Children of the Pill: The Effect of Subsidizing Oral Contraceptives on Children's Health and Wellbeing*

by

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Abstract

What is the lasting and intergenerational impact of providing women with contraception? This paper uses a series of municipal-level experiments in Sweden between 1989 and 1998 to study the role of oral contraception (the pill) subsidies on women's and children's health, education, and economic outcomes. To examine the effects of the policy we combine differences in subsidy exposure across municipality, time, and age eligibility. We first show that subsidized contraception for young women increased pill sales, leading to fewer abortions and lower fertility. We then document significant selection effects on the type of mothers affected. Women giving birth despite being eligible for the subsidy were not as likely to graduate from high school and smoked less during pregnancy compared to similar women who had a child before the reform. While their children were born with better initial health, they do worse in school. Conversely, women who qualified for the subsidy but may have given birth at ages above the subsidy's mandated upper bound are more educated, earn higher wages, and are more likely to enter a father's name on the birth certificate in contrast to ineligible women of the same cohort. Children eventually born to women of the former group had better infant health and do better in school compared to their ineligible peers. Together the evidence shows that improved access to the pill have substantial effects on the next generation's educational and socio-economic success.

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I. Introduction

What is the lasting and intergenerational impact of providing women with additional means of contraception? Evidence on this important question remains scarce as greater availability affects the composition of women having children and the timing of conception both in the short- and in the long-run. That is, the "power of the pill" for women and their children crucially depends on whether and for whom it enables postponing the decision of having a child. In this paper, we shed light on the issue by exploring a nation-wide policy experiment in Sweden in the early 1990s that decreased the price of oral contraceptives. Using individual-level registry data and the fact that the reform induced quasi-experimental variation in the cost of the pill allows us to identify heterogeneous short- and long-term effects on health and education across different groups of women and their children.

A number of influential studies have established that the legalization of oral contraceptives (the pill) in the US had significant effects on women's fertility and career decisions (see, for example, Goldin and Katz, 2002; Bailey 2006, 2009; Guldi, 2008; Hock, 2007). Women who were given access to contraceptive technologies attained higher levels of education and delayed their first marriage and fertility. Moreover, simply lowering the cost of oral contraception has been found to increase the age at first childbearing, and lower overall fertility in the affected group of women (Bailey, 2011; Kearney and Levine, 2009). In short, better and cheaper access to contraception improves women's socio-economic standing.

A separate literature studies the strong and persistent correlation between family socio-economic status (SES) and children's health and wellbeing (see Currie, 2009 for a review). College educated mothers have healthier children (Currie and Moretti, 2002; Miller, 2005) and the association between maternal SES and children's health becomes more pronounced as children age, indicating that the long-term benefits of higher maternal SES might exceed the immediate gains in infant health (Case, Lubotsky, and Paxson, 2002; Case, Fertig, and Paxson, 2005). It is also well known that healthier children have better adult outcomes. For example, using registry data on twins Black, Devereux, and Salvanes (2005) show that higher birth weight twins are taller, have higher IQ scores, and achieve better earnings and education.

These facts suggest that the "power of the pill" extends beyond the affected generation of women into improved health and social wellbeing of their children. Better maternal SES might

not be the only channel through which improved access to contraceptive technologies affects future generations. Palme and Simeonova (2012) report that children slated for adoption at birth had worse health endowments at birth compared to their biological siblings who remained with the biological parents. Studying the long-term effect of abortion prohibition in Romania, Pop-Eleches (2006) shows that unwanted children had worse socio-economic outcomes. As easier access to the pill both increases the human capital of future mothers and improves the chances that their children will be "wanted", the long-term benefits of better access to contraceptive technologies might significantly exceed the short-term gains usually measured by reductions in the abortion rates and the education and the career benefits accruing to affected women. In this paper we use registry data on the universe of two generations of Swedish women and children to test whether and how providing cheaper access to oral contraception affects the inter-generational transmission of human capital.

We exploit a nation-wide policy experiment that reduced the price of the pill. The reform was implemented by Swedish municipalities between 1989 and 1998. To identify the effect of the subsidies we use a difference-in-difference-in-differences strategy comparing outcomes across municipality, time, and age eligibility. Specifically, we examine changes in outcomes before and after the experiment in treated and non-treated municipalities, attained for eligible mothers (ranging from ages 18 to 25) and their children relative to a set of ineligible mothers and children.

Our analysis shows that the price reduction changed the pool of mothers who conceived at ages covered by the subsidies and carried to term. These selection effects impact infant and children's health, as well as the long-term outcomes of children born to eligible mothers before and after the subsidy was put in place. Women who gave birth despite being eligible for the subsidy had better health status but lower educational outcomes compared to women who had a child before the reform. The likelihood of smoking during pregnancy decreased by 11 percent while high school completion rates dropped by 17 percent. Children born to these mothers are healthier but less successful in school. They are half as likely to be born prematurely, have low birth weight, or spend a night at the hospital during their first year of life. To put these results in perspective, the selection effects of subsidy implementation on low birth weight are several times bigger than the estimated effect of smoking cessation (Almond et al, 2005) and similar in magnitude to the impact of obtaining one year of college education (Currie and Moretti, 2005).

Despite the visible improvement in infant health, the probability of qualifying for high school is 2 percent lower for these children and the number of failed subjects on the national exams in 9th grade increases by 13 percent 15 years later.

We also examine the impact of the reform on women who qualified for the subsidy at some point in their lives but may have given birth at ages above the subsidy's mandated upper bound. They complete high school at a 4 percent higher rate, earn 11 percent extra income, and are 70 percent more likely to register a father's name on the birth certificate compared to women who were never eligible for the subsidy. The long-term effects on the children eventually born to women who were at some point eligible for the municipal subsidies are large and important. Their children are 25 percent less likely to be born of low birth weight, 11 percent less likely to experience an in-hospital stay before their first birthday, 50 percent less likely to die in infancy, and close to 20 percent more likely to qualify for high school on the national examinations in contrast to children born to ineligible women of the same birth cohort.

To verify that our findings are driven by expected changes in demand for oral contraceptives we further demonstrate that the subsidies increased pill sales in the affected regions and that the teen abortion and the fertility rates decreased as a result.

The closest study to this one is by Ananat and Hungerman (2012), who use Census data from the US to demonstrate that legalizing the pill improved infant health of the children eventually born to affected women and the average child's living circumstances. The question we are answering is slightly different, namely whether subsidizing the price of the pill has significant effects on the wellbeing of the next generation. In addition, we ask if these improvements translate into better children's health and socio-economic (SES) outcomes later in life and quantify the long-term effects of pill subsidies on children's educational success. This paper also overcomes several limitations of previous related work. First, we link mothers to children and trace out children's health from birth until early adulthood. Second, the nature of the Swedish municipal experiments allowed women of various ages access to lower price of the pill, so that the subsidies were offered both to teenagers and to women in their early to mid-20s. This controls for maternal age and reduces the potential confounding effect of maternal age at birth. Third, all women got access to the subsidies regardless of marital status, avoiding the potential problem of marriage as a means to obtaining the pill and the ensuing complications for identification (Edlund and Machado, 2011; Myers, 2011). This would be particularly problematic

when considering children's long-term health and educational outcomes. Finally, the pill subsidies were implemented more than twenty years after the sexual revolution in Sweden and fifteen years after the legalization of free abortion allowing us to disentangle the impact of the reform from other significant society-wide movements for women's economic liberation that may affect young women's behavior regardless of the availability of contraceptive means.

The rest of this paper is organized as follows. The next section discusses the most relevant previous literature and introduces the institutional background and the policy experiment. We use a simple conceptual framework to illustrate the expected impact of the subsidies on different groups of women in Section 3. Section 4 describes the data and the empirical strategy, and is followed by the results section. Section 7 concludes.

