Rich Dad, Smart Dad: Decomposing the Intergenerational Transmission of Income[*]

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Abstract

We construct a simple model, consistent with Becker and Tomes (1979), that decomposes the intergenerational income elasticity into the causal effect of financial resources, the mechanistic transmission of human capital, and the role that human capital plays in the determination of fathers' permanent incomes. We show how a particular set of instrumental variables could separately identify the money and human capital transmission effects. Using data from a thirty-five percent sample of Swedish sons and their fathers, we show that only a minority of the intergenerational income elasticity can be plausibly attributed to the causal effect of fathers' financial resources.

I. Introduction

The persistence of income disparities is an important feature of economies that is not commonly captured by cross-sectional index measures of inequality. Accurate measures of this persistence could provide clues about the degree of social mobility and economic opportunity in a society. Thus, labor economists have long sought to obtain summary measures of the degree to which differences in earned income are transmitted across generations.

This effort has given rise to a large literature, reviewed in Björklund and Jäntti (2009) and earlier in Solon (1999), which measures the intergenerational relationship between the permanent incomes of fathers and sons. In this literature the most commonly used summary measure of the intergenerational persistence of incomes is the intergenerational income elasticity (IIE), which can be interpreted much like a correlation between the incomes of fathers and the incomes of sons. For example, an IIE of 0.30 suggests that a father's income ten percent above the paternal cohort mean will be associated with his son having an income three percent above the filial cohort mean.   Thus, larger values of the IIE imply a higher persistence for income differences.

Despite the considerable data demands faced by these studies, chief among them the need for longitudinal data that approximates permanent incomes across at least two generations, this literature has provided widely accepted estimates of the intergenerational persistence of income differences. However, this research agenda includes few empirical studies that provide insight into the structural mechanisms that underlie this income transmission. Hence, it remains unclear whether an IIE estimate should be interpreted as the causal effect of financial resources on child quality, some sort of mechanistic persistence, or something else entirely. Here, mechanistic persistence refers to the transmission of human capital that occurs as a consequence of being a father's son, independent of the level of financial investment. This includes the genetic transmission of attributes, the power of example, and at-home non-financial investments. Without a deeper understanding of the relative importance of transmission mechanisms, it is difficult to correctly anticipate the intergenerational effect of policies designed to redistribute income or to subsidize the acquisition of human capital.

In this paper, we develop an approach to identify the mechanisms through which the IIE operates. We show how this approach can be used to discern the relative importance of paternal income versus human capital (broadly defined as in the preceding paragraph) in intergenerational income mobility. We begin with a simple two-factor model (consistent with Becker and Tomes 1979) in which paternal human capital and financial investments are combined to produce child quality as measured by income. If we allow each of the two factors of production, parental income and parental human capital, to have independent marginal effects on the child quality output, then ordinary least squares (OLS) estimates of the IIE converge to a weighted combination of the two effects. The weight on each factor depends on the relative importance of idiosyncratic variation and human capital in determining paternal income. We further show how instrumental variables (IV) estimates of the IIE identify different combinations of the paternal human capital and financial resource effects with weights that depend on a particular instrument's covariance with paternal human capital and income due to idiosyncratic labor market variation.

This insight allows us to test the assumption that the IIE operates through both human capital and direct financial mechanisms by comparing IV and OLS estimates. More specifically, under the null hypothesis that the IIE operates through a single mechanism, that is one of our model factors has a marginal effect of zero, OLS and IV estimates (or any two IV estimates obtained from different correlates of paternal income) should be statistically indistinguishable. This null hypothesis will be rejected if the IIE operates through at least two mechanisms as suggested by our model. Furthermore, given an instrument which is correlated only to the idiosyncratic component of paternal income and another instrument which is correlated only to the human capital component, we can identify the structural parameters underlying the IIE in our model. Even if such instruments prove unobtainable, we show that IV estimation allows us to identify upper bound estimates of the role of financial resources and lower bound estimates of the importance of human capital using instrument sets derived from correlates for father's income that satisfy a simple monotonicity condition.

We also demonstrate how to bound the structural parameters in the absence of an instrument that effectively isolates variation in paternal income due to labor market idiosyncrasies. More specifically, identification is achieved with an instrument that isolates variation in paternal income due to human capital combined with an estimate of the fraction of the variance in paternal income attributable to human capital. The r-squared from a Mincerian regression of paternal permanent income on measures of human capital provides a lower bound for this latter quantity. This, in turn, provides an alternative method to identify a lower bound of the mechanistic impact of human capital and an upper bound of the causal effect of paternal income.

Using a large dataset of Swedish fathers and sons that provides thorough data on permanent incomes, we estimate an IIE of 0.29, consistent with prior estimates in the literature for Scandinavian countries. When paternal permanent income is instrumented with education or related measures designed to capture the influence of human capital the estimated IIE is much higher, exceeding 0.40. The statistically significant difference in estimates obtained from these distinct estimators allows us to reject the hypothesis that the correlation in intergenerational incomes operates entirely through one channel. Though it is more difficult to find an instrument that captures paternal income variation due to idiosyncratic factors, we present some candidates which provide significantly lower IIE estimates, as predicted by our model.

These results indicate that approximately one-third of the observed intergenerational income transmission is due to variations in monetary income. This implies that further policies to reduce intergenerational inequality through equalizing financial investments in the next generation will have, at most, modest success. Our alternative identification strategy, which relies on an estimate of a Mincer r-squared, yields looser bounds and therefore cannot rule out a somewhat larger role for the direct effect of financial resources. We show that these estimates are robust to alternative specifications and weighting schemes.

This paper proceeds with a description of how our study fits into the literature on intergenerational income inequality, the transfer of human capital, and the importance of parental financial resources. We then outline a simple model for the intergenerational transmission of income, which leads directly to our empirical strategy for identifying the structural parameters of the IIE. We follow by describing our data and then present our estimation results. We discuss threats to identification and present various robustness checks. We then conclude.

II. Literature Review

Björklund and Jäntti (2009) and Solon (1999) review a substantial number of articles that attempt to measure the cross-generational income correlation. Since these reviews provide a summary of a large body of work, we will mention only a few key findings that motivate our present study. First, this literature indicates that there exists substantial cross-country variability in IIE estimates, with suggestive groupings of countries that share similar estimated values. The U.S. estimates calculated by Mazumder (2005) are similar in magnitude to others obtained with American data such as Solon (1992) and Zimmerman (1992), as well as those from Italy (Piraino 2007) and France (LeFranc and Trannoy 2005). Other developed countries, however, tend to show much lower persistence in earnings. Indeed, estimates in Nordic countries tend to fall in the 0.2-0.3 range (see Björklund and Jäntti 1997, Pekkarinen et al. 2009, and Björklund et al. 2012).

The literature also suggests that IIE estimates are quite sensitive to poor measures of permanent income. In particular, measurement error in fathers' permanent incomes leads to attenuation bias in IIE estimates. Since averages over a larger number of periods are more precise proxies for permanent income, longer series longitudinal data on income is important to overcome this bias. For example, Mazumder (2005) reports IIE estimates as high as 0.61 for the United States when sixteen years of earnings are used to construct measures of father's permanent income. This number falls to 0.47 when only six years of earnings are averaged. Furthermore, even with long series of data, an unbiased estimate requires it to be drawn from the proper portion of the life-cycle. Hence, Gouskova et al. (2010) show that estimates of the IIE for the United States rise from 0.30 to 0.63 when life-cycle attenuation bias is removed.

Given the descriptive facts about intergenerational income transmission that continue to emerge from this literature, a complementary investigation would seek to determine the causal mechanisms through which this propagation occurs. Such mechanisms arise in the explicit economic models of intergenerational income correlation constructed by Becker and Tomes

(1979), Becker and Tomes (1986), Solon (2004), and Hassler et al. (2007).[1] Unfortunately, for reasons discussed by Goldberger (1989), Mulligan (1999), and Grawe (2004) these models have proved difficult to test in a convincing fashion. In response to these difficulties, several recent studies have opted for a quasi-experimental approach for testing single mechanisms in isolation. For example, Pekkarinen et al. (2009) provide evidence that the Finnish comprehensive school reform of 1972-1977 lowered the IIE by 23 percent.

While we are not aware of any previous empirical studies that have attempted a systematic structural decomposition of the role of human capital versus monetary resources, there is a separate literature that examines the intergenerational transmission of parental characteristics that might, in part, explain the transmission of income.[2] For example, Black, Devereux, and Salvanes (2009), and Björklund, Hederos-Eriksson, and Jäntti (2010) describe the intergenerational transmission of IQ scores. Behrman and Rosenzweig (2002), Black, Devereux, and Salvanes (2005), Oreopoulos, Page, and Stevens (2006) and Holmlund, Lindahl and Plug (2010) examine the parental transmission of education. Hauser and Logan (1992) discuss the transmission of occupational status.

Similarly, a number of researchers have attempted to identify the causal effect of parental income on a variety of child outcomes, including earnings. Examples include Mayer (1997), Blau (1999), Shea (2000), Oreopoulos, Page, and Stevens (2008), and Dahl and Lochner (forthcoming). While these studies each demonstrate a link between parental income and child

[1] For example, in Solon's (2004) version of the Becker and Tomes (1979) model, the IIE is a function of 4 factors: (1) the heritability of income-related traits, (2) the efficacy of human capital investment, (3) the earnings return to human capital, and (4) the progressivity of public investment in human capital. Structural models such as this one can help us to think more clearly about differences in the IIE across countries and across time. Solon's model also rationalizes the log-linear intergenerational income regression commonly estimated by empirical researchers.

