Swedish Institute for Social Research (SOFI)

Stockholm University

PUTTING TEENAGERS ON THE PILL:
THE CONSEQUENCES OF SUBSIDIZED CONTRACEPTION

by

Hans Grönqvist

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ABSTRACT

This paper investigates the consequences of a series of Swedish policy changes in which several regions in the 90s introduced heavily subsidized oral contraception for teenagers. The results reveal that access to the subsidy significantly increased the use of the pill as well as reduced the abortion and teenage birth rate. The decline in teenage births was especially strong among financially constrained youths. The estimates are precise enough to rule out even moderate effects on the birth weight of the children to the exposed mothers. Despite the documented improvements in women's outcomes, the analysis reveals that the monetary costs of the subsidy substantially exceed its measurable social benefits.

JEL: J13
Keywords: Family planning; Abortions; Teenage childbearing;

I. INTRODUCTION

Unintended childbearing is both frequent and widespread, especially among youths. In the United States, more than 80 percent of all teenage pregnancies are unplanned (Thomas 2012). It is well-known that unintended pregnancy strongly predicts a range of adverse outcomes among the parents and children involved, including lower educational attainment and labor force participation of the mothers as well as worse infant health and increased criminal involvement of the children (Maynard 1996). Moreover, unwanted pregnancies, which represent a subset of unintended pregnancies, account for approximately 1.5 million abortions annually in the United States alone (Institute of Medicine 1995). Given these reasons it is hardly surprising that many countries have adopted policies to combat unintended pregnancy.[1] One of the most popular is publicly funded family planning services, which often involve subsidized contraception as a key element. Yet, in many countries there is also a fierce debate over the rationale for the government to sponsor these kinds of programs. Besides moral issues, one of the main controversies is over cost-effectiveness. In fact, despite the large investments made in family planning programs little is known about how successfully they accomplish its goal and, with a few exceptions (e.g. Kearney and Levine 2009), the policies that actually have been studied have not been implemented in a way that permits a proper evaluation.

This paper contributes to this policy debate by investigating the impact of a series of Swedish policy changes in which several regions in the early 1990s began subsidizing the birth control pill for teenagers. The subsidy rate was on average 75 percent and applied to all types of oral contraception. In contrast to most family planning programs the reform was introduced in different regions at different points in time and targeted specific age groups of young women. This particular institutional feature creates a unique possibility to disentangle the impact of the reform from that of other factors. Drawing on data from multiple sources, this paper considers the

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[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. Kearney (2008) summarizes different family planning programs targeted to teenagers.

consequences of the subsidy on outcomes along several dimensions, including: contraceptive use, abortions, fertility as well as the birth weight of the next generation of children born to the women who were exposed to the reform.

The main argument for subsidizing oral contraception for teenagers is that young girls lack stable incomes and therefore are more likely to interrupt their use of the pill. Since oral contraception needs to be consumed at regular intervals for it to provide maximum protection, even slight deviations from the schedule substantially 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 price of getting the pill. In the last case subsidizing the pill will not lead to a behavioral response but only result in an income transfer from the government to the individual. Furthermore, as pointed out by Kearney and Levine (2009), having access to inexpensive contraception could mean that women raise their level of sexual activity, increasing the likelihood of a pregnancy. This makes the net effect on fertility ambiguous. A lower relative price for the pill can also affect abortions if women substitute between the pill and other less efficient contraceptive technologies in order to avoid unwanted births.

There are several reasons for why reduced costs of contraception could matter also for the next generation. Early fertility has been linked to worse child outcomes, including low birth weight (e.g Institute for Medicine 1995). If the subsidy postponed childbearing it is possible that this translates into improved child outcomes. To the extent that the subsidy also lowers total fertility, it is conceivable that parents substitute away from quantity of children towards quality (e.g. Becker and Lewis 1973) by investing more in e.g. prenatal care.

Although the reform is different than the one considered in the present paper, the topic is related to recent studies highlighting the role of the introduction of the birth control pill in the United States in the 1960s and 1970s for women's well-being. Goldin and Katz (2002) show credible evidence that state level changes in age of majority laws, which increased access to the

pill, led to higher age at first marriage among young unmarried women. Bailey (2006) demonstrates that plausible exogenous variation in state consent laws postponed childbearing and increased female labor supply. Similar results are documented in Guldi (2008). Bailey (2010) takes advantage of changes in state regulations concerning contraceptive sales from 1873 to 1965 (Comstock laws) and shows that oral contraception accelerated the reduction in marital fertility rates. Ananat and Hungerman (2012) investigate the impact on the children born to the women that were granted access to the pill. They present convincing evidence that pill increased the fraction of children born to poor households and reduced the probability of low birth weight and short-term fertility. There is further suggestive evidence that the pill decreased abortions, but the estimates are too imprecise to be conclusive.

The legalization of oral contraception in the United States can be considered as an event that substantially lowered the cost of using the pill, counting also non-monetary costs like reduced social stigma. It is however important to be aware of that the institutional context in which women in western societies make their fertility decisions have undergone large changes since the 1960s, including increased female labor force participation and enrollment in higher education, new contraceptive technologies, the emergence of the HIV/AIDS virus etc. It is therefore unclear to what extent the results from this historical change extend to contemporary policies.

A more recent and closely related policy is studied by Kearney and Levine (2009), who examine the consequences of state-level Medicaid policy changes that expanded eligibility for family planning services to women with higher incomes and to Medicaid clients whose benefits would expire otherwise. This policy can be seen as a general subsidy on family planning services that include contraceptive methods as well as medical examinations and laboratory tests for women with income above the Medicaid cap. The results indicate that the reform led to a nine

percent decrease in births to eligible women age 20–44; a finding the authors attribute to more frequent contraceptive use.[2] [3]

The present paper contributes to the literature in several ways. It is the first to exploit a research design that allows for credible identification of the consequences of presumably the most crucial element of any family planning policy: publicly funded oral contraception. The policy considered in this paper is specifically about oral contraception, as opposed to family planning services in general, that involve a package of components including the full range of contraceptive methods. Separating the role of the pill from other factors is important as the pill represents the most efficient contraceptive technology and one of the most frequently used.[4] It is also one the few methods that allow women to exercise control over their own fertility. The fact that the subsidy targeted teenagers, a high-risk group often highlighted in the public debate, adds to the policy relevance of the paper. Another novelty is that the data used allow me to investigate whether the response to the subsidy is different in socioeconomically deprived groups. There are good reasons to suspect that financially constrained individuals should respond stronger to the subsidy. The paper also contributes by documenting the "first-stage" relationship between expanded access to the pill and oral contraceptive use. This provides a way to corroborate the main findings as well as to make it possible to calculate some of the monetary costs imposed on the society by the subsidy.

Besides being a different and more recent type of reform, another key difference between the reform studied in this paper compared to past investigations of the legalization of the birth control pill in the United States is that it applied to a country with a distinct institutional context. Sweden is well-known for its extensive welfare state which encompasses a number of measures to assist parents and their children. Child care is heavily subsidized and local governments are

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[2] Paton (2002) examines the impact of restrictions in family planning services to youths under age 16 in the United Kingdom. The results show that the restrictions did not affect pregnancies or abortions.

