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ABSTRACT

This paper asks whether parental income *per se* has a positive impact on children's human capital accumulation. Previous research has established that income is positively correlated across generations. This does not prove that parents' money matters, however, since income is presumably correlated with unobserved abilities transmitted across generations. This paper estimates the impact of parental income by focusing on variation due to parental factors -- union, industry, and job loss experience -- that arguably represent luck. When I examine a nationally representative sample, I find that changes in parental income due to luck have at best a negligible impact on children's human capital. On the other hand, I find that parental income does matter in a sample of low income families. These findings are potentially consistent with models in which credit market imperfections constrain low income households to make suboptimal investments in their children.

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I. INTRODUCTION

This paper asks whether parental income *per se* affects children's human capital accumulation. It might seem obvious that children born to rich parents have an advantage in the labor market, via access to more or better schooling, better health care and nutrition, better neighborhoods, and so on. If parents' money matters to children's skills, then income redistribution may be warranted on both equity and efficiency grounds.[1]

Economists have traditionally believed that the link between parental resources and children's outcomes operates through human capital investment. Previous research suggests that the strength of this channel should depend on credit market conditions and on public policy. If credit markets are perfect --in particular, if parents can borrow against children's future earnings-- then parents will invest in their children until the marginal product of further investment equals the interest rate; parental income *per se* will have no impact on children's skills. On the other hand, if credit markets are imperfect, then parents may face binding liquidity constraints, in which case the marginal impact of parental income on children's human capital will be positive (Loury (1981); Becker and Tomes (1986); Mulligan (1995)). Public policy, meanwhile, may reinforce or counteract the impact of parental income on children; Glomm and Ravikumar (1992), for instance, show that public education can increase social mobility, while Benabou (1996a) and Durlauf (1996) show that public schools can reduce mobility if schools are financed locally and communities are stratified by income. Aside from human capital investment, sociologists have suggested other channels through which parental income could matter for children. For instance, low income may increase parental stress, which may reduce parenting quality. Alternatively, low income may cause parents to develop patterns of thought and behavior, such as lowered expectations, that are helpful in coping with poverty but damaging to their children's development.[2]

Empirically, a substantial body of research shows that economic status is persistent across generations: children raised in high-income families earn more than children raised in low-income families. Solon (1992) and Zimmerman (1992), for instance, find that the correlation between fathers' and sons' permanent earnings is near 0.4, while Corcoran *et al* (1992), Hill and Duncan (1987) and others show that parental income remains important even after controlling for parental education and other observable parental characteristics.[3] While these studies are interesting in their own right, they do not prove that parents' money matters. High-earning parents presumably have more ability on average than low-earning parents. If ability is transmitted from parents to children through genes or culture, then incomes will be persistent across generations even if parental income *per se* doesn't matter. Put differently, an ordinary least squares (OLS) regression of children's income on parental income will yield an upward-biased estimate of the causal impact of parental income, due to a positive correlation between parental income and children's ability. One can presumably reduce this bias by controlling for observable measures of parental ability, such as education. However, some bias will remain if parental ability has a substantial unmeasured component.

Ideally, one would test whether parents' money matters by dropping money on the doorsteps of randomly selected parents, then tracking the subsequent labor market performance of their children. In this paper, I attempt to approximate such a natural experiment by isolating observable determinants of parental income that arguably represent luck. I focus on variations in fathers' labor earnings due to union status, industry, and involuntary job loss due to plant shutdowns and other establishment deaths. Existing research (Lewis (1986); Krueger and Summers (1988)) demonstrates that wages vary substantially with union and industry status, controlling for observable skills. Moreover, some economists contend that union and industry wage premia reflect rents rather than unobserved ability differences. If this interpretation is correct, then I can estimate the impact of parental income by comparing the children of union or high-wage industry fathers to the children of nonunion or low-wage industry fathers with similar observable skills. Similarly, Cochrane (1991), Jacobson *et al* (1993) and others show that involuntary job loss has a large and persistent negative impact on earnings. If plant closings are exogenous with respect to employees' unobservable skills, then I can estimate the impact of parental income by comparing the children of displaced fathers to the children of nondisplaced fathers with similar observable skills. Operationally, I draw samples of children from the Panel Study of Income Dynamics (PSID), and perform two-stage least squares (2SLS) regressions of children's income on demographic characteristics, fathers' observable skills, and measures of parental income, using fathers' union, industry and job loss variables as instruments for parental income.

My estimates of the impact of parental income could be upward biased for two reasons. First, luck may be correlated across generations. For instance, union fathers may be able to bequeath union jobs to their children via nepotism or social networks (Montgomery (1991)). Second, my instruments may be correlated with unobserved ability; for instance, union fathers may be more able than nonunion fathers with similar observable skills. If this unobserved ability is transmitted across generations, then children of union fathers will fare better than children of nonunion fathers even if parental income *per se* doesn't matter. I correct for the first source of bias in some specifications by removing the part of children's income due to children's observable luck and examining whether parental income affects children's skill-related income. Unfortunately, I cannot correct for the second source of bias. My estimates thus arguably represent an upper bound for the true impact of parental income on children's skills.

My results can be summarized as follows. When I analyze a sample drawn from the nationally representative component of the PSID, I find that parents' money does not matter. I find that that the impact of parental income on children's wages, earnings and years of schooling is positive, significant and economically large when estimated using OLS, but insignificant and usually negative using 2SLS. I find that union and industry status are persistent across generations, particularly for sons, so that removing the component of children's income due to luck further reduces the estimated impact of parental income on children's skills. These results hold for both sons and daughters; they hold for different measures of parental income; they hold for different specifications of the instrument list; and they hold when I allow children's human capital to be reflected in both own and spouse's income. When I focus on a sample of low-income households, however, I find that parents money does matter: among low-income households, children whose fathers experience good labor market luck fare better than children whose fathers have similar observable skills but who experience bad labor market luck. This finding is important, both because actual income redistribution policies are often targetted at the poor and because models of capital market imperfections imply that liquidity constraints are more likely to constrain investment in children's human capital at low levels of parental income.

While there are dozens of existing studies that document the association between parental income and children's outcomes, there are only a handful that attempt to overcome the endogeneity of parental income with respect to intergenerationally transmittable ability; I will critique these studies briefly here.[4] Scarr and Weinberg (1977) examine the relationship between IQ and parental attributes in samples of biological adolescents and adolescents who were adopted prior to their first birthday. Controlling for observable parental characteristics, they find a significant positive relationship between family income and IQ among biological children, but no relationship among adopted children. The authors conclude that the apparent impact of parental income on children's IQ is due to genetic factors. A critic would note that the authors' samples are small and homogenous (the adoptive sample, for instance, consists of 104 relatively well-off Minnesota families) and that family income is measured for only one year, potentially biasing the impact of income downwards in both samples.

Blau (1996) examines the relationship between parental income and children's test scores using the matched mother-child data from the National Longitudinal Survey of Youth (NLSY). Blau finds that parental income has a small positive effect on test scores in OLS regressions controlling for parental characteristics, but no effect in regressions controlling for child fixed effects, in which the impact of income is identified by comparing test results in years of high parental income to test results for the same child in years of low parental income. A critic would note that Blau's approach focuses attention on short-run variation in parental income, rather than cross-section variation in long-run income; the former type of variation will have less impact on children to the extent that parents can borrow and save, to the extent that short-run income fluctuations are due to measurement error, and to the extent that children's outcomes depend on long lags of parental income rather than current income.[5]

Mayer (1997) uses several different approaches to identify the "true" impact of parental income on children; I focus on two examples.[6] First, Mayer examines the link between children's outcomes and parental income from assets and child support payments, arguing that such "other" income is less correlated with parental ability than labor earnings or transfer payments. Mayer finds that other income has a smaller impact than overall income on children's test scores, teenage childbearing, dropping out of school, and single motherhood. However, total income and other income have similar positive and significant effects on children's years of schooling, wages and earnings, suggesting that that asset income and child support payments may be positively correlated with unobserved parental ability.[7] Second, Mayer examines the impact of state welfare benefit differences. She finds that children of both married-parent and single-parent families fare better in high-benefit states, consistent with the idea that states with stronger labor markets pay higher benefits; however, the gap between children of married parents and children of single parents does not narrow as the state benefit level increases, suggesting that benefit levels *per se* do not matter for children. On the other hand, Mayer does not establish that higher benefits narrow the gap in *parental resources* between single parents and married parents. Such narrowing is not automatic, as Mayer points out; for instance, higher welfare benefits are typically offset by lower food stamp benefits; not all single parents go on welfare; higher welfare benefits may induce labor force withdrawl by single mothers; and so on. It is therefore difficult to judge whether Mayer's results are due to a small "second-stage" effect of parental income on children, or to a small "first-stage" effect of state benefit levels on parental income.

