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

by

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Comments welcome

Abstract

What is the lasting and intergenerational impact of providing women with cheaper contraception? This paper uses a series of municipal-level experiments in Sweden between 1989 and 1998 and registry data on three generations of Swedes to study the role of oral contraception (pill) subsidies on women's and children's health, education, and economic outcomes. To identify the effects of the policy we make use of 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 realized fertility for the eligible age groups. On average, putting ten more women on the pill reduced the number of annual conceptions by at least four. We find that women affected by the subsidies were more likely to get some college education. Their children are in better physical and mental health. They also have better schooling outcomes, as measured by high school achievement scores. These effects are stronger in the population of women coming lower family socio-economic backgrounds.

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I. Introduction

What is the lasting and intergenerational impact of providing women with cheaper or additional means of contraception? Conclusive 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 substantially decreased the price of oral contraceptives for some population subgroups, but not for others. 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 labor 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 of 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. A very appealing feature of this setup is that abortion was legal and available at very low cost throughout the subsidy-implementation period.

We find strong positive effects of subsidy eligibility on the sales of oral contraceptives and strong negative effects on abortions and realized fertility during the subsidy periods. However, we also find that total completed fertility for eligible women somewhat increased. This increase is driven by the women who would not have had any children in the absence of the subsidy and had only one child. The estimates on age at first childbearing vary depending on the woman's socio-economic background. Those coming from less well-off families were less likely to have their first child at ages younger than twenty and more likely to postpone first births to ages older than thirty-five. We find no effects on first childbearing among women from better-off families. The immediate effects of subsidy implementation was strong selection into child-bearing during

subsidy-eligible ages among women from poorer families. Perhaps unsurprisingly, infant and child health decreased among the affected cohorts in the short term.

Women who were eligible for the subsidies obtained more education and received higher incomes into their thirties. They were also more likely to be married at the time they delivered their children. Children born to ever-eligible women were less likely to have been hospitalized, less likely to have been prescribed anti-psychotic and attention-hyperactivity deficit disorder (ADHD medications, and obtained higher scores in the national tests administered in grade 9. The estimates are generally larger for women who came from lower socio-economic status backgrounds as measured by their fathers' incomes.

This paper 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 allows us to adequately control 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 6 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 or professional 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) also use state-level variation in the age of majority to test whether access to the pill affected the living conditions of children born to women who were allowed legal access to the pill. They find that access to the pill allowed upwardly mobile women in the US to opt out of early childbearing, which we confirm in the case of Sweden. Earlier 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 freely provided already for a decade before our time period starts and for 15 years before the first Swedish 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; Currie, 2008), children's educational achievement (Meghir, Palme and Simeonova, 2012) as well as children's

long-term health (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, 2008 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].

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).

[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.

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 (~65SEK in 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 during which pill sales increased, made permanent (Socialstyrelsen, 1994). Different municipalities adopted subsidies covering different age groups and offering different discounts. In Table A1 in the Appendix, 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.

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

III. Conceptual framework

We present a simple 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.

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; and the number of birth to subsidy-eligible women. The unit of analysis is the municipality or county for the abortions and pill sales respectively. For the fertility outcomes, the unit of analysis is the individual woman. 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 some specifications we also include county-specific linear trends that absorb any location-specific trends over time. We utilize the individual panel nature of the data to estimate the effect of being exposed to the subsidy on the probability of giving birth to a child during the period of subsidy eligibility. We assign Pill treatment status based on the birth cohort of the woman and her municipality of residence at age 16[4].

We also investigate the effect of subsidy eligibility on the education outcomes of women, as well as the health and education of their children. The main estimating equation is the same as in (1), except in this case outcomes are measured for each individual woman, t stands for the woman's birth cohort and τ is the woman's birth cohort-specific fixed effect.

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[5] in the first minute after the delivery, whether

[4] We choose age 16 because the age is young enough for women to still be residing in their parents' households (and not have gone away for education or other reasons), because it is unlikely that women will change residence at ages younger than 16 in response to municipality subsidy status, and because the age of consent in Sweden is 15. Using municipality of residence at age 15 yields very similar results.

[5] 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.

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.

As a specification check for some models, we set up the estimation as an event-study analysis. We include dummies for the last three municipality-cohort cells in each municipality that just missed the treatment. We also include a dummy for birth cohorts that missed the subsidy treatments by 4 years or more. The first cohort that got partial exposure to the reform is omitted from the analysis. Dummies for municipality-birth cohort cells that were treated in the first and second years of the subsidies, as well as a dummy for those treated for 3 or more years are also included. As an example, if the first fully treated cohort in municipality A was the 1969 cohort, then the 1968 cohort in that municipality is included as cohort 0, the 1967 cohort is included in the first lead, the 1966 cohort is included in the second lead, 1966 is the third lead and 1963-1965 are the fourth lead. The rest of the estimation is the same as in (1). All lead and lag coefficients are estimated relative to the municipality-birth cohort cells that were partially treated to the reform. In addition to graphically presenting the results, this setup allows us to test for differential trends between treated and control municipalities.

Data

The data used in this analysis combine several registry data sources. The multi-generational register is used to link the index generation of women treated to the subsidies to their parents and children. 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. 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 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 mothers and their parents to Swedish registry databases which recording annual personal income, education, and employment status. We use the records of the mother's completed education by ages 30 and 35. We also use information on the grandparents' income to construct indicators for the index woman's family socio-economic status. We standardize the lifetime income for the women's fathers and define indicators for low socio-economic status for those whose overall income is below the median.

Using the unique ID for the child, we linked the infant health records to the inpatient data registry, the prescription drugs use registry, and to their 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.

The prescription drugs registry tracks all sales of prescription drugs starting in 2005. It includes information on the Anatomic Therapeutic Code (ATC) for the medication, the days' supply, and the unique ID of the person for whom the medication was prescribed. We obtained completed ATC codes (7-digit) for the children included in our analysis, so that we are able to assess the conditions for which the medications were prescribed with high levels of precision.

Table 1 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 25 depending on the geographic location. In the lower panel, we compare women who gave birth during subsidy-eligible ages and those that did not. This shows selection into having a child despite subsidy eligibility, corresponding to Type 2 women in our conceptual framework.

Table 1: Descriptive statistics of the main variables of interest.

