Schooling, Family Background, and Adoption: Is it Nature or is it Nurture?

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When parents are more educated, their children tend to receive more schooling as well. Does this occur because parental ability is passed on genetically or because more educated parents provide a better environment for children to flourish? Using an intergenerational sample of families, we estimate on the basis of a comparison of biological and adopted children that about 55 to 60 percent of the parental ability is genetically transmitted.

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I Introduction

In this paper we specify and estimate a structural model of schooling that focuses the attention on the role of ability. Specifically, we disentangle the effect of ability into a nature and a nurture component on the basis of differences in educational attainment between adopted children and children who are their parents' own offspring. In addition, the model allows for a host of selectivity effects in the data that are common to adoption analyses and are related to endogenous family income, treatment differentials among adopted and biological children, and the non-random process that links adoptees and families.

We have at our disposal a U.S. dataset, the Wisconsin Longitudinal Survey, that contains very detailed multigenerational information about households. Data collection started in 1957 on a group of high school students in Wisconsin all born around 1939. Information was gathered about their IQ, family background, and so on. In 1964, 1975, and 1992, the same students were contacted again and information was collected about their school careers, labor market status, family conditions, and the school careers of their children. For these children it is recorded whether they are their parents' own offspring or whether they are adopted.

The paper finds that parental ability measured as IQ is an important factor in explaining the children's school success. If we decompose the IQ transfers from parent to child into a genetic and environmental component, we find that about 70 to 75 percent of the ability effect relevant for school achievement measured by IQ is determined by nature. However, IQ is not the only determinant; family income matters too. It should be recognized that a portion of the IQ transmission channel runs through family income as well. When income is purged of its ability component, the genetic portion in the IQ transfer falls about 15 percentage points to 55 and 60 percent. We subject this estimate to a sensitivity test and determine a lower bound of 50 percent. The main conclusion of this paper is that nurture does not seem to play a dominant role.

In the following, Section II provides the background and motivation behind this study. Section III discusses the structural model of educational mobility. After a further description of the Wisconsin Longitudinal Survey in Section IV, the parameter estimates are presented in Section V. Section VI highlights the implications and conclusions of this study.

II Background and motivation

Many studies show that children raised by highly educated parents receive more schooling than children raised by less educated parents. The economics literature examines this family connection with models where parental resources are linked to the educational attainment of children through human capital investments (Becker and Tomes 1986; Haveman and Wolfe 1995). Supported by empirical findings that more family income, earned on average by highly educated parents, stimulate further schooling, economists put their emphasis on nurture in determining educational outcomes.

Alternatively, in *The Bell Curve* Herrnstein and Murray (1994) argue that it is ability measured as IQ that matters. Highly educated parents have more ability on average than less educated parents. If ability is transmitted genetically from parents to children, education turns out to be persistent across generations. Furthermore, not only are high ability parents highly educated, they also generate more income. If family income matters for educational achievement, ability effects run through income as well. Altogether, Herrnstein and Murray claim that it is nature rather than nurture that explains educational persistence across generations.

In this paper we compare both views and use the intergenerational mobility model of human capital proposed by Becker and Tomes (1986) to explore how both family income and ability (measured as IQ test scores) move across generations and affect the educational attainment of next generations. Our aim is to unravel what contribution parental ability makes to the educational attainment of children and to identify those parts of ability that are driven by family genes (nature) and family environment (nurture).

The relative importance of nature and nurture is clearly of relevance for both understanding the dynamics of the distribution of educational attainment and designing educational policies. If nurture drives the success of children in school, inequality in educational opportunity may well come from failing capital markets. In this situation income transfers to poor families will reduce past inequalities in schooling and the next generation of children will start out more equally. On the other hand, if children's ability is primarily inherited, inequality in opportunity is merely a reflection of the distribution of ability among the then-existing population. In this situation potential effects of pro-education policies are reduced.

To shed light on the importance of the heritability of ability, there are four possible strategies. The first strategy looks at environmental influences shared by twins. Under the assumption that twins share the same family environment, the differences between fraternal and identical twins identify the genetic contribution. There are, however, drawbacks that are typical to twin strategies. Nature and nurture outcomes may be biased because identical twins are treated more similarly than fraternal twins (Plomin, DeFries and McClearn 1990, p.315), because most twin studies rely on small samples, or because twin data usually describe twins who volunteer to participate. The second strategy extends the family tree and considers relatives who are either raised together or apart. Controlling for the genetic structure among relatives, the differences between relatives raised in different families are used to measure the environmental impact. Such data are of course much more widely available, but they also have one significant drawback: children who are relatives share more than just genes. It is plausible that their parents, who are adult siblings, create household environments that share similarities because of the way that they themselves were brought up together. Since it is not clear from the data how family environments of siblings in different families are related, resulting nature estimates are biased, and in most situations too high (Goldberger 1979). The third strategy, which is the one applied in this paper, compares children that are their parents' own offspring to children that are adopted. Since adopted children are genetically unrelated to the families that raise them, we control for the family environment in which children (both adopted and biological) are raised together, and thereby identify the genetic component. Still, this third strategy is often plagued by problems that affect the accuracy of outcomes. These problems usually relate to small sample sizes, the non-randomness of the placement of adopted children in adopting families, parents' emotional and material differentiation between biological and adopted children, and missing relevant information on the biological background and adoption history of adopted children.[1] The fourth and preferred research design would work with data on identical twins reared apart in different and unrelated families. Genetic differences would be controlled for, and environmental components would be identified. In practice, however, there is no survey with a sufficient number of reliable cases; and even if there are some identical twins raised separately in different families, it is unlikely that the assignment to these families is a random process.[2]

Plomin and Petrill (1997) summarize many of these family, twin, and adoption studies. Their main concern deals with heredity and cognitive ability measured as IQ (or IQ related) test scores, and their general finding is that about half of the variation in IQ test scores is explained by genetic factors. They also conclude that family environment accounts for about a quarter of the variation in IQ scores when children are young but that its influence is falling when these children grow up. The extent to which these results can be extended to educational attainment is not readily known and will be one of the central questions of this paper.

Where much of the previous work in the nature and nurture field has been done by psychologists, economists have mostly skirted the nature/nurture debate. Apart from responses made by various economists to views expressed by Herrnstein and Murray,[3] there is hardly any empirical evidence on the role of

[1] Studies involving adopted children find that the genetic influence of family background is more important than the influence of family environment in explaining the ability of children (Scarr and Weinberg 1978; Loehlin, Horn and Willerman 1994; Defries, Plomin and Fulker 1994).

[2] Two studies involving only 65 and 95 pairs of identical twins reared apart find that about 70 percent of ability is inherited (Bouchard and McGue 1981; Bouchard, Lykken, McGue, Segal and Tellegen 1990).

[3] The arguments, evidence, and research methods presented in *The Bell Curve* have been widely criticized by economists. The main gist of the critique is that IQ is an important but not a dominant factor in predicting economic and social success (Ashenfelter and Rouse 1999; Currie and Thomas 1999; Cawley, Heckman and Vytlacil 1999; Goldberger and Manski 1995; Korenman and Winship 2000). Note that these discussions are not new and very much resemble the IQ debate that took place among psychologists in the early seventies (Jensen

inherited ability in economic models of investment in schooling. There are a few exceptions. Behrman, Taubman and Wales (1977) did an important study that use correlations between identical and fraternal twins to separate nature from nurture effects. They observed that if parents constitute genetically random matches, about 44 percent of the variance in educational attainment would be attributable to genetic differences. Extending the sample with other siblings and relatives not raised in the same nuclear family, and allowing for assortative mating Behrman and Taubman (1989) again focus on inequality of educational opportunity and conclude this time that nature is much more important. They find that schooling is for roughly 80 percent driven by family genes.[4] Our study offers three clear contributions relative to these studies. The first contribution lies in the level of abstraction. To infer information with variance decomposition on whether nature or nurture is the determining factor in describing inequality in human capital is rather abstract as it only reflects relative contributions to R^2. In contrast, we estimate which part of ability is inherited and which part can be attributed to the environment. In doing so we decompose ability effects in the more concrete form of regression slopes. The second contribution concerns the flexibility in the role of income. Our study does not treat income as an explicitly environmental variable. Rather, it models ability transfers in a way that allows ability effects to run through income as well. The third contribution is one of focus. The economics literature thus far uses information on twins and relatives to isolate a genetic transmission mechanism. We apply information on adopted children to isolate the environmental transmission mechanism. Notice that the two models are complementary: both intend to describe the same intergenerational phenomena. Thus, it is interesting to have a well-developed parallel set of findings.