The rest of this paper proceeds as follows. Section II presents a simple model of intergenerational transmission. The model illustrates why OLS estimates are likely to overstate the true impact of parental income on children, shows how one can estimate the true impact using instrumental variables, and discusses possible biases arising from this approach. Section III describes the data, and Section IV presents empirical results. Section V concludes.

II. A SIMPLE MODEL OF INTERGENERATIONAL TRANSMISSION

This section presents a simple, mechanical model of intergenerational transmission, designed to fix ideas and to motivate the empirical work below.[8] To begin, assume that we observe permanent income for a sample of parents and their children. Assume that a child's income (Y_i) depends on own human capital (H_i) and luck (L_i):

$$Y_i = H_i + L_i, \tag{1}$$

where for our purposes, H_i could encompass factors such as innate intelligence, manual dexterity, education, training, ambition, and work ethic. Assume that children's human capital depends stochastically on both parents' human capital and parental income:

$$H_i = \rho H_{i-1} + \gamma Y_{i-1} + \varepsilon_i, \tag{2}$$

where ε_i is a disturbance term assumed orthogonal to parental attributes. The first term on the right hand side of (2) represents the transmission of ability from parents to children through genetic and cultural endowments. The second term represents the causal impact of parental income on children's human capital. My goal in this paper is to estimate γ.

Combining (1) and (2) we have

$$(3) \qquad Y_i = \gamma Y_{i-1} + \rho H_{i-1} + L_i + \varepsilon_i.$$

Now suppose we regress Y_i on Y_{i-1} using OLS. Equations (1) and (3) imply that the resulting estimate of γ is upward biased, since parental income Y_{i-1} is positively correlated with parents' human capital H_{i-1}.

Now suppose, however, that there exists a vector of observable variables, X_{i-1}, that may reflect either parental skill or luck, and a vector of observables, Z_{i-1}, that conditional on X_{i-1} reflect only parental luck. In the empirical work below, X_{i-1} includes variables such as fathers' education and occupation, while Z_{i-1} includes fathers' union, industry, and job loss experience. Assume these variables are related to human capital and luck as follows:

$$(4) \qquad H_{i-1} = \alpha_1 X_{i-1} + u^H_{i-1}$$

$$(5) \qquad L_{i-1} = \beta_1 X_{i-1} + \beta_2 Z_{i-1} + u^L_{i-1}$$

The key assumption in equations (4) and (5) is that the component of Z_{i-1} orthogonal to X_{i-1} is itself orthogonal to u^H_{i-1}, so that setting the coefficients of Z_{i-1} in (4) to zero is a valid exclusion restriction. In my application, this amounts to assuming that, conditioning on observable skills, fathers' union, industry and job loss experience are orthogonal to the part of unobserved ability transmitted across generations. This assumption would be valid, for instance, if conditioning on observable skills one's union, industry and displacement experience were solely a matter of luck.

Substituting (4) into (3), we have

$$(6) \qquad Y_i = \gamma Y_{i-1} + \lambda X_{i-1} + \rho u^H_{i-1} + L_i + \varepsilon_i,$$

where $\lambda = \rho \alpha_1$. Under the assumptions made above, we can now estimate γ consistently by regressing Y_i on Y_{i-1} and X_{i-1}, using Z_{i-1} and X_{i-1} as instruments. Intuitively, this procedure identifies γ by comparing the children of union (or high-wage industry, or nondisplaced) fathers to the children of nonunion (or low-wage industry, or displaced) fathers with otherwise similar observable characteristics.

There two obvious reasons why this procedure might produce biased estimates of γ. First, luck may be correlated across generations. For example, if union jobs pay rents, then there are presumably non-market mechanisms allocating union jobs to the lucky few. If these mechanisms include social connections or nepotism, then children of union fathers should have an edge obtaining union jobs. In this case, Z_{i-1} would be correlated with L_i via Z_i, and IV estimates of γ would be biased upwards. Below, I counteract this bias by removing the component of children's income due to children's luck (Z_i) and examining the relationship between parental income and the part of children's income due to skill. This relationship should be positive if high-income parents can invest more in their children's human capital.

Second, fathers with favorable Z_{i-1} may have higher unobserved ability than fathers with unfavorable Z_{i-1}. If this unobserved ability is transmitted across generations, then Z_{i-1} would be correlated with

u^H_{i-1}, and IV estimates of γ would again be biased upwards. It seems unlikely that unobserved ability would be correlated with job displacement due to establishment death. There is a theoretical presumption, however, that union and high-wage industry workers are more able than nonunion and low-wage industry workers, since jobs that pay rents should attract an excess supply of willing workers, affording firms the luxury of selecting the best applicants (Pettengill (1979)). Empirically, the strongest evidence for the unobserved ability view comes from studies using panel data (Chamberlain (1982); Jakubson (1991); Murphy and Topel (1990)). These studies find that union and industry switchers experience wage changes that are small relative to the corresponding cross-section wage differences, suggesting that union and industry premia are primarily due to differences in unobserved ability. Other studies, however, counter that spurious union and industry switches in panel data are common relative to true switches, biasing panel estimates of union and industry premia downward (Freeman (1984)). Furthermore, studies that attempt to reduce the impact of such measurement error find wage changes for switchers that are similar to cross-section wage differences (Chowdhury and Nickell (1985); Krueger and Summers (1988); Gibbons and Katz (1992)). Additional evidence is provided by Holzer, Katz and Krueger (1991), who find that union wage premia generate a significant increase in the number of applications per job opening, while industry wage premia have a smaller and insignificant effect on job queues. Since jobs paying rents should attract more applicants than jobs paying no rents, this evidence suggests that union premia are more plausibly interpreted as rents than industry premia.

In this paper, I adopt as an identifying assumption the view that my instruments are uncorrelated with unobserved ability. I concede, however, that union and industry premia are probably in part due to unobserved ability, in which case my estimates of γ arguably are an upper bound for the true impact of parental income on children's human capital. Of course, if one accepts that job displacement is a valid instrument, I can use overidentifying restrictions tests to examine the validity of union and industry status.

III. DATA

My principal data source is the Panel Study of Income Dynamics (PSID). The PSID is an annual survey that has followed a fixed group of families since 1968. An important feature of the PSID is that it tracks households that split off from original survey households, enabling me to link parents to their adult children. The PSID tracks two separate samples: the "random" component, intended to be representative of the US population, and the "poverty" component, which overrepresents low income households. For now, I focus on the random component. My sample consists of all children satisfying the following criteria: (1) the child is alive and less than 18 years old in 1968; (2) the child has at least one year between 1976 and 1992 in which s/he is a household head or spouse, aged 25 or older, with positive labor earnings and hours worked; (3) the child's father is the household head in 1968; (4) there is at least one year between 1968 and 1989 in which the child is less than 23 and in which the father is a household head, aged 25 to 64, with positive hours and earnings; and (5) information on education, occupation and industry are available for both father and child. My sample consists of 1669 children (830 sons and 839 daughters) matched to 783 fathers. My sample composition differs from Solon (1992) in that I allow multiple children from the same family and daughters as well as sons. In my empirical work, I allow disturbances to be correlated among children from the same family, and I examine both pooled results and results treating sons and daughters separately.

My empirical strategy requires that I measure the permanent incomes of parents and the human capital of their children. For parents, I use two measures of permanent income: fathers' labor earnings and total parental income, consisting of labor earnings, asset income and transfer income of head and spouse.[9] For children, I measure human capital using wages, labor earnings and years of schooling.[10] Income, earnings and wages are expressed in 1988 dollars. I average fathers' earnings and parental income over all years in which the father is a household head aged 25 to 64, and in which the child is less than 23 years old and thus potentially still dependent on parental support. For children, I average wages and earnings over all years in which the child is a household head or spouse aged 25 or older.[11] I compute average earnings including years of zero earnings, and compute average wages weighting by annual hours worked; results are similar if I exclude years of zero earnings. I average wages, earnings and income over many years to obtain the most accurate possible measure of permanent income; Solon (1989, 1992) and Zimmerman (1992) show that measurement error in fathers' permanent income biases estimated intergenerational income correlations downwards, and that averaging over several years attenuates this bias. To correct for the fact that I observe fathers and children at different points of the life cycle, my regressions include a constant and sample averages of fathers' and children's age and age squared; I also include an indicator for nonwhites.[12,13] I allow the coefficients on the constant, race, and children's age variables to differ by the child's gender.

