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Putting teenagers on the pill:
the consequences of subsidized contraception

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Putting teenagers on the pill: the consequences of subsidized contraception*

by

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Abstract

This paper investigates the consequences of a series of Swedish policy changes beginning in 1989 where different regions started subsidizing the birth control pill. The reforms were significant and applied to all types of oral contraceptives. My identification strategy takes advantage of the fact that the reforms were implemented successively over time and targeted specific cohorts of young women, in particular teenagers. This generates plausibly exogenous variation in access to the subsidy. The paper first demonstrates that access significantly increased pill use. Using regional, temporal, and cohort variation in access, I then go on to examine the impact on abortions. The estimates show that the subsidy significantly decreased the abortion rate by about 8 percent. Furthermore, long-term access decreased the likelihood of teenage childbearing by about 20 percent. However, there is no significant effect on labor supply, marriage, educational attainment or welfare take-up.

Keywords: Natural experiment; abortions; teenage childbearing; labor supply
JEL-codes: J13

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Table of contents

1 Introduction

Unintended childbearing is both frequent and widespread, especially among young women. The social and economic costs of unintended childbearing are potentially large since these births are associated with poor socioeconomic and health outcomes of both mothers and children. In addition, unwanted pregnancies account for approximately 1.5 million abortions annually in the U.S. alone (Institute of Medicine 1995). These concerns have motivated policy makers to instigate a wide range of family planning programs.[1] Despite the vast interest in such interventions there is however very scarce evidence on the efficiency of different policies.

This paper investigates the consequences of a series of Swedish policy changes beginning in 1989 where different regions started subsidizing the birth control pill. The reforms were significant and applied to all types of oral contraceptives. The subsidy rate was on average 75 percent. My identification strategy takes advantage of the fact that the reforms were implemented successively over time and targeted specific cohorts of young women, in particular teenagers. This generates plausibly exogenous variation in access to the subsidy, which is used to investigate the impact on abortions, fertility, marriage, educational attainment, and labor supply.

The main argument for subsidizing the birth control pill for teenagers is that young women may lack stable income sources, and therefore are more likely to prematurely end or delay the course of the treatment. Since the timing of the treatment is crucial for its success even short interruptions from the programme increases the risk of an unintended pregnancy. Still, it is not obvious that the demand for contraception is price elastic. Women who consider the cost of pregnancy as very high may either choose to completely abstain from sex or always pay the cost of getting the pill. Thus, it is not certain that subsidizing the pill will lead to a behavioral response. Furthermore, having access to inexpensive contraceptives could mean that women raise their level of sexual activity, increasing the likelihood of a pregnancy. This makes the net effect on fertility

[1] The Institute of Medicine (1995) reports that there are more than 200 local programs operating in the U.S. that in some way address unintended pregnancy.

ambiguous. If women substitute between the ”pill” and other not as effective contraceptive methods in order to avoid unwanted births, a subsidy that changes the relative price between these technologies can potentially also affect the abortion rate.

There are several reasons for why easier access to oral contraceptives could matter for socioeconomic outcomes as well. The most obvious mechanisms are: delayed childbearing, smaller families or reduced risk of shot-gun marriages.[2] Additionally, it has been suggested that oral contraceptives may raise the returns to investments in education and work by reducing uncertainty about future interruptions from the labor market and school (Bailey 2006; Goldin and Katz 2002; Weiss 1986; Mincer and Polachek 1974). This means that a subsidy can have a direct effect on socioeconomic outcomes. A similar story is provided by Chiappori and Oreffice (2007) who propose that access to oral contraceptives may improve the woman's bargaining position within a couple, leading to an increased share of the household's resources; something that potentially could reduce female labor supply through a standard income effect.

The topic of this paper is related to a series of recent studies highlighting the role of the birth control pill for women's well-being. Ananat and Hungerman (2007), Bailey (2006), Goldin and Katz (2002), and Guldi (2007) exploit cross-state and cross-time variation in different groups' access to the birth control pill in the U.S. in the 1960s and 1970s. The results suggest that access to the pill increased labor supply, lead to later age at first marriage, delayed childbearing, and reduced the abortion rate. Bailey (2007) takes advantage of variation in state laws regulating contraceptive sales from 1873 to 1965 (Comstock laws) and shows that access to the pill accelerated the reduction in U.S. fertility rates. More closely related to my paper is Kearney and Levine (2008) who examine the consequences of state-level Medicaid policy changes that expanded eligibility for family planning services to higher income women and to Medicaid clients whose benefits would expire otherwise. The results indicate that the reforms led to a

[2] Studies of the link between fertility, marriage, and socioeconomic outcomes include: Ashcraft and Lang (2007); Åslund and Grönqvist (2007); Holmlund (2005); Hotz, Mullins and Sanders (1997); Kearney and Levine (2007); Klepinger, Lundberg and Plotnick (1999); Maynard (1996); Stevenson and Wolfers (2007).

nine percent decrease in births to eligible women age 20–44; a finding that is attributed to greater contraceptive use.[3]

My paper adds to this literature in several ways. First and foremost, it is the first to evaluate the social and economic consequences of subsidized oral contraceptives. As already suggested, this is a question of great interest for policy makers. The fact that the subsidy focused on a group of individuals often targeted in various preventive programs makes the policy relevance even clearer. Second, the impact of a recent subsidy is arguably more relevant for the contemporary debate over contraception since most countries already have introduced the birth control pill. Third, the rich data used makes it possible to study a wide variety of different outcomes, and to examine differential effects with respect to socioeconomic background.

I begin the empirical analysis by exploiting county level panel data to examine the relationship between the subsidy and the sales of oral contraceptives. The results suggest that the subsidy increased sales by on average 5–7 percent, and there is suggestive evidence that this effect is larger for teenagers. I go on to study the impact on abortions. Using regional, temporal and cohort variation in access, I find that the subsidy reduced the abortion rate by about 8 percent. The analysis also shows tentative evidence of an effect on the birth rate, although the estimate is marginally insignificant at the 5 percent level. The estimates are robust to several sensitivity checks.

