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

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Abstract:

In this paper, we use the Swedish compulsory school reform to estimate the causal effect of parental education on sons' outcomes. We use data from the Swedish military enlistment register of the entire population of males and focus on outcomes such as cognitive skills, noncognitive skills, and various dimensions of health at the age of 18. We find significant and positive effects of maternal education on sons' skills and health status. Although the reform had equally strong effects on fathers' and mothers' education, we find little evidence that paternal education improves sons' outcomes.

Keywords: Education, cognitive skills, noncognitive skills, health, causality, school reforms.

JEL codes: I12, I28, J13.

I. Introduction

An individual's success in life is determined to an important extent by his or her abilities and health capital, as formed in childhood and youth. In the literature on skill formation, cognitive and noncognitive skills during childhood and adolescence 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 et al. 2006; Cunha and Heckman 2009; Lindqvist and Vestman 2011). Similarly, a recent literature, spanning over medicine as well as economics, shows the importance of early life health for a number of adult outcomes (see, for example, the recent surveys by Currie 2009 and Almond and Currie 2011).

But what determines a person's abilities and health in childhood and adolescence? A key factor is believed to be the human capital of one's parents. Children of more highly educated parents tend to have better outcomes along a number of dimensions, such as health and cognition, and, ultimately, labor market outcomes (see Currie 2009 for an overview). In particular, maternal education is widely believed to be of great importance and numerous studies of all types of countries have found a strong correlation between maternal education and various child outcomes, such as health, mortality, and schooling (see for instance Haveman and Wolfe 1995).

It is not clear, however, how one should interpret the correlation between parental education and children's outcomes. Does parental education actually improve child outcomes 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. Moreover, parental education would then be an important mechanism through which inequality is transmitted across generations. Unfortunately, there is very little evidence on the causal effect of parental education on child outcomes. There are a few studies in the context of developing countries where a causal effect of, in particular, mother's education is implied but it is unclear to what extent these results generalize to developed countries (Breierova and Duflo 2004; Chou et al. 2010). For developed countries, the evidence to date is also very limited and the few existing studies reach rather different conclusions about (Currie and Moretti 2003; McCrary and Royer 2011; Lindeboom et al. 2009; Chevalier and Sullivan 2007; Carneiro et al. 2012).[1] Clearly, the limited evidence makes it difficult to conclude that the relationships between parental education and child outcomes estimated in the literature are causal and we are left wondering if the relationships mainly reflect the influence of hard-to-observe factors such as genes, environment, and family background.

In this paper, we contribute to the literature by providing new evidence on the causal effect of mother's and father's schooling on their sons' health and skills. We do so by employing the Swedish compulsory school reform, which was rolled out over the country during the 1950s and 1960s. An important

[1] We review these studies in more detail below.

feature of our identification strategy is that the timing of the reform varied across municipalities, which gives us 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 have linked this data on the parental generation to register-based data on their children, taken from the Swedish military enlistment register. Since females are not obliged to enlist for the military in Sweden, this means that 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.

Another important feature of our data is that health and abilities are measured at the age of 18. Many previous studies have focused on the effect of parental schooling on child outcomes already at birth or in the very early stages of life, although it is known that many personal characteristics, such as cognitive and noncognitive skills, are not yet fully developed at these ages (Cunha et al. 2006). We are aware of no previous study that considered the effect of parental schooling on a wide range of offspring outcomes at age 18; by doing so, our study may thus be more suggestive of the more permanent consequences of parental education on the outcomes of children.

II. Background

Why may parental education matter for child outcomes? Commonly proposed causal pathways include the improved knowledge and the greater economic resources that accompany education. The latter pathway refers to the fact that higher education usually also means higher income. To the extent that this also spills over to the child, this is one mechanism through which parental education affects child outcomes. Causal evidence for income effects were obtained by Dahl and Lochner (2012), who found that increases in family income resulting from changes in the Earned Income Tax Credit (EITC) showed a positive effect on children's math and reading test scores. The gains were largest for children from disadvantaged families, perhaps reflecting the diminishing returns to additional family resources. Similar findings were reported in Duncan et al. (2011) and Milligan and Stabile (2011), using quasi-experimental variation in government income transfers, and in Løken et al.

(2012), exploiting regional variation in the income boom that followed the discovery of oil in Norway.

Possible explanations for the positive income effects include greater affordability of health care inputs (Currie 2009). This mechanism should perhaps not be overemphasized in a country like Sweden, however, where health care coverage is universal and of low cost. Still, income may be related to the quality of neighborhoods and schools, as well as to the affordability of cognitively stimulating material and activities (Yeung et al. 2002), which may all generate links between parental income and child outcomes.

An alternative mechanism is that higher income is related to lower fertility, since time-intensive child-caring becomes more costly as income rises. To the extent that there is a trade-off between child quality and child quantity (Becker and Tomes 1976), this suggests an alternative explanation for the effect of education on child outcomes.[2] In our empirical analysis, we will 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 could easily 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. Such an increase in 'productive efficiency' implies that additional education allows an individual to obtain better child outcomes from a given set of inputs. Similarly to productive efficiency, it is also possible that education facilitates allocative efficiency in health production, meaning that more educated parents are better able to choose a better *mix* of health inputs in the production of child health and skills (e.g., Thomas 1994).

It should be noted that any effects of parental education that are generated through improved knowledge or increased resources would be magnified under positive assortative mating. Improved education would then also lead to higher-quality spouses, boosting total resources and knowledge in the household. Since we have data on spouses' characteristics, we are able to shed light on this issue in our empirical analysis.

Although our discussion so far has focused on a possible causal relationship between education and child outcomes, any positive empirical relationship could also be generated through non-causal mechanisms. In

[2] An offsetting effect would be if parents with a high value of time invest less time in child health and the production of skills. In countries like Sweden, however, there may be high-quality and low-cost substitutes such as child care, with highly trained personnel.

[3] Increased parental income may also increase the bargaining power of the parent whose income was subject to the increase, which shifts household spending towards items that this individual values more. Since mothers are often found to value the family's health more than fathers do, higher income of the mother relative to the father is one additional route through which income may affect children's health and skills (see Behrman 1997 for a survey).

particular, since parents and children share common genes, any relationship between them may be generated through unobserved genetic endowments. More generally, preferences and personality traits, whether genetically induced or not, may be shared by parents and children, which, again, may engender a positive relationship between parental education and children's outcomes. A related hypothesis was formulated by Fuchs (1982), who suggested that education and health were related through an individual's time preferences. The argument is that both investments in health and education are of long-run character, since the benefits in both cases occur in the future, and that future-oriented individuals will thus invest in both. Translated to the parent-child relationship, it would mean that future-oriented parents invest more both in their children's health and skills and in their own education but there may exist no causal relationship between the two types of outcomes. If this or other underlying factors are the reason why parental education and child outcomes are correlated, it would mean that policies that increase parental education would have no effect on child outcomes.

