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Education policy and early fertility: Lessons from an expansion of upper secondary schooling

Hans Grönqvist and Caroline Hall

Uppsala Center for Labor Studies
Department of Economics
Uppsala University
P.O. Box 513
SE-751 20 Uppsala
Sweden
Fax: +46 18 471 14 78

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Education policy and early fertility: Lessons from an expansion of upper secondary schooling[*]

by

Hans Grönqvist[**] and Caroline Hall[♦]

November 24, 2011

Abstract

This paper studies effects of education policy on early fertility. We study a major educational reform in Sweden in which vocational tracks in upper secondary school were prolonged from two to three years and the curricula were made more academic. Our identification strategy takes advantage of cross-regional and cross-time variation in the implementation of a pilot scheme preceding the reform in which several municipalities evaluated the new policy. The empirical analysis draws on rich population micro data. We find that women who enrolled in the new program were significantly less likely to give birth early in life and that this effect is driven by women with higher opportunity costs of child rearing. There is however no statistically significant effect on men's fertility decisions. Our results suggest that the social benefits of changes in education policy may extend beyond those usually claimed.

Keywords: Schooling reform; teenage childbearing; fertility
JEL-codes: J13, I20

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[**] Swedish Institute for Social Research (SOFI), Stockholm University; e-mail: hans.gronqvist@sofi.su.se
[♦] Institute for Labor Market Policy Evaluation (IFAU) and Uppsala Center for Labor Studies (UCLS); e-mail: caroline.hall@ifau.uu.se

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1 Introduction

It is well known that women who give birth in their teens experience substantially worse long-run social and economic outcomes.[1] This is also true for their children. Given these facts it is hardly surprising that a strong policy debate has emerged in many countries over which policies are most successful in combating high teenage birth rates. The efficiency of various types of family planning policies has been documented in previous studies (e.g. Kearney and Levine 2009; Grönqvist 2009). It has also been proposed that fertility decisions are heavily influenced by education policy. This idea follows directly from the many descriptive studies which show that highly educated women tend to postpone childbearing for a considerable period of time (e.g. Maynard 1996). There are also strong theoretical arguments for why education policy could lead to delayed childbearing. First, educational investments are likely to alter an individual's cost/benefit calculation regarding sexual activity and contraception; for instance, by providing formal knowledge or by strengthening the proficiency to process and value information (Michael 1973). Higher education also increases expected future human capital, which could influence fertility decisions through higher opportunity cost of child rearing (cf. Becker 1991). Moreover, being enrolled in education may mechanically prevent individuals from having children since it restricts the time available for taking care of them.

Regardless of the underlying mechanisms, the question of whether education policy can be used as a tool to prevent teenage childbearing obviously hinges on whether or not education is causally linked to fertility. An individual's choice of schooling is not random and it is easy to imagine factors that may give rise to a spurious relationship. For instance, individuals with high career aspirations or more patience may be more likely to enroll in higher education and at the same time more inclined to postpone childbearing.

[1] Although the evidence is not undisputed it is supported by several compelling studies e.g. Ashcraft and Lang (2007); Geronimus and Korenman (1992); Hotz, Mullins and Sanders (1997); Klepinger, Lundberg and Plotnick (1999); Kearney and Levine (2007). Holmlund (2005) provides the most convincing evidence for Sweden by exploiting variation in the timing of childbearing across pairs of sisters.

The aim of this paper is to investigate the effect of education policy on early fertility. We study a major educational reform in Sweden occurring in the beginning of the 1990s in which vocational tracks in upper secondary school were prolonged from two to three years and the academic content of the curricula was increased substantially. The changes made students graduating from vocational tracks eligible to apply to university. Hall (2012) finds that this reform led to increased educational attainment among vocational students. Our identification strategy takes advantage of cross-regional and cross-time variation in the implementation of a pilot scheme preceding the reform in which several municipalities evaluated the new policy. This institutional feature creates a source of plausibly exogenous variation in access to the new tracks.

Our empirical analysis draws on data from administrative registers covering the universe of the Swedish working-age population observed annually from 1985 through 2007. The dataset includes a large set of standard individual characteristics (age, marital status, income, place of residence etc.) as well as detailed information of each individual's entire educational history, ranging from compulsory school through university. It also contains a link between children and their biological parents.

As already mentioned, several studies have documented a strong negative association between acquired levels of schooling and early fertility (e.g. Maynard 1996). Only a couple of papers have however studied shifts in education policies that are likely to be unrelated to other determinants of fertility. McCrary and Royer (2011) examine US school entry policies but find no significant effect of maternal education on early childbearing. Black, Devereux and Salvanes (2008) investigate the consequences of changes in compulsory schooling laws in the US and in Norway. Their results show that women who obtained more education as a consequence of the law changes had fewer teenage births.[2]

Our work makes several novel contributions to the literature. The main innovation is that we consider the effect of education policy along a different margin of the educational distribution: the upper secondary level. Little is known about the returns to education along this margin. Recent research however indicates that the financial return to

[2] Instead of early fertility Currie and Moretti (2003) study the effect of education on total fertility in the US and find that exogenous increases in education indeed lower fertility. It is possible that this effect is driven by postponed childbearing.

education is particular high for this level of education (Aakvik, Salvanes and Vaage 2010). There is also evidence that the non-monetary return to education is higher further up in the educational distribution. The results in Lochner and Moretti (2004) indicate that the effect of schooling on incarceration is more strongly negative at the upper secondary level. These findings suggest that the response in fertility to changes in education policy could actually be stronger once we move beyond the compulsory level.

Another major contribution of our paper is that it is the first to consider both women's and men's fertility decisions in a developed country.[3] This is especially important considering that women by tradition exercise greater control over fertility (e.g. by regulating the use of contraceptives or deciding on whether or not to abort an unwanted pregnancy) and that education policy therefore may have less capacity to influence men. Understanding whether the potential social benefits of changes in education policy are confined only to women is of course key knowledge when trying to assess the cost-effectiveness of different educational reforms.

Last, since we have access to measures of individual ability (proxied by compulsory school GPA) and parental socioeconomic background we are able to investigate possible differential effects of the policy with respect to the opportunity costs of child rearing. Although economic theory clearly suggests that individuals facing distinct opportunity costs make different fertility decisions, data limitations has prevented previous studies to examine such heterogeneous responses.

In the empirical analysis we investigate to what extent an individual's timing of childbearing is affected by his/her choice of upper secondary schooling track. Since the choice of track is likely to be endogenous we instrument for the length of the track using the share of the available vocational tracks in the municipality which constituted the new 3-year tracks. The results show that women who enrolled in the 3-year tracks were significantly less likely to give birth by age 20 and that this effect is driven by women with highly educated parents and women with high ability. These findings are consistent with the idea that individuals with higher opportunity costs of child rearing

[3] Breierova and Duflo (2002) examine the consequences of a school construction program in Indonesia in the 1970s. The results show that female education is a stronger determinant of early childbearing than male education. Osili and Long (2008) represents another study for a developing country which tries to account for endogenous schooling decisions.

postpone the decision to have kids. There is however no significant effect on men's fertility.

With these baseline results in mind we next set out to look at some of the channels through which education may affect early childbearing. After conditioning on upper secondary school graduation, we conclude that the absence of effects for some groups of students does not seem to be explained by an increased dropout rate. We next investigate how the reform affected the probability of having one's first birth at different ages. This analysis indicate that the negative effect on the probability of having the first child by age 20 for some groups of women could solely be due to an incapacitation effect, i.e. that prolonged education reduces the time and opportunities to have children while in school (cf. Black, Devereux, Salvanes 2008). Last, we find no evidence that the reform affected total fertility as measured at age 32.

The remainder of this paper unfolds as follows. Section 2 gives the institutional background to the educational reform as well as outlines our empirical strategy. It also provides details about Sweden's family policy. Section 3 presents the data. The results are provided in Section 4 and Section 5 concludes.

2 Background

2.1 The reform[4]

In Sweden, all individuals who have completed nine years of compulsory schooling are entitled to upper secondary education.[5] Schooling at the upper secondary level is voluntary although the enrollment rate for each cohort has in recent years been as high as 97 percent. Upper secondary school comprises several different educational tracks to which individuals apply based on their compulsory school GPA. Students generally attend a school in their municipality of residence, but if the track they desire to follow is not offered they can instead choose to attend in a nearby municipality.

[4] This section draws heavily on Hall (2012) to which we refer to for further details.

