'The Miracle Tablet *Maybe*'

Legalization of the Pill and Women's Childbearing and Career Decisions[*]

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Abstract: A substantial literature argues that the legalization of the birth control pill in the United States reduced fertility and enhanced career investments. This study questions whether the broader effects hold across contexts. We use administrative data to study the Norwegian legalization of the pill and find effects on teenage motherhood but not on subsequent career outcomes. Using survey data, we show that the pill's impact on fertility is largest for women with an early sexual debut; nevertheless, broader effects of pill usage are still absent. Our study indicates that the pill cannot explain the rise of Norwegian women's economic empowerment.

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Even before the birth of the pill, Margaret Sanger envisioned it as "a biological method of conception control, the miracle tablet maybe" (Sanger, 1955). The pill's release in the United States in 1960, and the legalization of access to it in the years that followed, have been found to give young women a greater opportunity to control pregnancy than previous generations (Bailey, 2018, 2006).[1] From the pill's early years, its impact was predicted to reach well beyond fertility decisions and include family formation, enrollment in higher education, and professional careers (Bailey, 2018, 2006, 2012; Eig, 2014; Goldin & Katz, 2000, 2002).

Early research on the consequences of birth control argues, however, that the introduction of the pill did not matter much for fertility and labor market outcomes. For instance, while Becker (1960) notes that birth control will reduce the quantity of children, he later changed his mind and concludes that it plays a minor role in explaining changes in fertility compared to forces determining the demand for children (Becker, 1991, p. 141).[2] The massive decline in fertility in Japan without the pill, which was introduced as late as 1999, as well as the absence of a cross-national relationship between pill introduction and fertility, is used in support of this view (Leridon, 2006).

Careful empirical research has recently challenged the claim that the pill is inconsequential for women's fertility decisions. By affecting the timing of births, as well as making it possible for women not to have children, a series of seminal studies have argued that the arrival of the pill should lead to a shift in both the timing and the number of children. Moreover, with greater power over when and whether to become pregnant, women with access to the pill could time their births to increase their participation in education and in the labor market. The pill is thus predicted to affect women's economic empowerment, defined as the access to resources because of employment, through its impact on women's fertility decisions. By exploiting United States state-level variation in the timing of legal access to the pill, Goldin & Katz (2000, 2002) and Bailey (2006, 2012) find that the pill decreased births given by teen mothers, increased mothers' average age at first birth, delayed the age of marriage, and subsequently increased years of education and increased mothers' labor supply (see

[1] In the US, access to the pill was initially restricted to married women, before it became available to unmarried young women in the late 1960s and early 1970s (Bailey, 2006; Edlund & Machado, 2015).
[2] In particular, he points to changes in the earnings power of women as more important (see also Greenwood, Seshadri & Vandenbroucke, 2005).

also Bailey, 2018, 2012; Bailey, Guldi & Hershbein, 2013; Christensen, 2012; Edlund & Machado, 2015). The role of the pill is not entirely clear, however, as Myers (2017) argue that abortion policies confound these estimates.

Nevertheless, beyond studies of cross-national variation in fertility, we know less about the effects of the first access to the pill outside the US.[3] Exploring the impact of the pill across different cultures and institutional contexts is key to understand when and where we should expect its effect on women's family, education, and employment decisions. In particular, we challenge that de jure access to the pill more or less automatically translate into de facto high take up rates, as the studies of the United States generally assumes. Instead, we need to understand the rates of pill takeup, who makes use of the pill, and in what societal context. For instance, differences in sexual activity, information about the pill, and norms and attitudes toward usage of the pill are all likely to have consequences for the usage and fertility effects of the pill. The impact of the legalization of the pill as contraception will consequently depend on this context, and the effect of actual use of the pill might be different from the effect of access. To understand how legal changes affecting women's economic empowerment may play out in subtle and differential ways is a central and reoccurring theme of this special issue (see Filgueira & Franzoni, 2018; Htun, Jensenius & Nunez, 2018; Tønnesen, 2018; Toyofuku, 2018).

Our study makes three contributions to the literature. First, we study the effects of two key pill reforms in Norway. Together with the other Scandinavian countries, Norway was a gender equality pioneer, with women increasingly entering into education, professional life, and politics from the 1960s and onwards. If the pill affects women's economic empowerment, such effects should be particularly likely here. Norway legalized the contraceptive use of the pill for women age 18 or above in 1967 and for women aged 16-18 in 1972. In difference from the US, where married women got access years before unmarried ones, the Norwegian reforms allow us to examine the effects of more comprehensive pill reforms.

Second, by access to rich, *population-wide* administrative data, we are able to describe the development of fertility patterns, as well as later education, employment, and

[3]See Gronqvist (2012), Madestam & Simeonova (2012), and Ragan (2017a,b) for unpublished papers studying the case of Sweden. We discuss these studies in the next section.

marriage decisions for whole cohorts who were young at the time of the legal reforms. The register data makes it possible to study a richer set of outcomes than in much of the previous literature. In addition, to assess actual the take-up of the pill, and not just the right to use the contraception, we examine survey data with detailed histories of the respondents' usage of the pill.

Third, we couple the administrative data with historical data on the varying distance to pharmacies, to approach the causal effect of the impact of the pill reforms on fertility, education, and labor market outcomes. For women with a long driving distance to the nearest pharmacy, the pill was much harder to obtain. Using a difference-in-difference design, we show that the pill did reduce the probability of teenage birth, which is in line with much of the United States literature. In contrast to most of the United States research, we find, however, limited evidence of downstream effects on marriage, education, and labour market outcomes. These findings are echoed in the survey data, where we use matching models to correlate pill usage with fertility, education, and labor market outcomes. The survey data also indicate that the absence of strong effects of the reforms are likely to be due to low take-up rates (particularly for the first cohorts that had access), with take-up particularly varying according to women's previous sexual activity, norms, and urban/rural upbringing. As such, our results complement other findings in this special issue and highlight the vital distinction between legal and ensuing behavioral changes (Htun, Jensenius & Nunez, 2018).

1 The pill, birth timing, education, and jobs

The literature on the effects of the pill argues that the advance in contraceptive technology allows women to decide the *timing* and *number* of children, which again increases women's possibilities to invest in education and participate in the labor market (see, notably, Bailey, 2018, 2006; Chiappori & Oreffice, 2008; Goldin & Katz, 2000, 2002; Steingrimsdottir, 2016). There are thus two steps in the argument: first that the pill affects fertility decisions and second that changes in fertility (and marriage) enables women to invest more in their career. Regarding the first step, Bailey (2006, p. 295) argues that the pill was the first highly effective contraceptive controlled by the woman herself, as the intrauterine device is less effective in this regard. As a woman's decision

to use the pill does not hinge upon men's approval to use contraception, women can independently decide over whether and when to get pregnant. Next, with the help of the pill, women can efficiently and independently plan their fertility in a way that allowed them to simultaneously maximize their professional career (see also Goldin & Katz, 2000, 2002). This effect is potentially powerful, as much research finds that motherhood is associated with lower relative earnings (see, e.g., Cools, Markussen & Strøm, 2017 and Kleven, Landais & Søgaard, 2018 for analyses using Norwegian and Danish data, respectively).

To test this line of reasoning, a series of seminal papers use quasi-experiments and variation in pill legalization across the United States and across time to identify the effect of legalization or availability of the pill on fertility and marriage (Bailey, 2018; Christensen, 2012; Edlund & Machado, 2015; Goldin & Katz, 2000, 2002), education (Ananat & Hungerman, 2012; Edlund & Machado, 2015; Steingrimsdottir, 2016), and labor market outcomes (Ananat & Hungerman, 2012; Bailey, Hershbein & Miller, 2012; Edlund & Machado, 2015). These papers, in other words, identify important effects of the pill.

We highlight three key challenges to the theoretical pill argument and the associated empirical findings. First, recent evidence suggest that, in the US, the effect of the pill is confounded with the effect of legalizing abortion. In an important study, Myers (2017) find that after controlling for changes in states' abortion legislation, there is little effect of early legal access laws on age at first birth and family formation.[4] As such, the empirical evidence from the United States is more mixed than at first glance.[5] However, recent evidence from Sweden, where abortion was legalized earlier than in the US, does indicate that the introduction of the pill affected timing and the number of births (Madestam & Simeonova, 2012; Ragan, 2017b). Clearly, more evidence is needed to verify the first step of the argument.

Second, even if we were to believe that the effects of the pill on the timing and number of births are real and important, it still leaves open the question whether the changes in fertility subsequently affects women's education and labor market

[4]Also evidence from Norway indicates that abortion access delayed and decreased fertility (Mølland, 2016).
[5]Studies also fail to find a cross-country correlation in the timing of fertility declines and the introduction of the pill (Leridon, 2006).

investment. Speaking to this question, Angrist & Evans (1998)—who use the sex composition of siblings as an instrument for whether parents have a third child—document that larger family size does decrease mothers' (but not fathers') labor supply. The effect, however, is small in size. A recent study by Cools, Markussen & Strøm (2017) employs the same research design as Angrist and Evans but with large-scale Norwegian administrative data on women's employment trajectories from childbirth to retirement. They find that, for women without a college degree, the short-term reductions in labor supply following childbirth in fact turn positive as the children grow up. For women with a college degree, on the other hand, there are lasting career penalties of motherhood. As such, the existing literature suggests that the second step in the argument is less straightforward than what is often portrayed in the power-of-the-pill literature.

To be sure, as these studies of the impacts of children on women's labor market careers only look at the effect of an *additional* child, they cannot rule out that the pill substantially affected women's education and labor supply through women choosing to stay childless or through changes in the timing of children (Bailey, 2006, p. 296). In line with the family size studies in the previous paragraph, however, Gronqvist (2012) shows that subsidization of oral contraceptions in 1989 in Sweden increased pill use and decreased teenage childbearing, but did not have any effect on labor market participation, marriage patterns, or educational attainment. There is thus consequently considerable disagreement in the literature regarding the second step—a disagreement that can only be resolved with more evidence from diverse contexts.

Finally, the existing studies, have little to say about the characteristics of women who, given availability of the pill, make use of it. By comparing women with and without legal access to the pill, we get the total effect of legalization of the pill. This effect can, however, be very different from the effect of pill use if pill users are few and not representative of the population at large. For instance, if it is mainly women who already are mothers who use the pill, the effects on both fertility and labor market outcomes might be very different from a situation where it is predominantly young women without children who are on the pill. In consequence, studies of the effects of legalization should be accompanied by studies of actual up-take of the pill.

In this study, we aim to contribute to addressing these three challenges. To this

aim, we use detailed administrative data on the whole female Norwegian population and survey data with a detailed record of the respondents' pill-use histories. Furthermore, to investigate both steps of the causal chain, we study three sets of outcomes. Concerning the first step, we examine the effect of the pill on *fertility and marriage* decisions, including teenage births, age at first and second birth, the number of children, childlessness, and the likelihood of ever being married and divorced. With regard to the second step, we study women's *education* and *labor market* investment. We look at years of education and college degrees, to capture key educational investment, and women's labor supply and wages at age 50, to assess the impact of the pill on long-term labor market attachment. Together, these outcomes will allow us to investigate both steps in the causal chain linking the introduction of the pill to women's economic empowerment.

