MATERNAL EDUCATION, HOME ENVIRONMENTS, AND THE DEVELOPMENT OF CHILDREN AND ADOLESCENTS

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Abstract
We study the intergenerational effects of maternal education on children's cognitive achievement, behavioral problems, grade repetition, and obesity, using matched data from the female participants of the National Longitudinal Survey of Youth 1979 (NLSY79) and their children. We address the endogeneity of maternal schooling by instrumenting it with variation in schooling costs during the mother's adolescence. Our results show substantial intergenerational returns to education. Our data set allows us to study a large array of channels which may transmit the effect of maternal education to the child, including family environment and parental investments at different ages of the child. We discuss policy implications and relate our findings to the literature on intergenerational mobility. (JEL: I21, I24, J13, J24)

1. Introduction

The following quote is from Sara McLanahan's presidential address to the Population Association of America, in which she documents a striking increase in inequality in children's home environments across families where mothers have different levels of education.

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> ... the forces that are driving the transition are leading to two different trajectories for women—with different implications for children. One trajectory—the one associated with delays in childbearing and increases in maternal employment—reflects gains in resources, while the other—the one associated with divorce and nonmarital childbearing—reflects losses. Moreover, the women with the most opportunities and resources are following the first trajectory, whereas the women with the fewest opportunities and resources are following the second. (McLanahan 2004)

The trends documented in these and other papers, starting with Coleman et al. (1966), are cause for great concern because the home environment is probably the best candidate for explaining inequality in child development.[1]

To address this problem, McLanahan (2004) ends her paper by proposing a set of changes to the welfare system. The effectiveness of such proposals is still to be assessed. However, given that home environments are rooted in the experiences of each family, they are probably difficult to change if we rely only on the welfare system. Furthermore, more direct interventions require invading family autonomy and privacy and are notoriously difficult to enforce. Therefore, one possible alternative is to target future parents in their youth, by affecting their education, before they start forming a family. In this paper we assess the potential for such a policy, by estimating the impact of maternal education on home environments and on child outcomes.

Our analysis is based on the Children of the National Longitudinal Survey of Youth of 1979, a data set with very detailed information on maternal characteristics, home environments, and child outcomes. The data allow a unified treatment of different aspects of child development across ages, including cognitive, noncognitive, and health outcomes.[2] Furthermore, using this single data set it is possible to estimate the impact of maternal education not only on parental characteristics like employment, income, marital status, spouse's education, age at first birth, but also on several aspects of parenting practices. Our paper provides a detailed analysis of the possible mechanisms mediating the relationship between parental education and child outcomes. The novelty of our work is in the systematic treatment of a very large range of inputs and outputs to the child development process, at different ages of the child, in a unified framework and data set. We also compare the relative roles of maternal education and cognitive ability,[3] and we show how the role of maternal education varies with the gender and race of the child, and with the cognitive ability of the mother.

We show that maternal education has positive impacts both on cognitive skills and behavioral problems of children, but the latter are more sustained than the former. This is perhaps because behavior is more malleable than cognition (e.g., Carneiro

1. For example, Jencks and Phillips (1998), Cameron and Heckman (2001), Fryer and Levitt (2004, 2006, 2007), Carneiro, Heckman, and Masterov (2005), Todd and Wolpin (2007) and others show how differences in home environments account for a large share of the black–white test score gap.

2. The dynamic aspect of cognitive and noncognitive skill formation is emphasized in the recent literature on child development, such as Carneiro and Heckman (2003), Cunha et al. (2006), Cunha and Heckman (2007), and Todd and Wolpin (2003).

3. Maternal cognitive ability is a central determinant of child's cognitive achievement. According to Todd and Wolpin (2007), racial differences in mother's cognition account for half of the minority–white test score gap among children.

and Heckman 2003). Especially among whites, there is considerable heterogeneity in these impacts, which are larger for girls, and for mothers with higher cognition. This is also a feature of many education interventions, in early childhood and beyond.

More-educated mothers are more likely to work and work for longer hours, especially among blacks. Nevertheless, there is no evidence that more-educated mothers do less breastfeeding, spend much less time reading to their children, or even taking them on outings. This is important because some studies suggest that maternal employment may be detrimental for child outcomes if it leads to reduced (quality) time with children.

Due to the nature of the data, this paper focuses on the effect of maternal, but not paternal, schooling. Because of assortative mating, part of the effects we find may be driven by the father's schooling through a mating effect. We interpret our estimates as total effects in that they capture both the direct effect of maternal education, and an indirect effect through father's education. Looking at the magnitudes of our estimates and those in the literature, we argue that it is unlikely that the total effect is driven exclusively by assortative mating.

The key empirical problem we face is controlling for the endogeneity of mother's schooling: factors that influence the mother's decision to obtain schooling may also affect her ability to bring up children or may relate to other environmental and genetic factors relevant to child outcomes. To deal with this issue we exploit differential changes in the direct and opportunity costs of schooling across counties and cohorts of mothers, while controlling both for permanent differences and aggregate trends as well as numerous observed characteristics such as mother's ability. The variables we use to measure the costs of education include local labor market conditions, the presence of a four-year college, and college tuition at age 17, in the county where the mother resided when she was 14 years of age. These variables have previously been used as instruments for schooling by Card (1993), Kane and Rouse (1993), Currie and Moretti (2003), Cameron and Taber (2004), and Carneiro, Heckman, and Vytlacil (2011), among others. We also control for county fixed effects, to allow for permanent differences in area characteristics and in the quality of offered education, as well as for mother's cohort effects, to allow for common trends, which means that results are only driven by differential changes in local costs of education between counties and cohorts.

One potential problem is that our instruments may be weak. We therefore estimate some of our models by limited information maximum likelihood (LIML), as suggested by Staiger and Stock (1997). The resulting estimates are larger in absolute value than our original two-stage least-squares (TSLS) estimates and further away from the OLS coefficients, but also have larger standard errors (as predicted by Blomquist and Dahlberg 1999).

Recently, several papers have appeared on this topic dealing with the endogeneity issue in different ways. Behrman and Rosenzweig (2002) compare the schooling attainment of children of twin mothers and twin fathers (with different levels of schooling). They find that the effect of father's education is strong and large in magnitude, but the effect of maternal education on child schooling is insignificant (see also Antonovics and Goldberger 2005, Behrman and Rosenzweig 2005).

A set of recent papers addresses endogeneity of parental schooling in an instrumental variable approach, exploring changes in compulsory schooling laws. This is likely to affect parental educational choice mostly at the low end of the educational distribution, and the corresponding effects need to be interpreted accordingly. Black, Devereux, and Salvanes (2005) study the effect of parental education on children's educational attainment in Norway. Regarding the effect of maternal education on child education (which is closest to this paper), their results indicate a larger effect on sons than on daughters. Although we cannot explicitly compare this effect to our study because our children are still too young to observe completed educational attainment, we also find indications that the effect of maternal education differs between girls and boys, but we do not find a uniform pattern of larger effects on boys. Oreopoulos, Page, and Stevens (2006) study grade repetition in the US as outcome, and their IV estimates are of similar magnitudes to the ones reported here. Chevalier, Harmon, O'Sullivan, and Walker (2010) study compulsory schooling laws in the UK, but emphasize the relative effect of parental education and income.[4] It is important to stress that, because they look at compulsory schooling, all these papers study mothers who are at the margin between taking more schooling or not at very low levels of schooling. Maurin and McNally (2008) study the effect of temporarily lower examination standards for a particular cohort in France, so that the affected individuals are those at the margin of entering higher education; the results indicate larger effects of fathers' education on child grade repetition than the effects of maternal education we find in this paper.

Currie and Moretti (2003) find that maternal education has significant effects on birthweight and gestational age. Maternal education also affects potential channels by which birth outcomes are improved such as maternal smoking, the use of prenatal care, marital status, and spouse's education. Related studies by Plug (2004), Sacerdote (2002), and Bjoerklund, Lindahl, and Plug (2006), which are based on adoptions data, compare the correlation between parental schooling and the outcomes of biological children, with the correlation between foster parents' schooling and adopted children's schooling. Adoption studies inform the debate by separating the effect of environmental and genetic factors (although their standard design can be problematic if there are substantial interactions between genes and environments), but they do not tell us directly about the causal effect of parental schooling on child outcomes. These studies cannot distinguish between the role of parental schooling and ability in the provision of better environments.[5] The general sense we get from the whole literature is that the

4. Chevalier (2004) and Galindo-Rueda (2003) also exploit the effect of compulsory schooling laws in the United Kingdom.

5. Plug (2004) finds weak effects of adoptive mother's schooling on child's schooling but large effects of father's schooling, and Bjoerklund, Lindahl, and Plug (2006) find strong effects of both adoptive father and mother's schooling. Sacerdote (2002) argues that a college-educated adoptive mother is associated with a 7% increase in the probability that the adopted child graduates from college.

results are quite disparate and a consensus has not formed yet (see Holmlund, Lindahl, and Plug 2010).[6]

The plan of the paper is as follows. In the next section we describe the data, followed by an explanation of our empirical strategy. Then we discuss our results on the impact of mother's schooling on child outcomes, followed by results on the possible mechanisms through which schooling may operate. Finally, we present a sensitivity analysis and a concluding section.

2. Data

We use data from the National Longitudinal Survey of Youth (NLSY79). This is a panel which follows 12,686 young men and women, aged between 15 and 22 years old in the first survey year of 1979. Surveys are conducted annually from 1979 until 1994, and every two years from 1994 onwards. We use data up to 2008.

To ensure that our sample is drawn according to predetermined characteristics, we limit the analysis to the main cross-sectional sample and the over-sample representative of blacks and hispanics.[7] Attrition rates are very low (see CHRR 2002). As we describe in what follows, for our purpose only the females of the NLSY79 are of interest.

We measure mother's schooling as completed years of schooling.[8] We are interested in the mother's schooling at the time when the outcome is measured.[9]

The data contain detailed information on family background of the mother, namely her parents' schooling, and whether she was raised by both her biological parents. Furthermore, we know the mother's score in the Armed Forces Qualification Test (AFQT), administered in 1980, which we use as a measure of mother's cognitive ability.[10] The original AFQT score may be influenced by the amount of schooling taken up to the test date, but it is possible to estimate the effect of schooling on the test score (see Hansen, Heckman, and Mullen 2004), and then construct a separate measure of

6. Holmlund, Lindahl, and Plug (2010) replicate the differing findings based on twin studies, adoptions, and instrumental variables within one Swedish data set, suggesting that the differences cannot be fully explained by country specifics or sample characteristics.

7. Apart from the main cross-sectional sample representative of the population, the NLSY79 contains an over-sample representative of blacks and hispanics, an over-sample of economically disadvantaged whites, and a sample of members of the military. In our analysis we exclude the over-sample of economically disadvantaged whites and the sample of the military.

