Intergenerational Effects of Mother's Schooling on Children's Outcomes: Causal Links and Transmission Channels

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Abstract

The objective of this paper is to investigate mother's education as a driving force behind children's schooling outcomes and to explore channels through which the effect of mother's schooling is transmitted. Using matched data from the female participants of the National Longitudinal Survey of Youth (NLSY79) and their children, we study the causal effect of mother's education on children's outcomes when they are aged nine to ten. We exploit geographical and intertemporal variation in mother's schooling cost at the time when the mother grew up. The data allows to control for mother's ability and family background factors. Our results indicate substantial intergenerational returns to education. We find that children's math test score and a measure of grade repetition are significantly affected by mother's education, but we do not find effects on an index of behavioural problems. The rich data set allows us to study different channels which may transmit the effect of mother's education on children's outcomes, including aspects of mother characteristics and parental investments. In particular, we find a significant effect of mother's education on the mother's age when she gave birth to her first child, on available family income and on the cognitive home environment provided by the parents. In line with related literature, we find IV results that are substantially higher than OLS results, indicating heterogeneity in returns.

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1 Introduction

This paper is concerned with the causal impact of mother's schooling on children's outcomes. While it is well known that there is high intergenerational correlation between parental schooling and children's outcomes, relatively little work has been done to study the causal effect of schooling in an intergenerational context. In particular, it is important to take account of unobserved factors which influence both mother's schooling decision and children's outcomes. Many studies linking parent's characteristics to children's outcomes completely ignore this potential source of bias (Behrman 1997).

The literature on returns to education is concerned with separating effects of schooling from unobserved ability, tastes and preferences. This paper transfers the insights won from the relation between education and wages to intergenerational effects of education. To do this, we instrument for mother's schooling by employing schooling cost variables which affect the mother's schooling choice. In order to capture different aspects of cost associated with schooling, we combine different cost variables. Furthermore, we directly control for mother's family background and mother's ability.

A main theme of the paper is to contribute to the literature which explores effects of education apart from the effect on wages. There is growing evidence that education has much wider effects than the observed impact on wages. This paper explores the intergenerational effects of education on the children of the mother. Other studies relate education to crime (Lochner & Moretti 2004) and mortality rates (Lleras-Muney 2005). One objective of this strand of literature is to establish whether social returns to education deviate from private returns, and to provide an indication of the magnitude of social returns. Intergenerational returns to education are of interest both to the individual making educational decisions, and to policy makers, who can affect these decisions through changes in policy parameters.

A focus of this paper is on transmission channels. We want to understand the mechanisms by which mother's schooling affects children. Our rich data set contains many variables about the environment and the behaviour of the children as they grow up, and we use this information to trace the effect of mother's schooling on children's outcomes. This is important because it allows to gain insights into why parent's education may matter for children's outcomes. As possible channels, we consider different mother's characteristics at the birth of her first child: The mother's age at birth, whether the mother is married or not, schooling of the mother's spouse, and family income. To consider a potential quality/quantity trade-off, we also consider the total number of children of the mother. We also look at parental investments into the child as measured by an index of the child's home environment; and finally mother's and child's aspirations in the form of expected eventual school attainment.

The key advantage of our study is that our data set contains detailed information about both mother and child. We are able to control for a variety of family background variables of the mother at the time of her schooling choice. Furthermore, the data set contains a test score of mother's ability, allowing us to avoid omitted ability bias. The detailed observations on mother's and child's characteristics allow us to trace out how the effect of education is transmitted from mother to child, thus providing insight into the intergenerational mechanisms at work.

Throughout the study, we focus on the effect of *mother*'s education, rather than treating both parents symmetrically. This approach is justified by broad evidence that mother's human capital is more closely related to children's schooling outcomes than father's human capital (Haveman & Wolfe 1995, Schultz 2002). We interpret our results as including mating effects. From a policy perspective, this may coincide with the coefficient of interest: If the target group for a potential intervention is a particular subgroup of the population, then the intervention will yield returns from mating as well.

In principle, the effect of mother's education on children's outcomes may vary with the age of the child. Becker (1993, p. 21) hypothesizes that the effect will increase over time due to a multiplicator effect of early differences. On the other hand, the increase in external influence with increasing age may lead to a reduction of the influence of mother's education. In order to provide a clear interpretation, we limit attention to one particular age range, and only consider child outcomes for children aged nine and ten.

The paper is organized as follows: Chapter 2 discusses the relation between mother's schooling and children's outcomes on a conceptual level and clarifies our understanding of channels. We then outline our instrumental variable identification strategy in more detail. The related literature is briefly summarized. Afterwards, we describe our data. Chapter 6 presents our results, and Chapter 7 provides a sensitivity analysis. The last section concludes.

2 Children's Outcomes and Mother's Education

The empirical question of interest is whether mother's schooling causally affects children's outcomes. As outcomes, we consider test score performance, grade repetition, and a measure of behavioural problems. These variables are of interest from an economic point of view: They can be understood as measures of cognitive development and human capital of the child. If human capital increases marginal productivity, higher human capital will result in higher wages. Empirically, test scores are strong predictors for future wages and employment probabilities (Neal & Johnson 1996, Murnane et al. 1995, Currie & Duncan 1999).

Grade repetition implies substantial cost both to the public and to the individual. Apart from the direct cost of schooling, individuals enter the labour force at a higher age (Eide & Showalter 2001). Their work reports a significant positive correlation between grade retention and dropping out of high school, and a significant negative correlation between grade retention and later earnings.

Apart from this micro-econometric evidence, human capital is seen as a driving force behind economic growth (Topel 1999); intergenerational spill-overs of human capital may also be relevant for economic growth.

Conceptually, we are interested to find out how an effect of mother's education is transmitted to a child's outcome. We follow the concept of transmission channels from Currie & Moretti (2003) to trace out potential pathways between parents schooling and children's outcomes. Modelling a child's school performance as the outcome from a human capital production function, there are several inputs to such a production function which may be affected by mother's education. In the following, we suggest different pathways through which mother's education may operate.

