Parental Education and Offspring Outcomes:
Evidence from the Swedish Compulsory School Reform[†]

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We use the Swedish compulsory school reform to estimate the causal effect of parental education on sons' outcomes. To this end, we use data from the Swedish military enlistment register on the entire population of males and consider outcomes, such as cognitive skills, noncognitive skills, and various dimensions of health at the age of 18. We find positive effects of maternal education on sons' skills and health status but no effects of paternal education. One reason behind this result may be that the fathers affected by the reform did not face any labor market returns to their increased schooling. (*JEL* I21, J12, J24)

An individual's success in life is determined to an important extent by his or her abilities, human capital, and health capital. In the literature on skill formation, cognitive and noncognitive skills during childhood, adolescence, and early adulthood have been found to predict adult outcomes, such as education, income, and engagement in criminal activities and risky behaviors (Currie and Thomas 1999; Duckworth and Seligman 2005; Heckman, Stixrud, and Urzua 2006; Cunha and Heckman 2009; Lindqvist and Vestman 2011). Similarly, a recent literature, spanning over medicine as well as economics, shows the importance of an individual's health capital for a number of adult outcomes (see, for example, the recent surveys by Currie 2009 and Currie and Almond 2011).

But what determines a person's abilities, health, and human capital? A key factor is believed to be the human capital of one's parents. In particular, children of more highly educated mothers tend to have better outcomes along a number of dimensions, such as health, cognition, education, and labor market outcomes (see, e.g., Haveman and Wolfe 1995 or Strauss and Thomas 1995). It is not clear, however, how one should interpret the correlations between parental education and children's

outcomes. Does parental education actually improve the outcomes of the next generation in a causal sense? In such a case, the returns to schooling would extend beyond the individual to also include his or her offspring's returns. Parental education would then also be an important mechanism through which inequality is transmitted across generations.

A small and recent literature provides estimates of the causal effect of parental schooling on very early life outcomes of children, such as birth weight, but there is limited evidence that parental schooling matters for these outcomes.[1] For young adults, however, there is recent and compelling evidence that parents' schooling matters for their offspring's schooling attainment (see the recent literature reviews by, e.g., Björklund and Salvanes 2011; Black and Devereux 2011; and Holmlund, Lindahl, and Plug 2011). In many cases, this literature finds that maternal schooling matters the most for children's schooling.[2]

There is almost no evidence on how parents' schooling affects other important outcomes of their young adult children, however. In this paper, we contribute to the literature by providing new evidence on the causal effect of mothers' and fathers' schooling on a wide range of outcomes of the next generation. We ask questions such as: does parents' schooling affect their young adult children's cognitive and noncognitive skills? Does it affect their health? What is the relative role of mothers' and fathers' education? What are the mechanisms behind the effect of parental schooling? We are not aware of any previous study that considered the effect of parents' schooling on such a wide range of outcomes of their young adult children. While we believe the answers are of interest in their own right, they can also shed light on the mechanisms that lie behind the causal effect of parental schooling on children's schooling. Moreover, if parents' schooling affects the skills and health of their children, the returns to education are larger than suggested by studies not taking these outcomes into account.

For the purpose of our analysis, we exploit the Swedish compulsory school reform, which was rolled out over the country during the 1950s and 1960s. An important feature of our identification strategy is that the timing of the reform varied across municipalities, which gives variation in reform exposure both within and between cohorts. This provides us with plausibly exogenous variation in schooling, which we exploit to estimate the causal effect of schooling on offspring outcomes. A crucial assumption of our identification strategy is that conditional on birth cohort fixed effects, municipality fixed effects and municipality-specific linear trends, exposure to the reform is in effect random. We provide a set of robustness checks, which, we argue, show that this assumption is valid.

Our empirical strategy requires data on parental education and children's outcomes. For this purpose, we use register-based data on the universe of individuals that were exposed to the school reform, which includes information on education, place of residence, and date of birth. Through the use of personal identifiers, we

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[1]McCrary and Royer (2011) find no effect of maternal schooling on birth weight, using US birth records. Using college openings as an instrument for maternal schooling, Currie and Moretti (2003) find a significant effect. There is also somewhat mixed evidence regarding the importance of birth weight for adult outcomes (see Currie and Almond 2011).

[2]We review this literature in more detail in Section I.

have linked this data on the parental generation to register-based data on their children, taken from the Swedish military enlistment register. Since women were not obliged to enlist for the military, we are only able to study outcomes among men. The benefit of the enlistment register, however, is that it includes information on more or less the entire population of men, since enlisting for the military was mandatory in Sweden during the time period considered. This gives us considerable statistical power in our empirical analyses, as well as an unusually high degree of representativeness of our results.

Our results show that higher parental education improves outcomes in the next generation along a number of dimensions, but that the effects are almost exclusively found for mother's education. In particular, we find that maternal education improves cognitive and noncognitive skills, as well as overall health, and leads to an increased stature. We also provide some clues to the possible mechanisms behind these results; whereas more education leads to substantially greater income among mothers, no such return is obtained for the fathers affected by the reform. We argue that the lack of return to education for fathers affected by the reform is a natural candidate for explaining the gender differences in results. Using previous causal estimates of the effect of parental income on children's health and skills, we show that the income gain that followed from mothers' exposure to the schooling reform can explain most or all of the effect of maternal education on their children's outcomes. This result is consistent with income being an important driver of improved health and skills of the second generation. In addition, we show that increased schooling of mothers led to reduced fertility and led them to marry with higher quality spouses, whereas none of these effects are present among the fathers who increased their education in response to the reform. Overall, our results provide new evidence on the beneficial impact of maternal education across generations.

The paper is organized as follows. In Section I, we discuss the previous literature and the potential mechanisms through which education could affect child health. In Section II, we outline of empirical strategy and provide details on the Swedish compulsory school reform. Section III discusses the data we use, and, in Section IV, we present the results. Section V concludes.

I. Previous Work

Why may parental education matter for child outcomes? Commonly proposed causal pathways include the improved knowledge and the greater economic resources that accompany education. Causal evidence for income effects were for instance obtained by Dahl and Lochner (2012), who found that increases in family income resulting from changes in the Earned Income Tax Credit had positive effects on children's test scores. Similar findings were reported in Duncan, Morris, and Rodrigues (2011); Milligan and Stabile (2011); and in Løken, Mogstad, and Wiswall (2012), where the latter exploited regional variation in the income boom that followed the discovery of oil in Norway. A possible explanation for the positive income effects is greater affordability of health care inputs (Currie 2009), although this particular mechanism should not be overemphasized in a country like Sweden, where health care coverage is universal and of low cost.

An alternative mechanism is that higher income leads to lower fertility, as the alternative cost of child-rearing rises. To the extent that there is a trade-off between child quality and child quantity, this is an alternative explanation for the effect of parental education (Becker and Tomes 1976). In our analysis, we will therefore consider to what extent income and fertility effects could explain the link between parental education and child outcomes.[3]

Besides increasing economic resources, it has been argued that education improves productive efficiency in health production (Grossman 1972). This line of reasoning can be generalized to investments in cognitive and noncognitive skills and to investments over generations, where, for instance, more well-educated parents would be more knowledgeable about how to use various health inputs and time inputs in the production of child quality.

Any empirical relationship between parental education and child outcomes could also be generated through noncausal mechanisms. Since parents and children share genes, this could partly explain why some parents and children end up being rather similar in terms of schooling, skills, and health. More generally, there are a myriad of factors, including preferences, personality, and environments that might be shared by parents and children and that could generate a noncausal relationship between parents and children.

What is then the evidence of a causal effect of parental education on the outcomes of young adults? The literature has almost exclusively focused on various measures of education. Black, Devereux, and Salvanes (2005), for instance, exploited a reform of the Norwegian school system and found that only mothers' education mattered for their children's education. Holmlund, Lindahl, and Plug (2011), using the Swedish school reform, found significant effects of both parents' schooling. Other IV-based studies have focused on outcomes such as post compulsory school attendance, grades, and grade repetition, and found evidence of both the importance of mothers' and fathers' schooling (Chevalier 2004; Oreopoulos, Page, and Stevens 2006; Maurin and McNally 2008; Carneiro, Meghir, and Parey 2013).