II. Previous literature and institutional background

Most of the previous studies have examined the short- and long-term impact of legalizing the pill on women in the US. In a seminal paper Goldin and Katz (2002) showed that legalizing pill access for young unmarried women increased the probability that they would attain college education and raised the age at first marriage. A number of subsequent papers have extended this research to show that the "power of the pill" resulted in lower fertility (Bailey, 2006; Bailey, 2009; Guldi, 2008) and increased female labor supply and women's compensation (Bailey, 2006). This literature utilizes changes in state laws across time to identify the effects of legalizing oral contraceptives on different groups of women. Ananat and Hungerman (2012) use the same source of variation to test whether access to the pill had long-term consequences on the children eventually born to women who were allowed legal access to the pill. They find that upwardly mobile women in the US opted out of early childbearing, which we confirm in the case of Sweden in the present study. Access to the pill in the US did not significantly affect long-term fertility, but raised the education and SES profile of women who were eligible for legal contraceptives. A shared concern for all US-based papers utilizing between-state variation in legal access to oral contraception is that by and large, abortion legalization happened around the same time in the same states, so that the separate effects of the pill and abortion are hard to identify. By contrast, abortion was legalized and free of charge already for a decade before our time period starts and 15 years before the first municipality experimented with pill subsidies.

A related literature exploits US public policy changes that reduced the price of oral contraception for some women relative to others to investigate the effects of lowering the price of the pill on fertility. Kearney and Levine (2007) use the expansion of Medicaid family planning subsidies in the early 1990s and find large reductions in the birth rates of affected women. Bailey (2012) uses the introduction of family planning program during the war on poverty and finds large reductions in childbearing among poor women who were made eligible for subsidized contraception through these programs.

It is fairly well established that reducing barriers to access to contraceptive technologies for women in the US results in reduced fertility and improved long-term socio-economic outcomes for the affected groups. Both of these channels could potentially affect the short- and the long-term health and socio-economic outcomes of the next generation. There is significant evidence that high levels of maternal education affects infant health (Currie and Moretti, 2005; etc), children's educational achievement (Meghir, Palme and Simeonova, 2012) as well as the long-term health of the next generation (Palme and Simeonova, 2012). Better-off families raise healthier children, and the family SES-children's health gradient becomes steeper as children grow up (Case, Paxson and Lubotsky, 2006). The intimate connection between early life health and long-term SES (see Currie, 2011 and Currie, 2208 for a review of the literature) suggests that the well-established short- and long-term effects of the "power of the pill" for women could have significant long-term effects on their children's health and socio-economic wellbeing.

The Swedish municipal reforms and institutional background

Abortion was legalized in Sweden with the adoption of the Abortion Act in Sweden in 1974 and has been available to women ever since[1]. The Abortion Act entered into force on January 1st, 1975. Legal abortions were performed even before 1975, but a signed statement from two physicians was required, saying that the procedure was necessary for medical reasons. Thus, the cost of abortion decreased sharply in early 1975. In Sweden, abortions are considered a medical intervention and are paid for by the universal health insurance system. Abortions have been available to Swedish women practically free of charge since the mid-1970s[2].

[1] Abortion is up to the decision of the woman up to the 18th week for any reason whatsoever. Between the 18th and the 22nd week the woman has to obtain permission from the National Board of Health and Welfare (Socialstyrelsen).
[2] For youths below the age of 18 abortions are free of charge. The rest pay a "patient fee" which differs slightly between counties, but the range is between $90 and $110.

The Swedish equivalent of the US Comstock Act was repealed in 1938. The Swedish National Board of Health and Welfare approved oral contraceptives for widespread use in 1964, and the pill came to the market the year after. In Sweden one cannot legally buy birth control pills without a prescription (except for emergency contraceptive pills). Oral contraceptives are sold by prescription written by a medical doctor or a midwife. There are several options available to young women seeking to get on the pill. They can visit a youth clinic or a private or a public health care facility. Youth clinics are facilities that offer free consultations about contraceptives and reproductive health to teenagers, as well as associated medical care. Minors can get a prescription for the pill, and parental consent is not required. Medical confidentiality rules apply also to parents, and it is up to the provider of medical care to determine whether a parent should be informed of a minor's contact with the medical care system. In general, providers are not expected to contact the parents unless the child has a medical condition that requires direct parental supervision (Socialstyrelsen, 2001).

By the late 1960s, one in four women aged 15-44 were using oral contraceptives (Jonsson, 1975), a practice that increased over time. In 1987, 34 percent of the Swedish women of fertile age who wished to avoid pregnancy used oral contraceptives (Riphagen and Schoultz, 1989). The corresponding user rate of intrauterine devices was 19 percent. A national survey carried out in 1994 disaggregated usage by age showing that oral contraceptives where by far the method of choice for young women, accounting for up to 61 percent of the contraceptive use among women age 15-24 (Oddens and Milsom, 1996) .[3] Intra-uterine devices are not recommended for use by women who have not given birth in Sweden, and this fact likely explains the strong preference for the pill among younger women (Socialstyrelsen, 2001).

Oral contraceptives were offered at highly subsidized prices sponsored by the national government until 1984. The out-of-pocket cost for a yearly supply of the pill was 15SEK in 1984 (~65SEKin 2001 or around 8 dollars in 2001). Women of all ages, residing anywhere if Sweden, were eligible for the subsidies and paid the same out-of-pocket price until January 1st, 1985 (Socialstyrelsen, 2001). In 1984 the subsidies were abolished and everyone had to pay the sticker price of the pill. The sales of oral contraceptives decreased and the number of teen abortions started increasing. In the late 1980s, some Swedish municipalities decided to implement their own subsidies. The subsidies were initially implemented as pilots, and after a short test period

[3] These rates are very similar to rates in the same cohorts in the US reported by Goldin and Katz (2002).

during which pill sales increased, made permanent (Socialstyrelsen, 1994). Different municipalities adopted subsidies covering different age groups and offering different discounts. In Table 1, we report the eligible age groups and the year of implementation for different municipalities. The average subsidy was 75 percent of the sticker price of the pill (Socialstyrelsen, 1994). The unsubsidized price of a yearly supply of oral contraceptives in 2000 ranged between $45 and $120 (Socialstyrelsen, 2001). The average annual total earned personal income among 16-19 year old women in 2000 was 2500 USD and among 20-25 year old women around 11 800 USD.

Table 1: Subsidy implementation by location and affected cohorts

Location	Starting date	Eligible cohorts
Gävle (municipality)	Nov 01, 1989	<= 19*
Sandviken (municipality)	Nov 30, 1989	<= 19*
Partille (municipality)	Jan 01, 1990	<= 20
Hofors (municipality)	Mar 31, 1990	<= 19*
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)	Jan 01, 1992	<= 19
Västernorrland (county)	Jan 01, 1992	<= 19
Älvsborg (county)	Jan 01, 1992	<= 19
Västmanland (county)	Jan 01, 1992	<= 19
Kopparberg (county)	Jan 01, 1992	<= 19
Värmland (county)	Mar 01, 1992	<= 24*
Jämtland (county)	Apr 01, 1992	<= 24
Göteborg (county))	Jul 01, 1992	<= 20
Bohuslän (county) except (Partille and Göteborg municipalities)	Jul 01, 1992	<= 20
Gävleborg (county) (except for Gävle, Sandviken, Hofors and Ockelbo)	Nov 09, 1992	<= 19*
Uppsala (county)	Mar 01, 1993	<= 19
Malmö (municipality)	Mar 26, 1993	<= 18
Halland (county)	Jul 01, 1993	<= 19
Norrköping (municipality)	Jul 01, 1994	<= 22
Finspång (municipality)	Jul 01, 1994	<= 22
Söderköping (municipality)	Jul 01, 1994	<= 22
Valdermarsvik (municipality)	Jul 01, 1994	<= 22
Östergötland (county)	Jan 01, 1997	<= 18
	1998	<= 19
Jönköping (county)	Apr 01, 1994	< 20
Kalmar (county)	Mar 15, 1994	< 21
Göteborg (municipality)	Jan 01, 1998	<= 19
Skaraborg (county)	Jan 01, 1998	<= 19
Västerbotten (county)	No subsidies ever	
Norrbotten (county)	Jan 01, 1996	<= 19

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

III. Conceptual framework

We present a conceptual framework that helps fix ideas about who would be the marginal woman affected by the subsidy implementation. We remind the reader that abortion is available at low cost for all women throughout the period. We assume a sequential decision-making process where a woman first decides whether to use a contraceptive technology that would allow her to avoid getting pregnant and, second, conditional on pregnancy, decides whether to abort or keep the fetus. If someone does not use contraception, they become pregnant with probability P. For simplicity, assume that all women use the pill perfectly, that is, the probability P that a woman becomes pregnant using the pill is zero. There are two relevant costs: the cost of contraception, C_c, and the expected costs of pregnancy, $E(C_p)$, which varies across women. The difference in $E(C_p)$ arises from two sources. First, the mental cost of aborting, always an option until the 16th week of gestation in Sweden, likely varies across individuals. Second, the cost of carrying the pregnancy to term also varies. Thus, the level of contraceptive intensity depends on the perceived expected costs of pregnancy and the costs of obtaining the desired level of contraception.