2 The pill in Norway

In Norway, the pill became available in 1963 but it was only from 1967 that it became legal for doctors to prescribe it as a contraceptive to all women aged eighteen and above (Noack & Østby, 1981).[6] Before 1967, some Norwegian women did get access to the pill as treatment for menstrual regulation, but this type of prescription is not well documented in historical sources.

The Norwegian legalization was late compared to the other Scandinavian countries and Germany. In Sweden, for instance, the Swedish Board of Health and Welfare, approved the contraceptive use of the pill in 1963 (Ragan, 2017b, p. 7). The late introduction might be because Karl Evang, the powerful director of the Directorate of Health, was for a long time a skeptic to legalization of the pill, because the research on the potential risks of pill use was thin. According to Moen (2017), the debate and introduction of the pill in Norway was shaped by legal actions against producers of the pill (the producers won the court cases), which might have slowed down the use of the pill. Since 2002, the pill has been free of charge for all sixteen to twenty year old women, and it is possible to get a prescription from the school nurse.

[6] Access to the pill was never restricted to married women only.

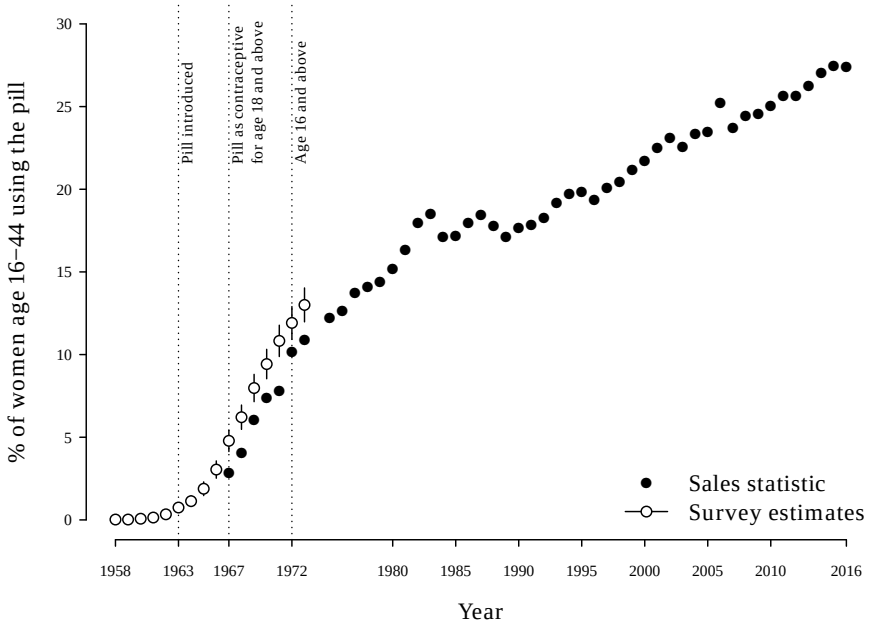


Figure 1: The development in usage of the birth control pill in Norway, 1958-2016.

Figure 1 shows the percentage of women age 16 to 44 using the pill in a given year, with data from aggregate annual sales registers and survey data from the 1977 Norwegian Fertility Survey. As evident, consumption of the pill was minimal before 1967 but also in the years after its legalization less than ten percent of women in the age group made use of the pill. The dotted vertical lines give the years of the key pill reforms. With the end of the contraindication of the pill for women under the age of eighteen in 1972, there was a hike in pill utilization and by the early 1980s about fifteen percent were on the pill. In comparison, ten percent of Swedish women age 15 to 44 used the pill already in 1965 and a decade later the number had surged to 30 percent (Ragan, 2017b, p. 7). Pill use in Norway thus considerably lagged that of Sweden.

Using the survey data we can also examine the development in women's pill usage as teenagers across cohorts. Figure 2 shows, by cohort, the share of respondents reporting that they used the pill at least one of the years when they were between sixteen and nineteen years old. Basically no one reports using the pill in the pre-1947 cohorts. Pill usage starts with the 1947 cohort, and from the 1950 cohort and onwards the share using the pill increases rapidly, before it stabilizes at a level of around 25 percent for the 1955 cohort. Thus, although the legal change had a substantial effect on use, it was still a minority of the early cohorts that was on the pill. One reason for this might be the controversy around the legalization, the uncertainty about the

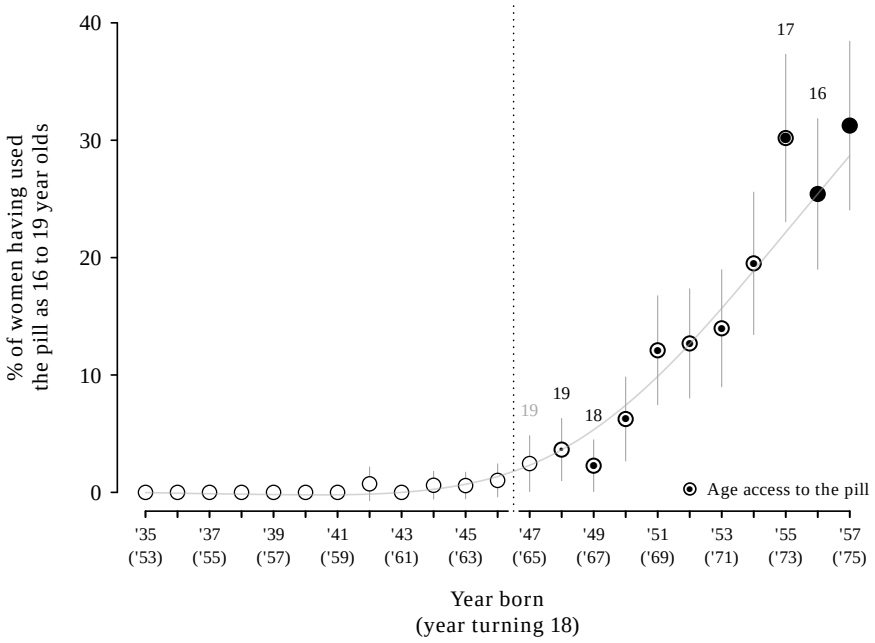


Figure 2: The percentage of each cohort that has used the pill while between 18 and 22 years old.

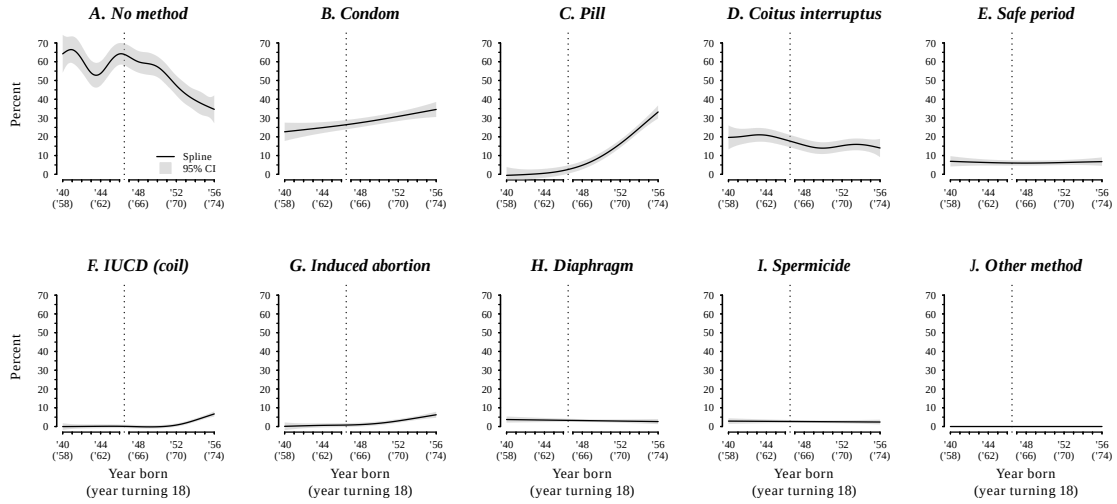


Figure 3: Birth control usage and abortion as a teenager (as a percentage of women with a sexual debut age below 20).

side effects, and the legal action against the producers. In any case, the total effect of the legal change will of course be limited when use is limited. Any effects of the pill on fertility, education, or labor market outcomes should thus be more visible for the cohorts born from the mid 1950s and onwards.

Finally, we examine the usage of the pill as a method of birth control versus other methods that existed in the 1960s and the 1970s, as well as versus induced abortion. Figure 3 plots the share of women (with a sexual debut age below 20) who made use

of a given method as a teenager.[7] Before the introduction of the pill (marked by the dotted, vertical line), sexually active women either did not use any form of contraceptive method (panel A in the figure), had a sexual partner who used the condom (panel B) or practiced coitus interruptus (withdrawal) (panel D).[8] Some also used safe periods (panel E). Other methods—including the IUCD (panel F), the diaphragm (panel H), and spermicide (panel I)—were uncommon. The decrease in the share of women who did not use any method coincides with the arrival of the pill. For the cohorts from 1947 and onward, the pill constitutes the major *shift* in the type of birth control used. The IUCD, which was legalized at the same time as the pill, also increased in usage for these cohorts, though it remained an infrequent form of contraception. The condom was on the other hand widely used throughout the period, yet the *change* in usage was slow and linear. Induced abortion was first made available through medical committees in 1964 (and self-determined abortion in 1978); still, in comparison with birth control methods such as the pill and the condom, the rates remained quite low. For the cohorts examined in this study, the clear shift in birth control usage was therefore the arrival of the pill.

3 The pill and trends in fertility, marriage, and the labor market

3.1 Making use of population-wide administrative data

We rely on population wide public registers made available for research by Statistics Norway. The registers include unique identifiers, which allow us to merge registers with individual level information on for instance demographic characteristics, fertility and marriage decisions, educational outcomes and labour market outcomes. To assess trends in our outcomes both prior to and after the reform, we study women born between 1935 and 1959, i.e., who were eighteen years old between 1953 and 1977.

We study trends in fertility, marriage, education, and the labor market using the

[7] As the respondents could list several methods, the categories are non-exclusionary, with the exception of "no method".

[8] For women with a sexual debut age below 20 in the 1942 to 1947 cohorts, the median age when first giving birth was 22 and 33 percent had a child before turning 21.

register data. Existing theories posit that the effect of the pill on women's educational and labor market participation runs through its impact on fertility and marriage choices, and in particular teenage motherhood and age of first birth (Bailey, 2018, 2006; Goldin & Katz, 2000, 2002; Michael & Willis, 1976). The operationalization of each of the variables within the different categories of outcomes is described in detail in Section A.1 of the Online Appendix.