8. In doing so, we follow a large number of existing studies. Although potentially interesting, we do not address the question of how year effects compare to possible degree effects, and leave this question for future research.

9. Occasionally, sample members do not answer this question in the year of interest. In order to include these observations, we take as the measure of schooling the maximum number of completed years reported up to the year of interest.

10. In doing so, we follow a broad strand of literature which argues that the AFQT can be viewed as a proxy for cognitive skills. Exemplarily, Heckman, Stixrud, and Urzua (2006) write "The AFQT is a general measure of trainability and a primary criterion of eligibility for service in the armed forces. It has been used extensively as a measure of cognitive skills in the literature" (see Heckman, Stixrud, and Urzua (2006) for corresponding references).

ability.[11] Throughout the paper, we refer to the AFQT score as this schooling-corrected ability measure, normalized to have mean zero and standard deviation one.

In 1986, another data set, the Children of the NLSY79, was initiated. It follows the children of the female members of the NLSY79 over time and surveys each child throughout childhood and adolescence. We match the information on each child of the NLSY79 to the data of the mother. Even though the NLSY79 surveys a random sample of potential mothers, the design of the children's sample leads to an initial over-sample of children of younger mothers, until all women are old enough and have completed their child-bearing period. In 2000, the women of the NLSY79 have completed an average of 90% of their expected childbearing (CHRR 2002). Nonetheless, as one focuses on older children, the sample eventually becomes increasingly selective.[12] Figure A.1 in the Web Appendix shows the distribution of child birth cohorts in our data. The median child is born in 1986, with first and third quartiles corresponding to 1982 and 1991. The 95th percentile of the distribution is in 1998, so that almost all children in the data reach the 7–8 age bracket, which we focus on in what follows. We also present results for older children, and compare effects between age groups 7–8 and 12–14. We document in the sensitivity analysis that the time pattern we find in that analysis between ages 7–8 and 12–14 is not driven by increasing selectivity.

Table 1 presents an overview of the different outcomes for reference. In order to measure the child's cognitive ability we use the Peabody Individual Achievement Tests (PIAT) in math and reading, which are widely used in the literature. Behavior problems are measured using the Behavior Problems Index (BPI).[13] We also construct grade repetition[14] and child obesity indicators.

In addition, we examine potential transmission channels: mother's age at birth, an indicator variable for whether the mother is married, schooling of the mother's spouse, log of total family income (for couples, it includes both husband's and wife's incomes), number of hours the mother worked in a year, maternal aspirations of the child's educational achievement, and number of children. We take the child's age as the relevant reference point for observing the measures of interest.

One unusual feature of the data set we use is that it contains direct measures of parenting behaviors, which can also be studied as mediating channels. In particular, we look at whether: the child is taken to the museum; there is a musical instrument

11. Since all cohort members took the AFQT test in the same year, there is randomness in the educational attainment at the date of the test which this procedure exploits. Our measure of ability is the residual of a regression of the AFQT score on schooling attainment at the time of the test, holding (final) completed schooling constant.

12. Focusing on the Wisconsin Longitudinal Study, this is investigated by de Haan and Plug (2011).

13. Based on data from the UK National Child Development Survey, Currie and Thomas (2001) and Carneiro, Crawford, and Goodman (2007) show that early test scores and early measures of behavioral problems are strongly associated with adolescent and adult labor market outcomes, health, and engagement in risky behaviors.

14. In the NLSY79, mothers are asked whether their child ever repeated a grade in school and which grade the child repeated. We set observations to missing if the mother's set of answers to grade repetition is not consistent. Because this variable has a large number of missing observations in the earlier years of the data, we only include observations from 1996 onwards.

TABLE 1. Outcome variables.

Child outcomes (ages 7–8 and 12–14)	
PIAT math	Peabody Individual Achievement Test Mathematics. Age-specific score with population mean 0 and variance 1
PIAT read.	Peabody Individual Achievement Test Reading Comprehension. Age-specific score with population mean 0 and variance 1
BPI	Behavior Problem Index. Gender-age specific score with population mean 0 and variance 1
Grade repetition	Indicator for whether child has ever repeated a grade
Overweight	Indicator for whether child is overweight: Takes value 1 if child's Body Mass Index (BMI) is larger than the 95th percentile of age-gender specific distribution
Family environment (ages 7–8)	
Maternal age*	Age of the mother at birth of the child (in years)
Number of children*	Total number of children ever reported by the mother
Marital status	Indicator for whether the mother is married
Spouse's schooling	Years of schooling of mother's spouse
Hours worked	Number of hours mother worked in past year
Log family income	Log of total annual family income (in 2002 prices, using the CPI)
Maternal aspirations	Indicator for whether mother believes that child will go to college
Parental investment measures (ages 7–8 and 12–14)	
Museum	Indicator for whether child is taken to museum several times or more in last year
Musical instrument	Indicator for whether there is a musical instrument child can use at home
Special lessons	Indicator for whether child gets special lessons
Mother reads	Indicator for whether mother reads to child at least three times a week
Newspaper	Indicator for whether family gets a daily newspaper
Computer	Indicator for whether child has a computer in his/her home
Adult home	Indicator: takes the value 1 if adult is present when child comes home after school, and 0 if no adult is present or if child goes somewhere else
Joint meals	Indicator for whether child eats with both parents at least once per day
Early child outcomes (ages 0–1)	
Low birthweight*	Indicator for whether child's birthweight is 5.5 lbs or less
Motor skills	Motor and social development scale (MSD), gender-age specific score standardized to mean 0 and variance 1
Early investments (ages 0–1)	
Smoking during pregnancy*	Indicator for whether mother smoked in the year prior the child's birth
Weeks breastfeeding*	Number of weeks mother was breastfeeding
Formal child-care	Indicator for whether formal child-care arrangements were in place for at least six months over past year
Hours worked	Number of hours mother worked in past year
Mother reads	Indicator for whether mother reads at least three times a week to the child
Books	Number of books child has
Soft toys	Number of cuddly, soft or role-playing toys child has
Outings	Indicator for whether the child gets out of the house at least four times a week
Adolescent outcomes (ages 18–19)	
Enrollment	Indicator for enrollment status of the young adult
Conviction	Indicator for whether the young adult has been convicted up to the age of interest
Own children	Total number of own children born to the young adult up to the age of interest

Notes: Age ranges (in italics) refer to the child and define at which child age this outcome is included in the outcome regression. Not all variables vary across time, but we follow the same sample selection principle for consistency. Variables which do not vary across time are indicated by a star (*).

at home; the child gets special lessons; the mother reads to the child; newspaper and computer are available; there is adult supervision after school; and there are joint meals with both parents (Table 1).

We also study children's outcomes very early in life. Early measures include an indicator for low birth-weight, and the standardized score on the Motor and Social Development scale (MSD), an assessment of early motor, social, and cognitive developments. We focus on ages 0 to 2. As early investments, we study smoking during pregnancy, weeks breastfeeding, use of formal child care and hours worked, and indicators for whether the mother reads to the child, how many books and soft toys the child has, and an indicator for whether the child gets out of the house regularly. In the Web Appendix, we also report results for the young adults; adolescent outcomes are measured at ages 18–19 and include school enrollment, criminal convictions, and number of own children.

In the next section we discuss in detail our instrumental variable strategy. Before we do so, we explain how the instruments are constructed. The instruments for mother's schooling are average tuition in public four-year colleges[15] (in 1993 prices), distance to four-year colleges (an indicator whether there is a college in the county of residence), local log wage and local unemployment rate. When assigning the instruments to mothers, our approach is the following: we assign values that correspond to the year when the mother was 17, in order to be relevant for educational choices towards the end of high school; in order to avoid any potentially endogenous re-location around that period, we use maternal location at age 14. The local wage variable is county-level log wages (based on county data from the Bureau of Economic Analysis, Regional Economic Accounts, and adjusted to 2000 prices using the CPI). The state unemployment rate data comes from the BLS.[16] The distance variable, which is from Kling (2001), is an indicator of whether in 1977 there is a four-year college in the county. Tuition measures are enrollment weighted averages of all public four-year colleges in a county, or at the state level if there is no college in the county.[17]

The data set contains information on a total of 4,458 white children from 1,969 white mothers, and 3,097 children from 1,222 black mothers. For some children, we observe the outcome more than once during the age range of interest. To increase precision of our estimates, we pool all available observations within the specific age range. We cluster all standard errors by cohort and county of mother's residence at age 14, thus allowing for arbitrary dependence between repeat observations from a

15. In our sensitivity analysis, we also present results where we incorporate tuition in two-year colleges as well.

16. State unemployment data are available for all states from 1976 on, and are available for 29 states for 1973, 1974 and 1975, and therefore for some of the individuals we have to use the unemployment rate in the state of residence in 1976 (which corresponds to age 19 for those born in 1957 and age 18 for those born in 1958).

17. Annual records on tuition, enrollment, and location of all public two- and four-year colleges in the United States were constructed from the Department of Education's annual Higher Education General Information Survey and Integrated Postsecondary Education Data System "Institutional Characteristics" surveys. By matching location with county of residence, we determined the presence of two-year and four-year colleges.

TABLE 2. Descriptive sample statistics.

	Whites		Blacks	
	Mean	St. dev.	Mean	St. dev.
Mother's yrs. of schooling	13.459	2.315	12.777	1.982
Mother's AFQT (corrected)	0.413	0.883	−0.435	0.786
Grandmother's yrs. of schooling	11.828	2.287	10.627	2.680
Grandfather's yrs. of schooling	11.961	3.147	9.887	3.668
"Broken home" status	0.200	0.400	0.439	0.496
Child age (months)	95.314	6.983	95.906	6.945
Child female	0.496	0.500	0.502	0.500
College availability	0.526	0.499	0.602	0.490
Local tuition	2.109	0.838	1.961	0.825
Local unemployment	7.174	1.766	6.945	1.543
Local wages	10.270	0.185	10.247	0.214
Observations	2,869		1,377	

Notes: The table reports sample means and standard deviations for covariates and instruments, based on the sample of our PIAT math outcome regression for children aged 7 to 8 (see Tables 4 and 5).

particular child, and between outcomes of several children from one mother, and more generally for arbitrary dependence within county-cohort cells.

Table 2 shows summary statistics for the covariates based on the sample from our PIAT math regression. There are some strong differences between the black and the white sample.[18] Average years of schooling are 0.68 years higher for whites. Since the AFQT score is normed to have a standard deviation of 1 in the population, the means of these two groups are more than 0.8 of a standard deviation apart. The "broken home" status indicates whether the mother grew up with both biological parents (taking the value 1 if the mother did not grow up with both parents, and 0 otherwise); it is more than twice as prevalent in the black sample compared to the white.