Additional evidence on the importance of parental schooling for children's schooling comes from twin and adoption studies. In the latter category, Dearden, Machin, and Reed (1997); Sacerdote (2002, 2007); and Plug (2004) found significant effects of both maternal and paternal schooling in the United States, whereas adoption studies based on Scandinavian data tend to find smaller effects (Björklund, Lindahl, and Plug 2006; Hægeland et al. 2010; Holmlund, Lindahl, and Plug 2011). The twin literature often finds that whereas fathers' schooling influences children's schooling, the effect of mothers' schooling is smaller or insignificant (Behrman and Rosenzweig 2002; Bingley, Christensen, and Jensen 2009; Holmlund, Lindahl, and Plug 2011; Pronzato 2012).[4] Holmlund, Lindahl, and Plug (2011) applied the twin design, adoption design, and IV strategy to Swedish register-based data, and

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[3] Increased parental income may also increase the bargaining power of the parent whose income was subject to the increase (see Behrman 1997 for a survey).
[4] Hægeland et al. (2010) used a twin design to study the effect of parents' schooling and children's exam test scores but found no effect of neither parent's education.

concluded that the differences in results across designs are partly explained by differences in the identifying assumptions used across methods.

As this brief literature review shows, we know very little to date about the effect of parents' schooling on other outcomes of their young adult children, such as their cognitive and noncognitive skills and their health. With our paper, we therefore hope to fill an important gap in the literature.

II. Method

A. *The School Reform*

In this section, we provide a brief overview of the Swedish compulsory school reform.[5] In the 1940s, prior to the implementation of the reform, all children in Sweden went to a common school (*folkskolan*) where they normally stayed until at least sixth grade. Students with sufficient grades were then selected for the three- to five-year long junior secondary school (*realskolan*), whereas the rest remained in the common school until compulsory schooling was completed. At this time, compulsory schooling spanned seven years or, in some large municipalities, eight years.

In 1948, a parliamentary committee delivered a proposal to introduce a new compulsory school, consisting of nine compulsory years. In order to facilitate an evaluation of the reform before implementing it nationwide, it was decided that the reform should be gradually implemented across municipalities.[6] Beginning in 1949, 14 municipalities introduced the school reform and more municipalities were then added year by year. In most cases, the reform was implemented by all school districts within a municipality at the same point in time. In 1962, the Swedish parliament decided that the reform should be implemented throughout the country and that municipalities had to have the new system in place no later than in 1969.

Besides increasing the number of years of mandatory schooling, the reform also meant a few other changes to the school system. First, whereas the old system separated students into different schools after grade six, based on their performance, the new system kept students together in the same school until the ninth grade. In the new school system, students were allowed to choose between different courses, such as languages or vocational training, and, from 1955 onward, between harder and easier courses in some subjects. Most schools streamed students into different classes according to these choices. Marklund (1987, 180) notes that "the reform school between 1955 and 1960 conformed to a streaming system that in terms of routes was not too much different from the old parallel school with one common school route and one junior secondary school route." In 1962, the streaming before grade nine was abolished (Marklund 1985). In Math and foreign languages, students were still able to choose between harder or easier classes, but up until eighth grade they were kept together with their regular classmates in all other subjects.

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[5] The text is a summary of online Appendix A, which gives a more detailed description of the reform.

[6] During the assessment period, only municipalities that had shown interest in the reform were selected to implement it, meaning that reform implementation was not random. Previous studies using the reform have shown that it was implemented earlier in municipalities with higher average income or education levels, a result that we also find. These results point to the importance of correctly controlling for unobserved differences across municipalities.

Second, the reform meant a change to a national curriculum. In practice, however, the most important change to the curriculum appears to be that English became a compulsory subject in reform schools and was now taught from fifth grade. English was not a compulsory subject in the old school system. Beginning in 1955, however, the same requirement was also introduced in nonreform schools.[7]

As with any reform that increases the number of mandatory years of schooling, the Swedish reform imposed an additional resource burden on reform municipalities. In particular, the reform increased the demand for teachers, which led to a teacher shortage during the reform period. As a consequence, some schools had to hire teachers that were not formally qualified. Several teachers colleges were however opened during the time period and the shortage began to ease in the mid-1960s (Marklund 1981).

Next, we explain how the features of the Swedish reform guide our empirical specification and the interpretation of our results.

B. *Econometric Method*

Our main empirical model is based on the following two equations:

$$(1) \qquad H^c = \alpha_0 + \alpha_1 S^p + \boldsymbol{\alpha_2} \mathbf{Y^p} + \boldsymbol{\alpha_3} \mathbf{M^p} + \boldsymbol{\alpha_4} \mathbf{Trends^p} + \varepsilon,$$

$$(2) \qquad S^p = \beta_0 + \beta_1 R^p + \boldsymbol{\beta_2} \mathbf{Y^p} + \boldsymbol{\beta_3} \mathbf{Mp} + \boldsymbol{\beta_4} \mathbf{Trends^p} + \upsilon.$$

In these equations, c denotes the child and p a parent. S refers to parental years of schooling, $\mathbf{M}$ is a vector of municipality indicators, $\mathbf{Y}$ is a vector of birth year indicators, H is the outcome of interest, and $\mathbf{Trends}$ is a vector of municipality-specific trends.[8] R is a dummy variable indicating whether the individual was exposed to the reform or not.

In order to obtain some "baseline results" regarding the relationships between parental education and the various child outcomes, we first estimate OLS regressions based on equation (1). We then apply Two Stage Least Squares (2SLS), using equation (2) as the first stage, where schooling is instrumented with reform status. The identification of α_1, the coefficient of interest, relies on the part of the variation in parental schooling that is generated by variation in reform exposure. Our empirical strategy is similar to the one used in previous reform-based papers, such as Black, Devereux, and Salvanes (2005) and Holmlund, Lindahl, and Plug (2011).

Two key assumptions behind our identification strategy are that the reform only affected child outcomes through its effect on years of parental schooling, and that

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[7]Other than English, the change to a national curriculum did not imply any large changes to the total number of hours taught or to the number of hours allocated to different subjects (Marklund 1982). The introduction of English as a compulsory subject came at the expense of somewhat reduced teaching in Swedish as a first language (Marklund 1981).

[8]We focus on specifications using municipality-specific linear trends, but we also show results without trends and results where we use interactions between year of birth and county of residence instead of trends. As a sensitivity check, we also add quadratic trends. There is a risk, however, that adding such quadratic trends will pick up accelerations or decelerations in outcomes that result from the reform itself (Wolfers 2006; Holmlund, Lindahl, and Plug 2008; Hjalmarsson, Holmlund, and Lindquist 2011). As a result, they may partly control for the effect that one tries to estimate.

exposure to the reform was as good as random, conditional on the controls included. While our extensive set of controls for area and time effects are intended to take care of the latter, exposure would still be nonrandom if individuals decided to move to or from reform municipalities in a systematic way. We are not able to investigate whether this may have been the case, but rely on Meghir and Palme (2005) who had access to data on municipality of birth as well as municipality at school age, and obtained no evidence that selective mobility was an important issue.[9]

The assumption that the reform only affected child outcomes through years of parental education would be violated if the reform also affected the quality of the education provided. We noted previously that nonqualified teachers were sometimes hired when the demand for teachers increased. While we do not have any statistical information about the extent to which nonqualified teachers were hired in reform schools, we expect nonqualified teachers to have, if anything, a negative impact on the quality of teaching. Therefore, any bias would be negative, leading us to potentially underestimate the effects of schooling.

The quality of teaching may have been especially affected by the reform in the short run. For instance, it is likely that some schools hired a number of teachers in advance and started reorganizing already some time before the introduction of the reform, whereas there was a shortage of teachers and a lower organizational quality right after the reform was implemented. In order to avoid such short-run adjustment effects, we exclude individuals born in the first reform cohort, and those born one year before and after it, in our main analysis.

Moreover, there are two other reasons why the cohorts right around implementation are problematic. First, individuals were not always in the right grades according to their ages, so they would be misclassified with respect to reform status. Svensson (2008) showed that only 88 percent of all children born in 1949 were in the right grade in 1961, reflecting both that some students repeated a class and that some students started school earlier. Second, there is some measurement error in the reform indicator since it has not always been possible to define a clear-cut starting date of the reform. This is mainly a problem for the cohorts one year ahead or behind the assumed starting date of the reform, however (Holmlund, Lindahl, and Plug 2008). In the sensitivity analysis, we will test to what extent the exclusion of these cohorts affects our results.