What then does the existing literature say about the existence of a causal effect of parental education on child outcomes? Unfortunately, there are few studies on the topic and the existing ones reach quite different conclusions. Regarding health outcomes, Currie and Moretti (2003) found that maternal education improved birth weight (and reduced smoking) in the US, whereas McCrary and Royer (2011) found no effect on birth weight and other birth outcomes using US data. As noted by Royer and McCrary, this most likely reflects that different identification strategies were used. Currie and Moretti (2003) exploited variation in the access to colleges, whereas McCrary and Royer used birth date variation to obtain exogenous variation in schooling. It is therefore likely that the subgroups affected by the instruments differed between the studies.

Yet another source of exogenous variation in schooling was used in two studies in a UK context. Lindeboom et al. (2009) used the Britain’s compulsory school reform in 1947 in to estimate the causal effect of schooling on child health. They found no evidence of a causal effect of parental schooling on child health outcomes at birth or at ages seven, 11, or 16. Due to a small sample size, however, their estimates suffered from a relatively low precision. Using the same reform, Chevalier and Sullivan (2007), obtained evidence of heterogeneous effects and found that the most impacted groups experienced larger changes in infant birth weight. One disadvantage of the British school reform was that it affected entire cohorts, which makes it difficult to separate out reform effects from cohort effects.

In the spirit of Currie and Moretti (2003), Carneiro et al. (2012) used US data and instrumented mothers’ education with variation in schooling costs during their adolescence. They found a significant effect of maternal education on child test scores, and also on measures of behavior problems.

Other studies have tried to get at the causal effect of parental education on child outcomes with alternative research designs. Lundborg et al. (2011b) used both a twin design and an adoption design and applied these to data from the Swedish enlistment register. Under both designs, parental education was significantly related to improved cognitive skills, noncognitive skills, and health. A twin design was also adopted by Bingley et al. (2009), who related parental education to children's birth weight, finding a small but significant effect.[4]

The different results in the literature should come as no surprise, since different outcomes are studied, different identification strategies are used, and the contexts are different. Yet, as noted by Currie (2009), the contrasting results also make it difficult to state definitely that the relationship between parental background and children's health is a causal relationship. In this paper, we expect the instrument to impact most on low-educated people, who would not have gone through an additional year of schooling had they not needed to. For policy purposes, this is a group of special interest, since compulsory school reforms may have a big impact on just this group.

III. Method

A. *The School Reform*

The Swedish compulsory school reform has been previously described by Holmlund (2008), Meghir and Palme (2005), and, more extensively, by Marklund (1980, 1981). Here, we provide a brief overview of the reform. In the 1940s, prior to the implementation of the reform, children in Sweden went to a common school (*folkskolan*) until either fourth or sixth grade. Individuals with sufficient grades were then selected for the junior secondary school (*realskolan*), where they stayed for three, four or five years; the exact arrangements differed from municipality to municipality. Individuals who were not selected for junior secondary school had to remain in the common school until compulsory schooling was completed. Compulsory schooling spanned seven years or in some municipalities (mainly in the large cities) eight years.[5]

There was growing political pressure for school reform throughout the 1940s, however. In particular, the Swedish educational system was deemed insufficient compared with the many other Western countries that had already introduced eight or nine years of compulsory schooling, or were about to do so. In 1948, a parliamentary committee delivered their proposal to introduce a new compulsory school, consisting of nine compulsory years. In the new compulsory school, students would be kept together until the eighth grade and in the ninth grade follow different tracks; this streaming in the ninth grade was later abandoned, however.[6] The reform also affected the curriculum somewhat,

[4] The estimates in the twin studies are identified for the (parental) twin pairs that differ in schooling. If such differences are found all over the education distribution, twin estimates may come closer to estimating an average treatment effect. This, however, requires twins not to differ in other respects that are related to their education as well as offspring outcomes.

[5] As children in Sweden generally start school during the calendar year in which they turn seven, this means that compulsory schooling normally lasted until the age of 13 or 14.

[6] The reform might thus also have affected class composition, due to the changes in the

mainly by introducing English as a compulsory subject.

Rather than being introduced across the country at the same time, the school reform was set to be implemented gradually, to enable evaluations of its appropriateness before decision was taken whether to implement it nationwide.[7] Beginning in 1949, 14 municipalities introduced the school reform. More municipalities were then added year by year; the reform was generally implemented by all school districts within the municipality, with the exception of the three big city municipalities of Stockholm, Göteborg, and Malmö, where the reform was implemented in different parts of the municipalities at different times. In 1962, the Swedish parliament decided that the reform should be implemented throughout the country and that all municipalities needed to have the new system in place no later than 1969.

This paper is not the first to use the Swedish compulsory school reform as a source of exogenous variation in schooling. Meghir and Palme (2005) established that the reform increased educational attainment and led to higher labor incomes. They were also able to account for selective mobility, that is, whether their results would be biased by individuals moving to or from reform municipalities to choose suitable schooling for their children. They found no evidence of this being the case. A study by Holmlund et al. (2011) used the reform as an instrument for parental schooling, and found evidence of a causal effect of parents' educational attainment on children's educational attainment.

Regarding health outcomes, Spasojevic (2010) used Swedish survey data and found some weak evidence that one year of additional schooling generated by the reform led to better self-reported health and a higher likelihood of having a BMI (body mass index) in the healthy range. Lager and Torssander (2012) documented that individuals exposed to the reform had a somewhat lower mortality risk after the age of 40. Meghir et al. (2012) investigated hospitalizations as well as mortality among individuals aged 40 to 60 exposed to the reform but were not able to find significant effects on any of these outcomes. No previous study has examined the effects on health and skills among children of those who were exposed to the reform.

B. *Econometric Method*

Our empirical model is based on the following two equations.

$$(1)\ H^c = \alpha_0 + \alpha_1 S^p + \alpha_2 Y^p + \alpha_3 M^p + \alpha_4 Trend^p + \varepsilon,$$

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timing in ability tracking. In particular, some students might have benefitted from having more high-ability individuals in their class after the reform (e.g., Ding and Lehrer 2006). Moreover, patterns of assortative mating could also have been affected. We discuss these issues in more detail in the Methods subsection. Streaming in ninth grade was abandoned in 1969.