First, education may increase the mother's ability to acquire and access information and knowledge relevant for the production of human capital of the child. It may help the mother to make informed educational decisions. In terms of a child's human capital production, this can be interpreted as increasing marginal returns to other inputs (Rosenzweig & Wolpin 1994). Also, the cognitive and emotional surrounding of the child may be a direct determinant of human capital. Second, more education may increase the family's income (either directly or through a mating channel). The causal effect of education on earnings is well documented in a large number of studies (Card 1999). Such an increase in earnings can be interpreted as modifying the budget constraint, allowing to increase inputs in household production (Becker 1965). Higher income would be expected to increase consumption of goods relevant to the education of the child. A third possible alley is through an effect on fertility. If education changes either the number of children or the timing decision, this may translate into changes in children's outcomes. With respect to timing decisions, we focus on the mother's age at the birth of the first child. The hypothesis is that younger mothers are on average less mature, less prepared, and can provide less parenting skills and resources. This implies that there is a positive relation between birth age and child outcomes. Empirically, early child-bearing is associated with poor social and economic outcomes for mother and child. The view that this is a causal relationship has been challenged based on intra-family comparisons (Geronimus et al. 1994, Geronimus & Korenman 1992), but other identification strategies come to different conclusions (Ribar 1999). Fourth, education may change the mother's tastes and preferences, which in turn may lead to different behaviour. This may, for example, affect mother's health and translate into the child's well-being. Furthermore, higher mother's education may lead the mother or the child to have higher scholastic aspirations with respect to the child's advancement in school. Potentially, these aspirations may lead the child to be more successful in school. All of these pathways lead to the hypothesis that higher mother's education leads to improved cognitive development and better schooling outcomes for the child.

3 Related Literature

In his survey on the relation between mother's schooling and children's educational outcome, Behrman (1997) argues that the major drawback of most of the existing literature on intergenerational effects of education is that the effect of unobserved variables which are correlated both with mother's schooling and child's outcome is not accounted for. There is a number of related recent studies which aim to address this issue, using instrumental variables, twins, or adoptees as an identification strategy. In the following, we will briefly review these approaches in turn.

Black et al. (2003) study the effect of parental education on their children's years of schooling using changes in the Norwegian compulsory schooling laws, which increased minimum mandatory schooling from seven to nine years over the period from 1960 to 1972. They find that the instrumental variables (IV) estimates are generally below the OLS estimates; except for the effect of mother's education on son's schooling, they find no significant effects. The authors interpret their results as evidence that the association in educational attainment within families is selection rather than causation.

Chevalier (2004) uses the minimum school leaving age reform in England and Wales in 1972 as instrument for parental schooling. The children are observed when they are between 16 and 18 years of age, and the dependent variable is a dummy for being in higher education. An interesting aspect about this data is that it allows to separate children living with both natural parents from children living with at least one step parent, and the author presents evidence that the effect of parental education differs between these groups.

Oreopoulos et al. (2003) use data on compulsory schooling laws to investigate the effect of parental schooling on the probability of grade repetition and and dropouts in the US. They find that additional parental schooling significantly reduces the probability of grade repetition and dropping out.

Currie & Moretti (2003) study the effect of mother's education on the incidence of low birth weight and preterm birth in the United States over the period 1970 to 1999. They use college openings as instrument for mother's education; their IV results imply that an additional year reduces the probability of low birth weight by about 20 percent, and the probability of premature birth by about 14 percent. As potential transmission channels, the authors consider timely beginning of prenatal care, smoking during pregnancy, marriage and husband's education. A key advantage of this study is the large data set. However, they face certain data limitations: Information is neither available for mother's family background nor for mother's ability. As Carneiro & Heckman (2002, p. 712) point out, many commonly used instruments are highly correlated with mother's ability. This poses problems for the validity of the instruments if mother's ability matters for the child's outcome of interest. In our study, we avoid these problems by controlling for mother's ability. Furthermore, in the work of Currie & Moretti (2003), mother's location (needed to assign the instrument variable) is observed at the time of the birth of the child.

This may be problematic if there is sorting based on unobservables: Assume that there are two types of students (in the following labelled active and passive). If active students relocate to counties where new colleges opened up, and obtain high levels of education, while passive students do not move and obtain less education. If these two types differ in the way they raise their children, the instrument may be invalid. We improve on this in several ways: First, we observe the women at an earlier point in time. Second, we include a variety of family background variables, allowing to capture any selection effects with respect to family location.

The study by Maurin & McNally (2005) uses lower examination standards during the 1968 student revolution in France as an instrument for schooling of the cohorts 1946 to 1952, and report estimates of the effect of parental education on grade repetition. For the father's cohort of 1948 and 1949, they find a large and significantly positive effect on the difference between actual grade and the grade normal for the age. While this study is attractive because of the natural experiment involved, it is difficult to interpret the treatment: The effect could be due to the additional schooling, but also to the experience and the participation in the student revolution.

Behrman & Rosenzweig (2002) use a small sample of identical twins to compute within-twin estimates of the effect of mother's schooling on children's schooling. While OLS results imply a strong positive correlation of parents schooling on child's schooling, the with-in twin effect of mother's schooling is negative. The authors interpret that as evidence of the importance of mother's time spent at home with the children, since more educated mothers participate more in the labour market. Antonovics & Goldberger (2003) criticize these results for being highly sensitive to both sample selection and variable coding. The approach of identifying the effect of schooling on within twin differences is criticized in Bound & Solon (1999, p. 174): They argue that if twins were completely identical, then there would be no reason to expect any differences in educational attainment; it is questionable whether observed differences are random.

In an effort to separate the independent effect of schooling from genetic influences, Plug (2004) compares children who grow up with their biological parents to children who have been adopted. Regressing child's schooling on his or her biological parents' schooling, both parents' education matters significantly. When turning to adopted children, it turns out that the effect of the adoptive mother's schooling does not have a significant influence on the adoptive child's schooling. This is taken as evidence that mother's schooling does not have an independent influence, and that the findings from other studies are mainly driven by genetic endowments and mating effects. We believe that our concept of potential channels can help to interpret their finding. While some children may be adopted immediately after their birth, others are adopted later during their childhood. In the latter case, the outcome will be influenced by the biological mother's schooling. Even if the child is adopted immediately after the birth, the biological mother's schooling may matter, for example through the effect on pre-natal care or health status. We address this by considering channels which relate to very early stages of the child's life cycle. In summary, the findings from Plug (2004) do not necessarily imply that mother's education does not matter. Instead it may be the education of the biological mother that matters, or the effect of mother's education may be concentrated very early in the child's life.

4 Identification Strategy

Let the child outcome be determined as

$$\text{Outcome} \quad = \quad \beta S + \delta X + u \tag{1}$$

where S is mother's years of schooling, X is a set of exogenous covariates, and u is an error term. There are several reasons why OLS estimates may be biased: First, unobserved heterogeneity in tastes and preferences may affect both schooling choice and the child's outcome. Such heterogeneity may lead to a correlation between the error term and the schooling variable. Second, mother's schooling may be measured with error. Therefore, we apply an instrumental variable strategy. This strategy exploits the variation in observed mother's schooling that is due to differences in the cost of schooling. We assume that mother's schooling is determined as

$$S \quad = \quad \gamma Z + \theta X + \epsilon \tag{2}$$

where Z is a schooling cost variable and ϵ is the error term of the schooling equation.