Since the reform meant that students were kept together in the same school until higher ages, peer group composition as well as patterns of assortative mating may have been affected. We believe that such potential effects should not be overemphasized, however, since students were able to choose between different streams also in the new school system, and therefore sorted themselves into different classes. Moreover, the literature on tracking and streaming often finds effects that are insignificant or close to zero; see the overview by Betts (2011).[10] If separating high- and low-ability students, on average, has beneficial effects on various outcomes, and

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[9]They used two different approaches to investigate this. First, they reestimated their regressions, only including individuals who did not change reform status as a result of moving to another municipality between birth and school age. Second, they instrumented reform status using as instruments the reform status in the municipality of birth (when this was available) and an indicator for whether the reform status in the municipality of birth was known.

[10]See also the recent paper by Kerr, Pekkarinen, and Uusitalo (2013), which finds no effect on cognitive test scores of a reform in the Finnish school system that delayed tracking by as much as 5 years, from age 11 to 16.

if such sorting became less pronounced in the new school system, one might expect our estimates to be somewhat downward biased.[11]

III. Data

Our dataset was constructed by integrating registers from Statistics Sweden (SCB) and the Swedish National Service Administration (NSA).[12] The former include virtually the entire Swedish population alive in 1960 and was used to extract information on parents, including their year of birth and their municipality of residence in 1960.[13] These data also include information on years of schooling as of 1999, being calculated from information on highest educational degree completed. In our main analysis, we use parents born between 1940 and 1957. These cohorts are essentially the ones for which there is variation in terms of whether the reform had been implemented or not.

From the military records, we extract information on child outcomes for males born between 1955 and 1979. It is important to note that at the time under study, military enlistment was mandatory for men in Sweden, with exemptions only granted for institutionalized individuals, prisoners, individuals convicted for serious crimes (mostly violence-related and abuse-related crimes), and individuals living abroad. Individuals usually underwent the military enlistment procedure at the age of 18 or 19.[14] Refusal to enlist was punishable with a fine, and eventually imprisonment, implying that the attrition in our data is very low; only about 3 percent of each cohort of males did not enlist.

In order to classify parents as exposed or unexposed to the reform, we use an algorithm provided by Helena Holmlund, which is described in Holmlund (2008).[15] The algorithm uses historical evidence on reform implementation and assigns reform exposure based on parents' year of birth and municipality of residence in 1960.

In our analysis, we use two different measures of overall health. First, depending on the conscript's health conditions and their severity, NSA created a 14-step scale indicating the conscript's overall health status, which was used to determine the individual's suitability with respect to different types of military service. We will refer to this variable as "global health," where we assign a variable equaling zero to individuals belonging to the most healthy category, minus one to those belonging to the second most healthy category, etc., and we then standardize this variable to have standard deviation one.

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[11] Regarding the change to a national curriculum, with the most important change being the introduction of English as a compulsory subject, we find it unlikely that this would be a major threat to the exclusion restriction. Note that this particular change also applied to schools not yet affected by the reform from 1955 onward.

[12] More detailed information on the data and the sample selection can be found in online Appendix B.

[13] More specifically, the data contain information on parish of residence. We use this information to derive municipality of residence as well as county of residence. A county is an administrative unit comprising a number of municipalities. During the study period of time, Sweden was divided into 25 counties and about 1,000 municipalities. Hospitals in Sweden are administered at the county level, meaning that we will be able to account for any changes in the organization of these in our specifications controlling for county-by-year effects.

[14] According to our data, 80 percent of all individuals enlisted during the year they turned 18, whereas 18 percent did so during the year they turned 19. Virtually no individuals enlisted before the age of 18.

[15] We are grateful to Helena for generously sharing the reform coding with us.

As our second measure of overall health, we use (standardized) height, which is measured at the military enlistment. An adult's height is known to be related to many aspects of his childhood health status (e.g., Bozzoli, Deaton, and Quintana-Domeque 2009) and has been referred to as 'probably the best single indicator of his or her dietary and infectious disease history during childhood' (Elo and Preston 1992). Moreover, Lundborg, Nystedt, and Rooth (forthcoming) found that height measured during the enlistment was an important predictor of earnings among adult Swedish men.

We also make use of three more specific health variables from the military enlistment records: physical work capacity, obesity and hypertension. Physical work capacity is measured as the maximum number of watts attained when riding on a stationary bike (for about five minutes) divided by weight in kilograms. An individual is obese if his BMI (kg/m^2) is higher than or equal to 30. Finally, an individual is hypertensive if either his systolic blood pressure is higher than or equal to 140 mmHg or his diastolic blood pressure is higher than or equal to 90 mmHg.

In addition to the above health variables, we make use of the military's measures of cognitive and noncognitive abilities in our analyses. Cognitive ability is measured by written tests of logical, verbal, spatial, and technical skills, whereas noncognitive ability is determined based on an interview with a psychologist. The objective of the interview with the psychologist is to "assess the conscript's ability to cope with the psychological requirements of the military service and, in extreme case, war." In particular, this implies an assessment of personal characteristics, such as willingness to assume responsibility, independence, outgoing character, persistence, emotional stability, and power of initiative (Lindqvist and Vestman 2011). Both cognitive and noncognitive ability are standardized versions of the measures calculated by the military enlistment service, which range from one to nine.

Excluding parents with missing information on the variables used, our estimation sample includes 405,845 sons where mothers' schooling is observed and 326,600 sons where fathers' schooling is observed.[16] Descriptive statistics for the sample are reported in Table 1. As can be seen from the table, individuals in the parent generation were, on average, born in 1945, whereas the children were, on average, born in 1971. The number of municipalities implementing the reform accelerated over time, meaning that our data primarily covers individuals not exposed to the reform; 20 percent of the mothers and 14 percent of the fathers were affected by the reform.

IV. Results

A. *OLS Relationships*

We start our empirical analysis by providing OLS estimates of the relationships between parental education and sons' outcomes, based on equation (1). As shown in Table 2, all estimates of parental schooling are statistically significant and are in general similar across mothers and fathers. The strongest relationships are found for

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[16] The reason for the smaller number of sons for whom a father is observed is partly because fathers are generally older than mothers and are thus more likely to have been born before the start of our sample period, and partly because SCB in some cases have problems linking individuals to their biological fathers.

Table 1— Descriptive Statistics

	Observations	Mean	Standard deviation
Year of birth	503,768	1,971	5
Global health	501,883	−3.06	4.42
Physical capacity	449,829	4.23	0.72
Height	483,148	179.45	6.48
Obesity	482,123	0.02	—
Hypertension	475,694	0.19	—
Cognitive ability	485,320	5.09	1.90
Noncognitive ability	461,390	5.11	1.69
Mother's education	405,845	10.85	2.66
Mother's year of birth	405,845	1,946	4
Mother exposed to reform	405,845	0.20	—
Father's education	326,600	10.80	2.96
Father's year of birth	326,600	1,945	4
Father exposed to reform	326,600	0.14	—

Note: The table shows summary statistics before the normalization of nonbinary outcome variables.

Table 2—OLS Results

	Global health (1)	Height (2)	Physical capacity (3)	Obesity (4)	Hypertension (5)	Cognitive ability (6)	Non-cognitive ability (7)
Panel A. Mother							
Years of schooling	0.014*** (0.001)	0.034*** (0.001)	0.061*** (0.001)	−0.002*** (0.000)	0.001** (0.000)	0.119*** (0.001)	0.065*** (0.001)
Observations	404,381	389,626	364,954	389,604	384,039	391,399	372,768
Panel B. Father							
Years of schooling	0.017*** (0.001)	0.026*** (0.001)	0.052*** (0.001)	−0.002*** (0.000)	0.002*** (0.000)	0.109*** (0.001)	0.058*** (0.001)
Observations	325,413	313,008	288,799	312,998	307,951	314,339	297,904

Notes: Dummies for parent's year of birth, municipality of residence in 1960, and municipality-specific linear trends are included. Each estimate represents the coefficient from a different regression. Regressions are run using robust standard errors that are clustered at the municipality level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

cognitive and noncognitive ability, where one year of maternal education is associated with about 0.12 standard deviations higher cognitive ability and 0.07 standard deviations higher noncognitive ability. The estimates are almost identical for fathers.

