The Intergenerational Effects of Compulsory Schooling

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This article attempts to improve our understanding of the causal processes that contribute to intergenerational immobility by exploiting historical changes in compulsory schooling laws that affected the educational attainment of parents without affecting their innate abilities or endowments. We examine the influence of parental compulsory schooling on children's grade-for-age using the 1960, 1970, and 1980 U.S. censuses. Our estimates indicate that a 1-year increase in the education of either parent reduces the probability that a child repeats a grade by between 2 and 4 percentage points.

I. Introduction

A high degree of persistence in economic status exists across generations, but we know very little about the causal processes that drive this phenomenon. For example, children who grow up in more highly

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educated families have better labor market outcomes as adults than children who grow up in less educated families, but we do not know whether this is because education changes something about childhood experiences or because genetic or environmental factors that contributed to the parents' educational levels are shared by their children. This article attempts to improve our understanding of the causal processes that contribute to intergenerational immobility by exploiting historical changes in compulsory schooling laws that affected the educational attainment of parents without affecting their innate abilities. Quantifying the extent to which children's well-being can be improved by increasing their parents' education also has important implications for public policy: most discussions about the government's role in providing educational aid, for example, focus on the individual's return to education and ignore the possibility of social benefits. Knowing that there are intergenerational returns to increased schooling would provide a further rationale for such programs.

Few studies have attempted to isolate the causal effect of education on the next generation's well-being.[1] This is at least partly due to the fact that it is difficult to find plausible sources of identifying variation. It is also hard to find large, nationally representative data sets that simultaneously provide information on parental characteristics and children's outcomes. Our use of compulsory schooling laws applied to census data allows us to overcome both of these problems. Several studies have already demonstrated a strong relationship between these laws and individuals' educational attainment and have used this relationship to identify the effects of education on earnings (Acemoglu and Angrist 2000), criminal activity (Lochner and Moretti 2004), mortality (Lleras-Muney 2005), and subjective measures of well-being (Oreopoulos 2003), but this is the first study to estimate the intergenerational effects of the U.S. laws.[2]

Our main analysis is based on a sample of children ages 7–15 taken from the 1960, 1970, and 1980 individual U.S. census files. We examine the effects of parental education on children's human capital accumu-

[1] By "causal" we mean the effect of an exogenous increase in education itself, rather than the simple correlation between a child's outcome and her parents' education, which also reflects the effect of innate parental characteristics. Changes in parental schooling levels may change many family background characteristics such as income, choice of marriage partner, and decisions about children's education. Our IV estimates will capture the effect of all family background characteristics that are affected by education but will not reflect the effect of innate parental characteristics such as intelligence.

[2] On the relationship of the laws and individuals' educational attainment, see Angrist and Krueger (1991), Goldin and Katz (2003), and Lleras-Muney (2005). Harmon and Walker (1995) and Oreopoulos (2003) find that a similar relationship exists in the United Kingdom and Canada.

lation and find that it has substantial and significant positive effects. A 1-year increase in parents' combined schooling reduces the probability that a child is at the normal grade given her age by 2–4 percentage points. This effect is somewhat larger than OLS estimates would suggest. We also find evidence that among teenagers still living at home, parents' educational attainment decreases children's likelihood of dropping out of high school.

II. Background Literature

A few previous studies have attempted to isolate the causal influence of parental education by using variation within sibling or twin pairs (Rosenzweig and Wolpin 1994; Behrman and Rosenzweig 2002; Currie and Moretti 2003). This enables them to control for mother fixed effects but may exacerbate biases if within-pair differences in characteristics affect outcomes independent of their effect on education.[3] Such estimates are also known to be more prone to measurement error problems (Griliches 1979). Currie and Moretti (2003) also isolate the effect of mothers' education on birth outcomes using county-level data on college openings between 1940 and 1990 to capture differences in the availability of educational services among different cohorts of women. Sacerdote (2002, 2004) compares outcomes of adoptees who were randomly assigned to their adoptive parents. This approach is appealing because it is impossible to ascribe the intergenerational link to a genetic inheritance, but an observed correlation could still be driven by parental characteristics that are not genetically transmitted to children but that nevertheless affect both individual and offspring's human capital accumulation. Across these studies, the magnitude and precision of the estimated effects of parental education varies, but most find that parental education has at least a small impact on children's outcomes.[4]

Most existing studies have also relied on small, nonrepresentative data sets. The Behrman and Rosenzweig study, for example, uses samples of 212 female twin pairs and 122 male twin pairs who participated in the Minnesota Twin Registry. Rosenzweig and Wolpin (1994) use a sample of unusually young mothers from the National Longitudinal Survey of Youth. Likewise, Sacerdote's analyses focus on fewer than 200 adoptees per sample who were born in Britain (the first sample) or adopted in Colorado (the second sample).

[3] Behrman, Rosenzweig, and Taubman (1994), e.g., find that birth weight differences between twins correlate significantly with schooling differences and subsequent earnings. Recent work by Antonovics and Goldberger (forthcoming) questions the robustness of Behrman and Rosenzweig's (2002) results.

[4] In a related paper, Bleakely and Chin (2004) show that children of immigrant parents with stronger English language skills have better educational outcomes, but they do not explicitly estimate the causal effect of parental education.

Recent papers by Chevalier (2003) and Black, Devereux, and Salvanes (2005) also use changes in compulsory schooling laws to identify the effect of parental education on the next generation's outcomes. Chevalier's study uses a change in the compulsory schooling requirement that took place in Britain in 1957. He finds large effects of mother's education on children's educational attainment but does not find statistically significant effects of father's education. Because identification is based on a single change in the minimum schooling law, however, he is unable to disentangle compulsory schooling effects from cohort effects. Chevalier's sample of children is also confined to those who are still living at home with their parents, and the distribution of educational attainment among children living at home will be different from that in the population.

Black et al. examine the effects of an increase in mandatory schooling from 7 years to 9 years, which was phased in across municipalities in Norway between 1959 and 1973. Using the timing of the law changes to instrument for parental education, they find weak (but not statistically significant) evidence of a causal relationship between parental education and children's outcomes. They do find, however, that among mothers with low levels of education, the mandated increase in education had a statistically significant effect on their children's educational attainment.

The larger variation in compulsory school law changes in the United States and the larger samples from the U.S. censuses enable us to arrive at much more precise estimates than the Norwegian analysis. Institutional differences across countries may also lead to different results. Cross-country comparisons of intergenerational mobility find that Americans exhibit less mobility than most Europeans (Solon 1999; Bjorklund et al. 2002). Finally, the Black et al. estimates may be downward biased because all of the children in the sample are subject to the 9-year schooling requirement, whereas only some of the parents are affected by the change in the law. We are able to avoid this because we focus on children's grade retention instead of completed education.

III. Empirical Strategy

Let y_{if} denote a relevant outcome for child i living in family f. Suppose that the true model for y_{if} is

$$y_{if} = \alpha + \beta_1 FathEd_f + \beta_2 MothEd_f + \beta_3 X_f + \varepsilon_{if}, \tag{1}$$

where $FathEd_f$ and $MothEd_f$ indicate the educational attainment of the child's father and mother, X_f is a vector of all the other family background characteristics that affect the child's outcome, and ε_{if} is an error term representing the effects of individual specific factors that are uncorrelated with family background. If we could observe everything that belongs in

X, then estimates of β_1 and β_2 would capture the effects of parents' educational attainment that are independent of their other characteristics. Because this is not possible, we address the omitted variables problem by using U.S. compulsory school laws as instruments for completed education. Schmidt (1996), Lleras-Muney (2002), and Goldin and Katz (2003) have documented the effectiveness of these laws from 1910 to 1940, specifically examining the effects of the latest age allowed before requiring school entry, the minimum school leaving age, and the minimum age at which a child could obtain a work permit to exempt her from school. All three studies conclude that these laws had modest but statistically significant effects on educational attainment. For example, Goldin and Katz (2003) conclude that changes in compulsory school legislation over this 30-year period account for about 5% of the more than doubling of secondary school enrollment.

