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Subsidized Contraception, Fertility, and Labor Supply: Evidence from Regional Policy Changes

by

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Abstract

Concerns about the implications of unintended childbearing have caused policy makers to instigate various family planning programs. Despite the huge interest in such interventions there is however very scarce evidence on their effectiveness. This paper provides the first attempt to evaluate the economic consequences of one type of preventive policy: subsidized contraception. I make use of a series of unusual policy experiments in Sweden where different regions beginning in 1989 started subsidizing the birth control pill. These reforms are attractive because they did not coincide with other changes in the Swedish family planning services and because they were significant: on average the subsidy was about 75 percent of the price and applied to all types of oral contraceptives. My identification strategy takes advantage of the fact that the reforms were implemented successively and only targeted specific cohorts of young women, mostly teenagers. This generates cross-section and cross-cohort variation in exposure to the reforms which is used to identify the effect of interest. Using extensive Swedish population micro data seaming from administrative registers I study the impact of the subsidies on: fertility, earnings, monthly wages, educational attainment, welfare dependence, disposable income, employment and marital status. The results suggest that the subsidies significantly improved the socioeconomic outcomes of exposed women and reduced teenage childbearing rates. The estimates are robust to a number of different sensitivity checks.

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1. Introduction

Unintended childbearing is both frequent and widespread. For instance, in the U.S. more than 60 percent of all pregnancies are unplanned. The social and economic consequences are potentially severe since unintended childbearing among other things is associated with low birth weight, childhood abuse, and worse socioeconomic and health outcomes of both mother and child. In addition, unplanned pregnancies currently lead to approximately 1.5 million abortions annually in the U.S. alone (Institute of Medicine 1995). Concerns about the implications of unintended childbearing have caused policy makers to instigate various family planning programs.[1] Despite the huge interest in such interventions there is however very scarce evidence on their effectiveness. This is likely because most programs have been introduced simultaneously for all women, and therefore simply do not allow for a meaningful evaluation.

This paper investigates the importance of one type of preventive policy: subsidized contraception. I make use of a series of unusual policy changes in Sweden where different counties beginning in 1989 started subsidizing the birth control pill. The reforms are attractive because they did not coincide with other changes in the Swedish family planning services and because they were significant: on average the subsidy was about 75 percent of the price and applied to all types of oral contraceptives. My identification strategy takes advantage of the fact that the reforms were implemented successively and only targeted specific cohorts of young women,

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

mostly teenagers. This generates cross-section and cross-cohort variation in exposure to the reforms which is used to identify the effect on women's fertility and socioeconomic outcomes.

There are many reasons for why subsidizing the birth control pill might matter for women's outcomes. The first argument is that young women may lack stable income sources, and therefore be more likely to prematurely end or delay treatment. Since the timing of the treatment is crucial for its outcome even slight violations from the programme increases the risk of unintended pregnancies. Of course, having access to non-expensive contraceptives might also mean that women raise their level of sexual activity, increasing the likelihood of a pregnancy. This makes the net effect of a subsidy on fertility an empirical question.

A subsidy might also affect socioeconomic outcomes through its effect on fertility and marriage. Early childbearing, family size and marital status are all factors that have been shown to be correlated with educational and labor market outcomes.[2] Moreover, it has been suggested that oral contraceptives raise the returns to investments in education and work by reducing unexpected interruptions from the labor market and school (Bailey 2007; Weiss 1986; Mincer and Polachek 1974). This means that the birth control pill can have a direct effect on socioeconomic outcomes. A similar story is provided by Chiappori and Oreffice (2007) who propose that access to contraceptives may improve the woman's bargaining position within a couple,

[2] References include: Angrist and Evans (1998); Ashcraft and Lang (2007); Åslund and Grönqvist (2007); Björklund, Ginther and Sundström (2007); Bronars and Groggers (1994); Geronimus and Korenman (1992); Holmlund (2005); Hotz, McElroy and Sanders (2005); Hotz, Klerman and Willis (1996). Hotz, Mullins and Sanders (1997); Kearney and Levine (2007a); Klepinger, Lundberg and Plotnick (1999); Maynard (1996); Stevenson and Wolfers (2007).

leading to an increased share of the household's resources; something that potentially could reduce female labor supply through a standard income effect.[3]

This paper is related to a series of recent studies investigating the role of the birth control pill for women's outcomes. Bailey (2006), Goldin and Katz (2002) and Guldi (2007) exploit regional variation in the access to the birth control pill in the U.S. at the time of its introduction in the 1960s. The results suggest that access to the pill lead to increased labor supply, later age at first marriage and delayed childbearing. Bailey (2007) takes advantage of variation in state laws regulating contraceptive sales from 1873 to 1965 (Comstock laws) and show that access to the pill accelerated the reduction in U.S. fertility rates. More closely related to my paper is Kearney and Levine (2007b) who investigates how expanded family planning services in the U.S. affected fertility. The results show that the reforms lead to a nine percent decrease in births to women age 20–44.[4]

My paper extends the literature in several important ways. First and foremost, it is the first to evaluate the economic consequences of subsidizing oral contraceptives. As already suggested, this is a question of great interest for policy makers. Second, the impact of a recent subsidy on oral contraceptives is arguably

[3] If having access to the subsidy means that women have more sex partners it is also possible that the policy can influence socioeconomic outcomes through increased self confidence. Because of the many mechanisms that might be at work, this paper will not be able to disentangle the direct affect of the subsidy through fertility versus the indirect effect through these other channels.