[7] 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. Meghir and Palme (2003) as well as Holmlund (2008) document that there is no evidence that reform implementation is associated with various personal characteristics however, although Holmlund points out the importance of controlling for municipality fixed effects, birth year fixed effects, and municipality trends when the reform is used as an instrument for schooling. We return to these issues in the Econometric Method subsection.

$$(2)\ S^p = \beta_0 + \beta_1 R^p + \beta_2 Y^p + \beta_3 M^p + \beta_4 Trend^p + \upsilon.$$

In these equations, *c* denotes the child and *p* one of his parents. *S* refers to parental years of schooling, *M* is a set of municipality fixed effects, *Y* is a set of birth year fixed effects, *Trend* is a set of municipality-specific linear trends, and *H* is the outcome of interest. *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 relationship between parental education and the various child outcomes, we first estimate OLS regressions based on equation (1). Given that schooling is likely to be correlated with various omitted factors such as abilities and other personality traits, these results cannot be interpreted as causal effects of parental schooling. We therefore apply Two Stage Least Squares (2SLS), using equation (2) as the first stage, where schooling is instrumented by reform status. The identification of α_1, the coefficient of interest, then relies upon that part of variation in parental schooling that is generated by the reform. Our empirical strategy is similar to the one used in previous reform-based papers, such as Black et al. (2005). The first stage is based on a difference-in-differences approach (DiD), wherein individuals treated by the reform are compared with individuals in the same municipality before treatment as well as with individuals in other municipalities, while possible trends at the municipality level are also taken into account.

Given that certain conditions are fulfilled, 2SLS estimates can be interpreted as weighted averages of the causal responses of those individuals whose treatment status is changed by the reform instrument (e.g., Imbens and Angrist 1994; Angrist and Imbens 1995). First of all, the *independence assumption* requires that reform exposure is as good as random, conditional on the controls included. As we expect reform implementation to be correlated with both municipality and time-specific effects, these need to be controlled for. Reform implementation may, however, also be correlated with factors that change both within municipalities and over time. In our main specification, we account for this by including municipality-specific trends. As an alternative way to pick up such characteristics that may change both over time and in space, we explore specifications using interactions between home county and birth year.

In order for the independence assumption to hold, it is also important that individuals do not choose their reform status. This could be the case if individuals moved to or from reform municipalities in a systematic way. We are not able to investigate this issue in detail, but rely on Meghir and Palme (2005), who had access to data on municipality of birth as well as municipality at school age, and, as mentioned, obtained no evidence of selective mobility being an issue.[8]

[8] They used two different approaches to investigate this. First, they re-estimated their regressions, only including individuals who did not change reform status as a result of moving

A second assumption required for 2SLS to reflect an average of causal responses is the *exclusion restriction*, saying that the reform should only affect child outcomes through its effect on years of parental schooling. This assumption would be violated if the reform also affected the quality of the education provided. In particular, it is possible that some schools hired a number of teachers in advance and started re-organizing already some time before the introduction of the reform, and that for some schools 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 will not consider individuals born in the first reform cohort and in the cohorts immediately following and preceding it.[9]

Moreover and as already mentioned, class composition was influenced by the reform since ability tracking was postponed. This can affect peer group composition as well as patterns of assortative mating. Additionally, a more highly educated population in general may have indirect effects on individuals' outcomes through better (or worse) labor market opportunities. It is unclear to what extent such general equilibrium effects would be present and we note that most studies relying on school reforms would be subject to this risk.

As a third condition, the reform must, on average, *affect educational attainment* in order for it to be used as a source of exogenous variation in schooling. It is also important that the effect on educational attainment is rather strong. In the Results section, we show that this is indeed the case, among both mothers and fathers.

Fourth and finally, the *monotonicity assumption* requires that the sign of the response to the reform is homogeneous in the study population. In our case, that means that no individuals reduced their investments in schooling as a result of the reform. In principle, one could imagine that some individuals who would have continued to higher education when not affected by the reform instead chose to stop after nine years of schooling when forced to do at least nine; for example owing to changing preferences or institutional arrangements. Although such possibilities cannot be ruled out, we document that the reform rather had a positive impact on the share of individuals obtaining more than nine years of education in our samples of mothers as well as fathers.

IV. Data

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.

[9] In line with adjustment effects, but also with the possibility that some individuals might not have been in the right cohort for their age or that the reform coding was subject to some measurement errors, Holmlund et al. (2008) documented that the Swedish compulsory school reform had a substantial impact on educational attainment even in the cohort one year prior to the first reform cohort, as well as evidence of the reform having a different effect on schooling in the first reform cohort and in the cohort right after it compared to later cohorts. We tried to replicate this finding and obtained similar results, which is not surprising as the datasets are similar.

Our dataset was constructed by integrating registers from Statistics Sweden (SCB) and the Swedish National Service Administration. The former includes the census on population and housing (*Folk- och bostadsräkningen*) from 1960, virtually covering the entire Swedish population alive in this year, and the multi-generation register (*Flergenerationsregistret*), allowing us to link parent individuals to (biological) children born during later years. There are also data on educational attainment for 1999, which is expressed in terms of the highest degree attained. On this basis, a standard number of years of schooling has been assigned. Our data include parents with information on educational attainment who were born between 1940 and 1957; these are essentially the birth cohorts for which there is variation in terms of whether the reform was implemented or not.

Data on home municipality were obtained from the census on population and housing; there are 1,029 municipalities in our data. During the study period of time, Sweden was divided into 25 counties. We obtained data on people's home county from data on home municipality in 1960.

The reform assignment is based on an algorithm provided by Helena Holmlund, which was described in Holmlund (2008).[10] The algorithm uses historical evidence on reform implementation and assigns the reform exposure variable to individuals depending on their year of birth and home municipality in 1960. Individuals need to be in the correct grade according to their age in order for the algorithm to classify them correctly with respect to reform status.

As noted earlier, the reform was implemented for all school districts at the same time in entire municipalities, with the exception of the three big city municipalities in Sweden, where implementation was more gradual. With the algorithm provided by Holmlund, the implementation cohort in these three cities is only set to one when the entire municipality has transferred to the new system; parishes within these municipalities that are known to have implemented the reform in the first years are dropped. Still, it is possible that measurement errors are larger in these three city municipalities compared with the rest of the country (Holmlund 2008). In one of our sensitivity checks, we drop these cities.