[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. Ananat, Gruber and Levine (2007), Ananat, Gruber, Levine and Staiger (2009), Gertler and Molyneaux (1994), Miller (2005), Prichett (1994).

[4] .

obliged to provide care to cover the time that parents spend on market work and education. There are also extensive earnings-related parental leave benefits and most women participate in the labor force. It is easy to imagine that such factors may influence how much individuals respond to changes in institutions governing fertility.

The first part of the empirical analysis relies on data on the total amount of oral contraception sold in each county and year to identify the impact of the subsidy on contraceptive use. Controlling for fixed regional differences in the demand and supply of oral contraception, the results reveal that the subsidy increased total sales by about 5.5 percent. An auxiliary analysis using an independent data source containing information on self-reported consumption shows that the increase was largely driven by teenagers' use of the pill.

I go on to examine the effect of the subsidy on abortions and birth weight using another dataset that in addition to providing a county-level panel also hosts age-specific information on the outcomes of interest. By drawing on the fact that the subsidy was only offered to young women, the data make it possible to control for most potential confounding factors. The strategy is to compare the outcomes of eligible versus ineligible age groups in regions where the subsidy was in place, and contrast these differences to those in areas where the subsidy still had not been introduced. The benefit of this differences-in-differences-in-differences estimator is that it controls for all unobserved factors that may be correlated with the timing of the adoption of the subsidy as long as these do not affect the relative outcomes between different age groups. The results show that the subsidy significantly reduced the abortion rate by about 8.8 percent. There is however no evidence that it affected birth weight and the precise estimates makes it possible to rule out even moderate effect sizes.

The last part of the paper considers the effect of the subsidy on fertility. This analysis uses population micro data containing detailed information on individual background characteristics. Not only does this allow me to identify financially constrained segments of the population but it also makes it possible to compute individual measures of the length of exposure

to the subsidy. Needless to say, women who had access for the subsidy for just a brief period are unlikely to show a behavioral response. The results show that the subsidy decreased the incidence of teenage pregnancy. As expected, the decline in teenage births increases monotonically with exposure length. Girls that had access to the subsidy for throughout their teenage years are found to be 27.2 percent less likely to experience teenage birth. The effect is significantly stronger for women from worse socioeconomic background. There is however no significant effect on family size or relational stability, as measured by the probability that the second born child had the same father as the first born child.

The paper ends with back-of-the envelope calculations on some of the social costs and benefits associated with the subsidy. Despite the documented improvements in women's outcomes this accounting exercise reveals that the public costs of the subsidy by far outweigh its measurable benefits.

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 oral contraceptive sales. Section 3 examines the impact on abortions and birth weight. Section 4 contains an analysis of fertility related outcomes based on population micro data. Section 5 provides a cost-benefit analysis and Section 6 concludes.

II. BACKGROUND

Since its introduction in 1964 the birth control pill has grown to be the leading contraceptive method among young Swedish women (Santow and Bracher 1999).[5] The aim of this section is to describe the institutional setting surrounding the use of the birth control pill in Sweden. It is followed by an analysis of the effect of the subsidy on oral contraceptive sales.

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

A.Institutional setting[6]

In Sweden, oral contraceptives are sold by prescription from a doctor or midwife. The typical procedure for a girl wishing to use the pill is first to schedule an appointment at a youth clinic to see a physician. Youth clinics are health centers for teenagers that offer free consultation about contraception as well as related medical examinations. Virtually all municipalities have at least one clinic. Youths are free to visit other private or public health care facilities, but the procedure does not differ. If the physician deems oral contraception appropriate (s)he 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, which means that in cases where a girl does not want her parents to know about the treatment the physician cannot contact them. In these situations it is however common practice that the doctor or midwife tries to convince the girl to tell her parents.

The question of providing financial support for oral contraception targeted to young women was raised in the late 1980s. The Swedish government had since 1974 directed large resources towards various family planning policies, including a national subsidy on oral contraception for all women. However, the discount was abolished in 1984 and as a consequence the price of the pill quadrupled. Users were at the same time required to renew their prescriptions every third month, instead of once a year, meaning that using the pill would call for more planning. Immediately after the removal of the discount sales started to fall and reports from youth clinics stated that many teenage girls had began to interrupt their treatment. Following a period of decreasing teenage abortion rates, abortions started to increase. These events seem to have been what motivated the new reform.

As the first region, the municipality of Gävle started subsidizing oral contraception for teenagers in 1989. The reform was evaluated by the local authorities and the results showed that consumption of oral contraceptives among teenagers increased from 42 to 60 percent after the

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[6] This section draws on detailed descriptions of the reform outlined in: Csillag (1993), National Board of Health and Welfare (1994, 2001, 2005) and Västragötalandsregionen (2000).

introduction of the subsidy.[7] Moreover, the teenage abortion rate had fallen by almost 50 percent. The experiment was considered as a success and in the following years other regions launched reforms based on the same principle as in Gävle, meaning that the subsidy only targeted specific age groups 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] Upon introduction the subsidy temporarily received attention by the local media and information posters were often highlighted at the youth clinics.

Table 1 outlines the implementation of the subsidy up until 1993, which is the last year for which this information is available.[9] We can see that most of the regions which introduced the subsidy are counties, but a few municipalities also participated. By the end of 1993, eight counties had still not implemented the reform. From Table 1 it is clear that both the starting dates and targeted age groups vary across localities and that only two areas provided the subsidy to women older than 20. It is worth mentioning that the reform did not overlap with other major changes in Swedish family policy (Björklund 2006).

Prior to the reform, a full year's supply of the birth control pill sold for just below USD 100 (in current prices).[10] Although the price might seem fairly low, for young teenage girls without own incomes the costs of obtaining oral contraception could very well amount to a large share 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 purchase the pill, and is worsened by the strong regularity requirements surrounding the treatment: in order for oral contraception 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 reform suggests that many teenage girls

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[7] The evaluation consisted of a simple before and after analysis.
[8] Unfortunately, there is no information about the regional specific subsidy rates.
[9] The reform is described in (National Board of Health and Welfare 1994).There is no information on whether some localities may have implemented similar reforms after 1993 and if so what specific age groups were targeted.
[10] The price varied slightly depending on the type of product but there was no regional variation in prices prior to the reform.

who had become pregnant stated that they had not afforded the pill on the day the treatment was scheduled to begin and therefore had been forced to postpone treatment for a full month (National Board of Health and Welfare, 1994).

A few words about Swedish abortion laws are also warranted.[11] Abortions have been allowed in Sweden on demand and basically free of charge since 1975 when the modern abortion law was implemented (Santow and Bracher, 1999). The law allows every woman herself to decide whether or not to have an abortion until week 18 of gestation. After this week permission is required from the National Board of Health and Welfare. The common praxis is that the Board does not grant abortions after week 22. Parental consent is not required for minors. The most frequently used abortion technology was in the early 90s chirurgical abortion. Medical abortion (induced by chemicals) was introduced in 1992 but was rarely used in the first years.

Details of the evolution of national trends in abortions and fertility have been documented in many previous studies. The abortion rate has remained fairly constant since 1975 of about 18-21 abortions per 1000 women aged 15-44 (SOU 2005). 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. In 1980 the birth rate of women aged 15-19 was close to 14 (Darroch et al. 2001). In 1990 this number had fallen to about 9. The vast majority of Swedish women are cohabiting at the time of their first birth and marriage rates have been declining for several decades.