Women background characteristics

	Never Eligible	Eligible	Child during no eligibility	Child during eligibility
Foreign origin	0.2576	0.0626	0.0501	0.0765
	0.4373	0.2422	0.2182	0.2658
Mother foreign	0.3191	0.1244	0.1188	0.1517
origin	0.4661	0.3300	0.3235	0.3588
Mother divorced	0.0941	0.1393	0.1238	0.1896
	0.2920	0.3463	0.3294	0.3920
Standardized	-0.0210	0.0223	-0.0031	-0.2302
father's income	0.9540	0.9155	0.9324	0.5856
Mother has				
compulsory	0.4599	0.3604	0.4418	0.4992
education	0.4984	0.4801	0.4966	0.5000
		Child characteristics		
Low birth weight	0.0382	0.0378	0.0392	0.0426
	0.1916	0.1908	0.1940	0.2019
Infant death	0.0048	0.0048	0.0052	0.0056
	0.0693	0.0689	0.0717	0.0745
Child death	0.0056	0.0056	0.0060	0.0066
by age 5	0.0749	0.0747	0.0772	0.0809
Mother age	26.4307	27.5707	26.7358	21.2213
at first birth	5.1530	4.9673	5.0573	2.0312
Single mother	0.1009	0.0917	0.1011	0.1765

Comparisons among women who were never eligible for the subsidy and those who were eligible at some point underscore the importance of controlling for birth cohort and municipality of residence unobserved characteristics. Never eligible women are more likely to be of foreign origin and have mothers who were born outside of Sweden. However, they are less likely to have grown up in divorced households. There are relatively small differences in average father's income, however those never eligible for the subsidies grew up with mothers who were substantially more likely to have completed only compulsory education. Differences in child health characteristics between the two groups are minimal.

Mothers who gave birth at subsidy-eligible ages were more likely to be of foreign origin or have foreign-born mothers. They are more likely to have grown up with divorced parents and their fathers' average lifetime incomes are about 25% lower than the mean. Further, their mothers are substantially more likely to have completed only compulsory education. Overall, women who were subsidy-eligible and gave birth during subsidy eligibility periods were of significantly lower SES than the rest of the sample. They are also more than 5 years younger at the time of first birth and are 70% more likely to not have reported a father's name on the birth certificate. Perhaps unsurprisingly, their children's health outcomes are worse – with higher rates of low birth weight, infant deaths and child deaths at ages younger than 5.

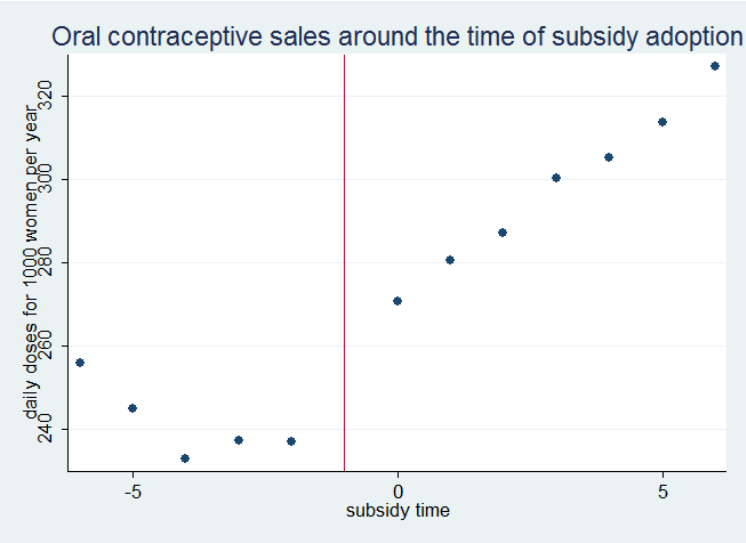
Data on abortions were obtained from the Swedish National Board of Health and Welfare. The data were aggregated by age group and municipality to comply with privacy rules. The age cutoffs used by the data guardian do not always correspond exactly to the age cutoffs for subsidy eligibility implemented by the municipalities. We use the higher age cutoff, whenever that is the case, and so the estimated effects are likely a lower bound of the true impacts of subsidy adoption on the abortion rate. .

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



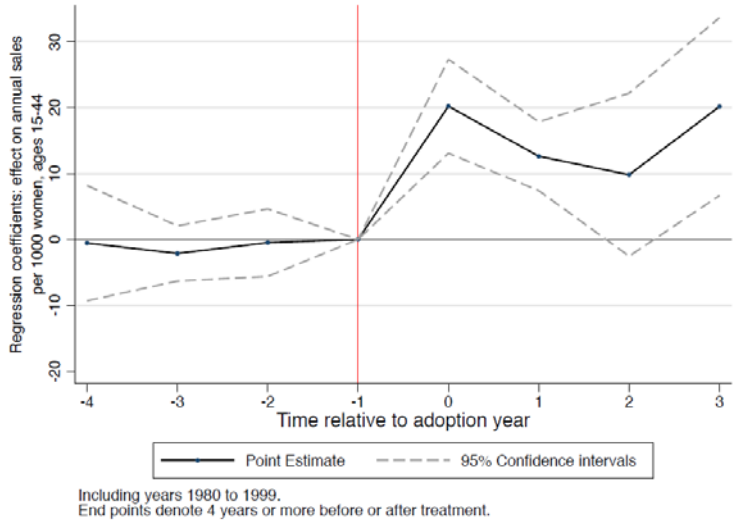
Results from the formal regression analysis are presented below. In table 2 we show the coefficient estimates from specifications testing for the effect of subsidies on pill sales to women aged 15-44. On average, introducing pill subsidies increased pill sales by 6 to 7 percent. Even after controlling 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. Thus, roughly 13-18 additional women per each 1000 women used the pill as a result of the subsides. It is instructive to consider this estimate in the context of the effect of implementing the subsidies on realized fertility. As the estimates below indicate, subsidizing the pill reduced the number of births to eligible women by about three to five per 1000 and the number of abortions by at least to three per 1000. A rough estimate would be that every year, about four conceptions were avoided for every ten additional women using the pill.

Table 2: The effect of subsidies on pill sales

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Annual sales per 1000 women ages 15-44					
Average Effect	18.4515***	15.6493***	18.1610***	17.1546***	15.6953***	13.6205***
	(4.6434)	(3.3861)	(4.2027)	(3.6454)	(2.9443)	(3.4755)
Observations	480	288	480	288	480	288
R-squared	0.8809	0.9415	0.9001	0.9485	0.9379	0.9652
Years	1980-1999	1988-1999	1980-1999	1988-1999	1980-1999	1988-1999
Ages	15-44	15-44	15-44	15-44	15-44	15-44
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
female population and unemployment trends	No	No	Yes	Yes	No	No
County linear trends	No	No	No	No	Yes	Yes
Dep variable mean	265.7	266.7	265.7	266.7	265.7	266.7

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

Figure 2: The effect of subsidy adoption on contraceptive pill sales: event study framework 4 years prior to 3 years after adopting the subsidies



In table 3 we present similar regression analysis on the effects of subsidies on the number of abortions 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 2. Because the data are only reported in age groups, we are somewhat restricted in obtaining estimates exactly for the age groups that were affected by the pill subsidies. Instead, we use age categories – younger than 30 and younger than 20 for municipalities that authorized subsidies for women of ages up to 24(26) and up to 19 (21) respectively.