[4] Kearney and Levine argues that the policies are analogous to a subsidy on all types of contraceptive technologies. However, since the reforms also encompassed other features associated with family planning services (e.g. medical examinations and laboratory tests) it is not obvious that they can be considered as equivalent to a subsidy only on contraceptives.

more relevant for the contemporary debate over contraception since most countries already have introduced the birth control pill. Third, the rich population register data used in this paper allows a unique opportunity to study a wide variety of different economic, demographic and educational outcomes, ranging from adolescence to adulthood, and to study differential effects with respect to socioeconomic background.

I begin the empirical analysis by investigating the relationship between the reforms and fertility. I find that women who had access to the subsidy for more than 6 years are about 30 percent less likely to become teenage mothers. This effect is stronger for women from poor socioeconomic background. There is however no significant effect on total fertility or marital status. I proceed by studying the economic consequences of the policies. My results suggests that long-term access is associated with 4 percent higher annual earnings, one month more of schooling and 30 percent lower probability of receiving welfare.

It is important to recognize that my empirical strategy hinges on the supposition that regional and cohort specific fixed effects together with a set of covariates control for unobserved factors that may confound the estimates. I provide several pieces of evidence supporting this identifying assumption. First, under the hypothesis that observed variables are at least equally as important as unobserved variables dropping the former can provide insight as to whether the results are likely to be driven by omitted characteristics. Reassuring is that I find the estimates robust to dropping key covariates. Second, if the estimates are spuriously driven by long-term differential trends across regions in e.g. fertility I would likely find similar results for the women's mothers. However, when assigning daughters exposure to the reforms to their mothers I find no significant estimates. Third, the results show no significant effect of exposure on the probability of high school graduation but a strong effect on the probability of graduating from university. I interpret this as suggestive evidence that the results are not suffering from omitted variable bias because: (i) unobserved heterogeneity plausibly affect all levels of education in a similar way; (ii) it is not likely that the subsidies would affect women as early as in high school because the amount of exposure is not sufficiently large. Last, because the oldest individuals affected were 24 years old the subsidies should affect the probability of early childbearing but not the likelihood of having a child before, say age 26 (other than possible through learning or peer effects). Indeed, I find that the estimates turn insignificant above the critical point. All together, these findings strengthen my belief that the estimates are not suffering from omitted variable bias.

The rest of this paper is structured as follows. In Section 2 I describe the institutional background and discuss my empirical strategy. Section 3 discusses the data and sample selections. Section 4 contains the estimation results and Section 5 concludes.

2. Background

Because of its relative efficiency and few side effects the birth control pill is the leading contraceptive method among young Swedish women. The aim of this section is to describe the institutional setting surrounding the birth control pill and the subsidies. I then discuss under what assumptions it is possible to identify the impact of the reforms on women's outcomes.

2.1 Institutional setting[5]

In Sweden, oral contraceptives are sold by prescription from a doctor or midwife. The typical procedure for a young woman wishing to use the pill is that she schedules an appointment at a youth clinic where she meets the physician. Youth clinics are health centres for teenagers that offer consultation about contraceptive issues and medical examinations and there is at least one clinic in each municipality. Instead of going to a youth clinic, it is possible to visit a hospital or a private doctor, but the procedure is the same. Once at the clinic there is a discussion about various contraceptive methods. [6] If the physician deems oral contraceptives appropriate he/she prescribes the drug and the woman can then collect it at the state pharmacy. Important to note is that it is not required that parents give their consent to the treatment. The physician is bound by the professional secrecy and if a girl does not want her parents to know about the treatment the physician cannot contact them. It is however standard practice that the doctor or midwife in these cases tries to convince the girl to tell the parents herself.

The issue of providing targeted financial support for oral contraceptives was raised in the late 1980s and was a reaction to a period of high abortion rates among teenagers. Teenage abortion rates had been rising steadily since the mid 1960s and

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[5] This section draws primarily on Socialstyrelsen (1994, 2005).
[6] Of course, this is conditional on the physician having found contraceptives appropriate.

the general opinion among policy makers was that remedial measures were needed to fight unintended pregnancies. The Swedish government had already since the early 1970s been directing large resources towards various family planning policies including the establishment of youth clinics and nationally subsidized oral contraceptives for *all* women. However, in 1984 the government changed the discount regulations surrounding the birth control pill and women's cost for the treatment quadrupled. After that year the sales of oral contraceptives dropped significantly. This event in combination with the high teenage abortion rates seems to have been what motivated the reforms.

In 1989 the municipality of Gävle was the first region to introduce a subsidy and in the following years many regions launched reforms based on the same principles: meaning that the policies targeted specific cohorts of young women. On average the subsidy was 75 percent of the price on *all types* of oral contraceptives (Socialstyrelsen 1994).[7] Table 1 contains information about the reforms up to 1993, which is the last year for which this is available. We can see that most of the regions that introduced the subsidy are counties but there are also a few municipalities on the list. Eight counties had not implemented the subsidy by the end of 1993.[8] Note that both the starting dates and the targeted cohorts vary across regions. We can see that only a few regions provided the subsidy to women above age 20. In this context it is worth mentioning that it was not possible to get access to

[7] Unfortunately, I do not have access to information about the regional specific subsidy rates. However, this information should be possible to obtain through official records and the plan is to collect it.

[8] The fact that some regions may have implemented the reforms after 1993 introduces some complications for my analysis. This is an issue I will return to in subsequent sections.

the subsidy by simply going to a youth clinic in a neighbouring region since it was tied to region of residence. Observe also that the reforms did not coincide with other changes in the Swedish family planning system (Björklund 2006).