We now turn to the choice of the variable measuring cost of schooling. We use geographical and intertemporal variation in both institutional features of the schooling system and labour market conditions. As additional instruments, we also interact these variables with the mother's family background variables.

One form of cost of schooling is the amount of tuition fees a student faces. Tuition variables have frequently been used as instruments (e.g. Kane & Rouse (1993)). Another major cost of acquiring higher education is foregone earnings. If an individual assesses potential earnings as the expected value of earnings when entering the labour market, then this expected value will be affected by the labour market conditions. We choose two aspects of these conditions: The local unemployment rate and the local blue-collar wage rate. The unemployment rate reflects the probability of not finding a job, and the blue-collar wage rate can be interpreted as the relevant wage for an unskilled worker entering the labour force. In particular, business cycles will affect local labour market conditions, and the individuals may respond to temporary economic downturns by choosing to acquire further education. Local earnings has also been used as an instrument by Cameron & Taber (2004), and state unemployment rate has been studied as an instrument for schooling by Arkes (2005).

For our IV strategy to be successful, two condition need to be satisfied: The schooling cost variables need to be correlated with mother's schooling, and the cost variables must not have an independent effect in the outcome equation except through mother's schooling. Considering the former aspect first. A utility-maximizing individual will choose education to equate marginal costs and marginal benefits. If our instruments affect marginal costs, but leave marginal benefits unchanged, standard assumptions yield the prediction that higher schooling costs reduce the chosen level of schooling. Thus, we expect to find higher wages, higher tuition costs, and lower unemployment rates to be associated with less years of schooling.

Of course, underlying the use of geographical variation in schooling costs is the presumption that *local* variables matter to the choice of the individual. In principal, individuals might move to a different location for their studies, e.g. 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. For example, the student is prevented from the option of living at home (Card 1993, p. 10). Another example of a cost of moving states is that the newcomer may be disadvantaged in the form of higher out-of-state tuition. Currie & Moretti (2002, Table 11) report evidence that student mobility is limited: Their analysis shows that the majority of students do not move to a different state to go to college. Hoxby (1997, pp.46-47) shows that in 1968 and 1981, about 90% of students from public colleges attend college in-state. After including private colleges, the figure is about 80%. She also presents evidence that college choice is significantly determined by cost variables: Higher college tuition and larger distance to college significantly reduce the probability of a college-student match.

The second requirement for our instruments is that they do not have an independent effect on the outcome. We assume that the error terms are mean-independent of the instruments Z,

$$\mathbb{E}[u|Z] \ = 0 \tag{3}$$

The claim we make is that after controlling for mother's family background, mother's ability, and state fixed effects, there is no systematic relation between the instruments and any unobserved factors that affect the outcome of interest. In the following, we address potential criticism of this assumption. We consider systematic location choice (sorting), college quality, and potential correlation between the instrumental variables and regional quality.

Geographical sorting relates to the question whether individuals move to certain counties in a way which creates a correlation between the instrumental variables and unobserved factors. If these factors are relevant for the outcome of interest, they are captured in the error term of the outcome equation. A correlation between the instrument and this error term would make the instrument invalid. To assess this point it is important to understand who makes the location choice. We consider schooling cost variables at the time when the mothers are in their last years of high school. Most of the respondents still live with their parents, and it is the parents who make location choices. Given the fluctuations in the instruments over time, it is unlikely that location choice will be related to the instruments at the time when the mother takes the college decision. We also control for different characteristics of mother's family background. If location sorting occurs based on observable characteristics, we will capture this with the family background variables. Furthermore, in our sensitivity analysis reported below we investigate whether there are indications of systematic sorting by including a control for rural versus urban residence. In summary, given our detailed family background controls and the fluctuations in the schooling cost variables, we do not believe that parental sorting is likely to influence our results.

The second concern relates to college quality. If higher tuition is associated with higher college quality, and if higher college quality makes mothers better at child rearing, then there is a correlation between the instrument and the unobserved college quality, which would bias our results. First, we use tuition from public colleges only; any 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. We also capture any college quality differential between states by including state fixed effects. Therefore, it does not seem likely that, after controlling for mother's ability, mother's family background, and state fixed effects, endogeneity of tuition due to college quality will pose a problem.

The third concern is about a potential relationship between our labour market variables and some unobserved state quality. For example, high wage states may also offer good infrastructure and public services. We address this concern by including state fixed effects in all our regressions. Then, any components of our labour market variables that are constant over time will be absorbed by the state fixed effects, and our labour market variables, which are observed on state level, have the interpretation of business cycle effects. A related concern is about local neighbourhood effects (Solon 1999) on county level. These neighbourhood effects could only affect our results through tuition, which is observed on county level; but we do not believe that there is a connection between neighbourhood effects and tuition rates. In addition to that, we can test for the validity of the instruments by testing the over-identifying restrictions, and we report the results below.

5 Data

We use data from the *National Longitudinal Survey NLSY79*. This data set follows 12,686 young men and women, who are between 15 and 22 years old in the first survey year of 1979. Surveys are conducted annually from 1979 until 1994, and biennial from 1994 onwards. The last currently available survey round is from year 2002. The NSLY79 is a panel in the sense that the same individuals are followed over time. The NLSY79 consists of four sub-samples: Apart from the main cross-sectional sample representative for 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. Similar to Cameron & Taber (2004), we restrict our analysis to the first two sub-samples, excluding the over-sample of economically disadvantaged whites and the sample of the military. This ensures that our sample is restricted to a sample drawn according to pre-determined characteristics. Attrition rates are very low: In the year 2000, after 19 survey rounds, retention rates are still above 80% (Center for Human Resource Research 2001, p. 27).

Sample means and standard deviations can be found in Table 1. We measure mother's schooling as completed years of schooling. For example, this variable takes the value 10 if the respondent completes the sophomore year of high school and then drops out, the value 12 if the respondent completes high school, and the value 13 if the respondent completes the first year of college, and 16 if the respondent completes a four-year college degree. Since we observe mothers over a number of years, we have multiple observations of years of schooling. We are interested in the mother's schooling at the time when the outcome is measured. Occasionally, sample members do not answer this question in the year of interest; to include these observations, we take as measure of schooling the maximum number of completed years reported up to the year of interest.

The data contains detailed information on family background: For example, we know about the schooling of the respondents' parents. Furthermore, the data set contains the mother's score in the Armed Forces Qualification Test (AFQT). This test score is a composite score from different sections of the Armed Services Vocational Aptitude Battery, administered in 1980 (Center for Human Resource Research 2001, p. 93). We use this test as a measure of mother's ability. The original AFQT score may be influenced by the amount of schooling taking up to the test date, but it is possible to estimate the effect of schooling on the test score (Hansen et al. 2004). This estimate can then be used to separate a measure of ability. Throughout the paper, we refer to the AFQT score as this schooling-corrected ability measure, normalized to have mean zero and standard deviation one. We also include two indicator variables for mother's race being black or hispanic, respectively.