Table 3: The effect of subsidies on abortions

VARIABLES	(1)	(2)	(3)	(9)
	Annual abortion rate			
Average effect	-0.0015***	-0.0018***	-0.0015***	-0.0025***
	(0.0004)	(0.0004)	(0.0005)	(0.0006)
Observations	27,318	27,318	27,318	9,106
R-squared	0.5188	0.5520	0.5288	0.5152
Years	1985-2000	1985-2000	1985-2000	1985-2000
Ages	<30	<30	<30	<20
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Municipality-year trends	No	Yes	No	No
Municipality-age trends	No	No	Yes	No
Dependent variable mean	0.0231	0.0231	0.0231	0.0205

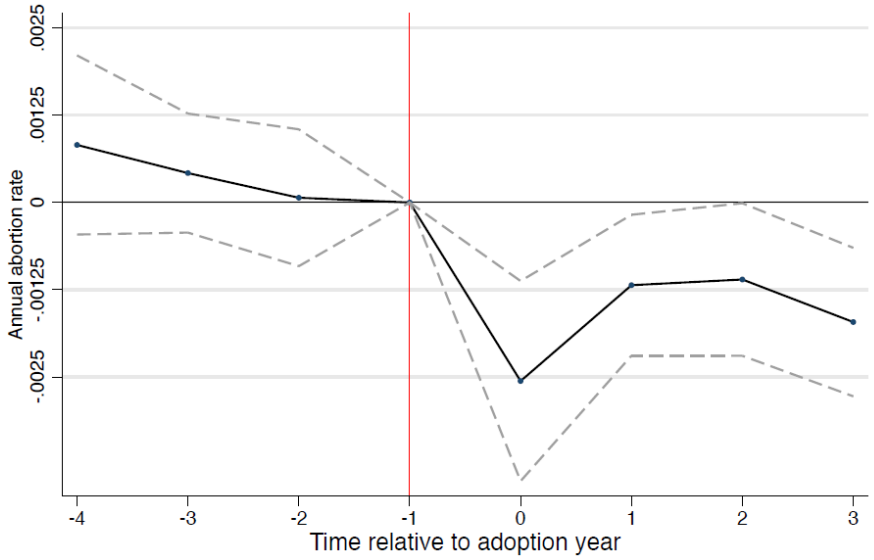
Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: Data are available on the municipality-year-age group level. All regressions include analytic weights based on the number of women of eligible ages residing in the municipality during the year of consideration. All standard errors are clustered on the municipality level.

On average, implementing pill subsidies reduced the probability of abortion by at least 6.5 to 8 percent among women younger than 30. In the younger group of women up to the age of 20, access to reduced cost contraceptives decreased the abortion rate by 12 percent. Note that because of the nature of abortions data that require aggregation to maintain confidentiality, these coefficients underestimate the true effects in the treated population.

Figure 3 presents the coefficients from event-study type analysis of the abortions data. We consider the effect of the subsidy on abortions among women up to age 30. The setup is the same as in the regressions in Table 3, except we now plot the coefficient estimates for up to 3 years before and after the adoption of the subsidies. The drop in the abortion rate in the first year after adopting the subsidies is fairly large. In subsequent years the rate stabilizes at around the average effects we estimate in the Table.

Figure 3: The effects of pill subsidies on the number of abortions



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. The estimates presented in Table 4 show that eligibility for pill subsidies reduced the probability that a woman would have a live birth in a year by about three to five percent in the sample of women born 1963-1975. These women form our main analysis sample because they are old enough to have had an opportunity to bear a child after they are no longer covered by the subsidies. The down side of using this sample is that the full effects of subsidy exposure are not completely realized, as many of these cohorts are only eligible for pill subsidies for one or two years at the end of the age eligibility range (so when they are 22 or 23 years old).

Expanding the birth cohorts to include those born up to 1985 unsurprisingly yields larger estimates. The implied effects are around a seven to eight percent reduction in the annual probability of giving birth during subsidy-covered ages. This effect is in the ballpark of the 7-10 percent drop in fertility due to pill access legalization in US states reported in Ananat and Hungerman (2012).

Table 4: The effects of pill subsides on annual fertility during the ages covered by the subsidies

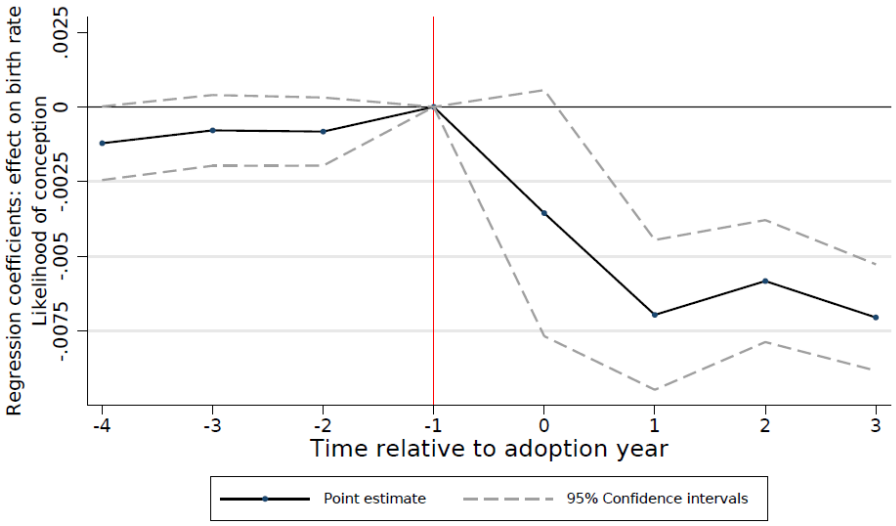
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Likelihood of having a child					
Average effect	-0.0036***	-0.004***	-0.0046***	-0.0025***	-0.0022***	-0.0035***
	(0.0005)	(0.0005)	(0.0004)	(0.0006)	(0.0007)	(0.0006)
Observations	6,897,226	6,897,226	6,897,226	4,698,545	4,698,545	4,698,545
R-squared	0.0360	0.0361	0.0365	0.0273	0.0274	0.0276
Years	1988-2000	1988-2000	1988-2000	1988-2000	1988-2000	1988-2000
Ages	16-25	16-25	16-25	16-25	16-25	16-25
Birth years	1963-1984	1963-1984	1963-1984	1963-1975	1963-1975	1963-1975

Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes
Muni-year trends	No	Yes	No	No	Yes	No
Muni-birth year trends	No	No	Yes	No	No	Yes
Individual covariates	No	No	No	No	No	No
Dep var mean	0.0511	0.0511	0.0511	0.0667	0.0667	0.0667

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 4 shows the corresponding plot of the coefficients obtained in an event-study type analysis. There are no differences in the probability of conception between women of the same birth cohorts living in municipalities that adopted the pill subsidies and those that did not. However, there is a sharp drop in the fertility rate among those living in locations that got the subsidies relative to the rest. The downward trend continues for two years right after the adoption of the subsidies, and levels off in the years after at a level that is 0.75% lower in the treated municipalities.