3. Empirical Strategy

We assume that child outcomes (y_i) are determined by mother's years of schooling (S_i) as well as a set of observable (X_i) and unobservable factors. Schooling is determined by the same factors as child outcomes, and by a set of instruments (Z_i) that reflect the measured direct and indirect costs of schooling. In interpreting the results we assume that the effects of schooling on outcomes depend on unobservables and that the IV estimates will represent Local Average Treatment Effects (LATE).[19]

18. We have tested whether the sample means reported in Table 2 are the same across the two race samples (at the 5% level of significance). Using the same level of clustering as in the main results reported in what follows, we find no significant differences in the means of the child gender indicator and the local wage variable. All other means differ significantly between the two race samples.

19. See Imbens and Angrist (1994).

We also allow the coefficient on maternal schooling to vary with gender of the child and maternal AFQT. We define four groups depending on the sex of the child and on whether the mother is characterized by high or low ability based on her AFQT score (male–low AFQT, male–high AFQT, female–low AFQT, female–high AFQT). These four group indicators will be denoted by D_{ij}, and take the value 1 if observation i belongs to group j ($j = 1, \ldots, 4$). A_i denotes child age. Thus our estimating equation is

$$\begin{aligned} y_i = {} & \sum_j \beta_j D_{ij} S_i + \sum_j \gamma_{1j} D_{ij} X_{mi} + \sum_j \gamma_{2j} D_{ij} + \sum_j \gamma_{3j} D_{ij} A_i \\ & + \gamma_4 \,(\text{county FE}) + \gamma_5 \,(\text{cohort FE}) + u_i, \end{aligned} \tag{1}$$

where X_{mi} (indexed by m for maternal characteristics) is a set of conditioning characteristics and includes corrected AFQT score, grandmother's schooling, grandfather's schooling, and an indicator for mother's broken home status. "county FE" and "cohort FE" refer to dummy variables for the mother's birth cohort and the county where she grew up, respectively. If we did not restrict the coefficients on county and cohort fixed effects to be the same across groups this would be equivalent to running separate regressions for different groups. The corresponding first-stage regressions ($k = 1, \ldots, 4$) are

$$\begin{aligned} S_i D_{ik} = {} & \sum_j \delta_{1j} D_{ij} Z_i + \sum_j \delta_{2j} D_{ij} (X_{mi} * Z_i) + \sum_j \delta_{3j} D_{ij}((\text{cohort FE}) * Z_i) \\ & + \sum_j \delta_{4j} D_{ij} X_{mi} + \sum_j \delta_{5j} D_{ij} + \sum_j \gamma_{6j} D_{ij} A_i \\ & + \delta_7 \,(\text{county FE}) + \delta_8 \,(\text{cohort FE}) + \epsilon_i, \end{aligned} \tag{2}$$

where the asterisk ($*$) denotes the Kronecker product. Note that in the first term we leave out the variable "distance to college", because in our data set this variable does not vary over time (since it is only measured in 1977). To estimate average effects across groups, we apply the Minimum Distance procedure (Rothenberg 1971; Chamberlain 1984) using as weights the covariance matrix of the unrestricted coefficients.[20]

One part of the direct cost of schooling is the amount of tuition fees a student faces and how far she has to travel to attend college. These variables have frequently been used as instruments (e.g. Kane and Rouse 1993; Card 1993; Currie and Moretti 2003; Cameron and Taber 2004; Carneiro, Heckman, and Vytlacil 2011). Another major cost of acquiring higher education is foregone earnings. We proxy these variables by using the local unemployment rate, reflecting the speed with which someone can find work, and the local wages, as a direct measure of foregone earnings and as a determinant of expectations about future conditions. Both these variable also capture

20. We provide a brief outline of this procedure in the Web Appendix (see Section A.1 and Table A.1).

TABLE 3. Maternal schooling choices and schooling costs.

Dependent variable: Mother's years of schooling	
Mother's AFQT (corrected)	1.089
	[0.109]***
Grandmother's yrs. of schooling	0.169
	[0.026]***
Grandfather's yrs. of schooling	0.146
	[0.022]***
"Broken home" status	−0.365
	[0.141]***
Local unemployment	−0.030
	[0.059]
Local wages	−1.783
	[0.816]**
Local tuition/1,000	0.056
	[0.148]

Notes: The table reports marginal effects for the corresponding covariates based on the first-stage regressions. To compute the marginal effects, we estimate the four first-stage regressions jointly in a stacked specification. When we compute the marginal effects, we only evaluate the block-diagonal elements (corresponding to the respective group k in equation (2)). Marginal effects are evaluated at the overall means in the sample. The sample is selected to be identical to the PIAT math regression in our main results, see Table 4. Standard errors, clustered by birth cohort and county are reported in brackets. See text for details.
** Significant at 5%; *** significant at 1%.

temporary shocks to family income. Therefore, it is not possible to determine a priori whether these variables have a positive or negative effect on maternal schooling, and the effect may well vary across individuals.[21] A key element of our approach is that we include both cohort and county fixed effects, thus relying on the way the instruments change within counties and cohorts to identify our effects. This ensures that we do not use permanent differences across cohorts or regions as source of identifying variation.

Our instruments must be correlated with mother's schooling, but must not have an independent effect on the outcome equation except through mother's schooling. We discuss these conditions in turn.

Underlying the use of geographical variation in schooling costs is the presumption that *local* variables matter for the schooling choice of the individual. In principle, individuals might move to a different location for their studies—for example, in order to avoid high tuition costs. Still, it seems reasonable to believe that local variation matters. Moving is costly for a variety of reasons: the student is prevented from the option of living at home. Furthermore, movers may be disadvantaged in the form of higher out-of-state tuition. Currie and Moretti (2002) report evidence that the majority of students do not move to a different state to go to college (see also Hoxby 1997).

Table 3 shows the effect of schooling cost variables on maternal schooling, where for consistency the sample of interest are white children aged 7 and 8. Similar results

21. See Cameron and Taber (2004) and Arkes (2010).

hold for other ages. The table reports marginal effects of each regressor based on the first stage.[22] Mother's ability level and grandparents' schooling are important determinants of maternal education. The instruments are jointly significant although they are not all individually significant.

We have allowed the instruments to interact with a number of covariates reflecting maternal background and home environments. This is likely to be true in the data so our model should reflect it. Furthermore, it helps improve the overall predictive ability of the instruments. In our sensitivity analysis we show that our results are robust to very flexible specification of the outcome equations by including polynomials in maternal covariates as well as interactions between them; thus the interactions in the instrument set are not picking up nonlinearities left out of the outcome equations, but allow better predictions by modeling the heterogeneity in the schooling choice.[23]

The second requirement for our instruments is that they should not have an independent effect on the outcome, conditional on other covariates. Thus the differential changes in the costs of schooling should not predict child outcomes, conditional on covariates. By controlling for county fixed effects we avoid biases due to geographical sorting. The latter relates to individuals moving to certain counties in a way which creates a correlation between the characteristics of the region (e.g., local labor market conditions, tuition fees, etc.), and outcome relevant variables such as the unobserved human capital of the person moving—the mother in our case. The fact that such sorting takes place is well established (e.g., Solon 1999; Dahl 2002).

The second concern relates to college quality as well as local labor market conditions. If higher tuition fees are associated with higher college quality, and if higher college quality makes mothers better at child rearing, then this could bias our results. First, we use tuition from public colleges only; the link between cost and quality can be expected to be weaker in comparison to private colleges. Second, a main determinant of college quality is the quality of the students; this aspect is captured by including an ability measure of the mother, and by including family background variables. But perhaps most importantly we *do not* rely on comparing mothers who faced different tuition levels. We exploit changing tuition, which relies on the trends being common across regions, as in the differences-in-differences context. Therefore, it does not seem likely that, after controlling for mother's ability, mother's family background, and county fixed effects, endogeneity of tuition due to college quality will pose a problem. A similar argument can be made for the local labor market conditions (which should be seen as local business cycles).

22. The main effect of living near a college is not identified because it does not vary with time and we include county fixed effects. However, we do interact it with a number of maternal background characteristics as described previously.

23. We have also re-estimated Table 3 for this more flexible specification, and the results are very similar to the baseline results.

4. Results

4.1. Effects on Child Outcomes

Our main outcomes are the PIAT math and reading test, the BPI, and binary indicators for grade repetition and child obesity.[24] We measure these variables at both ages 7–8 and 12–14.

4.1.1. White Children. Tables 4(a) and (b) present our main results for white children. The first line shows the estimates of the impact of maternal schooling on child outcomes for the whole sample (of white mothers), while the following four lines show how the impact of maternal schooling varies with gender of the child and AFQT of the mother. More precisely, they show the impact of maternal schooling on child outcomes for male children, female children, children of mothers with high AFQT, and children of mothers with low AFQT. The row at the bottom of the table, denoted as "Impact of Maternal AFQT", shows the impact of the mother's AFQT score on the outcome of interest. We report it to be able to compare the relative importance of maternal schooling and cognitive ability for child outcomes. Each estimate is computed as Minimum Distance estimates based on equation (1), so that all estimates reported in one column are based on one regression corresponding to equation (1). Standard errors are clustered at the county-cohort level.

OLS results indicate that one year of additional mother's education increases mathematics standardized scores by 5.6% of a standard deviation at ages 7 and 8, while the IV coefficient is 9.4% (the difference between OLS and IV is significant at the 10% level). The results for the reading score at ages 7 and 8 are similar to those for the math score, but somewhat smaller. However, at ages 12 to 14 the effect of mother's schooling on math scores becomes smaller in the IV results. This is essentially true for females.

Mother's education also has strong effects on child behavioral problems (BPI) at both ages.[25] There is an interesting pattern in these results: the effects on math decline with the age of the child, while the effect on behavior remains constant or, if anything, is increasing. We note, however, that given the precision of the estimates we cannot statistically reject that there are no differences across the two age groups. Effects on reading vary across groups, with no definitive pattern. At face value it seems that a better-educated mother may be able to help accelerate academic achievement, an effect

24. The PIAT tests and the BPI are standardized to have mean zero and variance 1 in a nationally representative sample.

25. In the Web Appendix, we also report results for subscales of the BPI (see Table A.10). For white children, we find significant effects on the subscales "Antisocial", "Headstrong", and "Peer conflicts / withdrawn" (IV estimates).

TABLE 4. Child outcomes, OLS and IV results: White children.