Suppose the population consists of, broadly speaking, three types of women: (i) those whose expected costs of pregnancy significantly exceed the costs of insuring 100% contraceptive efficiency; (ii) those who want to conceive, and therefore experience pregnancy "benefits" and will not engage in any level of contraception; and, finally, (iii) those whose expected costs of pregnancy (including the expected costs of abortion) are similar to the actual costs of obtaining perfect contraception.

Type 1: $C_c<E(C_p)$ or $C_c<P*C_p$
Type 2: $C_c>E(C_p)$ or $C_c >P*C_p$
Type 3: $C_c\sim E(C_p)$ or $C_c\sim P*C_p$

Reducing the cost of contraception C_c will only affect Type 3 women, who are at the margin of using it. By lowering C_c, the subsidies decrease the cost of contraception relative to the cost of pregnancy and thus induce more Type 3 women to use more (any) contraception. This immediately implies that the number of abortions and the number of births will decline as a consequence of the subsidy. It also implies a change in the mix of children born after the subsidy is implemented towards more "wanted" children, as the children born to Type 2 women will

comprise a larger fraction of the pool. However, it is not theoretically clear that these children will have better health. On one hand, the marginal child born to a subsidy-eligible mother post-subsidy is less likely to be born to an indifferent mother - a better-planned pregnancy may reduce stress and ensure more conducive behavior (to children's later health outcomes) while pregnant. On the other hand, women who choose to give birth at young ages are likely to be of lower SES, or to have lower expectations of their own future career and educational achievements, and so their children are more likely to be born with worse human capital endowments.

Some supportive evidence from the 1985 pill subsidy abolition

To get a sense of who the Type 3 women are, we use the abolition of the general pill subsidy in 1985 which worked in the opposite direction to the changes we are exploring in the main analysis and affected women of all ages. As a first test of the predictions, we consider changes in characteristics of the pool of mothers due to the 1985 abolition of the national subsidies.

Comparing mothers who conceived in 1984 (the last year of nation-wide subsidy availability) to mothers who conceived in 1985, we find that the latter were 17.5 percent more likely to be teenagers and the average age for first time mothers fell by four months. However, women who conceived in 1985 were about one percent more likely to have graduated from high school in 2000 and made about 1700SEK more in 2009 despite their relatively younger ages (and thus less work experience). This suggests that the marginal woman who was affected by the abolition of the general subsidy in 1985 was young and more likely to attain higher levels of education and earnings later in life. Rather than affecting the poorest and least educated societal strata, the municipal pill subsidies are thus most likely to enable young aspiring women to delay their first childbearing. Our Type 3 women are therefore relatively better-off educated individuals, who bear unwanted children but for whom the cost of abortion is higher than the cost of carrying to term. Under the assumption that Type 3 women are of a relatively higher SES background, we have the following predictions.

Prediction 1: Women who conceive when affected by the price decrease will be less educated and have a lower future income. The short- and the long-term impact on their children is *ambiguous*: while they are more likely to be "wanted", they also grow up in a lower SES environment.

Prediction 2: Women who do not conceive when affected by the price decrease will be more educated and have a higher future income. The short- and the long-term impact on their children is *unambiguous*: they are both more likely to be "wanted" and grow up in a higher SES environment.

IV. Empirical framework and data description

Empirical Strategy

We use two related approaches in the empirical analysis. Due to data limitations, we are constrained to difference-in-differences models in the estimation of subsidy effects on abortions and pill sales. We will exploit two sources of variation: across time and across municipalities. The empirical model is:

$$Outcome_{mt} = \alpha + \beta * Pill_{mt} + \mu + \tau + \varepsilon_{mt} \quad (1)$$

Where m indexes the municipality or county, t indexes time and the outcomes of interest are the number of daily pill doses sold per 1000 women of ages 15-44; the number of abortions performed; the teen conception rate, and the number of birth to subsidy-eligible women. The unit of analysis is the municipality (or county)-year cell. The municipality (or county)-specific fixed effect μ absorbs any time-invariant location-specific unobserved effects, while the calendar year dummy τ absorbs time-specific trends that are common across all locations in Sweden. In our preferred specifications we also include county-specific linear trends that absorb any location-specific trends over time.

Whenever possible, we use triple difference estimations in which we exploit three sources of variation: across time, across municipalities, and across maternal age (or maternal birth cohort). The main analysis is based on such triple differenced models of the subsidy effects on maternal selection into childbearing at different ages, children's health, and children's long-term education outcomes. We perform two sets of analyses. We first study the effect of subsidy implementation on the outcomes of interest for women who were of subsidy-eligible age at the time of first childbearing and their children. The main estimating equation of interest is:

$$Outcome_{jmt} = \alpha + \beta * Pill_{mt} + \tau_{jt} + \theta_{jm} + \mu_{mt} + \varepsilon_{jmt,} \quad (2)$$

where $Outcome_{jmt}$ denotes the outcome for eligible women of cohort j in municipality m at time t. $Pill_{jmt}$ is a treatment indicator equal to 1 if these women were subsidized in a given municipality in a given year and 0 otherwise. The equation includes municipality-year (μ_{my}), cohort-municipality (θ_{jm}), and cohort-time (τ_{jt}) fixed effects. The municipality-year fixed effects control for any unobserved time-varying characteristics that may have led some municipalities adopt the subsidies earlier. The cohort-year fixed effects control for nation-wide unobserved shocks to women of certain ages in the given year. The cohort-municipality fixed effects control for unobserved time-invariant characteristics of cohorts of women residing in the municipality.

Using this specification, we study the selection among mothers before and after the subsidy and their children's outcomes. The outcomes of interest are the mother's education and the mother's income, and total fertility. Marital status is not recorded on the Swedish birth certificate and does not carry the same meaning as in the US, since most couples co-habit and have children before actually marrying. Thus, to proxy for the mother's civil status, we use an indicator variable for a missing father's name.

The set of infant-health outcomes that can be constructed from available data include: infant death (death in the first 12 months after birth), low birth weight at delivery (below 2500 grams), very low birth weight (below 1500 grams), premature delivery (defined as birth before the 37th gestational week), very premature delivery (before the 35th week), the apgar score[4] in the first minute after the delivery, whether the child had an inpatient overnight stay at various ages, and the child's educational attainment as measured by her performance on the high school qualifying exams. The high school qualification exams are administered at 9th grade and determine whether the pupil can continue to academic high school or is better suited for vocational education.

Our second set of empirical analyses test the subsidy effects on birth cohorts of women who were eligible for the subsidized contraception at some point of their lives and the children they eventually had. The main specification is again a triple differenced model using variation in the

[4] The Apgar score is an acronym based on the following criteria: Appearance, Pulse, Grimace, Activity, Respiration. Each of these characteristics of the newborn is evaluated right after birth on a scale from 0 (bad) to 2(good). The respective scores are then summed to form the Apgar score. Thus the resulting score ranges from 0 to 10.

timing of subsidy implementation across different municipalities, and variation across subsidy eligibility across different birth cohorts of women.

$$Outcome_{ijmt} = \alpha + \beta * Pill_{jmt} + \mu_{mt} + \theta_{jt} + \sigma_{jm} + \gamma * X_i + \varepsilon_{ijmt}, \quad (3)$$

where $Outcome_{ijmt}$ denotes the outcome of interest for woman or child i of cohort j in municipality m conceived at year t. $Pill_{jmt}$ is the treatment indicator. It equals 1 if the mother was eligible for a pill subsidy at any point in the given municipality and 0 otherwise. The indicators μ_{mt}, θ_{jt} and τ_{jm} stand for a set of municipality-conception year specific interaction dummies, a set of eligibility cohort-conception year interaction dummies, and a set of eligibility cohort-municipality interaction dummies. The eligibility cohorts are defined based on the municipality-specific subsidy regulations in Table 1. For example, in the first subsidy implementer, Gävle municipality, all women aged 18 and under are indicated as part of the (potentially) eligible cohort at all times.