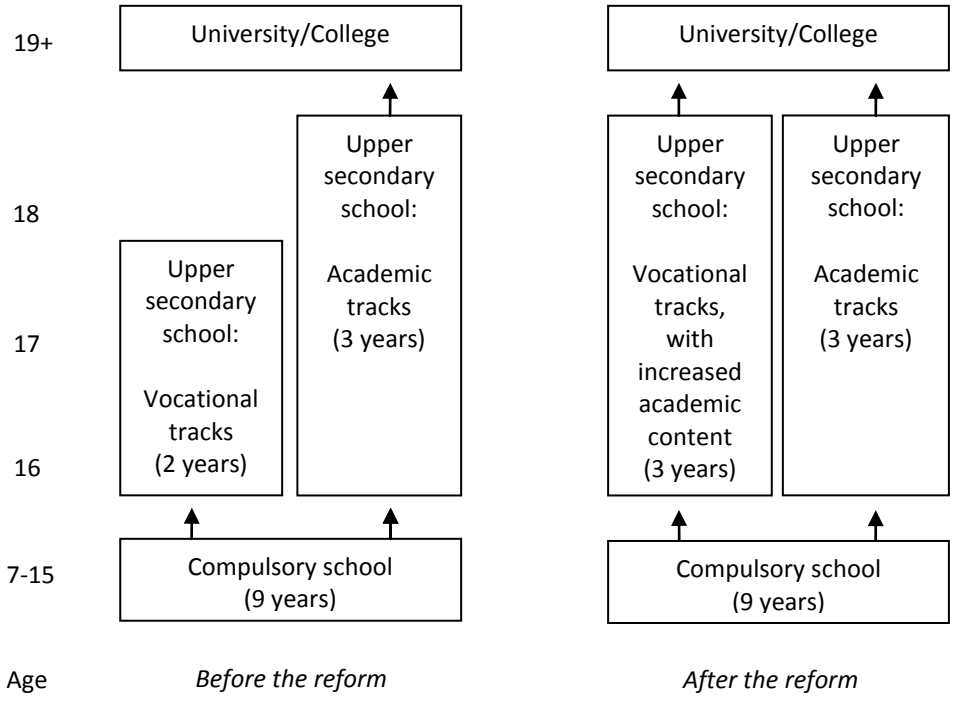
[5] Individuals who are older than 20 when they begin upper secondary education are not entitled to attend a general upper secondary school, but instead enter the adult education system. Within this system, both those who lack any upper secondary education and those who dropped out before graduating can finalize a degree. It is also possible to supplement e.g. a 2-year upper secondary degree in order to obtain a 3-year degree. For more information on the adult education system, see Stenberg (2009).

In 1991 the Parliament decided on a major reform of upper secondary education. Before the reform, upper secondary education consisted of a few academic and several vocational tracks. The vocational tracks were two years long and consisted mainly of vocational training. The academic tracks typically lasted three years and prepared the students for higher education. In the years leading up to the reform, around 45 percent of the upper secondary school students were enrolled in vocational tracks. Construction, electrical engineering and caring services represented some of the most common tracks. Compared to the academic students, the group of vocational students was negatively selected in terms of compulsory school GPA and parents' educational background.[6]

The general aim of the reform was to bring about a higher quality of education as well as to increase the flexibility of the upper secondary school system. The largest changes concerned the vocational tracks, which through the inclusion of several general theoretical subjects in the curriculum received a considerably higher academic content. The length of the vocational tracks was also extended from two to three years. These changes were motivated by the view that there was an increasing need for a broader education in working life as well as by the desire to enable everyone to continue to university studies. As a result of the reform, all students graduating from a vocational track attained basic eligibility[7] for university studies. Figure 1 illustrates the Swedish school system before and after the reform.

[6] Own calculations based on the Upper Secondary School Application Record for 1986-1990.
[7] Note that 'basic eligibility' does not mean eligibility to all university studies as some programs have special requirements.

Figure 1: The Swedish school system before and after the reform



The reform was preceded by a nation-wide pilot period between 1988 and 1993 in which new 3-year vocational tracks were tried out in several municipalities.[8] The vocational tracks in the pilot had increased academic content compared to the regular 2-year tracks. While Swedish was the only general theoretical subject included in all 2-year tracks, the pilot tracks also contained English, Social Studies and an elective course. Math appears to be by far the most common choice of elective.[9] Another difference between the 2- and 3-year tracks was that the latter located a larger share of the vocational training in workplaces rather than in schools.[10]

The pilot scheme contained 6,000 educational slots in 1988, 10,000 in 1989, and 11,200 in 1990. This represented approximately 11-20 percent of the total number of

[8] This extensive pilot scheme was the outcome of a thorough evaluation of the vocational upper secondary education conducted by a government appointed committee (ÖGY). See e.g. Prop. 1987/88:102 for a description of the pilot scheme.
[9] The National Board of Education (1990a) reports that 86 percent of the students in 1988 chose to study Math.
[10] Compared to the pilot tracks, the 3-year tracks that were implemented after the 1991 reform contained even more academic subjects and somewhat less training in workplaces.

slots in vocational tracks. A class in the pilot scheme would always replace a class in a corresponding 2-year track, implying that the total number of slots in vocational tracks was not expanded. On top of this, in 1987 there was a very limited pre-pilot scheme only including 500 educational slots.[11]

The National Board of Education was responsible for allocating the pilot scheme among the different vocational tracks, as well as for deciding the municipalities in which it was to be located. The allocation of slots among the different tracks was done primarily on the basis of proportionality; the goal was that each track would receive the same share of 3-year slots as they received of 2-year slots. There were however some deviations from this principle, e.g. tracks with a smaller number of slots were somewhat overrepresented. The allocation decision was further restricted by the fact that in the beginning of the pilot period no curricula had yet been prepared for some of the 3-year tracks. This meant that all vocational tracks could not be included in the pilot scheme the first years. Appendix A.1 shows which tracks were included each year as well as each track's number of available and share vacant slots.[12]

The government stipulated that the pilot scheme be distributed among regions with different industry and population structures. There should also be variation regarding the extent to which different regions participated: in some regions, all or a large share of the vocational tracks should be converted to 3-year tracks, while in other regions only a few of the tracks should be prolonged. The motive behind these requirements was to get an idea of how the more extensive workplace training worked in different types of labor markets, as well as of the strain on the local labor market if it was implemented on a large scale. On top of these criteria, the National Board of Education tried to assess whether the local labor market would be able to arrange the extended workplace training in a relatively short time. To judge this they relied upon recommendations from employer and union representatives in different sectors. The initiative to participate always came from the municipalities themselves as they had to apply in order to be con-

[11] The 1987 tracks were somewhat different as they did not contain more extensive workplace training. The description of the implementation process below refers to the actual pilot scheme, as the implementation of the pre-pilot scheme has not been documented.

[12] The share of vacant slots was in general somewhat lower for the 3-year than for the 2-year vocational tracks. Table A.2 describes the mapping of old to new tracks.

sidered. The interest to participate was large; each year the demand for places in the pilot by far exceeded the number of available slots.

Sweden had 284 municipalities during this time period. Only about 68 percent of them offered vocational tracks. Students residing in the other municipalities hence had to attend school in a nearby municipality if they wanted to obtain a vocational degree. When the pilot scheme was introduced in 1988, about 40 percent of the municipalities were granted participation. In 1990, this share had increased to about 52 percent. The extent to which the municipalities participated also increased each year, as more tracks were included in municipalities that already participated.

All through the pilot period most participating municipalities came to offer both 2- and 3-year vocational tracks. Sometimes the exact same track was offered in both lengths within the same municipality. Even in municipalities only offering either 2- or 3-year tracks, students could in some cases have a choice of program length if a nearby municipality offered tracks of a different length.

2.1.1 Exploiting the pilot scheme as a policy experiment

To understand our empirical strategy consider the following baseline specification

$$\text{Early childbearing}_{ijs} = \gamma \text{Edu}_{ijs} + \theta' X_i + \kappa_j + \lambda_s + \varepsilon_{ijs} \qquad (1)$$

where *i* indexes individual, *j* municipality of residence, and *s* upper secondary school starting year; *Early childbearing*$_{ijs}$ is an indicator equal to one if individual *i* experienced his/her first birth by a given age and zero otherwise; *Edu*$_{ijs}$ is a dummy which takes the value one if the individual chose to attend a 3-year vocational track, and zero if he/she attended a 2-year track; X_i is a vector of individual and family characteristics (compulsory school GPA, sex, age at enrolment, immigrant background, each parent's highest education level and whether both of the parents have immigrant background[13]); κ_j and λ_s represent municipality of residence and upper secondary school starting year fixed effects; ε_{ijs} is an error term. The parameter of interest is γ which ideally gives the causal effect of attending the longer and more academically oriented vocational track.

[13] The background characteristics are displayed in Table 1.