In the Online Appendix section A.2 we study descriptive trends in these variables across cohorts. This analysis indicates a shift in fertility that might be ascribed to the pill, while there is less evidence for effects beyond the pill. Obviously, confounding trends is a concern when looking at descriptive trends. Therefore, in the next section we use the distance to pharmacies to attempt to provide estimates of the effect of the pill.

3.2 Estimating the effect of the pill reforms: distance to pharmacies

Although eighteen and nineteen year olds had legal access to the pill from 1967 and onwards, it did not mean that it was easily available to them. Teenagers first had to get a prescription from a physician and then go to the pharmacy to buy the pill. Whereas 83 percent of municipalities had a doctor in 1972, pharmacies were fewer and farther between. For instance, only 157 out of the 444 municipalities in 1972 had a pharmacy. To move beyond the descriptive graphs, we use the variation in the geographical distribution of pharmacies to distinguish between women who could and could not easily get hold of the pill after it was legalized. We then use a difference-in-differences (DiD) research design: We compare the differences in fertility between those who resided close and far away from pharmacies before and after the legal change. We reason that it is the women residing close to a pharmacy that the legalization of the pill should lead to *de facto* access to the pill. Given no confounding trends, our DiD design gives us an estimate of the causal effect of easy access to the pill on fertility (Angrist & Pischke, 2009).

To calculate the distance to the nearest pharmacy we combine detailed information on the location of all pharmacies in Norway in 1967 and 1972 and data on travel times. Using these data sources we to proxy the driving time to the nearest pharmacy for each woman in our data. We describe our approach in detail in the Online Appendix.

We find that the median distance is about 22 kilometers. Given that the legal driving age was 18 and that the number of cars per capita was still quite low as a result of rationing until 1960, female teenagers living in a municipality with median or longer driving times to the nearest pharmacy in practice had limited access to the pill also after the reforms. Consequently, we use those with above median distance to the nearest pharmacy as a comparison group.

In our DiD setup, D is a treatment variable that is equal to one for all individuals who in 1967 resided in a municipality, m, that had less than the median driving time to the nearest pharmacy. Otherwise it is equal to zero. We create a separate treatment indicator variable for each cohort in our sample, D_c, $c = \{1936, 1938, \ldots, 1959\}$. For instance, D_{1947}, is equal to one an individual both lived in a municipality with less than median time to the nearest pharmacy *and* is born in 1947. Because only cohorts born in 1947 or later had legal access to the pill as teenagers, the treatment variables for cohorts 1936 through 1946 are placebo treatments. We estimate the following model:

$$Y_{imc} = \eta_m + \delta_c + \sum_{c=1947}^{1959} \alpha_c D_{cm} + \varepsilon_{imc} \tag{1}$$

We thus include one treatment indicator for each of the treated cohorts ($c \geq 1947$). η_m is the municipality fixed effects and δ_t is the cohort fixed effects. The treatment coefficients α_c thus captures the differences in (i) the outcome between cohort c and the pre-treatment cohorts for close-to-pharmacy municipalities and (ii) the same difference in not-close-to-pharmacy municipalities. As we, unfortunately, do not observe which individuals in the administrative data that used the pill or not, our results will be intention-to-treat (ITT) estimates. The estimates give us a lower bound on the effect since the control group was not completely excluded from access to the pill. We estimate the model with ordinary least squares.

Our DiD specification rests on the assumption of parallel trends between the treatment and control group absent of the reform. As we do not observe what happened absent of the reform, this assumption is not directly testable. However, we can lend credence to the assumption if we can show that the trends in the treatment and control group prior to the pill reform in 1967 did not differ systematically from one another. We assess the plausibility of this assumption by examining whether treatment indicator

variables for the pre-treatment cohorts are jointly equal to zero (for details, see Borusyak & Jaravel, 2017):

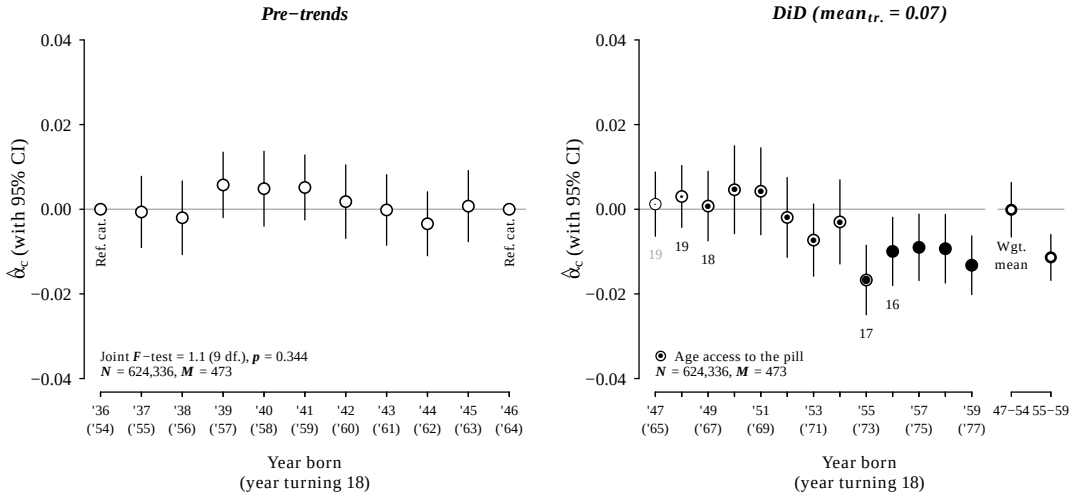
$$Y_{imc} = \eta_m + \delta_c + \sum_{c=1937}^{1945} \gamma_c D_{cm} + \sum_{c=1947}^{1959} \alpha_c D_{cm} + \varepsilon_{mci} \tag{2}$$

Using an F-test we test whether the γ_c coefficients are jointly equal to zero. Note that since our sample includes the 1936 to 1959 cohorts, the 1936 and 1946 are our chosen omitted categories. The F test is valid in this case and will detect non-linear differences in time trends between the treated and untreated units (Borusyak & Jaravel, 2017).

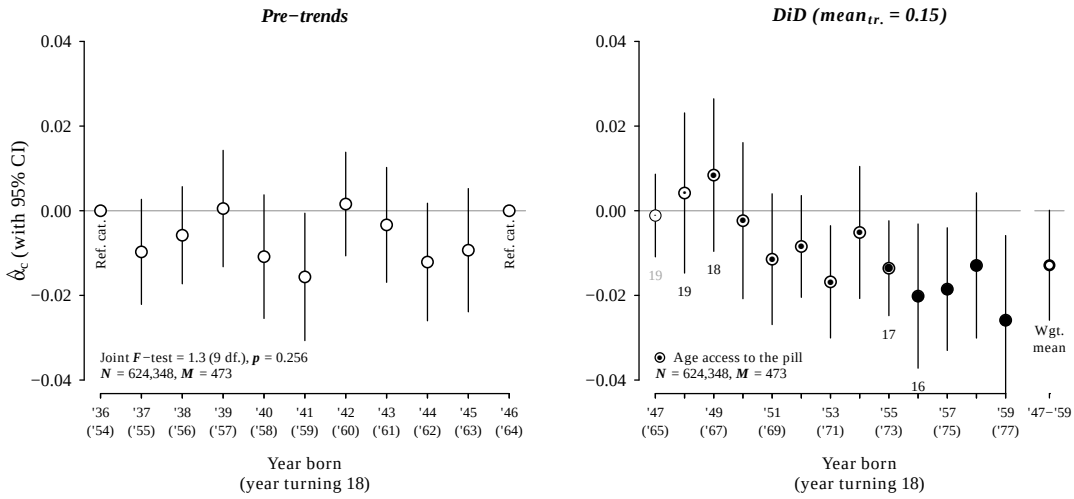
In Figure 4 the panels on the left display the pre-treatment γ_c coefficients for the two key fertility outcomes, namely the probability of becoming a mother before age 19 and during ages 19 to 20. Each of the black dots represents the estimated γ coefficient for that cohort, relative to the reference categories 1936 and 1946. The plots also provide the joint F-test of the null hypothesis that the coefficients are jointly equal to zero. As both a visual inspection of the coefficients and the joint F-test testify, there is little evidence of diverging pre-treatment trends between the treated and untreated municipalities. This lends credence to the parallel trends assumption.

Next, the panels on the right side in Figure 4 plot the estimated effect of having legal access to the pill *and* residing in a municipality with less than median distance to a pharmacy, for each of the treated cohorts (see Equation 1). Figure 4a displays the probability of becoming a mother before age 19 and Figure 4b the probability of becoming a mother at age 19 or 20. Each point in the figure is the treatment effect for each cohort, that is, the DiD estimate of having the pill available. In addition, each of the panels plots a coefficient giving the weighted mean of the cohort-specific treatment effects. The weights are based on the share of the ages 16 to 19 that a cohort had access to the pill (see the note to Figure 4 for further details).

Figure 4 conveys several insights. First, the bottom-right panel shows that whereas there is no treatment effect for the first cohorts exposed to the pill, there is a decreasing chance of becoming mother at age 19 or 20 as we move toward the latest cohorts in our sample. For the 1953 cohort, for instance, our estimates indicate that individuals in the treated municipalities are expected to have a 1.7 percentage points lower chance of becoming a mother as a 19 or 20 year old. Moreover, the weighted mean of the



(a) Mother before age 19.



(b) Mother at age 19 or 20.

Figure 4: Pre trends and treatment effects for the cohorts with legal access to the pill. Differences in the estimated probability of becoming a mother before turning 19 years old (panel A) and as a 19 or 20 year old (panel B). *Note:* The *left* panels plot the estimates of γ_c in Equation 2; the *right* panels plot the estimates of α_c in Equation 1. The weighted-mean coefficient is calculated as a weighted sum of the coefficients on the 1947 to 1959 cohorts: $\sum_{c=1947}^{1959} \omega_c \hat{\alpha}_c$, where ω_c are the weights and $\sum \omega_c = 1$. The coefficients are weighted according to the share of the teenage years 16 to 19 the cohort had legal access to the pill. The mean$_{tr.}$ of the dependent variable denotes the mean for the treated individuals in cohorts 1947 to 1959.

cohort-specific treatment coefficients points to a 1.3 percentage points decrease in motherhood at age 19 or 20. With 14.9 percent of the treated women becoming mothers at this age, this is about an eight percent decrease from the counterfactual share of women becoming a mother at age 19 or 20.[9]

Second, in the top-right panel, which examines the effect of becoming a mother *before* turning 19, there is no clear treatment effect for the 1947 to 1954 cohorts. These cohorts did not have access to the pill before they were 18, and there should thus be no treatment effect for these cohorts. Instead, we see that there is a pronounced downward shift in the treatment effect starting with the 1955 cohort and continuing with the subsequent cohorts. These are precisely the cohorts for whom we should see an effect: the 1955 cohort was the first cohort to gain access to the pill before they turned 18. If it is access to the pill that is driving our results, and not other reforms such as particularly access to abortion, then this is the timing of the effects we should see, and it echoes the overall trends in teenage fertility portrayed in Figure A.1a in the Online Appendix. Moreover, whereas the weighted mean of the treatment coefficients for the 1947 to 1954 cohorts is zero, the weighted mean for the 1955 to 1959 cohorts gives an expected reduction in motherhood before age 19 of 1.1 percentage points—that is, a fourteen percent decrease from the counterfactual fertility level. Thus, although the pill reforms did not, as in Sweden (and the US), lead to a massive change in fertility among the first cohorts who had access to the contraceptive pill, they nonetheless had substantive impacts on teenage fertility.