The set of eligibility cohort-municipality specific fixed effects control for any unobserved time-invariant characteristics that are common across all subsidy-eligible women residing in the same municipality before and after the subsidy implementation. The conception year – municipality fixed effects absorb any municipality-year specific variation that is common across all women who conceived in the municipality during the same year. The eligibility cohort-conception year fixed effects control for any cohort-specific unobserved characteristics that are the same across all women of subsidy-eligible age regardless of their municipality of residence. Specifically, equation (2) examines changes in outcomes, before and after the subsidy was implemented across municipalities, for eligible women and children of eligible women relative to a set of ineligible control women and children of ineligible control women. The vector X_i includes mother characteristics, such as a dummy for the mother's age at the time of birth.

Data

The data used in this analysis combine several registry data sources. Infant health data are based on birth certificates. They cover all births, including stillbirths and late-term miscarriages, that took place in Sweden since 1973. In the analysis we use births resulting from conceptions beginning in 1985. The vital statistics data include information on maternal health and some

demographic characteristics of the mother such as whether she was born in Sweden, her age, and whether she provided a father's name to be entered on the certificate. The number of prenatal visits and an indicator for mother smoking during pregnancy were also recorded starting in the late 1980s.

The vital statistics records also include the county and the municipality where the birth took place, and a unique personal identification number for the mother, the father, and the child, that was used to link the birth records to the same women across births and to other registry-based data. The vital statistics also offer detailed information on the child's health at birth, including birth weight, estimated gestation, an APGAR score (see footnote 4) in the 1st, 5th, and 10th minutes, whether the child was born with any inborn defects or was stillborn. The variable gestation age is measured in days. Together with the month of birth, it is used in tracing back the birth to the month of conception. The month of conception, together with the mother's age at conception and the municipality of birth are used to assign subsidy treatment status.

Using the unique mother's identification number we link the infant health records to LISA, which is a Swedish registry database which records personal income, education, and employment status in 5-year intervals. We have obtained records of the mother's completed education in 2000 and her personal annual labor earnings in 2009. Completed education is not reported (missing) for women who report that they are still pursuing their studies at the time of data gathering, thus we have incomplete records of mother's education for later cohorts. Similarly, if the mother did not have any earnings from labor-related activities (she was not taxed), there is no annual earnings record.

Using the unique ID for the child, we linked the infant health records to the inpatient data registry and to the school records. The National Inpatient Registry records all overnight hospital stays nation-wide starting in 1987. It also contains administrative information such as date of admission, number of days in hospital care as well as discharge diagnoses classified according to the 9th and 10th versions of International Classification of Diseases (ICD). The National Patient Register records a hospital admission only if it included an overnight hospital stay. Emergency room visits and shorter-term (less than 24 hours) inpatient stays are not recorded.

Table 2 presents descriptive statistics of outcomes of interest and the main controls used in the analysis of registry-based individual-level data. The top panel presents simple means of mother's characteristics and outcomes of interest by subsidy eligibility status. Subsidy eligibility status is determined by age and municipality of residence, and extends from women up to the age of 18 to women of ages up to 24 depending on the geographic location. Thus, among other factors, differences between the subsidy-eligible and subsidy-ineligible groups reflect differences between women who give birth to their first child at different ages. We split the subsidy-eligible age group into two subgroups – those who gave birth before the implementation of the subsidies and those who gave birth at subsidy-eligible ages even though they were eligible for pill subsidies.

Mothers who gave birth at subsidy-eligible ages are less likely to have graduated from high school and have lower earnings in 2009. They are also less likely to have recorded a father's name on the birth certificate and more likely to have smoked during pregnancy. Potentially subsidy-eligible women who conceived before the implementation of subsidies are in between the subsidy-eligible post-implementation sample and the subsidy-ineligible sample. The one exception is the probability of the mother having smoked during pregnancy – about 40% of these women reported having smoked, compared to 18% of similarly aged women post-subsidy implementation and 15% of never eligible women. In light of this, it is not come as a surprise that infant health outcomes are worst in the subsidy-eligible pre-implementation group. The probability of infant death is almost twice as high, the incidences of child deaths and hospitalizations up to ages 1 and 5 are also significantly higher in this group. This simple analysis of means suggests that the implementation of pill subsidies negatively selected mothers on SES characteristics, but positively selected them on the basis of health behaviors that affected their children's initial health endowments.

In the lower panel of Table 2 we repeat the analysis of means but we split the sample on the basis of affected birth cohorts of women. The "before subsidy" group here comprises women of subsidy-eligible birth cohorts who gave birth to their first child before the subsidy could affect them. For example, if a 17 year-old woman gave birth to a child in Gävle in 1988, she would be part of that group. If the same woman gave birth to a child in Gävle in 1995, she would be considered part of the "after subsidy" group. The most striking differences between the before-

subsidy and after-subsidy groups are in the age at first birth and number of children born during the observation window. On average, the pill subsidies allowed women to delay first births by five years, and avoid one extra pregnancy. Another large difference is again in the incidence of smoking during pregnancy. Women from subsidy-eligible birth cohorts who gave birth after subsidy implementation were significantly less likely to smoke than their peers from the same cohort-municipality cell who chose to have children before the subsidy experiments started and less likely to smoke than the rest of the maternal population who were never eligible for subsidies either because of their birth cohort or because of their geographic location. Overall, the infant and children's health outcomes are consistent with this finding – children born to the group of women who were least likely to smoke were healthier on average than the rest of the infants in the sample. Women who were subsidy-eligible at some point of their lives and delivered their first child after the subsidy implementation had healthier children.

Table 2: Descriptive statistics of the main variables of interest. Singleton first births only. Standard errors in square brackets under continuous variables means

	Subsidy eligible age groups				Subsidy ineligible	
	Before subsidy		After subsidy			
Variable	Obs	Mean	Obs	Mean	Obs	Mean
high school graduate	24160	0.540	4314	0.355	695204	0.758
Earnings in 2009	25932	1396	26952	758	931443	1693
		[1084]		[878]		[1293]
Mother born in Sweden	26842	0.871	28363	0.811	963687	0.844
Missing father's name	26842	0.020	28363	0.031	963687	0.013
Mother smoked	14437	0.398	17921	0.177	641091	0.154
N children	26844	2.6	28363	2	963693	1.83
N prenatal care visits	575	11.4	22390	10.7	436177	10.9
Low birth weight (<2500 grams)	26808	0.049	28284	0.048	961668	0.042
Very low birth weight (<1500 grams)	26808	0.009	28284	0.009	961668	0.008
Premature (<37 gestation weeks)	26842	0.072	27671	0.065	960104	0.061
very premature (<35 weeks)	26842	0.029	27671	0.028	960104	0.024
Infant death	26842	0.007	28363	0.004	963687	0.004
Death below age 1	26842	0.008	28363	0.005	963687	0.005
Hospitalization 0-1 age	26842	0.332	28363	0.276	963687	0.270
Hospitalization 1-5 age	26842	0.131	28363	0.096	963687	0.104
N abortions/county	145	214	215	203	360	200
N births/municipality	1557	17.5	3210	8.6	3570	258
	Affected cohorts of women					
	Subsidy eligible cohorts				Subsidy ineligible	
	Before subsidy		After subsidy			
high school graduate	6110	0.442	55867	0.251	661700	0.79
Earnings in 2009	8433	1282	235150	1178	740741	1817
		[1043]		[1044]		[1321]
Mother born in Sweden	8718	0.851	244181	0.811	765991	0.85
Missing father's name	8718	0.020	244181	0.019	765991	0.01
Mother smoked	3167	0.260	200333	0.068	469940	0.20
Age at first birth	8718	20.134	244181	25.303	765991	28.52
N children/ mother	8718	2.466	244181	1.718	765991	1.89
N prenatal care visits	1518	9.9	225752	10.6	227270	11.1
Low birth weight (<2500 grams)	8708	0.046	243646	0.040	764403	0.04
Very low birth weight (<1500 grams)	8708	0.010	243646	0.007	764403	0.008
Premature (<37 gestation weeks)	8718	0.070	243907	0.062	761987	0.061
very premature (<35 weeks)	8718	0.028	243907	0.025	761987	0.024
Infant death	8718	0.006	244181	0.003	765991	0.005
Death below age 1	8718	0.007	244181	0.003	765991	0.005
Hospitalization age 0-1	8718	0.301	244181	0.243	765991	0.281
Hospitalization age 1-5	8718	0.118	244181	0.072	765991	0.115

Data on abortions were obtained from the Swedish National Board of Health and Welfare. The data were aggregated by age group and county (municipality) to comply with privacy rules. To obtain the total number of conceptions we added the number of abortions by age group to the number of births to mothers of the same age group. Of course, this number does not include an unobserved number of early miscarriages, but this is unlikely to significantly bias the statistics.