In 1986, when the females of the NLSY79 are between 22 and 29 years old, another data set, the *Children of the NLSY79*, is initiated. This data set follows the children of the female members of the NLSY79 over time and contains detailed observations on the child throughout childhood and adolescence. Questionnaires are tailored to the age of the child, and information is collected from both the mother and the child. We match the information on each child of the NLSY79 to the data of the mother. When several children of one mother are contained in the data set, we include all children, but correct standard errors for possible dependence by block-bootstrapping by mothers. The group of mothers evolves over time: While the members of the NLSY79 remain unchanged over time, the fraction of females with children increases over time. In the first waves of the Children of the NLSY79, the data set contains mostly children from the earlier cohorts of mothers, and from mothers that are relatively young. In 2000, the women of the NLSY79 have completed an average of 90% of their expected childbearing (Center for Human Resource Research 2002, p. 6).

We now turn to the children's outcomes of interest. One focus of this paper is on test score performance in a standardized math test. These test scores reflect a child's "ability to learn or to achieve in school" (Smith et al. 1997, p. 132). The Peabody Individual Achievement Test (PIAT) in mathematics measures academic achievement in math, and is both widely used and possesses good reliability and validity properties (Center for Human Resource Research 2002, p. 106). The child is asked to answer 84 multiple-choice questions of increasing difficulty. The child's answers are used to compute a raw score. This raw score in then transformed into an age-specific percentile score. This percentile score is informative about the relative performance of a child compared to other children of the same age. We use this percentile score in all of our regressions, because it allows a natural interpretation in terms of movement within the distribution of test scores.

Children take these tests at different ages. A question of interest is whether the influence of mother's education changes over the course of childhood. In principle, it is perceivable that the influence of mother's schooling changes over time in either direction. For example, as the child gets older, it may be subject to more external influences, and this may reduce the influence of mother's schooling. On the other hand, Becker (1993, p. 21) suggests that differences grow over time because children that are well prepared may find it easier to accumulate human capital. To allow for a clear interpretation of our results, we restrict attention to one particular age group. We focus on test scores when the children are nine or ten years of age. When children take several tests within this period, we use the first score only. Since the questions in the math test cover a considerable range of difficulties, some of them easy for children entering elementary school, some of them difficult for children in their last years of high school, we expect that the test will be able to discriminate effectively between different achievement and ability levels at our chosen age range.

A second measure of school performance considers grade repetition. Our measure, similar to the one used in Oreopoulos et al. (2003), compares the child's actual grade at the interview to the grade we would expect the child normally to be enrolled in. We apply the following simple rule with respect to school entry ages: We assume that every child born in the months from January to September would normally be enrolled in first grade in the fall of the calender year of his or her sixth birthday. We expect children born from October to December to be enrolled in the calender year after their sixth birthday. We use the following formula to determine the normal grade for children born between January and September:

$$\text{Normal grade} = Int\left(\frac{\text{age in months at interview} - 70 + \text{birth month}}{12}\right) \quad (4)$$

For children born between October and December, we compute the normal grade as

$$\text{Normal grade} = Int\left(\frac{\text{age in months at interview} - 82 + \text{birth month}}{12}\right) \quad (5)$$

where $Int(\cdot)$ denotes the floor function. We then take the difference between normal grade and actual grade. We limit attention to those observation where this integer variable lies between -2 and +2. Negative values indicate that a child is ahead of the normal grade, positive values that he or she is held back.

A third outcome we consider is the Behavior Problem Index (BPI). The BPI measures frequency, range, and type of a child's behavioural problems (Center for Human Resource Research 2002, p. 91), and we again take this variable as an age-specific percentile score.

We now turn our attention to the potential channels. First, we look at mother's characteristics. The mother's age at birth is the age in years at the birth of her first child. We also consider an indicator variable for whether the mother is married. To investigate mating effects, we consider the years of schooling of the mother's spouse. Income effects are addressed by looking at log family income. All of these variables are observed in the survey after the birth of the mother's first child. Thus, we take the child's birth as the relevant reference point. To investigate a quality/quantity-tradeoff, we study the effect of mother's education on the number of children, inspired by Becker & Lewis (1973). We observe this variable in the last available survey round (2002).

We then study parental investments by looking at the Home Observation Measurement of the Environment (HOME) assessment, which is an index of the quality of the child's home environment (Center for Human Resource Research 2002, p. 80). We look at two subscales of this assessment: The Cognitive HOME score is the percentile score of the cognitive stimulation subscale. The Emotional HOME score is the percentile measure of emotional support provided by the parents. For the schooling aspirations, mother and child are asked separately to indicate how far they believe the child will advance in school. We create two indicator variables, taking the value 1 if the mother, or the child, respectively, believe that the child will graduate from college or get more education than that. We believe that this variable is informative about mothers' and children's scholastic ambitions.

As instruments, we use local tuition, local wages, and local unemployment. The local tuition variable is an enrollment-weighted average of public four-year colleges in the county in which the respondent lives at 17, based on the Higher Education General Information Survey (HEGIS); in counties with no college, state means are assigned. Local unemployment rate at age 17 is on state-level. Local blue-collar wage rate from the Bureau of Labor Statistics (BLS) is observed on state level at age 17 for cohorts 1959 to 1964, and in 1976 for the first two cohorts. We also use interactions with family background (mother's corrected AFQT, grandparents' schooling, race indicators) as additional instruments.

The data set, limited to the subsamples of interest, contains information on a total of 9,595 children from 3,985 mothers. Of these, we have information on family background, mother's residence at 17, and the schooling cost variables for 6,604 children. Limiting our sample to our selected age range of interest, we observe 4,280 children. Since the outcomes of interest are not observed for every child in the data set, the final sample sizes are 3,978 children (from 2,100 mothers) for PIAT math assessment, 4,240 children (from 2,206 mothers) for grade repetition, and 4,037 children (from 2,164 mothers) for the BPI score.

6 Results

First, we focus on child outcomes as dependent variables. Table 2 shows the first stage estimates for these regressions, regressing years of mother's schooling on the set of instruments, including interactions with family background, and our set of control variables. Since sample sizes differ slightly between the three child outcomes math percentile score, grade repetition, and BPI percentile score, we report the first stage separately for these three samples; as can be seen from Table 2, the estimates are very similar. An increase in local tuition reduces mother's completed schooling; mothers with higher AFQT are more sensitive to increases in tuition. The set of instruments is jointly significant with an F-statistic of around 2.