B. The impact on sales and consumption

Of reasons outlined earlier, subsidizing oral contraception may not necessarily increase the use of the pill. To investigate whether the reform actually increased the use of the pill I collected data from the state pharmacy (Apoteket) on the universe of oral contraception sales in each county and

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[11] This paragraph draws from SOU (2005).

year starting in 1980. At the time of the reform the state pharmacy was the sole provider of prescriptive drugs in Sweden. Sales should therefore provide a fairly good proxy for consumption. Sales are reported in terms of the annual number of defined daily dosages sold per woman aged 15–44.[12]

Before proceeding to the empirical analysis it is useful to graphically illustrate the evolution of sales over time. Figure 1 plots total sales from 1980 through 1993. 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 nationwide subsidy described earlier. We also see that exactly the same year as the reform was introduced, sales start to rise.

Although suggestive, one cannot from the graphical evidence alone rule out the risk that other events affecting oral contraceptive sales may have coincided with the introduction of the subsidy. To account for potential confounders I turn to estimating regressions of the following form

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

where $Sales_{ct}$ is the (log) number of defined daily dosages sold per woman aged 15–44 living in county c in year t. $Subsidy_{ct}$ is a variable measuring the fraction of the year the subsidy was in place. For example, $Subsidy_{ct}$ takes the value .75 if the subsidy was in place for three months during the year and 1 for the following years. μ_c is a set of county fixed effects. μ_t is a set of year fixed effects. $\mu_c \times t$ represents linear county trends. The county fixed effects account for all permanent regional differences in the level of demand and supply of oral contraception. This could be differences in access to family planning services or idiosyncratic demographic conditions. Similarly, the year fixed effects control for nationwide changes in sales that are

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[12] This measure used by pharmacologists takes into account the fact that the content of hormones may vary across products.

common to all counties. Examples of such factors are business cycle fluctuations or changes in the awareness of sexually transmitted diseases. The linear trends control for all smoothly evolving factors within each county. This could for instance be slowly adjusting socioeconomic conditions. β is thus identified off changes in sales that deviates from the linear trend. The basic intuition behind this standard differences-in-differences regression model is to use regions where the subsidy still has not been implemented as a control group. Under the assumption that unobserved regional-specific shocks on sales are orthogonal to the timing of the introduction of the subsidy this research design makes it is possible to disentangle the effect of interest from that of other factors. A total of 18 counties observed from 1980 through 1993 are included in the analysis.[13] As discussed earlier, β is likely not only to capture the effect of the price change but also the consequences of improved information on contraception.

The results are shown in Table 2. The average annual number of daily dosages sold per woman aged 15-44 during the period was close to 100. All regressions are weighted by the number of women in the relevant age in each cell to replicate the underlying micro data. Reported standard errors are robust to serial correlation at the county level. Column (1) presents estimates without regional trends, i.e. $\rho = 0$. The coefficient suggests that the subsidy increased sales by about 10.7 percent and the estimate is highly significant. Bear in mind however that since the regressions include relatively few counties the standard errors may understate the standard deviation of the estimator (Bertrand, Duflo and Mullainathan 2004). To grasp how severe this potential problem may be I ran Prais-Winsten regressions assuming an AR(1) process. I also

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[13] The following counties are excluded from the analysis: Älvsborgs län, Göteborg and Bohuslän, Kristianstads län, Malmöhus län, Skaraborgs län. It was not possible to collect data for these counties as they had merged to form larger regions between the time of the reform and the year when data became available. Since these counties implemented the subsidy at different points in time it was not possible to assign a common reform indicator. Gävleborgs län is excluded since some municipalities in this county introduced the subsidy in 1989 while others did so in 1990 and in 1992, again making it impossible to assign a common year of implementation. Note also that I cannot use information for later years since regions included as controls may have introduced the subsidy after 1993 and there is no documentation on whether this was the case (the description of the reform was written in 1994; see The National Board of Health and Welfare 1994) and, if so, what was the exact date of implementation and what age groups were targeted.

estimated block-bootstrap standard errors. It is reassuring that the standard errors produced by these approaches are very close to those in Table 2.

Column (2) shows that the effect size shrinks when controlling for regional trends. The coefficient suggests that the subsidy raised contraceptive sales by about 5.6 percent. This illustrates the importance of controlling for smoothly evolving factors in the analysis. Column (3) reveals that estimating the model using the level of sales instead of the log of sales as the dependent variable does not change the conclusions. In relation to the sample mean the estimate suggests that the subsidy increased sales by about 7.2 percent (7.103/98.76). Later in the paper I examine the impact of the subsidy on abortions, birth weight and fertility using datasets where the first year of observation is 1985. To ensure that the results presented so far are not sensitive to the choice of observation period I re-estimated the regressions using a consistent time frame. As evident from column (4), the results from this exercise are similar to baseline. The weighted regressions in Table 2 should be interpreted as providing the impact of the subsidy on the average individual. Column (5) instead supplies results from unweighted regressions giving the impact for the average county. We can see that this estimate is slightly larger than baseline.

Recall that my research design requires that sale trends in the reform regions should parallel those in non-reform areas in the absence of the subsidy. To investigate the validity of this identifying assumption I ran regressions exploring the relationship between future subsidy and current sales. If causality runs from the subsidy to sales then one should not expect to find that a placebo subsidy assigned to the closest year preceding the actual subsidy affect current sales, conditional on current subsidy.[14] The results, displayed in column (6), show that the coefficient on future subsidy indeed is close to zero and statistically insignificant.[15]

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[14] This "falsification" test has previously been used by Lochner and Moretti (2004), Black, Devereux and Salvanes (2008) and Dahl (2005) to investigate the exogeneity of compulsory schooling laws.

[15] Another way to investigate the identifying assumption is to examine whether the timing of the subsidy correlates with observed regional characteristics. If circumstances such as the local economy or public health are associated with the subsidy then one could be concerned that other unobserved factors are important as well. To do this I collected data on fraction of households on welfare, the unemployment rate and the mortality rate. These data were available from 1980. There was however some missing values in the earliest years for the fraction of households on welfare. I regressed (the log of) each of these county characteristics on the subsidy indicator along with county and year fixed effects and trends. The policy impact on the mortality rate, the

It is important to remember that the estimates presented in Table 2 reflect the average effect of the subsidy on oral contraceptive sales across all ages and thus cannot tell how much of the effect is due increased pill use among young women. To address this issue I collected data from the ULF survey (Undersökningen av Levnadsförhållanden) administrated by Statistics Sweden. The survey asks women aged 16 and above whether they have consumed oral contraception in the past two weeks prior to the survey date. The question was asked in one round preceding the reform (1980/81) and in one round succeeding the reform (1996/97). The survey covers 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.[16] 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 during the past two weeks. The same figure for 21–24 year olds was 35.8 percent, and for 25–30 year olds it was 25.2 percent. By the time of the next round, all age groups had increased their use of the pill. The corresponding numbers were: 35, 45.9 and 30.6 percent. This means that consumption grew by 36 percent for the youngest cohort, by 28 percent for individuals age 21–24, and by 21 percent for 25–30 year olds. Thus, the increase in consumption was indeed largest for the two eligible age groups. Still, especially when considering the long window between the two survey rounds, this finding can be due to a range of factors unrelated to the subsidy. The results should therefore be interpreted with caution.[17]

It is possible to use information from the ULF survey to calculate how much of the sales increase that was driven by teenagers consumption. Taking into account the age distribution of women it turns out that young females aged 16-24 in 1980/81 accounted for 47 percent of all individuals who state that they consume oral contraception. This means that youths this age have had to increase their use of the pill by 12 percent (.056/.47) to generate a 5.6 percent increase in

unemployment rate and the fraction of households on welfare were: -.005 (.013) -.063 (.108) .021 (.031), respectively. As can be seen, all coefficients are insignificant.