Prior to the reforms a full year's supply of the birth control pill was sold for around 640 SEK (in today's prices), approximately 100 USD.[9] Although this price might seem fairly low, for young teenage women without own incomes the costs of oral contraceptives could very well amount to a large fraction of their budget. This situation is especially likely to be important for girls that for some reason could not ask their parents for money to get the pill, and is worsened by the regularity requirements surrounding the treatment programme. In order for oral contraceptives to provide maximum protection against pregnancies the treatment must be administrated for 21 days followed by a seven day recess. Should the pill be taken for a less than 21 days, or if the recess is longer than one week, protection is immediately endangered. In fact, anecdotal evidence from clinics suggests that many unintended pregnant girls stated that they had not been able to start a new treatment because they had not afforded the pill at the day the programme was scheduled to begin.

Because of the large public interest in the reforms the National Board of Health and Welfare (Socialstyrelsen) launched an evaluation of its impact on abortions. By comparing teenage abortion rates in regions which had introduced subsidies to regions which had not, the evaluation concludes that teenage abortion rates fell by roughly 25 percent (Socialstyrelsen 1995). However, since the

[9] Since the state pharmacy charges a uniform price there is no regional variation in the price of oral contraceptives prior to the reforms.

empirical strategy is based on comparing time trends across regions the results should be interpreted with caution.

Did the subsidies really increase the use of the birth control pill? Unfortunately there is no information on consumption of oral contraceptives for my main sample. However, using other sources of data I present two pieces of evidence that together provide suggestive evidence that this actually was the case. First, the state pharmacy (Apoteket) provided me with annual information on the sales of oral contraceptives for each county starting in 1980.[10] Sales are here measured in terms of average daily dosages per 1000 women.[11] Figure 1 plots sales against time separately for regions which had implemented the reforms by 1993 (treatment regions) and regions which had not (control regions). We can see that both the treatment and the control regions experienced increased sales up until 1984, after which there is a sharp decline. This decrease is likely due to the major nation wide change in discount regulations. We can also see that the sales in the treatment and control regions match almost perfectly up until 1989 (marked by the vertical line) and then start to diverge, increasing more in treatment regions.

[Figure 1 about here]

Although suggestive, the graph masks whether the increase is differentially stronger for young women. To investigate this I use two rounds of Undersökningen

[10] The state pharmacy is the sole provider of prescription drugs in Sweden. Thus, reported sales should very well approximate consumption.
[11] The measure is standardized for varying strengths of the pill.

av Levnadsförhållanden (ULF), which is a survey asking women whether they have consumed oral contraceptives within the last two weeks of the survey date. ULF is a recurrent survey of a random sample of about 3,500 Swedish women aged 16–84 and the sample size net of attrition is sufficiently large to allow me to disaggregate the data by cohort.[12] The question was not asked in all rounds, so I use information from 1980/81 and 1996/97; one round before and one after the reforms.[13]

Statistics Sweden which administrates ULF compiled the data on my behalf. It turns out that in the first round 25.8 percent of 16–20 year olds stated that they had taken oral contraceptives within the last two weeks prior to the survey. The same figure for 21–24 year olds was 35.8 percent, and for 25–30 year olds 25.3 percent. All cohorts experienced increased use of the pill up until the 1996/97 round where the respective numbers were 35, 45.9 and 30.6 percent. This means that the consumption of oral contraceptives increased by 32 percent for age group 16–20, by 27 percent for individuals aged 21–24 and by 23 percent for 25–30 year olds. Thus, the increase is indeed largest in the cohorts affected by the reforms. Of course, this can be due to a range of different factors not related to the reforms. The most obvious objection is that the Swedish women may have made their sexual debut earlier. However, the average age at first intercourse has been stable around age 16 since the 1960s (Forsberg 2005). In fact, during the 1980s this number did actually increase.

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[12] Attrition in ULF is generally about 25 percent.

[13] Unfortunately, sample size restrictions, in combination with the fact that some regions may have implemented the reforms after 1993, prevents me from disaggregating the data by regions.

I believe that these two facts together provide sufficient evidence that the reforms really did increase the use of oral contraceptives among young women; although the exact magnitude is very difficult to tell.

2.2 Evaluation framework

This paper investigates the consequences of subsidizing oral contraceptives for women's economic and demographic outcomes. My empirical strategy takes advantage of the cross-section and cross-cohort variation generated by the reforms to identify the effect of interest. This is done by estimating regression models of the following form

$$Outcome_{ibc} = \alpha_0 + Exposure_{bc}\alpha_1 + X_i'\alpha_2 + \lambda_b + \lambda_c + v_{ibc}$$

where the outcome is indexed for individual *i* in birth cohort *b* from county *c*; $Exposure_{bc}$ is a measure of treatment intensity, i.e. the cumulative exposure to the policy; X_i is a vector of background characteristics; λ_b and λ_c are birth cohort (year×month) and county specific fixed effects.

This is a standard difference-in-differences model in which variation in exposure depends on the interaction between region of residence and birth cohort. It therefore ignores permanent differences between regions and cohorts which are absorbed by the fixed effects. Identification is made possible through the changes in the outcome across regions and cohorts generated by the introduction of the policies. Thus, the model hinges on the assumption that once I condition on region and cohort (possible also on background characteristics) there should be no unobserved factors correlated with exposure and the error term, i.e. $E[v_{ibc} | Exposure_{bc}, X_{iac}, \lambda_b, \lambda_c] = E[v_{ibc} | X_i, \lambda_b, \lambda_c]$. This assumption is violated if there are differential trends in the outcome across regions or if the introduction of the subsidy corresponded to a shock affecting the outcome; an issue I will address carefully in the empirical analysis.