Results from the second stage are reported in Table 3. Column (1) and (2) show OLS and IV results for the PIAT Math test score. According to the OLS estimate, an additional year of mothers schooling implies that the child will do better on the test by about 1.5 percentage points. The following rows report estimates for family background variables: Increasing mother's AFQT by one standard deviation is associated with an improvement in test score of almost 8 percentage points. Grandparents' schooling also increases the expected test score, while the indicators for black and hispanic show that these groups are associated with lower test scores. The second column reports the corresponding IV estimates. The coefficient on mother's completed years of schooling increases to about 5 percentage points. Although less precisely estimated compared to the OLS results, the effect is still highly significant.

Column (3) and (4) show results for the grade repetition measure. We find a similar pattern here: The OLS estimate indicates that an additional year of mother's schooling advances the child in school by about 0.025 years or 1.3 weeks. The IV estimate is again higher, implying that an additional year of schooling advances the child by about 4.6 weeks. According to this, going to college or not makes a difference of about 0.35 years. How do our estimates compare to those found by other studies? Maurin & McNally (2005, p. 30) use a similar measure of comparing actual to normal grade. They also find a similar pattern of higher IV estimates compared to OLS; but their IV estimates imply that three additional years of parental schooling advance the child by one year, and are thus considerably higher than the results presented here. Oreopoulos et al. (2003, Tables 4-5) use a dummy variable for lagging behind the normal grade. Their result implies that an additional year of mother's schooling reduces the probability of being behind the normal grade by around five to eight percentage points.[1]

The third outcome we study is the Behavioral Problem Index (BPI). While the OLS result indicates that an additional year of mother's schooling is associated with an improvement of the child by about 1.7 percentage points, the IV estimate is insignificant, both because the magnitude of the coefficient drops and because of a larger standard error. In summary, we find that mother's education significantly improves test score performance and reduces grade repetition, but we find no significant effect on the index of behavioural problems.

We now turn to potential channels that may transmit the effect of mother's education to children's outcomes. We first address mother's characteristics and investigate to what extent they are driven by mother's education. These results are reported in Table 4, and we again report OLS and IV estimates for each characteristic. Since our object of interest is a characteristic of the mother, we limit our sample to one child per mother, and take only the first child of each mother. Furthermore, we do not control for child characteristics (age and sex) as we did in the child outcome regressions. Looking at the IV results, we find that an additional year of schooling increases maternal age at first birth by about 1.7 years. If time spent in education and time spent raising children were perfect substitutes, we would expect a coefficient of 1. If increasing age comes with more maturity, we can interpret our finding of delayed child-bearing in the sense that mothers with more education are substantially more mature when they have their first child. We now relate this finding to the claim from Plug (2004), who finds that the adoptive mother's education does not matter. If mother's characteristics around the time of the birth of the child affect the child outcomes, this will not be captured by the adoptive mother's education. Thus, the estimates from Plug (2004) may omit part of the effect of maternal education.

Next we address whether the mother is married at her first birth. While we find that the IV coefficient is larger than the OLS coefficient, the increased standard error implies that the IV estimate is not significant at a 5% level. Turning to mating effects, we consider the schooling of the mother's spouse at the time of her

[1] If we transform our grade repetition measure for comparison purposes into an indicator variable for being behind in school, we obtain a similar estimate to Oreopoulos et al. (2003).

first birth (columns (5) and (6)). Note that our results for this particular channel can only be indicative, since our IV strategy relies on the instruments affecting mother's education, but not spouse's education. If the spouse grows up at the same time in the same county as the mother, the instruments will affect spouse's schooling independently. Since we do not know where the spouse grows up, we cannot assess how likely this is to affect our results. According to our IV results, increasing maternal schooling by one year increases her spouse's schooling by about 0.66 years, implying that a child born to a more highly educated mother can benefit from a substantially higher human capital of the mother's spouse as well. With respect to family income, we focus on log family income in the year of the first child's birth. Both OLS and IV results indicate that an additional year of schooling implies an increase in family income of about 13%. While this estimate may be similar to other returns to education estimates, our measure of income is very different in that we take the entire family income rather than income of one family member, and in that we do not fix a particular age, but rather take the child's birth as a reference point.

We also investigate number of children to investigate a potential quality/quantity tradeoff as suggested by Becker & Lewis (1973), and consider all children reported up to the last survey round (2002). While the OLS results indicates that more highly educated mothers are in fact associated with less children, this effect disappears once we instrument for education.

Table 5 addresses parental investments as a potential channel, focusing on the cognitive and emotional environment provided by parents as measured by the respective subscales of the HOME score. The IV results indicate that in fact there is a significant effect of maternal education on the cognitive environment: An additional year of schooling moves the cognitive percentile score by 4.9 percentile points. In contrast to that, we do not find a significant effect on the emotional subscale of the HOME score.

Finally, we investigate child and mother aspirations to see whether maternal education leads children and parents to be more ambitious with respect to the education of the child. Child and mother are asked separately how far they believe the child will go in school. Our outcome variable is an indicator taking the value 1 if child or mother, respectively, believe that the child will graduate from college (or get more education than that). Our results indicate that an additional year of maternal schooling implies that the probability of high child aspirations increases significantly by more than 6 percentage points.

In summary, our findings indicate that potential transmission channels include mother's age at first birth, mating effects, family income, cognitive environment, and child aspirations.

A key feature of the results presented is that the instrumental variables estimates are consistently higher than the OLS estimates. This finding is common in the returns to education literature (see Card (1999)). There are different possible explanations for this finding, and these explanations are not mutually exclusive.

A first explanation relates to measurement error in reported schooling. In the classical errors-in-variables case it is assumed that the true years of schooling variable is uncorrelated both with the reporting error and the other control variables. Then the OLS coefficient of interest will suffer an attenuation bias (Wooldridge 2002, p. 75). In our application, this would imply that the estimated coefficient of years of schooling is biased downwards. Card (1999, p. 1816) indicates that reliability of self-reported schooling is about 90%, leading to a downward bias of about 10% in the coefficient of interest. Since the increase in the IV estimates relative to the OLS estimates is considerably higher than that, measurement error may explain part of the difference, but not all of it.

A second explanation is heterogeneity in returns: If the intergenerational returns on the outcome of interest to education are not identical for all children, then there is a distribution of returns, and the estimated IV coefficient will be a functional of this distribution. In the model of Imbens & Angrist (1994), the IV estimator can be interpreted as the Local Average Treatment Effect (LATE), which captures the average treatment effect for the subpopulation affected by the instrument. Although their result does not directly carry over to our case (since it is derived for the binary treatment case and a scalar instrument), we still follow their intuition. The IV estimator will then be informative about the return to education for the sub-group of mothers who are affected by the schooling cost variable.