Even though the model includes a rich set of covariates as well as controls for ability, as measured by compulsory school GPA, one could still be concerned that standard OLS estimates may be biased due to non-random selection into education. There are potentially a large number of unobserved factors included in ε_{ijs} which could be correlated with an individual's choice of track. For instance, individuals with high career aspirations or low discount rates may be more likely both to invest in education and to reduce fertility. It is also possible that investments in education and family are determined simultaneously.

To account for endogenous schooling choices we exploit variation across regions over time in the implementation of the pilot scheme which preceded the reform. As previously mentioned, the pilot scheme gave some students the choice of attending a 3-year rather than a regular 2-year vocational track. The extent to which a person had this choice depended jointly on: (i) which year the student finished compulsory school, (ii) the student's municipality of residence. We argue that this plausibly exogenous variation, conditional on upper secondary school starting year and municipality of residence, is a valid instrument for the length of the chosen track.[14]

More specifically, our instrument is the degree to which the individual's municipality of residence participated in the pilot scheme by the time he or she began upper secondary school, as measured by the share of the available vocational tracks which constituted 3-year track[15], i.e. $N^{-1}\sum_{l}^{N} 1\{Track_l > 2 year\}$ [16]. Assuming that the instrument is uncorrelated with any unobserved variables affecting the conditional outcomes of interest, and that it had no direct effect on the outcomes other than through influencing whether the person attended a 2- or a 3-year track, an instrumental variables (IV) estimator of γ is consistent. In the empirical analysis we provide several pieces of evidence which supports the validity of these assumptions. If the effect of attending the 3-year track varies across individuals, the IV estimate should be interpreted as the effect for

[14] This identification strategy has previously been used by Hall (2012) to investigate the effect of the reform on educational attainment and earnings. Similar strategies have also been used in other studies; see e.g. Duflo (2001).

[15] Ideally, the instrument would be measured as the share of *slots* in vocational tracks which represented 3-year tracks. However, such data are not available at the municipality level

[16] N is the number of available vocational tracks. The instrument is zero for municipalities not offering any vocational tracks.

individuals who on the margin are induced to select the new tracks because of the pilot scheme and the margin that varies with the instrument (e.g. Björklund and Moffitt 1987; Heckman and Vytlacil 2005).

It is important to note that the design of the pilot scheme generates a setting where some students were given *the choice* of attending a 3-year rather than an ordinary 2-year vocational track. Because individuals are allowed to drop out of school, the parameter we estimate is thus not necessarily the same as in studies on compulsory schooling reforms where individuals are forced to stay in school. However, as long as countries decide to keep their higher education voluntary, we believe that this parameter is relevant for public policy. We discuss effects of the reform on the risk of dropping out in Section 4.3. It is also worth mentioning that just as for studies exploring compulsory schooling reforms our estimates reflect the combined effect of prolonging education as well as of making it more academic.

2.2 Sweden's family policy

Sweden is well-known for its extensive welfare state which encompasses a number of measures to assist children and their parents (Björklund 2006). Child care is heavily subsidized and local governments are obliged to provide care to cover the time the parents spend on market work and education. There is also a flat rate child allowance. The Swedish parental leave system is earnings related, and individuals that had low earnings prior to the birth of the child receive lower benefits for the entire parental-leave period. In the early 1990s, the replacement rate amounted to 90 percent of the previous earnings. Individuals that had not been working before the birth (i.e. students enrolled in upper secondary education) were entitled to the minimum replacement rate of about 8 EUR/day (in current prices) for a maximum of 15 months (RFV 2002).

As in most western countries, the fertility rate has gradually declined over the past decades and is currently below the replacement level. In 1997, the fertility rate reached an average of 1.52 children per woman (Andersson 1999). This development has coincided with a general shift towards postponed childbearing. The vast majority of Swedish women are cohabiting at the time of their first birth and marriage rates have been declining for several decades. Between the years 1993 and 2003 the average age at first marriage increased from 28.3 to 31.3 (Statistics Sweden 2006).

Abortions have been allowed basically on demand and free of charge since 1975 and the abortion rate is fairly high in comparison with other western countries (Santow and Bracher 1999). The birth control pill was introduced in 1964 and has since 1989 been heavily subsidized. The pill is the most common contraceptive method among young women and about 60 percent of Swedish women age 18–24 regularly use oral contraceptives. Sex education has been part of the national curriculum in compulsory school since 1956 and includes information on contraception as well as sexually transmitted diseases (Edgardh 2002).

3 Data and sample selections

The analysis exploits rich micro data covering the entire Swedish working-age population during the period 1985–2007.[17] The database includes a large set of standard individual characteristics (age, marital status, incomes, place of residence etc.) as well as detailed information of an individual's entire educational history, ranging from compulsory school through university. It also contains a link between children and their biological parents. This is advantageous since it minimizes the problem of poor child-parent match rates inherited in many previous studies. Another benefit of using register data is that it decreases measurement error in education.

We used the Upper Secondary School Application Record to obtain information on when and where an individual began upper secondary school as well as what track (type and length) he/she started. The register is used to construct the sample of individuals and to acquire information on which educational tracks each municipality offered each year. Based on this information we then determined which municipalities participated in the pilot scheme each year and the share of the available vocational tracks which constituted 3-year tracks.[18]

Our main sample consists of all individuals who completed compulsory school during 1986–1990 and thereafter continued directly to upper secondary school. Only indi-

[17] The data originates from Statistics Sweden but was obtained via IFAU.
[18] The tracks in the 1987 'pre-pilot scheme' are also categorized as part of the pilot scheme.

viduals who began a vocational track are included in the sample.[19] Another restriction is that only pilot tracks which corresponded to tracks in the regular system are included, and vice versa. Our sample consists of 185,189 individuals.

The empirical analysis centers on a few different fertility outcomes. We are primarily interested in the probability of giving birth by age 20. But we also study the number of children at age 32 (the oldest age all individuals in the sample have reached the last year for which we have data) and the probability of being childless at this age.

We augmented the dataset with information on each parent's education measured in the year the students began upper secondary school and on their foreign background. Region of residence is defined as the municipality of residence during the fall semester of the subject's last year of compulsory school. This way we avoid the possibility that municipality of upper secondary school attendance may be endogenous with respect to the location of pilot scheme.[20]

Table 1 provides summary statistics for selected variables broken down by gender.[21] We can see that teenage childbearing is more frequent among women than among men. The share of women who experienced their first birth by age 20 is 9.1 percent while the corresponding number for men is 2.1 percent. However, conditional on having the first child no later than age 20 women and men are about the same age when their first child is born. It is also evident that women have their first child together with men that are substantially older. Conditional on giving birth to the first child by age 20 women are on average four years younger than their partner, while men have partners of similar age.

Figure 2 analyses the age difference between partners in more detail by showing the age difference between the individuals in our sample and the parent of their first child, conditional on having the first child by age 20. The top figure shows the distribution of

[19] In Section 4.3 we show evidence indicating that the pilot scheme did not affect selection into vocational tracks.

[20] It is unlikely that students would move already during compulsory school in order to take advantage of the pilot tracks, especially as it was already possible to apply to upper secondary schools in municipalities other than ones own. Moreover, the decision of where to locate the new available pilot slots each year was not taken until during the following spring, i.e. after the point in time when we measure municipality of residence (see SOU 1989:106 for details on the implementation process).

[21] Table A.3 contains a detailed description of how the variables we use in the analysis have been constructed and from which registers the information was collected.

the age gap for the women in our sample and the bottom figure displays the same gap for the men. A positive difference indicates that the father of the child is older than the mother. We can see that virtually no females have their first child together with a younger male. In contrast, there is no clear age difference between teenage fathers and the mother of their child. If an older partner indicates a more stable relationship, this discrepancy could mean that women's early pregnancies to a greater extent are planned than among teenage fathers. An additional indication that the women may be in more stable relationships can be seen in Table 1 where it is shown that a larger share of the teenage mothers are married/cohabiting with the child's other parent at age 31, compared to the teenage fathers.