(a) OLS estimates: White children

	PIAT math		PIAT read.		BPI		Grade repetition		Overweight	
	7–8 yrs (1)	12–14 yrs (2)	7–8 yrs (3)	12–14 yrs (4)	7–8 yrs (5)	12–14 yrs (6)	7–8 yrs (7)	12–14 yrs (8)	7–8 yrs (9)	12–14 yrs (10)
Impact of Maternal Schooling for:										
Whole sample	0.0557	0.0379	0.0304	0.0297	−0.0727	−0.0798	−0.00380	−0.0104	−0.00714	−0.00699
	[0.0106]***	[0.0130]***	[0.0108]***	[0.0114]***	[0.0143]***	[0.0130]***	[0.00210]*	[0.00293]***	[0.00419]*	[0.00393]*
Males	0.0540	0.0454	0.0362	0.0243	−0.0696	−0.0886	−0.00480	−0.0113	−0.0118	−0.0107
	[0.0142]***	[0.0188]**	[0.0136]***	[0.0165]	[0.0166]***	[0.0158]***	[0.00255]*	[0.00369]***	[0.00538]**	[0.00605]*
Females	0.0570	0.0336	0.0246	0.0335	−0.0776	−0.0692	−0.00284	−0.00945	−0.00135	−0.00527
	[0.0132]***	[0.0151]**	[0.0136]*	[0.0142]**	[0.0194]***	[0.0170]***	[0.00252]	[0.00386]**	[0.00597]	[0.00447]
High maternal AFQT	0.0564	0.0416	0.0419	0.0403	−0.0643	−0.0786	−0.00360	−0.00797	−0.00541	−0.00590
	[0.0131]***	[0.0171]**	[0.0139]***	[0.0147]***	[0.0175]***	[0.0163]***	[0.00215]*	[0.00317]**	[0.00535]	[0.00462]
Low maternal AFQT	0.0543	0.0333	0.0122	0.0139	−0.0885	−0.0819	−0.00642	−0.0242	−0.0101	−0.00961
	[0.0179]***	[0.0188]*	[0.0177]	[0.0179]	[0.0238]***	[0.0215]***	[0.00600]	[0.00740]***	[0.00713]	[0.00701]
Impact of Maternal AFQT	0.162	0.213	0.140	0.250	−0.0580	−0.0107	−0.0201	−0.00293	−0.0176	−0.0367
	[0.0384]***	[0.0455]***	[0.0424]***	[0.0478]***	[0.0557]	[0.0562]	[0.00824]**	[0.0127]	[0.0172]	[0.0175]**
Observations	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177
Mean	0.369	0.355	0.513	0.0853	0.258	0.374	0.0248	0.0557	0.125	0.124
Standard deviation	0.808	0.902	0.801	0.848	0.995	0.985	0.156	0.229	0.331	0.330

TABLE 4. Continued.

(b) IV estimates: White children

	PIAT math		PIAT read.		BPI		Grade repetition		Overweight	
	7–8 yrs (1)	12–14 yrs (2)	7–8 yrs (3)	12–14 yrs (4)	7–8 yrs (5)	12–14 yrs (6)	7–8 yrs (7)	12–14 yrs (8)	7–8 yrs (9)	12–14 yrs (10)
Impact of Maternal Schooling for:										
Whole sample	0.0935	0.0601	0.0546	0.0521	−0.0664	−0.0771	−0.0148	−0.0207	−0.0183	−0.00777
	[0.0252]***	[0.0286]**	[0.0278]**	[0.0312]*	[0.0361]*	[0.0345]**	[0.00557]***	[0.00709]***	[0.0117]	[0.0113]
Males	0.0625	0.0735	0.0447	0.0562	−0.0333	−0.0423	−0.0147	−0.0184	−0.0176	0.0000423
	[0.0355]*	[0.0405]*	[0.0367]	[0.0450]	[0.0472]	[0.0469]	[0.00651]**	[0.0107]*	[0.0170]	[0.0165]
Females	0.117	0.0523	0.0626	0.0499	−0.0950	−0.104	−0.0149	−0.0220	−0.0187	−0.0117
	[0.0317]***	[0.0331]	[0.0339]*	[0.0358]	[0.0446]**	[0.0423]**	[0.00712]**	[0.00837]***	[0.0148]	[0.0128]
High maternal AFQT	0.127	0.0939	0.0720	0.106	−0.0700	−0.0811	−0.0193	−0.0242	−0.0229	−0.0133
	[0.0351]***	[0.0404]**	[0.0363]**	[0.0419]**	[0.0468]	[0.0475]*	[0.00652]***	[0.0102]**	[0.0146]	[0.0147]
Low maternal AFQT	0.0479	0.0286	0.0288	−0.00578	−0.0605	−0.0725	−0.00448	−0.0175	−0.0106	−0.00110
	[0.0416]	[0.0390]	[0.0443]	[0.0435]	[0.0606]	[0.0506]	[0.00956]	[0.00978]*	[0.0188]	[0.0159]
Impact of Maternal AFQT	0.107	0.201	0.127	0.218	−0.0784	−0.00793	−0.00306	0.00630	−0.0116	−0.0423
	[0.0420]**	[0.0496]***	[0.0458]***	[0.0532]***	[0.0584]	[0.0612]	[0.0116]	[0.0131]	[0.0191]	[0.0212]**
Observations	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177
Mean	0.369	0.355	0.513	0.0853	0.258	0.374	0.0248	0.0557	0.125	0.124
Standard deviation	0.808	0.902	0.801	0.848	0.995	0.985	0.156	0.229	0.331	0.330

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. A description of the outcome variables is found in Table 1. Standard errors reported in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

that becomes weaker in the long run. However, the impact on behavior is sustained and possibly reinforced with time.[26]

The results in columns (7) and (8) of Tables 4(a) and (b) examine grade repetition. A one year increase in mother's education reduces the probability of grade repetition by around two percentage points for both age groups (IV). Child obesity is not influenced by maternal schooling at either age in the IV results.[27]

At the bottom of each table we report the impact of the maternal AFQT score on child outcomes. The cognitive ability of the mother is a strong predictor of the cognitive ability of the child. The IV results show that the effect of mother's AFQT on child's performance in math and reading is larger at 12–14 than at 7 to 8. At ages 7 to 8, each year of maternal education produces a slightly smaller increase in the math score of the child than a one standard deviation in maternal AFQT, so that (very roughly) a four-year college degree produces the same increase in math at 7 and 8 as a four standard deviation increase in mother's cognition (a large effect). Equally striking is the result that mother's AFQT does not predict either child's behavior or child's grade repetition, although mother's schooling is a strong determinant of both.

These results resemble the findings of Cunha and Heckman (2008), who estimate that parental background has a strong effect on the child's cognitive skill at early ages which disappears later on, and a weaker initial effect on her noncognitive skill which becomes stronger as the child ages. In their model, cognitive and noncognitive skills are not equally plastic across ages and they estimate that cognitive skills are less malleable than noncognitive skills. This result has been argued to be true in other papers (e.g., Knudsen et al. 2006).

We also present estimates for four different subsamples, defined according to the gender of the child and (separately) the AFQT of the mother.[28] We find that at age 7–8 our estimates for math are the highest for female children and for high-AFQT mothers, although they decline at ages 12–14. Results are similar for reading, with no decline in the effect for high-AFQT mothers. The effect on the behavioral problems index does not decline with age and the impact is substantial and significant, at least in the overall

26. To investigate our finding on children's behavior further, we have also studied whether children from better-educated mothers are more likely to take medication to control behavior. We do not find evidence in favor of this—the estimates tended to be small in magnitude and statistically insignificant.

27. To study this further, it would be interesting to look at eating habits and physical activity directly. Unfortunately, the NLSY does not contain the required variables to investigate this in more detail.

28. We divide white mothers into two groups: white high-AFQT mothers have a score above or equal to 0.4, while white low-AFQT mothers have a score below 0.4. For blacks, we set the cutoff point at -0.44, so that these cutoffs are close to the relevant means reported in Table 2. This is done to account for the different distributions of AFQT between whites and blacks. There are two reasons why the effect of maternal education on child outcomes can vary across these two groups of mothers. First, this parameter can be a function of AFQT. Second, even within AFQT cells, this parameter can vary across observationally similar mothers. In that case the instrumental variables estimate will be an average of the effects of maternal education for the set of mothers affected by the instrument, and this set can be very different in the high- and low-AFQT groups, since AFQT and unobservable ability both determine the schooling decision of mothers. Unfortunately, our procedure confounds the two phenomena, but it is still of great interest especially if we can interpret it as (within each AFQT group) the effect of schooling for those mothers most likely to change schooling in response to a decrease in the costs of attending university (measured by our set of instrumental variables).

sample. The impact of mother's education on grade repetition is also persistent across ages. Overall, at ages 7–8, results are always stronger for mother's with high AFQT.

Generally, the IV results for white children are higher than the OLS ones. This may seem surprising because an ability bias intuition would tell us otherwise. However, this result is common in the returns to schooling literature (Card 1999), and also emerges in the papers by Currie and Moretti (2003) and Oreopoulos, Page, and Stevens (2006). Part of the difference can be explained by measurement error in maternal education (Card 1999), which could bias downwards the OLS results. Beyond these common arguments the standard intuition that is valid in the fixed coefficient model no longer applies when the impacts are heterogeneous. In this case IV estimates may well exceed OLS estimates of the effect of maternal schooling on child outcomes (see, for example, Carneiro, Heckman, and Vytlacil 2011). On the one hand, with heterogeneous effects the OLS estimates do not have a clear direction of bias; on the other hand the IV estimates, under a suitable monotonicity assumption (see Imbens and Angrist 1994), pick up the effect on the marginal individual, which can be larger than the average effect. A natural concern is that our instruments may be weak; we discuss this in our sensitivity analysis (Section 4.4).

4.1.2. Black Children. It is well documented that there are large differences in the processes of human capital accumulation of blacks and whites.[29] Furthermore, ethnic differences in skill formation are an important source of concern for education policies in many countries. Therefore we compare the role of maternal education for white and black children.

Tables 5(a) and (b) present estimates of the effect of maternal education on outcomes for black children. Results are similar to the ones for white children, with the impacts on math and reading, BPI, and grade repetition being large and significant, and the impact on obesity being imprecisely determined. There are, however, some differences. First, estimated impacts (IV) tend to be stronger at 12–14 than at 7–8 for PIAT reading. Second, in the IV estimates the impact on grade repetition for 12–14 year olds is larger for black children than for whites, although the difference is not statistically significant. For children of low-AFQT mothers, a year of education reduces the probability of grade repetition by 3.2 percentage points (which partly mirrors differences in prevalence of grade repetition). Third, maternal AFQT is a stronger predictor of child outcomes for blacks than for whites. Fourth, the role of maternal schooling is larger for males than for females.

4.2. Home Environments

The impact of mothers education on child outcomes is strong in a number of dimensions. Since we do not have an explicit model of child development, we cannot

29. See, for example, Currie and Thomas (1995), Jencks and Phillips (1998), Fryer and Levitt (2004), Carneiro, Heckman, and Masterov (2005), Neal (2006) , Todd and Wolpin (2007).