As far as we know, there is only one approach that is closely related to the one we present in this paper which was independently developed by Sacerdote (2000, 2002). With data on biological and adopted children, he finds only small effects of the parents' education and income on the adoptees' IQ, but large effects on the child's schooling. In fact, he concludes that for years of schooling and college education nurture seems to be the dominating factor. With respect to the influence of mother's and father's years of education on the schooling of the child about 36 and 43 percent is genetically determined, respectively. In a related fashion, we explore how much nature and nurture contribute to the educational attainment of children using another but somewhat larger set of adopted children. In view of the sparse literature, it is certainly useful to have more than one study using comparable methodologies with different data. But our study also complements the work of Sacerdote in at least two other directions. First, we model the mobility of educational attainment such that it is consistent with Becker and Tomes' model of investment in schooling. Sacer-

1973; Herrnstein 1973; Jencks 1972).

[4]It remains unclear, however, what is generating the difference between both findings.

dote's study is mainly empirical and aims to estimate the nature and nurture components of various economic outcomes. Second, and perhaps more importantly, we explore what happens to our nature and nurture outcomes when we address some of the traditional pitfalls that are common to adoption analyses. Using a larger available set of adopted children, we do not merely treat family income as an explicit environmental variable but allow family genes to run through family income as well. We further explore what happens if there are treatment differentials among adopted and biological children by allowing parents to invest different amounts of money in the school career of adopted and biological children. And finally, we will use the fact that adopted children are not always randomly assigned to the new family of rearing to put a meaningful upper bound on our nurture estimate.

These studies form the backdrop for the mobility model of educational attainment, to which we will now turn.

III The mobility model

The mobility of human capital is modelled akin to Becker and Tomes (1986), with the exception that this model considers the transmission of human capital instead of income. Let us start by defining the following variables: e indicates ability, y denotes income, h represents human capital. Subscripts t and $t-1$ are indices for the current generation and its parents, respectively. At the outset, it should be pointed out that the survey on which our empirical work is based measures ability only of the parents and not of the children.

Ability e transfers from parent to child through genes and culture. Thus, if we were to write an ability mobility equation in the form of

$$e_t = b_0 + b_1 e_{t-1} + v_t, \tag{1}$$

where v is a non-structural component of ability, the coefficient b_1 might be thought of as the sum of b_{g1} and b_{c1}, the genetic and cultural components in the transmission mechanism. Clearly, the values of b_{g1} and b_{c1} are of greater interest than their sum: indeed, this decomposition is the objective of the paper, and we shall focus on this below. But equation (1) is a little too simplistic. If parental ability e_{t-1} impacts the child's ability e_t not only genetically but also culturally, it is clear that the cultural contribution occurs after the birth of the child. Once one admits to this, it is obvious that equation (1) ought to be expanded with other post-birth determinants of ability, which are generically denoted by the variable x. In particular, one could make a case that parental income might matter: e.g., wealthier parents purchase better prenatal care for themselves, leading to a healthier newborn baby.[5] Later on in life, music lessons or vacation trips to far-away places provide children with valuable life

[5]This is indeed a liberal interpretation of a "cultural" transmission factor, but it properly fits under the heading of "nurture."

experiences. Moreover, richer parents are more able to provide costly additional tutoring to remedy potential learning deficiencies of their children (Currie and Thomas 2001). Thus, let us specify the ability mobility relationship as

$$e_t = b_0 + b_1 e_{t-1} + b_2 y_{t-1} + b_3 x_t + v_t, \tag{2}$$

If the transmission of ability is purely genetic, b_2 and b_3 equal zero. Thus, equation (2) is more general.

According to Becker and Tomes parents invest in human capital of their children. In their model family income and individual ability are the ingredients of the children's human capital function. For completeness, we add the generic factors x to this relationship

$$h_t = c_0 + c_1 e_t + c_2 y_{t-1} + c_3 x_t + w_t. \tag{3}$$

Like v, w is considered random variation. Both disturbances have zero means.[6] Parental ability affects the human capital investment of children directly and indirectly through family income, which is clearly seen when we combine (2) and (3) and we write down for today's generation

$$h_t = (c_0 + b_0 c_1) + b_1 c_1 e_{t-1} + (c_2 + b_2 c_1) y_{t-1} + (c_3 + b_3 c_1) x_t + w_t + c_1 v_t. \tag{4}$$

To measure the importance of the heritability of ability, we introduce a novel approach. For parents and their biological children, ability transmissions run through both genetic and cultural channels. For adopted children, however, genetic transfers do not exist. Define the variable d_t to denote the biological status of the child: $d_t = 1$ if the child is adopted, and $d_t = 0$ if the child is a biological offspring. If e^*_{t-1} represents the parental abilities of biological parents of adopted children, the parental ability of the child in question may be written as $(1 - d_t)e_{t-1} + d_t e^*_{t-1}$. Separating the genetic and cultural component of b_1 and recognizing that the adoptive parents impart the cultural contribution to ability, the ability mobility relationship (2) is modified as follows:

$$e_t = b_0 + b_1 e_{t-1} + b_{g1} d_t (e^*_{t-1} - e_{t-1}) + b_2 y_{t-1} + b_3 x_t + v_t. \tag{5}$$

Since we do not observe abilities of the natural parents of adopted children, we replace $b_{g1} d_t e^*_{t-1}$ with $b^*_0 d_t$ to correct for this omission. In effect, $b^*_0 d_t$ then measures the average value of $b_{g1} d_t e^*_{t-1}$ across the subsample of adopted children. Inserting (5) into (3) yields a human capital function suitable for a sample of both biological and adopted children:

$$\begin{aligned} h_t = {} & c_0 + b_0 c_1 + b^*_0 c_1 d_t + b_1 c_1 e_{t-1} - b_{g1} c_1 d_t e_{t-1} + (c_2 + b_2 c_1) y_{t-1} \\ & + (c_3 + b_3 c_1) x_t + w_t + c_1 v_t. \end{aligned} \tag{6}$$

[6] Goldberger (1989) speaks of mechanical rather than economic mechanisms when he discusses intergenerational transmission models. For our exercise to be developed in this paper we do not necessarily need the assumption that parents maximize their utility.

Under the assumption that our functional form is correct, estimates of $b_1 c_1$ and $b_{g1} c_1$ produce our nature and nurture estimates where a simple division $b_{g1} c_1 / b_1 c_1$ disentangles environment from genes. This is the most important identification achieved by the use of information on adoption status. The contributions of parental income and other factors to ability and schooling are not separately identifiable, because adopted children are subject to the same cultural influences as biological children.[7]

One should be aware that the adoption strategy we have chosen in this Section does not separate the effects of the family environment from the family genes perfectly. In fact, we are quite convinced that estimates of the nature and nurture effects according to (6) are biased. In what follows, we outline the potential dangers that usually affect the accuracy of adoption estimates, and explore what happens to the nature and nurture estimates if we attempt to solve these problems.