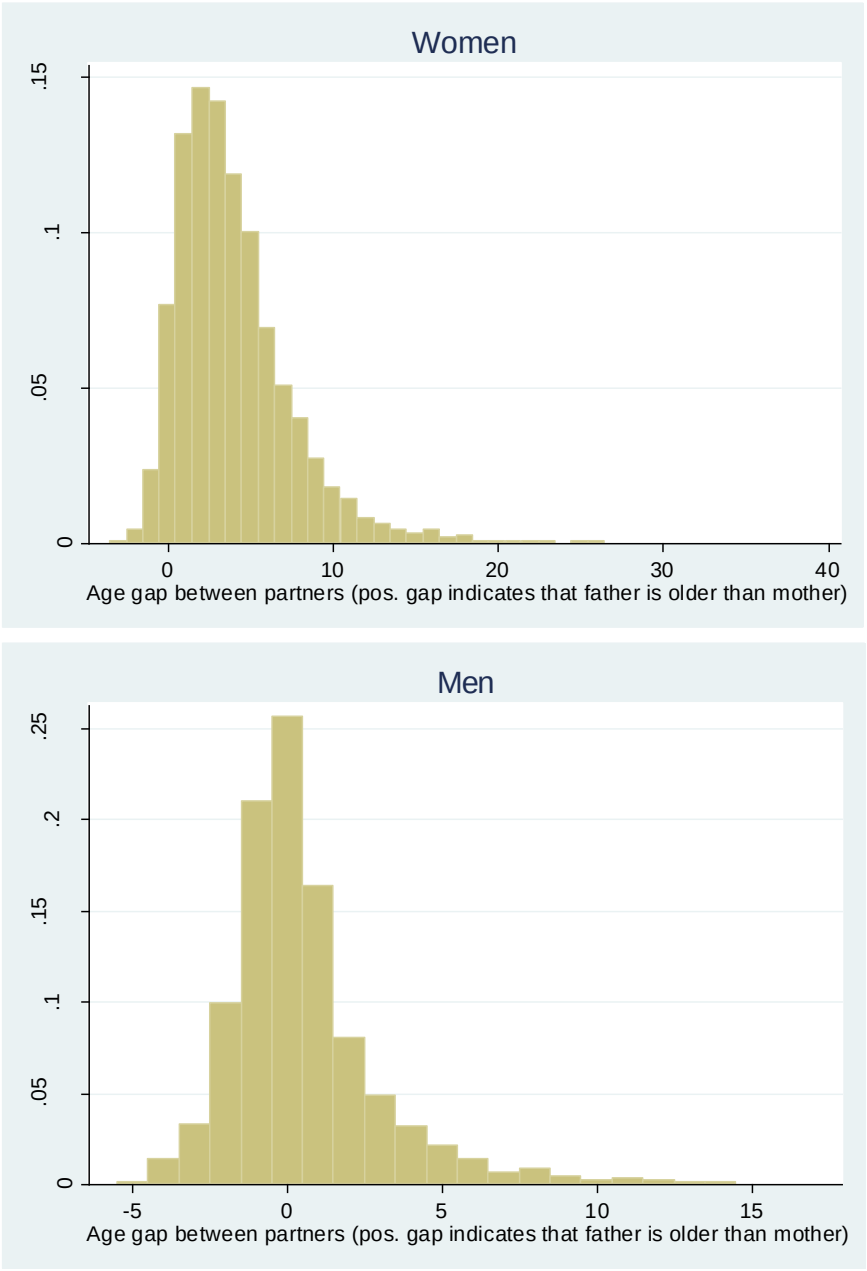
As could be expected based on the previous literature, Table 1 shows that those who have children in their teens have less education and lower earnings later in life compared to the rest of the sample. This pattern exists for both women and men. Note also that with the exception of compulsory school GPA the background characteristics are similar across gender.

Table 1: Sample characteristics by gender

	Women	Men
First birth by age 20	.091 (.288)	.021 (.142)
Age at firth birth, conditional on first birth by age 20	19.28 (.890)	19.37 (.922)
Age of partner at first birth, conditional on first birth by age 20	23.30 (3.65)	19.86 (2.60)
Married/cohabiting with the same partner at age 31, conditional on first birth by age 20	.366 (.482)	.302 (.459)
Number of children by age 32	1.44 (1.08)	.928 (.997)
Childless at age 32	.258 (.437)	.455 (.498)
At least 3 years of upper secondary educ. by age 31, conditional on first birth by age 20	.318 (.466)	.196 (.397)
At least 3 years of upper secondary educ. by age 31, conditional on *no* child by age 20	.532 (.499)	.384 (.486)
Annual wage earnings [a] at age 31, conditional on first birth by age 20	115,300 (90,740)	193,200 (116,200)
Annual wage earnings [a] at age 31, conditional on *no* child by age 20	132,200 (95,150)	219,900 (117,300)
Individual background characteristics:		
Foreign-born	.018 (.133)	.017 (.128)
Age at enrolment in upper secondary school	16.02 (.162)	16.05 (.218)
GPA compulsory school[b]	3.01 (.513)	2.77 (.509)
Parental characteristics:		
Both parents foreign born	.028 (.165)	.029 (.167)
Mother has at most compulsory education[c]	.477 (.499)	.459 (.498)
Mother has 2 years of upper secondary education	.390 (.488)	.392 (.488)
Mother has at least 3 years of upper secondary educ.	.133 (.340)	.148 (.355)
Father has at most compulsory education[c]	.531 (.499)	.519 (.500)
Father has 2 years of upper secondary education	.271 (.445)	.275 (.447)
Father has at least 3 years of upper secondary educ.	.198 (.398)	.206 (.405)
Number of observations	74,220	110,969

Notes: a) In SEK 2006. b) Compulsory school GPA is set on a scale 1 through 5. c) This category includes individuals where information on mother's/father's education is missing.

Figure 2: Age gap between partners by gender for individuals who had their first birth by age 20



Note: The top figure shows the age difference between the women in our sample and the fathers of their first child, conditional on the woman having her first birth by age 20. The bottom figure replicates the graph for the men in our sample.

4 Results

This section presents the results from our empirical analysis. As previously mentioned, we relate the timing of childbearing to an individual's choice of whether or not to attend the longer and more academically oriented vocational track. To account for endogenous selection into schooling we instrument for the choice of track using the share of the available vocational tracks which constituted the new 3-year tracks in the individual's municipality of residence.

We do all analyses separately for men and women. We also present results both for the entire group of vocational students and for population subgroups defined by parents' educational background and the subject's ability. We define "Parents with low education" as neither parent having more than two years of upper secondary education, and "Parents with high education" as at least one parent having completed more than two years of education. "Low GPA" is defined as having a final compulsory school GPA (strictly) below the average among all students throughout the country, and "High GPA" as having a final GPA above this average. To deal with potential serial correlation and heteroscedasticity, the standard errors are estimated by clustering at the municipality level (284 cells) (e.g. Bertrand, Duflo and Mullainathan 2004).

In subsection 4.1 we present our main results and subsection 4.2 provides falsification and robustness checks. In subsection 4.3 we extend the analysis by investigating the mechanisms through which education policy may affect early childbearing. Here we also examine the effects on long-run fertility.

4.1 The effect on early childbearing

Before proceeding to the results we show estimates for the first-stage relationship, i.e. the relationship between the pilot scheme intensity in an individual's municipality of residence and his/her choice of vocational track. Table 2 displays estimates from regressions both with and without controls for background characteristics. We can see that the first-stage relationship is strong and that standard F-tests clearly indicate that a weak instrument is not a concern (cf. Staiger and Stock 1997). Note also that the coefficient for the instrument is very robust to the inclusion of covariates in the model. In Table

A.4 we show separate first-stage estimates for the different subgroups. The estimates are similar across subgroups, indicating that any differences in effects between the groups cannot be explained by differential responses to the instrument.

Table 2 also shows that the students in the 3-year tracks are positively selected in terms of parents' educational background and, for women, also in terms of compulsory school GPA. Women with foreign-born parents are less likely to enroll in the longer vocational tracks, while there is no difference depending on foreign background for men.

Table 2: First stage regressions: The effect on the probability of enrolling in a 3-year track

	Women		Men	
Entire sample	(1)	(2)	(3)	(4)
Pilot intensity in municipality of residence (instrument)	.461***	.462***	.660***	.659***
	(.047)	(.047)	(.059)	(.059)
Birth year (dummies)	No	Yes	No	Yes
GPA compulsory school		.006***		.000
		(.001)		(.001)
Foreign-born		.016		.007
		(.013)		(.012)
Both parents are foreign-born		-.017*		-.002
		(.009)		(.007)
Mother's education level:				
≤ 9 years of compulsory educ.		Ref.		Ref.
2 years of upper secondary educ.		.006*		.000
		(.002)		(.002)
≥3 years of upper secondary educ.		.027***		.012***
		(.004)		(.003)
Missing data		.002		.001
		(.005)		(.005)
Father's education level:				
≤ 9 years of compulsory educ.		Ref.		Ref.
2 years of upper secondary educ.		.005*		.004**
		(.003)		(.002)
≥3 years of upper secondary educ.		.019***		.010***
		(.003)		(.003)
Missing data		.007*		-.000
		(.004)		(.004)
F-statistic on the instrument	96.00	97.83	124.02	124.11
Number of obs.	74,221	74,221	110,968	110,968

Notes: The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted 3-year tracks. All regressions include municipality of residence and upper secondary school starting year fixed effects. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/*** denotes significance on the 10/5/1 percent level.