The last part of the paper uses population micro data to examine the effects on fertility, labor supply, educational attainment, and marriage. The results show that women with long-term access to the subsidy (>4.5 years) are 20 percent less likely to have a child before age 21. Consistent with the notion that access to inexpensive contraceptives matters more for financially constrained individuals this effect is found to be significantly stronger for women from poor socioeconomic background. However, I find no statistically significant effect on number of children, marriage, educational attainment, or labor supply; although some of the coefficients are relatively imprecisely estimated.

[3] In a broader context my paper is related to studies on the impact of abortion policies on women's outcomes and to a large literature on the relationship between birth control programmes and fertility in developing countries; see e.g.

The rest of this paper is structured as follows. Section 2 describes the institutional background and presents evidence on the relationship between the subsidy and the sales of oral contraceptives. Section 3 uses county level panel data to examine the impact on abortions and birth rate. Section 4 contains an analysis of economic and demographic outcomes based on population micro data. Section 5 concludes.

2 Background

Since its introduction in 1964 the birth control pill has grown to become the leading contraceptive method among young Swedish women (Santow and Bracher 1999).[4] The aim of this section is to describe the institutional setting surrounding the birth control pill. I then investigate whether the subsidy affected women's use of the pill.

2.1 Institutional setting[5]

In Sweden, oral contraceptives are sold by prescription from a doctor or midwife. The typical procedure for a young woman wishing to use the pill is to schedule an appointment at a youth clinic to meet with a physician. Youth clinics are health centers for teenagers that offer free consultation about contraception as well as associated medical examinations. Virtually all municipalities have at least one clinic. Individuals are also free to visit any private or public health care institution, but the process is still the same. If the physician deems oral contraceptives appropriate she prescribes the drug and the girl can then collect it at the state pharmacy. Parental consent to the treatment is not required. The physician is bound by the professional secrecy and if a girl does not want her parents to know about the treatment the physician cannot contact them. It is however common practice that the doctor or midwife in these cases tries to convince the girl to tell her parents.

Gruber, Ananat and Levine (2007), Gruber, Ananat, Levine and Staiger (2006), Gertler and Molyneaux (1994), Miller (2005), Prichett (1994).

[4] Almost 60 percent of Swedish women age 18–24 regularly use oral contraceptives (National Board of Health and Welfare, 2001).

[5] This section draws heavily on Csillag (1993), National Board of Health and Welfare (1994, 2001, 2005) and Västragötalandsregionen (2000)..

The question of providing financial support for oral contraceptives targeted to young women was raised in the late 1980s. The Swedish government had since 1974 been directing large resources towards various family planning policies, including a national subsidy on oral contraceptives for *all* women. However, in 1984 the discount was abolished and the price of the pill quadrupled. The new policy also required users to renew prescriptions no later than every 3 months, instead of once a year, which of course meant that using the pill would call for more planning. Immediately after the policy change the sales of oral contraceptives started to fall and many youth clinics reported that teenage girls had begun to interrupt their treatment. Following a period of decreasing teenage abortion rates, abortions started to increase.[6] These events seem to have been what motivated the new reforms.

As the first region, the municipality of Gävle started subsidizing oral contraceptives for teenagers in 1989. The reform was evaluated by the local authorities and the results showed that the consumption of oral contraceptives among teenagers increased from 42 to 60 percent after subsidy was introduced.[7] Moreover, the teenage abortion rate fell by almost 50 percent. The experiment was considered as a success and in the following years other regions therefore launched policies based on the same principle as in Gävle, meaning that the subsidy targeted specific cohorts of young women. The subsidy rate was on average 75 percent and applied to all types of oral contraceptives (National Board of Health and Welfare 1994).[8] When introduced the policy temporarily received large attention from the local media and posters with information were printed and highlighted at the youth clinics.

Table 1 contains a description of the reforms up to 1993, which is the last year for which this information is available. Observe that most of the regions which introduced the subsidy are counties, but some municipalities also participated. By the end of 1993 eight counties had still not implemented the reform.[9] From Table 1 it is clear that both the starting dates and targeted cohorts vary across regions and that only two areas

[6] Abortions have been allowed in Sweden on demand and basically free of charge since 1975 (Santow and Bracher, 1999).
[7] The evaluation consisted of a simple before and after analysis.
[8] Unfortunately, I do not have access to information about the regional specific subsidy rates.

provided the subsidy to women older than 20. In this context it is worth mentioning that the reforms did not overlap with other major changes in Swedish family policy (Björklund 2006).

Table 1 The implementation of the reforms

Regions which introduced the subsidy before 1994	Starting date	Eligible cohorts
Gävle (municipality)	Nov 01, 1989	≤ 19*
Sandviken (municipality)	Nov 30, 1989	≤ 19*
Partille (municipality)	Jan 01, 1990	≤ 20
Hofors (municipality) and Ockelbo (municipality)	Mar 31, 1990	≤ 19*
Örebro (county)	Jun 01, 1990	≤ 18*
Kristianstad (county)	Nov 29, 1990	≤ 18*
Kronoberg (county)	Jan 01, 1991	≤ 19
Blekinge (county)	Mar 01, 1991	≤ 19
Solna (municipality)	Sep 01, 1991	≤ 22
Gotland (county)	Oct 01, 1991	≤ 20*
Södermanland (county)	Jan 01, 1992	≤ 19*
Malmöhus (county) (except Malmö municipality), Västernorrland (county), Älvsborg (county), Västmanland (county), Kopparberg (county)	Jan 01, 1992	≤ 19
Värmland (county)	Mar 01, 1992	≤ 24*
Jämtland (county)	Apr 01, 1992	≤ 24
Göteborg (county) and Bohuslän (county) (except for Partille and Göteborg municipalities)	Jul 01, 1992	≤ 20
Gävleborg (county) (except for Gävle, Sandviken, Hofors and Ockelbo municipalities)	Nov 09, 1992	≤ 19*
Uppsala (county)	Mar 01, 1993	≤ 19
Malmö (municipality)	Mar 26, 1993	≤ 18
Halland (county)	Jul 01, 1993	≤ 19
Regions which did not introduce the subsidy before 1994		
Stockholm (county) (except for Solna municipality); Östergötaland (county); Jönköping (county); Kalmar (county); Göteborg (municipality); Skaraborg (county); Västerbotten (county); Norrbottens (county);		

* Individuals are eligible for the subsidy until the calendar year they turn this age.