A Indirect nurture effects of ability

The way we treat our ability and income parameters for both adopted and biological children rests on the assumption that income is an explicitly environmental variable; see equation (2). Whether this is a fair description of reality is questionable, since ability effects may operate through income as well: it should be expected that more able parents earn higher incomes. This creates an indirect channel through which nurture component of ability is transmitted from parent to child (Dickens and Flynn 2001). The magnitude of this channel should be quantified as well in order to get an accurate assessment of the nature/nurture ratio.

One way to do so is to drop family income as an explanatory variable in our analysis and let the remainder of the income effect be absorbed into parameter of e_{t-1}. This approach denies the idea that at least a portion of income is environmental and independent of ability. Moreover, such an approach has the disadvantage that it complicates matters when it comes to testing for potential differences in upbringing between biological and adopted children (Section III B). The better way to quantify the indirect channel is to isolate that component of income that is unrelated with parental ability. Let l_{t-1} denote that part of family income that is orthogonal to e_{t-1}. If l is substituted for y in equation (6), the new human capital function reads as

$$\begin{aligned} h_t = c_0 + b_0 c_1 + b_0^* c_1 d_t + b_1 c_1 e_{t-1} - b_{g1} c_1 d_t e_{t-1} + (c_2 + b_2 c_1) l_{t-1} \\ +(c_3 + b_3 c_1) x_t + w_t + c_1 v_t. \qquad (7) \end{aligned}$$

The parameters $b_1 c_1$ and $b_{g1} c_1$ pick up that part of income that is generated

[7] In principle, one might explore differences in this regard as well if one has information about the child's age at adoption. We must leave that for future research, since the present data set does not include such information.

by ability. As a consequence, the ratio $b_{g1}c_1/b_1c_1$ computes the share of genetic transfers in the total, rather than merely the partial, impact of ability.

B Different allocation rules for biological and adopted children

The ratio $b_{g1}c_1/b_1c_1$ is interpreted as a nature effect under the condition that parents do not differentiate between their biological and adopted children. That is, families treat their children equally with respect to the time and money they invest in them. Although potential treatment differentials are partly accounted for through adoption dummies in (7), interpretation of our heritability factor becomes troublesome if differences in upbringing affect the estimate of $b_{g1}c_1$.

In the economics literature there are some models available where parents treat their children differently in response to differences in their children's individual ability. In these models (Behrman, Pollak and Taubman 1982; Behrman, Rosenzweig, and Taubman 1994; Becker 1991; Ermish and Francesconi 2000) parents may have an aversion to inequality of earnings among their children and the way they invest in their children's education reflects both equity and efficiency motives. If parents care equally about their children's welfare, parents choose to invest in less talented children to compensate for their ability deficit. But if parents are less altruistic and only invest to generate the highest return, less talented children receive less educational funding and talented children are reinforced. The existing empirical literature thus far has not been conclusive on family allocation rules. Some studies suggest that parents compensate (Ashenfelter and Rouse 1998; Ermish and Francesconi 2000) while other studies find that parents reinforce (Behrman, Pollak and Taubman 1982; Behrman, Rosenzweig and Taubman 1994; Miller, Mulvey and Martin 1995).

These economic allocation models are applicable for adopted and biological children if there are structural differences in ability between adopted and biological children, and if parents treat their adopted children differently. With respect to ability differences, there is reason to believe that these differences exist and that children who are given up for adoption are on average less intelligent.[8] With respect to treatment differentials, recent work by Case, Fin and McLanahan (2000) shows that adopted children are indeed raised differently. In particular, parents spend significantly less money on food in the presence of adopted children. If lower spendings on food also implies that parents devote relatively less of their income to the education of their adopted children, we expect parents to

[8]The mechanism is built on the positive relation between ability and parental income. Low-income families and young single mothers face on average more difficulties to make ends meet, and are therefore more likely to register their children for adoption (Medoff 1993). Since components of ability are heritable, adopted children will be on average less endowed. An alternative mechanism would be that if parents could choose they would probably put their inferior rather than their superior children up for adoption (Becker 1991). Again, adoptees will be less endowed.

reinforce the ability differences between their adopted and biological children.[9]

To allow for different allocation rules among adopted and biological children, the income effects in our model should be different for adopted and biological children, and should only capture income transfers that are independent of parental ability. Hence, we return to our model in (7) and let the income parameters b_2 and c_2 be different for adopted and biological children: we add subscripts a for adopted children and b for biological children. The ability mobility and human capital relation are redefined as

$$\begin{aligned} e_t &= b_0 + b_0^* d_t + b_1 e_{t-1} - b_{g1} d_t e_{t-1} \\ &\quad + b_{2b}(1-d_t) l_{t-1} + b_{2a} d_t l_{t-1} + b_3 x_t + v_t \end{aligned} \tag{8}$$

and

$$h_t = c_0 + c_1 e_t + c_{2b}(1-d_t) l_{t-1} + c_{2a} d_t l_{t-1} + c_3 x_t + w_t. \tag{9}$$

By repeating the same procedure as before, these equations are combined and together they produce a new human capital function

$$\begin{aligned} h_t &= c_0 + b_0 c_1 + b_0^* c_1 d_t + b_1 c_1 e_{t-1} - b_{g1} c_1 d_t e_{t-1} \\ &\quad + (c_{2b} + b_{2b} c_1) l_{t-1} + ((c_{2a} - c_{2b}) + (b_{2a} - b_{2b}) c_1) d_t l_{t-1} \\ &\qquad + (c_3 + b_3 c_1) x_t + w_t + c_1 v_t. \end{aligned} \tag{10}$$

If parents reinforce (compensate for) more able children, they underinvest (overinvest) in their adopted children. This implies that both $c_{2a} - c_{2b}$ and $b_{2a} - b_{2b}$ are negative (positive). It is easily seen that $c_{2a} - c_{2b}$ and $b_{2a} - b_{2b}$ cannot be identified separately. However, given that greater ability would raise the years of schooling and thus that c_1 would be positive, we learn that parents reinforce (compensate for) differences in ability if $(c_{2a} - c_{2b}) + (b_{2a} - b_{2b})c_1$ is negative (positive).

Implications for our nature estimates also depend on whether and in what way parents treat their adopted children differently. If parents invest less in their adopted children, both b_{c1} and c_1 will be higher for biological than for adopted children, which implies that in an empirical model that ignores allocation differences nature effects will be overestimated. On the other hand, if parents invest more in their adopted children the effects are reversed.

C The non-randomness of adoption experiments

Samples that mix adopted and biological children (like ours) produce unbiased estimates if children are randomly given up for adoption and if these children

[9] A difference between ability among biological and adopted children is by no means a necessary condition to create treatment differentials. For example, if parents expect closer ties (financial and otherwise) in their old age with their biological children than with their adopted ones, they will invest more in the education of their biological children as well. Other mechanisms to explain why adopted children may end up with lesser schooling can be found in Case, Fin and McLanahan (2000, 2001).

are then randomly assigned to the new family of rearing. In practice, these randomization requirements are rarely met and create various types of ability and selection bias that might have an affect on our estimates. We discuss three different sources of bias and the assumptions needed to identify the effects of nurture and nature properly.

The first potential source of bias arises when we consider adoptees. Children who are given up for adoption are more likely to come from poor and low ability families. If genes matter this means that non-adoptees will be better endowed than adoptees. In our model, however, differences in mean ability will be swept into the adoption parameter $b_0^* c_1$. With the adoption dummy included and the omitted ability variable uncorrelated to other variables, there will be no bias in the nature/nurture ratio.