7 Sensitivity Analysis

This section reports results from our sensitivity analysis. First, we report results from an overidentification test. Since our number of instruments exceeds the number of endogenous variables, we can test for the validity of the instruments. We perform a regression-based overidentification test which accounts for clustering; see Baum et al. (2003, p. 18) and Wooldridge (2002, p. 123). Table 3 reports the results for the overidentification tests in the bottom row.[2] We cannot reject the null hypothesis that the instruments are valid. Thus, our argument above in favour of the validity of the instruments is supported by this statistical analysis.

Second, we study how sensitive our estimates are to the choice of instruments by using only subsets of our instruments to estimate the effect of mother's education on children's outcome. Table 6 shows the estimated effect of mother's education on the three child outcomes, using different sets of instruments. The first row shows the results reported in the previous section. Those estimates used local tuition, wages, and unemployment rates as instruments, supplemented by interactions with family background characteristics. The second row excludes local wages, and employs only local tuition and unemployment rate, both including interactions, as instruments, and the following rows employ other subsets as indicated in the table. The results essentially confirm our earlier results. As expected, the reduction in the number of instruments leads to an increase in the standard error of our estimates. The first column shows the results for the PIAT math test. The estimates remain significant at 5%; interestingly, the estimate is highest when we use only tuition as instrument. A similar pattern is observed for grade repetition in the second column. The third column reports the effect of mother's education on the BPI. We do not find any effects here, and that confirms our previous result.

Third, we address the possibility of systematic location choice. One concern might be that families sort into particular areas. For example, families in rural areas might be systematically different from families in urban areas, and this might potentially be driving our results. We investigate this by controlling for whether the mother was brought up in an urban or in a rural environment. The results can be found in the last row of table 6. Again, we find that the results are not sensitive to this inclusion, and we conclude that systematic location choices are not likely to be driving our results.

[2] Note that these regressions are computed for a slightly reduced sample; computation of the test statistic requires absence of singleton dummy variables as controls. Excluding states with only one or two observations, we loose a total of seven (PIAT math) or six (grade repetition and BPI) observations. Coefficients and standard errors are not sensitive to this slightly reduced sample.

8 Conclusion

This paper studies the intergenerational effects of mother's education on children's outcomes, and investigates the channels which transmit the effect. While many existing studies provide intergenerational correlations, they largely ignore bias from unobserved variables. We follow an instrumental variable strategy, using mother's schooling cost variables to identify the causal effect of mother's education on measures of children's schooling performance. The key advantage of our study is that we observe a rich set of covariates, which allow us to control in detail for mother's ability and mother's family background. Furthermore, we observe a variety of possible channels that increase our understanding of how effects of education are transmitted.

We find that mother's education significantly affects children's test scores. According to our estimates, an additional year of mother's schooling causes the child to move about 5 percentage points in the distribution of test scores. For grade repetition, we compare actual grade with expected grade according to age. Our results indicate that college attendance will advance the child by about 0.35 years.

We then turn to transmission channels. Looking at mother's characteristics, we find that mother's education significantly affects maternal age at her birth of her first child, as well as schooling of the mother's spouse and family income. With respect to parental investments, our results show that mother's education improves the cognitive environment provided by the parents, but it does not have a significant effect on the emotional environment. In general, our results indicate that there is heterogeneity in returns, and we interpret our estimated return as a weighted average for those affected by the instruments.

We then present results of our sensitivity analysis. Overidentification tests do not reject our choice of instruments. Using only subsets of our instruments, we find that the nature of our results remains unchanged. In order to analyze possible systematic location choice, we also provide estimates controlling for whether the mother was brought up in a rural or in an urban area, and find that the results are essentially unchanged.

Our results contribute to the growing literature which finds that there are substantial returns to education that are not captured in wages. Future research should attempt to characterize the heterogeneity of returns more directly, and to address what our findings imply for the role of education in intergenerational mobility.

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A Appendix

Table 1: Descriptive sample statistics

	(1) PIAT Math	(2) Grade repetition	(3) BPI
Outcome	52.370	-0.046	60.661
	[28.721]	[0.661]	[27.315]
Mother's yrs. of schooling	12.682	12.673	12.698
	[2.171]	[2.179]	[2.177]
Mother's AFQT (corrected)	-0.079	-0.089	-0.072
	[0.927]	[0.925]	[0.924]
Grandmother's yrs. of schooling	10.452	10.439	10.472
	[3.142]	[3.159]	[3.151]
Grandfather's yrs. of schooling	10.206	10.194	10.241
	[3.945]	[3.941]	[3.939]
Black	0.302	0.301	0.296
	[0.459]	[0.459]	[0.457]
Hispanic	0.194	0.194	0.192
	[0.395]	[0.395]	[0.394]
Child's age (months)	118.965	118.900	118.901
	[6.784]	[6.778]	[6.787]
Child female	0.494	0.492	0.493
	[0.5]	[0.5]	[0.5]
Local tuition	18.188	18.250	18.239
	[7.811]	[7.842]	[7.846]
Local unemployment rate	6.133	6.126	6.123
	[1.424]	[1.429]	[1.431]
Local wages	5.592	5.594	5.601
	[1.569]	[1.56]	[1.556]
Observations	3978	4240	4037

The table reports sample means and (in brackets) standard deviations. Since sample sizes differ slightly between outcomes, we report means and standard deviations separately for each child outcome (PIAT Math, Grade repetition, BPI). *PIAT Math* is the child's percentile score in the Peabody Individual Achievement Test (PIAT). *Grade repetition* compares the child's actual grade with the expected grade based on age and normal progression. This variable takes integer values from -2 to $+2$, where positive values mean that the child is lagging behind. See text for details. *BPI* is the percentile score in the Behavioral Problems Index.

Table 2: First stage regressions (dependent variable: mother's years of completed schooling)