Prior to the reforms, a full year's supply of the birth control pill sold for just below USD 100 (in 2008 year's price level). [10] Although the price might seem fairly low, for young teenage girls without own incomes the costs of obtaining oral contraceptives could very well amount to a large fraction of their budget. This situation is especially likely to be problematic for girls that for some reason can not ask their parents for money to get the pill, and is worsened by the strong regularity requirements surrounding the treatment

[9] The fact that some regions may have implemented a subsidy after 1993 introduces some complications for my analysis; an issue I will return to later in the paper.

programme. In order for oral contraceptives to provide maximum protection against pregnancy the treatment must proceed for 21 days followed by a seven day recess. If these conditions are not fulfilled, protection is immediately endangered. In fact, anecdotal evidence from youth clinics prior to the reforms suggests that many unintended pregnant girls stated that they had not been able to start a new treatment because they could not afford the pill at the day the program was scheduled to begin and therefore had been forced to postpone the treatment (National Board of Health and Welfare, 1994).

2.2 The impact on sales and consumption

Did the subsidy really increase the use of the pill? To answer this question I use information from the state pharmacy (Apoteket) on the total sales of oral contraceptives in each county and year starting in 1980. The state pharmacy is the sole provider of prescriptive drugs in Sweden, so sales should provide a good proxy for consumption. Sales are reported in terms of the annual number of (defined) daily dosages sold per woman age 15–44.[11]

Before proceeding to the econometric analysis it is useful to start the examination by graphically illustrating how sales have evolved over time. Figure 1 plots sales by year from 1980 through 2000. We can see that sales increase up until 1984, after which there is a sharp decline. This decline coincides perfectly with the abolishment of the major nationwide subsidy of oral contraceptives described earlier. The vertical line marks the starting year of the new reforms and we can see that sales starts to rise in precisely this year.

[10] The price varied slightly depending on the type of product but there was no regional variation in prices prior to the reforms.
[11] The measure takes into account varying content of hormones in different products.

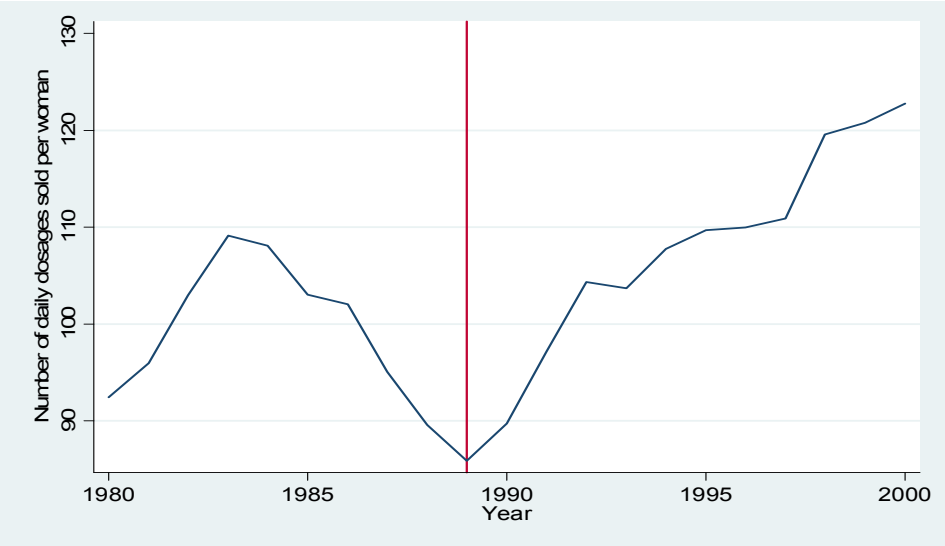

Figure 1 Number of (defined) daily dosages sold per woman by year

Notes: Vertical line marks the starting year of the reforms.

Although suggestive, one cannot be certain from the graphical evidence that no unobserved factors affecting sales occurred simultaneously as the reforms. One such factor could be increased awareness of the risks associated with HIV/AIDS. To rule out potential confounders I turn to a more formal analysis by estimating regressions of the following form

$$Sales_{ct} = \beta Policy_{ct} + \mu_c + \mu_t + \rho(\mu_c \times t) + \varepsilon_{ct} \quad (1)$$

where $Sales_{ct}$ is the (log) number of dosages sold per woman age 15–44 living in county c in year t. $Policy_{ct}$ is a dummy for the county having implemented the subsidy; μ_c is a set of county fixed effects; μ_t is a set of year fixed effects; $\mu_c \times t$ is a set of linear county trends.

This is a standard difference-in-differences specification where the county-specific fixed effects take into account all persistent county characteristics affecting sales, such as permanent differences in fertility, access to family planning services, etc. Similarly, year fixed effects control for all time-varying factors that affect sales in different counties in the same way, e.g. changes in the national family policy. Linear trends

control for smoothly evolving factors within each county. The model assumes that no unobserved regional-specific events affecting sales happened at the same time as the introduction of the subsidy. A total of 19 counties observed from 1980 through 1993 are included in the analysis.[12]

The results from the regressions can be found in Table 2. Column (1) presents estimates excluding linear county trends, i.e. $\rho = 0$. The reported standard errors are robust to serial correlation at the county level. The coefficient suggests that the subsidy increased sales by just below 7 percent. The estimate is highly significant. Nevertheless, one should bear in mind that, because there are rather few counties, the standard errors may understate the standard deviation of the estimator (Bertrand, Duflo and Mullainathan 2004). Column (2) shows that the coefficient is robust to including linear county trends.

One potential concern is that regions which introduced the subsidy even in its absence would have experienced increased sales. To investigate this I have run regressions exploring the relationship between future subsidies (t+2 years) and current sales. If causality runs from the subsidy to sales then one should find that future subsidies do not affect current sales, conditional on current policy. [13] The results are displayed in column (3). As can be seen, the coefficient on current policy is still significant and the estimate virtually unchanged. In contrast, the coefficient on future policy is insignificant.