[16] Unfortunately, sample size restrictions prevent me from also disaggregating the data by region.

[17] 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).

overall sales if other age groups did not increase their usage. This figure can be compared with the results in Goldin and Katz (2002) who find that more lenient state regulations regarding minors was associated with 33–40 percent greater pill use by young unmarried women.

III. THE IMPACT OF THE SUBSIDY ON ABORTIONS AND BIRTH WEIGHT

Having documented a connection between the subsidy and the use of the pill I turn to investigate whether the subsidy affected abortions and birth weight. The data used in this part of the analysis were collected from publicly available registers kept by the National Board of Health and Welfare containing annual information from hospital records dating back to 1985 on the birth weight of all newborns as well as every legal abortion performed. [18] Importantly, besides allowing for disaggregation by region and year, the data can also be broken down by age group in five year intervals: 15–19, 20–24, 25–29. This feature makes it possible to use ineligible age groups as an additional control group *within* each county-year cell to better control for confounding factors.[19]

The baseline regression model is specified as follows

$$(2) \qquad Outcome_{cat} = \gamma Subsidy_{ca,t(-1)} + \theta_{ct} + \theta_{at} + \theta_{ca} + v_{cat}$$

where *c*, *a*, and *t* denote county, age group and year, respectively. The outcome is either the (log) abortion rate or the (log) average birth weight. As before, $Subsidy_{ca,t(-1)}$ measures the share of the year that the subsidy was in place.[20] When studying birth weight, $Subsidy_{ca,t(-1)}$ is assigned to the nearest year preceding the subsidy to take into account of the natural lag induced by the gestation period. All regressions are weighted by the number of individuals in each county-age-year cell. The θ's represent fixed effects for county-year, age-year and county-age, respectively.

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[18] The data can be found at www.socialstyrelsen.se
[19] Remember that the subsidy applied to teenagers but that two counties also made youths age 20–24 eligible.
[20] In a few cases the subsidy was only available up until age 18 I have assign .8 to the age cell and in the one case when the subsidy applied to youths up to age 20 I have assigned .2 to the cell.

This differences-in-differences-in-differences regression model controls in a flexible way for most potential confounders. θ_{ct} absorbs county level shocks on the outcome of interest. This could be some local policy or other event that coincides with the introduction of the subsidy. Similarly, θ_{at} sweeps out national trends unique to each age group; for example, an increased high school drop-out rate. Last, θ_{ca} allows for county characteristics to affect age groups differently. Some counties may for instance offer better access to youth clinics than others. The identifying assumption is that county-specific shocks occurring simultaneously as the introduction of the subsidy should not affect the *relative* outcomes between the different age groups.

It is again useful to begin by visually inspecting the data before proceeding to the formal regressions. Figure 2 shows national abortion trends for different age groups. Time is normalized so that year zero corresponds to the year of adoption. We can clearly see that the trend of increasing teenage abortions is abruptly reversed the same year as the subsidy is launched. In contrast, the abortion rate for the other age groups is fairly constant showing only smaller decreases that do not coincide with the timing of the reform.

The results from the regressions are shown in Table 3. The average number of abortions for individuals in my sample is 26 per 1000 women. I start by providing evidence on the impact of the subsidy on teenage abortions, only relying on cross-county and cross-time variation in access to the subsidy. The specification is analogous to the model used by Ananat and Hungerman (2012) who show that the diffusion of the pill in the United States in the 1960s and 1970s lowered the abortion rate by about 18 percent among young unmarried women. Kearney and Levine (2005) find inconclusive evidence that improved access to family planning services in the United States affects abortions. A concern with the results presented in Ananat and Hungerman (2012) and Kerney and Levine (2009) is however that the estimates are very imprecise.

In column (1) we can see that the subsidy decreased teenage abortions by about 4.4 percent. The estimate is significant at the 10 percent level. Column (2) adds to the sample age groups ineligible for the subsidy. As already explained, even if an unobserved shock coincided with the subsidy this will not bias the estimator as long as it did not also affect the relative abortion rate between different age groups. We can see that the effect size increases (in absolute terms) to 8.8 percent. The estimate is significant well below conventional levels. There are two ways to interpret this finding. First, youths aged 20–24 may have responded stronger to the subsidy so that adding this group to the regressions magnifies the effect. If anything one would however expect this age group to respond weaker because it is less likely to be financially constrained. Results from auxiliary regressions revealed that dropping youths age 20–24 renders a point estimate of −.083 (.040), which is quite close and not significantly different from baseline. The most likely explanation is therefore that that unobserved events positively correlated with abortions may have been systematically associated with the reform and that failure to control for this may bias the estimator. The fact that the baseline effect size is smaller compared to Ananat and Hungerman (2012) is expected since the introduction of the birth control pill is likely to have had larger consequences for the use of oral contraceptives.

Column (3) switches the dependent variable from logs to levels. This again produces an effect size that is roughly in line with baseline when placed in relation to sample mean. Column (4) investigates whether the results are sensitive to the choice of age groups included in the regressions. In deciding which age groups to include there is a trade-off between precision and the risk for bias: statistical power will improve if many age groups are included but this come at the cost of an increased risk for bias. It is therefore reassuring that the point estimate remains stabile when individuals aged 25–29 are excluded from the sample. By the same argument as earlier, column (5) presents results from unweighted regressions. The coefficient is only slightly more negative compared to baseline.

Table 3 also provides results from two placebo regressions. Column (6) again tests the exogeneity of the subsidy by investigating the relationship between future subsidy and current outcomes. As can be seen, the coefficient on future subsidy is close to zero and insignificant. Column (7) assigns reform status to the closest succeeding age group that was not eligible for the subsidy. This regression also controls for the actual subsidy. We can see that the estimate is insignificant also for this placebo regression.

Table 3 also investigates the consequences of the subsidy for the next generation of children born to the women potentially exposed to the reform as measured by the average birth weight. Previous studies have linked low birth weight to adverse long-term outcomes such as health, IQ, education and earnings (e.g. Almond, Chay and Lee 2005; Black, Devereaux and Salvanes 2007). The results in Table 3 are conclusive in showing no significant impact of the subsidy on birth weight. The precise estimates are able to rule out even moderate effect sizes. For instance, a 95% confidence interval discards the possibility that the subsidy average birth weight increased by more than 1.8 percent (.004+1.96*.007). As a benchmark, the regression adjusted association between maternal smoking during pregnancy and birth weight has been shown to be about three times higher (e.g. Brooke et al. 1989). These regressions identify the impact of the subsidy on the average birth weight. It is conceivable that any improvements are confined to infants with very low birth weight. This kind of heterogeneity is difficult to detect without individual level information on birth weight. Ananat and Hungernan (2012) estimate that the marginal child not born due to pill diffusion would have been 15% less likely to be low birthweight (<2,500 grams).

