easier to deal with for the parents because of the rich information available in the 1984 SIPP. In addition, it also possible to simply use the parents' earnings data directly from the SIPP for the years 1984 and 1985 for the analysis rather than using the SER data. As noted earlier, this reduces sample sizes considerably. In this study, results based on using the parents' SIPP earnings is used to complement the analysis based on the father's SER earnings. This serves as a useful check for many of the results.

The monthly survey questions are particularly useful with respect to the first problem, that zero earnings might reflect non-covered employment. For the years 1984 and 1985 there is very detailed monthly survey information from the 1984 SIPP on labor force status and pay. For these years, it is quite easy to identify whether individuals who had zero SER earnings also reported no paid weeks of employment for the full year.[36] For earlier years, a topical module from the second wave on labor force history is used to classify fathers with zero earnings in each year as either non-covered or non-workers. Specifically, the questionnaire asks individuals a series of questions about recent employment experiences such as tenure and time between jobs,

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[35] If $y_{1i} = y_{1i}^* + \tau$, where y_{1i}^* is the actual child's earnings and τ is the measurement error, then, plim of ρ "hat" = ρ + (Cov(τ,y_{0i})/Var(y_{0i}). If errors are larger in magnitude (more negative) at low values of fathers' earnings, y_{0i}, then Cov(τ,y_{0i}) will be positive.

[36] For individuals who are not in the SIPP for the full year of 1984 or 1985, the criteria are modified based on whatever survey information is available in order to classify zero earners.

that enables one to construct instances of year long unemployment spells, reasonably well.[37] Because of the likelihood of poor recall into the distant past, the process is only used to classify non-workers going back to 1979.[38] Finally, those not classified as non-workers are classified as non-covered and can either be dropped from the analysis or recoded using their SIPP earnings.

As noted earlier, the issue of topcoding is far more severe for the fathers since the taxable maximum affected a higher share of the sample in earlier decades. Specifically, for the sample of fathers, the topcode rate was above 50 percent during the early 1970s falling to about 20 percent by the mid-1980s. The approach taken to correct for this is to divide the fathers into 6 groups by race and education level. For each year from 1981 to 1985 the *full sample* of the 1984 SIPP-SER dataset is used to create imputed values for each group. Specifically, the mean value of SIPP earnings in 1984 for each topcoded group is calculated and used for imputation.[39]

For the years 1970 to 1980, the imputation values are derived from each year's March Current Population Survey (CPS) instead of the 1984 SIPP. Given the well-documented change in the earnings distribution from the 1970s to the 1980s, it is clearly inappropriate to use the 1984 earnings distribution to calculate the imputed values during the 1970s. For these years, the actual taxable maximum published by the Social Security Administration is used as a cutoff point for

[37] The actual process of translating the answers from the questionnaire into the classification scheme is very tedious. Those interested in the algorithm should contact the author for the program. Unfortunately, a set of questions in the topical module that actually asks about the start and end dates of all lifetime unemployment spells appears to be error-ridden and was not used.

[38] Before 1979, the classification procedure implies much lower rates of non-working than after 1979 suggesting that recall bias becomes a problem.

[39] Only topcoded individuals for whom SIPP earnings in 1984 is greater than or equal to 1984 SER earnings and who are in the SIPP for all 12 months of 1984 are used in the calculations. For the years 1981 to 1983, and 1985, calculating the imputations involves an added step. The percentile to use as a cutoff for calculating the imputed values for each year is determined by using the percent topcoded in that year based on the SER data for all the sample members in the 1984 SIPP-SER dataset (not just the fathers). For example, in 1980, 8.8 percent of those with positive earnings in the full sample of the 1984 SIPP-SER matched dataset, had topcoded earnings. The strategy then, was to use the top 8.8 percent of the SIPP earnings distribution in 1984 to calculate the imputed values for each of the 6 groups for 1980. Of course, the 1984 dollar values were then converted to 1980 dollars using the CPI.

the CPS analysis. The mean value of earnings above the taxable maximum for each group is used to impute earnings for those who were topcoded during these years.[40]

The problem of measurement error due to fathers with both covered and non-covered earnings is handled through the use of the "class of worker" variable in the 1984 SIPP. This variable identifies those who worked for the government or who were self-employed at any point that they were in the SIPP. In addition to removing downward bias due to measurement error it has the additional advantage of reducing the possible bias arising from the mismeasurement of children’s earnings, as was discussed earlier. One drawback of this approach is that it reduces the sample size by roughly a third. In addition, because there is no information on class of worker for years not covered by the 1984 SIPP, the classification must be imposed when averaging fathers' earnings over many years.