	(1) **PIAT Math**	(2) **Grade repetition**	(3) **BPI**
Local tuition	-0.073	-0.068	-0.059
	[0.035]*	[0.032]*	[0.035]
Tuition * Mother's AFQT (corrected)	-0.018	-0.016	-0.015
	[0.007]**	[0.007]*	[0.007]*
Tuition * Grandmother's yrs. of schooling	0.007	0.006	0.005
	[0.003]*	[0.003]*	[0.003]
Tuition * Grandfather's yrs. of schooling	0.000	0.000	0.000
	[0.002]	[0.002]	[0.002]
Tuition * Black	0.008	0.007	0.004
	[0.014]	[0.013]	[0.013]
Tuition * Hispanic	0.001	0.003	0.006
	[0.023]	[0.021]	[0.021]
Local unemployment rate	-0.245	-0.178	-0.217
	[0.214]	[0.185]	[0.199]
Unemployment rate * Mother's AFQT (corrected)	0.062	0.073	0.072
	[0.046]	[0.038]	[0.040]
Unemployment rate * Grandmother's yrs. of schooling	-0.004	-0.015	-0.010
	[0.019]	[0.018]	[0.019]
Unemployment rate * Grandfather's yrs. of schooling	0.011	0.016	0.015
	[0.012]	[0.012]	[0.012]
Unemployment rate * Black	0.089	0.090	0.094
	[0.076]	[0.072]	[0.073]
Unemployment rate * Hispanic	0.279	0.269	0.228
	[0.134]*	[0.130]*	[0.139]
Local wage	-0.207	-0.156	-0.082
	[0.170]	[0.174]	[0.168]
Wage * Mother's AFQT (corrected)	-0.022	-0.029	-0.022
	[0.035]	[0.034]	[0.035]
Wage * Grandmother's yrs. of schooling	0.028	0.025	0.018
	[0.015]	[0.016]	[0.015]
Wage * Grandfather's yrs. of schooling	-0.010	-0.012	-0.011
	[0.010]	[0.010]	[0.010]
Wage * Black	-0.005	-0.015	-0.031
	[0.066]	[0.063]	[0.065]
Wage * Hispanic	-0.152	-0.174	-0.180
	[0.100]	[0.102]	[0.100]
Mother's AFQT (corrected)	1.056	0.992	0.943
	[0.288]**	[0.260]**	[0.265]**
Grandmother's yrs. of schooling	-0.112	-0.017	0.001
	[0.132]	[0.126]	[0.127]
Grandfather's yrs. of schooling	0.068	0.044	0.050
	[0.084]	[0.087]	[0.089]
Black	-0.185	-0.100	0.005
	[0.497]	[0.458]	[0.485]
Hispanic	-0.379	-0.137	0.121
	[1.034]	[0.978]	[1.016]
Child's age (months)	-0.003	-0.002	0.000
	[0.004]	[0.004]	[0.004]
Child female	0.069	0.057	0.048
	[0.055]	[0.051]	[0.056]
Constant	14.448	16.041	15.425
	[2.100]**	[1.888]**	[2.091]**
Observations	3978	4240	4037
F-stat. of IVs (first stage)	2.22	2.21	1.92
p-value of IVs (first stage)	0.00	0.00	0.01

Note: This table reports estimates from the first stage. Since sample sizes differ slightly between outcomes, we report the first stage separately for each child outcome (PIAT Math, Grade repetition, BPI). Regressors not listed here are state fixed effects. Standard errors are block-bootstrapped (500 replications) by mothers to account for possible clustering within families. * indicates significance at 5%, ** indicates significance at 1% level.

Table 3: Child outcomes (measured at child age 9-10)

	PIAT Math		**Grade repetition**		**BPI**	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Mother's yrs. of schooling	1.463	5.027	-0.025	-0.089	-1.746	0.670
	[0.237]**	[1.738]**	[0.006]**	[0.041]*	[0.286]**	[2.126]
Mother's AFQT (corrected)	7.915	4.375	-0.043	0.020	-0.017	-2.416
	[0.662]**	[1.838]*	[0.014]**	[0.042]	[0.706]	[2.179]
Grandmother's yrs. of schooling	0.481	0.047	0.003	0.011	0.109	-0.197
	[0.208]*	[0.324]	[0.005]	[0.008]	[0.265]	[0.398]
Grandfather's yrs. of schooling	0.277	-0.034	0.001	0.006	-0.316	-0.530
	[0.162]	[0.209]	[0.003]	[0.005]	[0.196]	[0.265]*
Black	-11.320	-13.046	0.029	0.061	2.421	1.226
	[1.350]**	[1.672]**	[0.029]	[0.036]	[1.399]	[1.742]
Hispanic	-4.983	-6.521	0.053	0.087	-1.902	-3.247
	[1.764]**	[1.923]**	[0.038]	[0.045]	[1.822]	[2.326]
Child's age (months)	-0.065	-0.052	-0.004	-0.004	0.073	0.073
	[0.056]	[0.059]	[0.001]**	[0.001]**	[0.059]	[0.066]
Child female	-1.167	-1.381	-0.110	-0.107	-4.691	-4.792
	[0.840]	[0.840]	[0.021]**	[0.020]**	[0.875]**	[0.886]**
Constant	59.268	40.193	-0.100	1.624	69.120	38.572
	[14.898]**	[25.533]	[0.473]	[0.677]*	[20.153]**	[33.399]
Observations	3978	3978	4240	4240	4037	4037
R-squared	0.23	0.18	0.05	0.03	0.05	0.03
F-stat. of IVs (first stage)		2.22		2.21		1.92
p-value of IVs (first stage)		0.00		0.00		0.01
Overidentification test statistic		12.86		25.00		10.08
Overidentification test p-value		0.75		0.09		0.90

Note: *PIAT Math* is the child's percentile score in the Peabody Individual Achievement Test (PIAT). *Grade repetition* compares the child's actual grade with the expected grade based on age and normal progression. This variable takes integer values from -2 to $+2$, where positive values mean that the child is lagging behind. *BPI* is the percentile score in the Behavioral Problems Index. Regressors not listed here are state fixed effects. Standard errors are block-bootstrapped (500 replications) by mothers to account for possible clustering within families. * indicates significance at 5%, ** indicates significance at 1% level. F-test refers to the hypothesis that all instruments are jointly zero on the first stage. Overidentification test refers to a regression based test of overidentification which accounts for clustering; see Baum et al. (2003, p. 18) and Wooldridge (2002, p. 123). See text for details.

Table 4: Mother's characteristics

	Mother's age at first birth		Mother married at first birth		Spouse's schooling at first birth		Log family income at first birth		Number of children in last survey round	
	(1)	(2)	(3)	(4)	(5)	(6)	(5)	(6)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Mother's yrs. of schooling	1.463	1.698	0.041	0.070	0.547	0.659	0.132	0.131	-0.076	0.009
	[0.044]**	[0.326]**	[0.004]**	[0.041]	[0.028]**	[0.160]**	[0.008]**	[0.065]*	[0.012]**	[0.104]
Mother's AFQT (corrected)	-0.253	-0.509	0.033	0.001	0.147	0.021	0.133	0.133	0.008	-0.089
	[0.116]*	[0.371]	[0.011]**	[0.046]	[0.066]*	[0.185]	[0.019]**	[0.070]	[0.031]	[0.121]
Grandmother's yrs. of schooling	0.020	-0.015	0.000	-0.004	0.067	0.051	0.012	0.012	-0.007	-0.019
	[0.036]	[0.059]	[0.003]	[0.007]	[0.025]**	[0.035]	[0.006]*	[0.011]	[0.010]	[0.018]
Grandfather's yrs. of schooling	0.000	-0.016	-0.003	-0.005	0.059	0.049	0.009	0.009	-0.008	-0.014
	[0.027]	[0.036]	[0.003]	[0.004]	[0.016]**	[0.020]*	[0.004]*	[0.007]	[0.008]	[0.011]
Black	-2.327	-2.398	-0.500	-0.508	-0.057	-0.150	-0.412	-0.412	0.190	0.137
	[0.226]**	[0.240]**	[0.022]**	[0.024]**	[0.148]	[0.205]	[0.035]**	[0.040]**	[0.066]**	[0.091]
Hispanic	-0.272	-0.402	-0.093	-0.109	-0.282	-0.366	-0.104	-0.104	0.224	0.170
	[0.293]	[0.311]	[0.028]**	[0.039]**	[0.192]	[0.220]	[0.050]*	[0.062]	[0.079]**	[0.096]
Constant	4.159	2.147	0.342	-0.021	6.416	5.056	1.677	1.680	3.174	2.288
	[4.795]	[5.552]	[0.247]	[0.504]	[2.282]**	[2.799]	[0.756]*	[1.023]	[0.771]**	[1.368]
Observations	2803	2803	2782	2782	1859	1859	2736	2736	2378	2378
R-squared	0.44	0.43	0.31	0.30	0.43	0.42	0.37	0.37	0.07	0.05
F-stat. of IVs (first stage)		2.00		1.96		2.55		2.00		2.08
p-value of IVs (first stage)		0.01		0.01		0.00		0.01		0.00