Figure 4: Effects of pill subsides in fertility rate: event study type analysis



V. Regression Model Results

The effects of pill subsidies on selection into motherhood and overall fertility

The most immediate effect of the subsidies could be selection among women who gave birth during the ages under treatment. In Table 5 we show the profile of women who had children during the period when

they were eligible for the subsidies. The estimates indicate that higher socio-economic status women selected out of fertility during the subsidy period. The average child during those years was born to a mother who was more likely to be foreign-born, to have a foreign-born or divorced mother. Further, the average income of the maternal grandfather was 6% of a standard deviation lower, and the maternal grandmother was 4.8 percentage points (12 percent, evaluated at the mean) more likely to have completed only compulsory education. Therefore, the immediate effect of the subsidies was a change in the pool of mothers towards those of lower socio-economic status as measured by family background characteristics.

Table 5: Socio-economic profile of women who had a child while eligible for pill subsidies

	(1) Mother foreign	(2) Granny foreign	(3) Granny divorced	(4) Grandpa income	(5) Granny educ
Avg effect	0.0199***	0.0323***	0.0355***	-0.0610***	0.0484***
	(0.00472)	(0.00708)	(0.00467)	(0.0166)	(0.00678)
Constant	-0.00672	-0.00702	-0.0190	-0.486*	1.025***
	(0.0218)	(0.0419)	(0.0331)	(0.254)	(0.0659)
Observations	1,327,843	1,327,843	1,327,843	1,292,016	1,307,257
R-squared	0.027	0.048	0.016	0.078	0.060
Mean of dep var	0.045	0.112	0.125	-0.010	0.446

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include fixed effects of mother's birth cohort, municipality of residence at age 16, and birth year of the child. Standard errors clustered at the municipality of residence.

Even though we find that fertility declined during subsidy eligibility, it is not immediately clear what could be the effects of pill subsidy exposure on fertility later in life. Table 6 shows the estimates of the average effect of subsidy exposure on life-time fertility. We consider women born from 1963 to 1975, so that we can estimate the effects of the pill subsidies on the probability of having a child and the number of children born to a woman by age 40 for all birth cohorts. We also consider the probability of giving birth by age 20 and at ages older than 35. Odd-numbered columns present results from models including only basic controls, such as birth cohort fixed effects and municipality of residence (at age 16) fixed effects. Even-numbered columns include demographic controls, such as foreign-born status of the woman, the same for her mother, a dummy for a divorced mother, and dummies for the income quintiles of the grandfather's lifetime income.

Exposure to the subsidies decreases the probability of having no children by 2.5 percent. This is driven by increases in the probability of having one child, as the effect on the total number of children born is very close to zero and fairly precisely estimated. In the full sample of women we see a decrease in the

probability of giving birth before age 20 and an increase in the probability of giving birth for the first time at ages 35 and over. Neither of these are statistically significant though the decrease in teen childbearing is economically significant at 5 percent.

Table 6: Effect of exposure to the subsidy on lifetime fertility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No children		N children		<20 at first birth		>35 at first birth	
Avg effect	-0.00396**	-0.00410**	0.0111	0.0111	-0.00172	-0.00163	0.000422	0.000369
	(0.00177)	(0.0018)	(0.0068)	(0.0070)	(0.0012)	(0.0012)	(0.0013)	(0.0013)
Constant	0.157***	0.156***	1.956***	1.964***	0.0556***	0.0279***	0.0450***	0.0699***
	(0.00224)	(0.00234)	(0.0079)	(0.0075)	(0.00129)	(0.0012)	(0.0014)	(0.0015)
Demographic controls		x		x		x		x
Birth cohorts				1963-1975				
Obs	730,358	707,905	730,358	730,358	730,358	707,905	730,358	707,905
R-sq	0.003	0.005	0.007	0.008	0.004	0.015	0.005	0.007
Mean of dep var	0.161		1.879		0.035		0.069	

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income. Standard errors clustered on the level of mother's municipality.

Table 6A: Effect of exposure to the subsidy on lifetime fertility: low SES women only

VARIABLES	(1) No Children	(2) N children	(3) <20 at first birth	(4) >35 at first birth
Average effect	-0.00373	0.0153**	-0.00192	0.00286**
	(0.00231)	(0.00751)	(0.00160)	(0.00135)
Constant	0.138***	2.011***	0.0615***	0.0423***
	(0.00265)	(0.00848)	(0.00135)	(0.00140)
Demographic controls	x	x	x	x
Birth cohorts	1963-1975	1963-1975	1963-1975	1963-1975
Observations	688,909	688,909	688,909	688,909
R-squared	0.039	0.053	0.009	0.017
Mean of dep var				

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects and demographic controls. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother. Standard errors clustered on the level of mother's municipality.

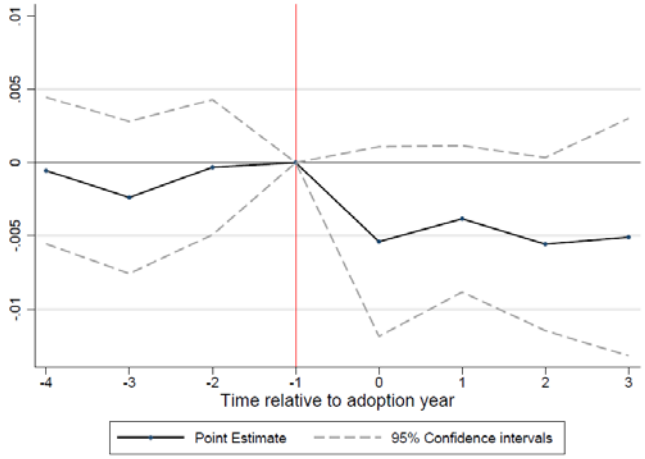
Table 6A reports estimates from the sample of women from low SES families. Here we see similar results, except the relative magnitudes of the estimated effects are larger. One substantial difference is in the probability of giving birth after age 35, which is statistically significant and large at 17 percent of the mean for this subsample, indicating that this change driven by the women coming from low SES families is behind positive effect in the overall sample.

For completeness, Appendix Table A2 repeats the analysis for the sample of women born 1963-1985. The advantage of using this expanded sample is that it is more likely to capture the full effects of the subsidies as the proportion of women who were eligible for the subsidies during their teenage years is larger. The downside is that women from more recent cohorts are too young to have completed their fertility, and so we are only able to compare completed fertility up to a certain age across women in treated and untreated municipalities. The results from this expanded sample are qualitatively the same, and again the effects appear to be stronger in the population of low SES women.

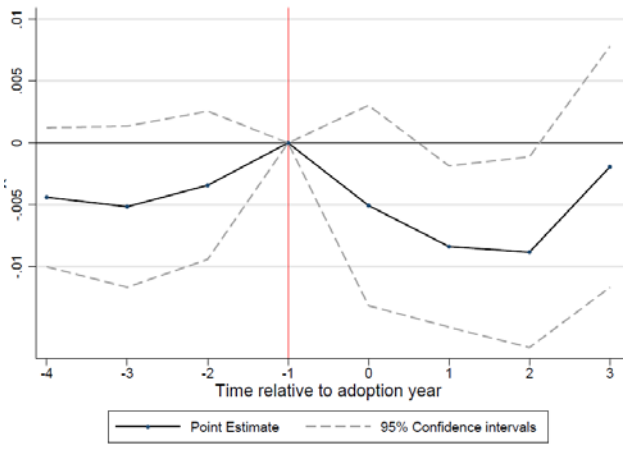
Figure 5 illustrates the findings presented in Table 6 and 6A in an event-study framework. The graph on the left uses the full sample of women, and the one on the right restricts the sample to those with low SES backgrounds. The plots show that there were no significant differences in the pre-trends in the probability of having no children between treated and control municipalities in the years leading up to the subsidy implementations. However, we see a significant and consistent drop of about 0.5 percent after the subsidies started. Among low SES women the estimates are noisier but point in a similar direction.