IV. MICRO DATA EVIDENCE ON THE FERTILITY RESPONSE

A. Data and research design

This section analyzes the effect of the subsidy on fertility using micro data from population registers collected by Statistics Sweden and maintained by the Institute for Labor Market Policy

Evaluation (IFAU). The data offers the possibility of tracking each individual over time which facilitates an assessment of how the subsidy affected long term fertility. The data also allows me to compute exact measures of the amount of time each individual has been exposed to the subsidy. Another benefit is that the registers include detailed information on individual and family characteristics, making it possible to identify financially constrained segments of the population.

My sample covers all women born in Sweden between 1966 and 1976. All subjects have been linked to their biological parents and information was added about each parent's education and earnings in 1985. There is also information on the birth date of the subjects' children. The latest year for which data is available is 2004 when the subjects are aged 28-38. Region of residence is defined according to where the girl lived at age 16; however, individuals born 1965–1968 were assigned a residential area based on their place of residence in 1985, which is the first year when data become available. The interaction between region of residence and date of birth (year and month) is used to construct the cumulative length of exposure to the subsidy, starting at age 15 and ending when the individual no longer is eligible.

The age restrictions ensure that most individuals still live at home at the time when I first observe them in the data (in 1985), which otherwise would induce measurement error and the risk for selective sorting. Moreover, there is only information about the reform up until 1993 and the restrictions minimize the possibility that younger cohorts living in non-reform areas may have been exposed.[21]

The baseline specification used to isolate variation in exposure to the subsidy is the following

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

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[21] Some regions did in fact introduce the subsidy after 1993, although there is no documentation which age groups were eligible or the exact starting date.

where fertility is observed for individual i in age cohort b living in municipality m. Fertility is either the probability of having given birth by age 20, total number of children or the probability that the second born child has the same father as the first child. The later variable is intended to proxy for relational stability. $Exposure_{bm}$ measures the length of exposure to the subsidy. X_i is a vector of background characteristics; λ_b and λ_m represents year of birth and municipality fixed effects, respectively; $\lambda_m \times b$ represents municipality trends. As the regression model effectively compares outcomes within municipalities across birth cohorts, or conversely within birth cohorts across municipalities, $\hat{\alpha}_1^{OLS}$ represents the conventional differences-in-differences estimator. The model assumes that, conditional on municipality, cohort, and possibly also individual characteristics, an individual's exposure to the subsidy should be uncorrelated with the error term. Table A.1 contains descriptive statistics of the variables included in the analysis.

B. Estimation results

The key variable of interest in the analysis is the length of exposure to the subsidy measured in terms of number of years. To allow for a possible non-linear relationship between exposure length and fertility I also present results from models where exposure length is defined by a set of dummies, the reference group being individuals with no exposure. All regressions include fixed effects for municipality of residence and year of birth. The regressions also control for each parent's earnings and age (linearly), each parent's educational attainment (five levels), missing information on education or earnings, immigrant status and linear trends. The standard errors are clustered at the municipality level to take into account possible serial correlation (286 cells).[22]

Table 4 contains the estimation results. I start by asking whether the subsidy affected the probability of experiencing the first birth by age 20. As can be seen in Table 4, 6.5 percent of the women in my sample gave birth to their first child by age 20. Column (1) reveals a negative

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

significant coefficient on years of exposure. Each additional year of exposure to the subsidy is found to reduce the risk of teenage pregnancy by .44 percentage points. Evaluated at four years of exposure this implies a reduction of about 1.76 percentage points. In relation to the mean of the dependent variable this translates into a decrease of about 27.2 percent (1.76/6.47). This estimate can be compared 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. Note that the difference between the model used by Bailey (2006) and the one used in this paper is that the former relies on aggregated data and therefore mixes the effects of long and short term exposure to the policy. Although the average effect in her study is 16 percent, it is quite likely that the impact is stronger among those individuals who were exposed for a longer duration in their teens. The evidence presented in Table 4 is consistent with the results in Kearney and Levine (2009) who show that income-based family planning waivers for all women had a particular strong effect on teen births. One plausible interpretation of their findings is that teens responded stronger to the policy because they are more financially constrained.

The importance of distinguishing between exposure length becomes clearer when we turn to column (2) showing the coefficients on the different dummies for exposure length. The first thing to note is that the coefficients are monotonically decreasing in exposure length, suggesting a dose-response relationship. While girls that were exposed to the subsidy only for up to two years on average are .59 percentage points less likely to become teenage mothers, the corresponding number for girls that were exposed for more than four years is 3.24 percentage points. Besides being individually significant, the coefficients are also jointly significantly different from zero, as indicated by the p-value of the F-statistic.

The impact of long-term exposure is significant and it is relevant to ask whether the results make sense. It is here worth mentioning that the 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 access to inexpensive contraception is more important in the early teens this could potentially explain the relatively large effects.[23]

Columns (3) and (4) show results for number of children. We can see that although the estimates are negative and increasing in exposure length none of the coefficients are significant at the 5 percent level. The F-statistic also suggests no significant effect. Remember though that number of children is observed when the women are aged 28 and 38, meaning that the analysis does not capture the effect of the subsidy on completed fertility. The average number of children born to the women in my sample is 1.33, which is below the average completed fertility rate of 1.8 children. My results can be compared to those in Kearney and Levine (2009) who find that income based waivers reduced overall births among eligible women by about 9 percent.

Columns (5) and (6) present results for relational stability, as measured by the probability that the second born child has the same father as the first born child. More than 90 percent of the second born children have the same father as their younger sibling. This analysis is motivated by the notion that better planned children may result in more long lasting relationships. A potential caveat is that these regressions can only be estimated for women who had at least two children. As we have seen, the subsidy implied a postponement of childbearing. This means that some of the women may not have got their second child by the time when I observe their outcomes. With this in mind we can see that the effect sizes are small and not statistically significant. It therefore appears that the subsidy did not influence relational stability.

C. Robustness checks and extensions of the analysis

Recall that my identification strategy assumes that the timing of the reform must not be correlated with regional trends in fertility. This assumption may be corrupted if, for example, municipalities with better socioeconomic conditions are overrepresented among the early adopters. Fortunately it is possible to partially assess this issue by removing key covariates. If the estimates are

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[23] I an effort to distinguish between these competing hypotheses I tried interacting exposure length with the age at first exposure. Unfortunately the estimates were too imprecise to be informative.

sensitive to removing controls for family income then one might suspect that other omitted variables could important as well. Another concern is if unobserved municipal shocks are systematically correlated with the timing of adoption. This can be assessed by grouping the municipalities into counties and controlling for county-year effects in the regressions. Again, if the coefficients exhibit large changes then one could worry that failure to properly control for idiosyncratic shocks may bias the results.

The results from these exercises are presented in Table 5. To conserve space I only report estimates for the variable measuring years of exposure, but the results are similar when using dummies. As we can see the coefficients are robust to including county-year fixed effects. For example, the coefficient on years of exposure in column (1) only changes from -.0044 to -.0047. It is also clear that the results are not sensitive to removing controls for parental education and earnings. These results lend credit to the validity of the research design.