Acemoglu and Angrist (2000) have also collected and recorded information on compulsory schooling laws between 1915 and 1969 and have used them to estimate both individual and social returns to education. They simplify the laws by converting them into two variables: the minimum length of time required in school before being allowed to leave and the minimum length of time required in school before being allowed to obtain a work permit. While both types of laws were influential, work exemptions often allowed students to leave school before the minimum school leaving age. Recognizing this, our instruments are based on the minimum number of years of schooling required in order to obtain a work permit, and we use Acemoglu and Angrist's data collection and simplification.[5] Estimates based on school leaving age restrictions are very similar.[6] In the following section, we describe more precisely how we utilize their information on the laws.

In practice, the model we estimate is slightly more restrictive than equation (1) because we use the sum of mother's and father's completed education as the key regressor instead of including each parent's education separately. When we include each parent's education separately in the same regression, the standard error estimates that are produced are too large to be able to discern differences between the effects of mother's and father's education. This is because the instruments for fathers' and mothers' education (the compulsory schooling laws) are very highly correlated. We have also conducted separate analyses of the effects of mothers' and fathers' education, recognizing that if mother's and father's educational

[5] The variables collected by Acemoglu and Angrist, Goldin and Katz, and Lleras-Muney are very similar. Using these alternative sets of compulsory schooling variables for our analysis yields very similar IV estimates. We chose to work with Acemoglu and Angrist's laws because they allowed us to examine parents from a wider range of birth cohorts.

[6] Available from the authors.

attainments are correlated, then the estimated education coefficient in each regression will also reflect the effect of the omitted parent's schooling level. Since separate analyses complicate the interpretation of the education coefficients, we instead use the sum of parents' completed education as the regressor of interest, which allows us to directly estimate the effect of a 1-year increase in either parent's schooling level.[7] Total parental education is approximately twice as large in two-parent families as in single-parent families, so we estimate separate regressions for the two groups, noting that our estimates will not capture any indirect effect of education on children via family structure.[8]

A related issue is that changes in education induced by the compulsory schooling laws could also affect fertility and thus alter the sample of children we observe. McCrary and Royer (2005) find no evidence that education affects fertility, but Black, Devereux, and Salvanes (2004) find that compulsory schooling laws have a negative effect on the probability of having a child as a teenager. Using a sample of 15–60-year-old household heads and spouses from the 1960–80 censuses, we find no evidence that compulsory schooling laws affect the probability of being a parent. This suggests that while the laws may affect the timing of fertility, they do not affect the probability of ending up in our sample. Since we condition on the age of the parents, our estimated education coefficients will not reflect the effects of early childbearing.

We aggregate our data by parental state of birth, parental birth cohort, and census year.[9] Specifically, a cell is defined by father's state of birth, mother's state of birth, father's birth year, and mother's birth year. This approach recognizes that our identifying variation occurs at the parental state of birth by birth cohort level. After generating cell means for all

[7] Of course, this specification assumes that the effect of father's education and mother's education are the same. As discussed above, the compulsory schooling laws do not vary enough between parents to determine whether the effects are, in fact, different. Results with regressions run separately for mothers and fathers are provided in Oreopoulos, Page, and Stevens (2003).

[8] We have used our sample to estimate IV regressions of the probability of living in a two-parent family on mother's education (using the compulsory schooling variables as instruments) and find that education has no effect on marital status. The coefficient estimate is -0.007, and the standard error estimate is 0.006.

[9] We have also run individual level regressions, regressions that are based on aggregation to the parental state and year of birth only, and regressions in which the cells are defined by parental state of birth/parental year of birth/census year and child's state of residence. The latter specification allows us to include state fixed effects, which control for differences in educational quality experienced by children across states. These specifications all produce very similar estimates. We aggregate separately by census year because we wish to include census year fixed effects, which control for changes in completed education as the cohorts age across census years, along with minor differences across survey instruments.

variables in the regression specification, we estimate the following first-stage equation:

$$\begin{aligned} ParEd_{yjklm} = {} & \alpha_0 + \gamma_1 CL7_{j,k+14} + \gamma_2 CL8_{j,k+14} + \gamma_3 CL9_{j,k+14} \\ & + \eta_1 CL7_{l,n+14} + \eta_2 CL8_{l,m+14} + \eta_3 CL9_{l,m+14} \\ & + \phi X_{yjklm} + u_y + u_j + u_k + \mu_l + \mu_m + u_{jk} + \mu_{lm}, \end{aligned} \tag{2}$$

where $ParEd_{yjklm}$ is parental (the sum of mother's and father's) education for youths observed in census year y, with fathers from state j born in year k, and mothers from state l born in year m. The vector X includes variables that capture the child group's percentage of black, Asian, female, and average age. We also include fixed effects for the relevant census year, parent's state of birth, and parent's year of birth. In the second stage we estimate

$$y_{yjk} = \delta_0 + \delta_1 ParEd_{yjklm} + \delta_2 X_{yjklm} + \varepsilon_y + \varepsilon_j + \varepsilon_k + \varepsilon_l + \varepsilon_m + \varepsilon_{yjk}, \tag{3}$$

where y_{yjklm} is the average child outcome for each cell. Standard errors in each stage will be estimated using Huber-White standard errors and clustering by parent's state and year of birth.[10] The excluded instruments are the dummy variables $CL7$, $CL8$, and $CL9$ (for each parent), which denote required years of schooling prior to obtaining a work permit of 7 years, 8 years, or 9 or more years. More detail on these variables is given in the data section.

Identifying the effects of the compulsory schooling laws on parental education is made possible through differences in the timing of the changes in these laws across states. One concern is that states that raised compulsory schooling requirements might also be those that did a better job of educating the second generation. Consider, for example, how changes in the educational attainment of adults in the child's state, brought on by changes in compulsory schooling laws, might affect support for programs that improve the quality of schools. If more highly educated adults are more likely to support school taxes, and higher school expenditures affect children's human capital, then compulsory schooling laws will not allow us to isolate the effect of growing up in a more educated family from the effect of growing up in a more educated community. Fortunately, the literature suggests that any "spillover" effects of the laws are small. Goldin and Katz (2003), for example, find that compulsory schooling laws only explain about 5% of the convergence in educational attainment between 1915 and 1940. In addition, the laws appear to have had no impact on educational attainment beyond grade 12. This, combined with the fact that those affected by a change in the law will comprise only a small

[10] Clustering at the state level does not affect inference.

fraction of the total stock of adults, suggests that changes in compulsory schooling had minimal affects on the education level of the adults in the child's community. Furthermore, Acemoglu and Angrist (2000) find little evidence that education generates positive externalities (in the form of higher wages). Nevertheless, we take the possibility of instrument endogeneity seriously and employ a number of strategies to verify that our compulsory schooling variables are not picking up the effect of other changes in the child's environment. We discuss these strategies in more detail in Section IV.