Figure 5: Event-study type analysis of the effect of subsidies on the probability of having no children

All women born 1963-1975



Low SES women born 1963-1975



The effects of pill subsidies on women's education

We next present evidence on the effect of subsidy exposure on women's education by age 30. We choose age 30 because we can reasonably assume that most women have completed their high school and possibly college education by that age. Regressions using education by age 35 yield very similar results.

The estimates show a positive and economically large effect on the probability of having completed post-high school education such as some years of college education or a certificate in a trade that requires post-high school education.

Table 7: The effect of exposure to the subsidies on women's education by age 30

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Compulsory education		Professional education		College		Some college/Certificate	
Avg effect	-0.00106	-0.00169	-0.000623	-0.000357	-0.00668	-0.00595	0.00614**	0.00652**
	(0.00366)	(0.00370)	(0.000691)	(0.000732)	(0.00603)	(0.00544)	(0.00267)	(0.00277)
Constant	0.141***	0.0704***	0.0115***	0.0264***	0.103***	0.263***	0.132***	0.179***
	(0.00200)	(0.00200)	(0.000631)	(0.00107)	(0.00264)	(0.00327)	(0.00203)	(0.00243)
Birth cohorts	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975
Demographic controls		x		x		x		x
Obs	703,001	683,010	703,001	683,010	702,600	682,612	703,001	683,010
R-sq	0.015	0.041	0.004	0.009	0.068	0.112	0.006	0.012
Mean dep var								

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: Professional education is a professional post-secondary degree such as a JD, a PhD or an MD. All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income. Standard errors clustered on the level of mother's municipality.

Table 7A: The effect of exposure to the subsidies on women's education by age 30: low SES women only

	(1)	(2)	(3)	(4)
	Compulsory education	Professional education	College	Some College/Certificate
Avg effect	-0.00248	-0.000305	-0.0105	0.0102***
	(0.00474)	(0.000692)	(0.00667)	(0.00327)
Demographic controls	x	x	x	x
Birth cohorts	1963-1975	1963-1975	1963-1975	1963-1975
Constant	0.171***	0.00729***	0.0619***	0.111***
	(0.00268)	(0.000637)	(0.00271)	(0.00247)
Obs	425,147	425,147	424,957	425,147
R-sq	0.020	0.003	0.065	0.005
Mean dep var				

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

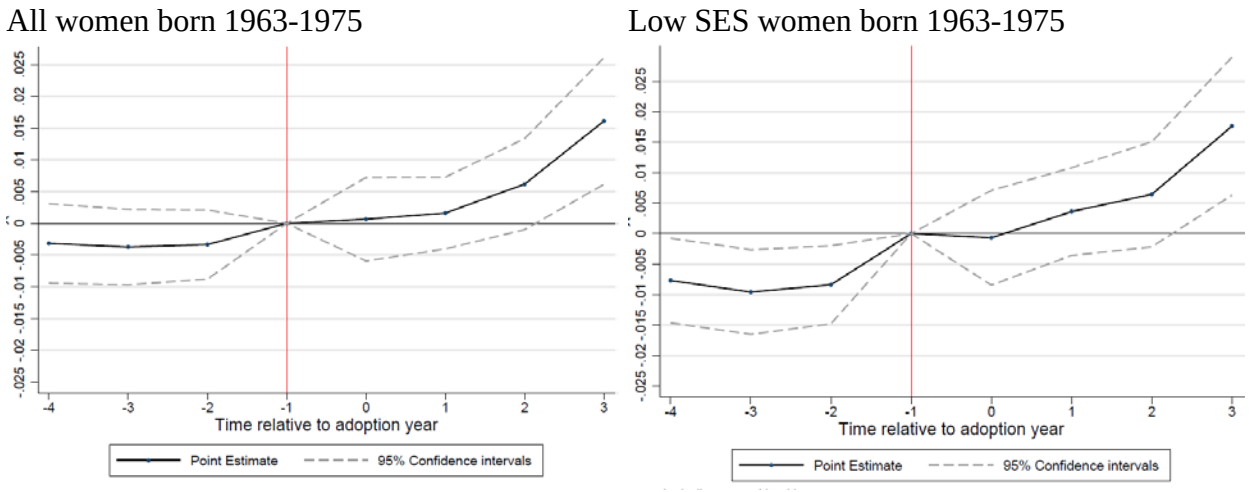
Note: Professional education is a professional post-secondary degree such as a JD, a PhD or an MD. All regressions include municipality fixed effects and mothers' birth cohort fixed effects and demographic controls. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother. Standard errors clustered on the level of mother's municipality.

Table 7A shows that the effects are stronger for women from low SES backgrounds.

Table A3 in the Appendix displays the estimates for men of the same generations. Though a priori it is not entirely clear that men would not be affected by the pill reform, in contrast to the results for women, the evidence shows no changes in men's propensity to achieve different levels of education.

Figure 6 illustrates the significant findings for women in an event-study type framework. As the figures demonstrate, there are no significant pre-trends in the pre-adoption probability of obtaining some college education among women from municipalities that adopted the subsidies and those that did not. We see a gradual increase in attaining some college education after the subsidies kick in. The effects are more pronounced in the sample of low SES women.

Figure 6: Event-study type analysis of the effect of subsidies on the probability of obtaining some college education



<u>The effect of pill subsidy exposure on children's health</u>

The first set of outcomes for children we consider are based on infant health indicators. Table 7 reports a series of regressions estimating the average effect of exposure to the subsidies for women on the probability of low birth weight child, infant death, child death before the age of 5, and single motherhood at the time of birth. All of the estimated effects are negative, suggesting a positive effect of pill subsidy exposure on children's health at birth. Some estimates, such as the reduction

in the probability of infant and children's deaths, are also non-trivial in size, compared to the mean. However, none of the effects attains statistical significance at conventional levels.

Table 8: The effect of mother's exposure to subsidies on infant health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LBW		Infant death		Child death <5 yo		Single mother	
Average Effect	-0.000638	-0.000661	-0.000186	-0.000275	-0.000204	-0.000292	-0.00257	-0.00241
	(0.000926)	(0.000941)	(0.000261)	(0.000265)	(0.000270)	(0.000272)	(0.00189)	(0.00189)
Constant		0.0482***	0.00480***	0.00402***	0.00561***	0.00472***	0.148***	0.132***
		(0.000869)	(0.000255)	(0.00028)	(0.000288)	(0.00032)	(0.0014)	(0.00206)
Demographic controls		x		x		x		x
Obs		1,370,937	1,313,397	1,278,162	1,313,397	1,278,162	1,313,397	1,278,162
R-sq		0.002	0.000	0.001	0.001	0.001	0.016	0.019
Mean of dep var	0.041		0.004		0.004		0.110	

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income. Standard errors clustered on the level of mother's municipality.