The second potential source of bias arises when we consider adoptive parents. Adoptees are usually placed in families with favorable socioeconomic characteristics. This means that adoptive parents will be better endowed than non-adoptive parents. For our estimates, however, this is of no concern because we observe parental ability. But what happens if high ability adoptive parents tend to be better parents as well? Suppose that adoptive parents are better, as measured by an unmeasured parental characteristic q_{t-1} for parenting qualities. The solution looks to be parallel with the first: define a dummy variable to indicate whether children are raised by adoptive parents. The parameter estimate on this variable will capture the average value of the parenting qualities among adoptive parents over and above the non-adoptive parents. If indeed these unobserved parenting qualities are uncorrelated with other variables, no bias is present. However, when unmeasured parental quality q_{t-1} (in deviation from its mean) is correlated with parental ability e_{t-1}, an upward bias in the parameter estimate $b_{c1} c_1$ does exist and the effect of nurture is overestimated.

The third and final source of potential bias arises as a results of the selection of adoptees into adoptive households. If high-ability parents manage to adopt children from high-ability natural parents (mostly mothers), adoptees with high ability adoptive parents will have more education due to better treatment and selection. Again, the effect of nurture is overstated because of the positive correlation between e_{t-1} and e_{t-1}^*.

With the data at hand it is impossible for us to find a remedy to remove these biases. What we can do, however, is exploit these potential sources of bias to put a meaningful upper bound on our nurture estimate.

D Measurement errors

Most models assume error free measurement in their regressor variables. In practice measurements are seldom if ever perfect and our empirical representation of ability forms no exception to this rule. However, we do not expect that measurement error will strongly affect our outcomes when we decompose the ability effects into nature and nurture components. Recall that in our research design we isolate the genetic component of all ability transfers by dividing two ability parameters based on the same ability measure. Thus, even if our IQ test score is measured with error, as long as both the denominator and nominator are similarly affected, our nature/nurture decomposition remains unaffected.[10]

IV Data

This paper employs the Wisconsin Longitudinal Survey which is a unique U.S. data set with information on people who were born around 1939. The collection of these data started in 1957 with a questionnaire administered to the complete cohort of students who graduated from a high school in Wisconsin in that year. The information in that first wave relates to the students' social background (parents' education and occupation, numbers of older and younger sibling), intelligence (measured as standardized IQ test scores), and aspirations. Subsequently, research was continued on a randomly selected one third of the original cohort. In 1964 and 1975, the respondents was approached again to obtain information about, among others, their schooling and labor market careers. In 1992, the same sample of persons was contacted once more in order to collect new information about their labor market experiences between their late 30s and early 50's. As well, this latest round contained questions about many facets of life events and attitudes. For more information on the WLS data, see, among others, Sewell and Hauser (1992) and Hauser et al. (1996).

Of particular interest for the present study, a set of questions targeted the educational attainment of the respondents' children. Respondents were asked to list for each child the highest grade or year of regular school that child ever attended, whether (s)he completed this grade or year, and whether (s)he attended a regular school in the last 12 months. From the information on educational attainment we create two variables. "Years of schooling" equals the number of years nominally required for the highest level of education that the child completed. "College attendance" indicates whether or not a child completed more than 12 years of education. Children who were still in school constitute censored observations and will be treated accordingly in our empirical analysis; this is the case for about 25 percent of our sample.

Respondents often have more than one child and report information on each of them. This allows us to construct several explanatory variables: gender, dichotomous variables indicating whether the child is the oldest or the youngest of the family, age of the child which, given the controls for relative age effects, cov-

[10]There are two other reasons why our outcomes are rather insensitive to measurement error. The first reason is that the IQ measure we apply in this paper (*Henmon-Nelson Test of Mental Ability*) does not suffer much from measurement error. Buros (1959 p.342) reports of high reliability ratios ranging from 0.87 to 0.94 creating only a small downward bias in the ability estimates. The second reason is that potential measurement error due to time and cohort effects are ruled out at forehand because all the parents in our sample took the IQ test in 1956 when they were about 16 to 17 years old.

ers a cohort effect, and a dichotomous variable distinguishing adopted children from children living with their biological parents.

Two other explanatory variables are common to all children of a family. First, the ability variable e_{t-1} is the respondent's Henman-Nelson IQ score measured during junior year in high school, that is, in 1956; see Section III D above for additional discussion.[11] Second, the income variable y_{t-1} is family income, measured both in 1975 and in 1992. Since income is positively correlated with ability, we need an ability-free income measure (l_{t-1}) to separate income effects from ability effects. Through a procedure outlined in detail in Section V A we identify an income component that is not correlated with observed ability.

The number of original observations in 1957 equals 10317, but we work with a subsample of 5823 families with 18677 children of whom 685 were adopted. Non-response is a threat to the validity of any study. In our case, using 5823 of the original 10317 respondents gives the appearance that non-response is serious. Of the 4477 respondents who fell outside our sample, about 570 had died by 1992, around 300 could not be located, and some 900 did not cooperate with the 1992 survey. Given that 35 years had elapsed since the initial round in 1957, this response rate is in fact very high. In this paper we do not want to get involved in complications that arise if children are brought up in incomplete families. This eliminates roughly 1800 respondents who did not have a partner and/or children. Finally, the relevant variables must have been measured. In this regard, the main problems exist with the income values: about 1500 families had missing income values in 1975 and 1992 and for about 100 families income was an unrealistically small amount (families with less than \$100 per month in either 1975 or 1992). Excluding these families would cause a substantial reduction in sample size and possibly introduce sample selection bias. The effect could go either way: supposing that households from both the bottom and the top of the income scale were lost, it is unclear a priori whether the bias on the nature/nurture ratio is upward or downward. Yet, rather than losing so many observations due to incomplete measurement, we have imputed our 1992 income measure with available income measures in 1975 using regression analysis. In the end, about 220 observations were excluded from the analysis because of missing income, leaving us with exactly 6476 families. Then about 650 families dropped out because their children were too young, because their children were neither adopted nor the biological offspring of both parents, and because for lack of information on their children's educational attainment. Descriptive statistics on all children in the WLS sample appear in Table 1.

V Results

For a first glimpse at the results, Table 2 divides the sample of children by adoption status and parental IQ. For this table, the sample pertains to children

[11] Note that the respondent is one of the children's parents.

aged 23 or older in an attempt to measure completed schooling only and not to reduce the sample too much. Children with higher ability parents obtain more schooling and are more likely to attend college, and adopted children are at a slight disadvantage. Both of these effects are statistically significant.[12] Most importantly for the purpose of this study, however, the increase in schooling with rising IQ is greater for biological children than for adopted children: biological children enjoy the effect of both genetic and cultural transfers, whereas adopted children benefit only from cultural transfers.

To quantify more precisely how human capital is transferred across different generations, the empirical results will be presented along the lines set out in Section II. Table 3 presents our first regression estimates. The structure of the table is as follows. The first two columns present estimates of censored regressions on years of education using all children in our sample.[13] The last two columns present probit estimates of college attendance where children younger than 23 with no college education are excluded from the analysis.[14] In all regressions the explanatory variables consist of individual and family characteristics. The family level variables are log family income, parental IQ test scores (of either mother or father, measured when teenager), the number of siblings, and an indicator whether or not parents are adoptive parents. The individual control variables are the child's sex, age, indicators whether or not the child is the oldest or the youngest sibling in the family, and the biological status of the child. Each column represents an alternative specification.[15] With respect to the adopted child, the regressions presented in column one and three only include an adoption dummy. Regressions in columns two and four also include the IQ $\times$ adoption interaction effect to isolate that part IQ that stems from genetic transmission. The estimates reported in Panel A are based on all children in the WLS sample. In Panel B we exclude all children younger than 23 to see how sensitive our results are for children who are still of a school going age.

Among family-level variables in the first column we find, not surprisingly, that high income parents stimulate their children's education, and that high

[12]There is one exception. Differences between educational outcomes of adopted children raised in intermediate and high IQ families turn out to be statistically insignificant.