[2] The term "decomposition" has been used by previous authors in this literature (e.g., Blanden et al., 2007). Their decomposition experiments, however, are not intended to be interpreted in a causal sense. In contrast to this, we use the term "structural decomposition" to describe an exercise that examines the relative importance of different causal mechanisms underlying the IIE.

outcomes, they disagree on the extent to which the raw correlation between parental income and child outcomes should be given a causal interpretation. In the extreme, Mazumder (2005) and Corcoran et al. (1992) find that paternal education has no independent correlation with son's earnings once one controls for an accurate measure of father's permanent income. In the context of our model, this finding can be interpreted as accepting a single factor model of intergenerational income transmission. Their results are also consistent with a relationship between paternal and filial income that primarily reflects the causal effect of financial resources.

Several studies within this strand of literature examine the impact of parents' income on childrens' outcomes when they are quite young, such as scholastic achievement tests and measures of behavioral development. Blau (1999) argues that these types of child outcomes do not correlate strongly with adult outcomes. Furthermore, such income induced gains may also be short-lived (Dahl and Lochner forthcoming). In contrast, our paper examines the impact of fathers' *permanent income* on sons' *permanent income*, an outcome of greater eventual importance. We can also measure permanent income quite accurately using Swedish income tax data. Other papers that look at earnings or family income of adult sons use much noisier measures of income, making it more difficult to draw strong inferences.

The broader idea of running a structural decomposition exercise is not unique to our study. Several researchers have used adoption data in order to gauge the relative importance of nature and nurture. The most notable of these studies is Björklund, Lindahl and Plug (2006). They use Swedish adoption data to decompose the IIE into pre- and post-birth factors. Pre-birth factors include parents' genes and pre-natal environment. Post-birth factors include everything else. They find that both factors play a significant role in producing the observed IIE.

Our examination of intergenerational income transmission mechanisms helps to connect these disparate literatures. We posit a statistical model consistent with Becker and Tomes (1979) in which financial resources may have a causal effect on child outcomes. We allow paternal characteristics such as education to have an independent effect on child quality. In this way we can not only look for the causal effect of parental income, but can also move beyond a quasi-experimental study of the effect of money to provide context about the relative importance of money compared to other parental factors. Our approach is quite general and can be readily modified to include (for example) the type of nature-nurture decomposition presented in adoption IIE studies. Furthermore, the model suggests a methodology for establishing bounds on the relative effects of money and human capital in common data environments where strong quasi-experimental instruments are unavailable. Relying on this simple model and a very rich dataset of father-son pairs from Sweden, we provide estimates of both the causal impact of financial resources and the direct rate of transmission of paternal human capital.

III. **Model**

When researchers measure the intergenerational transmission of income, they commonly estimate an equation of the following form:

(1) $$inc_{son} = \beta_0 + \beta_1 inc_{father} + \varepsilon_{son},$$

where inc_{son} and inc_{father} are the natural logarithm of income for the son and father respectively and ε_{son} is a residual. Although empirical researchers typically make no claims regarding the causality of this relationship, it is useful to consider what structural parameters it actually captures. To see this, consider a slightly more complex model.

Suppose fathers differ in terms of human capital and income. Paternal income is a function of a father's human capital and other idiosyncratic factors. We write this relationship as:

(2) $$inc_{father} = \gamma + HC_{father} + \eta_{father} .$$

In the current context, human capital, HC_{father}, consists of education, health, and genetic endowments that carry a return in the marketplace and is denominated in dollar equivalents. More broadly, human capital includes any factor that would affect paternal income across a large number of counterfactual labor market realizations. For example, a father with more education would tend to earn higher wages in a broad variety of time periods and economic circumstances. On the other hand, η_{father} captures variation in paternal income due to idiosyncratic factors that are orthogonal to human capital and specific to a particular labor market realization. For example, a father might have benefited from a generous union contract and thus enjoyed a high realization of η_{father}. In an alternative career or time period, the father could not have expected the same favorable income shock. Other examples include an unusually good or bad job match or working at a firm that goes out of business. Given our broad view of human capital, these idiosyncratic factors are essentially a residual and thus the independence assumption is not unduly restrictive. For convenience, we'll refer to η_{father} as luck.

Fathers are interested in producing high quality sons as measured by income. A son's income is generated in the same fashion as his father's income. More specifically:

(3) $$inc_{son} = \gamma + HC_{son} + \eta_{son} .$$

Fathers increase their sons' human capital by direct transmission of their own human capital and through financial investments. Consequently,

(4) $$HC_{son} = \alpha + \pi_1 inc_{father} + \pi_2 HC_{father} + \varphi_{son},$$

where φ_{son} is the idiosyncratic component of a son's human capital which is unrelated to his father's income or human capital. Substituting equation (4) into equation (3), we can write the son's income as a function of paternal income and human capital.

(5) $$inc_{son} = \pi_0 + \pi_1 inc_{father} + \pi_2 HC_{father} + \nu_{son}.$$

Note that $\pi_0 = \alpha + \gamma$ and $\nu_{son} = \varphi_{son} + \eta_{son}$.

Equation (5) is identical to that derived in Becker and Tomes (1979) when one abstracts from the impact of average societal human capital. Thus, given the institutional environment of a particular country, each of the model parameters can be given a structural interpretation. More specifically, π_1 corresponds to the fraction of income invested in child quality multiplied by the efficacy of this investment.[3] Meanwhile π_2 captures the degree to which human capital is directly transferable to children, while allowing such transfers to be offset by reductions in financial investment.

While our empirical specification can be rationalized by the Becker and Tomes (1979) framework, it does not represent a test or validation of their theory. Even if the strong functional form assumptions of their model do not hold, equation (5) can be viewed as a linear approximation of a more complex behavioral or production model. Alternatively, π_1 and π_2 could represent mechanistic pathways that do not necessarily reflect optimizing behavior.[4] In

[3] In Becker and Tomes (1979) the fraction of income spent on investment in children is pegged by preference parameters of a Cobb-Douglas utility function. The efficacy of investments in equilibrium is determined by the market rate of interest.

[4] Goldberger (1989) and Mulligan (1999) point out the difficulties of determining whether the intergenerational correlation in income reflects optimizing behavior or a mechanistic convergence to the mean posited by Galton in the nineteenth century.

either case, our approach will be helpful in identifying the relative importance of paternal income and human capital in explaining the intergenerational income elasticity.

Given this assumed data generating process, it seems reasonable to simply estimate equation (5) by regressing sons' income on fathers' permanent income and proxies for fathers' human capital. Regressions by Corcoran et al. (1992) and Mazumder (2005), in which sons' income is regressed on fathers' permanent income and education, can be interpreted as attempts to proceed in this direction. This approach can be framed as an attempt to control for unobserved parental human capital in order to identify a causal effect of monetary resources. The insignificant relationships they find between fathers' education and sons' income, conditional on fathers' income might be seen as rejecting a role for fathers' human capital in intergenerational income transmission outside of increasing available financial resources.

However, because education and other available measures of human capital capture only a minority of the total variation in human capital, examining the estimated coefficients provides only a weak test of the one-factor model. Furthermore, since the probability limits of the reduced form regression coefficients are very difficult to interpret, it is unclear exactly what this methodology is measuring. In the context of our model, the estimated intergenerational income elasticity resulting from this regression is an upper bound to π_1 if all included covariates are correlated to paternal human capital and uncorrelated to paternal luck.

This is a very important qualification. If any of the included control variables is correlated with the idiosyncratic income component the resulting estimate is structurally uninterpretable, as it fails to converge to a causal effect of monetary resources or any other desired parameter. Even if sufficiently good proxies for human capital are available to identify the magnitude of the financial mechanism, π_1, it would still be unclear how the coefficients on the various human capital measures relate to the magnitude of the other mechanism, π_2.

Moving forward with an alternative approach, we substitute equation (2) into equation (5) to obtain the following expression:

(6) $$inc_{son} = \pi_0 + \pi_1\gamma + \left(\pi_1 + \pi_2\right) HC_{father} + \pi_1\eta_{father} + \nu_{son}.$$

This equation captures the intuition that paternal human capital can affect child quality through an increase in financial investment as measured by π_1 and directly through π_2. The component of a father's income that is particular to a certain labor market realization, which we refer to as luck, affects his son's income only through increased financial investment.

Given this model, the OLS slope estimator for equation (1), $\hat{\beta}_1^{OLS}$ converges to:

(7) $$plim\left(\hat{\beta}_1^{OLS}\right) = \pi_1 + \pi_2 \frac{\operatorname{var}\left(HC_{father}\right)}{\operatorname{var}\left(HC_{father}\right) + \operatorname{var}\left(\eta_{father}\right)}.$$

Note that the first term captures the impact of paternal income, holding constant human capital. The second term captures the impact of paternal human capital on son's income. The final expression, $\operatorname{var}\left(HC_{father}\right) \big/ \left(\operatorname{var}\left(HC_{father}\right) + \operatorname{var}\left(\eta_{father}\right)\right)$, is the fraction of variance in father's income attributable to human capital variation. The key insight is that if variation in paternal income is due primarily to luck, then $\hat{\beta}_1^{OLS}$ reflects primarily financial investments in child quality. On the other hand, if variation in paternal income is primarily due to differences in human capital, $\hat{\beta}_1^{OLS}$ will also reflect the direct impact of father's human capital on child quality. Thus the structural interpretation of any OLS estimate depends crucially on the source of income variation for fathers in that particular study.