Table 8A: The effect of mother's exposure to subsidies on infant health: low SES women

VARIABLES	(1) LBW	(2) Infant Death	(3) Child death <5yo	(4) Single mother
Average effect	-0.0000	-0.000260	-0.000285	-0.00310
	(0.00116)	(0.000315)	(0.000340)	(0.00224)
Constant	0.0522***	0.00446***	0.00535***	0.146***
	(0.000919)	(0.000301)	(0.000352)	(0.00169)
Observations	804,075	804,075	804,075	804,075
R-squared	0.002	0.001	0.001	0.019
Mean of dep var	0.043	0.004	0.005	0.111

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

We next consider the potential effects of subsidy exposure on children's health, as measured by the probability of having had an inpatient stay at different age cutoffs. All estimates are negative, but only one attains statistical significance at conventional levels. There is a substantial reduction on the probability of having been hospitalized by age 5 among children of women treated to the pill subsidies. When the sample is restricted to low SES women, the size of the coefficients increases for two out of the three outcomes considered. The probability of hospitalization by age 10 is now also significant at the 10% level.

Table 9: The effect of mother's exposure to subsidies on the probability of children's hospitalizations

	(1) Hosp by age 1	(2) Hosp by age 5	(3) Hosp by age 10
Average effect	-0.00292	-0.00794**	-0.00560
	(0.00332)	(0.00350)	(0.00361)
Demographic controls	x	x	x
Birth cohorts	1963-1975	1963-1975	1963-1975
Constant	0.126***	0.317***	0.385***
	(0.00198)	(0.00242)	(0.00241)
Mean dep var			
Observations	1,288,104	1,288,104	1,288,104
R-squared	0.011	0.017	0.021

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income. Standard errors clustered on the level of mother's municipality.

Table 9A: The effect of mother's exposure to subsidies on the probability of children's hospitalizations: low SES women only

	(1) Hosp by age 1	(2) Hosp by age 5	(3) Hosp by age 10
Average effect	-0.00264	-0.00946**	-0.00653*
	(0.00382)	(0.00394)	(0.00373)
Constant	0.144***	0.357***	0.432***
	(0.00240)	(0.00265)	(0.00272)
Mean dep var			
Birth cohorts	1963-1975	1963-1975	1963-1975
Observations	814,403	814,403	814,403
R-squared	0.011	0.016	0.020

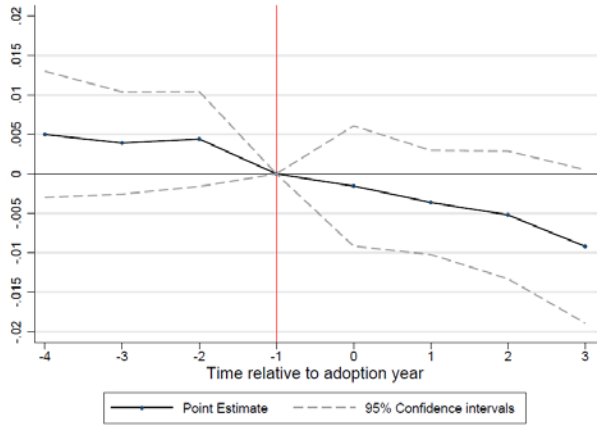
Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother. Standard errors clustered on the level of mother's municipality.

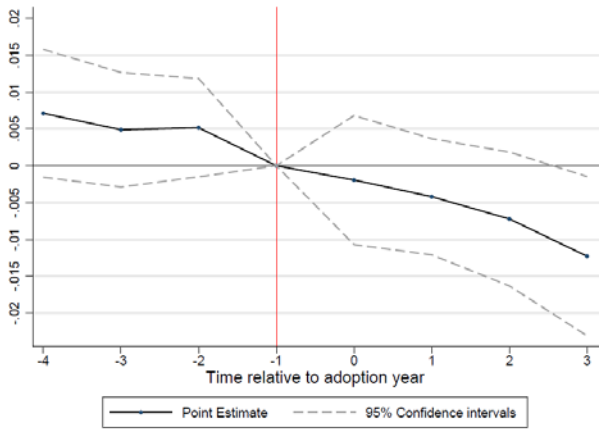
Figure 7 shows the estimates from an event-study type analysis for the probability of hospitalization by age 5. There are no differences in the pre-trends in children's hospitalizations across the set of treated and control municipalities. The plots also

Figure 7: Event-study type analysis of the probability of hospitalization by age 5

All women



Low SES women



Overnight hospitalizations for children happen relatively rarely and are indicative of serious health issues. Data on prescription sales linked to individual children offer a closer look at the types of health issues that affect the children of women affected by the pill subsidies. The evidence presented earlier that exposure to the pill improved women's socio-economic standing and the

likelihood that children will be born in two-parent households, suggests that children of women affected by the pill subsidies should exhibit better emotional and behavioral health (Akee et al, 2018). To test this hypothesis, we examine the effects of pill subsidies on the probability that children of affected women will ever consume anti-psychotic and ADHD-specific prescription drugs. Because the prescription data start in 2005, we limit the children's sample to those born after 1990. This is done for two reasons. First, the average age at which children are first prescribed ADHD drugs is 7, and the youngest age is 3. Many children "grow out of" the need to be medicated to control their ADHD by their late teenage years. Including adolescents in the sample, for whom we do not observe medication prescriptions at younger ages, and who are more likely to have mothers not treated to the subsidies, would thus bias the results. Second, children older than 15 are more likely to be out of their parents' household (age of majority in Sweden is 16). Additional analyses in the Appendix demonstrate that these sample restrictions do not substantially affect the results.

Table 10: The effect of mother's exposure to subsidies on children health outcomes: anti-psychotic and ADHD drugs

	(1)	(2)	(3)	(4)
VARIABLES	Ever Psychotics		Ever ADHD	
Average effect	-0.00264*	-0.00264*	-0.00204*	-0.00199*
	(0.00147)	(0.00148)	(0.00107)	(0.00108)
Constant	21.90***	21.28***	5.295***	4.963***
	(0.263)	(0.246)	(0.172)	(0.156)
Restrict birth year>1990	YES	YES	YES	YES
Linear birth year control	YES	YES	YES	YES
Demographic controls	NO	YES	NO	YES
Observations	1,135,897	1,105,634	1,135,020	1,104,789
R-squared	0.040	0.042	0.013	0.015
Mean of dep var	0.095	0.094	0.030	0.030

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

There are well documented trends over the 2000s in the rate of prescribing ADHD medications to children. To capture that trend, we include a linear control for the year of birth of the children. The estimates suggest that there is a 2-3 percent reduction in the probability that a child whose mother was exposed to the pill subsidies would ever have been prescribed anti-psychotic medications. The reduction in this probability for ADHD-specific drugs is a much stronger

reduction of about 6.6 percent. As Table 10A demonstrates, the effects are somewhat stronger in the population of low SES women.