Table 3 displays OLS and IV estimates of the effect of enrolling in the 3-year vocational track on the probability of giving birth by age 20. The IV estimates are presented both with and without controls for background characteristics. To conserve space we do not report the estimates for the control variables (available on request). In general, these show that early pregnancies are negatively correlated with GPA and parents' education, and positively correlated with immigrant status.

Starting with women, the OLS estimate in Panel A (Col. 1) shows a small and statistically insignificant negative effect of enrolling in a 3-year, rather than a 2-year, vocational track on the probability of having the first child by age 20. The IV estimates on the other hand indicate a negative effect. The coefficient in our preferred specification (Col. 3) suggests that enrolling in a longer and more academically oriented track reduces the probability of experiencing the first birth by age 20 by about 4 percentage points. This effect is statistically significant. In relation to the mean of the dependent variable it translates into a reduction of about 44 percent (–.040/.091). This is by all measures is a substantial effect.

The estimated effect is sizable also in comparison to other interventions explicitly designed to reduce teenage pregnancies. Our reduced form estimate, shown in Table A.5, suggests that going from a situation with no 3-year tracks in a municipality to a situation where all tracks are 3-year long decreases the probability of a teenage birth by about 20 percent (–.018/.091). The magnitude of the effect is thereby similar to the estimated effect of giving teenagers access to a major subsidy of oral contraceptives presented in Grönqvist (2009). It is more than twice as large as that of the compulsory schooling reforms in the US and Norway; Black, Devereux and Salvanes (2008) find that these reforms reduced the probability of a teenage birth by between 5 and 9 percent. One explanation for why our estimates are larger could be that education policy targeted at the upper secondary level involves greater non-monetary returns. Another reason might be that in contrast to the changes in compulsory schooling laws which took place in the 1960s and 1970s (at the latest) we are studying a reform that occurred quite recently. It is possible that institutional features relevant for fertility have changed up until today (e.g. new contraceptive technologies, access to abortions, increased enrolment in

higher education among women etc.) which may have increased the return to educational investments.[22]

Panels B and C show results by parents' educational attainment. The IV estimates show no statistically significant effect for women whose parents have low education. There is however a significant negative effect on early fertility for women with highly educated parents. A similar pattern can be observed in the separate analyses of women with a low respectively high compulsory school GPA (Panels C and D). The IV estimates show that women with a high GPA are significantly less likely to give birth by age 20 if enrolled in a 3-year track, but there is no significant effect for women with a low GPA.

The results in Panels B through D are consistent with the story that the effect of the reform is larger for women with higher opportunity costs of child rearing. Black, Devereux and Salvanes (2008) also find substantially larger effects in more affluent subgroups (urban residents and whites), which are likely to have higher opportunity costs of child rearing. Another possible explanation of the differential effects by parental background is that highly educated parents put more pressure on their daughters to not have children before finishing school. These results contrast to those presented in Grönqvist (2009) where the impact on early fertility of providing subsidized contraceptives is found to be stronger for socioeconomic disadvantaged groups. This suggests that different policies may induce different behavioral responses in some parts of the population.

As mentioned earlier, this is the first paper that investigates the effect of changes in education policy on men's fertility in a developed country. Columns (4)-(6) display the estimates for males. Looking at Panel A it is clear that there is no statistically significant effect of enrolling in the 3-year tracks on the probability of having the first child by age 20. This result holds also when analyzing different subgroups in Panels B through D. Both the OLS and IV estimates are statistically insignificant for all groups.

Why does education seem to matter only for women? One possible explanation is that females by tradition have had a greater control over the risk of becoming pregnant;

[22] Another explanation for the relatively large effect is that the estimate captures the average effect of attending a 3-year track for individuals who were induced to choose a longer program as a consequence of increased access to the new tracks. The effect on early childbearing in this group could differ from the effect in the total population.

for instance, by regulating the use of contraceptives or deciding on whether or not to abort an unwanted pregnancy.[23] The opportunity cost of having children while being enrolled in school is also likely to be higher for females because of the interruption due to giving birth and since they usually spend more time with their children than the fathers, at least in the months following the birth. In this context it is further interesting to note that our descriptive evidence indicated that women's early pregnancies may be planned to a greater extent compared to teenage fathers. Table 1 showed that teenage childbearing is more frequent among women and that the women who have children early tend to have older partners. If education policy more strongly influences planned births this could potentially also explain our findings.

One last thing to note in Table 3 is that the IV estimates for all subgroups remain remarkably stable when adding covariates to the model (compare Col. 2 and 3, and Col. 5 and 6). The fact that the estimates are robust to the inclusion of controls for some arguable very important background characteristics, suggests that omitted characteristics may be unimportant as well. This finding thus strengthens our confidence that omitted variables are not driving our results. In the next section we continue by presenting results from additional robustness checks.

[23] Although the possibility for women to control their own fertility need not necessarily be the same in other contexts, it is interesting to note that our findings parallel the results in Duflo and Breierova (2004) for Indonesia.

Table 3: The effect of enrolling in the 3-year vocational track on the probability of having the first child by age 20

	Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
A. Entire sample						
Effect of enrolling in the 3-year vocational	-.002	-.035	-.040*	-.001	-.005	-.005
track	(.003)	(.022)	(.022)	(.001)	(.007)	(.007)
Mean of dependent variable	.091	.091	.091	.021	.021	.021
Number of obs.	74,221	74,221	74,221	110,968	110,968	110,968
B. Parents with low education						
Effect of enrolling in the 3-year vocational	.000	-.017	-.025	-.002	-.009	-.010
track	(.004)	(.028)	(.027)	(.002)	(.009)	(.008)
Mean of dependent variable	.102	.102	.102	.023	.023	.023
Number of obs.	53,927	53,927	53,927	78,656	78,656	78,656
C. Parents with high education						
Effect of enrolling in the 3-year vocational	-.009**	-.080**	-.082**	.001	.008	.008
track	(.004)	(.039)	(.039)	(.002)	(.010)	(.010)
Mean of dependent variable	.064	.064	.064	.015	.015	.015
Number of obs.	20,294	20,294	20,294	32,312	32,312	32,312
D. Low GPA						
Effect of enrolling in the 3-year vocational	-.008	-.015	-.020	-.003	-.013	-.013
track	(.006)	(.043)	(.041)	(.002)	(.010)	(.010)
Mean of dependent variable	.133	.133	.133	.026	.026	.026
Number of obs.	33,194	33,194	33,194	69,146	69,146	69,146
E. High GPA						
Effect of enrolling in the 3-year vocational	-.004	-.055*	-.053*	-.000	.008	.007
track	(.003)	(.028)	(.028)	(.002)	(.007)	(.007)
Mean of dependent variable	.057	.057	.057	.011	.011	.011
Number of obs.	41,027	41,027	41,027	41,822	41,822	41,822
All control variables included	Yes	No	Yes	Yes	No	Yes

Notes: Each cell represents a separate regression. All regressions include municipality of residence and upper secondary school starting year fixed effects. Col. (1), (3), (4) and (6) additionally control for: compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. "Parents with low education" is defined as neither parent having more than two years of upper secondary education; and "parents with high education" as at least one parent having more than two years of upper secondary education. "Low GPA" is

defined as having scored below the mean compulsory school GPA; and "high GPA" as having scored at least the mean compulsory school GPA. The (potentially) endogenous variable takes the value one if the individual enrolled in the 3-year vocational track and zero if the individual enrolled in a regular 2-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted 3-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */** denotes significance on the 10/5 percent level.

4.2 Robustness checks

4.2.1 Testing the validity of the empirical strategy

Recall that our identification strategy hinges on several assumptions. First, the instrument should not be correlated with any unobserved variables affecting early fertility. The instrument should also have no direct effect on the outcome other than through influencing whether a person enrolled in a 2- or a 3-year vocational track. In this subsection we present results from several tests of the validity of our strategy.

One way of investigating if our results are spurious is to use the fact that the length of the academic tracks in upper secondary school was not altered during the pilot period. Hence, the extent to which a region participated in the pilot scheme should be uncorrelated with changes in the early fertility of academic students. Such a correlation would instead indicate the presence of some unobserved factor which is correlated with the pilot scheme as well as with the outcome of interest.

To carry out this falsification test we estimated the following regression for students enrolled in academic tracks

$$\text{Early childbearing}_{ijs} = \alpha\, N^{-1}\sum_{l}^{N} 1\{Track_l > 2 year\} + \pi' X_i + \rho_j + \omega_s + v_{ijs} \qquad (2)$$

where $N^{-1}\sum_{l}^{N} 1\{Track_l > 2 year\}$ is the share of the available vocational tracks, in an individual's municipality of residence, which constituted 3-year tracks by the time he or she began upper secondary school[24]; X_i represents a vector of control variables; ρ_j and ω_s are municipality and school starting year fixed effects.