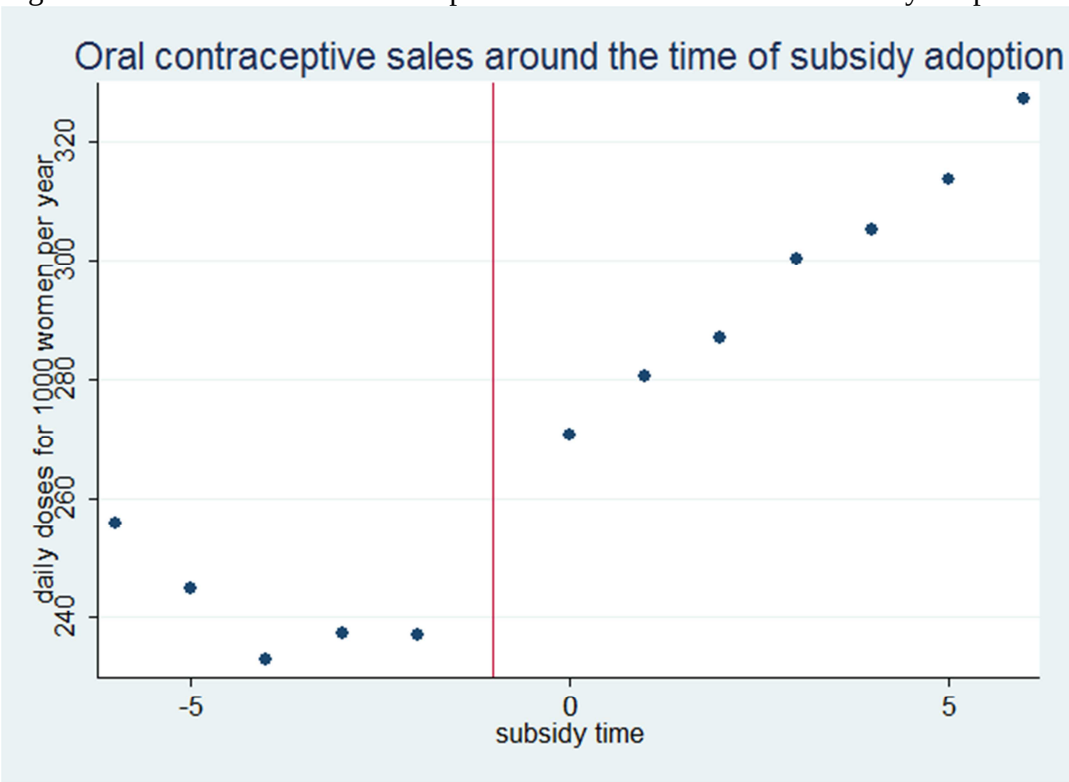
The Swedish pharmacy monopolist Apoteket provided data on sales of oral contraceptives by county. Since there is only one state-espoused pharmacy monopolist in Sweden, all drug sales necessarily take place in one of their stores. The data are recorded as the number of women who received a full yearly supply of oral contraceptives per thousand women of ages 15-44. Notably, these need not be the same women, as the statistics are calculated on the basis of daily doses sold. The data are not disaggregated by age group within the 15-44 range. We thus present analysis using the aggregated Apoteket data together with data from alternative sources to gauge the effect of subsidies on sales to the treated age groups.

It is important to note that the subsidies were most commonly decided on the municipal, not the county, level. Thus, a number of municipalities may implement subsidies before the rest of the county takes them up. For the purposes of this descriptive analysis, whenever there were discrepancies in the years of subsidy adoption between different municipalities in the same county, we classified counties as subsidy-eligible when the majority of municipalities implemented the subsidies. This is a conservative approach as it biases the analysis against finding a significant positive effect of subsidy adoption on pill sales. Our estimates are therefore likely attenuated towards zero.

Figure 1 shows a plot of the daily doses sold to women residing in counties that implemented the pill subsidies around the time of subsidy implementation. We re-center time around the first full year during which oral contraceptive subsidies were available in the county. The red vertical line indicates the last year before the first full year of subsidy. For example, in Jönköping county, the subsidies started on April 1st, 1994. The year 1994 is thus considered as the year before the first full year of subsidy for that county. As Figure 1 clearly shows, the average pill sales were declining or flat in the 5 years prior to subsidy adoption but increased significantly in the first full year of subsidy and continued trending upwards for the next 5 years. In all, the number of daily doses increased from 240 in the last year without any subsidies to 255

in the first full year, to over 316 daily doses 5 years later. In other words, the percentage of women of fertile ages using the pill increased by 6.25 percent in a little over a year, even though only a small fraction of those women were covered by the subsidy.

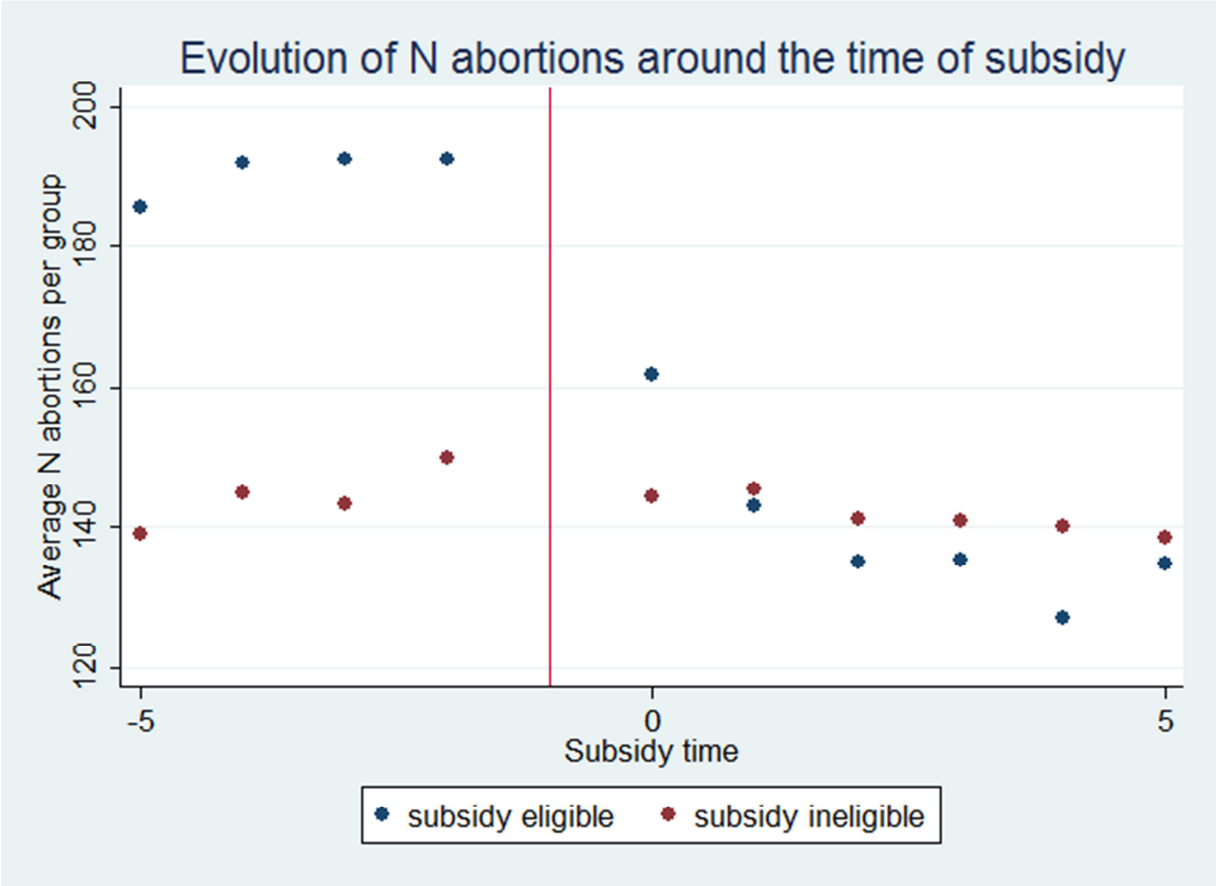
Figure 1: Evolution of oral contraceptive sales around the time of subsidy adoption