[12] The following counties are excluded from the analysis due to limited availability of data: Älvsborgs län, Bohusläns, Kristianstads län, Malmöhus län, Skaraborgs län. Note also that I cannot use information for later years since some regions may have introduced the subsidy after 1993.

[13] To be specific, $\text{Policy}_{(t+2)}$ is a dummy that switches from zero to one two years before the implementation of the subsidy and stays on. The reason for choosing t+2 years is as follows. In counties where the policy was introduced late during the year I have coded the reform to the subsequent year, implying that I do not control perfectly for current policy. This means that the estimate could capture some of the effect of current policy. Using t+2 alleviates this potential problem. Note that the results are similar when using other years than t+2. This "falsification" test has previously been used by Lochner and Moretti (2004), Black, Devereux and Salvanes (2004) and Dahl (2005) to investigate the exogeneity of compulsory schooling laws.

Table 2 OLS estimates of the effect of the subsidy on the sales of oral contraceptives

	Dependent variable: Log(Number of dosages sold per woman)		
	(1)	(2)	(3)
Policy	.068	.047	.044
	(.025)	(.018)	(.017)
$Policy_{(t+2)}$	-	-	.025
			(.021)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Linear county trends	No	Yes	Yes
N	266	266	266

Notes: Standard errors robust to heteroscedasticity and serial correlation at the county level (19 cells) in parenthesis. The sample consists of a panel of all Swedish counties (except Älvsborgs län, Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län), observed from 1980 through 1993.

Note that these regressions estimate the average effect of the subsidy across all cohorts and therefore cannot tell how much of the effect is due increased pill use among teenagers. To perform a cohort specific analysis I make use of a survey called ULF (Undersökningen av Levnadsförhållanden). The survey asks women age 16–84 whether they have consumed oral contraceptives within the last two weeks prior to the survey date. The question was asked in one round before the reforms and one round "after" (1980/81 and 1996/97). The survey consists of a (cross-sectional) random sample of about 3,500 Swedish women and the sample size net of attrition is sufficiently large to disaggregate the data by age cohort.[14] [15] Statistics Sweden compiled the data on my behalf.

In the first round, 25.8 percent of 16–20 year olds stated that they had consumed oral contraceptives within the last two weeks. The same figure for 21–24 year olds is 35.8 percent, and 25.2 percent for 25–30 year olds. All cohorts increased their use of the pill up to the second round where the corresponding numbers are: 35, 45.9 and 30.6 percent. This means that consumption grew by 36 percent for the youngest cohort, 28 percent for

[14] Attrition in ULF is generally around 25 percent.
[15] Unfortunately, sample size restrictions, in combination with the fact that some regions may have implemented the reforms after 1993, prevents me from disaggregating the data by region.

individuals age 21–24, and 21 percent for 25–30 year olds. Thus, the increase in consumption use was indeed largest in the eligible cohorts.[16]

Taken together, I believe that the results presented in this sub-section provide credible evidence that the subsidy actually did increase the use of oral contraceptives among young women.

3 The impact on abortions and birth rate

Having established that the subsidy increased pill use I now investigate whether access to it affected the likelihood that a woman had an abortion, as well as the consequences for the birth rate. The analysis makes use of publicly available data on all legal abortions performed in Swedish counties from 1985 and onwards. This information was obtained from the National Board of Health and Welfare (Socialstyrelsen).[17] The only related study in any field that I am aware of is Ananat and Hungerman (2007) who use cross-state and cross-time variation in the pill's diffusion at the time when it was introduced in the U.S. to explore whether access to the pill affected the risk of a young unmarried woman having an abortion. When analyzing the birth rate I use aggregated data from the IFAU-database.[18] My baseline estimates are obtained from the following model

$$Outcome_{cat} = \gamma Policy_{cat} + \theta_c + \theta_a + \theta_t + \theta_{ca} + \theta_{ct} + \theta_{at} + v_{cat} \quad (2)$$

where *c*, *a*, and *t* denote county, age cohort (five year intervals) and year, respectively. The outcome is either the (log) abortion rate or the (log) birth rate. The θ's represents fixed effects for county, age cohort, year, and all of their interactions.

This model is very flexible and takes into account most potential confounders. The fixed effects control for nationwide changes in the outcome over time, time-invariant

[16] Of course, this can be due to a range of different factors not related to the reforms and the results should therefore be interpreted with caution. The most obvious concern is that the Swedish women may have brought forward their sexual debut. However, the average age at first intercourse has been stable around age 16 since the 1960s (Forsberg 2005).

[17] The data can be found on the following web-page: http://www.socialstyrelsen.se/

county characteristics, permanent differences across cohorts, within-county and within-cohort shocks, as well as the fact that permanent county-specific differences could matter more for different cohorts. The identifying assumption is that there should be no unobserved county specific shocks occurring simultaneously as the introduction of the subsidy which also affect the *relative* outcomes between cohorts.

The results are shown in Table 3. As a benchmark, I start by providing evidence on the impact of the subsidy on *teenage* abortions, relying only on cross-county and cross-time variation in the introduction of the policies. The specification is analogous to equation (1) and to the model used by Ananat and Hungerman (2007), who find that access to the pill lowered the teenage abortion rate from 27 abortions per every 1000 women to 22, implying a decrease of about 18 percent. Column (1) shows that the subsidy decreased the number of teenage abortions by about 6 percent.

The fact that my estimates are smaller in magnitude than those presented by Ananat and Hungerman (2007) is natural since the introduction of the birth control pill in the US in the 1960s and 1970s is likely to have had larger consequences for the use of oral contraceptives. For instance, compared to my result showing that access to the subsidy increased sales by about 7 percent (cf. Table 2), Goldin and Katz (2002) finds that more lenient state regulations regarding minors was associated with 33–40 percent greater pill use by young unmarried women.