IV. Data

A. Census Data

Our analysis uses data from the Integrated Public Use Microdata Series (IPUMS) created by the Census Bureau. The IPUMS consists of individual and household level data from the decennial census and includes nearly all of the detail originally recorded by the census enumerations. Information exists at the individual level on a broad range of individual characteristics, including fertility, marital status, immigration, labor-force participation, income, occupational structure, education, ethnicity, and household composition. We use the 1% samples from the 1960, 1970, and 1980 censuses, which create for us a data set of 711,072 children living in two-parent families and 129,632 children living in single-parent families. The size of our data set is an enormous advantage in terms of enabling us to obtain precise estimates.

The main disadvantage of the IPUMS, and the reason that it has not been used for intergenerational mobility studies, is that it is a cross-sectional data set that contains little information on children's outcomes. It does, however, contain information on each individual's level of educational attainment. We use this information, together with information on the child's age, to determine whether or not she has repeated a grade. Grade repetition is a widespread phenomenon in the United States and is correlated with many more commonly used measures of educational achievement and socioeconomic success.[11] This suggests that if we can obtain estimates of the causal effect of parental education on the prob-

[11] A report from the National Longitudinal Study of Adolescent Health, for example, indicates that over 20% of American adolescents have repeated a grade (Resnick et al. 1997), and Feldman (1997) estimates that in many urban districts more than half of all students will be retained. There is also evidence that grade retention is correlated with other measures of children's success. The National Center for Education Statistics (1997) estimates that approximately one-quarter of young adults who had repeated a grade had dropped out of school by 1995. Similarly, Smith and Shepard (1989) find that students who have previously repeated a grade tend to have worse academic outcomes than similar students who have not repeated a grade.

ability that a child repeats a grade, we will also gain insight into the causal relationship between parental education and children's long-run success.

Determining whether or not a child has repeated a grade is complicated by the fact that there is variation across states and over time in the minimum age at school entry, incomplete information on school entry cutoff dates across states and over time, and questions about the degree to which school districts comply with those dates.[12] We classify a child as a repeater if her educational attainment is below the median for her state, age, quarter of birth, and census year cell. Details about the construction of this measure and alternative classification schemes can be found in the appendix. Our results are not sensitive to the particular measure we use. It is important to note, however, that our measure includes children who enter school late, even if they are never held back. For this reason, it may be more appropriate to think of our dependent variable as a measure of grade-for-age.

The analysis focuses on children between the ages of 7 and 15 years. Children younger than age 7 are not included because they are not old enough to have had the opportunity to repeat a grade. We exclude children older than age 15 in order to avoid overrepresenting children who left home at late ages. To adjust for the fact that older sample members have had more of an opportunity to repeat a grade, and to adjust for possible gender differences in grade repetition, all of our regressions include controls for age and gender. Summary statistics for our samples are shown in appendix table A1.

B. Compulsory Schooling Laws

Data on compulsory schooling laws have previously been collected by Acemoglu and Angrist and by Lleras-Muney. Lleras-Muney's collection of compulsory schooling data is available at http://www.princeton.edu/˜alleras/papers.htm. The Acemoglu and Angrist (2000) data we utilize are summarized in more detail in their appendix. As noted above, we follow earlier authors by using a variable designed to capture the minimum number of years of schooling that would be required before an individual is eligible for a work permit. Specifically, we use the following variable to instrument for education: CL = max(required years of schooling before

[12] We have collected state-level information on minimum school entry ages and cutoff dates for 1955 and 1965: 43 states in 1955 and 32 states in 1965. Twenty-two states maintained the same cutoff dates over the 10-year period, and seven states changed their cutoff date. Missing information prevents us from determining what happened in the remaining states. Incorporating this information into our definition of grade repetition does not alter our measure. One reason for this is that we know quarter of birth, but not month of birth, and so variation in entry cutoffs from October to December, e.g., does not help us to further distinguish potential repeaters.

Table 1
State Mandatory Schooling Laws by Year

	Number of States with Mandatory Years ≤ 7	≤ 8	≤ 9	Total States Observed
1915	32	46	47	47
1920	30	47	47	47
1925	15	33	48	49
1930	14	29	48	49
1935	9	21	48	49
1940	7	21	47	49
1945	7	21	47	49
1950	5	15	34	49
1955	3	15	32	49
1960	4	12	31	49
1965	3	11	29	49
1970	3	9	23	41

receiving a work permit; minimum age required for a work permit-enrollment age).

In order to capture a potentially nonlinear relationship between years of schooling required and educational attainment, we use the CL variable to create dummy variables of the form $CL6 = 1$ for $CL \leq 6$, and zero otherwise; $CL7 = 1$ for $CL = 7$, and zero otherwise; $CL8 = 1$ for $CL = 8$, and zero otherwise; $CL9 = 1$ for $CL \geq 9$, and zero otherwise. These dummies are matched to parents in our sample based on the school entry laws that were in place in their state of birth when they were 6 years old and the school leaving laws that were in place when they were 14 years old. We chose these as the relevant matching years because they are the lowest common entry and leaving ages across states. Although we would prefer to match according to the parents' state of residence at ages 6 and 14, that information is not available in the census. This inevitably leads to some mismatches, but the resulting errors are unlikely to be correlated with the laws because the laws were not likely a motivating factor behind cross-state moves. Over 90% of parents in our sample faced 6–9 years of mandatory compulsory schooling.

V. Results

A. First-Stage Results

Before turning to our IV estimates, we look at the results from the first stage, in which we regress parental education and compulsory schooling laws. Table 1 shows the frequencies of compulsory schooling laws over

Table 2
First Stage: Effects of Compulsory Schooling Laws on Parents' Education (Dependent Variable: Parents' Highest Grade of Schooling)

	Dual-Parent Families (Sum[Fathers' + Mothers' Ed])			Single-Parent Families (Head's Ed)		
	All	<Grade 12	>Grade 12	All	<Grade 12	>Grade 12
Years of schooling required before work permit:						
Father:						
7	.320	.286	.158	.282	.123	.185
	(.057)	(.066)	(.033)	(.080)	(.075)	(.074)
8	.443	.282	.011	.464	.348	.029
	(.051)	(.064)	(.057)	(.072)	(.067)	(.064)
9	.723	.543	.095	.733	.547	.085
	(.066)	(.086)	(.077)	(.085)	(.085)	(.081)
Mother:						
7	.304	.311	−.030			
	(.061)	(.072)	(.065)			
8	.470	.473	−.034			
	(.054)	(.067)	(.058)			
9	.748	.646	.027			
	(.069)	(.086)	(.077)			
Initial sample size:						
No. of cells	182,072	58,692	43,102	5,249	4,630	3,376
Adjusted R^2	.341	.334	.096	.690	.552	.248
Adjusted R^2 without instruments	.338	.331	.095	.682	.544	.247
F-test: instruments = zero	84.4	31.14	2.61	32.2	21	3.3

NOTE.—All regressions include fixed effects for parent's birth year, state of birth, and census year. Additional controls include percentage of black, Asian, female, and children at each age in each cell. Data are grouped into means by parent's birth year, state of birth, and census year, and weighted by cell population size. Huber-White standard errors are shown from clustering by birth cohort and state of birth. The omitted compulsory school law variable indicates whether the minimum years of schooling required before one is able to obtain a work permit was 6 or less. Standard errors are in parentheses. See text for more data specifics.

time.[13] For example, in 1940 there were seven states that required 7 or fewer years of schooling, 14 states that required exactly 8 years of schooling, and 26 states that required 9 years. Moreover, between 1915 and 1970, required years increased to more than seven in 29 states, while 24 states increased the required number of years to more than 10. About one-third of the variation in the laws is across states, and about two-thirds is within states over time.