I also require observable measures of parental ability and luck. In this paper, the ability vector X_{i-1} includes fathers' years of schooling and fathers' within-sample averages of eight one-digit occupational dummies, a marriage dummy, an SMSA dummy and a South dummy; results are similar if I also include mothers' education.[14,15] The luck vector Z_{i-1} consists of fathers' within-sample averages of a union dummy and eight one-digit industry dummies, along with an indicator for whether the father ever reports losing a job because the company folded, changed hands, moved out of town, or went out of business.[16,17,18]

Table 1 presents descriptive statistics for fathers and children. For all variables, I report the mean across sample members of individual averages over time.[19] Thus, for instance, the reported SMSA statistic for fathers could imply that 62 percent of fathers live in a city all the time, or that all fathers live in a city 62 percent of the time; the first case is closer to the truth in this and similar instances. On average, I have almost 12 years of data per father, and over 8 years of data per child, implying that I measure permanent incomes over a reasonably long time span on average.

My empirical strategy will be informative only if fathers' union, industry and displacement experience are important sources of cross-section variation in parental income. Accordingly, Table 2 presents results from the first-stage regressions of fathers' log average earnings and log average parental income on demographic variables, fathers' observable skills (X_{i-1}), and fathers' observable luck (Z_{i-1}). I report only the estimated coefficients on Z_{i-1}; standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families.[20,21] The results indicate that belonging to a union has a positive and significant effect on fathers' earnings and a positive but smaller impact on total income. The industry coefficients represent impacts relative to services; evidently, jobs in mining, construction, manufacturing and transportation generate significantly higher earnings and income than jobs in other industries, particularly services and agriculture. The coefficient on job loss is negative and significant for both measures of income. These instruments are highly significant; for both regressions, a Wald test easily rejects the null hypothesis of joint insignificance of Z_{i-1} at one percent. The final row reports the partial R-squared, equal to the squared correlation between the components of fitted and actual income orthogonal to demographic variables and observable skills. For earnings, the partial R-squared is 0.076; for income, the partial R-squared is 0.036. This suggests that my instruments capture more cross-section variation in fathers' earnings than in parental income.

IV. EMPIRICAL RESULTS

This section presents estimates of the impact of parental income on children's human capital. Table 3 presents results using total parental income. The first column of the first row shows results from an OLS regression of children's log average wages on demographic variables and log average parental income. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form and arbitrary error covariance within families. The estimated effect of parental income is 0.361 and is significantly different from zero. This estimate is consistent with Solon (1992), who finds intergenerational correlations of wages and earnings of around 0.4.

The second column of the first row presents OLS estimates of γ from the specification

$$(7) \qquad Y_i = \underset{\text{Variables}}{\text{Demographic}} + \gamma Y_{i-1} + \lambda X_{i-1} + \varepsilon_i,$$

where Y_i is the child's log wage, Y_{i-1} is log parental income, and X_{i-1} includes measures of fathers' education, occupation, region, marital status and urbanicity. When I control for fathers' observable skills, the estimate of γ remains statistically significant, but falls to 0.187, suggesting that estimates of γ that control only for demographics are biased upward by a positive correlation between parental income and abilities that are transmitted across generations.

While controlling for observable skills presumably reduces the upward bias in γ, some bias is likely to remain if there are important unobserved differences in ability among parents. Accordingly, the third column of Table 3 presents 2SLS estimates of (7) instrumenting for Y_{i-1} using the vector Z_{i-1}, consisting of dummy variables for fathers' union, industry and job loss experience. Instrumenting for parental income reduces the point estimate of γ from 0.187 to −0.106, which is not significantly different from zero. The final two columns of the first row present the p-values of two specification tests: a test of overidentifying restrictions, computed by regressing the estimated 2SLS residuals on demographics, X_{i-1}, and Z_{i-1}, then performing a Wald test of the hypothesis that the coefficients on Z_{i-1} are zero; and a Hausman test of the exogeneity of parental income in equation (7), computed by testing the hypothesis that the OLS and 2SLS estimates of γ controlling for X_{i-1} are identical.[22] I can reject neither the overidentifying restrictions nor exogeneity.

Recall that 2SLS estimates of γ may still be upward biased if luck is correlated across generations; for instance, if children of union fathers have an edge getting union jobs themselves, they may fare well even if parental income *per se* is irrelevant. The second row of Table 3 accordingly examines the impact of parental income on the component of children's wages due to skill. To estimate this component, I first regress children's log average wage (Y_i) on demographic variables, children's observable skills (X_i), and children's observable luck (Z_i), allowing all coefficients to differ by gender. I then set the "skill wage" equal to the actual wage minus the component of the fitted wage due to Z_i; this measure includes both the part of wages due to observable skills and the part due to unobserved "residual" ability. From Table 3, removing the part of children's wages due to luck has little impact on the OLS estimates of γ, but reduces the 2SLS estimate to -0.224; the difference between the OLS and 2SLS estimates is now statistically significant.

The fact that the 2SLS estimate is lower for skill wages suggests that luck is correlated across generations. Table 5 presents direct evidence on the persistence of labor market luck. The first column shows the number of father observations falling in different industry, union and job loss categories.[23] The second column shows the number of children falling into each category. The third column shows the number of father-child matches one would expect if union, industry and job loss were independent across generations. The fourth column shows the actual number of matches, while the fifth column shows the ratio of actual to expected matches. For example, 364 out of 1669 children (21.8 percent) and 623 out of 1669 fathers work in manufacturing. If the probability of working in manufacturing were independent across generations, we would expect to find 136 cases in which both father and child work in manufacturing (623 times 0.218 rounds to 136), while in truth we find

199 such cases; children whose fathers work in manufacturing thus have a 46 percent better chance of working in manufacturing than one would expect by chance (199 divided by 136 is 1.46). Pooling over all industries, 424 children work in the same industry as their father, a 49 percent higher matching rate than one would expect by chance. Union status also appears to be correlated across generations, with a 43 percent excess matching rate. Job loss is not persistent across generations; in fact, the number of job-losing father-child pairs is about 25 percent below chance.

The third and fourth rows of Table 3 present evidence using labor earnings to measure of children's human capital. The OLS estimate of γ controlling only for demographics is 0.467; this falls to 0.221 when I control for fathers' observable skills, but remains highly significant. Instrumenting for parental income, however, reduces the estimate to -0.161, and removing the part of children's earnings due to luck reduces the 2SLS estimate even further, to -0.306. These point estimates, taken literally, suggest that fathers' income has a negative impact on children's human capital, although the estimates are not significant. The earnings estimates are less precise than the wage estimates, so that the differences between OLS and 2SLS are not significant. A somewhat troubling result is that I reject the overidentifying restrictions at five percent for earnings and at ten percent for skill earnings.

The final row of Table 3 presents estimates of (7) using children's years of schooling as the dependent variable. When I use OLS and condition only on demographics, I find a strong positive relationship between parental income and children's schooling; the estimate suggests that doubling parental income produces almost two years of extra schooling per child. When I control for fathers' observable skills but continue to use OLS, the response of children's education to parental income declines but remains positive and significant. When I instrument for parental income, however, the estimate of γ becomes negative and significantly different from OLS at ten percent.

Table 4 presents results using fathers' earnings. These results are similar to those using parental income: the OLS estimates of γ are positive and significant, while the 2SLS estimates are negative and insignificant; I reject the overidentifying restrictions for earnings; and I reject exogeneity at five percent for skill wages and skill earnings. The 2SLS estimates using fathers' earnings are more precise than those using parental income, because my instruments are more strongly correlated with fathers' earnings than with total income. Nevertheless, since income is a more compelling measure of parental resources *a priori*, I will focus on parental income for the rest of the paper; results for fathers' earnings are broadly similar.

Alternative Instrument Lists

The 2SLS results reported above use all instruments. Table 6 reports results using industry, union and job loss separately as instruments. In these experiments, I reassign variables excluded from the instrument vector Z_{i-1} to the vector of observable skills X_{i-1}. Overall, the results are broadly robust to the choice of instruments; the 2SLS estimates of γ lie below the corresponding OLS estimate in all cases, and are negative in all but two cases. There is some tension, however, between the industry results and the union and job loss results, with industry dummies generating substantially higher point estimates in four of five cases. This tension is also evident in tests of overidentifying restrictions (not reported in Table 6), which are not rejected when I combine union and job loss, but are rejected for earnings and skill earnings when I use industry dummies alone or in combination with either union or job loss. One interpretation of these results is that industry is more positively correlated with unobserved ability than union and job loss experience. This interpretation is consistent with Holzer, Katz and Kruger (1991), who find that union jobs generate queues while high-wage industry jobs do not. It is also consistent with prior logic: it would not be surprising to find that union jobs pay rents, since generating rents is a primary goal of unions; on the other hand, it is harder to explain why some industries would pay rents relative to other industries in the long run.