Data on offspring health and skills are obtained from the military enlistment records, covering individuals born between 1950 and 1979, although no individuals from the earlier cohorts will be used as parents must have been born no earlier than 1940.[11] 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. Men usually underwent the military enlistment procedure at the age of 18 or 19.[12] Refusal to enlist was punishable with a fine, and eventually

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

[11] Also, the children had to live in Sweden during 1999 since the enlistment information was initially collected for the 1999 population data.

[12] According to our data, 80 percent of all individuals enlisted during the year they turned

imprisonment, implying that the attrition in our data is very low; only about 3 percent of each cohort of males did not enlist.

A. *Outcome Variables*

Our analysis uses several different measures of individuals' overall health status which are available in the military enlistment data. First, depending on the conscript's health conditions and their severity, the National Service Administration indicated the conscript's overall health status on a 14-step scale, which was used to determine the individual's suitability with respect to type of military service. As our first measure of overall health, which we will refer to as 'global health' 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 normalize this variable to have standard deviation one.[13] For ease of interpretation and comparison, all the non-binary outcome variables used in our analysis will also be normalized to have standard deviation one.

The assessment of individuals' health that underlies the assignment of the global health variable is based on a health declaration form that the individual filled in at home and had to bring with him, combined with a general assessment of the individual's health lasting for about 20 minutes, performed by a physician. Before meeting with the physician, the individual underwent a number of physical capacity tests and met with a psychologist who, if necessary, could provide the physician with notes regarding the individual's mental status. The individual was expected to bring any doctor's certificate, health record, drug prescription or similar proving that he actually suffered from the conditions he reported in his health declaration, making cheating difficult. Moreover, the incentives to cheat were probably low since almost everyone was required to undergo military service during the study period. It is an important advantage of our data that health status was not simply self-reported, but also based on obligatory assessments. Consequently, measurement errors from, for example, differences in health-seeking behaviors or in health awareness, which may be present in sources like hospital and insurance records or standard self-evaluations, should be less of an issue.

As an alternative measure of overall health, we use height, as determined at military enlistment. An adult's height relates to many aspects of their childhood health status (e.g., Bozzoli et al. 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). It has been documented that children of parents with more years of education tend to be taller (e.g., Thomas 1994), although it is not clear if this relationship reflects a causal effect.

In addition to height, we make use of three different physical test variables from the military enlistment records, which relate to certain dimensions of

18, whereas 18 percent did so during the year they turned 19. Virtually no individuals enlisted before the age of 18.

[13] In Lundborg et al. (2011a) we showed that this measure of global health strongly predicted adult incomes in a sample of siblings and twins.

individuals' health and capacities. First of all, we make use of physical work capacity, measured as the maximum number of watts attained when riding on a stationary bike (for about five minutes) divided by weight in kilograms. Measures of this type are often referred to as Maximum Working Capacity and have consistently been associated with lowered risk of premature deaths from mainly cardiovascular diseases and to a lesser extent with lowered risk of cancer-related mortality (Ekelund et al. 1988; Slattery and Jacobs 1988; Blair et al. 1989; Sandvik et al. 1993).[14] The measure is closely related to maximum oxygen uptake (VO_2 max), which has been labeled as 'the single best measure of cardiovascular fitness and maximal aerobic power' (Hyde and Gengenbach 2007). A large number of studies have found a positive relationship between parents' education and child or adolescent physical activity (Stalsberg and Pedersen 2010), which, if causal, would also suggest that more parental education may lead to a higher physical capacity of their children.

Second and third, we include indicators of obesity and hypertension. Using standard definitions, we classify individuals as obese if their BMI (kg/m^2) is higher than or equal to 30 and as hypertensive if either their systolic blood pressure is higher than or equal to 140 mmHg or their diastolic blood pressure is higher than or equal to 90 mmHg. Both obesity and hypertension are well-known risk factors of diseases such as cardiovascular diseases and diabetes (e.g., Sowers et al. 2001; Poirier et al. 2006). Obesity can also lead to discrimination in the labor market (e.g., Lundborg et al. 2010). It has been documented that children of parents with more years of education tend to have lower incidences of obesity and hypertension (e.g. Coto et al. 1987; Lamerz et al. 2005).

We also include measures of cognitive and noncognitive ability. Cognitive ability is measured by written tests of logical, verbal, spatial and technical skills. According to his results in these tests, the individual has been assigned a number on a nine-point scale, approximating a normal distribution.

Noncognitive ability is also measured on a scale between one and nine which approximates a normal distribution. The assignment of this number was done by a psychologist, based on a semi-structured interview lasting for about 25 minutes, whose objective was "to assess the conscript's ability to cope with the psychological requirements of the military service and, in extreme case, war" (Lindqvist and Vestman 2011). This in particular implies an assessment of personal characteristics such as willingness to assume responsibility, independence, outgoing character, persistence, emotional stability and power of initiative. In addition, an important objective of the interview is to identify individuals considered particularly unsuited for military service, such as those with antisocial personality disorders, those with difficulty accepting authority, those with difficulties adjusting to new environments and those liable to

[14] Moreover, in a field experiment, Rooth (2011) found that physical capacity (as measured at the Swedish military enlistment) has positive effects on subsequent labor market outcomes in terms of a higher probability to receive a callback for a job interview. Individuals with higher physical capacity were also found to have higher earnings.

violent and aggressive behavior (Andersson and Carlstedt 2003; Lindqvist and Vestman 2011).

B. *Sample Construction*

Our estimation sample was constructed by imposing the following restrictions. For the child generation, we excluded the small number of women (0.25 percent) who volunteered for the military. Next, for the parent generation, we excluded all individuals for whom male child was observed in our data (39 percent). This restriction implies that parents who have children at earlier ages are somewhat overrepresented in our sample, since no children born after 1979 were observed in our data. Parents with missing data on home municipality were then excluded (1 percent), and of the remaining parents, individuals in municipalities for which the algorithm was unable to assign reform status were also excluded (11 percent). Moreover, in order to avoid short-run adjustment effects of the reform as well as misclassification of individuals born right around the implementation cohorts, we exclude individuals belonging to the first reform cohort, and the cohorts immediately preceding and following it (14 percent). Finally, considering only children for whom at least one parent was observed in the sample, our estimation sample included 503,768 individuals in the child generation. For 405,845 of these, their mother was observed in the estimation sample, and for 326,600 their father was observed. The reason why the number of children for whom a father was observed is smaller than the number of children for whom a mother was 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, partly because Statistics Sweden has in some cases not been able to link individuals to their biological fathers.