Table 2 provides the coefficient estimates that are produced by equation (2), along with the partial R^2 and the F-statistic for the hypothesis that

[13] The table includes the number of states that are included in our sample for each year. We are missing compulsory schooling data for Alaska and Hawaii. The table omits additional states in a few years because our sample of children does not always include a match with a particular parental state/year cell.

the compulsory schooling laws are jointly equal to zero. Looking first at the relationship between the laws and married parents' educational attainment, we see that the coefficient estimates on the compulsory schooling dummies increase monotonically (as expected) and are all statistically significant. Importantly, given our goal of using this relationship as the basis for our instrumental variables strategy, the F-statistic on the joint test of instrument significance is 84, which is well above the cutoff value suggested by Bound, Jaeger, and Baker (1995) and Staiger and Stock (1997). A value of less than five would raise concerns that our instruments were only weakly correlated with the endogenous regressor. The partial R^2 on our instruments is 0.003.

To verify that the compulsory schooling laws are affecting parental education in the expected way, we separate the sample on the basis of the parents' completed education. Since compulsory schooling laws never mandate high school completion, we do not expect the law changes to have an effect on educational attainment beyond high school. The relationship that we see for the full sample in table 2 should therefore break down when looking only at those parents who had more than a high school education. For the subsample of children whose parents both obtained fewer than 12 years of schooling (col. 2), all of the relevant statistics are similar to those for the full sample. Among children whose parents each received some postsecondary schooling (col. 3), however, the compulsory schooling dummies show no evidence of being correlated with higher levels of educational attainment. The partial R^2 on the instruments is smaller than for less educated parents, and the F-statistic falls to 2.6. This suggests that compulsory schooling laws are not picking up more general state-level changes in educational attainment.[14] The first-stage estimates for heads in single-parent families look very similar to those for two-parent families, including the pattern of results across more and less educated parents. These first-stage estimates suggest that compulsory schooling laws are strongly correlated with levels of educational attainment.

[14] We leave out those children whose parents have exactly 12 years of education because it is unclear how we would expect the education levels of these parents to have been affected by the change in the laws. On the one hand, since the laws do not ever mandate that an individual complete high school, we would expect the laws to have minimal explanatory power among parents with 12 or more years of schooling. On the other hand, individuals obliged to continue high school may perceive the costs of graduation to be smaller than if they could have left school earlier and may, therefore, decide to stay on until they obtain a degree. When we conduct our analyses by education category but include the parents with exactly 12 years of schooling, our results are similar to those we present here.

Table 3
Reduced Form Effects of Parents' Compulsory Schooling Laws on Children's Educational Attainment

	Dual-Parent Families <MEDIAN GRD	Single-Parent Families <MEDIAN GRD
Years of schooling required before work permit:		
Father:		
7	−.011	
	(.003)	
8	−.015	
	(.003)	
9	−.027	
	(.004)	
Mother:		
7	−.005	−.009
	(.003)	(.007)
8	−.008	−.015
	(.003)	(.006)
9	−.013	−.020
	(.004)	(.008)
No. of cells	182,072	5,249

Note.—All regressions include fixed effects for parent's birth year, state of birth, and census year. Additional controls include percentage of black, Asian, female, and children at each age in each cell. Data are grouped into means by both parents' birth years, states of birth, and census year, and weighted by cell population size. Huber-White standard errors are shown from clustering by birth cohort and state of birth. The omitted compulsory school law variable indicates whether the minimum years of schooling required before one is able to obtain a work permit is 6 years or less. Standard errors are in parentheses.

B. Reduced Form Results

Table 3 provides the reduced form estimates, obtained by regressing children's educational progress on the compulsory schooling dummies. The first-stage effects are strong enough to observe a substantive reduced form effect on children's grade repetition. Children whose fathers were required to stay in school for 7 years were 1.1 percentage points less likely to repeat a grade, on average, than those whose parents faced more lenient schooling requirements. The pattern of the estimates is also in keeping with what one would expect: children whose parents were required to stay in school more years generally experienced larger human capital gains. This pattern is similar when we use the laws faced by married mothers and single parents.

First-stage and reduced form results for parents with less than 12 years of schooling are shown graphically in figure 1. Since mothers and fathers may be exposed to different laws, we split the two-parent sample into two, (i) a matched child-father sample and (ii) a matched child-mother sample, and then stack these two data sets together with the data set containing children from single-parent families.[15] The combined sample

[15] If we were to use this sample as the basis for our regression analysis, then we would, of course, need to adjust the standard error estimates to account for the fact that we observe some of the children twice.

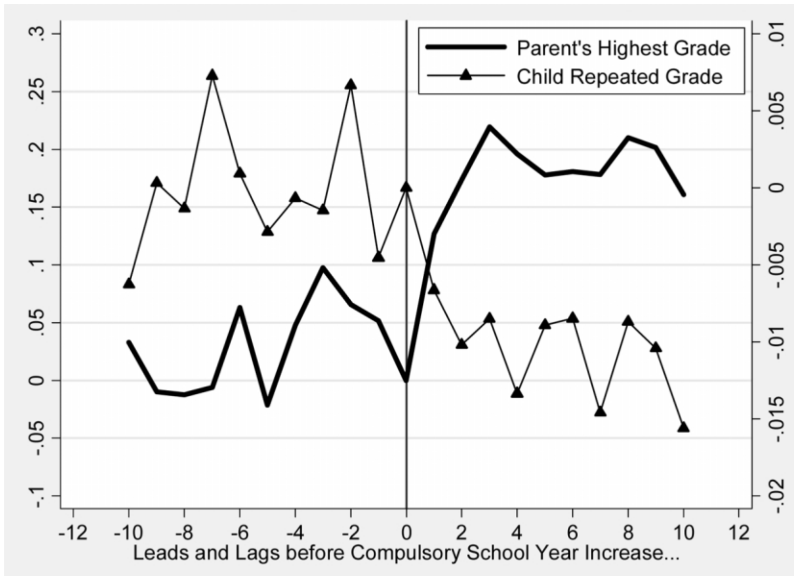

Fig. 1.—Parental education attainment and child grade repetition rate by years before and after increase in mandatory school years faced by parents (parents with less than high school education). The extracts discussed in the text are combined for mothers, fathers, and single parents whose highest grade attainment was less than grade 12. Data are aggregated by parent's state of birth, parent's year of birth, and census year. The sample includes only parent birth cohorts 10 years before or 10 years after an increase in the state's mandatory school years, determined when a parent was age 6 for entry laws and age 14 for leaving laws. Parent's average grade attainment and child's average grade repetition rate are regressed on 10 leads and 10 lags for the number of years between an increase in a state's mandatory school years when a parent was in school plus parental state and birth cohort fixed effects and census year indicators. The thick black line shows the lead and lag coefficients for parental grade attainment (left-hand axis). The thinner line shows the lead and lag coefficients for children's grade repeat status (right-hand axis).

is then aggregated by parent's state of birth, parent's year of birth, and census year, and includes only the cohorts of parents who turned age 14 during the 20-year period surrounding an increase in the state's schooling requirement. Parent's average level of education and children's average rate of grade repetition are regressed on dummy variables indicating the 10 years before and 10 years after the change.[16] The thick black line shows the estimated coefficients for the years around the law change among the sample of parents (scaled on the left-hand axis), and the thinner line shows comparable estimates for the children (scaled on the right-hand axis). The omitted category year is the year of the law change.