Sons, Daughters and the Marriage Market

The specifications reported above pool sons and daughters. However, it is possible that parental income affects boys and girls differently. Accordingly, in Table 7, I estimate equation (7) separately for each gender. Results are as follows. First, the OLS estimates of γ are positive and significant for both sons and daughters. Second, the 2SLS estimates of γ lie below the corresponding OLS estimates in all cases, and are negative in all but two cases. Third, the differences between OLS and 2SLS are no longer statistically significant, primarily because of smaller sample sizes. Fourth, removing the component of children's income due to luck makes a much larger difference for sons than for daughters. This suggests that fathers bequeath union and industry status primarily to sons. The bottom rows of Table 5 support this conjecture: the excess rate of industry matching is 66 percent for sons, but only 34 percent for daughters; similarly, the excess rate of union matching is 59 percent for sons, but only 17 percent for daughters.[24] Fifth, I reject the overidentifying restrictions in four out of five cases for daughters, but only once for sons.

The overidentifying restrictions tests suggest that the specification reported in Table 7 may be inappropriate for daughters. I have to this point assumed that wages, earnings and schooling are valid measures of human capital for both men and women. This assumption may be incorrect for cultures in which women are expected to specialize in home production. In such cultures, a woman's ability to attract a high-earning husband --her success in the marriage market-- may be as valid an indicator of her human capital as her own wages or earnings. Accordingly, Table 8 presents estimates of equation (7) averaging the wages, earnings and total incomes of children and their spouses.[25] Results are as follows. First, the OLS estimates of γ are positive and significant in all specifications. The OLS estimates are comparable to (and in many cases larger than) estimates obtained using children's income alone, suggesting assortative mating. Second, the 2SLS estimates lie below the OLS estimates in all cases, and are negative in 15 of 18 cases. Third, the OLS and 2SLS estimates are significantly different at five percent for skill wages in both the pooled and sons sample, and for skill income in the sons sample; the estimates are different at ten percent for unadjusted wages and skill income for the pooled sample, and for unadjusted income in the sons sample. Fourth, the overidentifying restrictions are not rejected in any specification. Overall, the conclusion that parental income has little impact on children's skills seems robust to averaging children's own and spousal income.

Are These Estimates Biased Downwards?

Taken literally, most of the 2SLS estimates to this point suggest that parental income has a detrimental effect on children's skills. While these estimates are not statistically significant, one is still naturally led to wonder if my estimates of γ could be biased downwards. In this section, I discuss four possible sources of such bias.

First, fathers' labor market luck may be negatively correlated with unmeasured child-care inputs into children's human capital production. Fathers with union jobs, for instance, may work many hours, while fathers with nonunion jobs may work less and spend more time with their children. If fathers' time is important to children's development, then the adverse effects of good labor market luck on father's time may counteract the benefits of extra parental income; in this sense, income variation due to labor market luck may generate lower estimates of γ than variation due to dropping money on doorsteps.

While this story can rationalize my results in principle, the resulting downward bias is unlikely to be large in practice. The impact of fathers' permanent wage on time spent with children depends on the static elasticity of labor supply. Existing research suggests that the intertemporal elasticity of labor supply for married men is quite low (Pencavel (1986)), and the static elasticity is presumably even smaller. Labor supply appears to be much more elastic for married women than for married men (Killingsworth and Heckman (1986)), suggesting that labor market opportunities and child care are more likely to be negatively correlated for mothers than for fathers. This is why I use only fathers' luck to identify the impact of parental income in this study.

Second, my estimates of γ could be biased downward if union and industry wage premia reflect compensating differentials rather than rents or payments for unobserved productivity differences. If union and industry premia compensate for low fringe benefits, then measured income differences due to union and industry will overstate true differences in family resources, biasing estimated γ downward. If union and industry premia are instead compensation for poor working conditions, the implications for intergenerational transmission are ambiguous. If families treat all sources of income identically when investing in their children, then wage premia due to poor working conditions should enable parents to raise their children's skills, and my estimates of γ should be unaffected. On the other hand, families may rationally decide to allocate rewards for poor working conditions to the worker's consumption bundle; a father who has to work in unpleasant conditions may feel entitled to spend his compensating differential on a new boat rather than on his son's education. In this case, measured income differences due to union and industry will again overstate cross-family differences in resources available to children, biasing my estimates of γ downward.

Empirically, there is little evidence that union and industry premia reflect compensating differences. Freeman and Medoff (1984) report that union workers express more concern with job safety than nonunion workers, but that actual workplace hazards are similar for union and nonunion jobs. Meanwhile, both Freeman and Medoff (1984) and Lewis (1986) cite evidence that union status has, if anything, an even larger impact on fringe benefits than on earnings. Similarly, Krueger and Summers (1988) find that fringe benefits reinforce rather than counteract industry wage differences, and that controlling for working conditions has little effect on industry premia. Overall, it seems unlikely that my estimates are biased by compensating differentials.[26]

Third, fathers' labor market luck may be negatively correlated with the return to human capital investment in children. When I examine the relationship between parental income and children's skills, I am implicitly assuming that the optimal level of human capital investment is independent of the child's expected union and industry status. This may not be true. Lewis (1986), for instance, notes that the union wage premium is higher for less-skilled workers; the corollary is that the return to skill is lower for union workers. Since children of union fathers are more likely to get union jobs themselves, their expected return to skill may be lower than the return for children of nonunion fathers. Fathers' industry, on the other hand, is less likely to influence children's expected return to skill; Katz and Summers (1989) and others show that industry wage patterns are similar by occupation, suggesting that the return to skill is independent of industry.

At a first glance, a negative interaction between fathers' union status and children's expected return to skill seems consistent with the results of Table 6, in which union estimates of γ are typically below industry estimates. Upon closer inspection, however, the evidence is less compelling. The negative interaction story would imply that fathers' union status should have a particularly large negative impact on children's schooling. Yet, from Table 6, the union estimate of γ for education is *higher* than the industry estimate. Further inspection also revealed that the union estimates of γ are much lower for daughters than for sons. This too is inconsistent with the negative interaction story, since fathers bequeath union jobs primarily to sons. These results suggest that there is some other reason why industry estimates of γ are higher than union estimates. One possibility, discussed above, is that industry is more correlated with unobserved ability than union status.

Fourth, the component of income due to union, industry and job loss may be less permanent than other components of income. My methodology attempts to isolate the variation in parents' permanent incomes due to luck. To the extent that I measure permanent income with error, my estimates of γ will be biased towards zero. Recall that I have almost 12 years of data per father on average; hence, I observe parental income for a large fraction of the typical childhood. Nonetheless, my measures of permanent income are not perfect. While this problem affects both my OLS and 2SLS estimates, it may cause larger 2SLS biases if luck is more transitory than other determinants of income.

To assess this possibility, I compare the persistence of different components of parental income. I begin by dividing each father's sample spell in half.[27] I then regress log average first-half parental income on fathers' first-half demographics, skill, and luck. I use the estimated coefficients to construct a skill component, a luck component, and a component due to the regression residual (I discard the component due to demographics). I then perform the same exercise on second-half data. I find that the correlation between first-half income and second-half income is 0.78. For the skill component, the correlation is 0.95, suggesting that income due to skill is particularly persistent. For the luck and residual components, meanwhile, the correlations are 0.67 and 0.65, respectively. Persistence considerations can thus potentially explain why controlling for observable skills reduces OLS estimates of γ, but cannot explain why 2SLS estimates of γ lie below OLS estimates controlling for observable skills.

Results for Low Income Families

My analysis to this point has focused on a nationally representative sample, implicitly assuming that the impact of parental income on children is the same for all families. If credit markets are imperfect, however, then parental income may matter more for children in poor families, since low income parents are presumably more likely to face binding liquidity constraints when investing in their children. In this section I examine the impact of parental income in a sample of low income families. In the interests of sample size, I use both the PSID random and poverty samples, the latter being drawn from urban and/or Southern families with incomes less than twice the poverty line in 1967. I restrict attention to parent-child pairs who obey the selection rules outlined in Section III, and for whom average annual total parental income in 1988 dollars is less than $32,407, the 25th percentile of average parental income in my representative sample.[28] My sample consists of 1356 children (652 boys and 704 girls) from 542 families; of these, 939 children from 354 families come from the poverty sample, and 417 children from 188 families come from the random sample. Table 9 shows descriptive statistics; evidently, fathers in the low income sample have less education, are more likely to be nonwhite, and are more likely to live in the South than their random sample counterparts. Table 10 shows first-stage regression results for total parental income; the union coefficient is larger and more significant in the low income sample than in the representative sample, while the coefficients on construction, manufacturing, and job loss are small and insignificant.