There is a possibility that our results are not driven by the changes in access to the pill but rather by liberalized abortion practices, as Myers (2017) document for the US.[10] Moreover, Mølland's (2016) shows that teenagers in the Oslo and Akershus region had better access to abortion between 1969 and 1972 compared to the rest of Norway. Our results may consequently be driven by abortion access in the Oslo and Akershus region. Additionally, after 1972, teenagers in the rest of Norway gained increased access to abortion, which implies that the also results for the later cohorts in our analysis could be driven by abortion access. To attempt to address these concerns, we re-estimate the

[9] We calculate the percent change from the counterfactual as: $\hat{\alpha}_{wgt.}/(mean_{tr.} - \hat{\alpha}_{wgt.}) = -0.013/(0.149 - [-0.013]) = -0.08$, where $\alpha_{wgt.}$ is the weighted mean coefficient.

[10] As condom usage is only slowly and linearly increasing across pre- and post-treatment cohorts, it is unlikely to drive our results. The same holds for IUCD, which is infrequently used (see Figure 3).

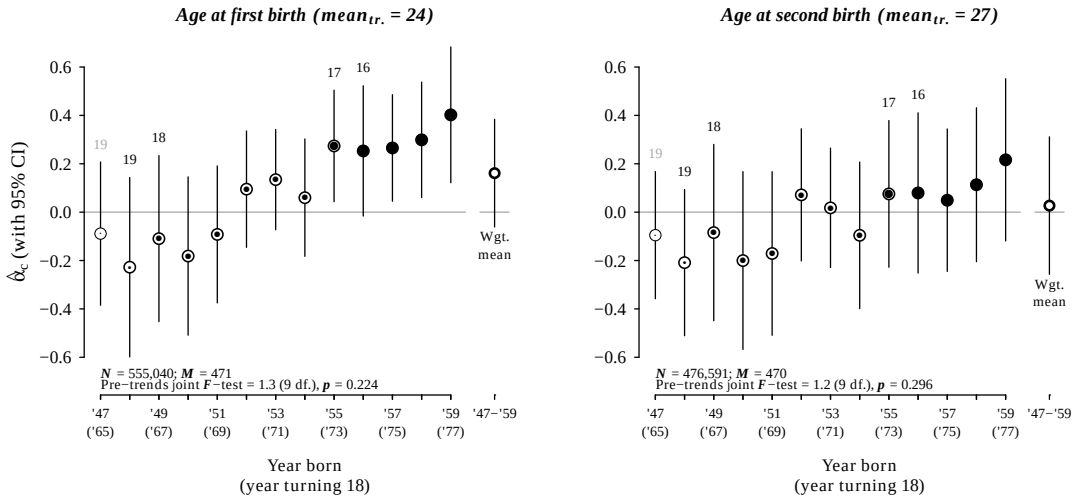


Figure 5: Treatment effect for the cohorts with legal access to the pill. Changes in the estimated probability of becoming a mother before turning 19 years old (left panel) and as a 19 or 20 year old (right panel).

results in Figure 4 in two ways. First, we remove all women who resided in Oslo or Akershus from the analysis; second, we include a time-varying, county-level measure of abortion rates among women age 15 to 49 as a covariate in the regressions. As Figures A.8 and A.9 in the Online Appendix show, these results are very similar to those displayed in Figure 4 and at least reduces the worry that our findings are driven by changes in access to abortion.[11]

Given the results on teenage fertility, we move on to assess the impact of the reform on our other fertility outcomes, as well as on education and labor market participation. Figure 5 displays the results of estimating the DiD model with mother's age at first and second birth as the outcome variables. To estimate these models we exclude all women who are childless. Unsurprisingly, given the effect of the reforms on teenage motherhood, there is an indication that the introduction of the pill on average slightly increased the mothers' age at first birth. However, the average age for the second birth did not budge as a result of the reform. In addition, Figure A.10 in the Online Appendix displays that the reform had statistically significant, though substantively negligible, positive effect on the number of children born to each mother. Moreover, it had no effect on childlessness (see Figure A.10). Thus, the pill reforms reduced teenage

[11] In Online Appendix Section A.7, we re-estimate Equation 1 with the continuous minutes-to-nearest-pharmacy variable and its square. Figure A.14 shows that the conclusions withstand this alternative specification.

childbirths without affecting overall fertility.

Since we find effects on teenage childbirths, it is possible that we find effects on the downstream variables as well, as having a child in young age might affect subsequent education, family and labor market decision. Beyond fertility, however, we fail to find significant effects in our DiD setup. In the Online Appendix, a series of DiD plots similar to Figures 4 and 5 display that there is no discernible impact of the reforms on the probability ever having been married or divorced (Figure A.11), on years of education or the probability of obtaining a higher education degree (Figure A.12), or on annual wages at age 50 or the probability of being employed at age 50 (Figure A.13). These results are strikingly different from the results in previous research from other countries (see, e.g., Bailey, 2006; Bailey, Hershbein & Miller, 2012; Ragan, 2017b). One potential reason that results are different is the generous family policies in Norway. For instance, subsidized public child care was rapidly expanded during the 1970s. Access to cheap and reliable child care eases the negative effects of unplanned childbirth and might explain the weak downstream effects (see also Gronqvist 2009).

3.3 Using survey data to study variation in take-up of the pill and its impact

Even though the administrative data is unrivaled in their quality and sample size, they do not contain information on pill take-up, which prevents us both from understanding who made use of the pill and from investigating how actual pill usage, and not just exposure to the reform, affects the outcomes. To address these shortcomings and to further understand the relatively small impact of the legalization of the pill even on teenage fertility, we therefore instead rely on survey data.[12] We use the 1977 Fertility Survey (SSB 1981), which was the first fertility survey that covered the whole of Norway. The main benefit of the survey is that it includes information on the respondents' yearly, historical use of the pill. Unfortunately, however, the survey does not include information about the respondents' municipality of upbringing or current residence, which means that we cannot use the DiD strategy utilized in the previous section to identify causal effects of the pill. Thus, whereas the benefit of the survey

[12]We assume that people answer sincerely, yet, as for all survey data, measurement error might be an issue.

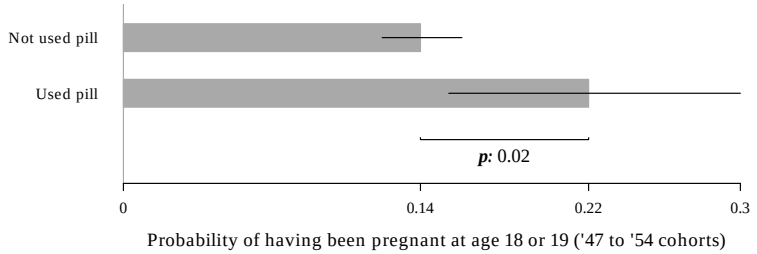


Figure 6: The *positive* correlation between pill usage and pregnancy during ages 18 and 19.

data is that we can directly examine the both the factors that are likely to lead to pill usage and how pill usage again affects fertility, education and labor market outcomes, we should refrain from giving these correlations a causal interpretation.

We first examine the raw correlation between having *used* the pill at age 18 to 19 and the probability of becoming pregnant at age 18 or 19 for the 1947 to 1954 cohorts. The 1947 cohort is the first that had access to the pill as a contraceptive and the 1954 cohort is the last cohort that was not affected by the 1972 reform. The result is displayed in Figure 6, where the two grey bars provide the predicted probability of being pregnant at age 18 or 19 among those who reported not having used and having used the pill, respectively. What is striking is that there is a *positive association* between pill usage and pregnancy: the respondents who report having used the pill are predicted to be eight percent more likely to also have been pregnant as a eighteen or nineteen year old. Of course, this does not mean that the pill increases teenage fertility; instead, it indicates considerable selection effects into pill usage. For instance, if those who are sexually active early on are both more likely to use the pill and to have children as teenagers, then there will be a positive correlation between pill usage and teenage childbearing. Correspondingly, if women who had a child during their teenage years are likely to *subsequently* use the pill, then this will also produce a positive correlation between pill usage and teenage fertility.

To investigate such selection effects, we can look at the background factors that are correlated with pill usage. Figure 7 displays the results from a set of linear probability models with having ever used the pill as a teenager as the binary dependent variable. The independent variables capture geographical variation, socioeconomic background, and sexual and religious norms that may affect pill usage (and also fertility choices).

The most striking pattern that emerges from Figure 7 is that although most of

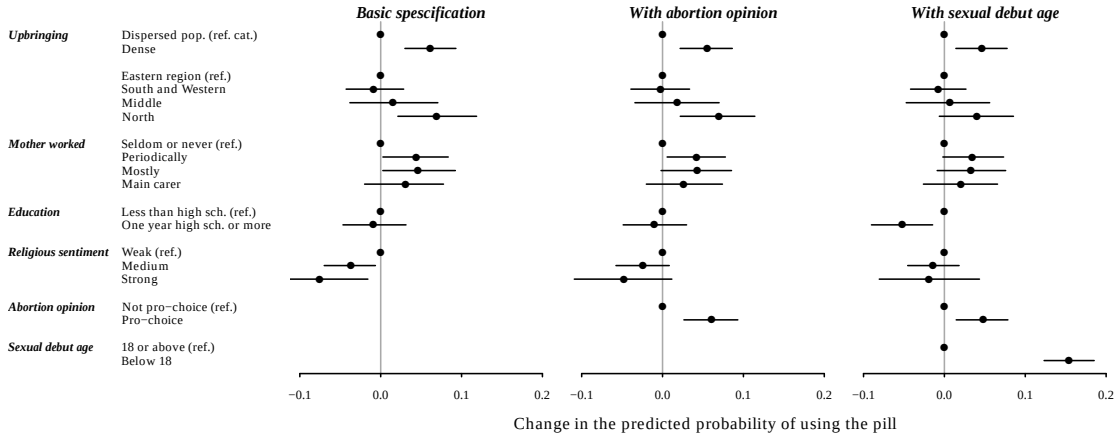


Figure 7: Differences in the predicted probability of having ever used the pill as a teenager (with 95 percent confidence intervals).

the variables are significantly correlated with pill usage, the by far most important predictor is sexual debut age. Women who report making their sexual debut before age 18 are about fifteen percentage points more likely use the pill than those who with a sexual debut age at age 18 or above. Thus, if we want to gauge the effect of pill usage on fertility, education, and labor market participation, we at least need to control for the factors in Figure 7. Given that the availability of the pill matters particularly for individuals who are sexually active at an early age, the effect of the pill should differ by sexual debut age.