Although the coefficients for our health variables are smaller in magnitude compared to the ones obtained for cognitive and noncognitive abilities, our results again suggest better outcomes for sons with higher educated parents. In particular, one additional year of parental education is associated with between 0.01 and 0.02 standard deviations better global health, about 0.03 standard deviations greater height, and about 0.06 standard deviations better physical capacity. Individuals with more highly educated parents are also significantly less likely to suffer from obesity. In contrast, and perhaps somewhat unexpectedly, we obtain a positive relationship between parental schooling and the incidence of hypertension.

Table 3—First Stage

	(A)	(B)	(C)
Panel A. Mother			
Exposed to reform	0.203***	0.221***	0.253***
F-statistic	(0.059)	(0.034)	(0.038)
	12.0	41.3	44.2
Observations	405,845	405,845	405,845
Panel B. Father			
Exposed to reform	0.237***	0.254***	0.354***
F-statistic	(0.080)	(0.048)	(0.052)
	8.9	28.4	46.8
Observations	326,600	326,600	326,600
County-by-year fixed effects	NO	YES	NO
Municipality-specific trends	NO	NO	YES

Notes: Dummies for parent's year of birth and municipality of residence in 1960 were included. Each estimate represents the coefficient from a different regression. Regressions are run using robust standard errors that are clustered at the municipality level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

B. *First-Stage Results*

We next turn to our 2SLS results, which use reform exposure as an instrument for schooling. In order for the reform instrument to be valid, it is important that it has a strong effect on parental years of schooling. We investigate this by considering the regression results for equation (2), which are presented in Table 3. In specification (A), we regress years of schooling on reform exposure and include birth cohort fixed effects and municipality fixed effects. The results suggest that being exposed to the reform, on average, increases years of schooling by about 0.2 for both mothers and fathers. The *F*-statistics are 12.0 for mothers and 8.9 for fathers.

In model (B), we then add interactions between county of residence and year of birth. This specification is flexible in that it allows for any kind of time-varying behavior in schooling enrollment decisions, given that these are the same for the municipalities within each county. This would be reasonable, for example, if preferences, demographics, or labor market conditions are similar within counties. The *F*-statistics now increase to 30 and 40 and the coefficients on reform status increase somewhat in magnitude.

Specification (C) is our preferred specification, where we include municipality-specific linear trends in addition to municipality fixed effects and birth cohort fixed effects. This implies a very large set of controls as there are more than 1,000 municipalities in our data. The coefficients obtained are similar to those in column B, and the results suggest that the reform, on average, increased mothers' educational attainment by 0.25 years and fathers' educational attainment by 0.35 years. Both *F*-statistics are about 45, showing a strong impact of the reform. It is important to note that the reform predicts both mothers' and fathers' education equally strong, because this means that, for a given relationship between parental schooling and

children's outcomes, significant effects on children's outcomes are equally likely to show up for both parents' education.[17]

The effects of the reform on educational attainment for mothers and fathers are illustrated graphically in Figures 1 and 2. Here, we have plotted the coefficients from a regression where we regress years of schooling on a sequence of timing dummies, indicating time periods up to five years before and five years after the reform implementation, and where we control for birth year and municipality fixed effects. In line with our first-stage results, there is a clear jump at reform implementation. For mothers, there is also evidence that the reform had an impact on individuals born one year before the first reform cohort, which might reflect that some individuals were not in the right grade according to their age and that there is some measurement error in the data on reform implementation. Since we exclude the cohorts right around the reform implementation in our main specification, this jump is not problematic.[18]

C. IV Results

Having established that our first stage is strong enough, we next turn to our 2SLS estimates, shown in Table 4. In panel A, which reports results from regressions controlling for birth-year fixed effects and municipality fixed effects, we find that mother's education significantly improves global health and cognitive ability. Moreover, both maternal and paternal education is associated with a greater physical capacity and increased risk of hypertension.

We next turn to our results from models including larger sets of controls, which we expect will produce more reliable results. Starting with the results in panel B, where county-by-year fixed effects are included, we find that mother's education significantly influences three out of seven outcome variables. First of all, the results suggest that mother's education has strong beneficial effects on her child's general health status, as measured by global health and height. These effects both amount to about 0.1 standard deviations and are much stronger than their OLS counterparts. The effects on the more specific health outcomes are all insignificant and smaller in magnitudes, however. Cognitive ability is positively affected by mothers' schooling, with an estimate that is almost identical to its OLS counterpart, but also to the result in panel A. Whereas there is no significant evidence that mother's schooling would influence noncognitive ability, it is interesting to note that the effects on global health, height, and cognitive ability are all about the same size.

Our results for mothers' schooling in panel C, from our preferred specification where municipality-specific trends are included, are very similar to those in panel B. Moreover, the effect on noncognitive ability is now significant at the 10 percent level, suggesting that mother's schooling also plays a role in shaping individual characteristics such as willingness to assume responsibility, independence and outgoing character. Again, the significant effects are very similar in magnitude and

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[17] Our first-stage results are similar to those obtained by other studies using the Swedish compulsory school reform (Holmlund 2008; Holmlund, Lindahl, and Plug 2011; Meghir and Palme 2005).

[18] The same prereform effect was obtained by Holmlund (2008); Holmlund, Lindahl, and Plug (2011); and Hjalmarsson, Holmlund, and Lindquist (2011).
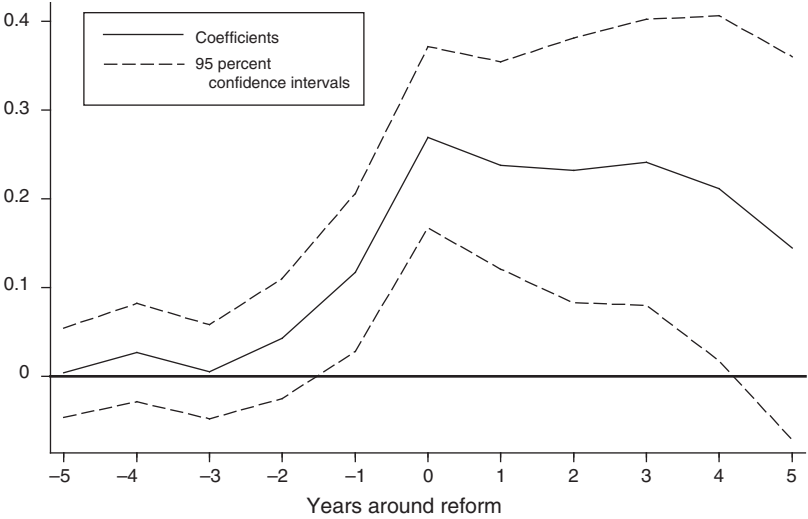
FIGURE 1. MOTHER'S YEARS OF SCHOOLING BEFORE AND AFTER REFORM IMPLEMENTATION

Notes: The figure displays the relationship between individuals' year of birth relative to the first reform cohort in their municipality, and their educational attainment. Municipality-fixed effects and birth year fixed effects are controlled for; estimates are clustered at the municipality level. The reference category is "more than five years before first reform cohort" and category "5" represents "at least five years after first reform cohort." The numbers on the x-axis refer to years before and after the first reform cohort.
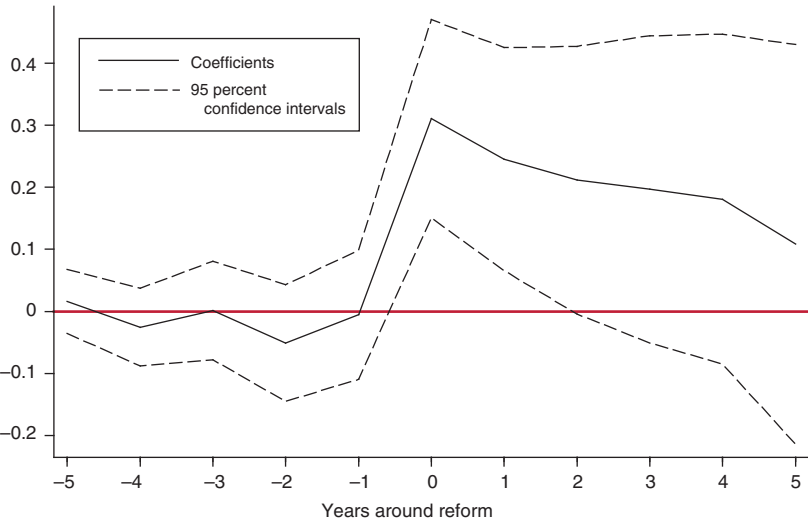
FIGURE 2. FATHER'S YEARS OF SCHOOLING BEFORE AND AFTER REFORM IMPLEMENTATION

Notes: The figure displays the relationship between individuals' year of birth relative to the first reform cohort in their municipality, and their educational attainment. Municipality-fixed effects and birth year fixed effects are controlled for; estimates are clustered at the municipality level. The reference category is "more than five years before first reform cohort" and category "5" represents "at least five years after first reform cohort." The numbers on the x-axis refer to years before and after the first reform cohort.