Table 10A: The effect of mother's exposure to subsidies on children health outcomes: anti-psychotic and ADHD drugs, low SES women only

VARIABLES	(1) Ever Psychotics	(2) Ever ADHD
Average Effect	-0.00432**	-0.00242*
	(0.00187)	(0.00124)
Constant	22.51***	5.096***
	(0.303)	(0.181)
linear birthyear control	YES	YES
Observations	678,188	677,683
R-squared	0.040	0.014
Mean of dep var	0.104	0.034

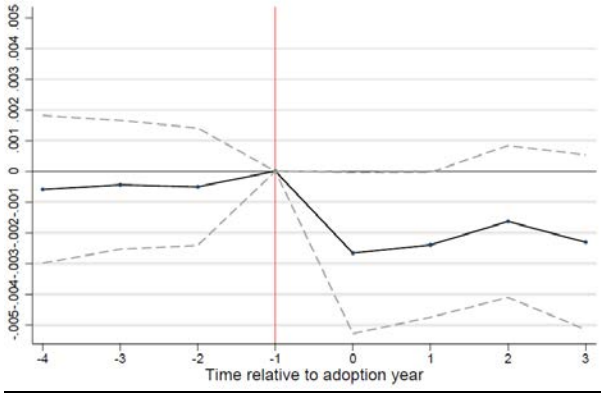
Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: Child birth year restricted after 1990; All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income

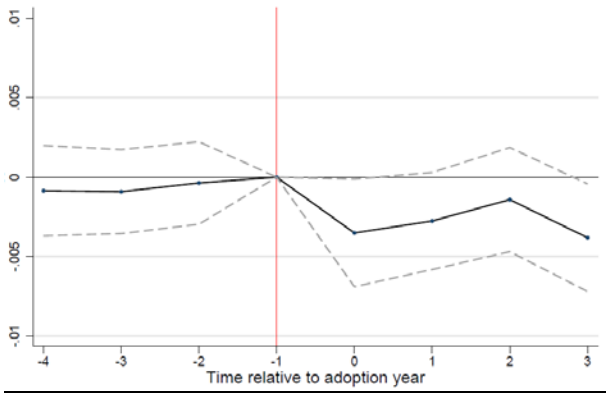
Figure 8 graphically presents the results from the corresponding event-study type analysis. There is no evidence of differences in the trends between treated and untreated women before the subsidies were adopted. At the same time, there is a clear downward shift in the probability of ADHD medication use among children of treated women relative to the rest.

Figure 8: Probability of using ADHD prescription medication

All women



Low SES women



The Appendix Table A4 presents the results from a series of placebo regressions in which we demonstrate that exposure to the subsidies had no substantial effects on the probability of children's use of prescription dermatology drugs or anti-epileptics. A priori, we wouldn't expect that maternal exposure to the pill subsidies would affect the propensity to use prescription medication for skin-related conditions. Epilepsy and seizures are rather rare and unlikely to be correlated with maternal socio-economic status or household composition. The estimates suggest no link between pill exposure and the incidence of these two conditions in the children's generation.

The effect of pill subsidy exposure of children's education outcomes

The final set of children's outcomes is the standardized score on the national achievement tests administered in 9th grade. This is an appropriate outcome because it is nationally representative and can be easily standardized across geographies. The total score is the sum of the individual scores pupils attain in English, Swedish, and Mathematics. All Swedish 9th-graders must take this exam, and the results from the tests are influential in the choice of high school (such as academic or vocational preparation). The last year for which we have data on these scores in 2014, which implies that the youngest children included in this dataset were born in 1999.

Table 11 presents the estimates. Because we standardize the final score, the coefficients are interpretable as percent changes. Thus, we find that exposure to the pill subsidies increases the total score of children eventually born to these mothers by 2.6 percent of a standard deviation. Including demographic controls for the mother's family SES increases this estimate to 2.85% of a standard deviation. Among low SES mothers, who are more likely to have been affected by the pill subsidies, the estimate is even higher at 3.3% of a standard deviation.

Table 11: The effect of mother's exposure to subsidies on children education outcomes: total score on standardized 9th grade national exam

	(1)	(2)	(3)	(4)
VARIABLES	Standardized total score			
Average effect	0.0263**	0.0285***	0.0328**	0.0244
	(0.0113)	(0.0105)	(0.0134)	(0.0160)
Constant	0.0280***	0.369***	-0.0586***	0.231***
	(0.00661)	(0.0101)	(0.00818)	(0.0101)
Demographic controls		x		
Sample	All	All	Low SES	High SES

Observations	678,807	657,172	448,997	228,075
R-squared	0.014	0.044	0.013	0.025

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

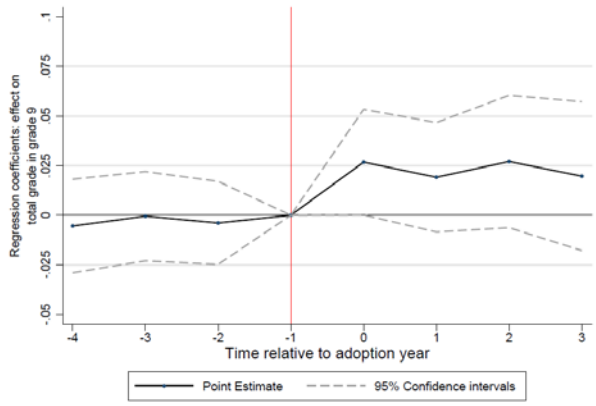
Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income

The size of these effects is similar to what has been reported from the STAR experiment on reducing class size (Krueger, 1999). They are also comparable to findings from Sweden by Frederiksson et al (2013), who argue that reducing class size by 1 results in a reduction in cognitive ability by 0.032 to 0.047 of a standard deviation.

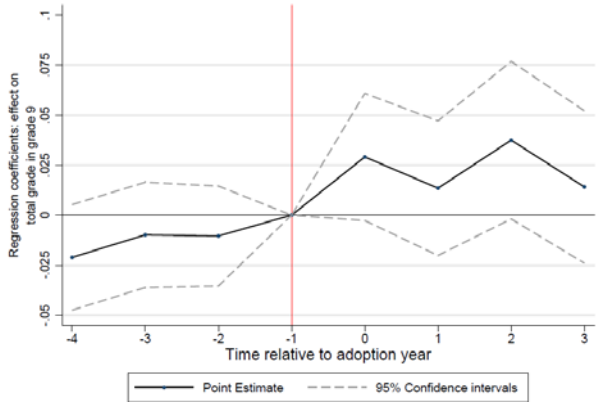
Figure 8 presents the results in an event-study type framework. The graphs show no significant differences in pre-trends between the treated and control municipalities before the initiation of the subsidies. They also demonstrate that the effects are already visible within the first cohort of treated women, and persist with similar sizes over time.