In addition, whereas most of the existing literature assumes that the pill is being used prior to becoming a mother, Figure 8 portrays a somewhat different picture. In the figure, each white bar gives the share of a cohort who has used the pill as a teenager, whereas the grey bars give the share of those who have used the pill who are *already a mother*. As is evident from the figure, a non-negligible share of the women on the pill are using it for the first time *after* they became a mother. These individuals are another reason for the positive association between pill usage and teen pregnancy in Figure 6 above. If we want to gauge the impact of the pill on teenage pregnancy and avoid reverse causality, we hence need to omit women who first used the pill after becoming a mother.

To investigate the impact of the pill on teenage pregnancy, and how this impact varies with sexual debut age, we use a matching strategy. Obviously, those using the pill might have different outcomes for other reasons than using the pill. We therefore adjust the difference between pill users and not pill users by reweighting the non-pill

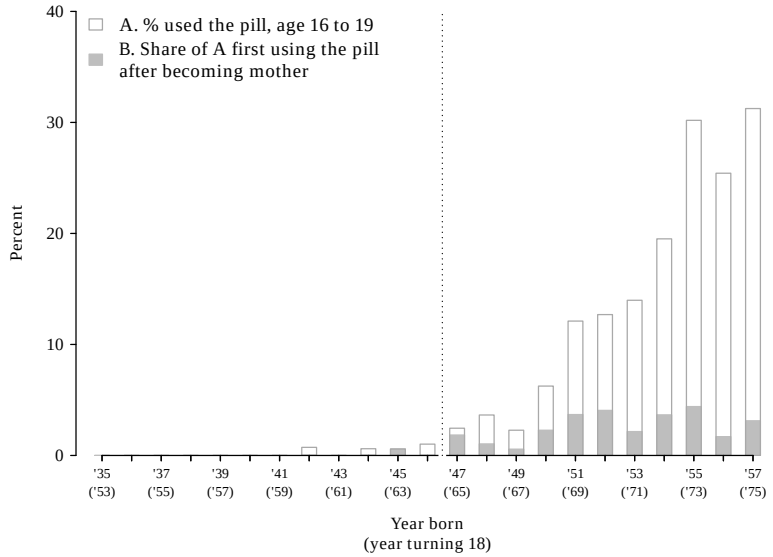


Figure 8: The percentage of a cohort who have used the pill during age 16 to 19 and the share of these users who used the pill for the first time *after* they became a mother.

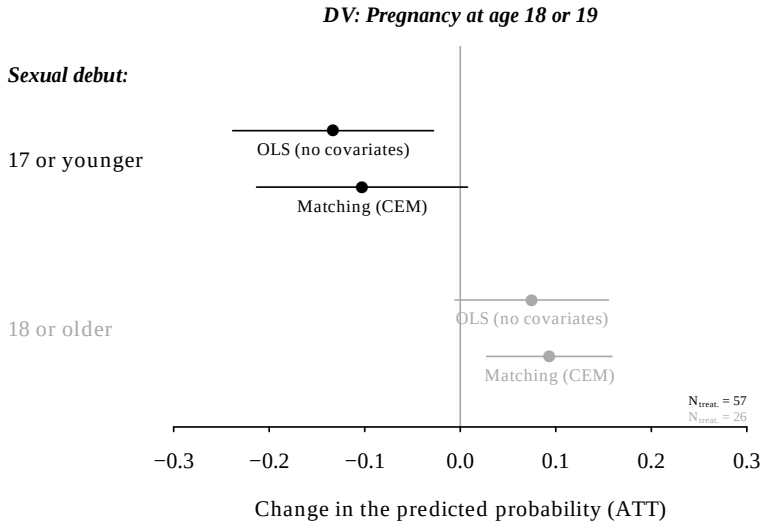


Figure 9: The sample average treatment effect for the treated, using uptake of the pill at age 18 or 19 as the treatment.

users so that they are similar to the pill users on the variables in Figure 7.[13] With this reweighted sample, we then use least squares to estimate the average treatment effect for the treated of pill usage on teenage pregnancy, as well as key fertility, education, and labor market outcomes. If we still see differences between the groups, differences are more likely to reflect the use of the pill, although we cannot rule out that there are other background variables that might explain the differences.

[13] We use Coarsened Exact Matching to adjust for observable differences between pill users and those who do not use the pill (Iacus, King & Porro, 2012). See Online Appendix section A.8 for details.

The estimated impact of the pill on teenage pregnancy is displayed in Figure 9. The estimates are for the 1947 to 1954 cohorts and pill use means having used the pill as an 18 or 19 year old, and teenage pregnancy is here pregnancy as a 18 or 19 year old. The results are notably different for respondents who had their sexual debut before turning 18 and those who did not. In the former group, having used the pill is predicted to lead to an about ten-percentage point decrease in the chance of becoming pregnant as a teenager. In the latter group, pill usage is instead positively correlated with teenage pregnancy. Consequently, the negative effect of the introduction of the pill on teenage births we found in the previous section is likely to be driven by women who were sexually active already before they had access to the pill. That we find a clear negative effect of the pill on teenage motherhood in analyses using both administrative and survey data, and with different research designs, strengthens our beliefs that the introduction of the pill did indeed cause a reduction in teenage pregnancy.

Do we see evidence in the survey data that the pill also influence fertility, education, and labor market outcomes? Figure 10 provides the matching estimates for a set of outcomes. Since the survey was carried out 1977, many of the respondents in the 1947 to 1954 cohorts are likely to have had one or more children *after* 1977. As such, we cannot look at the age of first birth or the number of children. With regards to *fertility*, we therefore instead look at how many children the respondents ideally would like to have, and whether they were married before they became pregnant. For both of these outcomes, however, pill usage is insignificant for both sexual debut groups. Still there is a tendency that respondents with an early debut age are more likely to be married before they become pregnant if they used the pill. The same holds for *education*. Also here there is a tendency that pill usage among women with sexual debut before age 18 increases the women's years of education.

The most unequivocal result emerges from looking at *labor market* outcomes. Labor market participation is defined as having a wage income in 1976. Among women with an early debut age, pill usage is predicted to lead to an almost twenty percentage point increase in the chance of participating in the labor market. Thus, our results point to an important *short-term* effect on labor market participation. However, the results from the administrative data, which estimated a zero effect the chance of being employed at the age of 50 (see Figure A.13), suggest that the effect of pill usage on employment

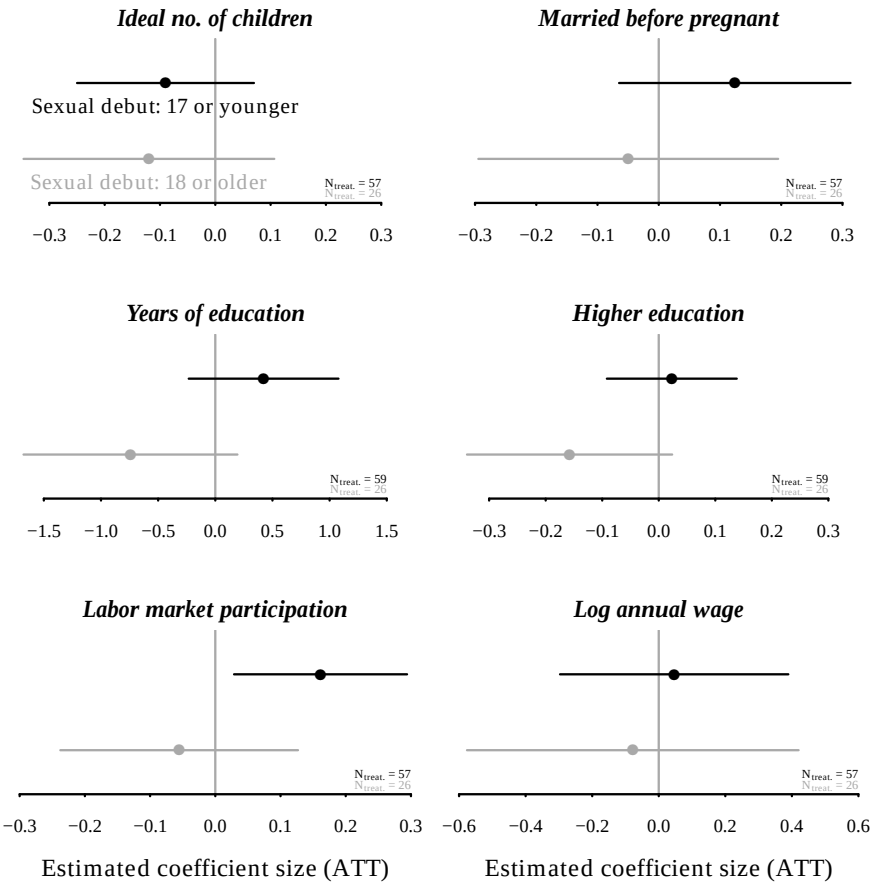


Figure 10: The sample average treatment effect for the treated, using uptake of the pill at age 18 or 19 as the treatment.

is short lived. What is more, as the right panel documents, pill usage has little effect on the wages among those participating in the labor market, which mirrors the DiD findings in the previous section.

4 Conclusions: the confined effects of a miracle tablet

Legal change does not necessarily translate into social change, as several of the contributions to this special issue demonstrate (Htun, Jensenius & Nunez, 2018; Toyofuku, 2018; see also Htun, 2016; Jensenius, 2017). Instead, we need to understand the conditions under which women's economic empowerment will and will not follow from the enactment of legal reforms. The invention and later legalization of the contraceptive pill is no exception. Already before its birth, activists and scientists alike saw the pill as a potential "miracle tablet." The first research on the United States—studying variation in legal access to the pill—largely confirmed these expectations, arguing that the legalization of the pill lead women to delay pregnancy and reduce number of

births, which again enabled women to invest in their professional careers. Yet, these studies have recently been challenged (Myers 2017), thus emphasizing the need for more evidence from other contexts than the United States to understand the scope of change in women's fertility and economic empowerment that the pill can bring about.

To this end, we have examined Norway's legalization of the pill using both population-wide administrative and survey data. With data on fertility, as well as marriage, education, and labor market participation for all women born between 1935 and 1959, we are able to provide a detailed assessment of the effects of the pill in a setting that is different from the US. Moreover, as the Norwegian legislation made the pill available for contraceptive purposes for *all women* in the age group at the *same* time—and not, as in the US, first married and later unmarried women—we examine a more comprehensive reform than in much of the previous literature.