TABLE 4—IV RESULTS

	Global health (1)	Height (2)	Physical capacity (3)	Obesity (4)	Hypertension (5)	Cognitive ability (6)	Non-cognitive ability (7)
Panel A. Only controlling for birth year and municipality fixed effects							
Mother's schooling	0.083* (0.048)	0.047 (0.040)	0.167** (0.068)	−0.007 (0.007)	0.096** (0.044)	0.089** (0.044)	−0.014 (0.050)
Father's schooling	0.002 (0.047)	−0.025 (0.045)	0.143* (0.077)	−0.005 (0.007)	0.058* (0.033)	−0.022 (0.052)	−0.051 (0.056)
Panel B. Controlling for birth year fixed effects and municipality fixed effects, and interactions between birth year and county of residence							
Mother's schooling	0.139*** (0.051)	0.080* (0.045)	0.055 (0.043)	−0.010 (0.007)	−0.002 (0.018)	0.106** (0.042)	0.048 (0.047)
Father's schooling	0.008 (0.044)	−0.034 (0.044)	0.058 (0.048)	−0.001 (0.006)	0.026 (0.016)	0.001 (0.044)	0.016 (0.047)
Panel C. Controlling for birth year fixed effects and municipality fixed effects, and municipality-specific trends							
Mother's schooling	0.103** (0.046)	0.089** (0.042)	0.023 (0.044)	−0.007 (0.007)	0.023 (0.018)	0.106*** (0.038)	0.076* (0.045)
Father's schooling	0.025 (0.041)	−0.055 (0.037)	0.048 (0.043)	−0.002 (0.006)	0.018 (0.014)	−0.037 (0.042)	0.032 (0.043)
Mothers	404,381	389,626	364,954	389,604	384,039	391,399	372,768
Fathers	325,413	313,008	288,799	312,998	307,951	314,339	297,904

Notes: Each estimate represents the coefficient from a different regression. Regressions are run using robust standard errors, which are clustered at the municipality level.

   *** Significant at the 1 percent level.
    ** Significant at the 5 percent level.
     * Significant at the 10 percent level.

amount to about 0.1 standard deviations, whereas the effects on more specific health outcomes are insignificant and small.

Compared with mother's education, we obtain no evidence that fathers' education affects the outcomes of their children. In both panels B and C, almost all coefficients are smaller in magnitudes compared with those obtained for mother's schooling, and they are all statistically insignificant. Note also that the standard errors are, in general, smaller, suggesting that the insignificant estimates are not simply due to lack of power. Our results suggest a profound difference between the effect of mothers' and fathers' education, irrespective of whether county-by-year effects or municipality trends are used.[19] In the next section, we will shed some light on possible reasons behind this difference.

Given sufficient precision, the effects that we have found should be manifested as jumps in son's outcomes around the time when the first cohorts of mothers in a municipality were exposed to the reform. To examine whether such jumps can be visually detected, we constructed Figures 3 and 4, which, just like Figures 1 and 2, show results from regressions including a sequence of timing dummies, but where we now replaced parental schooling with son's outcomes. We focus on cognitive

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[19] Likely because of the low variation in this measure, using an indicator for "poor health" (based the last two steps on the 14-step scale) produced insignificant effects for both mothers' and fathers' schooling.
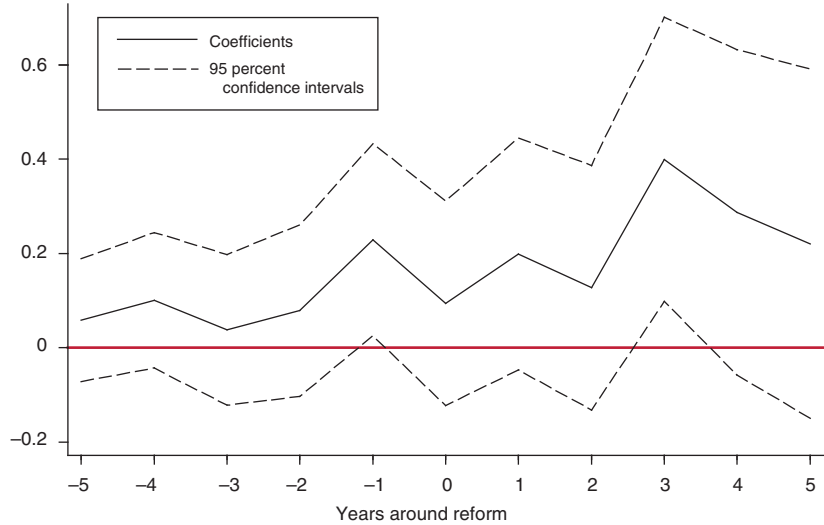
FIGURE 3. SON'S GLOBAL HEALTH BEFORE AND AFTER REFORM IMPLEMENTATION IN
MOTHER'S MUNICIPALITY

Notes: The figure displays the relationship between mother's year of birth relative to the first reform cohort in their municipality, and their son's global health status. Municipality-fixed effects and birth-year fixed effects are controlled for; estimates are clustered at the municipality level. The reference category is "more than five years before first reform cohort" and category "5" represents "at least five years after first reform cohort." The numbers on the x-axis refer to years before and after the first reform cohort.
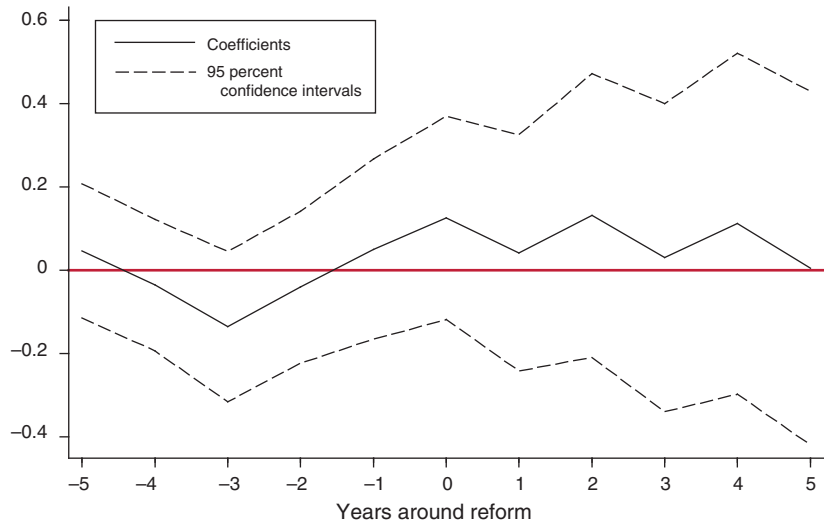
FIGURE 4. SON'S COGNITIVE ABILITY BEFORE AND AFTER REFORM IMPLEMENTATION IN
MOTHER'S MUNICIPALITY

Notes: The figure displays the relationship between mother's year of birth relative to the first reform cohort in their municipality, and their son's cognitive ability. Municipality-fixed effects and birth-year fixed effects are controlled for; estimates are clustered at the municipality level. The reference category is "more than five years before first reform cohort" and category "5" represents "at least five years after first reform cohort." The numbers on the x-axis refer to years before and after the first reform cohort.

ability and global health, which are the outcomes for which all our models suggested that mother's education would play a role. Although the figures are rather noisy, they do suggest better outcomes for sons whose mothers were exposed to the reform. For cognitive ability, for instance, the jump is about 0.02, which, given our IV and first-stage estimates, is in line with what one would expect from a reduced-form effect. In general there is not sufficient precision to obtain significant effects on a year-to-year basis, however. Note also that these graphs are constructed including the problematic cohorts just around reform implementation, and that the "jumps" become clearer when "visually" ignoring these cohorts.