Because each source of paternal income variation has a different implication for filial income, alternative identification strategies have the potential to shed more light on the mechanisms underlying the intergenerational transmission of income. In particular, suppose there exists a correlate of paternal income, Z_{father}. Using this variable as an instrument for paternal income to identify the intergenerational correlation of income relationship found in equation (1) yields the following probability limit:

$$\text{(8)} \qquad plim\left(\hat{\beta}_1^{IV}\right) = \pi_1 + \pi_2 \frac{\text{cov}\left(HC_{father}, Z_{father}\right)}{\text{cov}\left(HC_{father}, Z_{father}\right) + \text{cov}\left(\eta_{father}, Z_{father}\right)}.$$

Like the OLS estimate, $\hat{\beta}_1^{IV}$ reflects the impact of paternal income operating through financial investments, π_1. The second term of the IV estimator takes into account the direct effect of paternal human capital on child quality. The ratio in the final expression represents the proportion of the covariance between income and the instrument that is attributable to human capital. It follows that each instrument, depending on its covariance with luck and human capital, identifies a potentially different weighted combination of the model parameters. The intuition behind this observation is similar to that underlying the local average treatment effect (LATE) literature (e.g. Imbens and Angrist 1994).

This suggests that the properties of multiple estimates can be leveraged to reveal information about the structural parameters of our model. For example, the above equations imply that $plim\left(\hat{\beta}_1^{IV}\right) = plim\left(\hat{\beta}_1^{OLS}\right)$ if and only if $\pi_2 = 0$ or

$$\text{(9)} \qquad \frac{\text{cov}\left(HC_{father}, Z_{father}\right)}{\text{cov}\left(HC_{father}, Z_{father}\right) + \text{cov}\left(\eta_{father}, Z_{father}\right)} = \frac{\text{var}\left(HC_{father}\right)}{\text{var}\left(HC_{father}\right) + \text{var}\left(\eta_{father}\right)}.$$

Since equation (9) does not hold generally, a significant difference between OLS and IV estimates implies that $\pi_2 \neq 0$. Thus, a simple Hausman specification test can determine if the transmission of income comes partially through a genetically or environmentally mediated direct parental human capital effect, rather than exclusively through the investment of additional financial resources. By the same token, unless $\pi_2 = 0$, any two instruments should yield a different value for the intergenerational income elasticity as long as they differ in their relative covariance with luck and human capital. Thus, rejecting a test of overidentifying restrictions in an IV context allows us to conclude that financial investments are not the sole mechanism through which income is transmitted from generation to generation.

This approach of testing for the presence of a second factor overcomes the difficulties in the OLS approach of Corcoran et al. (1992) and Mazumder (2005) mentioned earlier. Because the human capital variables are used as instruments rather than controls, they do not have to capture the entirety of human capital variation or be completely orthogonal to the idiosyncratic income component. More generally, a comparison of OLS and IV estimates tells whether there is a single mechanism that drives the paternal-filial income correlation as opposed to multiple mechanisms. If a Hausman test is rejected, one can infer that multiple mechanisms are at work. Solon (1992) documents the difference between OLS and IV estimates of the intergenerational income elasticity. However, rather than seeing this as evidence of a model specification test, he views the IV estimate as upwardly biased. But, bias can only be determined in the context of the structural parameters under investigation. Since the OLS estimates are not structural, departures from them do not have to be a form of bias.

Beyond its implications in testing for multiple transmission mechanisms, our model implies that a combination of suitable instruments could be used to estimate the magnitudes of direct human capital and resource investment effects. Suppose that there exists an instrument that is related only to the idiosyncratic luck component of paternal income. Since $\hat{\beta}_1^{IV}$ then converges to π_1, the pure impact of a father's financial resources, researchers (e.g. Shea 2000, Dahl and Lochner forthcoming) have used such a single IV strategy in attempts to identify the causal impact of financial resources on child outcomes. Alternatively, an instrument that is correlated only to paternal human capital and not luck will yield an estimate that converges to $\pi_1 + \pi_2$, the impact of financial resources plus the impact of paternal human capital (where human capital is denominated in dollars). Thus, a direct comparison of the two estimates allows for the separate identification of the two structural mechanisms. An instrumental variables approach also has the advantage that it is robust to imperfect measures of permanent income as long as the instruments themselves are orthogonal to transitory fluctuations.

Even in the absence of two such ideal instruments, this methodology allows us to establish bounds on the structural effects. Consider any instrument that satisfies the monotonicity condition that $\text{cov}(HC_{father}, Z_{father})$ and $\text{cov}(\eta_{father}, Z_{father})$ have the same sign. If this condition holds, the probability limit of the resulting IV estimate will necessarily lie in-between π_1 and $\pi_1 + \pi_2$. Thus, abstracting from estimation error and assuming $\pi_2 \geq 0$, the minimum estimate from using an arbitrary set of instruments yields an upper bound for π_1. While the maximum estimate yields a lower bound for $\pi_1 + \pi_2$. Subtracting the minimum estimate from the maximum estimate yields a lower bound of π_2. If instruments are available such that π_1 and π_2 are identified, or at least closely bounded, one can back out the fraction of variance in paternal income due to human capital from $\hat{\beta}_1^{OLS}$.

In practice, due to finite data, each IV estimate combines the probability limit with estimation error. As a result, the probability limit of the largest IV estimate will be, on average, smaller than the estimated coefficient, with the opposite holding true for the smallest IV estimate. Bounding on the basis of the highest and lowest measured IV estimates is thus likely to lead to misleading bounds. One can address this by identifying ex-ante "luck" instruments, which are likely to have a relatively stronger relationship with η_{father} than HC_{father} and "human capital" instruments that behave in the opposite manner. One then simply uses the "luck" instrument to identify the upper bound of π_1 and the "human capital" instrument to identify the lower bound of $\pi_1 + \pi_2$. The monotonicity condition must still hold.

A second bounding procedure is possible using only measures of human capital. More specifically, suppose we have a set of instruments, Z_{father}^{hc}, which are correlated to paternal human capital but uncorrelated to luck. Under this assumption, instrumental variables estimation of equation (1), $\hat{\beta}_1^{IV}$, identifies $\pi_1 + \pi_2$. The corresponding OLS estimate, $\hat{\beta}_1^{OLS}$ still converges to the expression shown in equation (7). An estimate of $\operatorname{var}\left(HC_{father}\right) \Big/ \left(\operatorname{var}\left(HC_{father}\right) + \operatorname{var}\left(\eta_{father}\right)\right)$ then allows us to recover π_1 and π_2. Since by assumption Z_{father}^{hc} only affects paternal income through human capital, the r-squared of the Mincerian regression of paternal earnings on Z_{father}^{hc} yields a lower bound of $\operatorname{var}\left(HC_{father}\right) \Big/ \left(\operatorname{var}\left(HC_{father}\right) + \operatorname{var}\left(\eta_{father}\right)\right)$. This lower bound in conjunction with our OLS and IV estimates of the intergenerational income elasticity allow us to estimate a lower bound of π_2 and an upper bound of π_1.

IV. Data

A. Data Sources and Description

Our empirical analysis is based on data taken from a thirty-five percent sample of sons born in Sweden between 1950 and 1965 drawn from Statistic Sweden's multigenerational register (which covers all persons who were born in Sweden from 1932 onwards and have lived in Sweden at any time since 1961). Nearly all biological and adoptive parents of these sons are identified in this data set. The identification rate of fathers rises from 95 percent for those sons born in 1950 to 98 percent for those sons born in 1965. The multigenerational register also includes information on the year of birth and death (when applicable) of each individual. The register sample is then matched with data from the official Swedish tax register. We use data on income from all sources, or pre-tax total factor income, which is available from 1968 to 2005 to construct our main income measure for both fathers and sons.[5]

Our research design takes advantage of a number of potential correlates to fathers' income including education, occupation, and employment status. These variables are likely related to a range of possible effects on a father's human capital or on the idiosyncratic component of his income. The use of these variables as instruments will be discussed in more detail below.

Fathers' educational attainment is measured in 7 levels: (1) less than 9 years of primary, (2) completed 9 years of primary, (3) at most 2 years of secondary, (4) 2 to 3 years of secondary, (5) less than 3 years of upper secondary, (6) at least 3 years of upper secondary school, and (7) graduate studies. Most of this information has been taken from Sweden's national education

[5] The definition of this income measure changed in 1974 to include some social benefits, most notably unemployment compensation and illness benefits. Parental leave benefits were also included but were almost exclusively used by mothers. We have direct measures of these benefits for the 1974 to 1980 period, which we use to gauge the sensitivity of our estimates to their inclusion. We also include father birth-year dummies to control for this and other cohort-specific and/or time varying effects.

register for the year 1990. If a father's education was missing in this primary source, then secondary sources were searched. This was done in the following order: the national education registers for 1993, 1996 and 1999 and, finally, the 1970 Census.[6] Swedish Census data have also been used to identify a father's municipality of residence,[7] his occupation, and his employment status for the years 1960, 1965, 1970, 1975, 1980, 1985 and 1990.[8]

B. Measuring Permanent Income

To estimate the intergenerational income elasticity (IIE) posited in equation (1), we need measures of permanent income for fathers and sons. Our data, however, do not allow us to calculate actual permanent incomes for all fathers and sons. Instead, we are forced to use a proxy for permanent income.