In figure 2 we show a similar plot of the number of abortions to different groups of women around the time of subsidy implementation. Here we have the data disaggregated by (rough) age group as well as by county, so we can contrast the group of subsidy- eligible women to those who were never eligible. The data are roughly in 5-year age categories, starting with the group below 20. For counties that had subsidies covering women up to 22 or 23, we again took a conservative approach and included only fully-covered age groups in the eligible group (in this case, only abortions to women aged up to 20). On average, the number of abortions fell by 16.5 percent in 2 years and continued falling for more years after the first full year of subsidies. Between two years before implementation and two years after, the number of abortions performed on subsidy-eligible women fell by around 30 percent on average. Over the same

period, the average number of abortions by county to subsidy-ineligible women remained stable at around 140 abortions per county-year cell. If municipalities wanted to wipe out the difference in the incidence of abortions between women in their teens and early 20s and older women, the subsidies appear to have achieved that goal.

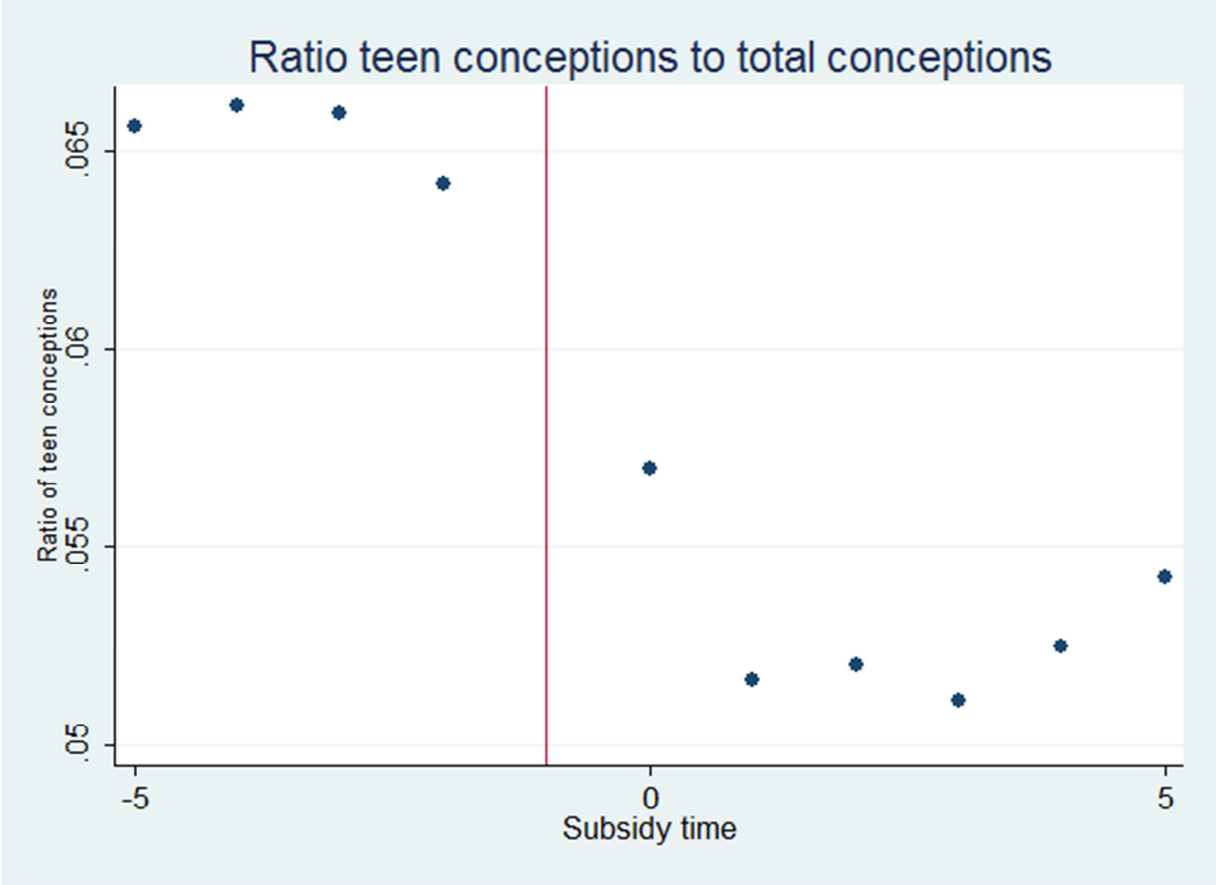
Figure 2: Abortions by subsidy-eligible and subsidy-ineligible women around the time of subsidy adoption



Another way to look at the average effects of the subsidies is to consider the trend in conceptions by subsidy-eligible compared to subsidy-ineligible women. In figure 3 we plot the average ratio of conceptions among subsidy-eligible to conceptions by subsidy-ineligible women by county and year 5 years before and 5 years after the subsidy adoption. Taking the ratio to the total number of conceptions rather than the raw number is preferable as it is not affected by secular trends that likely impact women of all ages. As the plot shows, the ratio of eligible to ineligible

conceptions fell by about 10% percent between the last full year without subsidies and the first full year with subsidies. It continues to fall for a total of 19 percent lower conception rate two years after subsidy implementation compared to two years before.

Figure 3: Ratio of teen to total N conceptions around the time of subsidy adoption



Results from the formal regression analysis are presented below. In table 3 we show the coefficient estimates from specifications testing for the effect of subsidies on pill sales, the ratio of conceptions to eligible mothers and the number of births to eligible mothers including year and county (municipality)-level dummies and county-level linear trends. Even after controlling for unobserved county-level and time-specific factors, we find that on average, the number of daily doses to subsidy-eligible women increased by around 13, the ratio of conceptions to eligible women falls between 4 and 7.5 percent. We use the vital statistics data to estimate the effect on fertility, which allows us to include municipality-level fixed effects. The rightmost panel of Table 3 thus presents the average subsidy-effects on the number of eligible births per

municipality-year cell. The number of births falls by around 1/6th to 1/10th post subsidy-implementation. This effect is somewhat larger than the 7-10 percent drop in fertility due to pill access legalization in US states reported in Ananat and Hungerman (2012) but in the same ballpark. Their results are also more likely to be attenuated towards zero by measurement error as they use imputations and state-year level data and the exact timing of their treatment is less precise.

Table 3: The effect of subsidies on pill sales and conceptions

	Yearly pill supply sold per 1000 women ages 15-44		Ratio conceptions to eligible mothers		N births to eligible mothers	
	County-level		County-level		Municipality-level	
Subsidy	13.45***	13.21***	-0.0044***	-0.0024**	-1.984**	-1.171**
	(2.494)	(2.796)	(0.00128)	(0.00108)	(0.582)	(0.444)
Constant	275.3***	275.4***	0.0597***	0.0583***	18.715**	460.462*
	(2.918)	(2.810)	(0.00175)	(0.00151)	(0.804)	(183.440)
Linear trends		x		x		x
Mean of dep var	266	266	0.06	0.06	12	12
Observations	504	504	400	400	4,552	4,552
R-squared	0.862	0.941	0.734	0.829	0.894	0.917

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: The data in the first 4 columns are organized by year-county-cell. The data in the last two columns are organized at the municipality-year cell level. All regressions cover the period 1985-1999.

In table 4 we present similar regression analysis on the effects of subsidies on the number of abortion to eligible women. We find reductions in the number of abortions similar to Gronqvist (2009) and of plausible magnitude given the findings on pill use reported in Table 3.