Figure 5 and 6 show the corresponding graphs for fathers' reform exposure. In contrast to the figures for mothers' schooling, these figures provide little evidence that children would have better outcomes if their father was born after reform implementation. This is also fully in line with our IV estimates for fathers, showing no significant effect of schooling on their children's outcomes.

D. Mechanisms

We next shed light on whether our main results could be driven by mediators such as parental income, assortative mating, or fertility. Specifically, we will investigate how these outcomes are affected by parental schooling, in specifications where we instrument parental schooling by reform status.

In the first column of Table 5, we show that schooling has a significant and negative effect on the total number of biological children among mothers, but not among fathers.[20] The effect for mothers is quite small, however, and amounts to less than 0.1 children per additional year of schooling. This effect appears too small to explain our estimated effects of maternal schooling on the various child outcomes. In particular, an increase in mothers' fertility by one child would need to be associated with one standard deviation deterioration in child global health, height, or abilities to explain our findings regarding these outcomes. Moreover, existing evidence in a Swedish context points to no effect of family size on children's educational outcomes (Åslund and Grönqvist 2010).[21]

Next, in the second column, we investigate the effect of education on the timing of fertility. Using a linear indicator for the child's year of birth, our results suggest that there is no evidence in favor of the hypothesis that more educated parents would postpone or advance childbirths. This result contrasts to findings from some other contexts, such as Monstad, Propper, and Salvanes (2008), but is consistent with the findings of McCrary and Royer (2011).

In column 3, we show that one additional year of maternal education led to a statistically significant positive effect on the spouse's education, amounting to about 0.5 years. It is thus possible that some of the positive effects on child's outcomes found for mother's education may be driven by positive assortative mating.

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[20] This variable reflects the individual's total number of biological children as of 1999.

[21] Åslund and Grönqvist estimated the effect of family size on various schooling outcomes of children, using twin births as an instrument for family size. They found small and insignificant effects. Black, Devereux, and Salvanes (2010) used Norwegian military enlistment data and found that one additional child decreased conscripts' cognitive ability by only 0.08 of a standard deviation.
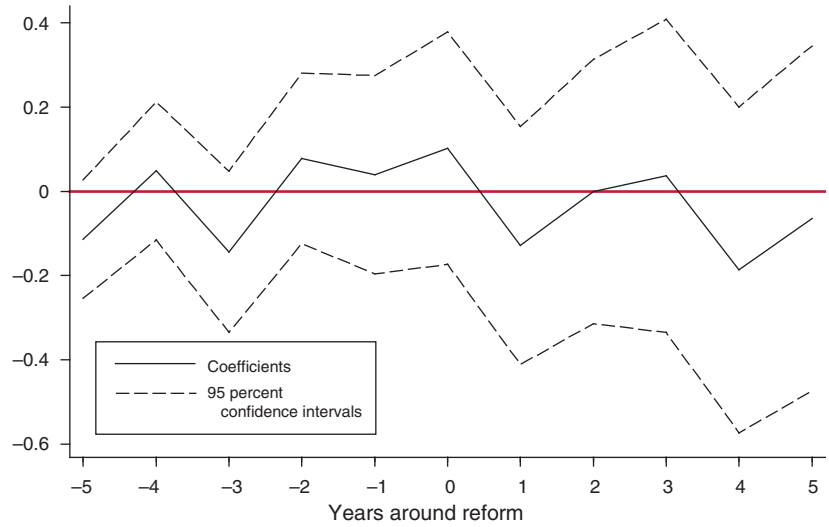
FIGURE 5. SON'S GLOBAL HEALTH BEFORE AND AFTER REFORM IMPLEMENTATION IN FATHER'S MUNICIPALITY

Notes: The figure displays the relationship between father's year of birth relative to the first reform cohort in their municipality, and their son's global health status. Municipality-fixed effects and birth-year fixed effects are controlled for; estimates are clustered at the municipality level. The reference category is "more than five years before first reform cohort" and category "5" represents "at least five years after first reform cohort." The numbers on the x-axis refer to years before and after the first reform cohort.
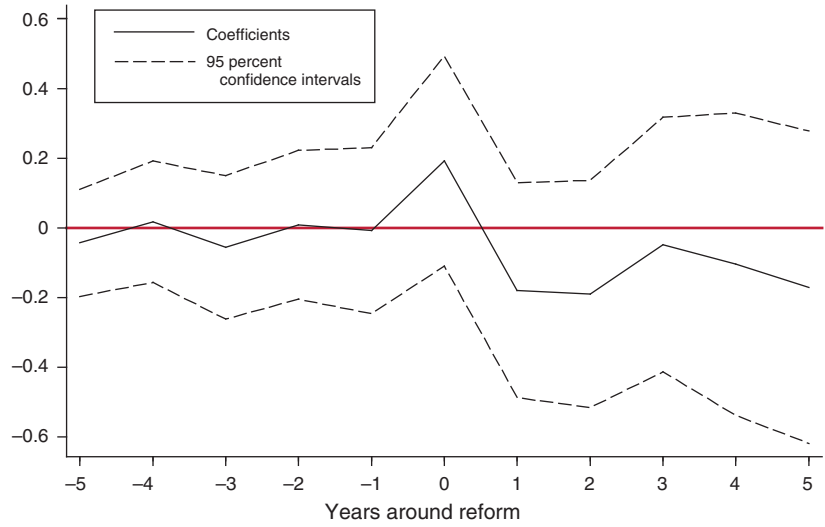
FIGURE 6. SON'S COGNITIVE ABILITY BEFORE AND AFTER REFORM IMPLEMENTATION IN FATHER'S MUNICIPALITY

Notes: The figure displays the relationship between father's year of birth relative to the first reform cohort in their municipality, and their son's cognitive ability. Municipality-fixed effects and birth-year fixed effects are controlled for; estimates are clustered at the municipality level. The reference category is "more than five years before first reform cohort" and category "5" represents "at least five years after first reform cohort." The numbers on the x-axis refer to years before and after the first reform cohort.

Table 5—Mediators

	Number of children (1)	Child's year of birth (2)	The other parent's education (3)	Parent's log income (4)	Parent's income expressed in SEK (5)	Total income of the couple (SEK) (6)
Panel A. Mother						
Years of schooling	−0.100** (0.051)	−0.006 (0.164)	0.511*** (0.138)	0.139* (0.080)	2,638*** (1,005)	3,791*** (1,465)
Observations	305,811	405,845	277,010	368,967	405,845	405,845
Panel B. Father						
Years of schooling	−0.020 (0.042)	−0.023 (0.117)	−0.062 (0.140)	0.014 (0.024)	971 (1,139)	710 (1,780)
Observations	249,273	326,600	292,660	322,535	326,365	326,365

Notes: Estimates are based on equation (1) and (2), where instead of child health or abilities, the above outcomes are considered. Dummies for year of birth and municipality of residence in 1960, and municipality-specific linear trends, have been included. Each estimate represents the coefficient from a different regression. Regressions are run using robust standard errors which are clustered at the municipality level.

*** Significant at the 1 percent level.
 ** Significant at the 5 percent level.
  * Significant at the 10 percent level.

Note, however, that if the full effects of maternal education were to be attributed to assortative mating, the effects of their partners' education would have to be twice as large as the estimates previously reported for mothers in our main results. Moreover, recall that we found no effect of fathers' schooling on any of the child outcomes that we study. For fathers, we also find no effect of education on the spouse's schooling.