TABLE 5. Child outcomes, OLS and IV results: Black children.

(a) OLS estimates: Black children

	PIAT math		PIAT read.		BPI		Grade repetition		Overweight	
	7–8 yrs (1)	12–14 yrs (2)	7–8 yrs (3)	12–14 yrs (4)	7–8 yrs (5)	12–14 yrs (6)	7–8 yrs (7)	12–14 yrs (8)	7–8 yrs (9)	12–14 yrs (10)
Impact of Maternal Schooling for:										
Whole sample	0.103	0.0763	0.0826	0.0720	−0.0681	−0.0497	0.00158	−0.0173	0.0105	0.0144
	[0.0184]***	[0.0169]***	[0.0174]***	[0.0148]***	[0.0255]***	[0.0221]**	[0.00642]	[0.00697]**	[0.00795]	[0.00862]*
Males	0.0924	0.0876	0.0775	0.0910	−0.0712	−0.0772	0.00904	−0.0256	0.0133	0.0190
	[0.0241]***	[0.0247]***	[0.0240]***	[0.0213]***	[0.0307]**	[0.0283]***	[0.00784]	[0.00964]***	[0.0101]	[0.00980]*
Females	0.110	0.0688	0.0857	0.0569	−0.0653	−0.0292	−0.00930	−0.0128	0.00731	0.00416
	[0.0213]***	[0.0207]***	[0.0202]***	[0.0192]***	[0.0296]**	[0.0257]	[0.00918]	[0.00789]	[0.0106]	[0.0134]
High maternal AFQT	0.148	0.0847	0.0838	0.0816	−0.0676	−0.0319	0.00504	−0.00826	0.0132	0.0118
	[0.0265]***	[0.0250]***	[0.0246]***	[0.0224]***	[0.0317]**	[0.0294]	[0.00773]	[0.00808]	[0.0127]	[0.0130]
Low maternal AFQT	0.0738	0.0694	0.0816	0.0636	−0.0690	−0.0741	−0.00367	−0.0376	0.00895	0.0162
	[0.0221]***	[0.0227]***	[0.0216]***	[0.0209]***	[0.0454]	[0.0346]**	[0.00914]	[0.0115]***	[0.00968]	[0.0109]
Impact of Maternal	0.155	0.252	0.283	0.233	−0.0561	0.0773	−0.0161	−0.0377	−0.0183	0.0272
AFQT	[0.0651]**	[0.0607]***	[0.0632]***	[0.0625]***	[0.0864]	[0.0777]	[0.0216]	[0.0250]	[0.0264]	[0.0297]
Observations	1,377	1,713	1,285	1,704	1,344	1,730	500	1,058	1,358	1,776
Mean	−0.230	−0.358	0.0817	−0.554	0.443	0.437	0.0520	0.150	0.194	0.203
Standard deviation	0.856	0.882	0.816	0.846	0.995	1.000	0.222	0.358	0.395	0.402

TABLE 5. Continued.

(b) IV estimates: Black children

	PIAT math		PIAT read.		BPI		Grade repetition		Overweight	
	7–8 yrs (1)	12–14 yrs (2)	7–8 yrs (3)	12–14 yrs (4)	7–8 yrs (5)	12–14 yrs (6)	7–8 yrs (7)	12–14 yrs (8)	7–8 yrs (9)	12–14 yrs (10)
Impact of Maternal Schooling for:										
Whole sample	0.117	0.107	0.0901	0.119	−0.0613	−0.0785	−0.00108	−0.0331	0.0168	0.00948
	[0.0302]***	[0.0274]***	[0.0287]***	[0.0281]***	[0.0381]	[0.0364]**	[0.00714]	[0.0113]***	[0.0144]	[0.0146]
Males	0.139	0.135	0.115	0.165	−0.0736	−0.0950	0.00277	−0.0286	0.0203	0.0277
	[0.0389]***	[0.0367]***	[0.0387]***	[0.0390]***	[0.0447]*	[0.0481]**	[0.00857]	[0.0156]*	[0.0178]	[0.0190]
Females	0.0962	0.0782	0.0674	0.0781	−0.0437	−0.0640	−0.00756	−0.0363	0.0132	−0.0103
	[0.0387]**	[0.0368]**	[0.0373]*	[0.0367]**	[0.0507]	[0.0458]	[0.0107]	[0.0136]***	[0.0178]	[0.0196]
High maternal AFQT	0.144	0.146	0.0542	0.145	−0.0549	−0.0735	0.00440	−0.0336	0.0194	−0.00307
	[0.0417]***	[0.0436]***	[0.0452]	[0.0441]***	[0.0524]	[0.0473]	[0.00871]	[0.0138]**	[0.0192]	[0.0204]
Low maternal AFQT	0.0902	0.0734	0.115	0.0966	−0.0700	−0.0860	−0.00766	−0.0322	0.0140	0.0209
	[0.0417]**	[0.0400]*	[0.0374]***	[0.0399]**	[0.0623]	[0.0576]	[0.00932]	[0.0188]*	[0.0196]	[0.0195]
Impact of Maternal AFQT	0.184	0.244	0.330	0.208	−0.0801	0.0927	−0.00846	−0.0139	−0.0180	0.0422
	[0.0638]***	[0.0608]***	[0.0650]***	[0.0615]***	[0.0873]	[0.0783]	[0.0178]	[0.0246]	[0.0271]	[0.0305]
Observations	1,377	1,713	1,285	1,704	1,344	1,730	500	1,058	1,358	1,776
Mean	−0.230	−0.358	0.0817	−0.554	0.443	0.437	0.0520	0.150	0.194	0.203
Standard deviation	0.856	0.882	0.816	0.846	0.995	1.000	0.222	0.358	0.395	0.402

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

firmly establish the role of these channels.[30] However, our results in this section paint a picture of how they may operate, and their detail makes them especially useful. The IV results for whites are reported in Table 6.[31] The maternal characteristics examined are maternal age at birth, educational aspirations for the child (does the mother believe whether the child will go to college), marital status, spouse's years of schooling (for those with a spouse), number of children, hours worked, and log family income (which includes spouse's income). All variables are measured when the child is 7 or 8.

An increase in mother's schooling by one year leads to increases in: maternal age at birth by about one year, family income by 14%, the probability of being married by 4.7 percentage points. The effect on fertility is surprisingly small.

Several economists have argued that it is important to account for the effects of assortative mating because the causal effect of maternal education on child performance may come through her ability to find an educated father for the child. Unfortunately we do not have good instruments for estimating the direct effect of spouse's education and cannot directly assess the validity of this argument. However, we can examine the effect of maternal schooling on spouse's schooling. Column (4) shows that an increase of one year in maternal education leads to an increase of 0.5 years of spouse's education. If we attributed all the effects of maternal education to assortative mating we would need father's schooling to have twice as large effects as the ones we estimate for mothers. We refer to the literature for studies which separate out the effect of maternal versus paternal schooling, and for comparability with this work we focus on IV approaches here.[32] Holmlund, Lindahl, and Plug (2010) are able to estimate separately the effect of maternal versus paternal schooling, where they treat both of these as endogenous and instrument (simultaneously) with corresponding compulsory schooling reform indicators.[33] Although not statistically significant, the point estimate for maternal education is consistently higher than the point estimate for father's education. Similarly, Chevalier (2004) estimates the effect of maternal and paternal schooling separately, again using compulsory schooling laws. He finds that the coefficient on maternal schooling is substantially larger than the paternal effect.[34] In summary, these studies do not provide evidence for the father's effect being substantially larger than the maternal effect. This leads us to conclude that although assortative mating effects may account for part of the effect we find, they are unlikely to fully drive our results.

A second argument in the literature is that maternal education can have ambiguous effects because if on one hand the child benefits from better home environments

30. In a purely descriptive way, Table A.9 in the Web Appendix shows correlations between a number of these potential channels and the outcomes PIAT math and BPI.

31. In the Web Appendix we also report the OLS results for completeness.

32. As we have emphasized previously, other identification strategies have come to different conclusions. An example is Behrman and Rosenzweig (2002), who estimate small or no effects of maternal education on child's schooling, while father's education has large and strong effects on this outcome.

33. The outcome is years of schooling, see Table 3.C in Holmlund, Lindahl, and Plug (2010).

34. See Tables 4A and 4B in Chevalier (2004). When the sample is limited to natural parents only, maternal and paternal schooling is found to be of equal magnitude.

TABLE 6. Family environment, IV results: White children.

	IV estimates: White children (7–8 years)						
	Maternal age (1)	Number of children (2)	Marital status (3)	Spouse schooling (4)	Hours worked (5)	Log family income (6)	Maternal aspirations (7)
Impact of Maternal Schooling for:							
Whole sample	1.083	0.0192	0.0468	0.489	71.67	0.135	0.0551
	[0.150]***	[0.0510]	[0.0156]***	[0.0851]***	[34.91]**	[0.0300]***	[0.0149]***
Males	1.223	0.0481	0.0536	0.469	76.39	0.139	0.0585
	[0.214]***	[0.0640]	[0.0195]***	[0.116]***	[44.19]*	[0.0417]***	[0.0209]***
Females	0.975	−0.00546	0.0401	0.501	67.93	0.132	0.0530
	[0.191]***	[0.0608]	[0.0194]**	[0.0980]***	[40.97]*	[0.0392]***	[0.0173]***
High maternal AFQT	0.909	−0.0759	0.0530	0.393	31.47	0.131	0.0591
	[0.218]***	[0.0802]	[0.0202]***	[0.124]***	[48.52]	[0.0400]***	[0.0183]***
Low maternal AFQT	1.287	0.117	0.0361	0.588	118.3	0.142	0.0482
	[0.238]***	[0.0816]	[0.0269]	[0.127]***	[52.38]**	[0.0482]***	[0.0231]**
Impact of Maternal AFQT	−0.262	0.0493	0.0137	0.0836	138.9	0.202	0.0121
	[0.247]	[0.0957]	[0.0262]	[0.157]	[55.27]**	[0.0527]***	[0.0332]
Observations	4,833	4,833	4,828	3,700	4,738	4,188	1,418
Mean	25.26	2.769	0.778	13.40	1162.6	3.749	0.774
Standard deviation	5.437	1.228	0.416	2.563	953.4	0.935	0.419

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

and perhaps richer investments, she will benefit of less maternal time because more-educated mothers spend more time in the labor market (see, for example, Behrman and Rosenzweig 2002). Here, we examine the effect of maternal schooling on maternal labor supply, and column (5) in Table 6 looks at the effects of maternal education on maternal employment measured in terms of annual hours worked. Annual hours worked increase by 72 hours per additional year of maternal schooling (6.2% of the mean of 1,163 hours worked per year), or roughly 1.8 weeks of full-time work per year, although the effect is imprecisely estimated. If we compared a mother with a college degree and another without, our estimates suggest that the former would work seven more weeks per year than the latter. Cumulating over several years of childhood, these will translate into much more family resources for the mother with a college degree, but less time at home. The latter can have an offsetting effect on the former, although it depends on what kind of substitutes educated mothers can find for their time with their child.