This figure shows a clear break in both average parental education and

[16] We also include fixed effects for parent's state of birth, birth cohort, and census year.

average grade retention following the introduction of a more restrictive compulsory schooling law. Average schooling among parents who were age 14 10 years after a law change is about .15 years higher than among those who were age 14 in the year of the law change.[17] The figure is important because it makes clear that increases in educational attainment follow changes in compulsory schooling rather than the other way around. Similarly, the timing of the fall in average grade retention rates occurs concurrently with the change in compulsory schooling (and average parental education). The grade repetition rate falls by about .01 (1 percentage point). Dividing this fall by the average increase in parental years of schooling gives a rough estimate of the effect of an additional year of compulsory schooling. The estimate (.01/.15 = .07) is roughly double the magnitude of our IV estimates, but our regression model uses the sum of parents' education as the regressor of interest. When we estimate the effects of mother's and father's education in separate regressions, we get results that are very similar to the estimates implied by figure 1.

Figure 2 shows the same coefficient estimates for the sample of parents with more than 12 years of schooling. Unlike the previous figure, there is no clear break in either series. This is what we should expect if compulsory schooling laws only affect parents who intend to leave high school without graduating (assuming that the laws do not induce many of these parents to obtain postsecondary education). It is not what we would expect if other factors are driving the discontinuity shown in figure 1.

C. OLS and IV Results

Table 4 displays our key results, beginning with the OLS estimates. For both family types, parental education is strongly significantly related to children's human capital accumulation. The point estimates suggest that an additional year of education will reduce the probability that a child repeats a grade by 1 to 3 percentage points. The marginal effect of education is smaller among more highly educated parents, suggesting that the intergenerational return to education falls as education increases, but the estimates are still substantive and statistically significant. These results are consistent with the existing literature, which has documented a positive intergenerational correlation between parents' education and that of their offspring. What is unclear is the extent to which these estimates reflect a causal relationship. To what degree can parents improve their children's progress through school by investing in their own human capital?

This question is addressed in the next column, where we use compulsory schooling laws as instruments for parental education. The IV esti-

[17] To keep the figure clean, the standard error bounds are not shown. For all lead and lag coefficients for the parental education regression, the standard errors are about .04. The standard errors are about .006 for the grade retention regression.

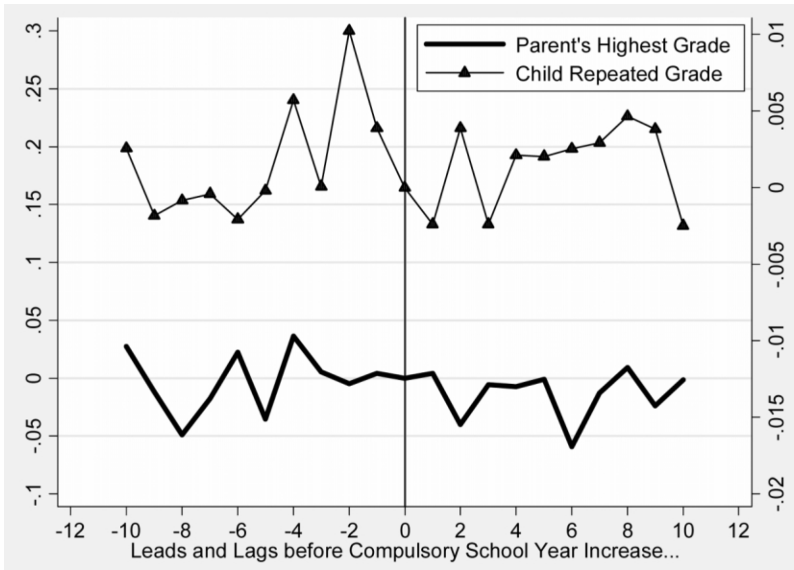

FIG. 2.—Parental education attainment and child grade repetition rate by years before and after increase in mandatory school years faced by parents (parents with more than high school education). The extracts discussed in the text are combined for mothers, fathers, and single parents whose highest grade attainment was more than grade 12. Data are aggregated by parent's state of birth, parent's year of birth, and census year. The sample includes only parent birth cohorts 10 years before or 10 years after an increase in the state's mandatory school years, determined when a parent was age 6 for entry laws and age 14 for leaving laws. Parent's average grade attainment and child's average grade repetition rate are regressed on 10 leads and 10 lags for the number of years between an increase in a state's mandatory school years when a parent was in school plus parental state and birth cohort fixed effects and census year indicators. The thick black line shows the lead and lag coefficients for parental grade attainment (left-hand axis). The thinner line shows the lead and lag coefficients for children's grade repeat status (right-hand axis).

mates indicate that an increase in parental education lowers the probability that a child is held back in school. The point estimates suggest that raising parental education by 1 year (about a third of a standard deviation) will reduce the probability of their children being retained by between 2 and 4 percentage points. The estimates are all statistically significant at the 1% level.[18]

[18] When the effects of father's and mother's education are estimated in separate regressions, the resulting coefficient and standard error estimates are −.059 (.007) (fathers) and −.049 (.006) (mothers). Since these estimates reflect the effect of the unobserved spouse's education as well, it is not surprising that they are approximately twice as big as the estimates in table 4. We have also conducted the analysis using parents' quarters of birth as instruments, similar to the approach adopted by Angrist and Krueger (1991) but using an indicator for whether a child repeated a grade as the main outcome (instead of earnings). The IV estimate for two-parent

Table 4
OLS and IV Estimates of Effect of Parents' Education on Children's Education (Dependent Variable: <MEDIAN GRD)

	OLS	IV	No. of Cells
Full sample:			
Dual parent: sum of parents' education	−.014	−.026	182,072
	(.0002)	(.002)	
Single parent: household head's education	−.026	−.027	5,249
	(.0020)	(.009)	
Parent's education < 12 years:			
Dual parent: sum of parents' education	−.022	−.037	58,692
	(.0005)	(.007)	
Single parent: household head's education	−.024	−.021	4,630
	(.0020)	(.016)	
Parent's education > 12 years:			
Dual parent: sum of parents' education	−.004	−.003	43,102
	(.0005)	(.022)	
Single parent: household head's education	−.008	.098	3,376
	(.0030)	(.104)	

NOTE.—All regressions include fixed effects for parent's birth year, state of birth, and census year. Additional controls include percentage of black, Asian, female, and average age of the children in each cell. Huber-White standard errors are shown from clustering by birth cohort and state of birth. Standard errors are in parentheses. See text for more data specifics.