[13]In an earlier version of this study we estimated a set of expanded econometric models that allowed for specific forms of heteroskedasticity and sibling correlation in the disturbance term of the educational attainment equation. The additional parameter estimates were statistically significant; however, the expanded model yielded regression estimates and nature/nurture decompositions that were very similar to those reported below. We do not report these estimates here because they are derived from smaller subsamples of children and thus are not as precise statistically. For more details, see Plug and Vijverberg (2001*b*).

[14]The age limit of 23 years is selected in recognition of the fact that some children enter the work force right after high school and end up attending college after a few years. The exact limit does not impact the estimates much.

[15]In our analyses we use the information of all children raised in one family. Since we use multiple observations of one family, standard errors are not independent within families and are biased downwards. Therefore, we estimate the model with clustered error terms to control for correlation within families and thus present robust standard errors in our tables.

scores on parental IQ tests raises the number of years of schooling.[16] Controlling for parental IQ and income, we find that it does not matter for own birth children whether or not they are raised in the presence of adopted siblings. Later on in the paper where we will discuss possible ability bias due to unobserved characteristics of adoptive parents, we will argue that this is a very nice result. Among individual-level determinants, daughters stay in school somewhat longer than sons. A child who grows up with siblings completes fewer years of schooling than an only child. Moreover, birth order matters: the oldest sibling completes the most schooling, and the youngest sibling the least. The direction of these relative age effects corresponds with the fact that younger children, who have older mothers, are at greater risk of birth defects, and also with the notion that parents focus more intensely on the achievements of their first born child and become more complacent about achievements of younger children. In any case, these relative age effects are in line with some earlier birth order studies (Behrman and Taubman 1986), but the existing literature on birth order effects is far from conclusive (Butcher and Case 1994; Keastner 1997; Hauser and Kuo 1998). Holding birth order constant, we also find that younger children invest more in human capital than older ones: this may be viewed as a cohort effect. To be precise, a child that is born ten years later goes to school about 1.5 years of schooling longer. With respect to being an adopted child, the parameter estimate indicates that, on average, adopted children receive almost one year less schooling than children who are raised by their natural parents, similar to Case, Fin and McLanahan (2001). To get an idea of the magnitude of ability and income effects, the ability elasticities range from 0.20 to 0.22. Income elasticities are much smaller and are between 0.04 and 0.05. Although research designs differ in estimation techniques, in the variables used, in variable specifications, and in sample designs, the income estimates taken from existing literature are comparable to our estimates. For example, Haveman and Wolfe (1995) review the potential determinants of children's schooling and find that income elasticities range from 0.02 to 0.20.

These results, however, cannot indicate the relative importance of nature and nurture on educational attainment. To isolate that part of IQ that stems from genetic transmission we need to include the interacted IQ $\times$ adoption effect. This is done in the second column in Table 3 which conforms to equation (6). The interaction effect turns out to be significantly negative, which corresponds with the idea that intelligence measured as childhood IQ is to a certain extent inherited. That is, the parameter estimates attached to the variable "IQ of parent" indicate the degree to which intelligent parents produce intelligent children who are more likely to obtain more schooling: these parameters combine cultural and biological effects, $b_1 c_1$. The parameters of the interaction effect "adoptee $\times$ IQ of parent" (i.e., $-b_{g1} c_1$) removes the direct genetical ability transfers that cannot occur with respect to adopted children. Thus, ac-

[16]We assume both parents to be in the same IQ class.

cording to these estimates, about 74 percent of all ability transfers relevant for educational attainment measured in years run through genes. Provided that this model is correctly specified, we may conclude that genetics are the primary factor in explaining schooling differences among children.[17]

In columns three and four we replace the dependent variable with college attendance. The findings are very similar to those reported in the previous two columns. Both parental IQ and parental income increase the probability of having a college education. The coefficient on being an adoptee is quite large and suggest that adopted children have about 15 percent smaller change on attending college. In the fourth column we examine the importance of nature and nurture on college attendance and find a negative significant interaction effect. The nature component relevant for college education drops slightly to 68 percent, but the observation remains that most of the ability effect relevant for school achievement can be attributed to family genes.

The results in Panel A are obtained from the sample of all children. We include children who are still in school in our analysis for two reasons. First, deleting these observations from the analysis would cause the results to be biased because we lose all our school going children who are relatively young and end up with a sample where younger children with less education are overrepresented. Second, we preferably work with the largest possible set of adopted children. If we would drop all censored observation, we lose relatively more adoptees because in our sample adopted children are relatively young.[18] To examine whether our results are affected by young children or teenagers who are still in school, we report in panel B the parameter estimates computed from the subsample that is older than 23 and has 4267 fewer observations. We find that all effects are rather similar to those found in Panel A. The nature component of the IQ transfers is 83 percent when the dependent variable is years of schooling, which is slightly larger than for the full sample of children. Using college education as the dependent variable yields the same nature component. From here on out, we shall concentrate on estimation results obtained from the full sample, since the older subsample yields similar conclusions.

All these nature values are very close to those found by Jensen (1972, 1973) and more recently Behrman and Taubman (1989).[19] The standard errors on our nature and nurture components, calculated using the delta method, are

[17]The model could be expanded in interesting ways with other interaction terms. For example, one might surmise that more intelligent children benefit more from a more nurturing home environment. Thus, equation (3) would be expanded with the term $+c_4 e_t y_{t-1} + c_5 e_t x_t$. It is not difficult to show that the nature-nurture decomposition is still identified. If smart children would indeed benefit relatively more from smart parents, the intergenerational transmission mechanism would become non-linear, implying that our results are sensitive for specification error. This is not the case in our paper: whether we model the outcome variable as continues (years of education) or dichotomous (college attendance) the nature/nurture decomposition remains rather unaffected.

[18]In our sample adopted children are almost 3 years younger than biological children, and, as a consequence, are more likely to be in school still. See Table 1.

[19]Note that they arrive at their nature estimate using variance decomposition on a sample

quite large. In fact, at this stage the estimates allow the notion that education-related IQ transfers are all genetics. However, we already mentioned that these estimates are subject to several important caveats that are common to adoption experiments like ours. We now turn to the question what happens with the estimates if we incorporate several of these caveats directly into the model.

A Income and nurturing ability effects

Our estimated impact of family income on children's educational attainment is based on family income measured in 1992 when about 75 percent of all children have just ended their schooling career. Observing similar income effects, Mayer (1997) argued that because family income is mostly generated after the educational outcome is observed, the positive impact of family income rather points to unobserved ability effects than to income effects per se.[20] Moreover, as argued in Section III A, when (observed) parental ability generates income which in and of itself is contributing to nurture, our nature/nurture percentage needs to be reexamined.

To find out how much these specific ability effects influence our nature and nurture estimates, we need to identify that part of income that is unrelated with parental ability and use this new income measure in our analysis instead of family income itself. To isolate that component of income that is unrelated with parental ability, we regressed parental income on parental IQ, parental education and education of both grandparents and obtained its residual.[21] Thus, even when we wish to focus simply on a nature/nurture ratio related to IQ-related ability, this residual abstracts from a broader set of ability traits, which allows a cleaner estimate of the nurture effect of income. In Table 4, this ability-free income component enters into the children's schooling equation as the parental income measure l_{t-1} defined in equation (7). As seen in the first column, the parental income effect is cut in half but remains significantly different from zero. At the same time, the influence of IQ increases since it picks up that part of income that is generated by ability. The size of the genetic component $(b_{g1}c_1)$

of relatives and twins while we decompose ability effects in the form of regression slopes on a sample of biological and adopted children.