As already mentioned, column (1) assumes that no other events affecting the outcome occurred in the same year as the subsidy was introduced. Column (2) relaxes this assumption by using older not eligible cohorts as control groups. Even if an unobserved shock occurred simultaneously as the subsidy this will not bias the estimates as long as it does not also affect the relative abortion rate between different cohorts. Column (2) shows that the estimates are similar to those in column (1). The estimate is statistically significant and the coefficient suggests that the abortion rate is reduced by about 8 percent.

Last, by the same argument as earlier, column (3) tests the exogeneity of the subsidy by investigating the relationship between future subsidies and the current abortion rate.

[18] The database is described in detail in Section 4.

As can be seen, the coefficient on future policy is close to zero and insignificant, suggesting that the policies indeed were exogenous.

Table 3 OLS estimates of the effect of the subsidy on the abortion rate

	Dependent variable:		
	Log(Teenage abortion rate)	Log(Abortion rate)	Log(Abortion rate)
	(1)	(2)	(3)
Policy	–.060	–.080	–.077
	(.031)	(.033)	(.036)
$Policy_{(t+2)}$	-	-	–.007
			(.032)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Cohort fixed effects	-	Yes	Yes
N	171	684	684

Notes: Robust standard errors in parenthesis. Additionally, the standard errors in column (1) are robust to serial correlation at the county level. The sample consists of a panel of all Swedish counties (except Älvsborgs län, Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län), observed from 1985 through 1993. The regressions in column (1) cover the teenage abortion rate and the unit of observation are all teenagers age 15–19, in a given county and year. The regressions in column (2) cover the abortion rate for all women in the following age cohorts: 15–19, 20–24. 25–29, 30–34, and the unit of observation are all women in each cell. In addition to county, year and cohort fixed effects, columns (2) and (3) include all interactions between these variables.

Table 4 examines the consequences of the subsidy for the birth rate.[19] The empirical approach is identical to the one used when analyzing the abortion rate. Column (1) shows results for the teenage birth rate. We can see that the subsidy decreased the teenage birth rate by about 7.5 percent. The estimate is however not significant. Column (2) uses older cohorts as control groups within each county-by-year cell. The estimate is basically identical to that in column (1) and the precision has increased, a finding that is natural since the number of observations has increased. Still, the coefficient is insignificant at the 5 percent level (p-value = .075). Column (3) tests the exogeneity of the policy by adding a dummy for future policy. As earlier, this coefficient is close to zero and insignificant.

[19] For comparison purposes I include the same set of counties and cohorts in the analysis as in Table 3. Note however the results are virtually identical to using all counties and cohorts.

Table 4 OLS estimates of the effect of the subsidy on the birth rate

	Dependent variable:		
	Log(Teenage birth rate)	Log(Birth rate)	Log(Birth rate)
	(1)	(2)	(3)
Policy	–.076	–.075	–.090
	(.054)	(.042)	(.045)
$Policy_{(t+2)}$	-	-	.033
			(.040)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Cohort fixed effects	-	Yes	Yes
N	171	684	684

Notes: Robust standard errors in parenthesis. Additionally, the standard errors in column (1) are robust to serial correlation at the county level. The sample consists of a panel of all Swedish counties (except Älvsborgs län, Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län), observed from 1985 through 1993. The regressions in column (1) cover the teenage birth rate and the unit of observation are all teenagers age 16–19, in a given county and year. The regressions in column (2) cover the birth rate for all women in the following age cohorts: 16–19, 20–24. 25–29, 30–34, and the unit of observation are all women in each cell. In addition to county, year and cohort fixed effects, columns (2) and (3) include all interactions between these variables.

4 Consequences for socioeconomic outcomes, fertility and marriage

This section examines the subsidy's effect on women's socioeconomic outcomes, fertility and marital status. The analysis exploits rich micro data covering the entire Swedish population age 16–65 during the period 1985–2004.[20] One part of the database includes annual information on standard individual characteristics (earnings, place of residence, etc). It also contains several registers with educational information, as well as a "multi-generation" register linking children to their biological parents.

My sample consists of all Swedish women born during the period 1965–1975. The reason for this restriction is that including older cohorts increases the likelihood that some individuals may have left their homes at the time when I observe them, enhancing the risk of both measurement error and selective sorting. Furthermore, younger cohorts cannot be used since I only have detailed knowledge about the reforms up until 1993 and wish to avoid the possibility that later cohorts in the control regions may have been

[20] The information is taken from the IFAU-database and was originally collected by Statistics Sweden.

exposed.[21] For most cohorts, region of residence is defined according to where the girl lived at age 16. Individuals born 1965–1968 are assigned a residential area depending on where they lived in 1985.

All subjects are linked to their biological parents and information is added on each parent's education and earnings in 1985. I then add information on the birth dates of the subjects' children.[22] Using place of residence in combination with the subject's birth date I construct a variable measuring the cumulative length of exposure to the subsidy, starting at age 14 and ending when she no longer is eligible.

The empirical analysis focuses on several types of outcomes: fertility, marriage, educational attainment, and labor market status. Teenage childbearing is defined as having the first child no later than age 20. I also study whether the woman has completed university or high school. My data contain information on a wide range of labor market and income variables as well: annual earnings, employment status, welfare take-up, and disposable income.

All outcomes are recorded in 2004 when the subjects are 29–39 years old. This avoids the possibility that some individuals may not have completed their education.[23] Table A 1 contains a detailed description of how the variables have been constructed and from which registers the information has been collected. Table A 2 contains summary statistics.

4.1 Main results

The empirical strategy takes advantage of cross-regional and cross-cohort variation in access to the subsidy to identify the parameters of interest. I estimate regression models of the following form

$$(3) \qquad Outcome_{ibm} = \alpha_0 + Exposure_{bm}\alpha_1 + X_i'\alpha_2 + \lambda_b + \lambda_m + \delta(\lambda_m \times b) + v_{ibm}$$

[21] I know that some regions did in fact introduce the subsidy after 1993, although I have no information on what cohorts were eligible or the exact starting date.
[22] Note that the multi-generation register contains information on the woman's number of children and her children's birth dates even though the children themselves may be too young to be included in the population sample of the database.
[23] The age restrictions imply that the estimates will not capture the impact of the subsidy on completed fertility.

where the outcome is indexed for individual *i* in birth cohort *b* from municipality *m*; $Exposure_{bm}$ is a measure of the cumulative exposure to the subsidy; X_i is a vector of background characteristics; λ_b and λ_m represents year of birth and municipality specific fixed effects, respectively; $\lambda_m \times b$ represents municipality specific trends. The model ignores regional and cohort differences which are absorbed by the fixed effects. Thus, the identifying assumption is that once that I condition on region, cohort, and possibly also background characteristics, exposure should not be correlated to the error term, i.e. $E[v_{ibm} | Exposure_{bm}, X_i, \lambda_b, \lambda_m, (\lambda_m \times b)] = E[v_{ibm} | X_i, \lambda_b, \lambda_m, (\lambda_m \times b)]$.