Like previous work that uses compulsory schooling laws as instruments, the IV estimates are larger than the corresponding OLS estimates (at least among two-parent families).[19] The ratio of the IV estimate to the OLS estimate for our sample of children living in two-parent families is 1.87. Previous studies have produced ratios that range from 1.5 to 2.5 (Angrist and Krueger 1991; Harmon and Walker 1995; Staiger and Stock 1997; Oreopoulos 2003). Some researchers have argued that this phenomenon occurs because errors-in-variables biases outweigh omitted variables biases in OLS regressions. Another explanation is that compulsory school-

families is −0.020, with a standard error estimate of 0.004. This result is very much in line with the estimates based on variation in compulsory schooling laws. However, some concern has been expressed in the literature about a possible correlation between quarter of birth and unobserved individual characteristics that might affect children's outcomes (Bound and Jaeger 1996). Concerns about these types of correlations can be addressed by interacting quarter of birth with year of birth and state of birth, but then the identifying variation is very similar to the variation obtained from compulsory schooling laws. Previous studies have also raised concerns about the first-stage power of these interactions (Bound et al. 1995). For these reasons, we chose to focus on the compulsory schooling results.

[19] The confidence intervals around the OLS estimates do not include the IV estimates, and vice versa. Hausman tests for equality of the OLS and IV estimates also reject the null of equality, but this test may not be appropriate, since we are using clustered data.

ing laws affect schooling levels at the bottom of the education distribution but not at the top. The marginal effect of an additional year of schooling is likely to be larger among those with low skill levels, and Angrist, Imbens, and Rubin (1996) note that IV estimates will reflect the marginal return for the group that is affected by the instrument.

The pattern of OLS and IV estimates across family types lends some support to a Local Average Treatment Effect (LATE) interpretation of the results. Relative to children from two-parent homes, children from single-parent homes are more likely to have parents who were affected by compulsory schooling laws. Similarly, the fraction of children who repeat a grade is higher for this group. The IV/OLS ratio is correspondingly much smaller for this sample.

The next panel of table 4 shows the results from IV regressions applied to the subset of children whose parents both have less than 12 years of education. While splitting the sample by the endogenous regressor is not an entirely legitimate practice, the evidence presented above on the power of the first-stage regressions suggests that the IV strategy is most appropriate for this group. As expected, these estimates are similar to the estimates generated by the full sample, although they are no longer statistically significant for children growing up in single-parent families. Also as expected, estimates based on a sample with more than 12 years of education are close to zero.

While not directly comparable because of differences in the dependent variables, our point estimates appear to be larger and are more precisely estimated than those produced by Black et al. (2005), who find that an additional year of parental education will raise the child's education by .04 to .11 of a year but whose estimates are not generally statistically different from zero. As noted above, the fact that our estimated effects are statistically significant may reflect the greater amount of variation in compulsory schooling laws in our sample relative to the Black et al. sample. Another difference between our study and theirs is that Black et al. consistently report IV estimates that are below their OLS estimates. Two possible explanations are that (1) measurement error in the Norwegian education data is likely very small because the data come from an administrative source, and (2) returns to education do not differ greatly across segments of the Norwegian population.

Since grade repetition and educational attainment for age are not typical dependent variables, we have also used our instruments to estimate the effect of parental education on the probability that a child drops out of school. Using census data on 15–18-year-olds living at home, we construct a variable that is equal to one if the child is not currently enrolled in school and has not completed 12 or more years of schooling, and we create the same measure for a subsample of 15–16-year-olds. Because the census files only allow us to look at outcomes for children while they

Table 5
IV Effects of Parents' Education on the Probability of Dropping Out of High School

	Mean Dropouts	OLS	IV	No. of Cells
15–18-year-olds:				
Dual parent: sum of parents' education	.07	−.009	−.021	102,507
		(.000)	(.002)	
Single parent: household head	.134	−.020	−.029	4,065
		(.002)	(.008)	
15–16-year-olds:				
Dual parent: sum of parents'education	.046	−.006	−.010	74,703
		(.000)	(.013)	
Single parent: household head	.082	−.013	−.025	3,817
		(.002)	(.011)	

Note.—All regressions include fixed effects for parent's birth year, state of birth, and census year. Additional controls include percentage of black, Asian, female, and average age of the children in each cell. Data are grouped into means by parent's birth year, state of birth, and census year, and weighted by cell population size. Huber-White standard errors are shown from clustering by birth cohort and state of birth. Standard errors are in parentheses. See text for more data specifics.

are in the same household as a parent, the high school dropout variable is not ideal: dropping out of school is likely to be correlated with moving out of a parent's household. As a result, the analysis will suffer from selection bias. Despite this concern, we present some results using dropout status as our dependent variable, with the caveat that the effects shown here cannot be generalized to the entire population of 15–18-year-olds. Selection issues may be less severe for the sample of 15–16-year-olds.[20]

The results from this exercise are presented in table 5. Across all samples, and regardless of the measure of parental education used, there is a negative and (with one exception) statistically significant relationship between parents' education and the probability of dropping out of high school. As with our analysis of grade repetition, the IV estimates are larger in absolute value than the OLS estimates and suggest a significant causal relationship running from parental education to children's educational attainment. Among 15–18-year-olds, these estimates suggest that an additional year of parental education reduces the probability of dropping out by 2–3 percentage points. The mean dropout rate for this sample is .07 among two-parent households and .13 among single-parent households. There is also evidence that sample selection affects the estimates. The coefficient estimates fall (in absolute value) when we limit the sample to 15- and 16-year-olds, who are more likely to be observed living with a parent. This might be expected if children not living with their parents

[20] Roughly 95% of 15-year-olds in the census live with at least one parent, but only 77% of 15-year-old high school dropouts live with a parent. At age 17, these fractions fall to 88% of all 17-year-olds and 67% of 17-year-old dropouts.

are also less influenced by parental characteristics and resources. The fact that dropout rates increase as age increases from 15 to 18 could also account for such a pattern.

These estimated effects are large relative to the mean dropout rate in our sample. It seems likely, however, that typical dropout rates among children whose parents were affected by compulsory schooling laws are substantially higher than average. The average dropout rate among children of fathers with less than a high school education, for example, is .12, compared with .07 for the full sample. While we do not wish to draw any strong conclusions from the dropout analysis, given the sample selection issues involved, the results in table 5 are consistent with those produced by our main analysis. Higher parental education appears to result in improved educational outcomes for children.

D. Further Investigation of Instrument Exogeneity

An important set of concerns with our identification strategy is that the instruments may be correlated with other state-level changes that affect children's progress through school. It is important to remember that since the relevant compulsory schooling laws pertain to the year that the parent turned age 14, such state-level changes would have to have an effect on children's outcomes approximately 20 years later. We have also demonstrated that compulsory schooling laws only affect educational attainment among parents with low skills, which suggests that they are not picking up a more general state trend. Nevertheless, in this section we devote our attention to a number of additional robustness checks.

First, we add a variety of state-year controls to our regressions. The additional variables are motivated by Lleras-Muney (2005), who explores the possibility that compulsory schooling laws are endogenous with respect to educational attainment. Specifically, we include the state population, the number of doctors per capita, the value per acre of farm land, the percentage of the state population that is foreign born, the percentage of the state population that is black, the fraction of the state population living in urban areas, the fraction who are manufacturing workers, and average manufacturing wages per worker. These controls are aligned with the parent's state of birth and the year he or she turned age 14. The results from this exercise are displayed in table 6. Inclusion of the state/year controls has virtually no effect on the estimated effect of parental education, and the first-stage *F*-statistics remain strong.[21] The estimates are also robust to the inclusion of regional trends, although the standard error estimates also increase.

[21] The estimated coefficients in the single-parent sample are hardly affected by the inclusion of state controls, but the estimated standard error increases so that the statistical significance of the estimates is reduced.