[40] An attempt was also made to use information in the SER data file on the quarter of the year in which full coverage was achieved. For years before 1978 this variable could be used to estimate full year earnings for those topcoded. The results, however, were no different using this strategy.

IV. Methodology and Main Results

SIPP Results

This study begins by estimating the intergenerational elasticity in earnings between fathers and their children using the SIPP earnings data for fathers. Although the SIPP is limited to just two years of earnings and necessitates a smaller sample, it serves as a useful benchmark for the main analysis that uses the SER data. The econometric approach follows the recent literature and estimates the following equation:

$$y_{1i} = \alpha + \rho y_{0i} + \beta_1 Age_{0i} + \beta_2 Age^2_{0i} + \beta_3 Age_{1i} + \beta_4 Age^2_{1i} + \varepsilon \tag{8}$$

Specifically, y_{0i}, the father's earnings, will be the log of the average annual earnings of fathers over 1984 and 1985. This includes earnings from up to two jobs and two businesses. In all aspects of this analysis, earnings are converted to 1998 dollars using the CPI. Only those fathers with earnings that are not imputed by the Census Bureau due to nonresponse are included. The father's age, Age_{0i}, and age squared, Age^2_{0i}, are measured in 1984. The son's or daughter's earnings, y_{1i}, is the log of average annual earnings over the years 1995 to 1998. These years are chosen so the kids are no younger than 27 in any of the years that their earnings are measured, thereby giving a more reasonable picture of lifetime earnings.[41] Each year's earnings for the sons and daughters are first adjusted using the procedure described in section III to identify and then impute the earnings of non-covered and non-workers. The children's age measures, Age_{1i} and Age^2_{1i}, use their age in 1998. Table 1 presents the key sample statistics. Unlike some previous

[41] Solon (1999) has argued that studies with young samples have found lower correlations because of mean reversion in the transitory income component, i.e. those with higher permanent income have lower transitory incomes at a young age, thereby inducing an attenuation bias. The average age of the kids in this study is 31 which is similar to the average age of 29.6 reported by Solon (1992) and 33.8 reported by Zimmerman (1992). Averages are taken over several years for the children to address the criticism by Couch and Lillard (1998) that Solon and Zimmerman both omit years of zero earnings among the children in their work.

studies, if more than one child is matched to a father, all father-child cases are used and the standard errors are corrected for within family correlation.[42]

The model is estimated in two ways to deal with the issue of topcoded earnings of the sons and daughters. One way is to simply use OLS, but adjust the dependent variable using the imputed earnings calculated from the 1996 SIPP-SER when sons or daughters have been topcoded. The second way is to set up a tobit model with an individual specific right-censoring point, as follows:

$$y^*_{1i} = \rho y_{0i} + \beta_1 Age_{0i} + \beta_2 Age^2_{0i} + \beta_3 Age_{1i} + \beta_4 Age^2_{1i} + \varepsilon_i \tag{9}$$

$$y_{1i} = y^*_{1i} \text{ if } y_{1it} < top_t \ \forall\ t \tag{10}$$

$$y_{1i} = k_i, \text{ if } y_{1it} \geq top_t \text{ in some } t \tag{11}$$

Here y_{1i} is the observed level of permanent earnings which is equal to the actual permanent earnings level, y^*_{1i}, only if annual earnings each year is below top_t, the taxable maximum earnings in each year. If earnings are topcoded in *any* one year, then the actual permanent earnings are treated as right-censored at the observed point k_i.[43] The disturbance term is assumed to be normally distributed and maximum likelihood estimation is used to estimate the intergenerational elasticity.[44] In the case of the daughters, there is likely to be little difference between the OLS and tobit estimates since few women in the sample are censored at the taxable maximum.