Table 4 displays our results. To facilitate comparison, the top part of the table shows the reduced form impact for vocational students. As expected, there is a statistically significant negative effect of the instrument on the probability of experiencing the first birth by age 20 for female students in vocational tracks; however there is no significant effect for women who enrolled in academic tracks. The effect for men is statistically

[24] Obviously, the reason for focusing on the reduced form impact instead of instrumenting for the choice of track length is that the academic tracks were unaffected by the reform. The reduced form estimates should be interpreted as the effect of going from a situation with no 3-year vocational tracks in the municipality to a situation where all tracks are 3-year tracks.

insignificant for both groups of students. We think that these findings provide strong support for the validity of our empirical strategy.

Table 4: Falsification tests using students in academic tracks. Reduced form estimates.

Outcome: Child by age 20	Women (1)	Men (2)
A. Vocational students	-.018*	-.003
	(.010)	(.004)
Mean of dependent variable	.091	.021
Number of observations	74,221	110,968
B. Academic students	.004	-.001
	(.006)	(.003)
Mean of dependent variable	.017	.004
Number of observations	119,808	104,453

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year FE:s, all regressions control for: compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. Robust standard errors in parentheses allow for clustering by municipality of residence. */** denotes significance on the 10/5 percent level.

Hall (2012) provides additional tests of the validity of the instrument by checking whether the localization of pilot slots is correlated with changes in observed student characteristics (basically the background characteristics listed in Table 1). The results show no clear relationship between the pilot intensity and changes in student characteristics, which suggests that the pilot also may be uncorrelated with changes in unobserved factors.

Another concern is that the availability of pilot tracks may have affected an individual's choice of whether or not to enroll in a vocational track. If this is the case, restricting the sample to vocational students could introduce some sample selection issues that could lead to bias in the results.[25] To examine this concern we regressed an indicator of enrolling in a vocational rather than an academic track on the intensity of the pilot scheme in the individual's municipality of residence, controlling for municipality of residence and school starting year fixed effects as well as the other covariates. In Table 5 we can see that there is no statistically significant effect of access to the pilot scheme on the likelihood of selecting a vocational track.[26] [27]

[25] Note that this is only a concern if our control variables are not rich enough to account for such compositional changes.

[26] Hall (2012) provides additional tests of this concern. She uses a difference-in-difference strategy in which she compares the share of students starting vocational or academic tracks before and after the pilot

Table 5: Effect of pilot intensity on the probability of enrolling in a vocational track

	Outcome: Enrolling in a vocational track	
	Women	Men
	(1)	(2)
Pilot intensity in municipality of residence	.006	.013
	(.015)	(.014)
Mean of dependent variable	.383	.515
Number of obs.	194,028	215,422

Notes: Each cell represents a separate regression. Pilot intensity is the share of available vocational tracks in the municipality of residence which constituted 3-year tracks (at the time of enrolment in upper secondary school). In addition to municipality of residence and upper secondary school starting year FE:s, the regressions control for: compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/*** denotes significance on the 10/5/1 percent level.

It is possible – though it seems unlikely – that the longer and more academic pilot tracks attracted students who would otherwise have chosen not to advance to upper secondary school. Hall (2012) investigates this potential concern using data on all students who graduated from compulsory school during 1988-1990. She finds no evidence that the pilot scheme affected selection into upper secondary school.

A final concern is that the pilot scheme may not only have affected the subjects themselves but may also have led to a behavioral response among potential partners. If individuals choose partners of a similar age in the same municipality then it is likely that also the partners were influenced. In this case the IV assumptions may be violated. For this reason it is reassuring that Figure 2 revealed that women who give birth early tend to have children with males that are substantially older. This suggests that this potential problem is less severe for women. However, we also saw in Figure 2 that a substantial fraction of the teenage fathers have their first child with a woman of the

scheme was introduced, and contrasts this estimate across municipalities which differed in their extent of participation. Her results indicate that selection into vocational tracks was not affected by the pilot. Using the same strategy she also investigates whether the composition of vocational students changed as a result of the pilot, but finds no evidence suggesting compositional changes.

[27] A possible explanation for why the pilot tracks does not seem to have attracted students from academic tracks has to do with uncertainty about the localization of the pilot scheme at the time when students applied to upper secondary school. SOU 1989:90 and 1990:75 report that the decision of where to locate new pilot tracks sometimes was taken *after* the deadline for application to upper secondary school. Case studies of the implementation process describe how municipalities then let the students *who had already applied to vocational tracks* reapply in order to be considered for the new tracks. The practices however are likely to have differed somewhat across municipalities.

same age or younger. It is therefore encouraging to note that the reduced form estimates parallel our main results in Table 3. The reduced form estimates (displayed in Table A.5) incorporate all channels through which the policy change may affect fertility and tells us about the overall effect of the pilot scheme on early fertility.

4.2.2 Alternative measure of pilot scheme intensity

Our instrument – the degree to which an individual's municipality of residence participated in the pilot when he/she began upper secondary school – is measured as the share of the available vocational tracks which constituted 3-year tracks. As mentioned in Section 2.1.1 a more ideal measure would be the share of *slots* in vocational tracks which represented 3-year tracks, but this type of data is not available at a disaggregated level. To check whether our results could be sensitive to this type of measurement error in the instrument, we have repeated our analysis with an alternative measure of pilot scheme intensity. We instead define pilot intensity as the share of students enrolling in vocational tracks, in an individual's municipality of residence, who enrolled in 3-year tracks. This is not a perfect measure of the supply of pilot slots either, as it is likely to be affected by student demand – it is common that not all slots are filled (see Table A.1) and it is conceivable that municipalities sometimes increase the number slots as a reaction to a large demand. Nevertheless, it is reassuring to see that our results are very similar when we use this alternative measure as instrument; see Table A.6.

4.3 Extensions: potential mechanisms and total fertility

Having established that our results are unlikely to be explained by factors that could invalidate our identification strategy, we continue the analysis by investigating some of the channels through which an extension of upper secondary education may affect early childbearing. We also provide some results on the impact on total fertility.

4.3.1 Are increased dropout rates explaining the (absence of) effects for some subgroups?

Recall that the pilot scheme implied a *possibility* of attending a prolonged and more academically oriented vocational track. Not all students, however, may benefit from an upper secondary education with a substantial academic content (Brunello and Checchi 2007). Beginning a longer and academically more challenging track may increase the

probability of dropping out and could thereby have a negative impact on some students' educational attainment. Hall (2012) examines the effect of the reform on the probability of graduating from upper secondary school and finds a negative effect for students with low ability and with non-academic parents. This could potentially explain why we do not find any significant effect of enrolling in a 3-year track on early fertility for some subgroups. To examine this issue we re-estimated our model conditional on actually graduating from upper secondary school.[28] The results (available on request) are similar in this analysis, which indicates that increased dropout rates are not explaining these results.[29]

4.3.2 Is it an incapacitation effect?

Black, Devereux and Salvanes (2008) argue that individuals enrolled in education may strictly mechanically not have the time, desire or opportunity to have a child. To test whether the effects of compulsory schooling on teenage childbearing in the US and Norway operate through such an 'incapacitation effect', they investigate how compulsory schooling laws affect the probability of having one's first child at different ages, conditional on not having a child before then. The idea is that if the delayed childbearing is *solely* caused by an incapacitation effect, then compulsory schooling laws should have no behavioral effect at ages beyond those directly affected by the reform. Their results however show negative effects on fertility also at higher ages, suggesting that the incapacitation effect is not the only mechanism at work.

We use the same strategy to investigate whether the negative effect of attending a 3-year track on the probability of having the first child by age 20, could be explained by incapacitation. Although the same argument not readily extends to voluntary education (as in the case of upper secondary education), since it does not force individuals to stay in school, it is likely that conditional on having enrolled individuals still face a high opportunity cost of dropping-out which restrains them from quitting school.

The policy change of interest in our paper delayed graduation from upper secondary school from (typically) age 18 to 19. Since a pregnancy lasts for about nine month, we

[28] A person is considered to have dropped out if he or she still has not graduated six years after being admitted (based on data from the Upper Secondary School Graduation Record).
[29] Note however that we are limiting the sample based on an outcome variable, which may introduce some sample-selection issues that could bias the results.

should therefore not expect any negative effects on births above age 20 if the incapacitation mechanism is the only mechanism at work. As pointed out by Black, Devereux and Salvanes (2008), this mechanism would be difficult to identify if the policy change also increased transitions to higher education. However, Hall (2012) found no evidence that the Swedish reform affected transitions to university studies.