[20] In an earlier version of this study we estimated the same specifications with family income measured in 1975 instead of 1992 family income and found that the estimated impact of income did not substantially change. The fact that we have similar income estimates at different stages of the parental life cycle is also consistent with life cycle theories of consumption, which permits temporal income to have an impact on educational attainment even before schooling is commenced or after schooling is completed. That is, if parents borrow money to finance their offspring's education, future income affects current expenditures. Vice versa, if parents save income to finance their offspring's education, past income affects current educational spending. Altogether, it is difficult to conclude that it is ability rather income that matters on grounds of estimates like these.

[21] Technically, l_{t-1} is orthogonal to IQ in the sample of household data but not in the sample of children because the unit of observation differs and households contain different numbers of children.

remains the same. The constancy of $b_{g1}c_1$ is striking: it points to a genetic transfer of a particular magnitude. The increase in b_1c_1 shows that a portion of the income effect comes from a cultural/environmental transfer of IQ, namely a channel that works through income (in line with Dickens and Flynn, 2001:352-353). Taking this into account, we find that the genetically transmitted portion of IQ is almost 60 percent (the ratio of $b_{g1}c_1$ over b_1c_1).[22] In the second column of the table, we drop family income from the model, which creates a test whether these nature and nurture estimates are affected by ability that is hidden in family income. This does not appear to be a problem: the estimated effects remain almost identical.[23] In columns three and four we switch to college education as the explained variable. These results are in line with what was already observed earlier: the impact of parental IQ increases, and the impact of interacted IQ remains the same. The consequence is that our nature estimate falls and turns out to be almost 55 percent.

To sum up, biological siblings resemble one another not only because they have similar genes but also because they share the benefits of a family income that is driven by related parental genes, allowing them to experience a family environment that is very similar. We show that if we ignore the correlation between ability and family income, our nature estimates is wrongfully crediting genes. If we remove this correlation, we find that our estimated nature ratio falls. Note that this time both nature and nurture are statistically responsible for educational success, but the observation remains that the largest part of ability relevant for schooling is still passed on genetically.

B Differentials in upbringing

There are many reasons why parents may treat their adopted children differently from their biological children (Case, Lin and McLanahan, 2000). Some parents may do so because there are ability differences between their adopted and biological children. To allow parents to spend different amounts of money on the education of their biological and adopted children in response to ability differences, we let the income parameters differ between adopted and biological children.

These estimates are presented in Panel A of Table 5. The estimates in the first column indicate that interacted family income effects are not significant and that the impact of family income on years of schooling is statistically identical

[22]We also experimented with alternative measures of ability free income. The results we obtained are very similar to the ones we present in this paper. For a more detailed exposition on how we identify alternative ability-free income measures, see Plug and Vijverberg (2001*a*). That paper examines the influence of transitory and permanent income on the educational attainment of children.

[23]In the WLS, lack of information on income is one factor that is responsible for a reduction in sample size. Because columns one and two (and also three and four) present almost identical estimates, it does not appear that eliminating these missing observations introduces sample selectivity bias.

for both biological and adopted children. As in the previous subsection, it is important to verify whether these results hold up when we acknowledge that ability effects run through income as well.[24] Thus, in the second column of Table 5, we replace total family income (y_{t-1}) with that part of family income that is unrelated with parental ability (l_{t-1}) and thus estimate equation (10). The interacted income variables yield slightly larger negative but still insignificant coefficients.[25] The last two columns repeat the test for treatment differentials in the context of college attendance. The estimated total income effects do not indicate differentiation among adopted and biological children, but for ability-free income we now find a marginally significant negative estimate on interacted income, demonstrating that parents might have a tendency to invest less in the college education of their adopted children.

These observations give no compelling reason to believe that, with respect to the money invested in schooling, parents make a clear distinction as to whether the child in question was adopted or not. But if they do, it appears that parents have a mild tendency to favor their own birth children over their adopted children. Regardless, there is no serious cause for alarm concerning our nature/nurture decomposition. Indeed, these results remain unaffected: about 55 to 60 percent of parental IQ relevant for schooling is genetically transmitted.

C Adoption as a natural experiment?

The analysis so far has treated adoption as a natural experiment. This might be debatable, as was already clear from the discussion in Section III C and descriptive statistics in Tables 1 and 2. As compared to their parents' biological children, adoptees are almost three years younger, have less education, and live in higher-income families with better educated parents who have a higher IQ. In this Section we take up the argument that adopted children and their parents are not necessarily randomly drawn from the population at large: how concerned we should be about that?

First of all, what happens if adoptees are on average low ability children? Our nature/nurture decomposition is affected only if this relative ability deficit of adoptees is somehow correlated with and biases the estimated impact of one of the other variables. Regarding family income we already showed that parents applied similar income allocation rules for their adopted and non-adopted children. But the question here asks what happens if we let all of the other coefficients vary by adoption status as well. If it turns out that those parameters are different, indeed, we should worry about the unobserved characteristics

[24]The complication is that differences between parental income effects relevant for educational outcomes of biological and adopted children are again tainted by genetic transfers and thus do not necessarily identify treatment differentials.

[25]Parallel findings are obtained in the study of Sacerdote (2000). With only family income as an explanatory variable he finds income coefficients for adoption and natural families that are statistically identical when explaining the years of schooling of children.

(ability, endowments, race, et cetera) of adoptees affecting our nature and nurture decomposition. Panel B of Table 5 reports on this. Nearly all interaction effects are statistically insignificant, with only two exceptions. With years of education as the explained variable, the age effect for adoptees is somewhat flatter than for biological children: there is more differentiation among biological children of different age than among adoptees. But this effect disappears when college attendance becomes the explained variable. In fact, there is only one effect that is consistently different between adoptees and non-adoptees across all four specifications and that is the parental IQ effect, which obviously corresponds with the idea that intelligence measured as childhood IQ is to a certain extent inherited. The likelihood ratio tests also indicate that except for parental IQ effects the other coefficients do not vary systematically by adoption status. Notice that the implied impact of the family genes drops about 5 percentage points from what was observed earlier. From this we conclude that selectivity of adoptees and their omitted ability bias are not our biggest concern.

Next, what happens if adoptive parents are better parents? Previous tables already included a dummy variable to control for the unobserved parenting qualities that adoptive parents might have. This coefficient is identified by the variation in educational attainment of biological children who live with and without an adopted sibling. We find that for years of education the parameter estimate on the adoptive household dummy is small, negative and statistically insignificant. For college attendance, we find that the same parameter estimate is small, positive and statistically insignificant. Note however that this variable does not necessarily remove the bias if parenting qualities are correlated with other variables. To get an idea whether this is a serious threat for our nature and nurture estimates, we employ the subsample of biological children to estimate separate schooling models for children living either with or without adopted siblings. Again, if these estimates are very different we need to worry about unobserved characteristics in adopting families. Panel C of Table 5 reports on these differences: likelihood ratio tests show that none of the estimated coefficients varies across adoptive and non-adoptive families.

In a related vein, the subsample of children, adopted and biological, that are living in adoptive households is also of interest. Since the adoptive environment is held constant, we indirectly control for variations in parental quality of adoptive parents thus for the omitted ability bias that it entails. Again, the nature and nurture estimates (Panel D in Table 5) are in line with what we observed in previous tables. On the basis of these results, we tentatively conclude that unobserved but distinctive parenting qualities of adoptive parents are not relevant and that the estimate of the nature/nurture ratio is, as far as this argument goes, sound.