Two main obstacles to constructing a high quality proxy for fathers' permanent income have been identified in the previous literature. The first is the presence of transitory income shocks in the data. This is likely to attenuate IIE estimates unless the proxy is constructed using a large number of years of fathers' income data (Solon 1992, Mazumder 2005). The second obstacle arises from the heterogeneity of life-cycle income profiles (Haider and Solon 2006, Grawe 2006). In short, this literature tells us that fathers' incomes must be observed in the correct age range to capture accurate measures of differences in permanent incomes across

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[6] 20 percent of the data on fathers' education come from the 1970 census. Nearly all of the remaining information comes from the 1990 national education register. Information concerning education could not be found for 11 percent of the fathers in the full sample. But less than 0.5 percent is missing in our baseline sample.

[7] Between 1962 and 1974, Sweden reduced the number of municipalities from 1037 to 278. After 1974, this number was allowed to rise. Today Sweden is comprised of 290 municipalities.

[8] Although all censuses report some measure of employment, the employment status variables change from one census to the next. Employment status is coded from 0-9 in 1960, from 0-5 in 1965, 1-9 in 1970, 1975 and 1980 and 1-4 in 1985 and 1990. These differences are largely due to evolving approaches to measure part-time employment. Note also that there is no information on occupation in the 1965 census.

individuals or groups. The problem of life-cycle bias also applies to our proxy of sons' permanent income, since it is a form of non-classical measurement error.

For our fathers, Böhlmark and Lindquist (2006) suggest that income measured after age 33 may act as a good proxy of permanent income. For our sons born in 1950, they tell us to look at a specific age, namely age 34. But since our sons are born between 1950 and 1965 and have (on average) more education than those studied by Böhlmark and Lindquist (2006), we choose to shift this age upwards by one year to age 35.

Our proxy for the permanent income of sons is calculated as follows. We use 11 years of income data for each son centered on age 35, i.e., from age 30 to age 40.[9] Nominal income is deflated using the Swedish consumer price index. We use the natural logarithm of an average of real income taken across these periods. Zero incomes and missing incomes are both treated as missing. A similar procedure is used to calculate the permanent income of fathers. The only difference is that fathers' income is measured between ages 30 and 60. We argue that this proxy of fathers' permanent income is a high quality measure of permanent income that is largely free from both life-cycle bias and attenuation bias.[10]

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[9] For sons born between 1950 and 1955 we can test to see if our proxy for sons' permanent income is free from life-cycle bias. We do this by re-calculating log average income for each son using income data from age 20 to 50. This longer time series of income should provide us with a relatively good measure of permanent income for these cohorts (or, at the very least, an improved proxy for permanent income). We then regress our initial proxy onto this new measure of permanent income. An OLS coefficient of 1 indicates no life-cycle bias (see Haider and Solon 2006). Unfortunately, we only have 32 sons in our sample of father-son pairs that are born between 1950 and 1955. We, therefore, turn to our original full sample of unmatched individuals in order to run this sensitivity analysis. In our full sample, we have 111,234 men born between 1950 and 1955. Regressing our initial proxy onto this new measure of permanent income produces an OLS regression coefficient equal to 1.01 (0.002), which implies that our 11-year proxy of sons' permanent income is largely free from life-cycle bias (at least for these cohorts). Unfortunately, we cannot do this for our younger cohorts, because we simply don't have enough information concerning their incomes above age 40.

[10] Mazumder (2005) argues that averaging over 30 years of income largely eliminates attenuation bias unless transitory shocks demonstrate a very strong degree of autocorrelation. In this case, the reliability ratio may be as low as 0.9 even after averaging over 30 years of income. If we drop our demand of observing all fathers in the same age window (30 to 60), then we can average their incomes over 38 years as opposed to 31 years. The reliability ratio

Constructing good proxies of permanent income demands a lot from the data. Our sample restrictions are implemented in the following manner. The original 35 percent probability sample of sons born between 1950 and 1965 contains information on 309,869 sons. Of these, 6,728 sons have unidentified fathers, which decreases our sample to 303,141 father-son pairs. Since our proxy for fathers' permanent income requires income data between the ages of 30 and 60, and since we have income data for the years 1968 to 2005, our fathers must be born between 1938 and 1945. This reduces our sample size to 30,353 father-son pairs. To be included in the sample, we require at least 10 years of non-missing observations of income within the correct age window. Implementing this requirement for fathers further reduces the sample to 28,896 pairs. Placing the same requirement on sons' income reduces the sample to 26,030 pairs. Lastly, we require that fathers' education and employment status be non-missing, which gives us our final, baseline sample of 24,114 father-son pairs. Descriptive statistics for fathers' and sons' permanent income used in our baseline estimation can be seen in Table 1.

In Table 2, we compare our baseline sample with the full sample along several dimensions. Income should differ between the two groups by construction, since individuals are dropped from our sample when we do not have a sufficient number of income observations for them within the correct age span. Despite this, the measured incomes differ only slightly between the two samples.

The average number of years of schooling obtained by our fathers is 0.33 years higher than for fathers in the full sample. This is mainly due to the fact that the median birth year of our fathers is 1940, while the median birth year of the fathers in the full sample is 1927. Most of our fathers faced an educational system with 9 years of compulsory schooling as opposed to the 7-

calculated as the r-squared from a regression of our 31-year average onto this new 38-year average (called full-data income father in Table 2) is equal to 0.98. The OLS coefficient is equal to 0.99 (0.001).

year system faced by those who were born before 1938. The median birth year of our sons is 1964. The median in the full sample is 1958. Taken together, these differences produce an average age difference between fathers and sons of 31.37 in the full sample and only 22.79 in our sample. This age difference is driven mainly by the fact that our selection rules have matched fathers to their first-born sons only. [11]

C. Our Instruments

Our strategy for estimating the structural parameters of our model of intergenerational income mobility is based on the idea that different sources of paternal income have different implications for filial income. In our model, income derived solely from luck identifies the direct effect that paternal income has on filial income, while income derived solely from fathers' human capital identifies the total effect that fathers' human capital has on their sons' incomes. In the absence of perfect instruments for luck and human capital, our strategy for bounding the structural parameters of the model entails investigating differences in a set of estimates of the IIE produced using an array of different instruments for fathers' permanent income. The only demands that we place on our instruments is that they satisfy the monotonicity condition stated earlier and that they be correlated with luck and human capital to varying degrees. In this manner, different estimates of the IIE will be identified using different sources of variation in fathers' permanent incomes that are more or less related to luck or to human capital.

Our instruments include fathers' level of education, years of schooling and occupation. We use paternal occupation in 1970, 1975, 1980, 1985 and 1990, which coincide with our

[11] We have run an alternative experiment that used only 11 income years for fathers. The sample of father-son pairs used in this experiment rose to 132,210. The average age difference was 26.49 years. The estimated IIE was only slightly lower than our baseline IIE.

income data. Our priors are that these instruments should be highly correlated with fathers' human capital. As instruments for luck, we wish to use instruments based on paternal employment status in 1975, 1980, 1985 and 1990. However, employment status may also reflect a father's human capital levels and other systematic factors. To deal with this possibility we first regress employment status on the past education and earnings history of the father and use the residuals, purged of human capital influence, as our instruments. While imperfect, this plausibly captures the loss of income due to bad "luck". [12]

V. Results

A. Estimates

Estimates of the father-son intergenerational income elasticity (IIE) are presented in Table 3. The reported standard errors come from a bootstrap procedure that accounts for clustering at the father level (n=100). Our baseline IIE is shown in column 1 of Table 3. The point estimate is 0.286 with a standard error of 0.010. Comparable estimates of the father-son IIE for Sweden can be found in Björklund and Jäntti (1997) and Björklund et al. (2012).

We now turn our attention to IV estimates of the IIE, which are also reported in Table 3. Recall that each estimate corresponds to a different combination of the impact of financial resources and the mechanistic transmission of human capital. We begin by examining the IIE when we instrument father's permanent income with years of education (column 2) and dummy variables for educational category attained (column 3). The resulting point estimates are virtually identical at 0.416 and 0.414, respectively. Both estimates are significantly higher than

[12] We also experimented with instruments based on municipality of residence at early points in the father's work history or municipality interacted with birth cohort to try and capture random locale shocks that were due to "luck." While these also produce low point estimates they are insufficiently precise to warrant any substantive conclusions.

our baseline OLS estimates, suggesting that we should reject a one-factor model of intergenerational income transmission. Furthermore, since these instruments plausibly isolate variation in paternal income associated with human capital, these IV estimates can also be used as plausible estimates of the parameters $\pi_1 + \pi_2$ in our model.

The next regressions use instruments for paternal income based on measures of fathers' occupations. In column 4, we instrument using the cell mean of permanent income of fathers with the same occupation in 1970. This corresponds to the early part of the fathers' careers. We expect that initial choice of occupation is largely a reflection of human capital.[13] Of course, to the extent that occupational wage differentials reflect job amenities or efficiency wages, occupational wages may also reflect variation in luck (in the context of our model), but we expect that to be a minor factor. Indeed, the resulting the IV estimate is 0.40, very similar to the results observed with the paternal education instruments. Once again, the human capital instrument produces a significantly different estimate than OLS.

In column 5, we expand our time frame and construct instruments using the cell mean of fathers' permanent income for each observed occupation in 1970, 1975, 1980, 1985, and 1990. This takes into account occupational transitions that may be associated with either human capital or luck. The resulting estimate is 0.335, lower than the estimate associated with initial occupation but still higher than the OLS estimates.[14]

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[13] Neal (1998) presents a model along with empirical evidence suggesting that highly skilled individuals sort to specialized occupations because they enjoy large rents to their occupation specific human capital.