In the first set of results, three different sample selection rules are used. First, fathers who do not have positive earnings in both 1984 and 1985 are dropped. Given that there are only two years of earnings, allowing zero earnings in any year is likely to add considerable noise. The

[42] The effects of restricting the sample to only the oldest child in a family is shown later in the section.
[43] A problem with this approach is that it treats individuals the same regardless of the number of times they were censored over the four years. A better approach would be to run a tobit for each year using a standard human capital earnings function and then average the predicted earnings over the four years for the censored observations. Given the lack of survey data for the sons and daughters as *adults*, this was not attempted.
[44] The "intreg" command in STATA is used which allows for a variable censoring point for each observation and for clustered standard errors.

other two exclusion rules drop fathers who have earnings below a cutoff point in either year. The cutoffs used are $1000 and $3000 in 1998 dollars.

The results are shown in Table 2. Without using any earnings cutoff, the father-son elasticity which has been the focal point of the literature, is estimated at 0.342 using OLS and a bit higher at 0.384 using the tobit specification. The elasticity between fathers and daughters is also quite similar. The tobit estimate is 0.360, which is only slightly higher than the OLS estimate of 0.341. The difference between OLS and Tobit should be quite small since only about 2 percent of the daughters are topcoded. The results for the daughters might be biased upwards if the high incidence of non-working among daughters is due to other factors such as child-bearing which in turn, is correlated with parent earnings. Using an earnings cutoff does not appear to change the results appreciably. In these cases, the father-son earnings elasticity appears to drop slightly while the father-daughter elasticity remains remarkably stable. A reasonable summary of Table 2 is that the intergenerational elasticity is about 0.35 and is not significantly different between sons and daughters.

It should be kept in mind that these results are based only on *two-year* averages of fathers' earnings. The comparable result from Solon (1992) is 0.385 and from Zimmerman (1992) is 0.481[45]. Couch and Lillard (1998) using selection rules similar to those employed by Zimmerman on the same data, find the elasticity to be 0.37 when using a four-year average. This suggests that simply using the two-year averages from the SIPP gives results similar to those obtained using the PSID and NLS. At a minimum, this adds further confirmation to the argument that the early studies that found correlations of 0.2 or less did not accurately reflect the degree of earnings mobility in the U.S.

SER Results

[45] This estimate for Solon is the average of the results found in Table 2, column 2 of Solon (1992). The estimate for Zimmerman is from Table 6 column 2 of Zimmerman (1992).

The second stage of the study uses the SER earnings data for the fathers. This not only significantly enlarges the sample, since SIPP nonresponse and attrition is eliminated, but it also allows for averaging fathers' earnings over many more years. This longer time period should largely eliminate the problem of attenuation bias stemming from measurement error and transitory fluctuations in earnings. Once again the earnings elasticity is estimated separately for sons and daughters and also with both groups pooled. In this exercise all the results are based on the tobit specification using the same dependent variable as in the prior analysis. Fathers' earnings are progressively averaged more years beginning with the two year average of 1984 and 1985 as was done with the SIPP earnings. Additional estimates are based on averages of four years, seven years, ten years and sixteen years. In all cases the averages are taken over the range of years ending in 1985.

Table 3 presents results using the SER data. There are two broad categories of selection rules on fathers' earnings that are used in this analysis. In the first set of results earnings must be positive in each year. This is particularly important when averaging fathers' earnings from before 1979, since it is impossible to know whether instances of zero earnings are due to non-employment or non-covered status in these years. In the second set of results, some years of zero earnings are allowed. Within each set of results, there are three additional selection rules. In the first set of estimates which require positive earnings in each year (row 1 of Table 3), those identified as non-covered are dropped from the analysis. Under the second rule (estimates in row 2) those identified as non-covered (going back to 1979 only) are imputed, as described in Section II. Under the third rule (row 3), those identified as government or self-employed workers at any time during the 1984 SIPP survey period are dropped.