Table 11 presents estimates of equation (7) for the low income sample. The results suggest that parents money matters among low income families: the 2SLS estimates of γ are statistically significant in three of five cases, and are larger than the corresponding OLS estimates in all cases. Further inspection revealed that these results were broadly robust to alternative instrument lists, to splitting the sample by gender, and to averaging children's own and spousal income. Another interesting result is that controlling for fathers' observable skills (X_{i-1}) has a much smaller impact on OLS estimates in the low income sample than in the representative sample. This suggests that fathers' education, occupation and other observable skills may be less indicative of intergenerationally transmittable ability in low income families, which is what we would expect if low income fathers' own human capital accumulation as children was hampered by binding liquidity constraints.

Why are the results for the low income sample so different from those for the representative sample? One possibility, of course, is that the true impact of parental income is positive among low income families, and zero or negative at higher levels of income. Another possibility is that the impact of parental income varies not by income but by race, education, region, or some other dimension along which the two samples differ. I estimated equation (7) separately for whites and nonwhites in the low income sample, and found no systematic variation in γ by race; I also found little variation in γ by education level in either sample.[29] I did find that γ was somewhat higher in both samples for families in the South than for families outside the South, although the 2SLS estimates of γ were still far higher for Northern low income families than for Southern representative families. A third possibility is that my instruments are more correlated with unobserved parental ability in the low income sample than in the representative sample.[30] This conjecture is supported by the first-stage partial R-squared statistics reported in Tables 2 and 10, which show that the instruments collectively explain more variation in parental income in the low income sample than in the representative sample, which is what one would expect if the instruments were more endogenous in the low income sample. On the other hand, this higher partial R-squared is due solely to the large union coefficient in the low income sample. While it is possible that union status is more positively correlated with unobserved ability at low levels of income, it is also possible that the true union premium is higher at low levels of income, given unions' egalitarian bias towards skill compression. One can therefore explain the higher partial R-squared in Table 10 without assuming that the instruments are more endogenous in the low income sample. Furthermore, note that the overidentifying restrictions test results in the low income sample are comparable to those in the representative sample; the restrictions are rejected only for earnings and skill earnings, and these rejections would disappear if I averaged children's own and spousal earnings.

V. CONCLUSION

There can be little doubt that economic status is positively correlated across generations. However, this fact does not necessarily imply that parental income *per se* matters for children's human capital accumulation. Distinguishing correlation from causality is critical to assessing the impact of policies that redistribute income among parents. In this paper, I attempt to unravel correlation and causality by isolating variation in parental income due to observable factors --father's union, industry, and job loss experience-- that arguably represent luck. Using a nationally representative sample, I find that changes in parental income due to luck have at best a negligible impact on children's wages, earnings, years of schooling, and total family income. However, I find that parental income does have a beneficial impact on children among lower income families. These findings are potentially consistent with models with capital market imperfections, since parental investment in their children is more likely to be liquidity constrained at low levels of parental income.

Future research should try to replicate this paper's results using different samples and different instruments, and should attempt to distinguish between competing channels through which parental income could matter for low income parents (health care and nutrition, better school districts, and so on). Future research should also investigate why parental income matters so little in the representative sample. The explanation offered by models with credit market imperfections is that the return to human capital investment is concave, so that above a threshold level of income parents would not borrow against their children's future earnings to finance additional investment (such as moving to a better school district) even if they had the opportunity. A second possibility is that public investment in children is sufficiently redistributive to counteract inequality in parents' resources spent on children. This story makes some sense for college, where access to financial aid is negatively related to parental wealth (Feldstein (1995)). It makes less sense for primary and secondary education, where inequality in per-pupil spending remains large despite recent court decisions forcing some states to redistribute resources from richer to poorer districts (Murray, Evans and Schwab (1996)). A third possibility is that unequal school funding has no impact on educational outcomes. Perhaps surprisingly, the empirical link between school spending and educational output is weak.[31] This does not imply, however, that parents cannot buy their children a better education. As long as school quality variation exists and is known to the public, houses in good school districts will be more expensive than houses in bad districts, creating a potential link between parental income and children's human capital.[32] A fourth possibility is that parents are not strictly altruistic towards their children: the fact that high income parents should be able to send their children to better schools does not automatically mean that they will do so, even if the return to additional human capital investment is high.[33] Finally, it is possible that parental income has a negative effect on children's own inputs of time and effort into human capital accumulation. Holtz-Eakin, Joulfaian and Rosen (1993), for instance, find that receipients of large inheritances reduce their labor supply. An objection to income effects as an explanation for my results, however, is that the children of lucky fathers in the representative sample do not in fact have higher total incomes than children of unlucky fathers. If income effects are indeed responsible for my results in the representative sample, they may take the form of unmeasured psychic gains experienced by the children of lucky fathers from not having to work as hard in school to attain a union or high-wage industry job.

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FOOTNOTES

[1]See Benabou (1996b) and Galor and Zeira (1993) for models in which parental income inequality can lead to inefficiently low levels of human capital investment in poor children, with adverse consequences for aggregate productivity and growth.

[2]Mayer (1997, chapter 3) discusses competing theories of the link between parental resources and children's outcomes at more length.

[3]See Haveman and Wolfe (1995) and Mayer (1997, chapter 4) for additional references and evidence.

[4]There are also several studies examining the impact of the Negative Income Tax experiments on children. Venti (1984) and Mallar (1977) find that the adolescent children of treatment families complete more schooling and are less likely to work than the children of control families, while Maynard (1979) finds mixed evidence on the effects of treatment on test scores of younger children. The NIT evidence is not very informative about the impact of parental income on children, however, because NIT treatment families were subject to high marginal tax rates, which presumably had an independent effect on schooling, parenting and labor market decisions. For example, we would expect NIT treatments to increase adolescent schooling even if parental income *per se* is irrelevant to schooling, because high tax rates on current earnings reduce the opportunity cost of going to school.

[5]Blau himself finds that the impact of parental income on test

scores in regressions without fixed effects is much higher using long-run averages of parental income rather than single-year measures. My critique of the child fixed effects would also apply to estimates (discussed but not reported in Blau's paper) that use mother fixed effects, which identify the impact of parental income by comparing children who grew up in years of high parental income to siblings who grew up when parental income was lower. Blau mentions that many mothers in the NSLY are themselves siblings, potentially enabling him to use grandmother fixed effects that would identify the impact of parental income by comparing children whose mothers had high permanent income to cousins whose mothers had lower permanent incomes.

[6]Mayer takes two other approaches to estimating the true impact of parental income. First, she compares the impact on children's outcomes of recent parental income to the impact of of parental income received after the outcome is observed, and typically finds stronger effects of future income than one would expect merely based on the correlation between current and future income. Since future income *per se* should not influence children's outcomes, Mayer argues that most of the apparent impact of parental income must be due to unobserved heterogeneity. I find this argument unconvincing for two reasons: (1) Mayer estimates "recent" income using only a five-year window, so that future income may appear to matter because it is correlated with income received *prior* to the five-year window, which may affect children's outcomes; (2) anticipated future income should affect current spending on children if households can borrow and save. Second, Mayer examines trends over time in the distribution of parental income and finds that they are not reflected in trends of the distribution of children's

outcomes. This evidence is interesting but may be confounded by trends in other variables such as changing social mores, rising drug use, and changes in the labor market for low-skilled workers.

[7] I experimented with regressing children's human capital on fathers' skills and parental income, instrumenting parental income with asset income plus child support payments. The resulting 2SLS estimates were slightly below the OLS estimates controlling for observable skills reported later in this paper, but were still positive and significant.

[8] Goldberger (1989) uses the adjective "mechanical" to describe models of intergenerational transmission which do not assume utility-maximizing behavior on the part of parents.

[9] I measure total parental income in year T as labor, transfer and asset income of the 1968 father in year T plus labor, transfer and asset income of the 1968 spouse (if any) in year T, regardless of whether the head and spouse are still living in the same household in year T. In years when asset and transfer income are only available for the head and spouse combined, I compute income for each parent by dividing reported combined income by the number of primary adults in the parent's household (which is always either one or two). My measure of asset income equals asset income reported in the PSID plus an imputed income stream equal to seven percent of reported housing equity; results are not sensitive to the inclusion of imputed housing income or to the assumed rate of return. I also experimented with measures of parental resources that include both income and parental wealth, where I measure wealth as housing equity plus asset wealth; I impute the latter using

PSID asset income and time series data on rates of return. The OLS results using this broader measure of resources were somewhat weaker than those using parental income alone, which is perhaps not surprising given likely measurement error in my measure of nonhousing wealth; the 2SLS results were qualitatively similar to those reported in the paper.