[14] Neal (1999) present models in which wages disperse over time as workers learn more about their skill set and sort to jobs that are good fits for them. This would suggest that mid-career occupation would be a better proxy for human capital than early-career occupation. This is inconsistent with the relatively low IIE we estimate when instrumenting paternal with occupation throughout the father's career. While revelation about an individual's human capital is one source of occupational change, another source is the demand for particular occupations and skill sets, which would reflect luck in the context of our model. For example, contractions or expansions in the demand for particular occupations would affect job changes for reasons unrelated to human capital. Similarly, the

Our next set of IV specifications relies on the employment status of fathers, which we observe every five years. We begin our examination in 1970 as prior to this many fathers in the sample have not yet finished their schooling. In column 6, we instrument father's permanent income with employment status dummies from all of the periods (1970-1990) simultaneously. This produces an estimate of 0.205, significantly lower than the OLS baseline.

During the working lives of fathers in our sample, Sweden had very low unemployment. As a consequence, those fathers who we observe unemployed may have had particularly low human capital or attachment to the labor force. To construct an instrument which more effectively isolates the employment variation attributable to luck, we orthogonalize fathers' employment status in a particular period against years of schooling and earnings up to the reference date. In column 7, we repeat our analysis using the residual measure of employment status in each time period after 1970. As expected, once purged of human capital effects, our estimates are even lower, albeit less precise, with a point estimate of 0.106 with a standard error of 0.065.

B. Decomposing the IIE

In our model, the observed OLS IIE is a function of three parameters: the causal impact of financial resources on child outcomes (π_1), the mechanistic transmission of human capital (π_2), and the fraction of variance of paternal permanent income explained by human capital (R^2). Given that our years of schooling variable induces variation in father's permanent income

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timing of colleague retirements could affect possibilities for promotion (and job change) that are also unrelated to a father's human capital.

only on account of human capital, we have a consistent estimate of $\pi_1 + \pi_2$. To fully identify all parameters of the model, we need additional information regarding either π_1 or R^2.

In order to capture a true estimate of π_1, we would need an instrument that induces variation in parental income solely due to luck. Obviously, a perfect instrument that captures only luck yet is sufficiently prevalent to induce enough variation to provide precise estimates is difficult to find. Indeed, if it were readily available, instrumental variables estimates settling the question of how much money matters would be ubiquitous. Fortunately, our model suggests that an imperfect luck instrument may still allow us to make progress in the decomposition by providing an upper bound for π_1. While we might argue that the residuals based on employment status are good luck instruments, it is more important that they provide an upper bound.

Thus, for the purposes of this specification, our estimate of π_1 is identified by the IV estimate of the impact of paternal income using the employment residual instruments, $\hat{\beta}_1^{IV\ EmpResid}$. Further, $\pi_1 + \pi_2$ is identified by the IV estimate that uses dummy variables for educational attainment as instruments, $\hat{\beta}_1^{IV\ EdCat}$. These, in conjunction with the OLS estimate of the IIE, $\hat{\beta}_1^{OLS}$, allow us to estimate $\pi_2 = \hat{\beta}_1^{IV\ Ed\,Cat} - \hat{\beta}_1^{IV\ EmpResid}$ and the fraction of variance in permanent income attributable to human capital, $R^2 = \left(\hat{\beta}_1^{OLS} - \hat{\beta}_1^{IV\ EmpResid}\right) / \left(\hat{\beta}_1^{IV\ Ed\,Cat} - \hat{\beta}_1^{IV\ EmpResid}\right)$. These results are reported in the first specification of Table 4. We note that the implied causal effect of paternal permanent income on the next generation's incomes is bounded from above by 0.11, making 0.31 a lower bound of the mechanistic impact of human capital on filial income. These estimates further imply that 58 percent of the variation in paternal income is attributable to human capital. This bounding exercise would suggest that 37 percent of the IIE reflects the causal effect of financial resources, while the balance captures the mechanistic impact of human capital.

As an alternative, we can identify the model using a credible estimate of the fraction of the father's income variation that is due to human capital, in other words the r-squared from a correctly specified Mincer regression. Of course, we are unlikely to observe all aspects of paternal human capital, so the observed Mincer r-squared is likely to represent a lower bound to the truth. We first calculate this r-squared using our baseline data set. We regress paternal permanent income on dummy variables for educational attainment and 1980 occupation.[15] The adjusted r-squared from this regression is 0.376. Using $\hat{\beta}_1^{IV\ EdCat}$ as our estimate of $\pi_1+\pi_2$, we calculate $\pi_1 = \left(\hat{\beta}_1^{OLS} - \hat{R}^2\hat{\beta}_1^{IV\ Ed\ Cat}\right)/\left(1-\hat{R}^2\right)$ and $\pi_2 = \left(\hat{\beta}_1^{IV\ Ed\ Cat} - \hat{\beta}_1^{OLS}\right)/\left(1-\hat{R}^2\right)$. The estimates are shown in the second specification of Table 4. In this case, our upper bound estimate of π_1 is 0.21 and our lower bound estimate of π_2 is 0.21. This implies that the causal effect of financial resources accounts for nearly three-quarters of the intergenerational income elasticity. The benefit of additional human capital on son's earnings operates about equally through mechanistic and financial channels.

Of course, each possible r-squared value implies different values for the other parameters. Figure 1 shows the implied causal effect of financial resources associated with each possible r-squared measure. These estimates are computed in a manner identical to the prior paragraph. We see that if the true r-squared value is low, the majority of the correlation between father's and son's income operates through a causal money effect. As the r-squared rises, the implied effect

[15] Occupation may reflect a realized favorable employment outcome in addition to human capital. This raises the possibility that the r-squared need not be a lower bound of the impact of human capital on fathers' permanent incomes. However, the fact that occupation dummies yield IV estimates of the impact of paternal human capital that are similar to our education dummies suggests that they primarily reflect human capital.

of financial resources falls. For a Mincerian r-squared of about 0.7, the implied causal effect of financial resources is zero.

C. Robustness Checks

Income Measurement and Spatial Correlations

There are a number of potential concerns regarding the validity and interpretation of our estimates. One concern is that our measure of permanent income is the log of the average of yearly incomes. Many prior researchers average log yearly income. This places greater weight on periods of low income relative to our analysis. In Table 5, we reprise our estimates and decomposition using this alternative measure of fathers' and sons' income. When we use the average of log income, the results are qualitatively similar though the causal effect of fathers' income, π_1, is a somewhat larger component of the IIE.

An additional concern is that sons have a strong tendency to live and work in the same cities or regions as their fathers. These father-son spatial correlations could lead to spurious father-son correlations in education and choice of occupation. This spatial pattern also exposes fathers and sons to similar market trends and market fluctuations. Furthermore, prices and wages tend to differ across regions and are typically higher in big cities than in rural areas. Regional price and wage differences, together with father-son spatial correlations, may lead to spurious correlations in income. On the other hand, a son living in the countryside may have the same level of *real* income as his big city father, despite the fact that the father has a higher nominal wage, since the prices of housing and other commodities tend to be lower in non-urban areas. This phenomenon would lead us to underestimate the IIE if what we are truly interested in is the association in economic status as measured by lifetime consumption (real purchasing power).

We have run an extensive set of robustness checks addressing these issues in order to gauge the extent to which such spatial mechanisms affect our estimate of the IIE and the interpretation of our two-factor model.[16] We find that controlling for spatial correlations alone does not affect our estimated IIE in any significant manner, nor does it change the conclusions of our IV bounding and decomposition exercise. In fact, any deviations from the aggregate IIE are being driven by the minority of fathers and sons living in different regions, in particular urban fathers with non-urban sons tend to have a lower IIE. The IIE for Urban father-son pairs and non-urban father-son pairs do not differ from our baseline estimates.

However, as reported in Table 6, deflating annual incomes using regional housing price indices (instead of the national consumer price index), produces an estimate of the IIE which is significantly higher (0.320) than our baseline IIE (0.286). Our decomposition results under this alternative actually produce a tighter upper bound of 12% on the impact of father's income on the IIE due to a new π_1 estimate of 0.039. We note, however, that this new estimate of π_1 is not statistically significantly lower than our baseline estimate. Furthermore, differences in housing prices across regions may overestimate the true regional price differences of a typical consumption bundle. Thus, the "true" regional price adjusted IIE most likely lies somewhere in-between our baseline estimate of 0.286 and the housing price adjusted IIE of 0.320.

Model Non-linearities

Another potential concern is that the identification of the independent effects of human capital and financial resources depends on the linearity of our model. One simple test of this assumption is to examine whether the observed relationship between father's and son's income is

[16] The full analysis is available from the authors on request.

linear. If we make the ancillary assumption that paternal luck and human capital are both normally distributed in the population, our data generating process, which assumes a linear relationship between a son's income and a father's human capital and luck, also implies a linear relationship between the father's and son's incomes. This is due to the conditional expectations of both father's luck and human capital being linear in our measure of father's permanent income (in our case the log of father's permanent income). Consequently, our model predicts the following linear relationship:

$$(10) \qquad inc_{son} = \pi_0 + \left(\pi_1 + \pi_2 \frac{\mathrm{var}\left(HC_{father}\right)}{\mathrm{var}\left(HC_{father}\right) + \mathrm{var}\left(\eta_{father}\right)} \right) inc_{father} + \varepsilon_{son} .$$

In Figure (2) we show the relationship between paternal and filial income in our data once we have purged both of year of birth effects. We estimate this relationship using a lowess procedure with a bandwidth of 0.8. The figure shows an apparently convex relationship. This could be due to a non-linear relationship between sons' income and fathers' luck and human capital. Alternatively, it might indicate that the distribution of luck and human capital is not normal. In Figure (3) we show the corresponding relationship when we drop the bottom and top 1 percent of observations. With this minor modification, the relationship appears quite close to linear. While a quadratic term is still statistically significant, it adds very little additional explanatory power—the r-squared increases only from 0.078 to 0.080. This suggests that the assumption of linearity may not be meaningfully violated. It also matches well with the observation of Björklund et al. (2012) that the IIE for Sweden is linear once you eliminate the top one percent of incomes.