The results from using the two-year average with SER data is lower than what was found using the SIPP. The highest coefficient is 0.289 when non-covered fathers are dropped from the analysis. The problem of measurement error in the SER data is the obvious explanation for this result. When SER earnings are required to be at least $3000 in each year, the estimated coefficient (dropping non-covered fathers) rises to 0.334 (not shown) which is comparable to the SIPP results from Table 2. This suggests that the results based on the SER may, in fact, be biased down by even more than would be the case with comparable survey data. It also suggests that the possibility of upward bias from correlated measurement error between fathers and children when using SER data is more than offset by the attenuation bias from measurement error.

Another finding that is readily apparent from Table 3 is that the estimated elasticity is only slightly lower when the imputed non-covered fathers are added to the sample. In fact, when fathers' earnings are averaged over short time horizons the results are sometimes larger with this adjustment. The most striking finding is that the elasticity rises dramatically as the fathers' earnings are increasingly averaged over more years. Indeed, the estimated father-son elasticity is 0.613 when the fathers' earnings are averaged over 16 years. The father-daughter elasticity is a bit lower at .570. When the sample of fathers is restricted to private sector, non self-employed workers, however, the father-daughter elasticity is estimated at 0.754. Such a high degree of transmission is rather surprising and may be due to the correlation between fathers' earnings and daughters' labor force participation.

Does Excluding Years of Non-Employment Matter?

The estimates in the lower panel of Table 3 also suggest that the results are not sensitive to the inclusion of years of zero earnings. For example, when averaging earnings from 1979 to 1985, allowing as many as four years of zero earnings to be averaged in, has almost no effect. When non-covered fathers are dropped, the father-son elasticity estimate falls slightly from 0.445 to 0.434. However, when non-covered fathers are imputed, the coefficient actually *rises*, from 0.376 to 0.403. While the choice of how many years of zero earnings to include is somewhat arbitrary, as long as one positive year of earnings is required, the estimated elasticity is raised substantially from the results that allow for zero earnings in all years.[46] To illustrate this, Appendix Table A3 shows the effects of varying the number of years of zero fathers' earnings that are included, on the father-son intergenerational elasticity. It seems reasonable to conclude that after only a few years of positive earnings are included in the average, the attenuation bias from including years of zero earnings is eliminated.[47] Given that children who are not working are also not excluded from the analysis, the criticism by Couch and Lillard (1998) that high estimates of the intergenerational elasticity are based on exclusion rules are not supported by this dataset.

The Role of Persistent Transitory Earnings

Given the high estimated elasticity in this sample, a natural question is what explains the difference between the results presented here and the earlier literature? One possible explanation, of course, is that the elasticity may be different for this cohort of sons compared to the earlier cohort, born in the 1950s that was studied by Solon (1992) and Zimmerman (1992). There is some suggestive evidence that family background played an increasingly important role during the 1980s through the rising returns to human capital investment.[48] The comparison here, however, is of young adults in the late 1990s compared to those in the *1980s*, not the 1970s. A second possibility might lie in the quality of earnings data. Perhaps officially reported earnings data significantly reduces the attenuation bias as compared to self-reported measures. However, as is shown here, the attenuation bias is larger with the SER data than with survey data in the SIPP. In any case, neither argument can explain the striking similarity of the results of this study

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[46] In cases where fathers' earnings are zero in *all* years, Couch and Lillard (1998) recoded zero earnings as $1 so that the log would be zero rather than negative infinity. It is not clear how to treat such cases in a log-log specification but allowing them to enter as zeroes on a *log*-scale may significantly alter the results due to the leverage of such observations.

[47] It should be noted that the results from the last row of Table 3 which average over 10 years and 16 years, include years of zero earnings that are due to *non-covered status*. In these cases, more restrictive rules are used. It was decided that fathers must have positive earnings in about 70 percent of the years in these cases. This was chosen because under this rule, the results for a seven-year average when non-covered zeroes are included is similar to the results when only zeroes due to non-working are allowed.

with the existing literature when fathers' earnings are averaged over short time periods. The difference appears to be due to the longer time horizon over which fathers' permanent earnings are calculated. In fact, any explanation that might be offered as to why the estimates are higher in this study must explain why this is only the case when averages are taken over long periods.