3. Data

The data used in the empirical analysis comes from the IFAU-database, which covers the entire Swedish population age 16–65 during the period 1985–2004.[14] One part of the database includes annual information on standard individual characteristics (earnings, place of residence, etc). It also contains several registers with educational information, as well as a "multi-generation" register linking kids to their biological parents. Below I describe the sample selections and the information used.

My main sample consists of all women born in Sweden in the years 1965–1975. The reason for making this restriction is that including older cohorts increases the likelihood that some individuals may have left their homes at the age when I can observe them, enhancing the risk of both measurement error and selective sorting. Furthermore, I cannot include younger cohorts since I only have detailed knowledge about the subsidies up until 1993 and wish to avoid the possibility that later cohorts in the control regions may have been exposed.[15] For most cohorts I define region of

[14] The database is based on information originally collected by Statistics Sweden.
[15] I know that some regions did in fact introduce a subsidy after 1993, although I have no information on what cohorts were eligible or the exact starting date.

residence according to where the woman lived at age 16. Individuals born 1965–1968 are assigned a residential area based on where they lived in 1985.

I link all subjects to their biological parents using the multi-generation register and add information on parents' education and earnings in 1985. With the help of the multi-generation register I then add information on the birth dates of the subjects' children.[16] Using place of residence in combination with birth date (year and month) I construct a variable measuring the cumulative length of exposure to the subsidies.

In the empirical analysis I focus on several outcomes of fertility, human capital and labor market status. Teenage childbearing is defined as having the first child no later than age 20. Years of schooling is imputed from information on highest completed level of education. I also study whether the woman has completed university or high school. In addition, my data contains information on a wide range of labor market and income variables: annual earnings, employment status, welfare, and disposable income. For a subsample of individuals employed in the municipality, county or private sectors there is also information on monthly wages.

I observe all outcomes in 2004 when the subjects are age 29–39, except welfare take-up which is measured at age 25. Table A.1 contains a detailed description of how these variables have been constructed and from which registers the information has been collected. Table A.2 contains summary statistics.

4. Estimation results

[16] Note that the multi-generation register contains information on the woman's number of children and her children's birth dates even though the children themselves may be too young to be included in the population sample of the IFAU-database.

This section presents the results from my empirical analysis. I begin by providing the main results and continue in Section 4.2 with some robustness checks. Further robustness checks and an attempt to sort out the mechanisms can be found in Section 4.3. Section 4.4 contains an analysis of differential effects with respect to background characteristics.

The key variables of interest in the regressions are four indicator variables measuring the cumulative exposure to the reforms. The reference group is individuals with no exposure. I also present results from identical models except that exposure instead is measured linearly. All regressions include fixed effects for county of residence and birth cohort (year×month). In addition, I control linearly for each parent's earnings and with dummies for each parent's highest completed level of education (five levels), missing information on education or earnings, and county specific trends. Because there are reasons to suspect serially correlated outcome variables all standard errors are clustered at the county level (cf. Bertrand, Dufflo and Mullainathan 2004).[17]

To conserve space, I do not report estimates for the control variables but it is worth mentioning that all estimates are significant and display expected signs: higher educated parents means a lower probability of becoming a teenage mother, fewer children, more years of schooling, higher earnings, lower probability of being

[17] I have also experimented with accounting for group error structure at the county×cohort level (cf. Moulton 1990). This approach produces somewhat more precisely estimated standard errors but does not affect the overall conclusions in the paper. Furthermore, clustering at the municipal level provides very similar standard errors.

non-employed or receiving welfare, and higher disposable incomes. The same is true for high income parents.

4.1 Main results

Table 2 contains the estimation results for fertility and marital status. I start by asking whether the policies affected the number of children borne. From the results in column (1) it is clear that this is not the case. The F-statistic which tests the null hypothesis that the coefficients on exposure are jointly zero is not significant. Column (2) displays estimates for the probability of becoming a teenage mother. We can see that woman exposed to the subsidy for more than 72 months are on average about 33 percent (–.22/.067) less likely to become teenage mothers. The F-statistic strongly rejects the null hypothesis that the coefficients are jointly equal to zero. A similar conclusion can be drawn from the linear measure in Panel B. On average, one additional year of exposure reduces the probability of becoming a teenage mother by .2 percentage points. It is also possible that the subsidy affected marital status, for instance through shotgun marriages. However, columns (3) and (4) show that exposure has no significant effect on the probability of being currently divorced or the probability of being currently married.

Since the reforms targeted specific age cohorts of young women, the oldest being age 24, we should not expect to see any evidence that they affected birth timing after age 24, other than possibly through cross-cohort spill over effects. If they do, one might suspect that the reforms were not completely exogenous. In columns (5)–(7) I show results from regressions where I investigate the consequences for the estimates of successively expanding the age restriction. Column (5) shows that there is a statistically significant effect on the probability of having the first child before age 25. Note though that both the precision and the magnitude of the estimates have dropped considerable compared to the results in column (2). Here, I find that being exposed for more than 72 months decreases the probability of having the first child before age 25 with about 10 percent (–.027/.264). This is expected since a few counties offered the subsidy to women above age 20. The estimate is still significant in column (6) but is even weaker in magnitude; in column (7) the coefficient is insignificant. In fact, increasing this limit further renders even less precise estimates. I believe that this finding gives more credit to my claim that there are no confounders driving the results.