Table 6 presents the results from this analysis. We only focus on women here since our main analysis found no effect for men. There is no evidence of negative effects of enrolling in the 3-year tracks on births beyond age 20. This result holds also when repeating the exercise for different subgroups of the population. Hence, the results suggest that the negative effect on the probability of having the first child by age 20 may be solely due to an incapacitation effect. When interpreting these results we should note that conditioning on not already having a child creates a selected sample. However, the conclusions remain the same if we include all individuals in the regressions (results available on request).

Table 6: The effect on women's probability of having a child at a given age, conditional on not already having a child

	‹=Age 18	Age 19	Age 20	Age 21	Age 22
A. Entire sample					
IV	-.007	-.022*	-.014	.012	.013
	(.012)	(.013)	(.018)	(.022)	(.023)
Sample mean	.017	.028	.049	.063	.073
Number of observations	74,221	72,984	70,906	67,454	63,189
B. Parents with low education					
IV	-.004	-.017	-.007	.021	.021
	(.014)	(.016)	(.023)	(.030)	(.029)
Number of observations	53,927	52,929	51,243	48,449	45,106
Sample mean	.018	.032	.055	.069	.080
C. Parents with high education					
IV	-.019	-.040*	-.031	.002	-.006
	(.019)	(.020)	(.030)	(.035)	(.035)
Number of observations	20,294	20,055	19,663	19,005	18,083
Sample mean	.012	.020	.033	.049	.055
D. Low GPA					
IV	-.015	-.025	.015	.001	.038
	(.023)	(.030)	(.035)	(.045)	(.049)
Number of observations	33,194	32,296	30,897	28,770	26,418
Sample mean	.027	.043	.069	.082	.087
E. High GPA					
IV	-.008	-.022	-.024	.021	.003
	(.011)	(.016)	(.022)	(.024)	(.026)
Number of observations	41,027	40,688	40,009	38,684	36,771
Sample mean	.008	.017	.033	.049	.062

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions control for: compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. "Parents with low education" is defined as neither parent having more than two years of upper secondary education; and "parents with high education" as at least one parent having more than two years of upper secondary education. "Low GPA" is defined as having scored below the mean compulsory school GPA; and "high GPA" as having scored at least the mean compulsory school GPA. The (potentially) endogenous variable takes the value one if the individual enrolled in the extended 3-year vocational track and zero if the individual enrolled in a regular 2-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted 3-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */** denotes significance on the 10/5 percent level.

4.3.3 The effect on fertility later in life

Our observation window is too short to observe completed fertility; the oldest age all individuals in our sample have reached the last year for which we have data is 32. We therefore provide evidence on the impact on fertility at this age, focusing on the total number of children and the probability of being childless.

Table 7 presents IV and OLS estimates for the entire sample as well as for the different subgroups. While the OLS regressions for women suggest a positive and significant relationship between enrollment in the longer and more academic track and the

number of children, and a significant negative relationship with the probability of being childless, both IV estimates are statistically insignificant (Panel A). The IV estimates are statistically insignificant also for men as well as for all subgroups of the population (Panel B−E). These results are well in line with the findings of Monstad, Propper and Salvanes (2008) for Norway. They find that the compulsory schooling reform, which led to postponement of first births away from the teenage years, had no impact on the women's completed fertility.[30]

[30] For the US the results are mixed: León (2004), who takes advantage of changes in state compulsory schooling laws, estimates a significant negative effect of educational attainment on total fertility. McCrary and Royers (2011) on the other hand, who exploit school entry policies, find that maternal education plays a minor role in fertility decisions.

Table 7: OLS and IV estimates of the effect of enrolling in the 3-year track on fertility at age 32

	Women		Men	
	No. of children	Childless	No. of children	Childless
	(1)	(2)	(3)	(4)
A. Entire sample				
OLS	.052**	-.018***	.015	-.009*
	(.014)	(.006)	(.010)	(.005)
IV	.026	.013	-.015	.003
	(.095)	(.031)	(.046)	(.021)
Mean of dependent variable	1.437	.258	.928	.455
Number of observations	74,221	74,221	110,968	110,968
B. Parents with low education				
OLS	.068***	-.023***	.015	-.010
	(.016)	(.006)	(.013)	(.007)
IV	.112	-.019	-.048	.015
	(.102)	(.035)	(.056)	(.025)
Mean of dependent variable	1.476	.246	.952	.445
Number of observations	53,927	53,927	78,656	78,656
C. Parents with high education				
OLS	.010	-.005	.010	-.006
	(.024)	(.010)	(.018)	(.009)
IV	-.218	.104	.060	-.029
	(.179)	(.078)	(.080)	(.037)
Mean of dependent variable	1.332	.287	.868	.480
Number of observations	20,294	20,294	32,312	32,312
D. Low GPA				
OLS	.058**	-.019**	.017	-.012*
	(.023)	(.009)	(.013)	(.007)
IV	.065	-.003	-.052	.014
	(.155)	(.049)	(.064)	(.028)
Mean of dependent variable	1.483	.251	.934	.455
Number of observations	33,194	33,194	69,146	69,146
E. High GPA				
OLS	.040**	-.016**	.011	-.005
	(.017)	(.007)	(.019)	(.009)
IV	.007	.021	.054	-.017
	(.119)	(.040)	(.069)	(.038)
Mean of dependent variable	1.399	.263	.917	.455
Number of observations	41,027	41,027	41,822	41,822

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions control for: compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. "Parents with low education" is defined as neither parent having more than two years of upper secondary education; and "parents with high education" as at least one parent having more than two years of upper secondary education. "Low GPA" is defined as having scored below the mean compulsory school GPA; and "high GPA" as having scored at least the mean compulsory school GPA. The (potentially) endogenous variable takes the value one if the individual enrolled in the extended 3-year vocational track and zero if the individual enrolled in a regular 2-year track. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted 3-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/*** denotes significance on the 10/5/1 percent level.

5 Concluding remarks

The social benefits of education are not yet fully understood. A large literature claims that teenage childbearing imposes large costs on society in terms of worse long-run social and economic outcomes for the mothers as well as their children. This paper considers the role of education policy in preventing early childbearing. We study a major educational reform in Sweden occurring in the beginning of the 1990s in which vocational tracks in upper secondary school were prolonged from two to three years and the curricula were made more academic. Our research design exploits cross-regional and cross-time variation in the implementation of a pilot scheme preceding the reform in which several municipalities evaluated the new policy. Relying on rich register data we find that women who enrolled in the new longer tracks were significantly less likely to give birth early in life and that this effect is driven by women with higher opportunity costs of child rearing. We also find that the policy change had no effect on men's fertility decisions.

The results in the present paper suggest that prolonging the vocational upper secondary education by one year and making it more academic reduced the probability of teenage childbearing by 44 percent. The evidence provided therefore indicates that the social benefits of education policy may extend beyond those usually claimed.

In this context it is relevant to ask whether the results in this paper may extend to other settings. Black, Devereux and Salvanes (2008) show that there are remarkable similarities in the effect of education on fertility between the US and Norway, despite that the two countries are so different institutionally. This suggests that the findings in the current paper very well could be relevant also for other countries.

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Appendix

Table A.1: Number of available places and share of vacant places by pilot track and year

	1987		1988		1989		1990	
	No. of places	Share vacant	No. of places	Share vacant	No. of places	Share vacant	No. of places	Share vacant
Electrical engineering	48	0.00	528	0.02	656	0.03	776	0.02
Health care	46	0.02	2 182	0.03	2 918	0.03	3 072	0.10
Heating, ventilation & sanitation	64	0.11	64	0.00	72	0.00	104	0.00
Industry	352	0.01	1 608	0.09	1 952	0.13	1 968	0.12
Business & services			210	0.01	660	0.03	990	0.05
Caring services: children, youth			256	0.01	420	0.01	420	0.08
Construction			296	0.08	408	0.02	432	0.01
Textile & clothing manufacturing			136	0.11	208	0.22	224	0.17
Transport & vehicle engineering			752	0.04	992	0.03	1 056	0.02
Use of natural resources			352	0.12	640	0.09	720	0.04
Constructional metalwork					56	0.14	56	0.05
Food manufacturing					224	0.08	256	0.11
Handicraft					32	0.03	64	0.05
Painting					56	0.04	88	0.05
Process technology					176	0.17	208	0.23
Restaurant					336	0.00	416	0.00
Wood technology					144	0.10	168	0.09
Graphic							112	0.00
Total	**510**	**0.03**	**6 384**	**0.05**	**9 950**	**0.06**	**11 130**	**0.07**

Notes: Share of vacant places by September 15th each year. Source: National Board of Education (1988), (1989a), (1989b), and (1990b).