In Table 1, we provide descriptive statistics for the estimation sample. Individuals in the parent generation were on average born in 1945, whereas the children were on average born in 1971. Since fathers are generally older, they had a somewhat lower probability of being exposed to the reform compared with mothers.

V. Results

A. *OLS Relationships*

In Table 2, we present OLS results on the relationship between parental education and sons' outcomes, based on equation (1). All estimates of parental schooling are statistically significant and are generally similar between mothers' and fathers' years of schooling. The strongest effects are found for 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. For fathers, one additional year of schooling is associated with 0.11 standard deviations higher cognitive ability and 0.06 standard deviations higher noncognitive ability.

Although the coefficients for our health and physical test variables are smaller in magnitude compared to the ones obtained for cognitive and noncognitive abilities, our results in most cases 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, as one year of parental schooling is associated with 0.2 percentage points lower probability of being obese. In contrast, and perhaps more unexpectedly, our data suggest a positive relationship between parental schooling and the incidence of hypertension.

B. *First Stage Results*

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 regression results for equation (2), which are presented in Table 3. In specification (A), we only include birth-cohort fixed effects, whereas specification (B) also controls for municipality fixed effects. In addition to these controls, specification (C) adds controls for county-by-year effects, whereas specification (D) instead adds controls for municipality-specific linear trends.

The simplest specification in column A suggests a very strong relationship between the reform and average years of schooling, with coefficients around 0.6 and F-values of about 2000 and 5000 for women and men, respectively. Both the coefficients and the F-statistics drop heavily when municipality factors are taken into account, however, as shown in column B, where coefficients are around 0.2 and F-values about 10. These are less convincing results as the rule of thumb (e.g., Staiger and Stock 1997) suggests that F-values should be at least 10 (and preferably higher) for the IV method to be valid.

In (C), we then add interactions between people's home county 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 behaviors are the same within each county. This may be reasonable, for example, if preferences, demographics or labor market conditions are similar within counties. F-statistics now increase to 30 and 40 and the coefficients on reform status have increased somewhat in magnitude.

It is possible that some unobserved factors also vary between municipalities within a county as well as over time. In specification (D), we therefore instead include municipality-specific linear trends. This implies a very large set of controls as there are more than one thousand municipalities in our data. The coefficients obtained are very similar to those in column C, and the results suggest that the reform on average increased mother's educational attainment by 0.25 years and father's educational attainment by 0.35 years. Both F-statistics are about 45, showing a strong effect of the reform. It is important to note that the reform predicts mothers' and fathers' education almost equally strongly, 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.[15]

C. *IV Results*

Having established that our first-stage is strong, we next turn to our 2SLS estimates, shown in Table 4. We again consider four sets of models corresponding to those for which the first stage was investigated above. First, in panel A, we report results from regressions where we only control for birth-year fixed effects, whereas in the models in panel B we then also control for municipality fixed effects. As can be seen, there are a large number of significant effects, particularly in panel A. For example, our estimates in both panel A and B suggest that parental education leads to a higher physical capacity and a higher cognitive ability, both for maternal and paternal education. Some coefficients have unexpected signs; for example, the results in panel A suggest negative effects on global health. Moreover, all the coefficient estimates in these panels suggest positive effects on the incidence of hypertension.

We now turn to our results in the models including larger sets of controls, which we expected to produce more reliable results, given that reform implementation may be correlated with factors that change over time within geographical regions or municipalities, and given that these models have stronger first stages than those in panel B. Beginning with the results in panel C, where county-by-year effects have been included, mother's education is found to significantly influence three out of our 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, are both economically and statistically stronger than those obtained in panel B, and are economically stronger than their OLS counterparts. The effects on more specific health outcomes, that is, physical capacity, obesity, and hypertension are now all insignificant and smaller in magnitudes. Cognitive ability is positively affected by mother's schooling, with an estimate that is almost identical to its OLS counterpart, but also to the result in panel B; one year of maternal schooling is associated with an 0.11 standard deviations higher cognitive ability. Whereas there is no evidence that mother's schooling would significantly 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 D, our preferred specification where municipality-specific trends have been included instead, are very

[15] Our first stage results are similar to those obtained by other studies using the Swedish compulsory school reform. For example, Holmlund (2008) finds coefficients of 0.20 and 0.28, and Holmlund et al. (2008) find coefficients of 0.26 and 0.33, for women and men respectively. Moreover, Meghir and Palme (2005) find coefficients of 0.34 and 0.25. Their study is somewhat different, however, in that only two birth cohorts (1948 and 1953) were used, and consequently municipality-specific trends have not been included.

similar to those in panel C. This is reassuring as quite different methods for dealing with time- and municipality-varying factors have been used. The effect on cognitive ability is even identical up to three decimal places in panel D compared with column C, whereas the effect on global height is somewhat lower in panel D and the one on height just somewhat higher. The effect on noncognitive ability has now become significant at the 10 percent level in panel D, suggesting that mother's schooling may play a role in shaping individual characteristics such as willingness to assume responsibility, independence and outgoing character. Again, all the significant effects are very similar in magnitude and amount to about 0.1 standard deviations, whereas the effects on more specific health outcomes are insignificant as well as small.

Compared with mother's education, there is much less evidence that father's education influences the outcomes of the child in panel C as well as in panel D. In fact, almost all the coefficients are smaller in magnitudes compared with those obtained for mother's schooling, and they are all statistically insignificant. Not only point estimates, but also the standard errors are in general smaller for the coefficients for father's education compared with mother's, showing that our insignificant results for father's education are not simply due to lack of power. Our results suggest a profound difference between the effect of mother's and father's education, irrespective of whether county-by-year effects or municipality trends are used.[16]

It should be noted that our results for global health, in the models with larger sets of controls, are much in line with those reported in Lundborg et al. (2011b), where father's education was insignificant whereas mother's education had a significant coefficient of 0.05 and 0.17 when applying the adoption and twin design, respectively. Our estimate for mother's education thus falls in between these estimates. In contrast, however, our results for cognitive ability suggest a much stronger influence of mother's schooling compared with the ones reported by Lundborg et al. (2011b) as our coefficients are more than twice as large. This may certainly reflect the fact that our instrument affected mothers with low levels of education. Interestingly, however, Lundborg et al. (2011b) also obtained evidence that father's schooling influenced cognitive ability, noncognitive ability and height, whereas there was no evidence of mother's schooling predicting noncognitive ability or height. While these differences may all reflect the fact that the estimates were identified for different populations in our respective studies, one should also note that the twin and adoption designs rely on a number of questionable assumptions. For example, the twin design assumes that any schooling differences within twin pairs will be unrelated to differences in non-genetic characteristics that may also affect the outcomes of the children of the twins. Similarly, the adoption design requires that any non-genetic

[16] Instead of using municipality-specific trends, one may also include year-of-implementation-specific trends to deal with the possibility of differential trends in treatment and control municipalities. Doing this yields almost identical results, both for mother's and father's education.

characteristics of the parents that may also directly affect the outcomes of the adopted children will be unrelated to parental schooling. If these assumptions fail, estimates may not reflect causal responses.