Column (7) shows that more-educated mothers are 5.5 percentage points more likely to believe that their offspring will complete college. These expectations may translate into different behavior on the side of the mother and the child.

The estimates presented in Table 6 are fairly similar for boys and girls, and for children of mothers with high and low levels of AFQT. There are only a few cases of interesting differences across groups. In particular, the effect of maternal education on maternal aspirations and marital status are smaller for low-AFQT mothers than for other groups, which may be one of the reasons why we found weak effects on child outcomes for this group of mothers. Labor supply and assortative mating effects are particularly strong for low-AFQT mothers.

One feature of the data set is the wealth of information on direct measures of home environments and parental investments, as reported in Table 7. For white children, an increase in mother's schooling by one year leads to increases in the probabilities that: there is a musical instrument in the home by 3.9 percentage points; there is a computer in the home by 5.7 percentage points; a child takes special lessons by 5.8 percentage points. Each extra year of schooling also means that mothers are 3 percentage points more likely to read to their child at least three times a week. There is no evidence that maternal education affects the amount of newspapers in the home, and time spent with the child in a museum or sharing meals. Notice that more-educated mothers do not seem to spend less time in activities with their children, even though they spend more time working. This pattern emerges throughout the paper, and we will comment on it with detail when we examine the child's early years.

Testing for gender differences in investments in Table 7, we have not been able to reject the hypothesis that the effect is the same for boys and girls. The only exception is in column (3), special lessons, where the difference is significant at 10%.

The results for black mothers are slightly different, and they are shown in Tables 8 and 9. Relatively to white mothers, education not only affects maternal age at birth, aspirations, marital status, spouse's schooling and income, but it also has large effects on fertility and employment. Each additional four years in school (a four-year university degree) decreases the number of children born to each woman by 1.4, and increase

TABLE 7. Investments, IV results: White children.

	IV estimates: White children							
	Museum 7–8 yrs (1)	Musical Instr. 7–8 yrs (2)	Special lesson 7–8 yrs (3)	Mother reads 7–8 yrs (4)	Newspaper 7–8 yrs (5)	Computer 12–14 yrs (6)	Adult home 12–14 yrs (7)	Joint meals 12–14 yrs (8)
Impact of Maternal Schooling for:								
Whole sample	0.00595	0.0393	0.0579	0.0296	−0.0183	0.0570	0.0304	−0.0126
	[0.0165]	[0.0169]**	[0.0147]***	[0.0154]*	[0.0175]	[0.0146]***	[0.0156]*	[0.0177]
Males	0.0276	0.0426	0.0826	0.0274	−0.0258	0.0476	0.0266	−0.0240
	[0.0231]	[0.0223]*	[0.0203]***	[0.0231]	[0.0237]	[0.0205]**	[0.0205]	[0.0229]
Females	−0.0120	0.0367	0.0413	0.0310	−0.0130	0.0644	0.0341	−0.00232
	[0.0213]	[0.0204]*	[0.0175]**	[0.0188]*	[0.0209]	[0.0185]***	[0.0202]*	[0.0219]
High maternal AFQT	0.00800	0.0543	0.0526	0.0246	−0.0180	0.0471	0.0203	−0.0271
	[0.0214]	[0.0227]**	[0.0180]***	[0.0205]	[0.0232]	[0.0186]**	[0.0210]	[0.0230]
Low maternal AFQT	0.00271	0.0173	0.0692	0.0372	−0.0189	0.0742	0.0426	0.00705
	[0.0270]	[0.0279]	[0.0265]***	[0.0259]	[0.0298]	[0.0246]***	[0.0230]*	[0.0267]
Impact of Maternal AFQT	0.0170	0.0336	−0.00116	−0.0277	0.0786	0.0248	−0.0679	0.0105
	[0.0278]	[0.0315]	[0.0263]	[0.0286]	[0.0298]***	[0.0314]	[0.0301]**	[0.0327]
Observations	3,064	3,063	3,062	3,069	3,065	1,681	2,907	3,218
Mean	0.420	0.545	0.686	0.499	0.533	0.681	0.684	0.556
Standard deviation	0.494	0.498	0.464	0.500	0.511	0.466	0.465	0.497

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE 8. Family environment, IV results: Black children.

	IV estimates: Black children (7–8 years)						
	Maternal age (1)	Number of children (2)	Marital status (3)	Spouse schooling (4)	Hours worked (5)	Log family income (6)	Maternal aspirations (7)
Impact of Maternal Schooling for:							
Whole sample	0.879	−0.347	0.0662	0.620	202.8	0.182	0.0501
	[0.178]***	[0.0648]***	[0.0203]***	[0.0625]***	[33.50]***	[0.0339]***	[0.0153]***
Males	0.953	−0.350	0.0780	0.606	223.2	0.231	0.0401
	[0.227]***	[0.0756]***	[0.0264]***	[0.0718]***	[49.73]***	[0.0407]***	[0.0198]**
Females	0.809	−0.343	0.0571	0.637	191.4	0.123	0.0599
	[0.222]***	[0.0773]***	[0.0241]**	[0.0763]***	[39.27]***	[0.0430]***	[0.0197]***
High maternal AFQT	1.107	−0.249	0.0445	0.542	157.2	0.223	0.0109
	[0.281]***	[0.0909]***	[0.0312]	[0.109]***	[46.54]***	[0.0526]***	[0.0217]
Low maternal AFQT	0.746	−0.433	0.0801	0.679	246.9	0.156	0.0855
	[0.219]***	[0.0859]***	[0.0253]***	[0.0917]***	[45.76]***	[0.0423]***	[0.0207]***
Impact of Maternal AFQT	0.0586	−0.0903	0.0452	−0.0614	94.03	0.206	0.0396
	[0.344]	[0.113]	[0.0394]	[0.192]	[71.41]	[0.0725]***	[0.0423]
Observations	2,767	2,768	2,765	1,002	2,742	2,228	468
Mean	22.65	3.118	0.382	12.75	1154.9	3.040	0.660
Standard deviation	5.185	1.448	0.486	2.110	998.2	0.890	0.474

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
** Significant at 5%; *** significant at 1%.

TABLE 9. Investments, IV results: Black children.

	IV estimates: Black children							
	Museum 7–8 yrs (1)	Musical Instr. 7–8 yrs (2)	Special lesson 7–8 yrs (3)	Mother reads 7–8 yrs (4)	Newspaper 7–8 yrs (5)	Computer 12–14 yrs (6)	Adult home 12–14 yrs (7)	Joint meals 12–14 yrs (8)
Impact of Maternal Schooling for:								
Whole sample	0.0184	0.0181	0.0964	0.0464	−0.0156	0.0768	−0.0247	0.0172
	[0.0177]	[0.0184]	[0.0192]***	[0.0175]***	[0.0177]	[0.0167]***	[0.0169]	[0.0162]
Males	0.0250	0.0205	0.109	0.0516	−0.0138	0.0647	−0.0270	0.0151
	[0.0209]	[0.0247]	[0.0255]***	[0.0207]**	[0.0227]	[0.0238]***	[0.0225]	[0.0220]
Females	0.00684	0.0160	0.0861	0.0377	−0.0180	0.0882	−0.0228	0.0191
	[0.0263]	[0.0231]	[0.0238]***	[0.0252]	[0.0262]	[0.0232]***	[0.0213]	[0.0211]
High maternal AFQT	−0.00503	0.0229	0.112	0.0430	−0.0192	0.0890	0.00480	0.0495
	[0.0265]	[0.0289]	[0.0265]***	[0.0235]*	[0.0250]	[0.0247]***	[0.0250]	[0.0245]**
Low maternal AFQT	0.0356	0.0146	0.0819	0.0493	−0.0121	0.0632	−0.0492	−0.0122
	[0.0229]	[0.0244]	[0.0258]***	[0.0220]**	[0.0246]	[0.0262]**	[0.0227]**	[0.0232]
Impact of Maternal AFQT	−0.0309	0.0607	0.0110	−0.0719	0.0236	0.0164	−0.00999	−0.163
	[0.0374]	[0.0408]	[0.0409]	[0.0360]**	[0.0406]	[0.0426]	[0.0402]	[0.0378]***
Observations	1,420	1,419	1,419	1,422	1,421	906	1,627	1,711
Mean	0.405	0.361	0.455	0.339	0.410	0.352	0.707	0.314
Standard deviation	0.491	0.480	0.498	0.474	0.496	0.478	0.455	0.464

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

maternal employment by over 800 hours (or roughly 20 weeks) a year. The effects of education on income are especially large for high-AFQT mothers, while the effects of education on employment and fertility are stronger for low-AFQT mothers.

It is remarkable that each year of maternal schooling among blacks increases the proportion of children who are read to at least three times a week by five percentage points (these are time-intensive activities). Part of this may be due to the fact that more-educated black mothers have fewer children to spend their time with. However, an extra year of maternal education also makes it 2.5 percentage points less likely that black children have adult supervision when they arrive home after school, although this effect is not statistically significant.

In summary, there exists strong evidence that maternal education affects home environments and child outcomes. The size of several of our estimates in this section is large, and suggests that we should seriously look at education policy as a way of improving the home environments of future generations of children. Educated mothers provide better surroundings for their children by postponing and decreasing childbearing, by increasing family resources, and by assortative mating. There is also strong evidence that educated mothers invest more in their children. However, educated mothers also spend longer periods outside the home working and earning. Still, whatever the negative consequences of spending time away from the children may be, they are outweighed by the positive effects. With the exception of adult supervision for black children from low-AFQT mothers, more-educated mothers do not spend less time with their children, either because they have less children, or less leisure time. If anything, our results indicate that the opposite is true.