[Table 2 about here]

I next look at the impact of the reforms on socioeconomic outcomes. There results are shown in Table 3. Column (1) provides the estimates for years of schooling. We can see that exposure to the subsidy significantly increases educational attainment. Being exposed for more than 72 months increases schooling by about 1 month. Turning to the labor market outcomes in columns (3) and (4) I find no statistically significant effect on the probability of being non-employed but a significant effect on annual earnings. Long term access to the subsidy increases earnings with about 4 percent. I also find that long-term exposure decreases the probability of receiving welfare by about 37 percent (–.033/.087), although the F-statistic is just above the 5 percent significance level. Last, I find no significant effect for disposable incomes, as shown in column (5).

[Table 3 about here]

In summary, the results suggests that exposure to the subsidy significantly lowers the probability of becoming teenage mother, increases years of schooling and earnings, and there are indications that it decreases the probability of becoming a welfare recipient. However, I do not find that employment status, marital status or disposable income is affected. Next I assess the robustness of these results.

4.2 Robustness checks

Remember that my identification strategy is based on several assumptions. First, individuals should respond to the introduction (or absence) of the subsidy by selectively moving. Second, there should not be differential trends in the outcomes between treated and control regions. Although I find it highly unlikely that families would change their residential area just because of the subsidy I do provide some evidence on whether unobserved characteristics may drive the results by investigating what happens to the estimates when removing some key covariates. Education and earnings is perhaps the variables most likely to be associated with selective moving. If unobserved factors are at least equally important as these observed characteristics dropping the latter can provide insights as to whether unobserved factors are likely to explain the results. If I find that the estimates are sensitive to removing covariates one might suspect that also omitted variables may be important. Similarly, removing county specific trends can give information on the likelihood of differential trends biasing the estimates.

Table 4 presents results where I successively remove covariates. To conserve space I only report estimates for the linear measure of exposure, but the results are similar to using dummies to define exposure. Reassuring is that the coefficients are not very sensitive to removing controls for parents' education, earnings or to excluding county specific trends.

[Table 4 about here]

It is still possible that women living in regions that introduced the subsidy would have experienced changes in the outcomes even in the absence of the policy. However, if policy changes are exogenous, then future values of the policies should not affect current outcomes, e.g. women's fertility patterns. To investigate this, I assigned observed exposure to the subjects' mothers. The results from this exercise are shown in Table 5. As can be seen, there is no evidence that future subsidies affected either fertility or socioeconomic outcomes. None of the F-statistics are significant on the 10 percent level which perhaps is surprising given the large sample sizes.

[Table 5 about here]

Having established that the results does not seem to be driven by omitted factors I next continue the analysis by trying to sort out what mechanisms may explain our results.

4.3 Sorting out the mechanisms

In this section I take a closer look at some of the socioeconomic outcomes. By doing this I hope to disentangle some of mechanisms may underlie our results. In the process I also provide more robustness checks.

I start by asking whether the significant estimates for years of schooling are different in various parts of the educational distribution. This is done by estimating separate equations for the effect of exposure on the probabilities of completing high school or university. The results are shown in columns (1) and (2) in Table 6. There is no significant effect on the probability of completing high school but a strong effect on the likelihood of graduating from university. I interpret this result as suggestive evidence that the results are not suffering from omitted variable bias because: (i) any unobserved heterogeneity plausibly affect all levels of education in a similar way; (ii) it is not likely that the subsidies would affect women as early as in high school because the amount of exposure may not be sufficiently large.

Previous I found that exposure is marginally insignificantly related to any receiving welfare. In columns (4)–(6) I explore if access to the subsidies affects the amount of welfare received. I do this by looking at the effect on the probability of being above the *j*th quartile in the welfare distribution (conditional on having received welfare). We can see that all estimates are statistically significant and the effect is economically stronger further up in the distribution.

[Table 6 about here]

4.4 Differential effects

I now turn to investigating whether the effect varies by background characteristics. Table 7 displays estimates for the linear measure of exposure; although the results are not sensitive to how I define exposure. The focus here is on parent's education and earnings. Each cell represents a separate regression. "Academic family" is defined as having at least one parent who has completed at least theoretical/preparatory high school. "Non-Academic family" is defined as both parents having at most vocational high school education. In a similar way, "High-income family" is defined as at least one parent having above median earnings (measured separately for mothers and fathers). "Low-income family" is families with both parents below the median in their respective earnings distribution.

We can see that the effect of exposure on teenage childbearing is significantly more negative for women from "Non-Academic" and "Low-income" families. This is consistent with the story that subsidizing oral contraceptives is more likely to be beneficial for women without any stable sources of income. There are also indications that the impact on welfare is driven by women from poor socioeconomic background. However, I do not find any evidence of differential effects for the other outcomes.

[Table 7 about here]

4.5 Back of the envelope calculations

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5. Concluding Remarks

Concerns about the implications of unintended childbearing have caused policy makers to instigate various family planning programs. Despite the huge interest in such interventions there is however very scarce evidence on their effectiveness. This paper provides the first attempt to evaluate the economic consequences of one type of preventive policy: subsidized contraception.

I make use of a series of unusual policy experiments in Sweden where different regions beginning in 1989 started subsidizing the birth control pill. These reforms are attractive because they did not coincide with other changes in the Swedish family planning services and because they were significant: on average the subsidy was about 75 percent of the price and applied to all types of oral contraceptives. My identification strategy takes advantage of the fact that the reforms were implemented successively and only targeted specific cohorts of young women, mostly teenagers. This generates cross-section and cross-cohort variation in exposure to the reforms which is used to identify the effect of interest.