Unfortunately, this simple linearity test depends on the normality assumption for human capital and luck. Consequently, it would be helpful to perform additional tests that lack this sensitivity. Consider the following generalized model:

(11) $$inc_{son} = f\left(inc_{father}, HC_{father}\right) + \nu_{son}.$$

Maintaining, as in equation (2), that father's income is the sum of HC_{father} and η_{father} along with a constant we can rewrite this as:

(12) $$inc_{son} = f\left(HC_{father} + \eta_{father}, HC_{father}\right) + \nu_{son}.$$

Next, we take a first order approximation of this function around values HC^0_{father} and η^0_{father}. This produces the following generalization of equation (6), where φ_{father} represents the approximation error:

(13) $$\begin{aligned} inc_{son} &= f\left(HC^0_{father} + \eta^0_{father}, HC^0_{father}\right) + \\ &\left[\frac{\delta f\left(HC^0_{father} + \eta^0_{father}, HC^0_{father}\right)}{\delta inc_{father}} + \frac{\delta f\left(HC^0_{father} + \eta^0_{father}, HC^0_{father}\right)}{\delta HC_{father}}\right]\left(HC_{father} - HC^0_{father}\right) + \\ &\frac{\delta f\left(HC^0_{father} + \eta^0_{father}, HC^0_{father}\right)}{\delta inc_{father}}\left(\eta_{father} - \eta^0_{father}\right) + \varphi_{father} + \nu_{son}. \end{aligned}$$

Here, the impacts of paternal human capital and luck depend on the point at which the function is evaluated. Suppose we evaluate this approximation at the actual paternal human capital and luck values of father-son pair *i*. Then this expression reduces to a random coefficients model of the following form:

(14) $$\begin{aligned} inc_{son,i} &= \pi_0\left(\eta_{father,i}, HC_{father,i}\right) + \left[\pi_1\left(\eta_{father,i}, HC_{father,i}\right) + \pi_2\left(\eta_{father,i}, HC_{father,i}\right)\right] HC_{father,i} \\ &+ \pi_1\left(\eta_{father,i}, HC_{father,i}\right)\eta_{father,i} + \nu_{son,i} \end{aligned}$$

In the absence of linearity, this expression suggests that the impact of human capital on son's income will vary across the distribution of human capital. Further, this random coefficients framework allows us to gain insight from the literature on local average treatment effects. Consider a case in which we have multiple binary instruments for human capital, which are uncorrelated to luck. Angrist and Imbens (1995) show that each instrument will possess a weighting function across the distribution of the treatment variable, paternal income in our case, proportional to the difference between the CDF evaluated at the two instrument values. Consequently, an instrument that induces variation in the bottom of the human capital distribution would produce a different weighted average of the random coefficients than an instrument that generates variation in the top of the human capital distribution. If the underlying function is linear, these weighting differences do not matter and the resulting estimates will be the same, regardless of the choice of instruments.

To test this conjecture, we estimate our model using an instrument that compares fathers in the lowest education category to the rest of the sample. This leverages variation in the bottom of the human capital distribution. We then compare the results for instruments which contrast fathers in each succeeding category to the remainder of the sample, ending with the top category. In this last case, most variation is coming from the top of the human capital distribution. Table 6 shows these IV estimates for which the instrument is a dummy variable which takes on a value of 1 if the father is in the relevant education group and zero otherwise. Examining the results, we see that the IV estimates are quite similar regardless of which education dummy variable we use as an instrument. The sole exception is when we instrument permanent income with a dummy variable that takes on a value of one if the father has exactly nine years of education.

This point estimate is very imprecise, however. We cannot reject that these instruments collectively yield the same point estimate.

Our linearity assumption also implies that if we stratify our sample by paternal human capital, the impact of paternal income on filial income should be the same across all levels of paternal human capital. Since we do not observe human capital, we cannot directly test this prediction. We can, however, address this in a rough fashion by regressing son's income on father's income separately by education category. These results are shown in Table 6. Because the procedure conditions on education, the average IIE within education category will be somewhat lower than our baseline OLS estimate. The estimates across education categories are qualitatively similar with the exception of fathers who completed graduate school. For this group, the IIE is much larger, though quite imprecisely estimated. Given that it is estimated from a sample of only 163 fathers, it should probably be discounted. Testing to see whether the estimates are jointly identical, we obtain a p-value of 0.055. The results from these three tests provide a pattern of evidence suggesting that our assumption of linearity is sound.

A three-factor model

A final potential concern is the model's restriction on the number of factors. Our model only includes two transmission mechanisms through which paternal income affects filial income: financial investments and human capital. However, human capital is an aggregate of genetics, education, social skills, and other factors. The aggregation of these components into a single factor is appropriate as long as each component, when denominated in income equivalents, has the same rate of transmission to filial income. More specifically, an increase in paternal IQ that generates \$1000 of paternal income needs to have the same effect on a son's income as an increase in paternal education that generates the same rise in paternal income.

To address this possibility, we next explore a potential test of the single human capital index assumption. Consider the case in which there are two types of human capital, $HC_{1,\,father}$ and $HC_{2,\,father}$ that may have different transmission rates. Consequently, son's income is generated in the following fashion:

$$(15) \qquad inc_{son} = \pi_0 + \pi_1 income_{father} + \pi_2 HC_{1,\,father} + \pi_3 HC_{2,\,father} + \nu_{son}.$$

Further suppose that father's income is simply the sum of the two human capital components, which may be correlated, and the luck component. In this case, the IV estimate of the IIE converges to:

$$(16) \qquad \begin{aligned} plim\left(\hat{\beta}_1^{IV}\right) &= \pi_1 + \pi_2 \frac{\operatorname{cov}\left(HC_{1,\,father}, Z_{father}\right)}{\operatorname{cov}\left(\eta_{father}, Z_{father}\right) + \operatorname{cov}\left(HC_{1,\,father}, Z_{father}\right) + \operatorname{cov}\left(HC_{2,\,father}, Z_{father}\right)} \\ &+ \pi_3 \frac{\operatorname{cov}\left(HC_{2,\,father}, Z_{father}\right)}{\operatorname{cov}\left(\eta_{father}, Z_{father}\right) + \operatorname{cov}\left(HC_{1,\,father}, Z_{father}\right) + \operatorname{cov}\left(HC_{2,\,father}, Z_{father}\right)}. \end{aligned}$$

It is clear from this expression that any two IV estimates may differ on the basis of their covariance with the luck component *or* because the instruments have differing covariance with the two types of human capital. Hence, rejecting our previous specification tests of equality across estimates implies the existence of at least two factors but does not provide information on whether there exist more than two.

Testing for the existence of three factors, or for two types of human capital, requires the existence of multiple instruments that are uncorrelated to luck. For such instruments, the expression in equation (16) simplifies to:

$$plim\left(\hat{\beta}_1^{IV}\right) = \pi_1 + \pi_2 \frac{\text{cov}\left(HC_{1,\,father}, Z_{father}\right)}{\text{cov}\left(HC_{1,\,father}, Z_{father}\right) + \text{cov}\left(HC_{2,\,father}, Z_{father}\right)} \quad (17)$$
$$+\pi_3 \frac{\text{cov}\left(HC_{2,\,father}, Z_{father}\right)}{\text{cov}\left(HC_{1,\,father}, Z_{father}\right) + \text{cov}\left(HC_{2,\,father}, Z_{father}\right)}.$$

This implies that two IV estimates will yield the same IIE only if $\pi_2 = \pi_3$. In that case human capital can be aggregated into a single index as $\text{cov}\left(HC_{1,\,father}, Z_{father}\right) / \left(\text{cov}\left(HC_{1,\,father}, Z_{father}\right) + \text{cov}\left(HC_{2,\,father}, Z_{father}\right)\right)$ is the same for both instruments. The latter is not generally true. Hence, a test of over-identifying restrictions based on a set of instruments which are uncorrelated to the luck component provides a test of our two-factor assumption. We previously argued that education and initial occupation likely have a low correlation with luck. Consequently, under a two-factor model, the IV estimates produced by these instruments should be similar. In Table 3, the education instruments of columns (2)-(3) produce similar results to the father's initial occupation instruments of column (4). Indeed, a test of over-identifying restrictions fails to reject that they are the same (p-value = 0.41). Although it is possible that the similarity comes from similar correlation patterns between the instruments and multiple dimensions of human capital, it is nevertheless encouraging to see little divergence.[17] Given this evidence, and the degree to which relaxing the assumption of two factors substantially complicates the analysis, a full treatment of this case remains beyond the scope of the current paper.

VI. Conclusion

[17] An ideal example might involve instrumenting father's income with education and IQ. It is likely that the latter has a higher correlation with the genetic component of father's human capital than the former. Unfortunately, data on father's IQ is not available for our sample.

There is a substantial body of economic evidence that income inequities persist across generations. Similarly, research has demonstrated the presence of cross-generation correlations in characteristics such as IQ and education level. How these elements interrelate is an important question with significant implications for understanding economic mobility and formulating government policy in a variety of areas, including income redistribution and education. Despite the importance of understanding the factors underlying intergenerational income persistence, there is, as yet, little agreement regarding the relative importance of different transmission mechanisms. In particular, there is no consensus on the expected effects of an extra dollar of parental financial investment on filial income holding other factors constant.