Note: *Mother's age at first birth* is the age of the mother in years at birth of the first child. *Mother married* is an indicator variable, taking the value 1 if the mother is married at time of the first survey following the child's birth, and 0 otherwise. *Spouse's schooling* is the number of years of schooling completed by the mother's spouse, as indicated in the first survey following the child's birth. *Log family income* is the logarithm of average family income; average family income is the average of all non-missing observations within a five year window around the first survey after child's birth. *Number of children* is the total number of children as reported in the last available survey year, 2002. See text for details. Regressors not listed here are state fixed effects. Standard errors are bootstrapped (500 replications). * indicates significance at 5%, ** indicates significance at 1% level. F-test refers to the hypothesis that all instruments are jointly zero on the first stage.

Table 5: Parental investments and aspirations (measured at child age 9-10)

	Cognitive HOME score		**Emotional HOME score**		**Child's aspirations**		**Mother's aspiration**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Mother's yrs. of schooling	2.521	4.882	0.856	0.534	0.034	0.065	0.051	0.036
	[0.308]**	[2.199]*	[0.318]**	[2.157]	[0.006]**	[0.033]*	[0.005]**	[0.031]
Mother's AFQT (corrected)	3.619	1.286	1.661	1.983	0.029	-0.001	0.085	0.101
	[0.773]**	[2.221]	[0.797]*	[2.243]	[0.017]	[0.035]	[0.014]**	[0.032]**
Grandmother's yrs. of schooling	1.052	0.745	0.053	0.092	0.001	-0.003	0.007	0.009
	[0.243]**	[0.401]	[0.252]	[0.358]	[0.005]	[0.007]	[0.004]	[0.007]
Grandfather's yrs. of schooling	0.188	-0.024	0.576	0.606	0.007	0.004	0.004	0.006
	[0.209]	[0.293]	[0.192]**	[0.286]*	[0.004]	[0.005]	[0.003]	[0.004]
Black	-11.044	-12.172	-17.076	-16.912	-0.024	-0.041	-0.015	-0.007
	[1.455]**	[1.836]**	[1.565]**	[1.887]**	[0.031]	[0.036]	[0.026]	[0.030]
Hispanic	-5.136	-6.394	-1.735	-1.566	0.023	0.015	0.049	0.057
	[1.921]**	[2.303]**	[1.984]	[2.235]	[0.041]	[0.045]	[0.035]	[0.038]
Child's age (months)	0.253	0.257	0.110	0.110	0.004	0.004	-0.003	-0.003
	[0.059]**	[0.059]**	[0.067]	[0.072]	[0.003]	[0.003]	[0.001]*	[0.001]*
Child female	5.255	5.123	-0.271	-0.253	0.075	0.076	0.087	0.087
	[0.870]**	[0.870]**	[0.959]	[0.938]	[0.022]**	[0.022]**	[0.017]**	[0.017]**
Constant	12.440	-9.937	13.624	67.308	-0.116	-0.426	0.074	0.513
	[27.464]	[38.543]	[25.523]	[34.970]	[0.600]	[0.739]	[0.346]	[0.570]
Observations	3866	3866	3659	3659	1649	1649	2947	2947
R-squared	0.22	0.20	0.13	0.13	0.09	0.07	0.19	0.19
F-stat. of IVs (first stage)		2.10		2.22		2.42		2.43
p-value of IVs (first stage)		0.00		0.00		0.00		0.00

Note: *Cognitive HOME score* is the percentile score of the cognitive stimulation subscale of the Home Observation Measurement of the Environment (HOME) assessment. *Emotional HOME score* is the percentile score of the emotional support subscale of the HOME assessment. *Child's aspirations* is an indicator variable, taking the value 1 if the child expects to graduate from college or get more than four years of college, and 0 otherwise. *Mother's aspirations* is an indicator variable, taking the value 1 if the mother believes the child will graduate from college or take further training after college, and 0 otherwise. See text for details. Regressors not listed here are state fixed effects. Standard errors are block-bootstrapped (500 replications) by mothers to account for possible clustering within families. * indicates significance at 5%, ** indicates significance at 1% level. F-test refers to the hypothesis that all instruments are jointly zero on the first stage.

Table 6: Sensitivity analysis

	(1) PIAT math	(2) Grade repetition	(3) BPI
Base case	5.027	-0.089	0.670
	[1.738]**	[0.041]*	[2.126]
excluding local wage	6.752	-0.077	-0.185
	[2.177]**	[0.056]	[2.440]
excluding local unemployment rate	4.761	-0.078	2.131
	[2.101]*	[0.054]	[2.605]
excluding wages and unemployment rate	7.056	-0.165	-0.279
	[3.435]*	[0.104]	[4.467]
Base case + indicator "rural"	5.182	-0.092	0.445
	[1.692]**	[0.044]*	[2.008]

Note: *PIAT Math* is the child's percentile score in the Peabody Individual Achievement Test (PIAT). *Grade repetition* compares the child's actual grade with the expected grade based on age and normal progression. This variable takes integer values from -2 to $+2$, where positive values mean that the child is lagging behind. See text for details. *BPI* is the percentile score in the Behavioral Problems Index. Regressors not listed here are family background characteristics (mother's AFQT (schooling-corrected), grandmother's years of schooling, grandfathers years of schooling, indicators for black and hispanic, child age, child sex, and state fixed effects. Standard errors are block-bootstrapped (500 replications) by mothers to account for possible clustering within families. * indicates significance at 5%, ** indicates significance at 1% level.