Our findings indicate several conclusions about the distinction between legal change and the social changes ensuing the reform. A first conclusion is that we cannot simply assume that legal changes have encompassing effects on women's economic empowerment. Despite legal access, only a minority of Norwegian women, particularly among the first cohorts with access, went on the pill. An important reason for this hesitance was probably skepticism from medical authorities and the public alike concerning the potential side effects of the pill. A non-negligible share of women also held (religious) norms that made them less likely to be sexually active before marriage and unfavorable to both self-determined abortion and pill consumption. Finally, for many women in Norway, the long distance to the nearest pharmacy significantly increased the cost of doing so for many who wished to use the pill.

As in other domains, legal pill reforms thus need to be coupled with resources that ensure its actual availability to all. Even then, however, the effects will depend on the degree to which the use of the pill is congruous with pre-existing norms. In this light it is less surprising that we find only confined effects of the pill reforms. If a majority of women do not use the pill despite its availability, the overall impact on women's fertility and economic empowerment will be limited, at least in the short and medium term. In the longer term, however, gradual norm change may make pill usage more acceptable, thus also increasing the impact of the pill.

Future research should thus also further investigate how timing and de facto

availability affect the social consequences of pill reforms. In 2002, for instance, the pill became free of charge for Norwegian women under age twenty, and nurses on each high school got the right to prescribe the pill. Given that norms about pill use changed substantially between the 1970s and the early 2000s, the greater availability of the pill across the country following the 2002 reform might turn out to have a larger impact on women's fertility and careers than the comprehensive legalization of the pill examined in this paper. To ensure that women can make fully independent fertility decisions, our study indicates that legalization in itself is insufficient.

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Online Appendix for

'The Miracle Tablet *Maybe*'

Legalization of the Pill Women's Childbearing and Career Decisions

Abstract: A substantial literature argues that the legalization of the birth control pill in the United States reduced fertility and enhanced career investments. This study questions whether the broader effects hold across contexts. We use administrative data to study the Norwegian legalization of the pill and find effects on teenage motherhood but not on subsequent career outcomes. Using survey data, we show that the pill's impact on fertility is largest for women with an early sexual debut; nevertheless, broader effects of pill usage are still absent. Our study indicates that the pill cannot explain the rise of Norwegian women's economic empowerment.

A Online appendix

A.1 Details on variable operationalizations

Fertility and marriage. In the administrative data, we know the birth year (but not the month) for each woman and her children. The Norwegian reforms legalized the pill for contraceptive purposes for women aged eighteen or above in 1967; only in 1972, however, could women below eighteen access the pill. If we want to study how these reforms affected *teenage pregnancy and motherhood*, we thus need to distinguish motherhood before age nineteen from motherhood at age nineteen or twenty.[1] Whereas the likelihood of motherhood at age nineteen or twenty should be affected by the 1967 reform, motherhood before age nineteen should not. The 1972 reform, on the other hand, should affect the chance of becoming a mother before age nineteen. In addition to these two motherhood variables, we also study mothers' age at first and second birth, the number of children (conditional upon being a mother), and the chance of being without children. Together these six variables allow us to detect potential effects of the pill reforms on a broad set of fertility outcomes. Additionally, we study the effects on *marriage and divorce*, namely the effect on ever being married and ever being divorced. Goldin & Katz (2002) and Edlund & Machado (2015) examine the effect of early legal access to the pill in the US on the chance of getting married before having a child. Unfortunately, we do not know the marital status before 1975, which limits our possibility to study this outcome.

Education. To investigate the effects of the pill on educational outcomes, we use the detailed educational codes in the administrative data. The codes provide information about the highest completed education for all individuals, which we use to create a variable measuring each individual's years of education beyond compulsory education and a binary variable measuring whether the individual has obtained a higher education degree or not.[2]

Labor market. Finally, we also look for downstream effects of the pill on wages and employment. We measure wages as total annual earnings from labour. The wage

[1] Most of the children born when the mother was twenty was conceived when she was nineteen.
[2] Primary and secondary education are compulsory in Norway.

data in our dataset is, regrettably, only available for the years 1994 to 2005. As wages typically rise over the life course, we need to compare different cohorts when they are at the same age. By measuring wages at the age of 50, we can include the 1944 to 1955 cohorts in our analysis. Though of course limiting what we can say about wages earlier in the life course, the US evidence does indicate that the effect of the pill on reducing the gender wage gap is largest at around age 50 (Bailey, Hershbein & Miller, 2012). In addition, we also use the wage data to determine whether an individual is in paid employment our not at age 50.[3] Our data should hence be able to detect downstream wage and employment effects of access to the pill.

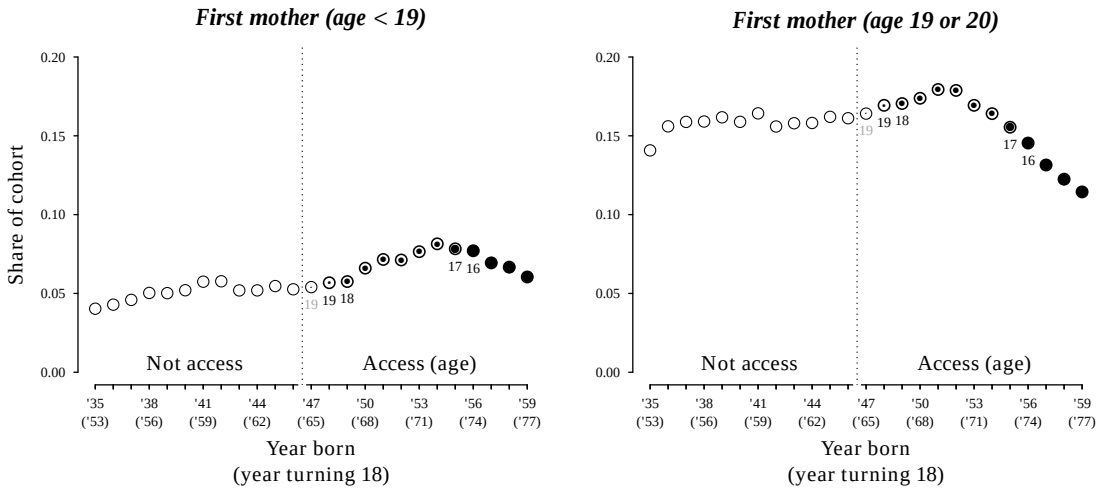
A.2 Descriptive trends across cohorts

Figure A.1 illustrate the trends in *fertility* across the cohorts born between 1935 and 1959. As the 1948 cohort, which turned 19 in 1967, was the first to benefit from the pill,[4] we include more than ten younger and older cohorts to visualize both the development before and after the 1967 and 1972 pill reforms.

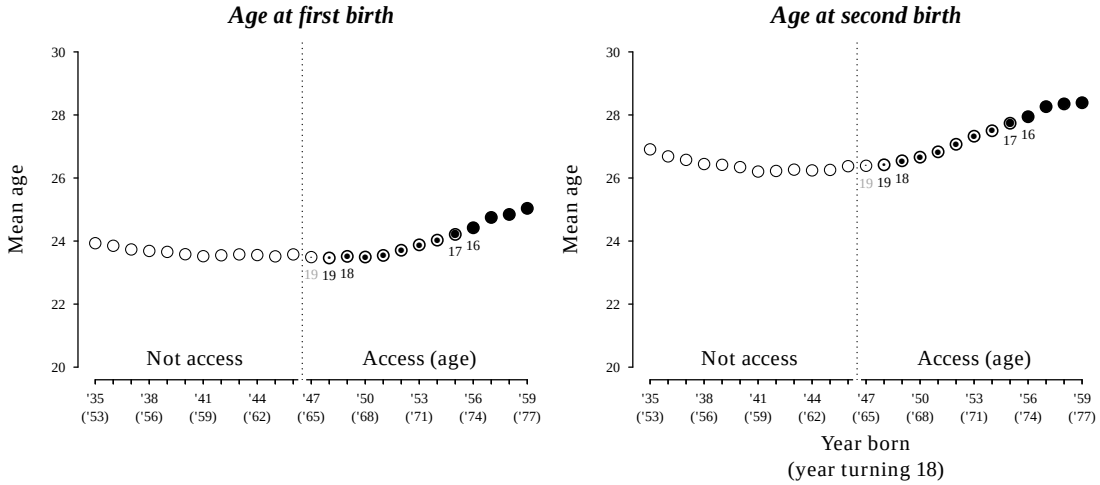
Figure A.1a shows the share of a cohort who first became a mother at age below 19 (left panel) and age 19 or 20 (right panel).[5] The x-axis shows the cohorts' birth year and the year they turned eighteen. To visualize the reform, the dotted vertical line separates the cohorts who were exposed to the reform from those who were not and the black-filled points specifies at what age the cohort got access to the pill. Given that the pill was made available to teenagers below eighteen in 1972, the 1955 cohort was the first to gain access at age seventeen and the 1956 cohort the first at age sixteen. Cohorts were thus differently exposed to the pill as teenagers. In the right panel, we see that becoming a mother among the 19 and 20 year olds is quite stable at 15 percent before the 1967 pill reform. After the reform, however, the percentage goes slightly up until the 1951 cohort, before it rapidly declines from the 1952 cohort and onwards.

It is possible that the downward trend is due to access to abortion and not the pill (Mølland, 2016).[6] Access to abortion became available to teenagers in the southeastern

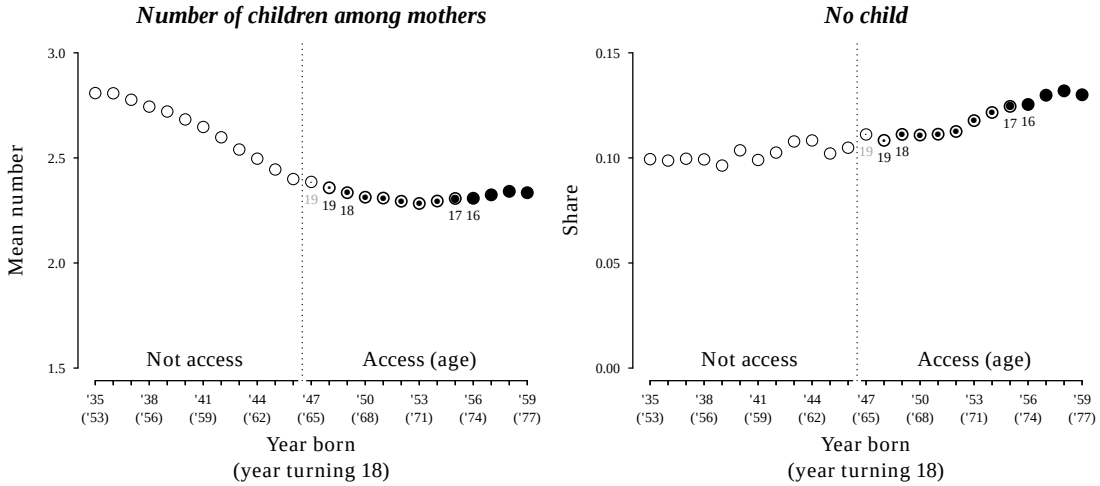
[3] If an individual has annual wage earnings above 50,000 NOK (about a fifth of an average full-time wage), we code the individual as being employed.
[4] To be precise, also individuals born late in 1947 would get access some months before they turned 20.
[5] Given the length of a pregnancy, most of the conceptions would thus occur while the woman was a teenager.
[6] This would be in line with Myers (2017), who shows that in the US, access to abortion was more



(a) Share women in a cohort who become a mother as teenagers.