Using extensive Swedish population micro data seaming from administrative registers I study the impact of the subsidies on: fertility, earnings, monthly wages, educational attainment, welfare dependence, disposable income, employment and marital status. The results suggest that the subsidies significantly improved the socioeconomic outcomes of exposed women and reduced teenage childbearing rates. The estimates are robust to a number of different sensitivity checks.

In continuing work I will try to investigate whether the impact on annual earnings is driven by an increase in labor supply or by a change in wages. I will also try to do some back-of-the-envelope calculations of the social costs and benefits of the subsidies.

This paper also offers several possible interesting avenues for further research:

Alternative outcomes: Abortions; Women's health; Risk of cancer;

Child outcomes: Birth-weight; Cognitive skills; delinquency;

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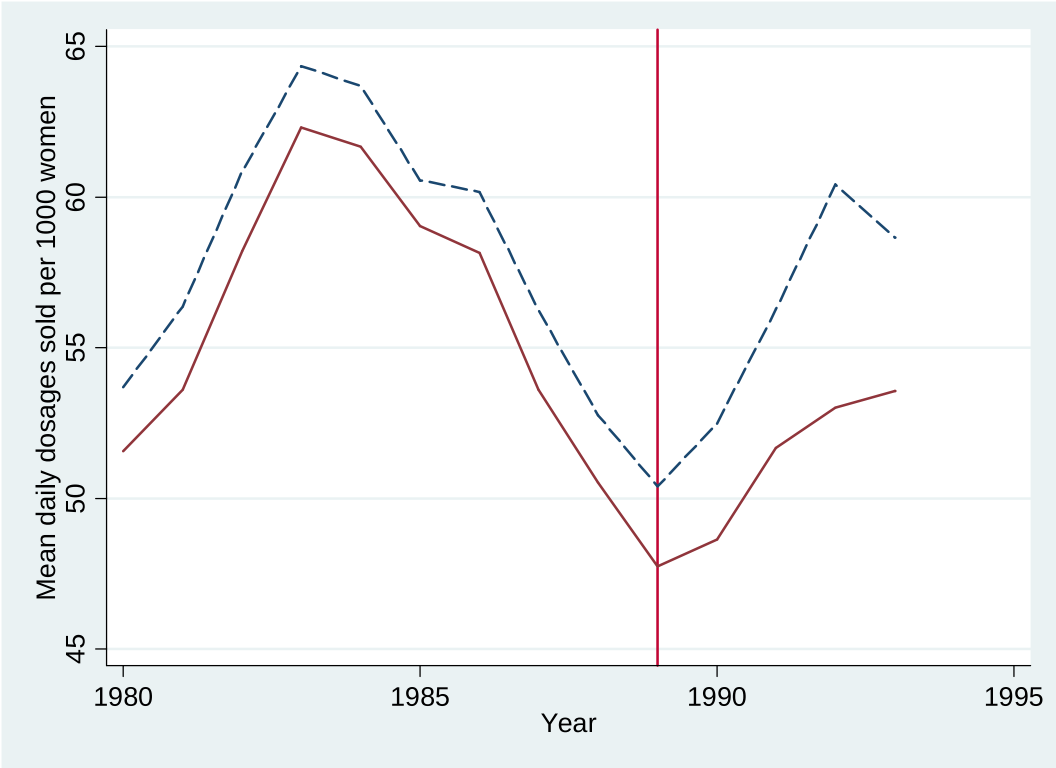

Figure 1. The mean number of daily dosages of oral contraceptives sold per 1000 women. Dashed line represents treated regions and solid line control regions. Vertical line is the first year of the reforms.

Table A.1. Definitions of key variables and data sources

Variable	Definition	Data source
Teenage mother	Indicator = 1 for having first child no later than age 20; 0 otherwise.	Multigeneration register
Number of children	Recorded number of children.	Multigeneration register
Years of schooling	Computed from highest completed level of education as follows: Short compulsory school = 6 years; Long compulsory school = 9; High school ≤ 2 years = 11; High school > 2 years = 12; University ≤ 2 years = 14; University > 2 years = 15; Graduate school = 19.	Employment register
High school	Indicator varialble = 1 for highest completed level of education being high school ; 0 otherwise.	Employment register
University	Indicator varialble = 1 for highest completed level of education being university ; 0 otherwise.	Employment register
Non-employed	Indicator variable = 1 for employment status "not employed" on November 1, 2004.	Employment register
Earnings	Labor related incomes (including self–employment) measured in hundreds of SEK.	Employment register
Monthly wages	Observed for all public employees and a sample of the women employed in the private sector.	Wage and occupation register
Welfare	Indicator variable = 1 for the incidence of welfare at age 25; 0 otherwise.	LOUISE
Disposable income	After tax income plus all transfers recieved.	LOUISE
Currently married	Indicator variable = 1 for being currently married in 2004 ; 0 otherwise.	LOUISE
Currently divorced	Indicator variable = 1 for being currently divorced in 2004 ; 0 otherwise.	LOUISE
Parental characteristics		
Education	Indicator variable = 1 for highest completed level of education; 0 otherwise (5 levels: compulsory school, high school ≤ 2 years, high school > 2 years, university ≤ 2 years, university > 2 years).	Employment register
Earnings	Labor related incomes (including self–employment) measured in hundreds of SEK.	Employment register

Table A.2. Summary statistics

Variable	Mean	Standard deviation
Teenage mother	.067	.250
Number of children	1.452	1.151
Years of schooling	12.649	1.937
High school	.931	.254
University	.423	.494
Non-employed	.176	.381
Log(earnings)	7.120	1.161
Log(Monthly wage)	9.951	.226
Welfare	.089	.285
Disposable income	7.084	.463
Currently married	.391	.488
Currently divorced	.070	.255
Exposed 1–24 months	.096	.294
Exposed 25–48 months	.089	.285
Exposed 49–72 months	.019	.135
Exposed > 72 months	.012	.109
Years of exposure	.650	1.483
Mother		
Compulsory school	.419	.493
High school ≤ 2 years	.344	.475
High school > 2 years	.052	.222
University ≤ 2 years	.090	.286
University > 2 years	.094	.292
Earnings	595.61	406.965
Father		
Compulsory school	.416	.493
High school ≤ 2 years	.249	.433
High school > 2 years	.153	.360
University ≤ 2 years	.068	.252
University > 2 years	.114	.318
Earnings	1079.96	746.11

Tablel 1. The structure of the subsidies

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

* Individuals are eligible until the calender year they turn this age.