Since there are no major signs suggesting that the results are driven by omitted factors I turn to examining whether the effect varies by family background. To do this I grouped individuals into two groups depending on parental education. "Academic family" is having at least one parent who have completed at least theoretical/preparatory high school. "Non-Academic family" is defined as no parent having completed more than vocational high school education. Similarly, "High-income family" is defined as individuals where the combined income of both parents exceeds the median. "Low-income family" is a family with incomes below the median.

Panel B of Table 5 displays the results from the subgroup analysis. We can see that the effect of exposure to the subsidy on teenage childbearing is significantly more negative for women from "Non-Academic" families. In fact, the coefficient for high educated families is not even statistically significant. We also see a tendency for stronger effects among "Low-income" families. The findings are consistent with the idea that expanded access to inexpensive contraceptives should have more intense effects for financially constrained youths. There is however no evidence of differential effects for the other outcomes.

V. ASSESSING SOME OF THE SOCIAL COSTS AND BENEFITS OF THE SUBSIDY

The results presented in this paper suggest that the subsidy increases the use of the pill as well as lowers the abortion rate and delays childbearing. As family planning programs in general, and subsidized contraception in particular, have high priority among policy makers it is informative to assess the cost-effectiveness of the subsidy. Although the public costs tied to the subsidy are fairly easy to measure, estimating its benefits is substantially more challenging. The following accounting exercise should therefore be considered as crude back-of-the-envelope calculations.

Since there is no documentation of the exact costs imposed on each region by the subsidy I estimate the costs as follows

$$(4) \qquad SC = \hat{\beta}(Pop_{pre}^{reform} \times \overline{E} \times \overline{C})$$

where $\hat{\beta}$ is the estimated effect of the subsidy on oral contraceptive sales, Pop_{pre}^{reform} is the number of individuals eligible for the subsidy living in the reform areas in the year prior to the reform, $\overline{E}$ is the fraction of eligible individuals that regularly use oral contraceptives, and $\overline{C}$ is the per capita cost of subsidizing oral contraceptives for an entire year.

98,586 girls satisfying the age qualifications lived in the reform areas in the year preceding the reform. Own calculations based on data from the first round of the ULF survey reveal that 46 percent of young women aged 16 to 24 states that they use oral contraceptives on a regular basis.[24] As described earlier, the annual consumer price of pill before the reform was about USD 100. Since the average subsidy rate was 75 percent it implies that the cost for the local government was USD 75. With this knowledge it is straightforward to estimate the public costs

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[24] The estimate is not far from that reported by the National Board of Health and Welfare (2001) which estimates that close to 60 percent of Swedish women age 18–24 regularly used oral contraceptives in the year 2000.

of the subsidy by plugging in the numbers into equation (4). Doing so reveals that the social costs amount to about USD 3.59 million 1.056 (98,586 .46 75).

This exercise assumes that youths responded similar to the subsidy as older individuals. Recall that the sales analysis cannot separate between the age of the users. This means that the coefficient is a weighted average of the impact of the subsidy across all ages. As youths responded stronger to the subsidy the estimate of the social costs presented here is therefore likely a lower bound of the true costs.

As the social benefits of the subsidy hinges on the size of the costs adverted by fewer abortions and teenage births they are readily estimated as follows

$$(5) \qquad SB = \hat{\gamma} \times (Abortions_{pre}^{reform} \times \overline{A}) + \hat{\alpha} \times (Births_{pre}^{reform} \times \overline{B})$$

where $\hat{\gamma}$ and $\hat{\alpha}$ represent the impact of the subsidy on the abortion rate and the incidence of teenage births, respectively; $Abortions_{pre}^{reform}$ is the number of abortions performed by youths in the eligible age groups in the reform areas in the year prior to the reform; $\overline{A}$ is the average medical cost of an abortion; $Births_{pre}^{reform}$ is the number of teenage births in the reform areas before the subsidy; $\overline{B}$ is the average cost of a teenage birth.

2,279 abortions were performed in the reform areas in the year prior to the subsidy. Combined with the estimated percentage reduction in abortions induced by the subsidy this implies that the reform led to 200 (.088 2,279) fewer abortions. The National Board of Health and Welfare (2005) reports that the average cost of a surgical abortion in 2005 was USD 1,230.[25]

1,157 teenage births were recorded in the reform areas before the subsidy. As shown in Table 2, one year's exposure to the subsidy reduces the teenage birth rate by 6.8 percent

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[25] Practically all abortions performed in Sweden were surgical up until 1993 when medical abortions were introduced.

(-.0044/.0647). This means that 79 fewer children were born to a teenage mother as a consequence of the subsidy. Holmlund (2005) exploits variation in early fertility within pairs of sisters and show convincing evidence that teenage childbearing is associated with .59 fewer years of schooling. Although Holmlund does not directly estimate the effect of teenage childbearing on income it is possible to use the results in Björklund et al. (2010) who summarize the most credible evidence to date of the returns to education in Sweden to assign a monetary value to her estimates. Björklund et al. conclude that results from twin studies as well as exogenous institutional changes suggest that the private returns to one more year of schooling is close to 3 percent.[26] According to publicly available data from Statistics Sweden the average annual earnings for Swedish women aged 16 and above in 1994 amounted to USD 22,894 (in current prices). This means that each additional year of schooling increases income by about USD 686, and consequently that .59 more years in school raises income by about USD 405 (686 0.59). Inserting these numbers into equation (5) gives an estimate of the social benefits of the subsidy close to USD 0.3 million (.088 2,279 1,230+.068 1,157 405).

To summarize, the simple accounting exercise shown in this section suggests that the social costs generated by the subsidy surpass the social benefits by about SC–SB= 3.59–0.3≈USD 3,1 million.[27] There is of course some degree of statistical uncertainty in these figures. But it turns out that even if the calculation of the social benefits is based on the upper limit of a 95% confidence interval, the costs greatly exceed the savings. The upper limit of the 95% confidence interval for $\hat{\gamma}$ is for instance 15.7 percent while the corresponding number for $\hat{\alpha}$ is 7.2 percent. Repeating the exercise above produces an estimate of the social benefits of about USD 0.47 million (.157 2,279 1,230+.072 1,157 405). This means that the net-benefits of the subsidy are almost unchanged.

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[26] There is plenty of evidence showing that education also brings non-monetary returns, such as improved overall well-being and better health (e.g. Oreopoulos and Salvanes 2010). Still, given the difficulties involved in quantifying the value of such returns I refrain from doing this.

[27] Note that this exercise assumes that the results from our analysis focusing on male youths can be generalized to the entire population. However, our conclusions would not change if we only consider male youths. This is because male youths account for a similar fraction of crimes committed in the total population as for their use of alcohol compared to the rest of the population (just above 12 percent).

V. CONCLUDING REMARKS

This paper investigates the social and economic consequences of a large scale Swedish reform that involved publicly funded oral contraception for teenagers. The results show that the subsidy increased total oral contraceptive sales and there is suggestive evidence that this increase is driven by teenagers' use of the pill. The analysis also shows that the reform reduced the abortion rate as well as the incidence of teenage births. The impact of the subsidy on teenage births is especially strong among financially constrained youths. The evidence indicates that the improvements in mothers' outcomes are not passed over to their children, at least not when considering the average birth weight of the next generation.