[10] When available, I measure the nominal wage as the reported straight time hourly wage; otherwise, I measure the nominal wage as annual labor earnings divided by annual hours. I convert earnings, income and wages reported in year t to 1988 dollars using the Consumer Price Index for year t-1.

[11] For purposes of determining parents' eligible sample years, I assume that children are automatically younger than 23 for survey years 1968-1973, and automatically older than 22 for survey years 1990-1992, regardless of the child's reported age. Otherwise, I use the child's reported age to determine eligible sample years for parents. For purposes of determining children's eligible sample years, I assume that children are automatically younger than 25 before survey year 1976.

[12] One could argue that removing life-cycle variation from income is unnecessary in my context; if parental income *per se* matters for children's success, then children born to older parents should tend to do better than children born to younger parents. As a practical matter, however, I must remove such variation from my data because sample attrition and truncation of the data in 1968 and 1992 imply cross-sectional differences in the extent to which I observe parents' and children's entire histories. For instance, I would not want to

conclude that child A is more successful than child B simply because child A turns 25 in 1976 and has earnings data available from age 25 through age 42, while child B turns 25 in 1992 and has only one year of earnings data available; similarly, I would not want to conclude that father A is more successful than father B simply because father A was 40 in 1968 with a 17-year old child while father B was 25 in 1968 with a 2-year old child.

[13]I experimented with replacing the constant term with a vector of period variables indicating the fraction of sample years spent in different time periods; this formulation corrects for business cycle or secular variation in income. Adding period variables made little difference to the results.

[14]In most cases, fathers' years of schooling is taken from the 1968 PSID individual file. If fathers' years of schooling is reported as a 0 or a 99 in 1968, I use categorical education data from 1968 through 1972 to impute fathers' years of schooling. I estimate children's years of education as of the first year in which they are eligible for sample inclusion; from 1976 through 1984 and 1991 through 1992, this is taken from family-level data, while between 1985 and 1990 this is taken from individual-level data. If children's reported education is 0 or 99, I use data from surrounding years to impute schooling. I dropped cases in which I was unable to impute years of schooling from my sample.

[15]The occupational categories are: professional and technical; managerial and administrative; self-employed businessman (available 1968-1975 only); sales and clerical; craftsmen and foremen; operatives;

farmers; and protective service (police and military). The excluded category is laborers and service employees. Some sample individuals do not report an occupation in some years; for these individuals, occupation dummies are averaged over all sample years for which some occupation is reported. If an individual is unemployed or retired in a given interview, I use reported occupation on the previous job when available.

[16]The PSID did not ask union questions in 1973; I compute fathers' average union status using only data from years other than 1973. In later years, the PSID asked both whether one's job is covered by a union contract and whether one belongs to a labor union; I use the contract question to define union status.

[17]The industry categories are: agriculture; mining; construction; manufacturing; transportation, communication and utilities; trade; finance, insurance and real estate; and government. The excluded category is services. The PSID did not ask industry questions until 1971; I define industry dummies averaging only sample years from 1971 on. As with occupation, some individuals do not report a valid industry in some years; for these individuals, industry dummies are defined as averages over all sample years for which some industry is reported. If an individual is unemployed or retired in a given interview, I use industry on the previous job when available.

[18]Notice that my job loss indicator equals one if a father ever reports a job loss due to establishment death; unlike other sample variables, I do not measure this indicator year by year and then divide

by each individual's total sample years. I measure job loss as a zero-one indicator rather than a sample average because I found that the former variable had more explanatory power in average earnings and wage regressions than the latter, which is reasonable if job loss due to establishment death has long-run consequences for earnings and wages. I also experimented with interacting job loss with the fraction of sample years occuring after the job loss, with little impact on the results.

[19]For fathers with more than one sample child, I include only one spell in Table 1, so that fathers' statistics are computed on only 783 spells rather than 1669 spells. Note, however, that some fathers' variables --such as the fraction of sample years in a union-- can vary from child to child in multiple child families, since for each child the father's data is averaged only over those years in which the child is less than 23. For fathers' statistics in Table 1, I use the father's spell corresponding to the oldest child.

[20]For the OLS regressions reported in this paper, standard errors are computed as follows. Let the 783 families in the sample be indexed by j. Let X_j denote the matrix of RHS variables for family j; this matrix has dimension $T_j * k$, where T_j is the number of children for family j and k is the number of RHS variables. Finally, let $\hat{\varepsilon}_j$ denote the $T_j * 1$ vector of estimated disturbances for family j. Then the estimated variance-covariance matrix is

$$\left[\sum_{j=1}^{783} X_j'X_j\right]^{-1} \left[\sum_{j=1}^{783} X_j'\hat{\varepsilon}_j \hat{\varepsilon}_j'X_j\right] \left[\sum_{j=1}^{783} X_j'X_j\right]^{-1}.$$

For 2SLS regressions, standard errors are computed in the same way, with X replaced by $\hat{X}$, the projection of X on the instruments.

[21]The first-stage regressions reported in Table 2 use all spells for each father, so that the nominal sample size is 1669 observations. Note that the reported standard errors correct for the resulting correlation of errors within families.

[22]Since the disturbance term in (7) is nonspherical, the formula for computing the variance of $\gamma_{OLS} - \gamma_{2SLS}$ given in Hausman (1978) does not apply, since OLS is not the most efficient estimator of γ under the null that father's income is exogenous. I instead compute the variance of $\gamma_{OLS} - \gamma_{2SLS}$ by modifying the formula presented in Hausman and Taylor (1981) for heteroscedasticity of unknown form and arbitrary error covariance within families, following footnote 20.

[23]For the purposes of constructing Table 5, I assign each father and child observation to the industry accounting for the plurality of sample years, where ties are resolved in favor of industries with the lower SIC code. Fathers and children are considered "union" if they are covered by a union contract in more than half of their sample years. Fathers and children are considered job losers if they ever report exogenous job loss.

[24]In principle, the relatively low excess rate of industry matching for girls could be due either to lower excess rates of matching for girls than for boys in each industry, or to the fact that girls are overrepresented in industries where excess matching rates are low for both sexes. In practice both effects seem to be present. From Table 1, daughters are relatively concentrated in trade and services, which from

Table 5 have low excess matching rates, while sons are more heavily represented in construction and manufacturing, which have higher excess matching rates. On the other hand, while excess matching rates for sons and daughters are similar in most industries, boys have a much higher excess matching rate than girls in the service industry (79 to 20 percent), which is quantitatively important because so many daughters work in services.

[25]Let w^C and w^S denote the sample average real wage of the child and spouse, respectively, where w^i is defined as the average of i's wages from t = 1 to T, weighting by i's hours in t. Then the wage in Table 7 is defined as $(w^C * h^C + w^S * h^S)/(h^C + h^S)$, where h^i is defined as total hours worked from t = 1 to T. Note that this wage measure is identical to own wages for children without spouses or for children with nonworking spouses. Similarly, let e^C and e^S denote the sample average real earnings of the child and spouse, respectively, dividing by the number of years the child is in the sample (including years of zero earnings or years in which no spouse is present). Let SFRAC denote the fraction of the child's sample years the spouse is present. Then the measure of average earnings used in Table 7 is defined as $(e^C + e^S)/(1 + \text{SFRAC})$. Average total income is computed in the same way as average earnings. In constructing skill wages and earnings in Table 7, I subtract the component due to luck only for the child, not for the spouse. Skill income is defined removing only part of the child's earnings due to luck; I do not attempt to remove the impact of children's luck on asset or transfer income.

[26]The industry for which wages seem most likely to be high due to

compensating differentials is mining. However, including the mining industry dummy among the control variables X_{i-1} rather than the instrument vector Z_{i-1} reduces 2SLS estimates of γ, suggesting that compensating differetials in mining are not important to my results.

[27]Industry is not available in the PSID prior to 1971. I classify all sample years from 1968 through 1972 as "first half" years regardless of how many years the father is in the sample, in order measure the industry component of parental income with reasonable accuracy. This implies that I must exclude fathers without data after 1972 when investigating persistence. I include only one observation per father, following footnote 19; overall, there are 756 (out of 783) fathers for whom I can decompose first-half and second-half income.

[28]Results are qualitatively similar for other nearby cutoff values for income.