Figure 9: Event-study type analysis of the effects on children's education outcomes

All women



Low SES women



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.

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 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 A1: 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.*

Figure A1: Geographic locations adopting subsidies by affected birth cohort

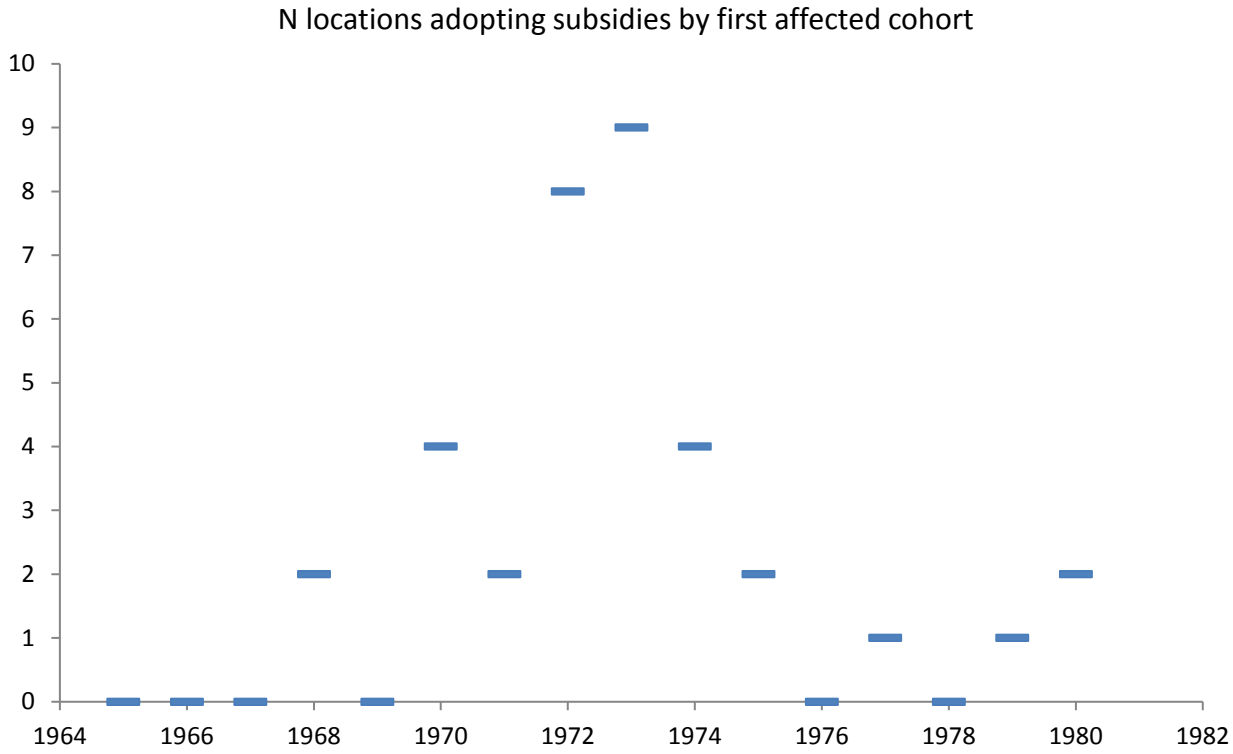

Table A2: Effects on fertility using the sample of women born 1963-1985

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No children		N children		<20 at first birth		>35 at first birth	
Avg effect	-0.0035*	-0.00383*	0.0129*	0.013**	-0.0017	-0.0017	0.00066	0.00051
	(0.0018)	(0.00194)	(0.0066)	(0.0067)	(0.0012)	(0.0012)	(0.0012)	(0.0011)
Constant	0.152***	0.154***	1.97***	1.97***	0.053***	0.029***	0.047***	0.065***
	(0.0022)	(0.00220)	(0.0073)	(0.00724)	(0.0012)	(0.0011)	(0.0011)	(0.0012)
Obs	1,169,869	1,131,526	1,169,869		1,169,869	1,131,526	1,169,869	1,131,526
Birth years	1963-1985	1963-1985	1963-1985	1963-1985	1963-1985	1963-1985	1963-1985	1963-1985
R-sq	0.040	0.042	0.059	0.059	0.005	0.014	0.020	0.022
Mean of dep var	0.207		1.708		0.030		0.050	

Table A2A: Low SES women only

	(1) No children	(2) N Children	(3) <20 at first birth	(4) >35 at first birth
Average effect	-0.00408*	0.0147*	-0.00179	0.00277*
	(0.00240)	(0.00836)	(0.00174)	(0.00166)
Constant	0.142***	2.001***	0.0629***	0.0397***
	(0.00263)	(0.00905)	(0.00155)	(0.00173)
Birth years	1963-1985	1963-1985	1963-1985	1963-1985
Observations	438,939	438,939	438,939	438,939
R-squared	0.005	0.009	0.009	0.004
Mean of dep var				0.059

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born men, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income

Table A3: Exposure to subsidies and educational outcomes for men by age 30

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Compulsory education		Professional education		College		Some college/Certificate	
Avg effect	-0.000481	-0.000842	-0.000440	-0.000471	-0.00278	-0.00228	-0.000427	-0.000328
	(0.00326)	(0.00319)	(0.000631)	(0.000607)	(0.00460)	(0.00419)	(0.00196)	(0.00205)
Constant	0.182***	0.249***	0.0161***	0.0128***	0.0952***	0.0346***	0.0541***	0.0302***
	(0.00256)	(0.00293)	(0.000696)	(0.000742)	(0.00214)	(0.00273)	(0.00176)	(0.00187)
Birth cohorts	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975	1963-1975
Demographic controls		x		x		x		x
Mean dep var								
Obs	743,065	721,911	743,065	721,911	743,065	721,911	743,065	721,911
R-squared	0.018	0.039	0.002	0.007	0.040	0.089	0.007	0.015

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born men, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income

Table A4: Exposure to subsidies and children's consumption of dermatology and anti-epilepsy prescription medications

VARIABLES	(1)	(2)	(3)	(4)
	Ever derma RX		Ever epilepsy RX	
Average effect	0.00249	0.00243	7.96e-05	0.000202
	(0.00209)	(0.00211)	(0.000535)	(0.000529)
Constant	8.074***	8.292***	2.446***	2.379***
	(0.315)	(0.305)	(0.0680)	(0.0688)
Linear birthyear control	YES	YES	YES	YES
Demographic controls	NO	YES	NO	YES
Observations	1,135,897	1,105,634	1,135,897	1,105,634
R-squared	0.007	0.007	0.004	0.004
Mean of dep var	0.357	0.356	0.013	0.013

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: Child birth year restricted after 1990; All regressions include municipality fixed effects and mothers' birth cohort fixed effects. Demographic controls include: a dummy for foreign born women, a dummy for maternal grandmother's foreign origin, a dummy for divorced maternal grandmother, and indicators for quintile of grandfather's income