As was discussed in section II, it might be the case that short-term fluctuations in earnings are highly persistent and are not adequately "averaged away", especially when averages are taken over short time periods. The simulation exercise presented earlier suggested that the intergenerational elasticity calculated with a five year average may be only two-thirds of the actual parameter value under plausible assumptions. Taking this approach a step further, the entire "path" of the attenuation factor can be plotted as the average of fathers earnings are taken over more years. This can then be compared to the results in this study under various assumptions on the true intergenerational elasticity. Figure 4 shows this comparison using assumptions based on a recent study by Hyslop (2000) and assuming that the true intergenerational elasticity is 0.7.[49] The theoretical attenuation bias declines but a slowing rate as more years are used in the averaging process. The results from Table 3, in contrast, show a more linear increase in the estimated coefficient. In fact, the results when fathers' earnings are averaged over 16 years, appear to be somewhat higher than what would be predicted using the theoretical model when the true intergenerational elasticity is assumed to be 0.7. One explanation might be that the transitory fluctuations vary over an individual's lifespan, where in this simulation it is treated as constant. Gordon (1984) and Baker and Solon (1999) for example, have shown that the transitory variance follows a "U-shape" over an individual's lifetime. If this is indeed the case then the attenuation bias should be somewhat lower than the model predicts when the fathers' earnings are averaged using years when their age is close to forty.

[48] See Levine (2000) and Duncan et al (1996).
[49] See footnote 14.

In the final analysis, this exercise is merely suggestive. The estimates of the intergenerational elasticity are subject to sampling error and there is no good way to know the correct parameters of the statistical model, or even if the model itself is appropriate. Analysis of other more recent matched SIPP-SER datasets ten years from now is probably needed to resolve this issue. Still, highly serially correlated transitory earnings appears to be a reasonable explanation for why the results from five-year averages might be so different from that found using a sixteen-year average.

Other Sample Selection Issues

There are some issues related to the construction of the matched dataset that can potentially bias the results. First, children must have been coresident with their parents or living away at college at the beginning of the survey. Second, in order to have been matched, they must also have a social security number that was provided to the interviewer. To handle the first problem, the sample of *all* individuals born between 1963 and 1968 in the 1984 SIPP were divided into 24 groups by year of birth, sex and race. The rate of "living at home" was calculated for each group. The inverse of these rates could then be used to weight the children in the intergenerational samples used in this study. For the problem of matches based on social security numbers, a probit analysis was done to predict the likelihood that individuals from the cohort in the SIPP would be matched to their fathers. The inverse of the predicted probabilities can also be used to weight the father-son pairs in the analysis. Table 4 shows the effects of incorporating these weights on the estimated elasticities using the SIPP-based sample of fathers. The first row simply presents the earlier estimated results from the bottom row of Table 2. The second row weights the observations by the inverse of the probability that they are both living at home and have provided a social security number. The overall elasticity when sons and daughters are pooled is identical at 0.365 but rises slightly for sons and falls slightly for daughters.

Other variations are also attempted in Table 4. Restricting the sample to only the oldest child in each family has a small but insignificant effect on sons and virtually no effect on daughters. Using those aged 19 or 20 in 1983 lowers the elasticity to 0.283. The difference is still within the sampling error but might indicate some effect. The result is consistent with the observation by Solon (1999) that using the earnings of sons when they are observed at a younger age can bias the results downwards. It is probably not due to the fact that older kids living at home are more similar to their parents since many of those aged 19 or 20 are actually attending college. The final two rows of Table 4 use different sample selection rules on children. Dropping those children identified as non-covered rather than imputing them has almost no effect for sons but a significant positive effect on daughters. Finally, more restrictive rules on averaging in years of "zero" earnings for the dependent variables has no effect.[50]