Table A.2: Vocational tracks in upper secondary school

Regular 2-year vocational tracks	3-year pilot tracks
Agriculture Forestry Gardening	Use of natural resources
Business & administration Distribution & administration	Business & services
Caring services Social services	Health care
Caring services: children & youth	Caring services: children & youth
Clothing manufacturing	Textile & clothing manufacturing
Construction	Construction Constructional metalwork Heating, ventilation & sanitation Painting
Consumer studies♦	
Electrical engineering	Electrical engineering
Food manufacturing	Food manufacturing Restaurant
Operation and maintenance engineering♦	
Process technology	Process technology
Vehicle engineering	Transport & vehicle engineering
Wood technology	Wood technology
Workshop techniques	Industry
-	Handicraft♣
-	Graphic♣

Notes: ♦Tracks which do not directly correspond to any of the pilot tracks, but are still included in the analysis as important elements of them appear to be present on one or more of the pilot tracks. ♣Tracks which are not included as they do not correspond to any of the 2-year tracks.

Table A.3: Variable definitions

Variables	Definitions
Instrument:	
Pilot scheme intensity in municipality of residence	Share of available vocational tracks which constituted 3-year tracks in the person's municipality or residence, when he/she began upper secondary school. (The Upper Secondary School Application Record)
Outcome variables:	
Age at first birth	Dummy variable = 1 for age at first birth being $\leq j$ (The Multi-Generation Register)
Number of children	Number of children at age 32 (The Multi-Generation Register)
Childless	Dummy variable = 1 if no children at age 32; 0 otherwise (The Multi-Generation Register)
Individual characteristics:	
Female	Dummy variable = 1 if female; 0 otherwise. (The Multi-Generation Register)
Immigrant background	Dummy variable = 1 if born in non-Nordic country; 0 otherwise. (LOUISE)
GPA compulsory school	GPA the last year of compulsory school. (Cohort 1986-87: the Upper Secondary School Application Record. Cohort 1988-90: the Compulsory School Graduation Record)
Parental characteristics:	
Immigrants	Dummy variable = 1 if both biological parents are born in non-Nordic countries; 0 otherwise. (LOUISE)
Upper secondary education	Dummy variable = 1 if the parents' highest education is upper secondary education; 0 otherwise. Measured the year the student finished compulsory school. (LOUISE)
Post-secondary education	Dummy variable = 1 if the parents' highest education is post-secondary education; 0 otherwise. Measured the year the person finished compulsory school. (LOUISE)

Notes: Statistics Sweden registers in parenthesis. Municipality of residence is measured on December 31st the person's last year of compulsory school.

Table A.4: First stage regressions: The effect on the probability of enrolling in a 3-year track

	Women		Men	
	(1)	(2)	(3)	(4)
A. Parents with low education				
Pilot intensity in municipality of residence (instrument)	.455*** (0.047)	.456*** (.047)	.657*** (.061)	.657*** (.061)
F-statistic on the instrument	92.88	94.53	116.42	116.42
Number of obs.	53,927	53,927	78,656	78,656
B. Parents with high education				
Pilot intensity in municipality of residence (instrument)	.483*** (.055)	.485*** (.055)	.676*** (.060)	.676*** (.060)
F-statistic on the instrument	76.33	78.65	127.78	128.54
Number of obs.	20,294	20,294	32,312	32,312
C. Low GPA				
Pilot intensity in municipality of residence (instrument)	.447*** (.052)	.447*** (.052)	.689*** (.062)	.688*** (.062)
F-statistic on the instrument	72.67	72.91	123.48	123.48
Number of obs.	33,194	33,194	69,148	69,148
D. High GPA				
Pilot intensity in municipality of residence (instrument)	.471*** (.052)	.470*** (.052)	.620*** (.065)	.620*** (.065)
F-statistic on the instrument	81.48	81.91	89.95	90.06
Number of obs.	41,027	41,027	41,822	41,822
All control variables included	No	Yes	No	Yes

Notes: Each cell represents a separate regression. The instrument is the share of available vocational tracks in the municipality of residence at the time of enrollment which constituted 3-year tracks. All regressions include municipality of residence and upper secondary school starting year fixed effects. Col. (2) and (4) additionally control for compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. "Parents with low education" is defined as neither parent having more than two years of upper secondary education; and "parents with high education" as at least one parent having more than two years of upper secondary education. "Low GPA" is defined as having scored below the mean compulsory school GPA; and "high GPA" as having scored at least the mean compulsory school GPA. Robust standard errors in parentheses allow for clustering by municipality of residence. */**/*** denotes significance on the 10/5/1 percent level.

Table A.5: Reduced form estimates of the effect of the reform on the probability of having the first child by age 20

	Women	Men
A. Entire sample		
Estimate	-.018*	-.003
	(.010)	(.004)
Mean of dependent variable	.091	.021
Number of observations	74,221	110,968
B. Parents with low education		
Estimate	-.012	-.006
	.012	(.006)
Mean of dependent variable	53,927	78,656
Number of observations	.102	.023
C. Parents with high education		
Estimate	-.040**	.005
	(.019)	(.007)
Mean of dependent variable	.064	.015
Number of observations	20,294	32,312
D. Low GPA		
Estimate	-.009	-.009
	(.019)	(.007)
Mean of dependent variable	.133	.026
Number of observations	33,194	69,146
E. High GPA		
Estimate	-.025**	.005
	(.012)	(.005)
Mean of dependent variable	.057	.011
Number of observations	41,027	41,822

Notes: Each cell represents a separate regression. In addition to municipality of residence and upper secondary school starting year fixed effects, all regressions control for: compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. "Parents with low education" is defined as neither parent having more than two years of upper secondary education; and "parents with high education" as at least one parent having more than two years of upper secondary education. "Low GPA" is defined as having scored below the mean compulsory school GPA; and "high GPA" as having scored at least the mean compulsory school GPA. Robust standard errors in parentheses allow for clustering by municipality of residence. */** denotes significance on the 10/5 percent level.

Table A.6: The effect of enrolling in the 3-year track on the probability of having the first child by age 20. Regressions using an alternative definition of the instrument

	Women		Men	
	IV: Original instrument (main results)	IV: Alternative instrument	IV: Original instrument (main results)	IV: Alternative instrument
A. Entire sample				
Effect of enrolling in the 3-year vocational track	-.040*	-.047**	-.005	-.008
	(.022)	(.022)	(.007)	(.006)
Mean of dependent variable	.091	.091	.021	.021
Number of obs.	74,221	74,221	110,968	110,968
B. Parents with low education				
Effect of enrolling in the 3-year vocational track	-.025	-.037	-.010	-.013
	(.027)	(.027)	(.008)	(.008)
Mean of dependent variable	.102	.102	.023	.023
Number of obs.	53,927	53,927	78,656	78,656
C. Parents with high education				
Effect of enrolling in the 3-year vocational track	-.082**	-.076**	.008	.007
	(.039)	(.037)	(.010)	(.010)
Mean of dependent variable	.064	.064	.015	.015
Number of obs.	20,294	20,294	32,312	32,312
D. Low GPA				
Effect of enrolling in the 3-year vocational track	-.020	-.030	-.013	-.015
	(.041)	(.038)	(.010)	(.009)
Mean of dependent variable	.133	.133	.026	.026
Number of obs.	33,194	33,194	69,146	69,146
E. High GPA				
Effect of enrolling in the 3-year vocational track	-.053*	-.059**	.007	.003
	(.028)	(.030)	(.007)	(.007)
Mean of dependent variable	.057	.057	.011	.011
Number of obs.	41,027	41,027	41,822	41,822

Notes: Each cell represents a separate regression. All regressions include municipality of residence and upper secondary school starting year fixed effects as well as controls for compulsory school GPA, immigrant status, birth year (dummies), each parent's educational attainment (3-levels), whether both parents are foreign-born, and missing data on parents' education. "Parents with low education" is defined as neither parent having more than two years of upper secondary education; and "parents with high education" as at least one parent having more than two years of upper secondary education. "Low GPA" is defined as having scored below the mean compulsory school GPA; and "high GPA" as having scored at least the mean compulsory school GPA. The (potentially) endogenous variable takes the value one if the individual enrolled in the 3-year vocational track and zero if the individual enrolled in a regular 2-year track. The original instrument is the share of available vocational tracks in the municipality of residence which constituted 3-year tracks. The alternative instrument is the share of students enrolling in vocational tracks in the municipality of residence who enrolled in 3-year tracks. Robust standard errors in parentheses allow for clustering by municipality of residence. */** denotes significance on the 10/5 percent level.

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