[29]There were not enough nonwhites in the random sample to estimate (7) separately by race.

[30]I examined the persistence of different components of parental income in the poverty sample, with results similar to those for the random sample. Thus, the differences between the random and poverty sample are not due to greater persistence of luck-related income in the poverty sample.

[31]Hanushek (1986) and Heckman, Layne and Todd (1995) present

evidence that school spending has little effect on outcomes, while Card and Krueger (1992) and Figlio (1996) argue that school spending does matter.

[32]Hanushek (1986) reports that the educational production function literature consistently finds large and persistent quality differences among schools. Black (1996), meanwhile, finds a significant positive relationship between school quality and housing prices on opposite sides of elementary school attendance boundaries in Massachusettes.

[33]See Altonji, Hayashi and Kotlikoff (1992) for evidence that families do not behave in accordance with a pure altruism model.

TABLE 1

Descriptive Statistics: Representative Sample

Variable Name	Fathers	Children	Sons	Daughters
Years in Sample	11.89	8.26	8.23	8.30
Real Hourly Wage	15.35	10.00	11.15	8.87
Real Annual Earnings	34879	18324	23912	12976
Total Income	45429	33686	33196	34170
Age	44.26	29.13	29.15	29.13
Nonwhite	0.08	0.09	0.08	0.09
Years of Education	12.06	13.33	13.39	13.28
Occupation:				
Professional & Technical	0.17	0.22	0.21	0.24
Managerial & Administrative	0.17	0.12	0.15	0.10
Self-Employed Businessman	0.04	---	---	---
Sales & Clerical	0.10	0.23	0.11	0.36
Craftsmen & Foremen	0.23	0.11	0.20	0.02
Operatives	0.16	0.12	0.17	0.08
Laborers & Service Workers	0.07	0.15	0.11	0.20
Farmers	0.05	0.01	0.01	0.00
Protective Services	0.02	0.03	0.05	0.01
Living in SMSA	0.62	0.53	0.53	0.54
Living in the South	0.28	0.30	0.31	0.29
Married	0.92	0.69	0.68	0.69
Union	0.30	0.13	0.16	0.09
Industry:				
Agriculture	0.06	0.03	0.05	0.01
Mining	0.01	0.01	0.01	0.00
Construction	0.09	0.07	0.13	0.02
Manufacturing	0.34	0.20	0.26	0.15
Transport/Utilities	0.08	0.06	0.07	0.04
Trade	0.13	0.18	0.16	0.20
FIRE	0.04	0.07	0.04	0.09
Services	0.18	0.33	0.21	0.44
Government	0.07	0.06	0.07	0.04
Job Loss	0.15	0.10	0.11	0.09

TABLE 2

Instrument Relevance

$$Y_{i-1} = \text{Demographic Controls} + \beta X_{i-1} + \mu Z_{i-1} + v_i$$

Instrument	Dependent Variable: Total Income	Dependent Variable: Fathers' Earnings
Union	0.081	0.178
	*(0.039)	*(0.046)
Agriculture	-0.050	0.111
	(0.118)	(0.150)
Mining	0.278	0.460
	*(0.077)	*(0.100)
Construction	0.207	0.336
	*(0.075)	*(0.077)
Manufacturing	0.116	0.220
	*(0.047)	*(0.057)
Transportation & Public Utilities	0.127	0.240
	*(0.061)	*(0.068)
Trade	0.079	0.186
	(0.062)	*(0.076)
Finance, Insurance & Real Estate	0.089	0.131
	(0.096)	(0.106)
Government	0.069	0.206
	(0.074)	*(0.086)
Job Loss	-0.079	-0.101
	*(0.038)	*(0.043)
Partial R-Squared	0.036	0.076
Wald Test	0.000	0.000

NOTES: This table presents results from the first-stage regressions of log average total parental income and fathers' log average earnings on demographic variables and observable measures of fathers' skill and luck. The table reports coefficients on the luck measures Z, as well as the partial R-squared and the significance level of a Wald Test of the null hypothesis that the coefficients on Z are jointly zero. The excluded industry category is services. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families. A (*) denotes significance at five percent.

TABLE 3

Estimates of γ: Parental Income

$$Y_i = \text{Demographic Controls} + \gamma Y_{i-1} + \lambda X_{i-1} + \varepsilon_i$$

Measure of Children's Human Capital	--------OLS--------- X(i-1) not Included	X(i-1) Included	--2SLS, X(i-1) INCLUDED-- $\hat{\gamma}$	Wald Test	Hausman Test
Wages	0.361 *(0.026)	0.187 *(0.035)	-0.106 (0.214)	0.17	0.16
Skill Wages	0.378 *(0.025)	0.188 *(0.034)	-0.224 (0.208)	0.36	0.03
Earnings	0.467 *(0.054)	0.221 *(0.073)	-0.161 (0.417)	0.01	0.35
Skill Earnings	0.492 *(0.052)	0.231 *(0.069)	-0.306 (0.407)	0.08	0.17
Education	1.934 *(0.109)	0.828 *(0.155)	-0.468 (0.803)	0.73	0.096

NOTES: This table presents estimates of the impact of total parental income on the human capital accumulation of children. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families. The final two columns present the p-values from a Wald test of overidentifying restrictions and a Hausman test of the null hypothesis of exogeneity of parental income. A (*) denotes significance at five percent.

TABLE 4

Estimates of γ: Father's Earnings

$$Y_i = \underset{\text{Controls}}{\text{Demographic}} + \gamma Y_{i-1} + \lambda X_{i-1} + \varepsilon_i$$

Measure of Children's Human Capital	OLS: X(i-1) not Included	OLS: X(i-1) Included	2SLS, X(i-1) INCLUDED: $\hat{\gamma}$	Wald Test	Hausman Test
Wages	0.303 *(0.032)	0.150 *(0.043)	-0.041 (0.124)	0.17	0.12
Skill Wages	0.318 *(0.035)	0.158 *(0.039)	-0.121 (0.119)	0.32	0.01
Earnings	0.404 *(0.053)	0.208 *(0.074)	-0.132 (0.235)	0.004	0.14
Skill Earnings	0.427 *(0.051)	0.232 *(0.071)	-0.234 (0.227)	0.06	0.03
Education	1.479 *(0.128)	0.506 *(0.132)	-0.284 (0.490)	0.72	0.10

NOTES: This table presents estimates of the impact of fathers' earnings on the human capital accumulation of children. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families. The final two columns present the p-values from a Wald test of overidentifying restrictions and a Hausman test of the null hypothesis of exogeneity of fathers' earnings. A (*) denotes significance at five percent.

TABLE 5

Can Fathers Bequeath Luck?

Category of Luck	# of Fathers	# of Children	Expected Matches	Actual Matches	Ratio
All Children:					
Agriculture	123	53	4	21	5.25
Mining	16	10	0	1	∞
Construction	164	129	13	34	2.61
Manufacturing	623	364	136	199	1.46
Tran/Comm/Utilities	136	86	7	7	1.00
Trade	189	287	33	33	1.00
FIRE	67	110	4	11	2.75
Services	251	544	82	112	1.37
Government	100	86	5	6	1.20
All Industries	1669	1669	284	424	1.49
In a Union	505	184	56	80	1.43
Job Loser	284	164	28	21	0.75
Sons Only:					
All Industries	830	830	151	251	1.66
In a Union	250	123	37	59	1.59
Job Loser	127	92	14	10	0.71
Daughters Only:					
All Industries	839	839	129	173	1.34
In a Union	255	61	18	21	1.17
Job Loser	159	72	14	11	0.79

NOTES: this table presents information on the frequency of intergenerational luck matches. For each industry, union or job loss category, the first column shows the number of father observations (out of 1669) assigned to this category; the second column shows the number of children in the same category; the third column shows the number of father-child matches one would expect if fathers' and children's luck were independent; the fourth column shows the actual number of matches observed in the PSID; and the final columns shows the ratio of actual to expected matches.

TABLE 6

Alternative Instrument Lists

$$Y_i = \text{Demographic Controls} + \gamma Y_{i-1} + \lambda X_{i-1} + \varepsilon_i$$

Measure of Children's Human Capital	OLS	2SLS ESTIMATES: All Instruments	Industry Only	Union Only	Job Loss Only
Wages	0.187 *(0.035)	-0.106 (0.214)	0.101 (0.268)	-0.709 (0.660)	-0.290 (0.463)
Skill Wages	0.188 *(0.034)	-0.224 (0.208)	-0.065 (0.273)	-0.945 (0.727)	-0.282 (0.461)
Earnings	0.221 *(0.073)	-0.161 (0.417)	0.166 (0.541)	-1.002 (1.215)	-0.781 (1.007)
Skill Earnings	0.231 *(0.069)	-0.306 (0.407)	-0.145 (0.540)	-1.361 (1.277)	-0.763 (1.004)
Education	0.828 *(0.155)	-0.468 (0.803)	-0.522 (1.106)	-0.187 (2.162)	-0.557 (1.732)

NOTES: This table presents estimates of the impact of total parental income on the human capital accumulation of children, using various instrument lists. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families. A (*) denotes significance at five percent.