Although the outcomes under study were somewhat different, we can also compare our results with those of Carneiro et al. (2012), who found that one additional year of maternal schooling increased (white) children's performance in a math test at age of seven to eight by about 0.10 standard deviations and decreased their "behavioral problems index" (BPI) by 0.09 standard deviations. The results when only girls were included were even stronger, whereas no statistically significant effects for these outcomes were found when only including boys. While this may be due to the relatively small sample size, the coefficients were, in general, also smaller in magnitudes for boys than for girls. Again, one should note that the group affected by the instrument in their study probably differed from the group affected by our instrument, which may explain the difference in findings.

D. *Mechanisms*

In this subsection, we shed light on whether our findings in the previous subsection may be driven by mediators such as parental income, assortative mating, or fertility. While a full analysis of the role played by these potential mediators would require one instrument for each of them, we can investigate how these are affected by parental schooling, in specifications where parental schooling is instrumented by reform status. This will provide some clues as to whether they represent important mechanisms behind our results.

Our IV results for the potentially mediating outcomes are shown in Table 5.[17] First, in Model 1, we investigate effects on fertility by running regressions where the outcome variable is the parent's number of children.[18] We find that schooling has a significant and negative effect on the number of children among mothers, but not among fathers. The effect of mother's schooling, however, is quite small and amounts to less than 0.1 children per additional year of schooling. This small effect on family size suggests that the quantity-quality hypothesis of children is unlikely to explain our estimated effects of maternal schooling on the various child outcomes. In particular, an increase in mother's 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.

Next, in Model 2, we investigate the possibility that more highly educated individuals may have children later (or earlier). 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 have children later or earlier. We thus rule out the timing of having children as a potential mediator of the

[17] From now on, we focus on specifications including, birth-cohort fixed effects, municipality fixed effects, and municipality-specific linear trends. Using interactions between county and year of birth or year-of-introduction-specific trends yields similar results, however.

[18] This variable reflects the individual's total number of biological children as of 1999, and thus not only the ones included in our sample.

relationship between parental education and child outcomes.

In Model 3, we then examine the possibility that assortative mating is reflected in our estimates. Our results show that one additional year of maternal education, induced by the reform, 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. 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, as fathers’ reform-induced schooling did not affect any of the child outcomes under study, there is no particular evidence that the education of fathers would play an important role in mediating our results. For fathers exposed to the reform, we find that increased schooling is negatively related to their spouse’s education but the estimate is close to zero and not statistically significant.

Next, in Model 4, we investigate the potential effects of parents’ education on their incomes and labor supply. Our income data come from the 1980 tax records and are based on earnings from work and self-employment. While precision is rather low, our findings suggest 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. These findings are in line with Meghir and Palme (2005), who also found smaller and non-significant effects on incomes in the population of males.

In order to shed some more light on the magnitude of the potentially important income effects, and to render our results comparable with estimated income effects in previous studies, 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 father’s schooling on his own income or on family income. On the other hand, one year of mother’s schooling is found to increase her own income by SEK 2,638 on average and the family’s income by SEK 3,791, the latter being equivalent to $1,026 in year 2000 US dollars.[19]

These findings for family income may be related to Dahl and Lochner (2012), Duncan et al. (2011) and Milligan and Stabile (2011), 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. They also documented evidence of somewhat stronger effects on boys, and that their results were mostly concentrated to children for which the

[19] 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.

mother had no more education than high school. The study by Duncan et al. (2011) reported similar results. Milligan and Stabile (2011) found no significant effect of family income on outcomes such as math test scores, vocabulary test scores, or height when considering the full sample. On the other hand, restricting their sample to individuals for whom the mother had no more education than high school, they found that math scores were significantly increased by 0.07 standard deviations per year of maternal schooling, height was increased by 0.04 standard deviations, and aggression scores were reduced by 0.10 to 0.15 standard deviations per $1,000 (year 2004 Canadian dollars) of family income; for math scores and height, these results were stronger for boys than for girls. These findings, together with ours, seem to suggest that the income gains that followed from the increase in mothers' schooling may be an important explanation for the effect of mothers' schooling on their sons' height, cognitive ability, and noncognitive ability.

One could also ask whether the increase in education that followed from the reform increased labor supply. If so, the effect of increased education would theoretically be more ambiguous, since any positive effects from greater economic resources and improved knowledge could be offset by less time spent at home by better educated parents. Since we do not have access to data on hours worked, we are restricted to examining the probability of parents having larger than zero incomes. Our results, shown in Model 7, provide some evidence that labor supply increased by 1.4 percentage points for mothers but the estimate is not significant. The estimate for fathers is also insignificant and very close to zero. This is less surprising given the higher labor market attachment of males.[20]

Finally, we investigate whether the reform affected the incidence of having more than nine years of schooling. Although the reform certainly did not force any individual to stay in school for more than nine years, obtaining nine years of education could, for example, affect the individual's preferences for schooling and thus the decision to invest even more in education. Or, as noted by Holmlund (2008), the pre-reform tracking system may have put some talented children at a disadvantage, whereas the reform instead pushed these children further up the educational ladder. We investigate such spill-over effects by estimating equation (2) with dummies indicating *more than* nine years of schooling, that is, education beyond the compulsory level. For comparison, we also estimate the same equation with dummies indicating *at least* nine years of education, that is, the legal minimum level of schooling after the reform was introduced. The results are shown in Table 6. We find that the reform led to an increased attendance in post-compulsory levels of education among mothers by about two percentage points. For fathers, this effect was weaker and amounted to about one percentage point. Still, these are substantial spill-over effects for both mothers and fathers given the relatively

[20] For fathers, the incidence of having positive earnings is 99 percent, whereas for mothers the corresponding figure is about 91 percent.

small share of individuals who were affected by the reforms to any extent at all; we find that the reform increased the incidence of obtaining at least nine years of education by about ten percentage points for mothers and 16 percentage points for fathers.[21,22]

Summing up, our analyses in this subsection reveal an interesting pattern; for men, the increase in schooling that was generated through the reform seems to have affected their lives to a much lesser extent than the corresponding increase among women. For women, the increase in schooling raised incomes, reduced fertility and led to higher quality of the spouse as well as to investments in further education beyond elementary school. For men, the effects were limited to obtaining somewhat more schooling beyond the compulsory level. In the light of this, it seems less surprising that the children of these men were unaffected by their father's schooling. One interpretation is that most of those males who were actually able to benefit from the increase in schooling were continuing beyond the compulsory level already before the reform was implemented. For mothers, our results may suggest that the pre-reform system held back some high-ability women, who after the reform were able realize their potential to a greater extent.