A possible problem when using the SER data for fathers' earnings is topcoding of the independent variable. In the absence of any correction, this would result in an upwards bias in the elasticity coefficient. Imputing the topcoded fathers with the mean level of earnings for those topcoded, ideally, should correct this problem.[51] A way to check the robustness of the results of this procedure is to simply drop the topcoded fathers. Table 5 presents the results of this exercise when fathers who are topcoded in *any year* over the relevant time horizon are dropped from the sample. The results are shown for sons and daughters pooled, in order to try to keep the sample as large as possible. For the most part it appears that dropping these fathers lowers the estimates of the elasticity. When using the seven-year average, however, the results are still quite similar. Including topcoded fathers results in an estimate of 0.472 while dropping these observations fathers results in an estimate of 0.439. Averaging fathers' earning using years before 1979 is particularly troublesome because the taxable maximum in real terms was so much lower during

[50] Those identified as non-workers are assigned the mean earnings from the 1996 SIPP of those with zero to two months of paid work, so there are no true cases of zero earnings.
[51] Of course, this assumes that true statistical model is a linear relationship between fathers' earnings children's earnings, which itself, is the subject of inquiry in Section V.

that time. As a result so many of the observations are topcoded, and hence, dropped, that it is not clear that the results are meaningful. In fact, the average over 1970 to 1985 has a sample too small to even precisely estimate the coefficient.

V. Further Extensions

Sources of Income

An interesting finding in some previous intergenerational studies is that family income is more highly correlated across generations than is fathers' earnings.[52] Most of these studies, however, have not discussed this result in much detail.[53] While this study is limited to the use of earnings as an outcome for children it can examine the effects of other measures of parental economic status. The use of family income provides a broader measure that includes not only the mother but also incorporates other forms of *non-earnings* income into the analysis. Although the SER data does not have data on other forms of income, the SIPP is particularly useful because it provides a very detailed breakdown of sources of income that can be used for the parents. Table 6 provides the results of an analysis that substitutes income for earnings in the model and also looks separately at two parent families, single mother families and both types of families pooled together. In all cases, only parents whose income measure exceeds $3000 in 1998 dollars in 1984 and 1985 are included. Using fathers' income rather than earnings raises the intergenerational elasticity quite a bit. For sons the estimate increases from 0.349 to 0.518. Using income rather than earnings also appears to raise the elasticity sharply when two parents are used and if only single mothers are examined. Adding mothers to the analysis also appears to raise the elasticity, particularly for daughters. For example, looking at both parents' income instead of just the fathers' income, raises the elasticity with daughters earnings from 0.496 to 0.708. The comparable increase for sons is from 0.518 to 0.553. Looking at single mothers only, the estimated elasticities are dramatically lower, and in most cases, statistically insignificant. This is

[52] These include Mulligan (1997), Shea (1997), Solon (1992), Altonji and Dunn (1991), Corak and Heisz (1999) and Peters (1992).

[53] The exception is Mulligan (1997) who argues that this result makes perfect sense in a standard intergenerational permanent income model. In such a model under certain assumptions earnings mobility is dictated by regression to the mean in ability which might be relatively rapid. Income mobility, however, might be much slower because of financial asset transfers from parents to children irrespective of

no doubt due in part, to small samples. However, the estimate for the effect of single mothers' income when pooling sons and daughters, is significant, but still only half the size of the two parent elasticity.

What might explain the higher results from parental income? For one thing, income may be a less noisy measure of economic status than earnings. This is likely to be particularly true at the low end of the parents' earnings distribution where individuals may receive income at times when they receive virtually no earnings due to unemployment, e.g. unemployment insurance or workers compensation. This may result in a higher estimated elasticity when parents' income is used rather than earnings because of a smaller attenuation bias due to measurement error or transitory shocks. In addition, there appears to be a sample selection effect. If the intergenerational elasticity is higher at the low end of the distribution, and if more fathers are dropped from the earnings analysis because of exclusion rules on earnings, then including these individuals by using income rather than earnings might raise the elasticity. In fact, if the same sample that is used to estimate the elasticity with fathers earnings in row 1 is also used to estimate the elasticity with fathers' income, then the latter estimate falls from 0.518 to 0.385 (not shown). In any case, it appears that using income rather than earnings for parents may give a more accurate reading of intergenerational mobility, especially when only a few years of parents earnings data are available.