TABLE 7

Estimates of γ: Sons and Daughters Separated

Sample	Measure of Children's Human Capital	--------OLS--------- X(i-1) not Included	X(i-1) Included	--2SLS, X(i-1) INCLUDED-- $\hat{\gamma}$	Wald Test	Hausman Test
Sons	Wages	0.326 *(0.169)	0.169 *(0.053)	0.075 (0.229)	0.03	0.68
	Skill Wages	0.356 *(0.032)	0.166 *(0.049)	-0.133 (0.219)	0.22	0.16
	Earnings	0.403 *(0.067)	0.258 *(0.084)	0.238 (0.513)	0.29	0.97
	Skill Earnings	0.446 *(0.062)	0.258 *(0.081)	-0.001 (0.497)	0.63	0.60
	Education	1.849 *(0.145)	0.779 *(0.220)	-0.678 (0.922)	0.63	0.11
Daughters	Wages	0.399 *(0.036)	0.210 *(0.048)	-0.065 (0.249)	0.02	0.26
	Skill Wages	0.402 *(0.037)	0.213 *(0.049)	-0.084 (0.238)	0.048	0.20
	Earnings	0.538 *(0.079)	0.199 (0.113)	-0.339 (0.565)	0.002	0.33
	Skill Earnings	0.544 *(0.079)	0.213 *(0.109)	-0.386 (0.541)	0.045	0.26
	Education	2.030 *(0.123)	0.895 *(0.176)	-0.102 (0.851)	0.67	0.22

NOTES: This table presents estimates of the impact of parental income on the human capital accumulation of children, allowing all coefficients to differ by child's gender. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families. The final two columns present the p-values from a Wald test of overidentifying restrictions and a Hausman test of the null hypothesis of exogeneity of fathers' earnings. A (*) denotes significance at five percent.

TABLE 8

Estimates of γ: Average Income of Child and Spouse

Sample	Measure of Children's Human Capital	OLS: X(i-1) not Included	OLS: X(i-1) Included	2SLS, X(i-1) INCLUDED: $\hat{\gamma}$	2SLS, X(i-1) INCLUDED: Wald Test	2SLS, X(i-1) INCLUDED: Hausman Test
All Children	Wages	0.323 *(0.026)	0.201 *(0.034)	-0.124 (0.205)	0.38	0.09
	Skill Wages	0.333 *(0.025)	0.201 *(0.033)	-0.237 (0.206)	0.62	0.02
	Earnings	0.432 *(0.043)	0.248 *(0.055)	-0.027 (0.339)	0.46	0.41
	Skill Earnings	0.442 *(0.042)	0.257 *(0.054)	-0.092 (0.335)	0.61	0.28
	Total Income	0.426 *(0.033)	0.301 *(0.047)	-0.058 (0.243)	0.79	0.12
	Skill Income	0.437 *(0.034)	0.309 *(0.048)	-0.123 (0.240)	0.85	0.054
Sons	Wages	0.309 *(0.034)	0.193 *(0.052)	-0.100 (0.217)	0.38	0.17
	Skill Wages	0.332 *(0.033)	0.191 *(0.050)	-0.279 (0.219)	0.65	0.02
	Earnings	0.451 *(0.066)	0.237 *(0.080)	0.057 (0.468)	0.57	0.70
	Skill Earnings	0.476 *(0.063)	0.247 *(0.078)	-0.038 (0.461)	0.67	0.54
	Total Income	0.465 *(0.049)	0.337 *(0.076)	-0.114 (0.259)	0.50	0.07
	Skill Income	0.489 *(0.047)	0.347 *(0.075)	-0.209 (0.263)	0.56	0.02
Daughters	Wages	0.338 *(0.035)	0.207 *(0.044)	0.048 (0.229)	0.24	0.48
	Skill Wages	0.335 *(0.037)	0.208 *(0.046)	0.011 (0.230)	0.28	0.39
	Earnings	0.414 *(0.042)	0.252 *(0.062)	-0.076 (0.333)	0.06	0.32
	Skill Earnings	0.410 *(0.046)	0.258 *(0.064)	-0.117 (0.343)	0.104	0.26
	Total Income	0.388 *(0.035)	0.264 *(0.048)	-0.043 (0.253)	0.40	0.21
	Skill Income	0.384 *(0.041)	0.269 *(0.055)	-0.084 (0.260)	0.42	0.16

NOTES: This table estimates the impact of parental income on children and their spouses. See text for further details.

TABLE 9

Descriptive Statistics: Low Income Sample

Variable Name	Fathers	Children
Years in Sample	11.00	7.82
Real Hourly Wage	8.12	7.49
Real Annual Earnings	15410	12454
Real Total Income	21904	21983
Age	44.94	29.09
Nonwhite	0.52	0.57
Years of Education	8.42	12.32
Occupation:		
Professional & Technical	0.03	0.09
Managerial & Administrative	0.03	0.06
Self-Employed Businessman	0.04	---
Sales & Clerical	0.05	0.21
Craftsmen & Foremen	0.22	0.12
Operatives	0.27	0.22
Laborers & Service Workers	0.28	0.27
Farmers	0.06	0.00
Protective Services	0.03	0.03
Living in SMSA	0.57	0.54
Living in the South	0.63	0.66
Married	0.89	0.60
Union	0.25	0.12
Industry:		
Agriculture	0.10	0.03
Mining	0.01	0.01
Construction	0.13	0.07
Manufacturing	0.28	0.24
Transport/Utilities	0.07	0.06
Trade	0.14	0.18
FIRE	0.02	0.04
Services	0.19	0.30
Government	0.06	0.08
Job Loss	0.20	0.12

TABLE 10

Instrument Relevance, Low Income Sample

$$Y_{i-1} = \text{Demographic Controls} + \beta X_{i-1} + \mu Z_{i-1} + v_i$$

Instrument	Estimate
Union	0.215
	*(0.038)
Agriculture	-0.145
	(0.084)
Mining	0.274
	*(0.118)
Construction	0.064
	(0.058)
Manufacturing	0.050
	(0.052)
Transportation & Public Utilities	0.143
	(0.076)
Trade	-0.049
	(0.055)
Finance, Insurance & Real Estate	0.152
	(0.119)
Government	0.050
	(0.083)
Job Loss	-0.044
	(0.031)
Partial R-Squared	0.117
Wald Test	0.000

NOTES: This table presents results for the low income sample from the first-stage regression of log average total parental income on demographic variables and observable measures of fathers' skill and luck. The table reports coefficients on the luck measures Z, as well as the partial R-squared and the significance level of a Wald Test of the null hypothesis that the coefficients on Z are jointly zero. The excluded industry category is services. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families. A (*) denotes significance at five percent.

TABLE 11

Estimates of γ: Low Income Sample

$$Y_i = \text{Demographic} + \gamma Y_{i-1} + \lambda X_{i-1} + \varepsilon_i$$

Measure of Children's Human Capital	OLS: X(i-1) not Included	OLS: X(i-1) Included	2SLS, X(i-1) INCLUDED: $\hat{\gamma}$	2SLS, X(i-1) INCLUDED: Wald Test	2SLS, X(i-1) INCLUDED: Hausman Test
Wages	0.248 *(0.049)	0.178 *(0.055)	0.328 *(0.148)	0.75	0.31
Skill Wages	0.216 *(0.045)	0.165 *(0.052)	0.177 (0.142)	0.76	0.03
Earnings	0.387 *(0.105)	0.369 *(0.116)	0.789 *(0.327)	0.02	0.20
Skill Earnings	0.367 *(0.100)	0.356 *(0.111)	0.694 *(0.320)	0.003	0.30
Education	0.916 *(0.434)	0.719 (0.432)	1.306 (0.849)	0.53	0.48

NOTES: This table presents estimates of the impact of total parental income on the human capital accumulation of children for a sample of low income families. Standard errors are in parentheses and are robust to heteroscedasticity of unknown form as well as arbitrary error covariance within families. The final two columns present the p-values from a Wald test of overidentifying restrictions and a Hausman test of the null hypothesis of exogeneity of parental income. A (*) denotes significance at five percent.