The key variables of interest are four dummies indicating the cumulative exposure to the subsidy. The reference group is individuals with no exposure. I also present results from identical models except that exposure is defined linearly. All regressions include fixed effects for municipality of residence and year of birth. In addition, I control for each parent's earnings and age, with dummies for each parent's highest completed level of education (five levels), missing information on education or earnings, municipality specific linear trends, and immigrant status. All standard errors are clustered at the municipality level to take into account possible serial correlation (286 cells).[24]

To conserve space, I do not report estimates for the control variables, but it is worth mentioning that these are all significant and display expected signs: having highly educated, as well as older parents, means a lower probability of becoming a teenage mother, fewer children, more schooling, higher earnings, a lower probability of being non-employed and receiving welfare, and higher disposable incomes. The same is true for children to high income parents.

Table 5 contains results for fertility and marital status. I start by asking whether the subsidy affected family size. We can see in column (1) that the coefficients are monotonically decreasing in exposure length, suggesting a dose-response relationship. Still, the F-statistic which tests the null hypothesis that the coefficients on exposure are jointly equal to zero is insignificant.

[24] I have also experimented with accounting for intra-group correlation at the municipality×cohort level with similar results (cf. Moulton 1990).

Column (2) displays results for the probability of becoming a teenage mother. We can see that women exposed to the subsidy for more than 54 months are on average about 20 percent (–.013/.067) less likely to become teenage mothers, although the effect of shorter exposure is more moderate. Also for this outcome there are clear indications of a dose-response relationship. The F-statistic rejects the null hypothesis of no joint effect. Similar conclusions can be drawn from the linear measure of exposure in Panel B. On average, each additional year of exposure reduces the probability of teenage motherhood by .3 percentage points. This implies that exposure for 5 years lowers the probability of teenage childbearing by 22 percent ((.003*5)/.067). These estimates are comparable to the results presented by Bailey (2006) who finds that the probability of experiencing the first birth by age 22 fell by 16 percent in states that had relaxed restrictions on older teens' eligibility to the pill.

The impact of long-term access is significant and it is relevant to ask whether the results make sense. In this context it is worth mentioning that these regressions cannot separate between age at first exposure and length of exposure: a cohort that experienced long-term exposure is also a cohort where the subjects were exposed early in life. If easier access to contraceptives is more important in the early teens this could potentially explain the relatively large effects.

Last, column (3) examines the impact on the probability of marriage. This effect is *ex ante* ambiguous since better planned births may both decrease the likelihood of (shot-gun) marriages as well as improve the quality of later marriage. The F-statistic in column (3) shows no significant effect of exposure to the subsidy on the probability of being currently married, although the coefficient on exposure for 37–54 months is significantly negative.

Table 5 OLS estimates of the effect of the subsidy on fertility and marital status

	Dependent variable:		
	Number of children	Pr (Teenage mother)	Pr (Currently married)
	(1)	(2)	(3)
Panel A			
Exposed 1–18 months	.004	.003	–.005
	(.009)	(.002)	(.004)
Exposed 19–36 months	–.010	–.003	–.008
	(.011)	(.003)	(.005)
Exposed 37–54 months	–.015	–.006	–.013
	(.013)	(.003)	(.006)
Exposed > 54 months	–.017	–.013	–.013
	(.024)	(.005)	(.009)
P-value of F-statistic	.447	.000	.269
Panel B			
Years of exposure	–.005	–.003	–.003
	(.004)	(.001)	(.002)
Municipality fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Mean of dependent variable	1.452	.067	.391
N	588,367	588,367	588,367

Notes: The sample consists of all women born 1965–l975 All regressions controls for each parent's earnings and age, and with dummies for each parent's education (five levels), for missing information on education and earnings, and immigrant status. The regressions include linear municipality trends. The outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust to heteroscedasticity and serial correlation at the municipality level (286 cells) are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure are jointly zero. See Table A 1. for definitions of the included variables.

Next, I examine the impact of the subsidy on socioeconomic outcomes. Columns (1) and (2) in Table 6 provide results for educational attainment. We can see that exposure to the subsidy is neither statistically significantly related to the probability of graduating from high school, nor to the likelihood of completing university. The F-statistics, as well as the individual coefficients, are all insignificant. I have also run regressions using (imputed) years of schooling as dependent variable with similar results. This conclusion holds for labor supply as well: columns (3) – (6) find no statistically significant effect on the probability of being non-employed, annual earnings, the probability of receiving welfare, or disposable income; however a few point estimates are just marginally insignificant for disposable income.

Given that I find a negative impact on the probability of teenage childbearing, it might at first glance seem surprising that there is no significant effect on socioeconomic outcomes. Furthermore, Bailey (2006) demonstrates that access to the pill before age 22, at the time when it was introduced in the US, raised the number of hours worked. Still, the results should be interpreted having in mind that some of the coefficients are fairly imprecisely estimated.

It is also relevant to ask how the estimates reconcile with past research on the consequences of early childbearing. To answer this question, consider the following thought experiment: if the entire (potential) effect of access to the pill on education is mediated through an effect on teenage childbearing, what would the results in previous studies imply for my estimates? The most credible Swedish study to date is Holmlund (2005) who uses within-family variation in childbearing decisions and shows that teenage motherhood decreases the average length of schooling by .59 years. Observe that my analysis is based on comparing outcomes across cohorts, while Holmlund's analysis is executed at the individual level. Taken together, this paper and Holmlund's results show that 3 out of 1000 individuals in the total population potentially prolonged their education by .59 years (cf. Table 5, Panel B, Column 2). This is not a particularly strong effect, and one that probably would be difficult to detect in the data.