E. *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 potentially problematic city municipalities of Stockholm, Göteborg and Malmö, for whom compulsory schooling amounted to eight years rather than seven before the reform was implemented, and for whom measurement errors may be larger since the reform was introduced in different school districts at different times. As shown in panel A in Table 7, the results have not changed much. Compared with our main specification, the effect of mother’s education on noncognitive ability has become somewhat smaller and is insignificant at the 10 percent level. Instead, there is some evidence that father’s education may have a negative impact on child’s height, although it is only significant at the 10 percent level.

Second, we consider what happens when only parents born in 1943 or later

[21] Some previous studies that also exploited reforms where compulsory schooling was raised to nine years, such as Black et al. (2005), restricted the sample to individuals with at most nine years of schooling, with the aim of improving the precision of the IV estimates. The idea is that the reform had the strongest bite on those at the lower end of the education distribution. This sample restriction assumes, however, that whereas some people increased their level of schooling from seven or eight to nine years as a consequence of being exposed to the reform, none of the individuals who in the pre-reform period would have stayed for only seven years decided to stay for more than nine years after the reform. However, as our results suggest exactly the existence of spill-over effects and as we already have sufficient precision in our IV estimates, we prefer not to impose such a restriction.

[22] We have also tried indicators for even higher levels of education, such as “extensive high school” (12 years of schooling or more) or university levels, but failed to find significant effects in these cases, suggesting that the spill-over effects of the reforms may have been limited to shorter programs, which is the Swedish context typically means less academic ones.

are included. We impose this restriction since home municipality in 1960, which we use 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 in Table 7, show little differences from our main results, however.

Finally, we note that there is a small group of children in our data who belonged to birth cohorts not yet exposed to the reform in their 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 child individuals born up until 1959. The results are reported in panel C in Table 7. Again, results are virtually unchanged.

VI. Conclusion

Based on a comprehensive dataset of Swedish males, and using the Swedish compulsory school reform as a source of exogenous variation in parental schooling, this study contributed to the scant literature on the effects of parental education on offspring's health and skills. 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 and global health are both about the same as the one on cognitive ability.

Whereas the reform had equally strong effects on mother's and father's schooling, there is little evidence that father's schooling would improve children's health or abilities. This result was obtained although the reform affected father' schooling to the same degree as mother's schooling. Our findings are robust to a number of sensitivity checks, and there is little evidence that our results were driven by mechanisms such as changes in fertility patterns or the timing of having children. Instead, our results point to the importance of the gain in income that followed from the increase in maternal schooling. These large income gains were obtained for the mothers who increased their schooling in response to the reform but no such gain was obtained among the fathers. This pattern may thus explain some of the large differences between mothers and fathers in terms of the effect of their schooling on their sons' outcomes. The 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. There is plenty of evidence, albeit mostly from developing countries, that women value outcomes such as child health more than men do (Behrman 1997).

Although our results for fathers do not provide any evidence that their education matters in terms of their offspring outcomes, it should be noted that this conclusion is only valid for the fathers affected by the reform. On the other hand, this result seems to be consistent with many previous findings where different identification strategies are used, casting some doubt on the potential effect of father's education on children's outcomes.

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TABLES

TABLE 1: DESCRIPTIVE STATISTICS

	Observations	Mean	Std. dev.
Year of birth	503,768	1971.37	4.70
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	1946.12	4.28
Mother exposed to reform	405,845	0.20	-
Father's education	326,600	10.80	2.96
Father's year of birth	326,600	1945.27	3.79
Father exposed to reform	326,600	0.14	-

Note: The table shows summary statistics before the normalization of non-binary outcome variables.

TABLE 2: OLS RESULTS

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

Notes: Standard errors in parentheses. * indicates 10 percent significance, ** 5 percent significance, and *** 1 percent significance. Dummies for parent's year of birth, home municipality 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 that are clustered at the municipality level.

TABLE 3: FIRST STAGE

	Dependent variable: Education (A)	(B)	(C)	(D)
Mother				
Exposed to reform	0.556***	0.203***	0.221***	0.253***
	(0.013)	(0.059)	(0.034)	(0.038)
F-statistic	1793.7	12.0	41.3	44.2
N	405,845	405,845	405,845	405,845
Father				
Exposed to reform	0.660***	0.237***	0.254***	0.354***
	(0.016)	(0.080)	(0.048)	(0.052)
F-statistic	4761.0	8.9	28.4	46.8
N	326,600	326,600	326,600	326,600
Birth cohort fixed effects	YES	YES	YES	YES
Municipality fixed effects	NO	YES	YES	YES
County-by-year fixed effects	NO	NO	YES	NO
Municipality-specific trends	NO	NO	NO	YES

Notes: Standard errors in parentheses. * indicates 10 percent significance, ** 5 percent significance, and *** 1 percent significance. Each estimate represents the coefficient from a different regression. Regressions are run using robust standard errors, which in Model B-D are clustered at the municipality level.