A closer examination of the income measure reveals that the overall income effect is a combination of effects that run in different directions. The SIPP collects detailed information on income each month that covers over thirty categories ranging from government transfer programs to family gifts to specific sources of financial asset income. The sources of income were divided into four main categories: earnings, "welfare" payments, asset income and "other" sources.[54]

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investment in children's human capital. That analysis, however, does not explain differences between the effects of parents' earnings as compared to parents' income on children's *earnings*.

[54] Welfare payments include AFDC, general assistance or general relief, foster child care payments, other welfare, WIC, food stamps and various forms of unemployment compensation. Other income includes a

Given the log-log specification it is not possible to decompose the intergenerational elasticity into a weighted average of these factors. However, it is possible to decompose these effects if the model is estimated in levels instead of logs. The effects of this are shown in Table 7. Estimating the model using only fathers' income on the pooled sample results in a coefficient of 0.113.[55] In the next set of rows, where each component is entered separately, earnings have a similar coefficient of 0.117. What is interesting is that the "other" category has a huge coefficient of 0.584 for daughters and is significant. For sons, the effect is negative but insignificant. While welfare payments enter with a negative sign in all samples, they are not significant. Neither is asset income which has a positive sign and a smaller coefficient than earnings in the pooled sample. It is not clear what to make of the strong finding for "other income" in the case of daughters and why it might not have the same effect on sons. One hypothesis is that given the high incidence of non-work among daughters it might be picking up some positive association between father's non earnings income and daughters decision to work. However, the results were not changed even after dropping daughters who were identified as non-workers through the probit analysis described in Section III. Since most of the types of income in the "other" category are arguably uncorrelated with ability or motivation, it also provides some suggestive evidence that financial resources might, in fact, matter for daughters' future success in the labor market.

Borrowing Constraints

Theoretical models of intergenerational mobility have emphasized borrowing constraints as a key factor in the transmission of earnings inequality. Becker and Tomes (1986) and Mulligan (1997) have argued that if parents can borrow from their children's future earnings, then all parents will invest the optimal amount in their children's human capital. If earnings are

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large variety of retirement related income such as social security and pensions. Also included are: disability benefits, child support and alimony, estates and trusts, income from roomers and boarders, veteran's benefits and gifts.

[55] The equivalent model in logs yields a coefficient of 0.449

determined by human capital, and human capital is a function of ability, then the intergenerational elasticity in earnings will only be positive if earnings and ability are correlated and will depend on the rate at which ability regresses to the mean. With borrowing constraints, however, parents with low income and able children will not invest the optimal amount in their children's education inducing a higher intergenerational elasticity in earnings.

There are several advantages that this dataset can bring to this question. First, with a larger sample it is possible to split the sample along some dimension that reflects fathers' inability to access capital, and still estimate the parameters reasonably well. Second, the topical module from wave 4 of the 1984 SIPP can be used to gather more detailed information on household balance sheets to more accurately classify families by their ability to invest fully in their children's human capital. It was decided to use net worth to classify fathers as either borrowing constrained or not borrowing constrained. This measure captures the ability of individuals to borrow against their current wealth or to draw down assets in order to finance human capital acquisition. One problem with this approach, of course, is that the measure is from 1984, when kids are aged 16 to 21 while the relevant period to measure borrowing constraints is arguably at an earlier point in the child's educational career. In addition, since net worth and income are highly correlated, any nonlinearities in the intergenerational income elasticity may also be reflected in differences in ρ by levels of net worth that may or may not be due to borrowing constraints.

Table 8 shows the results of this exercise. First, using the SIPP for parents' earnings, and dividing the sample by the median level of net worth (about $65,000 in 1984 dollars) the results point to a sharp difference between those below the median and those above. The elasticity is 0.422 for those with lower than median net worth but only 0.140 for those above the median level. While the difference is large, one could not reject the null hypothesis of equality at the 5 percent significance level. The second set of results compares those at or below the first quartile of net worth with those at the top quartile. In this case the difference is even more dramatic and is statistically significant. In fact for the top quartile, there appears to be zero elasticity. Indeed, the permanent income model would predict this result if income is uncorrelated with ability. Similar attempts were less conclusive using SER data for fathers' earnings as the bottom half of Table 8 shows. A likely explanation for this result is that the high topcoding rate of fathers compresses the fathers earnings distribution and given the strong correlation between net worth and earnings, the full variation in the intergenerational elasticity is also compressed.