(b) Mothers' mean age when giving birth for the first and second time.



(c) The mean number of children among mothers and the share of women who are childless.

Figure A.1: Cohort trends in fertility outcomes.

Source: Own calculations from administrative data.

region of Oslo and Akershus earlier than in the rest of the country due to more liberal doctors in this region. According to Mølland (2016, p. 9), the early 1950s cohorts in Oslo and Akershus were the first to receive access to abortion as teenagers. For these cohorts access was limited elsewhere in the country. Using a difference-in-differences design, Mølland (2016) demonstrates that access to abortion decreased teenage fertility but did not affect family size. The downward trends starting with the 1952 cohort in the right panel in Figure A.1a might thus be driven by women in the southeastern region's access to the pill. Yet, if we exclude the Oslo and Akershus region from Figure A.1a, the trends are nearly identical, with the decline in motherhood rates still starting with the 1952 cohort (see Figure A.4 in the Online Appendix). As such, the trend does not easily square with the alternative abortion hypothesis.

Moreover, in the left panel of Figure A.1a, which plots motherhood rates for teenagers age 16-18, we see that the downward trend in the motherhood rate starts precisely with the 1955 cohort, who was the first to gain access to the pill before they turned 18. Also this pattern remains almost identical if we exclude Oslo and the surrounding region (see Figure A.4). In short, the descriptive graphical evidence matches quite well with the timing of the pill reforms, particularly for the 1972 reform and motherhood before age 19.

Next, Figure A.1b displays the cohort development in the mean age at first and second birth. Although unsurprisingly less pronounced than the trends in Figure A.1a, also these trends are quite consonant with the introduction of the pill as at least a partially driving force.

When we turn to the trends in the number of children, the left panel in Figure A.1c indicates that the reform does not affect the mean number of children per mother. The right panel, however, might indicate that, at the extensive margin, the pill increased the possibility for women to choose to not have children, as the share without children rises for the cohorts after the reform. Both of these results are in line with Bailey's (2006) argument that the pill foremost affects the timing and not number of births among women who want children and makes it simpler for women who do not want children to remain without children.

important than access to the pill for delaying motherhood.

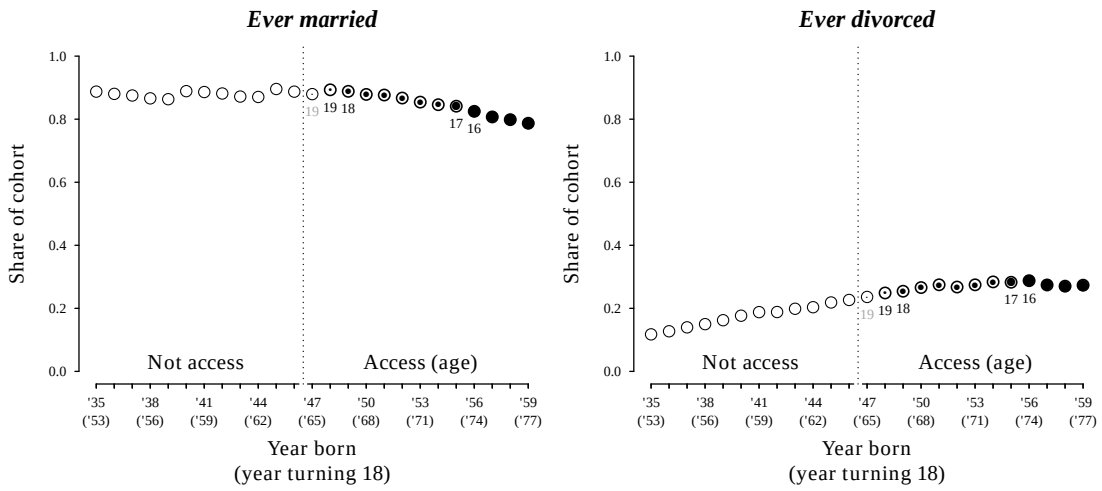
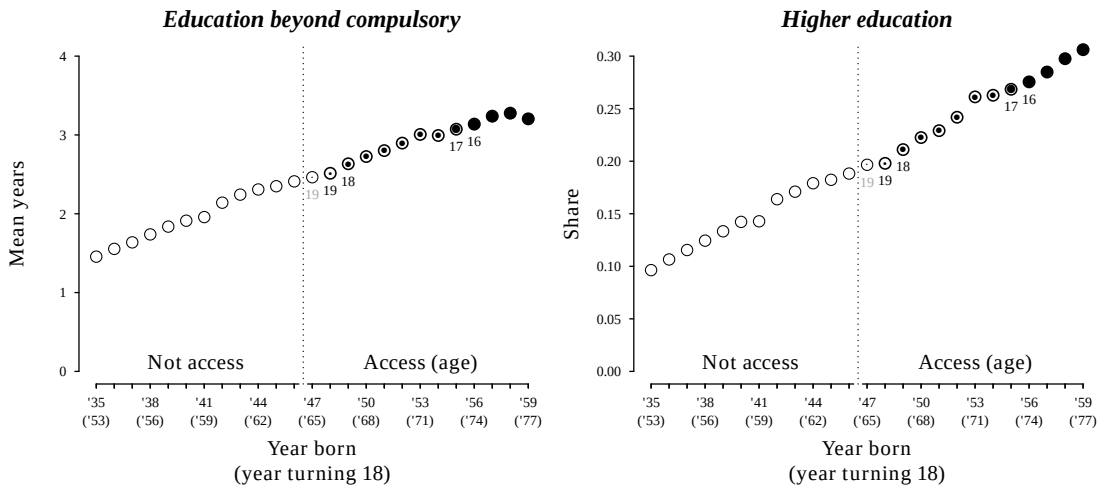


Figure A.2: Cohort trends in marriage and divorce.
Source: Own calculations.

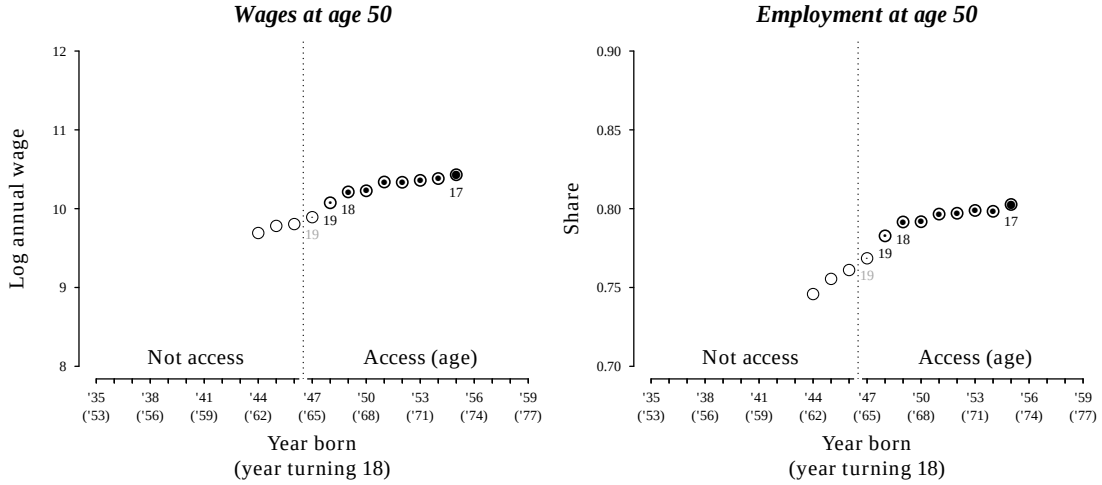
Obviously, any shifts in these trends that corresponds to the reform cannot be given a causal interpretation due to the potential for confounding trends; yet, at least the trends are broadly consistent with what access to the pill would lead us to expect. To further gauge these relationship, in sections below we exploit distance to pharmacies and use survey data to further understand the impact of the pill. First, however, we turn to graphical evidence on potential downstream effects of the pill on women's education and labor market participation.

For outcomes beyond fertility the trends are harder to square with a pronounced effect of the pill. Existing research argue that there should be important downstream effects on marriage, as well as educational and labor market investment (Bailey, 2006; Bailey, Hershbein & Miller, 2012; Goldin & Katz, 2002), although Gronqvist (2012) fail to find such downstream effects in his study of the 1989 pill subsidization reform in Sweden. Concerning *marriage and divorce*, Figure A.2 illustrates that there are no discernible shift in the trends after the pill reforms. The same holds for *education* in Figure A.3a, measured in both years (left panel) and the share having higher education (right panel). Finally, for *employment and wages* at age 50, depicted in Figure A.3b, there is no significant shift after the reform, although the data is for these variables more limited.

For all these variables we see slowly evolving changes in the expected direction of a higher frequency of divorce, increasing levels of education and of earnings. The developments across cohorts are, however, smooth as we move from less affected



(a) Years of education and share with a higher education degree among women.



(b) Wages and employment among women at age 50.

Figure A.3: Cohort trends in education, wages, and employment.

Note: The data on education specifies the highest completed degree by 1990. Wages are total annual labour earnings. Employment is defined as having annual wage earnings above 50,000 NOK. *Source:* Own calculations from administrative data.

cohorts to fully affected cohorts. Thus, the developments reflect broad societal and slowly evolving changes affecting these cohorts rather than the effect of the pill. It is important to keep in mind that these results do not imply that the pill had no important societal effects, but they suggest that if these effects exist, they affected all young cohorts around the legal reforms about the same and did not have a particularly strong immediate effect on the most exposed cohorts.

A.3 Trends in teenage fertility when excluding Oslo and Akershus

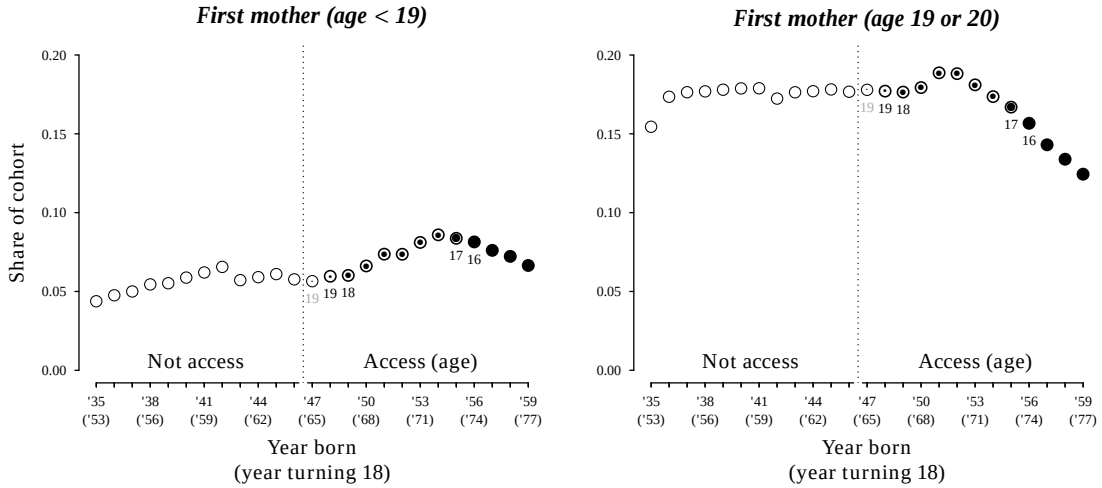


Figure A.4: Trends in teenage motherhood when excluding the Oslo and Akershus regions.