Next, in column 4, we investigate the effects of parents' education on their incomes.[22] The results show that one additional year of schooling lead to 13.9 percent higher income in the population of mothers. For fathers, the estimate is much smaller and statistically insignificant. Our findings are in line with Meghir and Palme (2005) who, exploiting the same reform but using a different sample, found small and nonsignificant average effects on incomes when considering the population of males.[23]

We also estimate the returns to schooling on income in terms of units of currency. In Model 5, this is done for the parent's own income, whereas, in Model 6, we examine effects on the sum of both parents' incomes. Again, our findings suggest no significant effect of fathers' schooling on their own income or on family income. On the other hand, one year of mothers' schooling is found to increase their own income by, on average, SEK 2,638 and family income by SEK 3,791, the latter being equivalent to $1,026 in year 2000 US dollars.[24]

We can relate our findings for family income to those of Dahl and Lochner (2012); Duncan, Morris, and Rodrigues (2011); and Milligan and Stabile (2011),

[22] Our income data come from the 1980 tax records and are based on earnings from work and self-employment.
[23] They did find evidence of positive effects on male incomes when restricting the sample to those with unskilled fathers, however. We do not have information on the parents of those who were exposed to the reform.
[24] This was calculated by multiplying SEK 3,791 by the consumer price index provided by Statistics Sweden and then dividing by the PPP exchange rate for private consumption provided by the OECD.

who investigated the causal effects of family income on child achievement. Dahl and Lochner (2012) found that a $1,000 (year 2000 US dollars) increase in family income raised combined math and reading scores by 0.06 standard deviations, on average. Similar results were obtained by Duncan, Morris, and Rodrigues (2011) and Milligan and Stabile (2011), who also considered height and behavioral problems. These findings, together with ours, suggest that the income gains that followed from the increase in mothers' schooling may explain most, or, perhaps, even all, of the improvements in child outcomes that we observe. Furthermore, the lack of returns to schooling for fathers affected by the reform may then be the reason for the finding that fathers' education has no effect on their children's skills and health.

E. *Comparison With Twin Results*

In the literature on the intergenerational transmission on schooling, there is a puzzle, where twin-based estimates often show that fathers' schooling matters as much, or even more, for children's schooling than mothers' schooling, whereas IV-based estimates often show the opposite.[25] Does this puzzle also arise for the outcomes we study? We can shed some light on this by comparing our estimates to the twin-based estimates of Lundborg, Nordin, and Rooth (2012), which use the same data source and study partly the same outcomes as this paper.[26] For our health measures, there is no evidence of such a puzzle, however, since Lundborg, Nordin, and Rooth (2012) found a significant effect of mother's education on global health when employing twin design, but no effect of father's education. For height, the twin estimates were insignificant for both mother's and father's education.

The twin-based estimates do suggest, however, a positive effect of *both* maternal and paternal schooling on the child's cognitive ability (0.05 and 0.04, respectively). Moreover, for noncognitive skills, and in contrast to our findings, the twin-based estimates show a positive effect of fathers' schooling, of 0.04, but no effect for mothers' schooling. This comparison of the twin-based and IV-based estimates thus confirms that there are differences across the methods for these outcomes, even when the same data are used and the same outcomes analyzed. How can this be explained? To shed some light on possible reasons for the divergence in results across methods, and in order to focus the discussion, we take a closer look at one of our outcomes—cognitive ability. Here, we see the puzzle at play, where the twin estimate suggests a positive effect of fathers' schooling, whereas our IV estimate suggests a zero effect.

To start with, we can rule out that the difference in the effect of fathers' schooling across the methods stems from differences in the average level of education across fathers.[27] Instead, the difference might reflect that the methods exploit different

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[25] See Holmlund, Lindahl, and Plug (2011), for instance, where they show that fathers' schooling matters more for children's schooling than mothers' schooling using the twin design but where the opposite result arises when using the IV design.

[26] Since Lundborg, Nordin, and Rooth (2012) also use data from the Swedish enlistment, we can be sure that any differences in results do not arise from how variables are measured or defined or from differences in the institutional context.

[27] In Lundborg, Nordin, and Rooth (2012), the average number of years of schooling was about 10.6 for both mothers and fathers, which is similar to the average of about 10.8 in this paper.

sources of variation in parental schooling and that the identifying assumptions differ. In the twin design, differences in schooling within twin pairs are used for identification, and this source of variation will, in our case, almost never be a result of differences in exposure to the Swedish school reform (since both twins in a twin pair would have been exposed to the same reform). Interestingly, identification of the twin-based estimates therefore excludes exactly the source of variation used by the IV strategy, where differential exposure to the school reform across individuals from different cohorts and municipalities is exploited for identification. If the returns to schooling vary across the education distribution, we would therefore not expect twin-based estimates and IV-based estimates to be the same.

Our intuition tells us that the IV strategy mainly captures the marginal effect of schooling at the lower part of the education distribution, since the reform mostly affected those who would not have obtained more than seven or eight years of schooling had they not needed to. On the other hand, the twin design likely exploits variation in schooling across the entire schooling distribution.[28] We can investigate this more precisely by comparing how the linear fixed effects estimator and the IV estimator weights together the marginal effects at different parts of the schooling distribution into a single estimate, using the formulas in Løken, Mogstad, and Wiswall (2012). In Table C1 in the online Appendix, we show that our IV estimator indeed attaches most weight, between 80 and 90 percent, to the marginal effects at the lower part of the education distribution, that is going from 7 to 9 years of schooling. As expected, the weights are more evenly distributed in the twin design, where only 25 percent of the weight is attached to the 7 to 9 margin. One likely explanation for the diverging results across methods is thus that that IV estimate attaches a much greater weight to the marginal effects at the lower end of the education distribution and that the effect of fathers' schooling on son's cognition are much lower at this part of the distribution. This result would also be in line with our findings in the previous section, where fathers affected by the reform faced a zero return to schooling.[29, 30]

We can shed some further light on the differences across the IV and twin design by focusing on how the income returns differ across the methods. If the return to schooling for fathers is greater at higher levels of schooling, but is essentially zero at the lower part of the distribution, we would expect the twin design, which puts more weight to the marginal effects at the upper part of the education distribution, to give a higher estimate of the return to schooling for fathers. If this is the case, this could also potentially explain why the twin design gives us a positive estimate of fathers' schooling on son's cognitive skills, since there might be an income effect at work. In Table C2 in the online Appendix, we report estimates of the income returns to schooling based

[28] See Lundborg (2013) for an argument that the twin design in some circumstances may come closer to estimate an average treatment effect (ATE) compared to the IV design.

[29] If this explanation holds, we would expect much smaller estimates of the effect of twin fathers schooling on cognitive skills when we restrict the twin sample used by Lundborg, Nordin, and Rooth (2012) to those with a maximum of nine years of schooling. This leaves us with only 7 out of 239 pairs who differ in schooling, however, and the estimates become very imprecise.

[30] In the case of mothers, both the twin FE and IV estimates give a positive and significant effect of education on son's cognitive ability, despite the fact that the weighting of the marginal effects for schooling is similar among mothers and fathers. This might suggest that the marginal effects of mothers' schooling are more similar across the education distribution. Recall also that for mothers, we showed a positive income return to education.

on the twin data used by Lundborg, Nordin, and Rooth (2012). Although precision is rather low when considering the individual's own income as the outcome variable, there is evidence of positive returns to schooling among both mothers and fathers. The effects become stronger when taking the spouse's income into account, however, and the results suggest that mother's schooling has a larger effect than father's schooling on the total income of the couple. Still, there is a substantial return to fathers' education, which makes it less surprising that we also find some positive effects of father's education on son's cognitive ability when employing the twin design.

Finally, the diverging results across the methods might simply reflect that the identifying assumptions differ. With the twin design, we have to assume that schooling differences within twin pairs are unrelated to unobserved differences in nongenetic factors that also affect the outcomes of the children of the twins. If this assumption fails, the estimates will not reflect causal responses and will typically be upward biased. One possibility is therefore that the twin-based estimates of fathers' schooling are more upward biased compared to the corresponding estimates for mothers' schooling and that the 'true' causal effect of fathers' schooling is the one obtained in our IV design.[31] This would also suggest that the twin design overestimates the income return to schooling, whereas the "true" return is closer to the one obtained in the IV design.

F. *Sensitivity Analysis*

In order to assess the robustness of our results in subsection C, a number of alternative specifications are explored in this subsection. We begin by dropping all parents in the city municipalities of Stockholm, Göteborg, and Malmö, for which compulsory schooling amounted to eight years rather than seven before the reform was implemented, and for which the reform was introduced in different school districts at different times, which means that measurement errors in the reform indicator may be greater. As shown in panel A in Table 6, the results do not change much. Compared with our main specification, the effect of mother's education on noncognitive ability has become somewhat smaller and is insignificant.