At this point it is useful to compare our estimates of the effect of maternal education to those of other childhood interventions. The large class size reduction of the STAR experiment (a reduction from 22 to 15 pupils per class, studied by Krueger 1999) yielded test score gains of 0.2 standard deviations, an equivalent of two years of maternal schooling. Dahl and Lochner (2006) estimate that a \$1,000 increase in family income improves performance on the math test score by 2.1% of a standard deviation (3.6% for reading). Currie and Thomas (1995) estimate that participation in Head Start increases performance in the PPVT vocabulary test by almost six percentile points (which is about 20% to 25% of a standard deviation). Bernal and Keane (2006) find that additional formal child care does not improve the average child test score performance, but may be beneficial for children of poorly educated mothers. Aizer (2004) estimates that adult supervision after school reduces the probability of a child engaging in risky behavior by about seven percentage points. Dustmann and Schönberg (2007) find that increasing paid maternity leave does not significantly improve long-term child outcomes, although Carneiro, Salvanes, and Løken (2010) find large effects on high school dropout rates. Our claim is that, although the nature of the different interventions differs quite a lot, the effects of maternal education are large when compared to those of other interventions. If the objective is to increase children's outcomes, additional maternal education is a serious competitor to the other types of interventions. Of course, in doing this kind of comparison, it is important to keep in mind that each of the interventions have different costs and may affect children along a variety of dimensions, and comparisons become difficult when trade-offs between different objectives are involved. One particular cost of raising child outcomes through maternal education is that it may involve a substantial time lag between the introduction of the policy and the time the children have grown up.

Furthermore, when interpreting the findings to predict effects of future policies, one needs to keep in mind that the introduction of large-scale policies may be accompanied by general equilibrium effects, which may partially offset the effects we estimate. This qualification applies in particular to the mating channel; if a policy was to raise the schooling of every (potential) mother, without affecting the schooling of (potential) fathers, there could not be aggregate gains in spouse's schooling (this is particularly important if most of the effects of maternal schooling on child development came through assortative mating, an issue we discussed previously).

4.3. Early Childhood and Young Adulthood

In this section we investigate which of these effects are visible at earlier ages of the child. This question is particularly interesting given the recent academic and policy emphasis on the importance of the early years.

A second question of interest is whether there is any evidence of effects of maternal schooling on environments and behavior during adolescence and young adulthood, when behavioral anomalies such as engagement in criminal activities, early dropping out of school, or early childbearing, may be the source of long-run problems. Ideally, we would like to follow individuals well into their adult lives, but unfortunately this is not yet possible with this sample. It is important to keep in mind that many children of the NLSY79 cohort members have not yet reached adulthood. Thus, the children we observe in this age range are mainly from the early cohorts and from mothers with very low birth ages, and the sample size is smaller than for the younger cohorts. To emphasize that this sample is still evolving as more children in the NLSY79 reach adulthood, we report these results in the Online Appendix.

4.3.1. Early Childhood. Here we present estimates of the effect of maternal schooling on the probability of the child having low birth-weight (weighing less than 5.5 pounds at birth), and the score on the MSD scale, which assesses the motor and social skills development, both for children up to 24 months. Results are shown for whites and blacks in Table 10.

Currie and Moretti (2003) find that one extra year of maternal education reduces the probability that a child is born with low birth-weight by one percentage point. Our estimates for whites are lower and insignificant, whether we use OLS or IV, although we have a much smaller sample than Currie and Moretti (2003). Results are only statistically strong for black mothers with low AFQT scores, for whom the coefficient is −0.048 (the incidence of low birth-weight is 13% in the sample of blacks).

TABLE 10. Early outcomes, IV results.

	IV estimates: Children 0–1 years			
	Whites		Blacks	
	Low birthweight (1)	MSD (2)	Low birthweight (3)	MSD (4)
Impact of Maternal Schooling for:				
Whole sample	−0.00276	−0.0809	−0.0180	0.0582
	[0.00683]	[0.0326]**	[0.0129]	[0.0408]
Males	−0.00386	−0.0814	−0.0163	0.0601
	[0.0102]	[0.0418]*	[0.0157]	[0.0455]
Females	−0.00160	−0.0804	−0.0209	0.0536
	[0.0105]	[0.0442]*	[0.0197]	[0.0645]
High maternal AFQT	−0.00812	−0.0565	0.0184	−0.0176
	[0.0101]	[0.0396]	[0.0191]	[0.0608]
Low maternal AFQT	0.00341	−0.132	−0.0478	0.123
	[0.0110]	[0.0576]**	[0.0173]***	[0.0560]**
Impact of Maternal AFQT	−0.00699	0.0141	−0.0113	−0.209
	[0.0130]	[0.0658]	[0.0259]	[0.112]*
Observations	5,600	2,155	2,813	787
Mean	0.0652	−0.0435	0.131	0.181
Standard deviation	0.247	0.996	0.337	1.215

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Looking at the relationship between maternal education and early motor and social skills of the child a new picture emerges. For whites, our estimates are negative, and especially important for low-ability mothers. This is the first and only instance in this paper where increases in maternal schooling are shown not to be good for children, perhaps because of increased maternal employment and less time with the child.

Table 11 presents the results for early home environments of whites, where the following outcomes are considered: smoking in the year prior to the birth of the child, weeks of breastfeeding, use of formal child care arrangements, annual hours worked by the mother, whether the child is read to, how many books and soft toys the child has, and whether the child is taken on outings regularly.

The two health inputs, (not) smoking and breastfeeding, are strongly affected by maternal schooling. Notice also that the effect on maternal hours worked is much larger when measured during the child's early years than later on (as we saw in Table 6). At the same time, the increase in formal child care is modest and only statistically strong for girls. The strong increase in hours worked that results from additional education is not accompanied by a strong increase in formal child-care, raising the question of how these children are cared for. This could be seen as support to the argument that more educated mothers spend more time working, with detrimental effects on child development. Still, even if this is true, children seem to recover, so that BPI and grade repetition at 12 and 14 are lower when maternal education is higher. Finally, there is

TABLE 11. Early channels, IV results: White children.

	IV estimates: White children 0–1 years							
	Smoking d. pregnancy (1)	Weeks breastfeeding (2)	Formal child care (3)	Hours worked (4)	Mother reads (5)	Books (6)	Soft toys (7)	Outings (8)
Impact of Maternal Schooling for:								
Whole sample	−0.0653	2.679	0.0142	97.21	0.00715	0.0654	−0.153	−0.000296
	[0.0165]***	[0.641]***	[0.00680]**	[28.46]***	[0.0134]	[0.0278]**	[0.395]	[0.0134]
Males	−0.0784	2.321	0.00577	115.4	−0.000902	0.0532	−0.433	−0.0137
	[0.0226]***	[0.849]***	[0.00996]	[38.85]***	[0.0182]	[0.0412]	[0.494]	[0.0183]
Females	−0.0545	3.030	0.0224	81.75	0.0165	0.0765	0.243	0.0121
	[0.0209]***	[0.842]***	[0.00979]**	[36.30]**	[0.0196]	[0.0390]**	[0.575]	[0.0176]
High maternal AFQT	−0.0541	2.655	0.0181	105.2	−0.00451	0.0216	−0.239	0.00632
	[0.0227]**	[0.929]***	[0.0113]	[37.71]***	[0.0175]	[0.0370]	[0.524]	[0.0163]
Low maternal AFQT	−0.0799	2.704	0.0121	85.68	0.0262	0.134	−0.0285	−0.0152
	[0.0261]***	[0.951]***	[0.00842]	[45.56]*	[0.0228]	[0.0474]***	[0.632]	[0.0249]
Impact of Maternal AFQT	−0.0808	0.769	0.0199	85.60	0.0488	0.145	2.400	0.0334
	[0.0290]***	[0.940]	[0.0100]**	[41.25]**	[0.0277]*	[0.0567]**	[0.721]***	[0.0259]
Observations	5,659	5,523	4,900	6,000	2,413	2,437	2,395	2,434
Mean	0.351	13.05	0.0678	928.7	0.611	3.253	16.65	0.753
Standard deviation	0.477	20.10	0.251	881.1	0.488	1.058	12.42	0.432

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

no evidence that, even though they work more, more-educated mothers spend less time breastfeeding, reading to their children, or taking them on outings. This is consistent with recent findings from time diary studies summarized in Blau and Currie (2006): mothers who work more do not spend less time with their children; instead, they have less leisure. Notice also that young children of educated mothers have more books than other children, especially if their mothers have low cognitive ability.

In summary, it is difficult to make the case that the large increase in employment of white mothers that results from additional education has detrimental effects on children. There may be some delays in their motor and social development, especially for low-AFQT mothers, but they do not appear to have any long-term undesirable consequences. In fact, it is for low-AFQT mothers that maternal education has the largest positive effects on home environments.

For black families this picture is even more evident. The impacts of maternal education on birth-weight and motor and social development are positive and large for low-ability mothers (columns (3) and (4) of Table 10). An additional year of education leads to about 154 extra hours of work, but also more regular use of formal child-care arrangements, more time reading to the child, and more children's books in the home (Table 12). Breastfeeding is prolonged by roughly one week.

The estimates displayed in Tables 11 and 12 tell a clear and important story: improvements in maternal schooling promote much better home environments during the early years of the child; although more-educated mothers work more, they do not spend less quality time with their children, and if anything the opposite is true; it is striking that for many outcomes, for both black and white mothers, it is for low-ability mothers that education has the largest impact on early home environments.

4.4. Sensitivity Analysis

In this section we examine the sensitivity of our main results, presented in Section 4.1. An important concern in this paper is with the potential weakness of the instruments (although the *p*-values of the instruments in the first-stage equations are very low). Most of the literature on weak instruments deals with models of fixed coefficients. One standard recommendation is to estimate the model using LIML (e.g., Staiger and Stock 1997). Therefore, we proceed by estimating the model by LIML. Here we present results for the main outcomes for the sample of white children.[35] Panel B in Table 13 shows that, at ages 7–8, the LIML estimates are of the same sign than the original TSLS estimates in the paper, but they have larger absolute magnitudes and they are more imprecise (which would be a prediction of most of the literature).[36] This means that the TSLS estimates are closer to OLS than LIML, which is what we would expect if the instruments were weak. Notice also that, even with the imprecise LIML estimates, the effect of maternal schooling on white children cognitive development

35. In Table A.11 in the Web Appendix, we show the corresponding results for black children.

36. Panel A reproduces our base case result for easy reference.

TABLE 12. Early channels, IV results: Black children.