Table 6
Sensitivity of IV Estimates to Additional Controls (Outcome variable: <MEDIAN GRD)

	IV Coefficients							
	Sum of Parents' Education				Single-Parent's Education			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parents' education	−.026	−.024	−.022	−.044	−.027	−.023	−.020	−.035
	(.002)	(.005)	(.010)	(.024)	(.009)	(.017)	(.032)	(.064)
Census parent's state controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Parent's regional trends	No	No	Yes	No	No	No	Yes	No
Parent's state trends	No	No	No	Yes	No	No	No	Yes
	First Stage							
Years of schooling required before work permit:								
Father:								
7	.327	.159	.15	.052				
	(.057)	(.059)	(.059)	(.066)				
8	.456	.149	.138	.066				
	(.051)	(.052)	(.053)	(.062)				
9	.728	.358	.217	.03				
	(.067)	(.067)	(.069)	(.080)				
Mother or head:								
7	.306	.130	.077	.112	.287	.087	.088	.101
	(.061)	(.062)	(.063)	(.072)	(.080)	(.069)	(.070)	(.082)
8	.470	.167	.095	.087	.472	.133	.113	.064
	(.054)	(.054)	(.056)	(.067)	(.072)	(.061)	(.062)	(.077)
9	.753	.398	.211	.074	.734	.368	.232	.139
	(.069)	(.069)	(.071)	(.082)	(.085)	(.073)	(.075)	(.090)
F-statistic (instruments)	85.1	21.3	5.4	.9	32.1	12.5	3.8	.9
No. of cells	178,988	178,988	178,988	178,988	5,077	5,077	5,077	5,077

NOTE.—All regressions include fixed effects for parent's birth year, state of birth, and census year. Additional controls include percentage of black, Asian, female, and average age of the children in each cell. Census parent's state controls include state population; doctors per capita; value per acre of farm land; percentage of foreign born, urban, manufacturing, and black; and average wages. Data are grouped into means by parent's birth year, state of birth, and census year, and weighted by cell population size. Huber-White standard errors are shown from clustering by birth cohort and state of birth. Standard errors are in parentheses.

When we add state-specific trends to the model (col. 4), we find that the estimated impact of parental education becomes even larger, although the estimated effects among single-parent families are no longer statistically significant. Inclusion of state-specific trends greatly reduces the first-stage power of the instruments, however, because most states experienced both upward trends in educational attainment and increases in compulsory schooling requirements.[22]

Given this lack of robustness to inclusion of the state trends, we conduct two additional exercises to explore alternative identification strategies. First, we replicate our analysis using the subset of states and years for which there was either a decrease or no change in the minimum required years of schooling. To do this, we create a subsample of state/year cells from the 5 years prior to a decline in the minimum schooling requirement through the 5 years following the change.[23] Like the rest of the country, these states experienced a positive trend in average educational attainment, but because their compulsory schooling laws were moving in the opposite direction, IV estimates based on this sample are less likely to be driven by an underlying trend. Table 7 shows that restricting the sample in this way actually increases the magnitude of the estimates.

Second, we examine the sensitivity of our estimates to an alternative identification strategy that is based entirely on cross-sectional variation. Note that there is a difference between the environment in which parents' and children's educational outcomes are determined when children and their parents grow up in different states. If we compare outcomes among children living in the same state and whose parents belong to the same birth cohort but were educated in different states from their children, then the identifying variation cannot be contaminated by parental state-of-birth trends. Specifically, we reestimate the first stage using individual-level data for children who do not reside in the state of either parent's birth. We drop from the specification the dummies for parents' states of birth and add dummies for the child's state of residence interacted with parents' birth cohort. This effectively compares children growing up in the same state with parents of the same cohort but whose parents them-

[22] Following a referee's suggestion, we have also tried a specification that controls for state trends by taking some of the predetermined state characteristics included in col. 1 of table 6 and interacting them with year dummies. The variables we chose were percentage of black, urban, and the manufacturing wage, because these variables are strongly correlated with the parent's schooling. This specification produces a first-stage F-statistic on the excluded instrument of 8.3, and a second-stage coefficient estimate of $-.018$ (0.007) for dual-parent families and -0.017 (0.024) for single-parent families.

[23] If a state experienced an increase in required years of schooling during the 5-year period after the initial law change, then it is dropped from the sample. If a second decline in required years occurred during the 5-year period, then the state is included in the sample.

Table 7
OLS and IV Estimates for States with No Upward Change in Mandatory School Years

	Dependent Variable:: <MEDIAN GRD	No. of Cells
OLS:		
Dual parent: sum of parents' education	−.015 (.001)	13,939
Single parent: household head	−.024 (.003)	1,014
IV:		
Dual parent: sum of parents' education	−.060 (.035)	13,939
Single parent: household head	−.038 (.022)	1,014

NOTE.—All regressions include fixed effects for parent's birth year, state of birth, and census year. Additional controls include percentage of black, Asian, female, and average age of the children in each cell. Data are grouped into means by parent's birth year, state of birth, and census year, and weighted by cell population size. Huber-White standard errors are shown from clustering by birth cohort and state of birth. Standard errors are in parentheses.

selves lived in different states and so faced different compulsory schooling laws. Because this specification focuses on cross-sectional variation in schooling laws, we also include dummies for parents' region of birth and control for the set of observable characteristics of the parent's state of birth included in table 6. Note that in this specification we use 3-year birth cohort dummies (rather than single-year cohorts) to preserve reasonable cell sizes and reduce the required number of interactions. The sample is now restricted to "movers." The second-stage regression contains the same sets of controls, except for the compulsory schooling laws. Since the children are not living in the same state in which their parents were educated, estimates produced by this exercise are based only on cross-sectional variation in the laws.

The first column of table 8 shows what happens to our estimates when we apply our original identification strategy to this new sample of movers. Among dual parent families, the estimates are very similar to those produced by the full sample. The estimates are much noisier for single-parent families because restricting the sample to movers reduces the sample size dramatically. In addition, the first-stage F-statistic falls below two for the single-parent sample, which suggests that this exercise will not be informative for that group.

Column 2 shows the results produced by the alternative identification strategy. Among two-parent families, the estimates are very similar to those in column 1. They remain statistically different from zero and suggest that a 1-year increase in parental education will reduce the probability of repeating a grade by approximately 2 percentage points. It is striking that we find similar, positive, and statistically significant effects

Table 8
IV Estimates for Sample of Children in States Different from Parents' States of Birth (Dependent Variable: <MEDIAN GRD)

	(1)	(2)	Sample
OLS:			
Dual parent: sum of parents' education	−.012	−.012	166,768
	(.0002)	(.0002)	
Single parent: household head	−.024	−.024	46,245
	(.004)	(.001)	
IV:			
Dual parent: sum of parents' education	−.029	−.022	166,768
	(.009)	(.011)	
First-stage *F* on instrument	5.9	4.1	
Single parent: household head	−.045	−.022	46,245
	(.049)	(.024)	
First-stage *F* on instrument	1.8	5.8	
Parent's state of birth dummies	Yes	No	
State of residence × parent's birth cohort (3 year)	No	Yes	
Parent's region of birth dummies	No	Yes	

Note.—All regressions include fixed effects for parent's birth year, characteristics of parents' state of birth (population; doctors per capita; value per acre of farm land; foreign born; percentage of urban, manufacturing, black; and average wages), and census year. Additional controls include dummies for black, Asian, female, and age of the children. Huber-White standard errors are shown from clustering by birth cohort and state of birth. Standard errors are in parentheses.