In this paper, we suggest a way to begin untangling the influence of two mechanisms through which the observed intergenerational transmission of income operates. Starting with a simple two-factor model, we demonstrate two methods for isolating the effect of monetary resources on the IIE from intergenerational correlations in income due to human capital elements. Coupled with a rich longitudinal data set this allows us to estimate the separate contributions of money and human capital to observed correlations in intergenerational income. We reject the one-factor model of intergenerational income correlation and estimate that no more than 37 percent of the correlation between fathers' and sons' incomes operates through the causal effect of financial resources. This evidence is consistent with the relatively small long run effects of financial resource increases on child outcomes mentioned in Blau (1999) as well as Dahl and Lochner (forthcoming).

The fact that a substantial majority of intergenerational inequality is due to factors other than differential parental access to financial resources may reflect the relatively egalitarian nature of Swedish social institutions. However, these results imply that further attempts to increase intergenerational social mobility through equalizing parental financial resources will have at most modest success.

Without further research we cannot know if the relative importance of financial resources, as opposed to human capital, will be the same in countries with relatively high levels of intergenerational income persistence, such as the U.S. However, the methodologies presented in this study are straightforward to generalize to other settings. In the Swedish context, our explication of how the OLS IIE, the estimated impact of paternal human capital on son's earnings, and the r-squared from a Mincer regression jointly identify the causal effect of financial resources produced less informative bounds than our alternative strategy. However, this method may provide a viable alternative when working with data from other countries where fewer potential instruments are available to represent the role of idiosyncratic labor market luck. Going forward, it will also be helpful to further test the assumptions underlying our model. Examining how the structural parameters underlying the IIE vary across countries could illuminate the role of financial resources versus human capital across institutional settings.

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Table 1. Descriptive Statistics for Fathers and Sons Used in Our Baseline Estimation.

	Log income[a]	Missing income observations				Birth year			Number of observations
	Mean (s.d.)	Min	Median	Mean	Max	Min	Median	Max	
					Fathers				
All	12.31 (0.392)	0	0	0.71	21	1938	1940	1945	24114
	Fathers tabulated according to the number of sons they contribute to the sample								
1	12.32 (0.392)	0	0	0.71	21	1938	1940	1945	22960
2	12.26 (0.374)	0	0	0.79	18	1938	1939	1945	1122
3	12.11 (0.345)	0	0	1.39	11	1938	1939	1942	31
4	11.78 (n.a.)	5	5	5	5	1938	1938	1938	1
					Sons				
All	12.39 (0.431)	0	0	0.03	1	1950	1964	1965	24114

a) Calculated using fathers' incomes between the ages of 30 and 60 and using sons' incomes between the ages of 30 and 40. Zero incomes and missing are both treated as missing. We require at least 10 years of non-missing incomes to be included in the sample.

Table 2. Descriptive Statistics for the Full Sample and for Our Sample.

	Full-data income[a] son	11-year average son's income[b]	Full-data income[a] father	11-year average father's income[b]	Years of schooling father	Median birth year son	Median birth year father	Mean father-son age difference
	Full sample							
Mean	12.09	12.26	12.19	12.24	9.51	1958	1927	31.37
(s.d.)	(0.495)	(0.591)	(0.518)	(0.521)	(2.983)			(7.038)
Number of obs.	303886	298160	294862	136359	276158	309869	303141	303141
	Our sample							
Mean	12.07	12.39	12.32	12.24	9.84	1964	1940	22.79
(s.d.)	(0.338)	(0.431)	(0.391)	(0.394)	(2.807)			(2.313)
Number of obs.	24114	24114	24114	24114	24114	24114	24114	24114

a) Log average income calculated using all data on income regardless of age and the number of available observations. Zero incomes and missing are both treated as missing.

b) Log average income calculated using sons' incomes between the ages of 30 and 40. Zero incomes and missing are both treated as missing. We require at least 10 years of non-missing incomes to be included in the sample.

Table 3. Estimates of the Father-Son Intergenerational Income Elasticity Relationships.[a]

		Instrument					
	Ordinary Least Squares	Father Years of Schooling	Father Level of Education	Father Occupation (1970)	Father Occupation (1970-1990)	Father Employment Status	Father Employment Residuals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fathers' permanent income	0.286*	0.416*	0.414*	0.400*	0.335*	0.205*	0.106
	(0.010)	(0.020)	(0.020)	(0.014)	(0.011)	(0.017)	(0.065)
P-value of test that IV=OLS		0.000	0.000	0.000	0.000	0.000	0.006
First Stage F-statistic		2630	564	6254	2756	858	39
[p-value]		[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Observations	24,114	24,114	24,114	24,114	24,114	24,114	24,114

a) In all specifications the dependent variable is the natural logarithm of sons' permanent income. With the exception of column (1) fathers' permanent income is treated as endogenous. Instruments are listed at the top of each column. All standard errors are calculated using a 100 iteration bootstrap clustered at the father level. * denotes significance at 5%.

Table 4. Identification of Structural Parameters (Log of Average Income).[a]

		Parameter				
		OLS IIE	π_1	$\pi_1+\pi_2$	π_2	$Mincer\ R^2$
(1)	Parameter Estimate	0.286*	0.106	0.414*	0.307*	0.585*
		(0.010)	(0.065)	(0.020)	(0.068)	(0.100)
	Identification Method	OLS	Employment Residuals IV	Education Category IV	Implied by Model	Implied by Model
(2)	Parameter Estimate	0.286*	0.209*	0.414*	0.205*	0.376*
		(0.010)	(0.015)	(0.020)	(0.029)	(0.007)
	Identification Method	OLS	Implied by Model	Education Category IV	Implied by Model	Estimated from Primary Sample

a) Regressions follow those in column (2) of Table 3, where the dependent variable is sons' permanent income and Fathers' permanent income is treated as endogenous. Instruments are listed at the top of each column. All standard errors are calculated using a 100 iteration bootstrap clustered at the father level. * denotes significance at 5%.

Table 5. Identification of Structural Parameters (Average of Log Income) [a]

		Parameter				
		OLS IIE	π_1	$\pi_1 + \pi_2$	π_2	$Mincer\ R^2$
(1)	Parameter Estimate	0.248*	0.135*	0.348*	0.213*	0.532*
		(0.011)	(0.064)	(0.028)	(0.072)	(0.182)[b]
	Identification Method	OLS	Employment Residuals IV	Education Category IV	Implied by Model	Implied by Model
(2)	Parameter Estimate	0.248*	0.196*	0.348*	0.151*	0.341*
		(0.011)	(0.016)	(0.028)	(0.037)	(0.007)
	Identification Method	OLS	Implied by Model	Education Category IV	Implied by Model	Estimated from Primary Sample

a) Regressions follow those in column (2) of Table 3, where the dependent variable is sons' permanent income and Fathers' permanent income is treated as endogenous. Instruments are listed at the top of each column. All standard errors are calculated using a 100 iteration bootstrap clustered at the father level.

b) In calculating this standard error, 3 of 100 draws were outside of the 0 – 1 interval. Consequently, we constrained the r-squared estimates to lie within that interval. * denotes significance at 5%.

Table 6: Robustness Checks

Specification	Estimate of IIE
OLS IIE [a]	0.286*
	(0.010)
IIE incorporating regional price indices	0.320*
	(0.008)
IV Estimates by Instrument(s)	
all education categories	0.417*
	(0.024)
less than 9 years	0.443*
	(0.029)
9 years	-0.263
	(0.359)
2 years secondary education	0.444*
	(0.075)
2-3 years secondary education	0.442*
	(0.046)
Less than 3 years upper secondary education	0.453*
	(0.055)
3 years upper secondary education	0.388*
	(0.038)
Graduate study	0.374*
	(0.082)
OLS Estimates by Education Category	
Less than 9 years	0.232*
	(0.014)
9 years	0.234*
	(0.031)
2 years secondary education	0.258*
	(0.019)
2-3 years secondary education	0.301*
	(0.024)
Less than 3 years upper secondary education	0.279*
	(0.035)
3 years upper secondary education	0.285*
	(0.039)
Graduate study	0.758*
	(0.260)

a) Standard errors are cluster-corrected at the father level except for the first row which is calculated using a 100 iteration bootstrap clustered at the father level. * denotes significance at 5%.