Finally, what happens if adopted children and their parents are not necessarily connected at random? In fact, this is our main concern. Regarding the question how adoptees are selected into adoptive households, there are of two potential sources of selection bias. First, there is selectivity because of adoptions by relatives. These adoptions involve primarily parents who raise and adopt their relatives' children, or parents who raise and adopt children whom their partner brings into the marriage. Consequently, our estimates will overstate the nurture effect, since part of the nurture effect as identified by adoption is in fact still genetic. We believe we have some reason to argue that this is not as great a problem as it might seem. The WLS provides information on parent/child relationships and uses nine different classifications: biological, adopted, step, foster, grandchild, legal ward, niece/nephew, other non-relatives, and child of partner/lover. On the basis of this classification, if we treat all parent/child pairs other than "biological" and "adopted" as related adoptions, we end up with 1085 children. Of all adoptions in our sample, this amounts to more than 60 percent, which is very close to the percentages found by Stolley (1993).[26] However, in our analysis we excluded all these alternative family-related relationships and assumed that all of our 685 adoptees are unrelated to the family of rearing. This assumption seems plausible enough. Second, if adoptees are unrelated but high ability parents manage to adopt children from high ability natural parents, we will overestimate the role effect of the family environment.[27] To test how serious this selection effect really is, one would need information on the socioeconomic background of the biological parents of adopted children. The WLS does not provide this information. Hence, direct testing is not possible.[28] Instead we provide an upper bound on the nurture effect.

VI Concluding remarks

The intergenerational mobility literature shows persistently that children raised in highly educated families are more educated than children raised in less educated families. This paper examines whether ability measured as IQ is the dominant factor behind this family connection. We find that parental IQ matters for the educational attainment of children. We further exploit a special feature of the dataset and disentangle persistence effects caused by nature and nurture. Using information whether these children are their parents' own offspring as opposed to adopted children, we find that if we interpret family income as an environmental factor, about 70 to 75 percent of the ability effects relevant for school achievement can be attributed to genetic effects. We then explore reasons why these nature estimates suffer from selectivity effects that are common to adoption experiments, and test how serious these selection effects really are. Results can be summarized as follows:

[26]Stolley (1993) reports that of all adoptions in the early seventies more than 50 percent were related adoptions and that this percentage rose to almost 75 percent in the early eighties.

[27]It is also possible that adoption agencies generate this selection bias when they use corresponding abilities of both natural and adoptive parents as a matching strategy.

[28]Fears about adoption selectivity bias should be allayed at least somewhat by the lack of impact of the adoptive family dummy variable in the various stages of our analysis.

- These nature estimates are biased because of ability effects run through family income as well. Purging contributions of ability to the measured family income causes the genetic ability transfer percentage to drop to 55 to 60 percent.

- The nature estimates may be biased because of treatment differentials among adopted and biological children. We find no clear evidence of a different upbringing. Again, about 55 to 60 percent of all ability relevant for schooling is genetically passed on.

- The nature estimates are downwardly biased if there is selective placement of adoptees. This selection effect enables us to estimate the minimum share of inherited ability. We find that at least 50 percent of all ability relevant for schooling is genetically passed on.

This study thus indicates that it is rather complicated to find out which factors are exactly behind this family connection. From our exercise we learn at least two things: (i) that it is only to a certain extent that ability is an important factor in explaining the educational attainment of children; (ii) but that the largest part of ability relevant for education is inherited.

Having said this, let us take one step back and evaluate what was found. One feature of the data is that parental IQ is measured for only one parent. This is rather unfortunate from a research perspective – but also quite unavoidable in view of the survey design, as it is impossible to measure teenage IQ of every potential marriage partner. The effect of parental IQ as it is estimated in this paper represents both the direct transfer (genetic and otherwise) from the given parent and the indirect transfer from the other parent, which is due to assortative mating and the ensuing correlation of the parents' ability. Even if the genetic transfer follows laws of nature, the cultural component of the transfer may well differ between fathers and mothers. Thus, our numbers represent, in a sense, a reduced-form estimate of a complex transfer process that, if unpacked, could yield surprising and fascinating details. A recent study by Behrman and Rosenzweig (2002) considers the impact of parental schooling on child schooling in the presence of unmeasured ability and assortative mating. Using twin data, they come to the surprising conclusion that the schooling of mothers has little if any impact on the schooling of children, holding everything else (including unobserved ability factors of both parents) constant. A similar decomposition with the present data could be enlightening but is left for future research.

As a final note, the public policy implications of these findings are rather significant. Much money is spent on the educational system, e.g., on improvement of facilities, on lowering student teacher ratios, on curriculum development, et cetera. The underlying rationale is to create an environment in which students flourish. If nurture drives the success of children in school, a one-time equalization of educational opportunities will erase past inequalities in schooling; the next generation of children will start out equally. On the other hand, if children's ability is determined to a large extent genetically, a nurturing school environment may help the less able children to overcome their disadvantage only at great cost; moreover, the ability of the next generation of children is still unequally distributed. In the former case, the rationale behind educational expenses is primarily productive and only once redistributive; in the latter case, educational expenses are repeatedly redistributive and only secondarily productive. This tension defines the political debate on educational financing and explains the boom-and-bust nature of educational budgeting.

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TABLE 1

Means and Standard Deviations of Selected Variables in WLS Sample

Variable	Adoptees		Own birth children	
Years of education	12.705	*2.697*	13.549	*2.546*
College attendance[a]	0.631	*0.482*	0.632	*0.482*
Still in school (censored)	0.391	*0.488*	0.234	*0.423*
Gender (daughter)	0.494	*0.500*	0.492	*0.499*
Age	23.947	*5.250*	26.658	*4.900*
Number of siblings	2.194	*1.661*	2.850	*1.681*
Oldest sibling[b]	0.400	*0.490*	0.308	*0.461*
Youngest sibling[b]	0.354	*0.478*	0.293	*0.455*
Raised in adoptive family	1.000	*0.000*	0.031	*0.173*
Log family income	11.095	*0.647*	10.935	*0.694*
IQ parent	10.402	*1.445*	10.056	*1.417*
Eeducation of father in years	14.299	*2.935*	13.441	*2.594*
Education of mother in years	13.276	*1.941*	12.796	*1.641*
Number of observations	685		17992	

Note–Standard deviations in italics.
[a]Mean and standard deviations are calculated for respectively 507 and 15805 observations. For this variable children younger than 23 with less than 12 years of education are not included.
[b]If child is only child, he/she is considered oldest sibling.

TABLE 2
MEANS OF EDUCATIONAL VARIABLES FOR ADOPTEES AND OWN BIRTH CHILDREN RAISED IN LOW, INTERMEDIATE AND HIGH ABILITY FAMILIES

	Adoptees	Own birth children
	Low ability families	
Years of education	13.201	13.409
College attendance	0.464	0.469
Number of observations	114	4951
	Intermediate ability families	
Years of education	13.602	14.048
College attendance	0.568	0.599
Number of observations	146	4858
	High ability families	
Years of education	13.685	14.676
College attendance	0.597	0.718
Number of observations	154	4313

NOTE–All children younger than 23 are excluded.