Table 2. OLS estimates of the effect of the subsidies on fertility and marital status

	Dependent variable:						
	Number of children	Pr (Teen mother)	Pr (Currently married)	Pr (Currently divorced)	Pr (First child ≤ age 24)	Pr (First child ≤ age 25)	Pr (First child ≤ age 26)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<u>Panel A</u>							
Exposed 1–24 months	.015	.001	.002	–.001	–.001	–.002	–.001
	(.011)	(.002)	(.005)	(.002)	(.003)	(.003)	(.002)
Exposed 24–48 months	.016	–.004	.001	–.000	–.006	–.006	–.005
	(.019)	(.003)	(.006)	(.002)	(.004)	(.004)	(.003)
Exposed 48–72 months	–.002	–.007	.006	–.000	–.012	–.012	–.008
	(.030)	(.002)	(.015)	(.003)	(.005)	(.006)	(.006)
Exposed ≥ 72 months	.013	–.021	.011	–.002	–.027	–.026	–.021
	(.041)	(.003)	(.019)	(.003)	(.007)	(.008)	(.007)
F–statistic	1.40	35.66	.10	.45	4.08	2.58	2.05
[p–value]	[.265]	[.000]	[.980]	[.775]	[.012]	[.041]	[.120]
<u>Panel B</u>							
Years of exposure (linear)	.002	–.002	.001	–.000	–.002	–.002	–.001
	(.006)	(.001)	(.002)	(.002)	(.001)	(.001)	(.001)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	1.452	.067	.392	.070	.264	.321	.379
N	588,367	588,367	588,367	588,367	588,367	588,367	588,367

Notes: The sample consists of all women born 1965–l975 All regressions controls (linearly) for both parents' earnings and with dummies for both parents' education (five levels), missing information on education or earnings and for having no children. The outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust for serial correlation the county level are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure duration are jointly zero. See Table 2 for variable definitions.

Table 3. OLS estimates of the effect of the subsidies on socioeconomic outcomes

	Dependent variable:				
	Years of schooling	Pr (Non-employed)	Log (earnings)	Pr (Welfare)	Log (Disposable income)
	(1)	(2)	(3)	(4)	(5)
Panel A					
Exposed 1–24 months	.044	–.006	.012	–.011	.005
	(.028)	(.004)	(.008)	(.008)	(.005)
Exposed 25–48 months	.072	–.012	.027	–.015	.013
	(.044)	(.010)	(.011)	(.012)	(.008)
Exposed 49–72 months	.097	–.018	.042	–.018	.020
	(.042)	(.014)	(.019)	(.016)	(.013)
Exposed > 72 months	.073	–.026	.040	–.033	.025
	(.070)	(.025)	(.033)	(.025)	(.022)
F–statistic	7.39	0.63	2.84	2.47	0.87
[p–value]	[.000]	[.648]	[.048]	[.073]	[.500]
Panel B					
Years of exposure (linear)	.021	–.004	.006	–.004	.003
	(.011)	(.003)	(.003)	(.003)	(.002)
County fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes
County specific trends	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	12.648	.176	7.120	.089	7.084
N	587,503	588,367	517,733	584,890	585,744

Notes: The sample consists of all women born 1965–l975 All regressions controls (linearly) for both parents' earnings and with dummies for both parents' education (five levels), missing information on education or earnings and for having no children. All outcomes are observed in 2004 except for welfare which is measured at age 25. Parental characteristics are measured in 1985. Standard errors robust for serial correlation at the county level are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure are jointly zero. See Table 2 for variable definitions.

Table 4. Consequences for the estimates of removing covariates

Dependent variable:	Change in specification: Estimate as in Tables 1 and 2 (1)	Removing controls for regional specific trends (2)	+ Removing controls for parent's education (3)	+ Removing controls for parent's earnings (4)
Number of Children	.002	.003	.003	.003
	(.006)	(.003)	(.003)	(.003)
Pr(Teenage mother)	–.002	–.001	–.001	–.001
	(.001)	(.000)	(.000)	(.000)
Pr(Currently married)	.002	.001	.001	.001
	(.005)	(.002)	(.002)	(.002)
Pr(Currently divorced)	–.002	–.000	–.000	–.000
	(.001)	(.001)	(.001)	(.001)
Years of schooling	.021	.015	.016	.018
	(.011)	(.005)	(.006)	(.006)
Pr(Non-employed)	–.004	–.001	–.002	–.002
	(.003)	(.001)	(.002)	(.002)
Log(earnings)	.006	.003	.003	.004
	(.003)	(.002)	(.003)	(.003)
Pr(Welfare)	–.004	–.001	–..002	–..002
	(.003)	(.002)	(.002)	(.003)
Log(Disposable income)	.003	.002	.003	.003
	(.002)	(.001)	(.001)	(.001)

Notes: The table reports the coefficient on "Years of exposure" in separate regressions. The sample consists of women born 1965–l975. All outcomes are observed in 2004 except for welfare which is measured at age 25. Parental characteristics are measured in 1985. Standard errors robust for serial correlation at the county level are shown in parenthesis. See Table 2 for variable definitions.