(among two-parent families) whether our identification is confined to cross-sectional or time-series variation.

VI. Conclusion

This article provides new evidence on the causal effect of parental education across generations. The limited literature on the intergenerational effects of parental education has struggled to find appropriate identification strategies; we draw on variation in compulsory schooling laws, which are arguably exogenous with respect to children's outcomes 15 or more years later. Using data from the 1960–80 census files, we estimate that an increase in parental education of 1 year will reduce a child's probability of being at grade-for-age by 2–4 percentage points. Since grade retention is negatively correlated with other academic outcomes, the positive effect of parental education on children's grade progression is likely to have long-term socioeconomic benefits as well.

Identification of a causal effect of parental education implies that at least some of the intergenerational transmission of inequality can be attributed to environmental influences. We have identified a significantly large environmental effect. There are several possible mechanisms that could explain this relationship, although disentangling which environmental factors drive these results cannot be determined using this article's research design. Parental income is an obvious possibility. Our results and others (e.g., Acemoglu and Angrist 2000; and Oreopoulos 2003) show large gains in earnings from additional compulsory schooling. With additional earnings, wealthier parents can afford better schools, better neighborhoods, and better nutrition. Currie and Stabile (2004), for example, find that children from low-income families are more likely to experience frequent cases of poor health and low test scores. Mulligan (1997) suggests wealthier parents are more likely to care about their children's development.

Parental education may also affect children in ways other than through income. More educated parents may have more favorable attitudes toward school. The additional skills acquired may also improve child development. Magnuson (2003) discusses several additional avenues by which parental education might improve children's education. Specifically, she notes that parents with more education may have "better" parenting and teaching styles, may engage their children in higher-quality verbal interactions and literacy activities, may provide more stimulating learning opportunities in the home, and may simply be more comfortable interacting with teachers and educational institutions.

Our results draw attention to the social externalities that are associated with education. While much research has been devoted to understanding the private returns to education via individual wage effects, designing effective education policy hinges crucially on taking the full social costs and benefits into account. There is increasing evidence that the social returns to education are substantial, and our estimates suggest yet another dimension along which positive externalities occur.[24] Taken together, these findings indicate that the total returns to education may be seriously underrepresented by estimates that focus only on individual wage effects.

Appendix

Determining whether or not a child has repeated a grade is complicated by the fact that there is variation across states and over time in the minimum age at school entry, incomplete information on school entry cutoff dates across states and over time, and questions about the degree to which school districts comply with those dates. We have collected state-level information on the minimum school entry age and cutoff dates for 1955–65: 43 states in 1955 and 32 states in 1965. Twenty-two states maintained the same cutoff dates over the 10-year period, and seven states changed their cutoff date. Missing information prevents us from determining what happened in the remaining states. Since we do not have school entry cutoff dates for each state and year, we have created several different measures for assessing "normal" educational progress.

One possible measure of grade retention is based on the assumption that all states use October 1 as their cutoff. We treat all children who

[24] For evidence that the social returns to education are substantial, see Currie and Moretti (2003), Lochner and Moretti (2004), and Moretti (2004).

turned age 6 in the first three quarters of the calendar year as though they entered first grade in the fall of that year, and all children who turned age 6 in the fourth quarter as though they entered first grade in the fall of the following year. The estimated effect of parental education using this measure is very similar to the one we highlight in the article, which is based on an alternative measure that classifies children whose educational attainment is below the median for their state, age, quarter of birth, and census year cell as below grade-for-age (<MEDIAN GRD). This second measure takes into account year and state-specific characteristics (such as the current cohort's likely enrollment cutoff date) and individual level characteristics (quarter of birth) that affect the age at which the child was likely to have enrolled in school. Because individuals' completed education levels are heavily clustered at the median, about 10%–15% of children in each cell are classified as repeaters. These percentages are similar to those reported in the literature.

Our measure of grade retention is not perfect. Students who entered school late, for example, will be classified as having been held back, and delayed entry into kindergarten is a fairly common practice. Nine percent of first and second graders in the mid-1990s entered kindergarten late, whereas only 5%–6% had repeated kindergarten (National Center for Education Statistics 1997). In a recent paper, Cascio (2003) compares directly reported measures of grade repetition in the 1992, 1995, and 1999 Current Population Survey School Enrollment Supplement to a below-grade proxy created using educational attainment data available in the census. She finds that about 20% of all children are incorrectly classified by the below-grade proxy and that about 94% of such errors consist of children who have not repeated a grade but who are classified as such by the proxy. For this reason, it is more appropriate to think of our dependent variable as a measure of grade-for-age.

Normally, researchers are not concerned that noisy dependent variables will generate biased estimates because measurement error in a normally distributed dependent variable merely generates inefficient standard error estimates. Cascio points out, however, that when the dependent variable is an indicator for whether or not the individual has repeated a grade, then consistency may be a problem. Measurement error in a binary dependent variable will produce attenuated parameter estimates (Aigner 1973; Hausman 2001). Cascio estimates that the attenuation factor that results from using dependent variables like <MEDIAN GRD may be as high as 0.35. While we would prefer to generate unbiased estimates, downward biased estimates will still be informative because our prior is that the intergenerational correlation captures effects that are not wholly causal. Therefore, estimates that are statistically different from zero will still allow us to reject the hypothesis that there is not an exogenous effect of parental education on children's human capital.

Also, although <MEDIAN GRD is binary, it is a less noisy variable than more commonly used measures of grade repetition (such as those that assume the same school entry cutoff dates across states) because it takes into account variation in state, age, and quarter of birth by normalizing attainment within each cell. Specifications based on this measure, therefore, should suffer less from attenuation bias than those that are commonly used in the literature.

Table A1
Descriptive Statistics

	Dual-Parent Families				Single-Parent Families			
	1960	1970	1980	Combined	1960	1970	1980	Combined
Father or household head among single-parent families:								
Age	40.85	40.43	39.49	40.29	38.84	38.22	36.26	37.49
	(7.26)	(7.13)	(7.09)	(7.18)	(7.93)	(7.68)	(7.20)	(7.61)
Highest grade	10.29	11.47	12.60	11.41	9.36	10.43	11.56	10.70
	(3.64)	(3.39)	(3.27)	(3.56)	(3.35)	(3.06)	(2.66)	(3.08)
Mother:								
Age	37.51	37.44	36.78	37.27				
	(6.53)	(6.57)	(6.32)	(6.49)				
Highest grade	10.56	11.38	12.17	11.35				
	(2.88)	(2.50)	(2.41)	(2.68)				
Children:								
Age	10.70	10.94	11.08	10.90	11.05	11.08	11.14	11.10
	(2.52)	(2.55)	(2.57)	(2.55)	(2.56)	(2.57)	(2.58)	(2.57)
Highest grade	4.22	4.43	4.55	4.40	4.43	4.52	4.55	4.51
	(2.57)	(2.62)	(2.64)	(2.61)	(2.63)	(2.65)	(2.62)	(2.63)
Black	.09	.10	.10	.10	.32	.34	.37	.35
Female	.49	.49	.49	.49	.50	.50	.49	.50
REPEAT	.15	.15	.15	.15	.25	.21	.21	.22
<MEDIAN GRD	.20	.19	.18	.19	.28	.24	.24	.25
Observations	233,080	266,265	211,727	711,072	26,939	45,462	57,231	129,632

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