Table 4: The effect of subsidies on abortions

	(1)	(2)	(3)	(4)
Eligible group		27.22***		28.57***
		(5.312)		(5.246)
Eligible*subsidy		-18.96**		-21.23***
		(7.646)		(7.665)
Subsidy	15.26	5.976	19.32	7.003
	(9.859)	(10.57)	(13.67)	(14.35)
County FE	x	x	x	x
Constant	290.8***	59.07***	379.9***	58.39***
	(16.78)	(7.716)	(11.25)	(10.26)
Mean of dep var	201	201	201	201
Linear trends			x	x
Observations	2,160	2,160	2,160	2,160
R-squared	0.994	0.800	0.995	0.801

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: The data are organized by year-county-age group cell. The age groups are: <=19, 20-24, 25-29, 30-34, 35-40 and over 40. The data cover the period 1985-2004.

The results presented in tables 3 and 4 suggest that the subsidies had sizeable effects on overall pill sales, and on the fertility rate and number of abortions to groups of eligible women.

V. Results

We first report our estimates of the effect of subsidy introduction on the selection of eligible mothers and their children's infant health and educational outcomes. In the second subsection we compare the health and economic outcomes of cohorts of affected women and the children they eventually bore.

The effects of pill subsidies on selection into motherhood

In table 5 we report the results from a series of triple-differenced specifications based on equation (2). We are interested in differences in the profile of women from age groups eligible for pill subsidies who gave birth before and after subsidy implementation. The results from the corresponding difference-in-differences specifications are reported in the Appendix.

Relative to women of subsidy-eligible ages who had children before the subsidy implementation, women who chose to give birth despite being eligible for pill subsidies were

thirteen percentage points less likely to have completed high school by 2000. They were also more likely to have been born in Sweden and not to have smoked during pregnancy. The coefficient on annual labor earnings in 2009 is positive but not significant. A priori it is not clear what should be the sign of this coefficient – on the one hand these women were more likely to have started working earlier and accumulated work experience. On the other hand as Table 5 shows, they were less educated. The general picture that emerges from the maternal selection results accords with the predictions of the theoretical model. Children born to women who could have used the subsidy (or abortion) to prevent births were more likely to be "wanted" but also more likely to be born in lower SES families.

Table 5: Selection of mothers – characteristics of women who gave birth conditional on being covered by the subsidies

	(1) Completed high school	(2) Income in 2009 in '00 SEK	(3) No father's name	(4) Mother smoked	(5) Mother Swedish
Eligible*subsidy	-0.134**	30.711	0.002	-0.018**	0.027**
	(0.023)	(23.338)	(0.002)	(0.007)	(0.010)
Mean of dep var	0.75	1660	0.014	0.16	0.84
Eligibility*year FE	x	x	x	x	x
Municipality*year FE	x	x	x	x	x
Observations	719,996	980,441	1,014,596	673,385	1,014,596
R-squared	0.183	0.144	0.043	0.122	0.061

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: the number of observations varies because we use all available data for the outcome of interest to maximize power

In Table 6 we report the estimates of these selection effects on infant health estimated using the triple differenced specifications. Again, the corresponding diff-in-diff estimates are reported in the Appendix. The results show an overwhelmingly positive selection effect on infant health and health up to age 1 of the children born to subsidy-eligible cohorts of women post subsidy implementation. The incidence of infant deaths decreases by 25 percent, which appears to have been driven at least partly by the significant decrease in very low birth weight (<1500 grams) babies and very premature births (<35 weeks of gestation). The incidence of both of these outcomes decreases by more than half among subsidy-eligible mothers who choose to give birth

after the subsidy adoption. The probability of overnight hospitalization in the first year of life of the infant is also reduced by almost 50 percent, again confirming that the health endowment of infants born to young women who chose to have them was superior to the endowment of infants whose mothers had fewer contraceptive options and might have been less willing to have them.

The differences in infant health results from Ananat and Hungerman's (2012) paper using the legalization of the pill for women under the age of 21 are not surprising. First, differences in infant health across maternal SES status are much larger in the US than in Sweden. Second, abortion was always a free option in Sweden, while it is unclear whether the costs of abortion were prohibitive for low SES young women in the US. Third, information about pill legalization is less likely to have reached low SES women in the US, while information about pill subsidies was distributed in youth clinics and pharmacies in Sweden. Fourth, teenage pregnancies, comprising the majority of pregnancies in the AH sample, are at much higher risk of low birth weight deliveries. All of these factors contribute to stronger selection into "wanted" births by women in Sweden compared to the US. Finally, measurement error is likely to attenuate the US estimates downwards.

Table 6: Infant health effects of selection due to subsidy implementation

	(1) Infant death	(2) LBW	(3) VLBW	(4) <37 weeks	(5) <35 weeks	(6) Apgar score	(7) Hosp 0-1	(8) Hosp 1-5	(9) Child death <5
Eligible*subsidy	-0.001+	-0.006**	-0.001	-0.013**	-0.006**	0.125**	-0.012**	0.002	-0.001+
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.011)	(0.004)	(0.003)	(0.001)
Mean of dep var	0.004	0.043	0.008	0.062	0.024	8.6	0.27	0.104	0.004
Eligible age*year FE	x	x	x	x	x	x	x	x	x
Municipality*year FE	x	x	x	x	x	x	x	x	x
Observations	1,014,601	1,012,572	1,012,572	1,014,601	1,014,601	1,006,025	1,014,596	1,014,596	1,014,601
R-squared	0.007	0.008	0.008	0.009	0.008	0.019	0.036	0.025	0.007

* significant at 10%; ** significant at 5%; *** significant at 1%

All standard errors are clustered on the municipality of birth level.

Note: LBW: low birth weight, born below 2500 grams; VLBW: very low birth weight, born below 1500 grams; APGAR score is a cumulative score of infant health at birth based on Appearance, Pulse, Grimace, Activity, Respiration (see also footnote 4).

In Table 7 we show the results of regressions testing for differences in education outcomes as measured by the national high school examinations. The maternal selection results suggest that children born to subsidy-eligible mothers after the subsidy implementation are negatively selected on maternal education and thus most likely family SES. On the other hand, these children were born with better initial health endowments, which predisposes them to perform better in school. In table 7 we report the coefficient estimates of the subsidy effect on three outcomes – the total score on the high school qualification exams, the probability of qualifying for high school based on the examinations, and the number of failed subjects. Children born to subsidy-eligible mothers are about 2.3 percent less likely to qualify for high school on the national exams and fail about 5% of a standard deviation more subjects than their peers. Thus, despite their better initial health endowment, their education performance is worse than that of their peers born to potentially subsidy-eligible cohorts of mothers who were conceived before the subsidies were implemented and thus their mothers had fewer contraceptive options.

Long-term effects of the subsidies on affected cohorts of women

Next, we turn to the analysis of the long-term effects of the subsidy policies on the economic outcomes of cohorts of women who were eligible for the subsidies at some point of their lives and on their children. We first report the estimates of subsidy reform effects on maternal outcomes in table 8. It is important to note that these effects are calculated over the birth cohort, and therefore average outcomes regardless of whether the mother gave birth during her subsidy-eligible or subsidy-ineligible years.

The results reported in table 8 show that on average, women who were affected by the subsidy were about 3 percentage points more likely to graduate from high school, had about 18000SEK higher annual incomes from labor earnings in 2009, were more likely to report a father's name on the birth certificate, and less likely to have smoked. These are unequivocably positive effects on women's education and career outcomes. The evidence on age at first birth and total completed fertility by 2010 suggests that the subsidies decreased the total number of children born to affected women by 5 per one thousand mothers. We can exclude effects larger than a reduction of eight births per thousand women who had at least one child. The effects on

age at first childbearing are positive. On average one in twelve women delivered her first child six months later than she would have in the absence of the subsidies. We emphasize that these data do not include women who never had children, and thus are likely to be biased downward if the subsidies increased the age at first childbearing sufficiently so that we do not yet observe first birth by some of the subsidy-eligible women.

These results imply that the long-term effects on infant health from subsidy adoption would incorporate both the “wanted child” effects and the influence of better maternal education and SES status due to the reforms.