Table 6 OLS estimates of the effect of the subsidy on socioeconomic outcomes

	Dependent variable:					
	Pr (High school graduate)	Pr (University graduate)	Pr (Non-employed)	Log (earnings)	Pr (Welfare)	Log (Disposable income)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Exposed 1–18 months	.001	.001	.001	–.003	.001	.001
	(.003)	(.004)	(.003)	(.011)	(.002)	(.004)
Exposed 19–36 months	.001	–.001	–.000	.007	.001	.005
	(.003)	(.005)	(.004)	(.012)	(.002)	(.004)
Exposed 37–54 months	–.001	.001	–.002	.008	.002	.008
	(.003)	(.007)	(.005)	(.014)	(.002)	(.005)
Exposed > 54 months	.002	–.009	–.007	.013	.008	.018
	(.007)	(.013)	(.009)	(.027)	(.004)	(.011)
P-value of F-statistic	.894	.662	.906	.867	.205	.483
Panel B						
Years of exposure	–.000	–.000	–.001	.003	.000	.002
	(.001)	(.002)	(.001)	(.004)	(.001)	(.001)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.931	.425	.176	7.120	.089	7.084
N	587,503	587,503	587,503	517,733	584,890	585,744

Notes: The sample consists of all women born 1965–l975. All regressions controls for each parent's earnings and age, and with dummies for each parent's education (five levels), for missing information on education and earnings, and immigrant status. The regressions include linear municipality trends. The outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust to heteroscedasticity and serial correlation at the municipality level (286 cells) are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure duration are jointly zero. See Table A 1. for definitions of the included variables.

In summary, the results suggest that exposure to the subsidy significantly lowers the probability of teenage motherhood. However, I find no statistically significant effect on number of children, marriage, educational attainment, labor supply, or welfare take-up. Next, I assess the robustness of the estimates.

4.2 Robustness checks

My identification strategy is based on several assumptions. First, the timing of the reforms should not be correlated with regional trends. Second, individuals should not respond to the policy by selectively moving. Although it is unlikely that families would change their residential area because of the subsidy I do provide some evidence on this issue by investigating what happens to the estimates when removing some key covariates. Parents' education and earnings is perhaps the variables most likely to be associated with selective relocation. If unobserved factors are at least equally as important as these observed characteristics, dropping the latter can provide insights as to whether omitted factors may be driving the results. If I find the estimates sensitive to removing covariates, then one might suspect that omitted variables are important as well. By the same argument, removing the municipality-specific trends can give information on the likelihood of differential trends biasing the estimates.

Table 7 presents results where I successively remove covariates. To conserve space I only report estimates for the linear measure of exposure, but the results are similar when using dummies. Reassuring is that the coefficients are not sensitive to removing controls for parents' education and earnings. In a few cases the results are somewhat sensitive to dropping linear trends. For instance, for the probability of graduating from university and logged disposable income the coefficients become almost significant at the 5 percent level, and for number of children, the coefficient switches sign but stay insignificant. For the other outcomes, the coefficients are however quite stable and the overall conclusions still hold. The results highlight the importance to control for trends to account for slow-moving economic and demographic changes in each region.

Table 7 Consequences of removing covariates

Dependent variable:	Change in specification:			
	Estimate as in Tables 5 and 6	Removing controls for each parent's education	+ Removing controls for each parent's earnings	+ Removing municipality trends
	(1)	(2)	(3)	(4)
Number of Children	–.005	–.005	–.005	.001
	(.004)	(.003)	(.003)	(.002)
Pr(Teenage mother)	–.003	–.003	–.003	–.001
	(.001)	(.001)	(.001)	(.000)
Pr(Currently married)	–.003	–.003	–.003	–.001
	(.002)	(.002)	(.002)	(.001)
Pr(High school graduate)	–.000	–.000	–.000	–.000
	(.001)	(.001)	(.001)	(.000)
Pr(University graduate)	–.000	–.000	–.000	.003
	(.002)	(.002)	(.002)	(.001)
Pr(Non-employed)	–.001	–.000	–.001	–.001
	(.002)	(.001)	(.001)	(.001)
Log(earnings)	.003	.003	.003	.002
	(.004)	(.004)	(.004)	(.002)
Pr(Welfare)	.000	.000	.001	–.000
	(.001)	(.001)	(.001)	(.000)
Log(Disposable income)	.002	.002	.003	.003
	(.001)	(.001)	(.002)	(.001)

Notes: The table reports the coefficient on "Years of exposure". The sample consists of women born 1965–l975. All regressions control for municipality and year of birth fixed effects, each parent's age, and immigrant status. All outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust to heteroscedasticity and serial correlation at the municipal level (286 cells) are shown in parenthesis. See Table A 1. for definitions of the included variables.

4.3 Differential effects

Since there are no strong indications that the results are driven by omitted factors I continue the analysis by examining whether the effect varies by family background characteristics. Table 8 displays estimates for the linear measure of exposure; however the results are not sensitive to how exposure is defined. Each cell represents a separate regression. The focus is on education and earnings. "Academic family" is defined as having at least one parent having completed at least theoretical/preparatory high school. "Non-Academic family" is defined as both parents having completed at most vocational high school education. Similarly, "High-income family" is defined as having at least one parent above the median in the earnings distribution (defined separately for mothers

and fathers). A "Low-income family" is a family where both parents are below the median in their respective earnings distribution.

We can see that the effect of exposure to the subsidy on teenage childbearing is significantly more negative for women from "Non-Academic" families, and there is also a tendency for stronger effects in "Low-income" families. These findings are consistent with the idea that access to inexpensive contraceptives may have more profound effects for financially constrained individuals. There are also indications of a negative effect on the likelihood of marriage for women from less educated families. However, I do not find any evidence of differential effects for the other outcomes.