The results presented in this paper suggest that subsidized oral contraception may be a fruitful way to reduce abortions and teenage pregnancies. Still, back-of-the-envelope calculations show that the societal costs of the subsidy by far outweigh its benefits. In this context it is important to bear in mind that this calculation leaves out potentially important benefits linked to the socioeconomic outcomes of the women that were given expanded access to inexpensive contraception.[28] There may also be unmeasured benefits for the next generation. If parents invest more in better planned children these kids may for instance be less likely to engage in crime (Joyce and Levitt 2003) and better off in terms of school performance. Another issue is that is difficult to estimate the monetary value that both women and men assign to using the birth control pill in relation to alternative contraceptive technologies. If individuals place a high value oral contraception then ignoring this will understate the social benefits. There are also potential costs not included in this analysis. One cost could be an increased prevalence of sexual transmitted diseases. Still, the main lesson from the cost-benefit analysis is that any benefits need to be substantial in order for the subsidy to be cost-effective.

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[28] In a previous version of this paper (Grönqvist 2009) I estimate the impact of the subsidy on women's long run socioeconomic outcomes. The estimates are however too imprecise to make policy predictions from.

In assessing the scope for generalizing the results beyond the context of the present paper paper it is important to note that Sweden's family policy encompass many measures that aim to assist parents and their children, including subsidized child care and parental leave. If these factors compensate for the potential hazards of teenage childbearing then the consequences of introducing a similar policy in a different country could very well be more pronounced.

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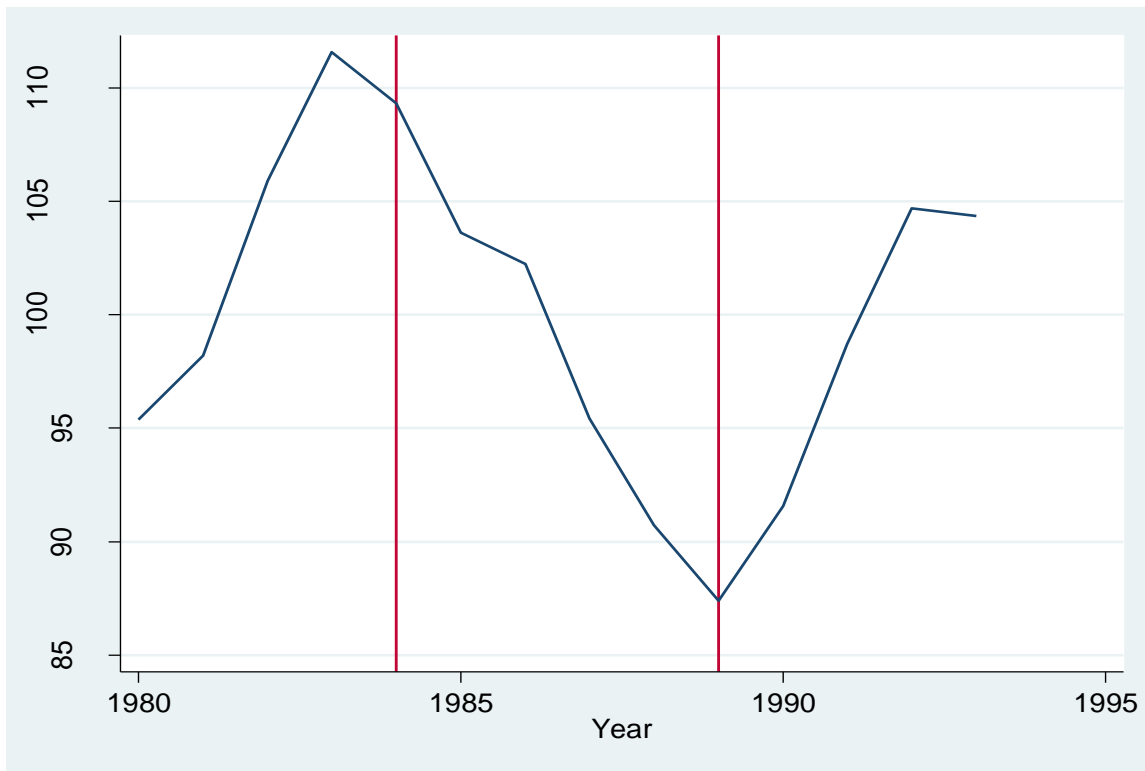
Figure 1 Number of (defined) daily dosages oral contraceptives sold per woman aged 15-45. *Notes*: Vertical lines mark the starting year of the reform (1989) and the ending year of the preceding nationwide subsidy (1984).
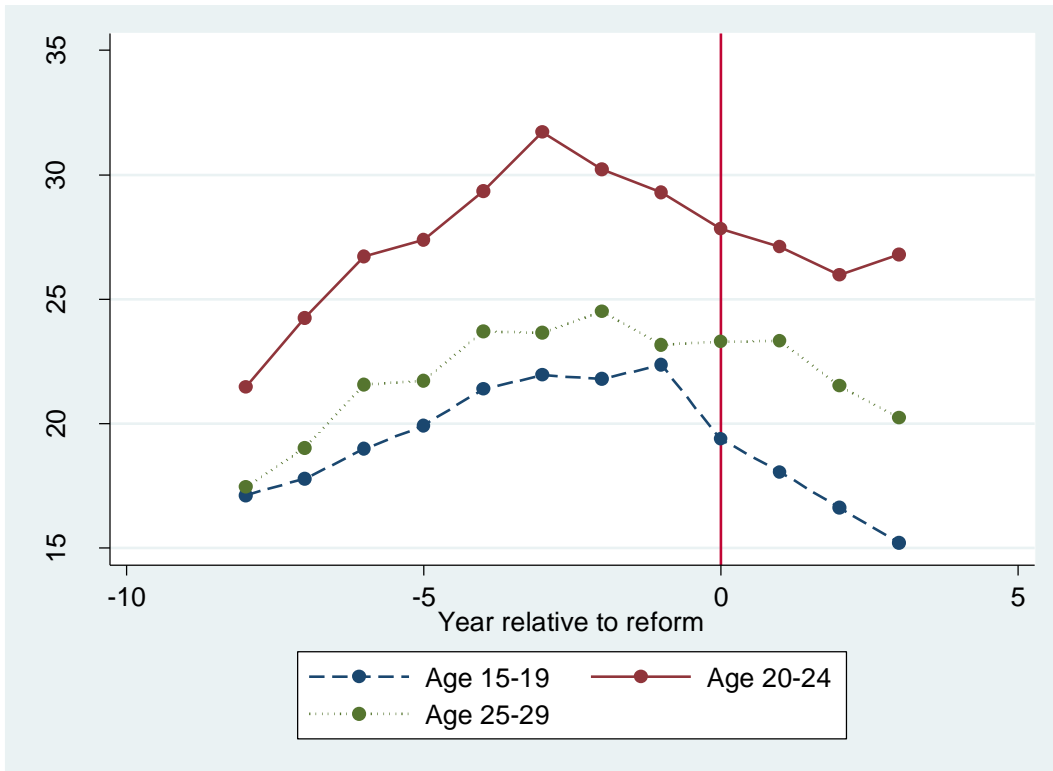
Figure 2 Abortion rate in counties that implemented the subsidy by age relative to starting year.