Table 7: Children's education outcomes – selection effects

	(1) Score	(2) High school qualified	(3) N failed subjects
Eligible*subsidy	-2.320	-0.020*	0.109**
	(1.476)	(0.008)	(0.034)
Mean of dep var	206	0.91	0.82
Eligible age*year FE	x	x	x
Municipality*year FE	x	x	x
Graduation year dummy	x	x	x
Observations	490,036	490,036	484,098
R-squared	0.098	0.035	0.042

* significant at 10%; ** significant at 5%; *** significant at 1%; All standard errors are clustered on the municipality of birth level.

Table 8: Long-term effects on affected cohorts of women – characteristics of women affected by the subsidies at some point of their lives

	(1) Completed high school	(2) Income in 2009 in '00 SEK	(3) No father's name	(4) Mother smoked	(5) Mother Swedish	(6) N children	(7) Age at first birth
Eligible*subsidy	0.029+	178.769**	-0.010**	-0.052	0.037**	-0.048**	0.041**
	(0.016)	(26.953)	(0.002)	(0.000)	(0.010)	(0.015)	(0.011)
Mean of dep var	0.75	1660	0.014	0.16	0.84	1.8	27.7
Eligible age*year FE	x	x	x	x	x	x	x
Municipality*year FE	x	x	x	x	x	x	x
Observations	720,590	981,226	1,015,410	673,859	1,015,410	1,015,410	1,015,410
R-squared	0.320	0.140	0.042	0.116	0.058	0.292	0.994

* significant at 10%; ** significant at 5%; *** significant at 1%; All standard errors are clustered on the municipality of birth level.

In table 9 we present the coefficients on the long-term effects of subsidy adoption on infant and children's health. The specifications are triple-differenced, and the corresponding diff-in-diff results are presented in the Appendix. The long-term effects of the pill subsidies on the health of the children of affected cohorts of women are positive and consistent across all outcomes. The impact on the infant death rate is very large and accounts for about half of the mean across all births in Sweden. The probability of low birth weight, very low birth weight, and premature births are likewise affected significantly and the effects are economically large – in the 50% to 30% range of the mean. Across most outcomes, the estimates of the long-term subsidy effects are larger than the estimates reported in table 6. This is most likely due to the double-positive effect that accrued to these children – they are both more likely to be "wanted" and born to mothers of higher SES, as we show in table 8. Thus, the long-term effects of pill subsidies, even though they affected women only in their very young ages (up to 24 in the most generous case), changed the composition of births in the long run and very sizably reduced the incidence of negative infant health shocks to the children of these women.

Based on our results on mothers' SES and on the infant health impacts, we expect that children born to cohorts of affected mothers would perform better in school than their unaffected peers. Again, this effect is consistent with the findings that parental (and in particular maternal) SES is strongly correlated with children's SES and that better health endowment at birth results in better educational outcomes later in life. In Table 10 we report the results from triple differenced specifications testing for long-term subsidy effects on the set of outcomes we presented in our selection analysis in table 7. The results imply large positive effects of the subsidies on the long-term educational outcomes of the children of affected cohorts of women. These results are the opposite to what we found when we considered the selection of subsidy-eligible mothers into early childbearing. The average child eventually born to a mother who was ever eligible for a subsidy performs better in terms of overall exam score and the probability of qualification to academic high school and fails fewer subjects than the average child born to women of the same cohort who were never eligible for the pill subsidy.

Table 9: Long-term effects on infant health of children eventually born to women who were affected by the subsidy at some point of their lives

	(1) Infant death	(2) LBW	(3) VLBW	(4) <37 weeks	(5) <35 weeks	(6) Apgar score	(7) Hospitalization 0-1	(8) Hospitalization 1-5	(9) Child death <5
Eligible*subsidy	-0.002*	-0.011**	-0.004**	-0.010**	-0.007**	0.034**	-0.030**	-0.003	-0.003*
	(0.001)	(0.003)	(0.001)	(0.003)	(0.002)	(0.013)	(0.006)	(0.004)	(0.001)
Mean dep var	0.004	0.043	0.008	0.062	0.024	8.6	0.27	0.104	0.004
Eligible cohort*year FE	x	x	x	x	x	x	x	x	x
Municipality*year FE	x	x	x	x	x	x	x	x	x
Observations	1,015,410	1,013,376	1,013,376	1,015,410	1,015,410	1,006,806	1,015,410	1,015,410	1,015,410
R-squared	0.007	0.008	0.008	0.008	0.008	0.018	0.032	0.024	0.007

* significant at 10%; ** significant at 5%; *** significant at 1%; All standard errors are clustered on the municipality of birth level.

Table 10: Long-term effects on the educational attainment of children eventually born to women who were affected by the subsidy at some point of their lives

	(1) Score	(2) High school qualified	(3) N failed subjects
Eligible*subsidy	5.218**	0.017**	-0.191**
	(1.066)	(0.005)	(0.061)
Mean dep var	206	0.91	0.82
Eligible cohort *year FE	x	x	x
Municipality*year FE	x	x	x
Observations	490,039	490,039	484,100
R-squared	0.096	0.034	0.040

* significant at 10%; ** significant at 5%; *** significant at 1%; All standard errors are clustered on the municipality of birth level.

VI. Conclusions

This research utilizes a social policy experiment implemented by Swedish municipalities during the 1990s to identify the effects of lowering the cost of oral contraception on abortions, fertility, and women's and children's long term health and socio-economic outcomes. Despite the large literatures linking maternal education and social well-being to children's health and education and the well-established positive effect of legalizing the pill on women's wellbeing, little is known about the long-term effects of easing maternal access to the pill on children's outcomes. We find both immediate and long term-effects that are economically large and significant. First, we document large positive demand effects of subsidizing access to the pill for young women and significant reductions in the abortion and fertility rates in the affected groups. Second, the pool of women who have access to subsidized contraception but elect to give birth is different from the women of the same age who give birth before the subsidies. Selection into early motherhood post-subsidies happens among women with lower SES. However, their children are born with better initial health endowments than the average child born to a woman of the same age group before the subsidies were adopted. Third, despite having better health at birth, children born to young women who selected into motherhood post-subsidy have worse educational attainment than their peers born before the subsidy.

Finally, we find large positive long-term effects of the subsidies on women who were eligible for them during their young adulthood and the children eventually born to these women. The long-term effects of the pill on infant and children's health are large and positive. The "children of the pill" also have higher educational attainment and enter adulthood better equipped to succeed in the labor market. Thus, the intergenerational effects of providing women with cheaper access to contraception likely exceed by a wide margin the immediate short-term effects of reducing abortions and fertility.

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Appendix tables and figures

Table YYY: The long-term effects of subsidy eligibility: excluding large city centers Stockholm (Solna), Malmo, Sundsvall, Goteborg

Table A6: Difference in Differences: Infant health effects of selection due to subsidy implementation - appendix

	(3) Infant death	(5) LBW	(6) VLBW	(8) <37 weeks	(9) <35 weeks	(10) Apgar score	(11) Hospitalization 0-1	(12) Hospitalization 1-5	(4) Child death
Subsidy	-0.001+	-0.003+	-0.001	-0.007**	-0.003*	0.092**	-0.016**	0.002	-0.001*
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.009)	(0.004)	(0.002)	(0.001)
Linear trends	x	x	x	x	x	x	x	x	x
Obs	1,014,602	1,012,573	1,012,573	1,014,602	1,014,602	1,006,021	1,014,602	1,014,602	1,014,602
R-squared	0.001	0.002	0.001	0.002	0.002	0.011	0.024	0.019	0.001

Robust standard errors in parentheses

Table A9: Difference in Differences: Long term effects of subsidies on infant health

	(3) Infant death	(5) LBW	(6) VLBW	(8) <37 weeks	(9) <35 weeks	(10) Apgar score	(11) Hospitalization 0-1	(12) Hospitalization 1-5	(4) Child death
Subsidy	-0.001+	-0.002*	-0.001*	-0.002+	-0.001+	0.005	-0.006+	-0.001	-0.001+
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.007)	(0.003)	(0.001)	(0.000)
Constant	0.236	0.900	0.453	2.208**	1.120*	-24.445**	4.280**	0.121	0.257
	(0.152)	(0.593)	(0.408)	(0.378)	(0.565)	(3.796)	(1.338)	(0.847)	(0.173)
Observations	517,877	516,536	516,536	517,877	517,877	514,762	517,877	517,877	517,877
R-squared	0.002	0.002	0.001	0.002	0.002	0.008	0.028	0.031	0.002