TABLE 4: IV RESULTS

	(1) Global health	(2) Height	(3) Physical capacity	(4) Obesity	(5) Hyper-tension	(6) Cognitive ability	(7) Noncognit ive ability
A. *Only controlling for birth year fixed effects*							
Mother's	-0.029***	0.013	0.111***	-0.002	0.027***	0.048***	0.023**
schooling	(0.010)	(0.010)	(0.011)	(0.002)	(0.004)	(0.010)	(0.011)
Father's	-0.037***	0.017*	0.091***	-0.001	0.020***	0.018*	0.005
schooling	(0.011)	(0.010)	(0.011)	(0.002)	(0.004)	(0.010)	(0.011)
B. *Only controlling for birth year and municipality fixed effects*							
Mother's	0.083*	0.047	0.167**	-0.007	0.096**	0.089**	-0.014
schooling	(0.048)	(0.040)	(0.068)	(0.007)	(0.044)	(0.044)	(0.050)
Father's	0.002	-0.025	0.143*	-0.005	0.058*	-0.022	-0.051
schooling	(0.047)	(0.045)	(0.077)	(0.007)	(0.033)	(0.052)	(0.056)
C. *Controlling for birth year fixed effects and municipality fixed effects, and interactions between birth year and home county*							
Mother's	0.139***	0.080*	0.055	-0.010	-0.002	0.106**	0.048
schooling	(0.051)	(0.045)	(0.043)	(0.007)	(0.018)	(0.042)	(0.047)
Father's	0.008	-0.034	0.058	-0.001	0.026	0.001	0.016
schooling	(0.044)	(0.044)	(0.048)	(0.006)	(0.016)	(0.044)	(0.047)
D. *Controlling for birth year fixed effects and municipality fixed effects, and municipality-specific trends*							
Mother's	0.103**	0.089**	0.023	-0.007	0.023	0.106***	0.076*
schooling	(0.046)	(0.042)	(0.044)	(0.007)	(0.018)	(0.038)	(0.045)
Father's	0.025	-0.055	0.048	-0.002	0.018	-0.037	0.032
schooling	(0.041)	(0.037)	(0.043)	(0.006)	(0.014)	(0.042)	(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: Standard errors in parentheses. * indicates 10 percent significance, ** 5 percent significance, and *** 1 percent significance. Each estimate represents the coefficient from a different regression. Regressions are run in STATA 12 using the ivregress command and the xtivreg2 command (Schaffer 2010). Regressions are run using robust standard errors, which in the models in panel B-D are clustered at the municipality level.

TABLE 5: MECHANISMS

	(1) Number of children	(2) Child's year of birth	(3) The other parent's education	(4) Log income	(5) Income (expressed in SEK)	(6) Total income of the couple (SEK)	(7) Positive income
Mother							
Years of	-0.100**	-0.006	0.5111***	0.139*	2,638.012***	3,790.901***	0.014
schooling	(0.051)	(0.164)	(0.138)	(0.080)	(1,004.579)	(1465.171)	(0.013)
N	305,811	405,845	277,010	368,967	405,845	405,845	405,845
Father							
Years of	-0.020	-0.023	-0.062	0.014	971.425	709.897	-0.000
schooling	(0.042)	(0.117)	(0.140)	(0.024)	(1,138.709)	(1,779.692)	(0.004)
N	249,273	326,600	292,660	322,535	326,365	326,365	326,365

Notes: Standard errors in parentheses. * indicates 10 percent significance, ** 5 percent significance, and *** 1 percent significance. Dummies for year of birth and home municipality in 1960, and municipality-specific linear trends, have been included. Each estimate represents the coefficient from a different regression. Regressions are run in STATA 12 using the xtivreg2 command (Schaffer 2010) with robust standard errors that are clustered at the municipality level. In model 1, parents are included only once rather than once for each child. As individuals may pair up with others that are not in our estimation sample, sample restrictions have not been imposed for 'the other parent' in Model 3. Even more generally, in Model 7 we have been able to include the income of the other parent irrespective of whether or not this individual himself (or herself) is in our dataset.

TABLE 6: NINE YEARS OF SCHOOLING AND SPILL-OVER EFFECTS

	At least 9 years of schooling	More than 9 years of schooling
Mother		
Exposed to reform	0.105***	0.018***
	(0.007)	(0.006)
N	405,845	405,845
Father		
Exposed to reform	0.158***	0.014*
	(0.012)	(0.007)
N	326,600	326,600

Notes: Standard errors in parentheses. * indicates 10 percent significance, ** 5 percent significance, and *** 1 percent significance. Dummies for year of birth and home municipality 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 that are clustered at the municipality level.

TABLE 7: SENSITIVITY ANALYSIS

	(1) Global health	(2) Height	(3) Physical capacity	(4) Obesity	(5) Hyper-tension	(6) Cognitive ability	(7) Noncognitive ability
A. *Excluding the municipalities of Stockholm, Göteborg, and Malmö*							
Mother							
Years of	0.085**	0.087**	0.037	-0.008	0.021	0.109***	0.061
schooling	(0.040)	(0.039)	(0.040)	(0.006)	(0.016)	(0.036)	(0.039)
N	362,763	349,407	327,086	349,385	344,819	351,007	333,730
Father							
Years of	0.034	-0.058*	0.022	-0.003	0.018	-0.013	0.0304
schooling	(0.037)	(0.033)	(0.037)	(0.005)	(0.013)	(0.035)	(0.037)
N	292,701	281,415	259,377	281,405	277,281	282,678	267,364
B. *Excluding parent individuals born prior to 1943*							
Mother							
Years of	0.090*	0.090*	-0.008	-0.005	0.035	0.103**	0.093*
schooling	(0.054)	(0.051)	(0.052)	(0.008)	(0.022)	(0.044)	(0.052)
N	311,800	299,685	276,680	299,677	294,911	301,029	285,035
Father							
Years of	0.011	-0.036	0.010	-0.001	0.020	-0.007	0.025
schooling	(0.051)	(0.048)	(0.052)	(0.008)	(0.017)	(0.044)	(0.048)
N	237,336	227,811	206,193	227,808	223,843	228,733	215,381
C. *Excluding child individuals born up to 1959*							
Mother							
Years of	0.100**	0.090**	0.022	-0.007	0.025	0.106***	0.073
schooling	(0.046)	(0.043)	(0.044)	(0.007)	(0.018)	(0.039)	(0.045)
N	400,379	385,673	361,006	385,655	380,090	387,440	368,854
Father							
Years of	-0.026	-0.055	0.051	-0.002	0.018	-0.037	0.033
schooling	(0.041)	(0.036)	(0.042)	(0.006)	(0.014)	(0.042)	(0.043)
N	324,846	312,448	288,239	312,438	307,391	313,779	297,351

Notes: Standard errors in parentheses. * indicates 10 percent significance, ** 5 percent significance, and *** 1 percent significance. Dummies for parent's year of birth and home municipality in 1960, and municipality-specific linear trends, have been included. Each estimate represents the coefficient from a different regression. Regressions are run in STATA 12 using the xtivreg2 command (Schaffer 2010) with robust standard errors that are clustered at the municipality level. The first stage in panel A produced a coefficient of 0.293 and an F-value of 73.8 for mothers and a coefficient of 0.424 and an F-value of 94.5 for fathers; in panel B the first stage produced a coefficient of 0.248 and an F-value of 38.9 for mothers and a coefficient of 0.337 and an F-value of 39.0 for fathers; in panel C the first stage produced a coefficient of 0.252 and an F-value of 44.3 for mothers and a coefficient of 0.3545 and an F-value of 47.3 for fathers.