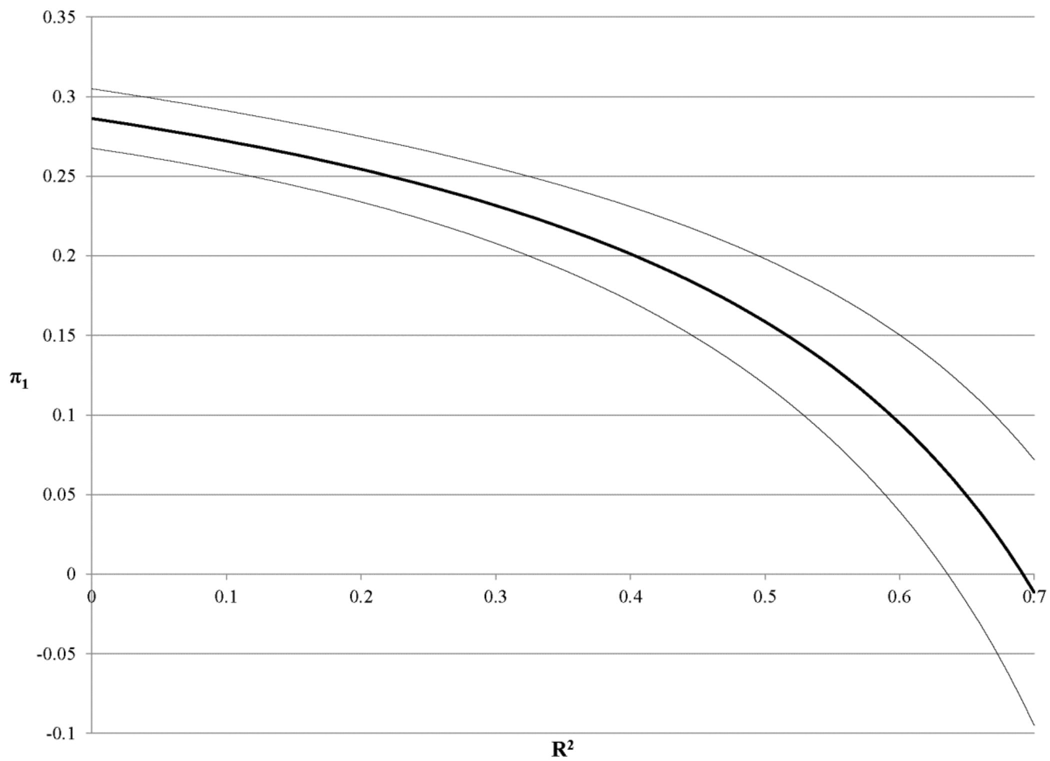

Figure 1: The relationship between the causal effect of paternal money on filial income (π_1) as a function of the fraction of paternal incomes explained by human capital (Mincer R^2).

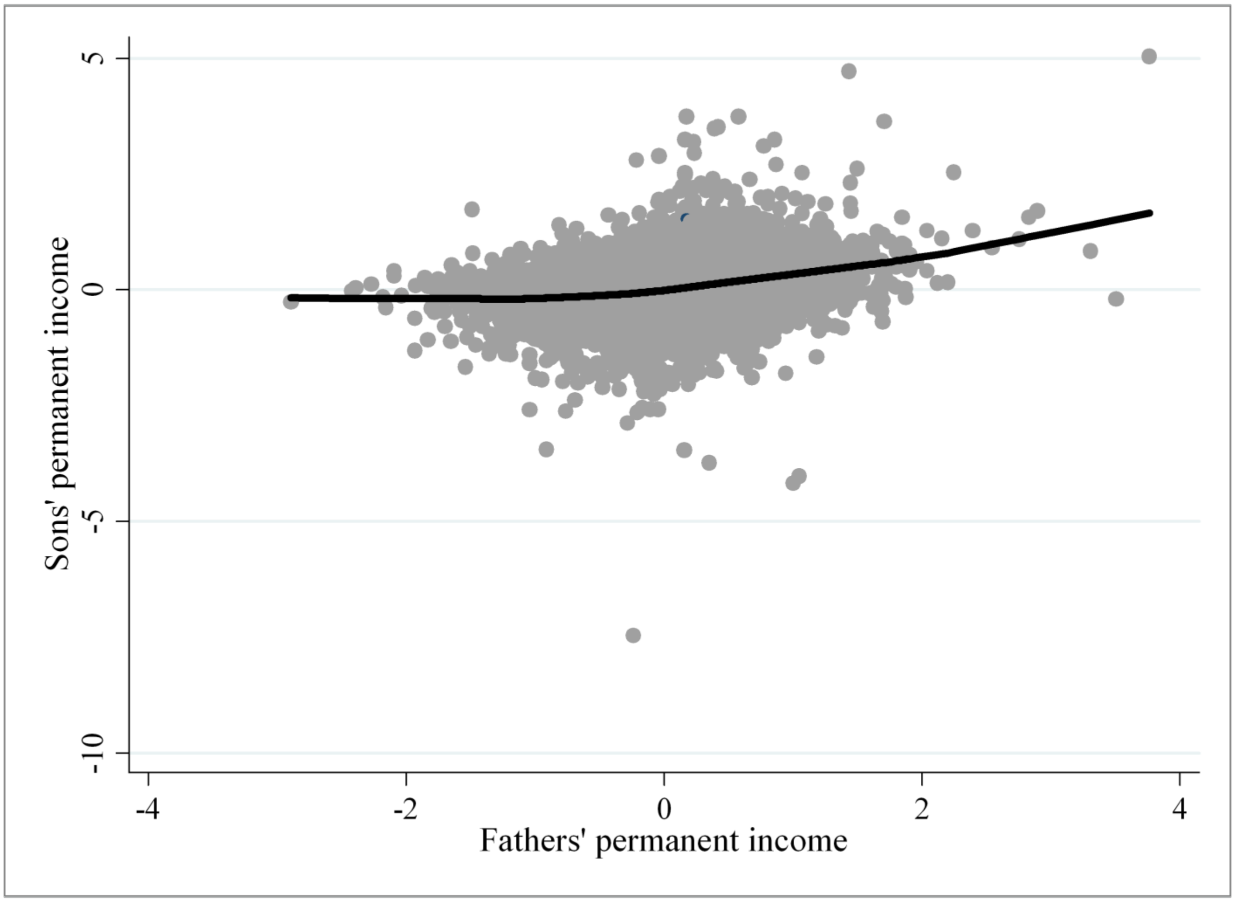

Figure 2: The lowess relationship between fathers' and sons' permanent incomes.

Notes: Both Fathers' and Sons' incomes have been purged of year-of-birth effects. Bandwidth = 0.8.

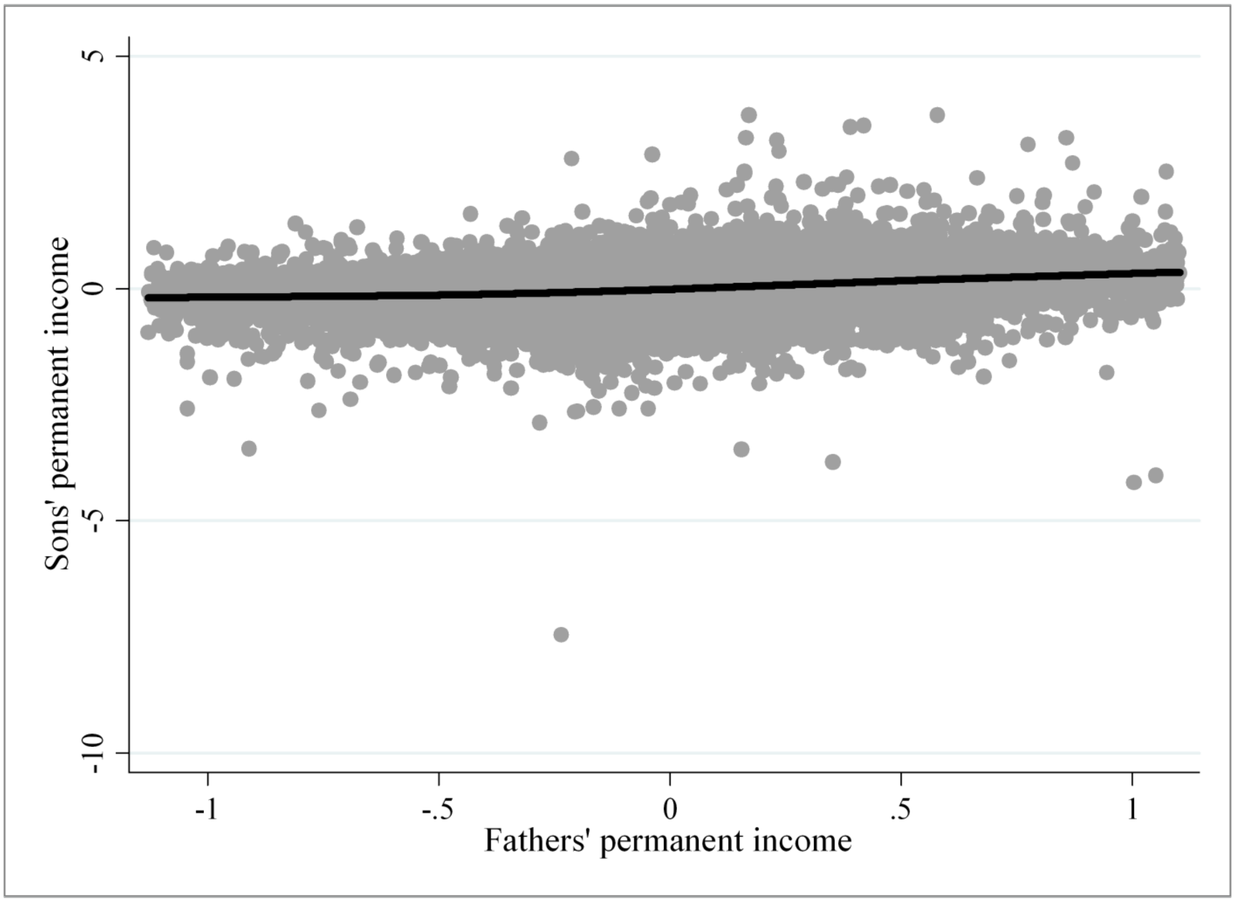

Figure 3: The lowess relationship between fathers' and sons' permanent incomes omitting the top and bottom 1% of observations.

Notes: Both Fathers' and Sons' incomes have been purged of year-of-birth effects. Bandwidth = 0.8.