Table 1. Outline of the reform

Regions which introduced the subsidy before 1994	Starting date	Entitled age groups
Gävle (municipality)	Nov 01, 1989	$\leq 19^*$
Sandviken (municipality)	Nov 30, 1989	$\leq 19^*$
Partille (municipality)	Jan 01, 1990	≤ 20
Hofors (municipality) and Ockelbo (municipality)	Mar 31, 1990	$\leq 19^*$
Örebro (county)	Jun 01, 1990	$\leq 18^*$
Kristianstad (county)	Nov 29, 1990	$\leq 18^*$
Kronoberg (county)	Jan 01, 1991	≤ 19
Blekinge (county)	Mar 01, 1991	≤ 19
Solna (municipality)	Sep 01, 1991	≤ 22
Gotland (county)	Oct 01, 1991	$\leq 20^*$
Södermanland (county)	Jan 01, 1992	$\leq 19^*$
Malmöhus (county) (except Malmö municipality), Västernorrland (county), Älvsborg (county), Västmanland (county), Kopparberg (county)	Jan 01, 1992	≤ 19
Värmland (county)	Mar 01, 1992	$\leq 24^*$
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	$\leq 19^*$
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 entitled for the subsidy until the calendar year they turn this age.

Table 2. The effect of the subsidy on oral contraceptive sales

	No trends	Baseline	Dependent variable entered in levels [mean:98.76]	Time-period 1985-1993	Unweighted regressions	Placebo policy assigned to year preceding actual reform
	(1)	(2)	(3)	(4)	(5)	(6)
Estimated policy effect	.107	.056	7.103	.052	.082	.011
	(.019)	(.020)	(1.991)	(.020)	(.029)	(.023)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Linear county trends	No	Yes	Yes	Yes	Yes	Yes
Number of cells	252	252	252	252	252	252

Notes: The dependent variable is the log annual number of defined dosages sold per woman aged 15–44. The sample consists of a panel of Swedish counties observed from 1980 through 1993. The regressions are weighed by the number of women aged 15–44 in each cell. Standard errors in parenthesis are robust to heteroscedasticity and serial correlation at the county level (18 groups).

Table 3. The effect of the subsidy on abortions and birth weight

	Sample only containing teens	Baseline	Dependent variable entered in levels	Sample excludes age group 25–29	Unweighted regressions	Placebo reform assigned to:	
						Year preceding actual reform	Succeeding age group
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:							
(log) Abortion rate	−.044	−.088	−.002	−.090	−.091	−.016	.016
	(.025)	(.035)	(.001)	(.041)	(.041)	(.033)	(.036)
(log) Birth weight	−.006	.004	13.29	−.004	−.008	.005	.003
	(.009)	(.007)	(24.81)	(.013)	(.008)	(.005)	(.007)
Controlling for	County	County×year	County×year	County×year	County×year	County×year	County×year
	Year	County×age	County×age	County×age	County×age	County×age	County×age
	-	Age×year	Age×year	Age×year	Age×year	Age×year	Age×year
Number of cells	162	486	486	486	486	486	486

Notes: The sample consists of a panel of Swedish counties observed from 1985 through 1993. Regressions in column (1) cover of women aged 15–19. A cell is defined as the interaction between county, year and age. The sample in columns (2)-(7) includes women aged 15–19, 20–24. 25–29. All regressions are weighted by the number of individuals in the relevant cell. Robust standard errors in parenthesis.

Table 4. Micro data evidence on the effect of the subsidy on fertility and family stability

	First birth by age 20		Number of children		Same father to 2nd child	
	(1)	(2)	(3)	(4)	(5)	(6)
Years of exposure	−.0044		−.0069		.0000	
	(.0003)		(.0037)		(.0017)	
Exposed 1–24 months		−.0059		−.0087		.0030
		(.0021)		(.0083)		(.0036)
Exposed 25–48 months		−.0105		−.0194		.0014
		(.0028)		(.0113)		(.0057)
Exposed > 48 months		−.0324		−.0349		.0149
		(.0057)		(.0231)		(.0124)
P-value of F-statistic		.0000		.3327		.5697
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	.0647	.0647	1.33	1.33	.903	.903
Observations	577,996	577,996	577,996	577,996	274,705	274,705

Notes: The sample consists of all women born 1966–l976. All regressions, estimated by least squares, control for each parent's earnings and age, and with dummies for each parent's educational attainment (five levels), missing information on education and earnings, immigrant status and municipality trends. Number of children is 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 columns (2), (4) and (6) is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure are jointly zero.

Table 5. Robustness checks and subgroup analysis

	First birth by age 20	Number of children	Same father to 2nd child
	(1)	(2)	(3)
Baseline	−.0044	−.0069	.0000
	(.0030)	(.0037)	(.0017)
Panel A. Robustness			
Controlling for county-year effects	−.0047	−.0097	.0021
	(.0006)	(.0088)	(.0023)
Dropping controls for parental education	−.0044	−.0070	.0000
	(.0009)	(.0037)	(.0017)
+ Dropping controls for parental income	−.0046	−.0075	.0003
	(.0009)	(.0038)	(.0018)
Panel B. Subgroups			
High educated family	−.0009	−.0078	.0003
	(.0008)	(.0056)	(.0030)
Low educated family	−.0063	−.0059	−.0000
	(.0013)	(.0047)	(.0021)
At least median family income	−.0033	−.0093	.0025
	(.0009)	(.0048)	(.0023)
Below median family income	−.0055	−.0039	−.0019
	(.0013)	(.0050)	(.0026)
Municipality FE	Yes	Yes	Yes
Year of birth FE	Yes	Yes	Yes

Notes: The sample consists of all women born 1966–1976. The regressions, estimated by least squares, control for each parent's earnings and age, and with dummies for each parent's educational attainment (five levels), for missing information on education and earnings, immigrant status, and municipality trends. Number of children and identity of the fathers are observed in 2004. Parental characteristics are measured in 1985. High educated family is defined as a family where at least one of the parents have completed at least academic track in upper secondary school. Family income is total annual earnings of both parents. Standard errors robust to heteroscedasticity and serial correlation at the municipality level (286 cells) are shown in parenthesis.

Table A.1. Summary statistics

Variable	Mean	Standard deviation
A. Aggregated data		
Annual daily dosages sold per woman aged 15–44	98.76	11.96
Abortions per woman aged 15–29	.026	.008
Birth weight women aged 15–29 in grams	3,477	46
B. Micro data		
First birth by age 20	.065	.246
Number of children	1.335	1.141
Same father to 2nd child	.903	.295
Exposed 1–24 months	.103	.304
Exposed 25–48 months	.079	.270
Exposed > 48 months	.021	.143
Years of exposure	.562	1.339
Mother		
Age at birth	26.469	5.268
Compulsory school	.402	.490
High school ≤ 2 years	.349	.477
High school > 2 years	.056	.229
University ≤ 2 years	.094	.292
University > 2 years	.098	.298
Earnings in 1985	586.887	405.199
Father		
Age at birth	29.172	6.443
Compulsory school	.405	.491
High school ≤ 2 years	.253	.435
High school > 2 years	.154	.361
University ≤ 2 years	.070	.255
University > 2 years	.119	.323
Earnings in 1985	1082.386	737.521