Table 5. Falsification tests assigning treatment to the women's mother

Treatment:	Dependent variable: Pr (Teenage mother) (1)	Number of children (2)	Years of schooling (3)	Log (earnings) (4)
Exposed 1–24 months	.000	.008	.001	–.022
	(.003)	(.023)	(.024)	(.015)
Exposed 24–48 months	–.002	.016	.009	–.026
	(.005)	(.030)	(.033)	(.022)
Exposed 48–72 months	.001	–.007	-.022	–.017
	(.006)	(.046)	(.039)	(.033)
Exposed ≥ 72 months	–.020	–.014	.001	.007
	(.008)	(.076)	(.049)	(.053)
F–statistic	2.20	0.61	0.71	1.53
[p–value]	[.100]	[.659]	[.596]	[.226]
Years of exposure (linear)	–.001	.004	–.005	–.005
	(.001)	(.009)	(.008)	(.007)
County fixed effects	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes
County specific trends	Yes	Yes	Yes	Yes
Mean of dependent variable	.124	2.687	10.080	6.270
N	481,142	478,082	454,881	389,085

Notes: The sample consists of *mothers* to women born 1965–1975. Where appropriate, regressions controls (linearly) for earnings and with dummies for education (five levels), missing information on education or earnings, and regional trends. Standard errors robust for serial correlation at the county level are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure duration are jointly zero. See Table 2 for variable definitions.

Table 6. A closer look at socioeconomic outcomes

	Dependent variable:				
	Pr (High school graduate)	Pr (University graduate)	Pr (Above 1st quartile in welfare use)	Pr (Above 2nd quartile in welfare use)	Pr (Above 3rd quartile in welfare use)
	(1)	(2)	(4)	(5)	(6)
Panel A					
Exposed 1–24 months	.005	.010	–.009	–.006	–.005
	(.004)	(.005)	(.007)	(.006)	(.003)
Exposed 24–48 months	.003	.015	–.012	–.008	–.006
	(.005)	(.010)	(.011)	(.009)	(.006)
Exposed 48–72 months	.006	.022	–.012	–.003	–.003
	(.006)	(.010)	(.014)	(.012)	(.008)
Exposed ≥ 72 months	.008	.004	–.024	–.012	–.009
	(.011)	(.018)	(.022)	(.018)	(.012)
F–statistic	0.51	13.54	3.31	4.18	4.13
[p–value]	[.731]	[.000]	[.028]	[.011]	[.012]
County fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes
County specific trends	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.425	.931	.063	.037	.016
N	587,503	587,503	584,891	584,891	584,891

Notes: The sample consists of women born 1965–l975. In column (3) the sample is further restricted to women employed either in the municipality, county or private sector. All regressions controls (linearly) for both parents' earnings and with dummies for both parents' education (five levels), missing information on education or earnings, mother's age at birth, the subjects number of siblings, birth order and sector. The outcomes are observed in 2004. Parental characteristics are measured in 1985. Standard errors robust for serial correlation at the county level are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure duration are jointly zero. See Table 2 for variable definitions.

Table 7. Differential effects with respect to parental background

Dependent variable:	Change in sample:				
	Estimate as in Tables 1 and 2	Academic Family	Non-Academic Family	High-Income Family	Low-Income Family
	(1)	(2)	(3)	(4)	(5)
Number of Children	.002	.003	.001	.003	.004
	(.006)	(.005)	(.007)	(.005)	(.007)
Pr(Teenage mother)	–.002	–.002	–.003	–.001	–.003
	(.001)	(.001)	(.001)	(.001)	(.001)
Pr(Currently married)	.002	.002	–.000	.001	.003
	(.005)	(.002)	(.003)	(.002)	(.003)
Pr(Currently divorced)	–.002	.000	–.001	.001	–.002
	(.001)	(.001)	(.001)	(.001)	(.001)
Years of schooling	.021	.021	.026	.018	.021
	(.011)	(.015)	(.015)	(.015)	(.014)
Pr(Non-employed)	–.004	–.004	–.005	–.003	–.005
	(.003)	(.003)	(.004)	(.002)	(.004)
Log(earnings)	.006	.011	.010	.011	.005
	(.003)	(.003)	(.003)	(.003)	(.006)
Pr(Welfare)	–.004	–.003	–.006	–.002	–.009
	(.003)	(.003)	(.005)	(.002)	(.006)
Log(Disposable income)	.003	.004	.007	.003	.005
	(.002)	(.002)	(.003)	(.002)	(.004)

Notes: The table reports the coefficient on “Years of exposure”. The sample consists of women born 1965–l975. Where appropriate regressions controls (linearly) for each parent’s earnings and with dummies for each parent’s education (five levels), missing information on education or earnings and for the subject having no children. All outcomes are observed in 2004 except for welfare which is measured at age 25. Parental characteristics are measured in 1985. Standard errors robust for serial correlation at the county level are shown in parenthesis. The omitted category in Panel A is women with no exposure to the subsidy. Reported F-statistic tests the null hypothesis that the coefficients on exposure duration are jointly zero. “Academic family” is defined as having at least one parent who has completed at least theoretical/preparatory high school. “High income family” is defined as having at least one parent above the median in each parent’s earnings distribution. See Table 2 for variable definitions.