Second, we consider what happens when only parents born in 1943 or later are included. We impose this restriction since municipality of residence in 1960, which we used to assign reform exposure, is likely to be a better indicator of the municipality where the individual went to school if only individuals who were under 18 years old in 1960 are included. The results, displayed in panel B, show little differences from our main results, however.[32]

Third, in panel C, we note that there is a small group of *children* in our data who belonged to birth cohorts where the reform had not yet been implemented in their

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[31] A third possibility is that twins are intrinsically different and face different returns to schooling than nontwins. Twins have lower birth weight, on average, for instance, and if the returns to schooling are heterogeneous with respect to birth weight, this could also explain why the twin and IV methods give different results. We investigated this by applying our IV strategy on the sample of twins, but the sample was too small and the first stages weak.

[32] Another option is to drop individuals that have left school by 1960. This may eliminate movers to an even larger extent, but also runs the risk of introducing selection issues. We show results for this specification in online Appendix D. Overall, the point estimates are similar to those of our main specification but the estimates for global health and noncognitive ability have become statistically insignificant for mothers' education.

TABLE 6—SENSITIVITY ANALYSIS

	Global health (1)	Height (2)	Physical capacity (3)	Obesity (4)	Hypertension (5)	Cognitive ability (6)	Noncognitive ability (7)
Panel A. Excluding the municipalities of Stockholm, Göteborg, and Malmö							
Mother							
Years of schooling	0.085** (0.040)	0.087** (0.039)	0.037 (0.040)	−0.008 (0.006)	0.021 (0.016)	0.109*** (0.036)	0.061 (0.039)
Observations	362,763	349,407	327,086	349,385	344,819	351,007	333,730
Father							
Years of schooling	0.034 (0.037)	−0.058* (0.033)	0.022 (0.037)	−0.003 (0.005)	0.018 (0.013)	−0.013 (0.035)	0.0304 (0.037)
Observations	292,701	281,415	259,377	281,405	277,281	282,678	267,364
Panel B. Excluding parent individuals born prior to 1943							
Mother							
Years of schooling	0.090* (0.054)	0.090* (0.051)	−0.008 (0.052)	−0.005 (0.008)	0.035 (0.022)	0.103** (0.044)	0.093* (0.052)
Observations	311,800	299,685	276,680	299,677	294,911	301,029	285,035
Father							
Years of schooling	0.011 (0.051)	−0.036 (0.048)	0.010 (0.052)	−0.001 (0.008)	0.020 (0.017)	−0.007 (0.044)	0.025 (0.048)
Observations	237,336	227,811	206,193	227,808	223,843	228,733	215,381
Panel C. Excluding child individuals born up to 1959							
Mother							
Years of schooling	0.100** (0.046)	0.090** (0.043)	0.022 (0.044)	−0.007 (0.007)	0.025 (0.018)	0.106*** (0.039)	0.073 (0.045)
Observations	400,379	385,673	361,006	385,655	380,090	387,440	368,854
Father							
Years of schooling	−0.026 (0.041)	−0.055 (0.036)	0.051 (0.042)	−0.002 (0.006)	0.018 (0.014)	−0.037 (0.042)	0.033 (0.043)
Observations	324,846	312,448	288,239	312,438	307,391	313,779	297,351
Panel D. Adding parent individuals belonging to the cohorts right around the implementation							
Mother							
Years of schooling	0.014 (0.032)	−0.001 (0.029)	0.004 (0.031)	−0.007 (0.005)	0.010 (0.011)	0.069*** (0.027)	0.071** (0.031)
Observations	473,316	455,682	424,404	455,660	448,742	457,712	434,985
Father							
Years of schooling	0.013 (0.025)	−0.015 (0.024)	0.030 (0.024)	−0.003 (0.004)	0.017* (0.009)	0.002 (0.024)	0.014 (0.027)
Observations	373,810	359,368	329,280	359,358	353,287	360,867	341,254
Panel E. Adding quadratic trends							
Mother							
Years of schooling	0.024 (0.066)	0.132** (0.063)	−0.029 (0.059)	−0.007 (0.010)	0.026 (0.027)	0.179*** (0.055)	0.032 (0.063)
Observations	404,381	389,626	364,954	389,604	384,039	391,399	372,768
Father							
Years of schooling	0.000 (0.058)	−0.039 (0.056)	0.054 (0.056)	−0.003 (0.008)	0.043** (0.019)	−0.054 (0.056)	0.029 (0.052)
Observations	325,413	313,008	288,799	312,998	307,951	314,339	297,904

Notes: Estimates are based on equation (1) and (2). Dummies for year of birth and municipality of residence in 1960, and municipality-specific linear trends, have been included. Each estimate represents the coefficient from a different regression. Regressions are run with robust standard errors that are clustered at the municipality level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

municipalities. In principle, assuming a positive relationship between parental and child reform exposure, our previous results may thus have been driven by a direct effect of the reform on the child. To avoid this risk, we drop children born 1959 or earlier.[33] Again, results are virtually unchanged.

Fourth, we consider specifications where we add individuals belonging to the first reform cohort and the cohorts immediately preceding and following it. As noted, these cohorts are likely problematic because of misclassifications of reform status, grade repetition, and adjustment effects. The results are reported in panel D. For cognitive and noncognitive ability, results are quite similar to our main specification. Global health and height have now become insignificant for mothers' education, however. It is not surprising that some of the estimates become insignificant as we now include cohorts where we know there is some measurement error in the reform indicator.

Finally, in panel E, we depart from our main specification by adding quadratic municipality-specific trends. Here, the effects of mothers' education on global health and noncognitive ability turn insignificant, whereas the effects on height and cognitive ability become stronger compared to our main results. As we noted in subsection IIB, municipality-specific quadratic trends may also pick up accelerations or decelerations that result from the reform itself. By making sure that one has a large number of prereform cohorts, this problem may become less severe, since the estimated trends to a larger extent will reflect prereform trends. We therefore obtained additional data on parent individuals born 1934–1939, not used in our main specification, and who were mostly not exposed to the reform.[34] As shown in online Appendix E, bringing in those individuals leads to more precise estimates and most of the effects that were significant in our main specification are now also significant in our specification where we add quadratic municipality-specific trends. Adding quadratic trends only removes the significant effect on height but, instead, the effect of maternal education on sons' physical capacity is now positive and significant.

V. Conclusion

We contribute to the literature by providing new evidence on the causal effect of mothers' and fathers' schooling on a wide range of outcomes of the next generation, including their health and their cognitive and noncognitive skills. In addition, we investigate through which mechanisms the effects of parental schooling arise. We base our estimates on a comprehensive dataset of all Swedish males and use the Swedish compulsory school reform as a source of exogenous variation in parental schooling.

Our preferred estimates suggest that mothers' schooling improves their children's general health status, as measured by height and global health, as well as their cognitive and noncognitive ability. In terms of standard deviations, the effects on height,

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[33] The year 1958 was the last reform cohort, but we drop children born up to 1959 to avoid misclassification issues.

[34] This also means that we add child individuals born 1950–1954.

global health, and cognitive ability are all of similar magnitude. In general, these findings are robust to a number of sensitivity checks.

Even though the reform had equally strong effects on mothers' and fathers' schooling, we obtain no evidence that fathers' schooling would improve their children's health or abilities. One possible explanation for this gender difference in results is that only mothers affected by the reform faced an income return to the increase in schooling. This difference in income gains across genders also opens up the possibility that an increase in the bargaining power of women explains part of the effect of maternal education on child outcomes.

Our results in some cases differed substantially from the twin-based estimates of Lundborg, Nordin, and Rooth (2012), which uses the same data source and study partly the same outcomes. We showed that one likely reason for the divergence in results is that our IV estimator attaches almost all the weight to the marginal effects at the lower end of the education distribution when calculating the IV estimate. The twin FE estimator, on the other hand, attaches a much lower weight to the marginal effects at this part of the distribution. If the effect of schooling is different across the schooling distribution, it is therefore not surprising that we get different IV and twin FE estimates.

Although our IV estimates for fathers do not provide any evidence that their education matters in terms of their offspring's outcomes, it should be noted that this conclusion is only valid for the fathers affected by the reform. Indeed, our twin estimates indicate that father's education may have a positive impact on young adult children's outcomes when positive economic effects of father's education are present. We leave it to future studies to examine whether paternal education has an effect on the health and abilities of young adults in contexts where also fathers benefit economically from an increase in schooling.

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