TABLE 3

Estimates of the Child's Schooling Model: Baseline Specification

	Years of Education		Years of Education		College Attendance		College Attendance	
	A: Full sample							
Intercept	8.367	*0.463****	8.279	*0.463****	-1.354	*0.281****	-1.395	*0.282****
Daughter	0.156	*0.035****	0.156	*0.035****	0.151	*0.022****	0.151	*0.022****
Age	-0.144	*0.006****	-0.144	*0.006****	-0.112	*0.003****	-0.112	*0.003****
Adoptee	-0.847	*0.163****	1.686	*0.771***	-0.410	*0.099****	0.718	*0.482*
IQ of parent (b_1c_1)	0.321	*0.016****	0.330	*0.016****	0.156	*0.010****	0.159	*0.010****
Adoptee × IQ of parent ($-b_{g1}c1$)			-0.244	*0.072****			-0.109	*0.045***
Log income	0.636	*0.038****	0.635	*0.038****	0.302	*0.023****	0.302	*0.023****
Raised in adoptive family	-0.134	*0.137*	-0.139	*0.137*	0.009	*0.080*	0.007	*0.080*
Number of siblings	-0.153	*0.015****	-0.152	*0.015****	-0.056	*0.009****	-0.056	*0.009****
Oldest sibling	0.475	*0.039****	0.475	*0.039****	0.323	*0.025****	0.323	*0.025****
Youngest sibling	-0.216	*0.045****	-0.215	*0.045****	-0.035	*0.028*	-0.035	*0.028*
Mean loglikelihood	-1.770		-1.769		-0.576		-0.576	
Sample size N	18677		18677		16312		16312	
Nature effects (b_{g1}/b_1)			0.739	*0.216****			0.681	*0.278****
Nurture effects (b_{c1}/b_1)			0.261	*0.216*			0.319	*0.278*
	B: Subsample of children older than 23 years of age							
Intercept	9.354	*0.505****	9.283	*0.505****	-2.692	*0.302****	-2.731	*0.303****
Daughter	0.127	*0.036****	0.126	*0.036****	0.153	*0.022****	0.152	*0.022****
Age	-0.164	*0.007****	-0.164	*0.007****	-0.069	*0.004****	-0.069	*0.004****
Adoptee	-0.910	*0.138****	1.815	*0.784***	-0.386	*0.083****	0.786	*0.486*
IQ of parent (b_1c_1)	0.306	*0.016****	0.314	*0.016****	0.160	*0.010****	0.164	*0.010****
Adoptee × IQ of parent ($-b_{g1}c_1$)			-0.263	*0.074****			-0.113	*0.046***
Log income	0.606	*0.039****	0.605	*0.039****	0.309	*0.024****	0.309	*0.024****
Raised in mixed family	-0.052	*0.123*	-0.058	*0.122*	-0.013	*0.074*	-0.014	*0.073*
Number of siblings	-0.133	*0.015****	-0.132	*0.015****	-0.070	*0.010****	-0.069	*0.010****
Oldest sibling	0.508	*0.041****	0.509	*0.041****	0.243	*0.025****	0.243	*0.025****
Youngest sibling	-0.251	*0.048****	-0.250	*0.505****	-0.104	*0.030****	-0.103	*0.030****
Mean loglikelihood	-2.053		-2.052		-0.619		-0.619	
Sample size N	14536		14536		14536		14536	
Nature effects (b_{g1}/b_1)			0.836	*0.231****			0.689	*0.274****
Nurture effects (b_{c1}/b_1)			0.164	*0.231*			0.311	*0.278*

Note–Robust standard errors are in italics; * significant at 10% level,** significant at 5% level, *** significant at 1% level. Columns two and four estimate the schooling model according to equation (6).

TABLE 4

ESTIMATES OF THE CHILD'S SCHOOLING MODEL: ADJUSTING FOR ABILITY EFFECTS THAT RUN THROUGH INCOME

	Years of Education		Years of Education		College Attendance		College Attendance	
Intercept	14.726	*0.259****	14.743	*0.256****	1.707	*0.159****	1.704	*0.156****
Daughter	0.156	*0.035****	0.150	*0.034****	0.147	*0.021****	0.140	*0.021****
Age	-0.153	*0.006****	-0.153	*0.006****	-0.116	*0.003****	-0.116	*0.003****
Adoptee	1.650	*0.769***	1.611	*0.741***	0.674	*0.477*	0.621	*0.463*
IQ of parent (b_1c_1)	0.408	*0.016****	0.409	*0.015****	0.191	*0.010****	0.194	*0.010****
Adoptee $\times$ IQ of parent ($-b_{g1}c_1$)	-0.241	*0.072****	-0.241	*0.070****	-0.104	*0.045***	-0.102	*0.043***
Log ability free income	0.320	*0.037****			0.150	*0.022****		
Raised in adoptive family	-0.110	*0.141*	-0.076	*0.143*	0.021	*0.080*	0.042	*0.080*
Number of siblings	-0.167	*0.015****	-0.178	*0.014****	-0.062	*0.025****	-0.068	*0.009****
Oldest sibling	0.496	*0.039****	0.495	*0.039****	0.330	*0.025****	0.330	*0.024****
Youngest sibling	-0.238	*0.045****	-0.240	*0.044****	-0.043	*0.028*	-0.043	*0.027*
Mean loglikelihood	-1.783		-1.786		-0.584		-0.586	
Sample size N	18677		19347		16312		16910	
Nature effects (b_{g1}/b_1)	0.591	*0.174****	0.590	*0.167****	0.543	*0.229****	0.525	*0.220****
Nurture effects (b_{c1}/b_1)	0.409	*0.174****	0.410	*0.167****	0.457	*0.229***	0.475	*0.220***

NOTE–Robust standard errors are in italics; * significant at 10% level,** significant at 5% level, *** significant at 1% level. Columns one and three estimate the schooling model according to equation (7).

TABLE 5

Various Specification Tests

	Years of education		Years of education		College attendance		College attendance	
	A: Different allocation rules[abc]							
Log income	0.639	*0.039****			0.307	*0.023****		
Adoptee × log income	-0.144	*0.205*			-0.151	*0.095*		
Log ability free income			0.324	*0.038****			0.154	*0.022****
Adoptee × log ability free income			-0.185	*0.210*			-0.177	*0.100**
Nature effects (b_{g1}/b_1)	0.690	*0.216****	0.602	*0.174****	0.576	*0.283****	0.557	*0.229****
Nurture effects (b_{c1}/b_1)	0.310	*0.216*	0.398	*0.174***	0.424	*0.283*	0.443	*0.229**
	B: Fully interacted schooling models for adopted and own birth children[ac]							
Age	-0.148	*0.006****	-0.156	*0.006****	-0.114	*0.003****	-0.117	*0.003****
IQ of parent (b_1c_1)	0.329	*0.016****	0.407	*0.016****	0.159	*0.010****	0.191	*0.010****
Adoptee × age	0.068	*0.026****	0.070	*0.026****	0.029	*0.021*	0.030	*0.021*
Adoptee × IQ of parent ($-b_{g1}c_1$)	-0.214	*0.072****	-0.227	*0.071****	-0.083	*0.045**	-0.097	*0.044***
Likelihood ratio test	10.56		10.87		8.96		9.40	
p-value	0.103		0.092		0.175		0.152	
Nature effects (b_{g1}/b_1)	0.650	*0.214****	0.558	*0.172****	0.526	*0.279**	0.507	*0.225***
Nurture effects (b_{c1}/b_1)	0.350	*0.214*	0.442	*0.172****	0.474	*0.279**	0.493	*0.225***
	C: Schooling models for own birth children, fully interacted with "raised in adoptive family"[ad]							
Likelihood ratio test	3.43		1.94		3.09		3.17	
p-value	0.904		0.982		0.928		0.923	
Sample size N	17992		17992		15805		15805	
	D: Fully interacted schooling models for adopted and own birth children raised in adoptive family[ac]							
IQ of parent (b_1c_1)	0.303	*0.096****	0.391	*0.095****	0.164	*0.058****	0.200	*0.057****
Adoptee × IQ of parent ($-b_{g1}c_1$)	-0.188	*0.122**	-0.214	*0.122**	-0.088	*0.072*	-0.106	*0.070*
Sample size N	1244		1244		951		951	
Nature effects (b_{g1}/b_1)	0.619	*0.250****	0.547	*0.199****	0.540	*0.312**	0.528	*0.248***
Nurture effects (b_{c1}/b_1)	0.381	*0.250*	0.453	*0.199***	0.460	*0.312*	0.472	*0.248**

Note–Robust standard errors are in italics; * significant at 10% level,** significant at 5% level, *** significant at 1% level.

[a] Columns one and three include income among the explanatory variables. Columns two and four include ability free income.

[b] Columns two and four estimate the schooling model according to equation (10).

[c] Only selected parameter estimates are reported. Estimates of the full models are available on request.

[d] None of the interacted estimated parameters is individually statistically significant.