	IV estimates: Black children 0–1 years							
	Smoking d. pregnancy (1)	Weeks breastfeeding (2)	Formal child care (3)	Hours worked (4)	Mother reads (5)	Books (6)	Soft toys (7)	Outings (8)
Impact of Maternal Schooling for:								
Whole sample	−0.0237	0.949	0.0196	153.9	0.0448	0.177	0.0402	0.0204
	[0.0207]	[0.518]*	[0.00763]**	[28.28]***	[0.0179]**	[0.0454]***	[0.393]	[0.0168]
Males	−0.0167	0.985	0.0157	140.7	0.0489	0.180	0.881	0.0201
	[0.0257]	[0.574]*	[0.00907]*	[35.75]***	[0.0210]**	[0.0525]***	[0.502]*	[0.0204]
Females	−0.0309	0.876	0.0271	170.2	0.0381	0.173	−0.498	0.0207
	[0.0257]	[0.728]	[0.0120]**	[38.96]***	[0.0256]	[0.0642]***	[0.441]	[0.0229]
High maternal AFQT	0.0000960	0.588	0.0336	140.8	0.0519	0.153	0.351	−0.000364
	[0.0274]	[0.802]	[0.0144]**	[42.28]***	[0.0243]**	[0.0595]**	[0.493]	[0.0207]
Low maternal AFQT	−0.0489	1.052	0.0137	166.1	0.0369	0.203	−0.408	0.0527
	[0.0281]*	[0.547]*	[0.00924]	[40.61]***	[0.0258]	[0.0609]***	[0.581]	[0.0251]**
Impact of Maternal AFQT	−0.0415	1.031	0.00357	139.5	0.0203	0.198	−1.028	0.0212
	[0.0374]	[0.845]	[0.0173]	[57.92]**	[0.0428]	[0.0882]**	[0.874]	[0.0352]
Observations	2,806	2,801	2,269	2,980	907	910	902	910
Mean	0.295	4.129	0.0718	770.1	0.375	2.354	11.18	0.702
Standard deviation	0.456	12.01	0.258	886.6	0.484	1.192	10.04	0.458

Notes: Table reports Minimum Distance estimates based on equation (1), see text for details. Standard errors in brackets, clustered by county-cohort.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE 13. Sensitivity (white children).

	Sensitivity analysis (white children)									
	PIAT math		PIAT read.		BPI		Grade repetition		Overweight	
	7–8 yrs (1)	12–14 yrs (2)	7–8 yrs (3)	12–14 yrs (4)	7–8 yrs (5)	12–14 yrs (6)	7–8 yrs (7)	12–14 yrs (8)	7–8 yrs (9)	12–14 yrs (10)
Panel A: Base case										
IV—Base case	0.0935	0.0601	0.0546	0.0521	−0.0664	−0.0771	−0.0148	−0.0207	−0.0183	−0.00777
	[0.0252]***	[0.0286]**	[0.0278]**	[0.0312]*	[0.0361]*	[0.0345]**	[0.00557]***	[0.00709]***	[0.0117]	[0.0113]
	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177
Panel B: LIML										
IV—LIML	0.132	0.0576	0.0780	0.0223	−0.0657	−0.0741	−0.0237	−0.0231	−0.0395	−0.00812
	[0.0547]**	[0.0566]	[0.0630]	[0.0727]	[0.0806]	[0.0945]	[0.0158]	[0.0155]	[0.0318]	[0.0230]
	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177
Panel C: Including additional controls										
including polynomials and interactions	0.0957	0.0870	0.0554	0.0685	−0.0784	−0.0879	−0.0135	−0.0223	−0.0173	−0.0127
	[0.0249]***	[0.0288]***	[0.0275]**	[0.0300]**	[0.0358]**	[0.0361]**	[0.00578]**	[0.00753]***	[0.0118]	[0.0114]
	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177
including group-specific cohort dummies	0.0847	0.0547	0.0313	0.0421	−0.0862	−0.0791	−0.0143	−0.0165	−0.0149	−0.00565
	[0.0255]***	[0.0291]*	[0.0277]	[0.0308]	[0.0369]**	[0.0349]**	[0.00556]**	[0.00737]**	[0.0121]	[0.0104]
	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177

TABLE 13. Continued.

	Sensitivity analysis (white children)									
	PIAT math		PIAT read.		BPI		Grade repetition		Overweight	
	7–8 yrs (1)	12–14 yrs (2)	7–8 yrs (3)	12–14 yrs (4)	7–8 yrs (5)	12–14 yrs (6)	7–8 yrs (7)	12–14 yrs (8)	7–8 yrs (9)	12–14 yrs (10)
Panel D: Varying the set of instruments										
IV—Using both two-year and four-year tuition	0.0819	0.0390	0.0430	0.0269	−0.0846	−0.0680	−0.0118	−0.0186	−0.0223	−0.0116
	[0.0225]***	[0.0263]	[0.0244]*	[0.0276]	[0.0314]***	[0.0306]**	[0.00458]**	[0.00631]***	[0.00975]**	[0.0100]
	2,865	2,950	2,724	2,935	2,971	3,211	1,609	2,509	2,926	3,173
IV—Excluding distance variable	0.111	0.0487	0.0748	0.0528	−0.0526	−0.0627	−0.0165	−0.0116	−0.0114	−0.00823
	[0.0283]***	[0.0318]	[0.0314]**	[0.0325]	[0.0381]	[0.0369]*	[0.00660]**	[0.00801]	[0.0126]	[0.0127]
	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177
IV—Excluding distance variable and tuition variable	0.124	0.0374	0.0624	0.0676	−0.0332	−0.0201	−0.00616	−0.00901	−0.0180	−0.0212
	[0.0338]***	[0.0352]	[0.0376]*	[0.0394]*	[0.0498]	[0.0475]	[0.00807]	[0.00981]	[0.0161]	[0.0153]
	2,869	2,954	2,728	2,939	2,975	3,215	1,610	2,512	2,930	3,177
Panel E: One observation per child, holding the sample of children constant across age groups										
IV—One observation per child, holding the sample of children constant across age groups	0.0683	0.0621	0.0720	0.0493	−0.0898	−0.107	−0.0224	−0.0236	−0.0284	−0.00479
	[0.0300]**	[0.0317]*	[0.0316]**	[0.0316]	[0.0394]**	[0.0344]***	[0.00672]***	[0.00763]***	[0.0114]**	[0.0122]
	2,090	2,090	1,978	1,978	2,224	2,224	1,113	1,113	2,185	2,185

Notes: This table reports IV estimates, showing the estimated average effect across all groups using the MD procedure as before. Panel A reproduces the main results for easy reference. Panel B shows LIML estimates. Panel C adds additional controls. "Polynomials and interactions" includes polynomials of AFQT and grandparents' education, and two-way interactions between AFQT, grandparents' education, and broken-home status. All of these additional regressors are also interacted with the four group indicators. "Group-specific cohort dummies" adds interactions of cohort indicators with group indicators to the base specification. Panel D presents IV estimates based on a modified set of the instruments. First, we use both 2-year and 4-year tuition. In the following, distance (and corresponding interactions) and distance and tuition (and corresponding interactions) are, respectively, excluded from the analysis. Panel E restricts the sample to those children who contribute observations to both age groups (age 7–8 and 12–14), and selects only the first observation from each age group. See text for details.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

drops substantially from ages 7–8 to ages 12–14, while that is not the case for grade repetition and BPI. These results suggest that, although we may suffer from a weak instruments problem, if anything our estimates understate the true impact of maternal education on child outcomes since TSLS is biased towards OLS (and the latter are generally smaller than the former in absolute value). However, we need to be cautious about conclusions from these results, since the literature on weak instruments we draw on refers to a fixed-coefficient model.

Another possible criticism of our procedure is that, since we are relying on interactions between controls and instruments, if the outcome equation is misspecified then some of our results might be driven by nonlinearities instead of genuine variation in the instruments. Therefore we re-estimate our model with a more flexible specification of the outcome equations, where we add the following variables to the set of controls: AFQT squared, grandmother's education squared, grandfather's education squared, and all two-way interactions between AFQT, grandmother's education, grandfather's education, and whether the mother lived in a broken home at age 14. These additional controls are also interacted with the four group indicators. The IV estimates of the coefficient on maternal schooling are presented in the first row of Panel C of Table 13. The results are virtually unchanged by this additional set of controls.

Another specification check is reported in the second row of Panel C, in which we address the possible concern that the four subgroups of interest may follow group-specific trends, by including *group-specific* cohort indicators. Results are essentially unchanged except for PIAT reading. Panel D shows results where we vary the set of instruments we use. First, we include both two-year and four-year tuition measures as instruments. We then show results where we exclude the distance variable (and corresponding interactions), and then both distance and tuition (and corresponding interactions), so that the results rely only on opportunity cost variables. This kind of experiment is interesting as different instruments may affect different subgroups, and this approach has been used to compare returns for different groups (Cameron and Taber 2004). There is of course a loss of efficiency connected to excluding some of the instruments, so the precision of these estimates is somewhat lower. The return in terms of PIAT scores for ages 7–8 goes up. When we exclude tuition as well, the BPI coefficient and the grade repetition coefficient go down and become insignificant. But overall, the results are very similar to the base case.[37]

Another concern is whether the age patterns between ages 7–8 and 12–14 may in part be driven by different samples between the two ages. In particular, some of the children included in the former age group may not have reached the latter in the window we observe. To investigate this, we present the following sensitivity check with a *balanced* panel of children. We restrict attention to all those children who contribute observations to both age brackets. We then select the earliest observation in each of those age groups, and re-estimate our results for our main outcomes. The results are

37. We should also mention that we have estimated more parsimonious models where we include state fixed effects instead of county fixed effects, which resulted in similar estimates to the ones we present.

shown in Panel E of Table 13, and they are very similar to our main results. This is reassuring as it indicates that sample selection is not driving the nature of our results.

5. Summary and Conclusion

In this paper we study the effect of maternal education on their children's outcomes, including cognitive development as measured by test score performance, behavioral problems, grade repetition, and health outcomes. We also examine home environments and parental investments. We instrument maternal schooling with local tuition fees, distance to college, and local labor market variables. In the outcome equations we condition on county and time effects, thus removing the impact of permanent differences and aggregate trends. We obtain additional variation in the instruments by allowing the effect to vary with family background of the mother.

Our results show that mother's education increases the child's performance in both math and reading at ages 7–8, but these effects tend to be smaller at ages 12–14. Maternal education also reduces the incidence of behavioral problems and reduces grade repetition, but we find no effect on obesity. More-educated mothers delay childbearing, are more likely to be married, have substantially better-educated spouses and higher family income. They are more likely to invest in their children through books, providing musical instruments, special lessons, or availability of a computer. Even though they work more, more-educated mothers do not spend less time breastfeeding, reading to their children, or taking them on outings.

A policy implication is that intergenerational transmission is important for understanding long-term policy effectiveness. This is important because many programmes are struggling to improve outcomes for poor children. Programmes which manage to increase mothers schooling are likely to be important not only for mothers now but also for their future children, and should be designed and judged with this in mind.

Our interest in understanding the effect of parental education on children's human capital is closely related to the study of intergenerational mobility. Solon (1999) points out that the high correlation between parental income and their offspring's income is well documented, but that the underlying causes are not very well understood. Our findings suggest that parental educational choices may be an important transmission channel of intergenerational inequality, and support the view that educational policy can influence intergenerational mobility.

Supporting Information

Additional Supporting Information may be found in the online version of this article:

Web Appendix. Online Appendix for "Maternal Education, Home Environments, and the Development of Children and Adolescents" (pdf file)

Replication Files.

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