Table 8 Differential effects with respect to family background

Dependent variable:	Change in sample:				
	Estimate as in Tables 5 and 6	Academic Family	Non-Academic Family	High-Income Family	Low-Income Family
	(1)	(2)	(3)	(4)	(5)
Number of Children	–.005	–.004	–.004	–.004	–.007
	(.004)	(.004)	(.005)	(.004)	(.007)
Pr(Teenage mother)	–.003	–.001	–.004	–.002	–.004
	(.001)	(.001)	(.001)	(.001)	(.002)
Pr(Currently married)	–.003	–.002	–.005	–.002	–.004
	(.002)	(.002)	(.002)	(.002)	(.003)
Pr(University grad.)	–.000	.001	–.003	–.001	.003
	(.002)	(.002)	(.003)	(.002)	(.003)
Pr(High school grad.)	–.000	.000	.000	–.000	–.000
	(.001)	(.000)	(.002)	(.001)	(.002)
Pr(Non-employed)	–.001	.000	–.001	–.002	.000
	(.001)	(.001)	(.002)	(.001)	(.002)
Log(earnings)	.003	.006	.005	.008	.001
	(.004)	(.005)	(.005)	(.004)	(.007)
Pr(Welfare)	–.004	–.000	–.000	–.000	.001
	(.003)	(.001)	(.001)	(.001)	(.001)
Log(Disposable income)	.002	.003	.003	.003	.001
	(.001)	(.002)	(.002)	(.002)	(.003)

Notes: Standard errors robust to heteroscedasticity and serial correlation at the municipality level (286 cells) in parenthesis. The table reports the coefficient on "Years of exposure". The sample consists of women born 1965–l975. Wherever appropriate, the regressions controls (linearly) for each parent's earnings and age, with dummies for each parent's education (five levels), for missing information on education and earnings, and immigrant status. All regressions include municipality and year of birth fixed effects, and linear municipality trends. All outcomes are observed in 2004. Parental characteristics are measured in 1985. "Academic family" is defined as having at least one parent who has completed at least theoretical/preparatory high school. "High income family" is defined as having at least one parent above the median in each parent's earnings distribution. See Table A 1. for definitions of the included variables.

5 Concluding remarks

While most countries are committed to reducing unintended childbearing, and thereby improve the well-being of both mothers and children, there is little consensus on the efficiency of different policies.

This paper examines the consequences a series of Swedish policy changes beginning in 1989 where different regions started subsidizing the birth control pill. The reforms were significant and applied to all types of oral contraceptives. My identification strategy takes advantage of temporal, regional and cohort variation in the implementation of the subsidy, generating plausibly exogenous variation in access.

Using county level panel data I find that the subsidy increased sales by some 5–7 percent and reduced the abortion rate by about 8 percent. The analysis also shows tentative evidence of an effect on the birth rate, although the coefficient is only significant at the 10 percent level. The estimates are robust to several sensitivity checks. Using rich population micro data I also examine the effect on socioeconomic outcomes, fertility, and marital status. The results show that women with long-term access to the subsidy are 20 percent less likely to have a child before age 21; an effect that is significantly stronger for women from poor socioeconomic background. I find no statistically significant effect on number of children, marriage, educational attainment, or labor supply, although some of the estimates are rather imprecise.

From a policy perspective, the results suggest that subsidizing oral contraceptives may be a fruitful way to both reduce abortions as well as the incidence of teenage childbearing. In this context, it is relevant to ask whether the results in this paper can be extended to other settings. 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 are extensive earnings-related parental leave benefits and parents have the right to reduce work hours to 75 percent. There is also a flat rate child allowance.

With these policies in mind it is perhaps not so surprising to find no significant effects on long-term socioeconomic outcomes. It is conceivable that Sweden's generous

family policy compensates women for the potential detrimental effects of having an unplanned child. Thus, it is possible that the consequences of introducing a similar reform as the one studied in this paper could be more far-reaching in other countries.

Appendix

Table A 1 Definitions of key variables and data source

Variable	Definition	Data source
Teenage mother	Indicator = 1 for having first the child no later than age 20; 0 otherwise.	Multigeneration register
Number of children		Multigeneration register
High school	Indicator variable = 1 for highest completed level of education being high school; 0 otherwise.	Employment register
University	Indicator variable = 1 for highest completed level of education being university; 0 otherwise.	Employment register
Non-employed	Indicator variable = 1 for employment status "not employed" on November 1, 2004.	Employment register
Earnings	Labor related income (including self-employment) in hundreds of SEK.	Employment register
Welfare	Indicator variable = 1 for the incidence of welfare; 0 otherwise.	LOUISE
Disposable income	After tax income plus all transfers recieved.	LOUISE
Currently married	Indicator variable = 1 for currently married; 0 otherwise.	LOUISE
Parental characteristics		
Education	Indicator variable = 1 for highest completed level of education; 0 otherwise (5 levels: compulsory school, high school ≤ 2 years, high school > 2 years, university ≤ 2 years, university > 2 years).	Employment register
Earnings	Labor related income (including self-employment) measured in hundreds of SEK.	Employment register

Notes: Parental characteristics are observed in 1985. All other variables are observed in 2004, if not indicated otherwise.

Table A 2 Summary statistics

Variable	Mean	Standard deviation
Teenage mother	.067	.250
Number of children	1.452	1.151
High school	.931	.254
University	.423	.494
Non-employed	.176	.381
Log(earnings)	7.120	1.161
Welfare	.089	.285
Log(Disposable income)	7.084	.463
Currently married	.391	.488
Exposed 1–18 months	.072	.258
Exposed 19–36 months	.072	.259
Exposed 37–54 months	.049	.256
Exposed > 54 months	.023	.216
Years of exposure	.650	1.483
Mother		
Age (in 1985)	41.581	6.148
Compulsory school	.419	.493
High school ≤ 2 years	.344	.475
High school > 2 years	.052	.222
University ≤ 2 years	.090	.286
University > 2 years	.094	.292
Earnings	595.61	406.965
Father		
Age (in 1985)	44.591	7.080
Compulsory school	.416	.493
High school ≤ 2 years	.249	.433
High school > 2 years	.153	.360
University ≤ 2 years	.068	.252
University > 2 years	.114	.318
Earnings	1079.96	746.11

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