Differences by Race

One of the key comparisons that has not been explored in previous studies is whether there are significant differences in mobility between blacks and whites. A higher elasticity among blacks might suggest that even if overall mobility is high, economic progress for blacks might be more difficult for other reasons such as borrowing constraints or discrimination. Once again, obtaining reasonable sample sizes for such a comparison has been virtually impossible in previous datasets. Table 9 shows the difference in estimates for blacks and whites. Using the seven-year average of fathers' earnings from the SER, the elasticity among blacks (0.487) was found to be nearly twice as high as the elasticity among whites (0.271) but the difference was not significantly greater than zero at the five percent level.[56] In order to keep the sample size as large as possible, the SER results imputed non-covered fathers and used all fathers with positive earnings in *any* year.[57] Additional results were attempted using the SIPP for parent earnings. The comparison of fathers' earnings by race yields a very similar result to what was found using the SER. The difference in elasticities is estimated at 0.222 which is nearly identical to the 0.216 obtained using the SER sample, but in this case the smaller sample leads to a far less precise estimate. The comparison of fathers' income elasticities leads to a larger difference, though it is

[56] The seven-year average was used because that is the longest average over which there is still a classification of social security coverage status among fathers. This allows inclusion of zero earning years that reflect non-employment but not non-coverage.

still estimated imprecisely. Looking at combined two parent earnings and income, however, leads to incredibly large estimates for blacks that exceed 1. If taken seriously, it implies no regression to the mean. The difference in estimates when using two parents are on the border of significance at the five percent level. The results are similar, though less precise, when the samples include only low net worth families (not shown) suggesting that the racial difference is not simply due to borrowing constraints.

One difficulty in these comparisons lies in family composition. A much higher percentage of black families are headed by single mothers where the estimated elasticities are substantially lower (see Table 6). The small sample size of single mother families, however, does not permit a breakdown by race. While further research is clearly needed, the results presented here are suggestive of less mobility among blacks. Some plausible explanations for the higher persistence might lie in employment discrimination, borrowing constraints, neighborhood effects, inferior schools or discrimination in access to housing.

[57] Using more restrictive exclusion rules raises the estimated correlation for whites slightly and lowers the correlation for blacks slightly. The difference remains large but insignificant.

VI. Conclusion

The study uses a new nationally representative intergenerational sample and finds strong evidence that there is far less intergenerational mobility in the United States than was previously thought. The unique advantage of this dataset is the availability of long-term earnings histories of fathers. It appears that it is precisely this characteristic of the data, that results in the higher estimates. Averages of fathers' earnings taken over long periods of time are less sensitive to transitory fluctuations that many studies have shown are highly persistent. The results point toward an intergenerational elasticity of 0.6 or higher. If accurate, this suggests that many well-documented wage gaps may persist for several generations.

The results appear to be fairly robust to sample selection rules, the match process, and to the problems that are inherent in the use of social security earnings data. Still, further research is needed on datasets that can provide long-term measures of permanent earnings to verify the results presented here. Given the current lack of availability of such data we might have to wait ten or fifteen years before a similar study can be undertaken perhaps on another matched SIPP-SER dataset.

While this study provides new descriptive evidence of the extent of mobility in the U.S. there is still a tremendous amount that is not understood about how the transmission process works. To what extent is the high estimate of the intergenerational elasticity truly a reflection of the importance of financial resources as opposed to vaguely defined characteristics that cannot be influenced by public policy? While far from conclusive, new evidence is provided suggesting that immobility appears to be related to access to capital. This offers a potential avenue by which greater mobility may be fostered through public policy. There is also some evidence that some forms of non-earnings based income, that are uncorrelated with ability, may have a long-term effect. This possibility should be investigated more carefully, though.

Some suggestive evidence also points to less mobility among blacks, a minority group which has struggled to achieve full economic parity many decades after the end of slavery. This suggests that the black-white wage gap may take considerably longer to equalize than discrepancies among other groups.

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