Source: Own calculations.

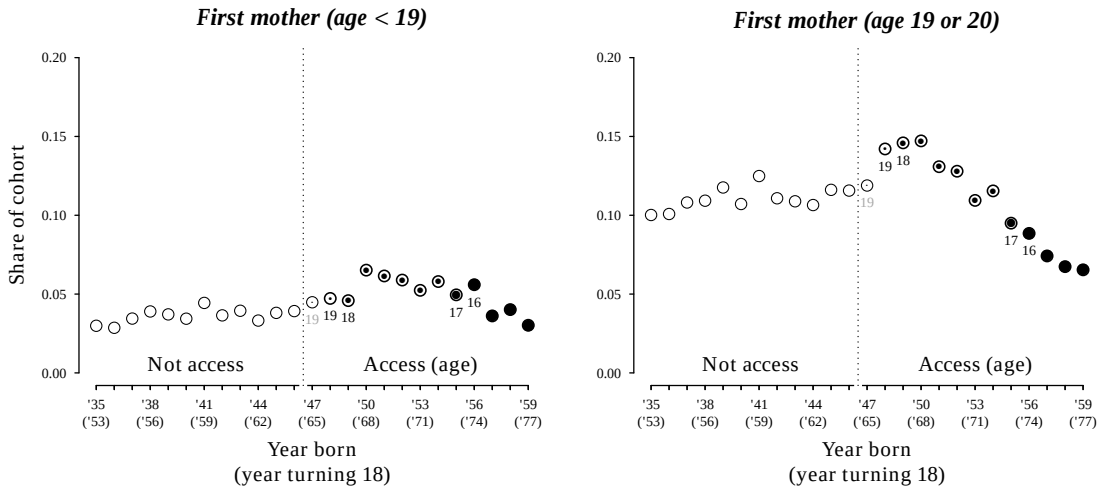


Figure A.5: Trends in teenage motherhood when in the Oslo and Akershus regions.

Source: Own calculations.

A.4 Distance to the nearest pharmacy

To calculate the distance to the nearest pharmacy for each municipality we proceed in the following steps. First, from the Norwegian Pharmaceutical Museum's archives in Oslo, we have obtained a complete list of pharmacies in Norway in 1967 and 1972, which also includes the address and name of each pharmacy. There was a state monopoly on pharmacies during this period, and the number of pharmacies stayed more or less constant. By digitizing the addresses, looking up missing addresses, and thereafter geocoding them, we created a dataset containing the exact geographical placement of each pharmacy, which are displayed as the black dots in the map of Norway in Figure A.6. Second, in our administrative data we know each individual's municipality of residence in 1967. We do not, however, know exactly where in the municipality an individual lived. To geographically locate the individual we therefore assume that individuals resided near the municipality's administrative center. From Lind (2014) we obtain the longitude and latitude of each administrative center.[7] Third, to calculate the distance from each municipal administrative center to the *nearest* pharmacy, we use geographical data on roads and ferry lines, which we link with the geocoded pharmacies, and the software ArcGIS's "Closest Facility" computation.[8] Specifically, we calculate the driving distance in minutes to the nearest pharmacy. The black lines in Figure A.6 displays the fastest driving route to a pharmacy from each municipality center.

The driving time to the nearest pharmacy varies considerably across municipalities. Figure A.7 plots the distribution of municipalities in terms of driving distances to the *nearest* pharmacy. In 25 percent of the municipalities, the driving time is 10 minutes or less. These are the municipalities in which there is a pharmacy in the municipality's administrative center. What is more noteworthy, however, is that the median driving time is almost half an hour, or about a 22 kilometers in distance. Given that the legal driving age was 18 and that the number of cars per capita was still quite low as a result of rationing until 1960, it is plausible that female teenagers living in a municipality

[7] Obtained from the Local Government Dataset (Fiva, Halse & Natvik, 2015; Fiva & Natvik, 2013).
[8] The data on roads and ferry lines is not available for historical time periods. We therefore use data from the 2010s, which is obtained from the Norwegian Mapping Authority.

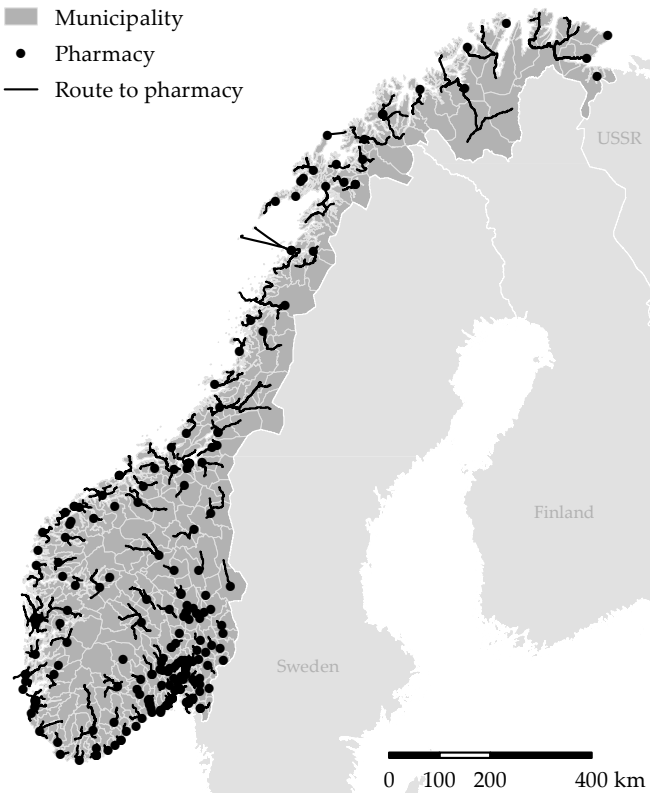


Figure A.6: Location of pharmacies in Norway in 1972.

Source: The Norwegian Pharmaceutical Museum and authors' own calculations.

with median or longer driving times to the nearest pharmacy in practice had very limited access to the pill also after the reforms in 1967 and 1972. Consequently, we can use this as a control group to gauge the effect of the introduction of the pill on fertility, education, and labor market outcomes.

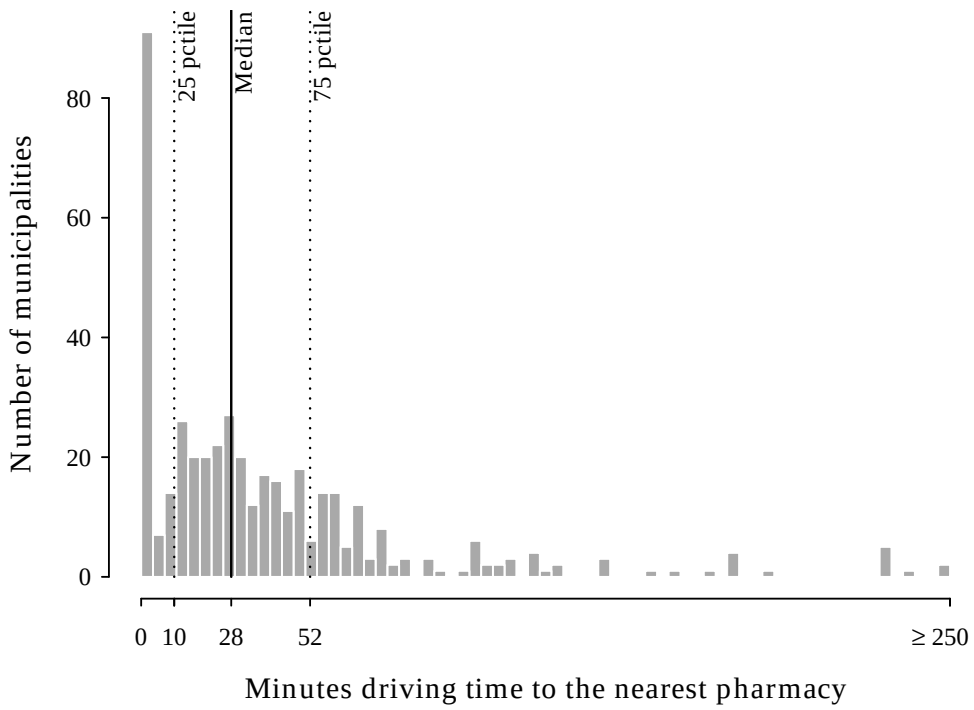
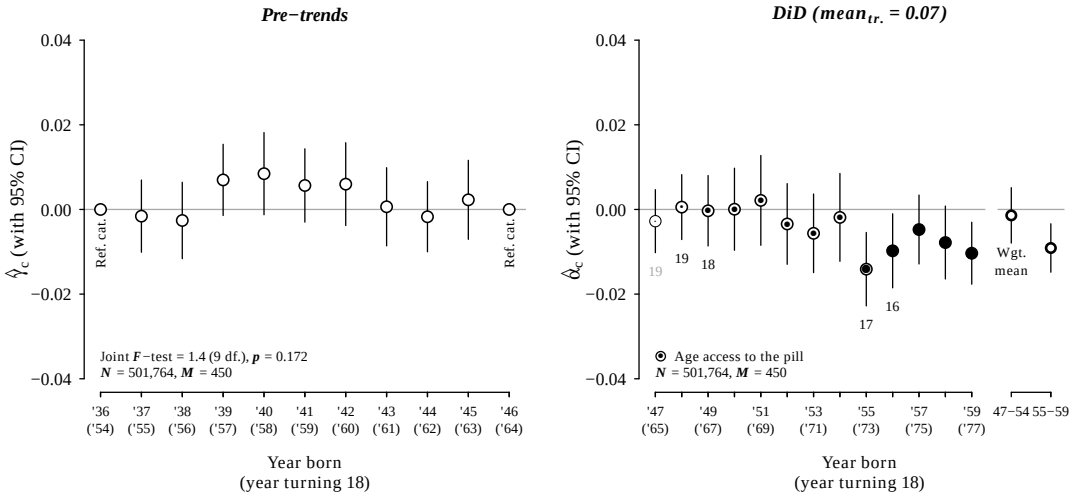
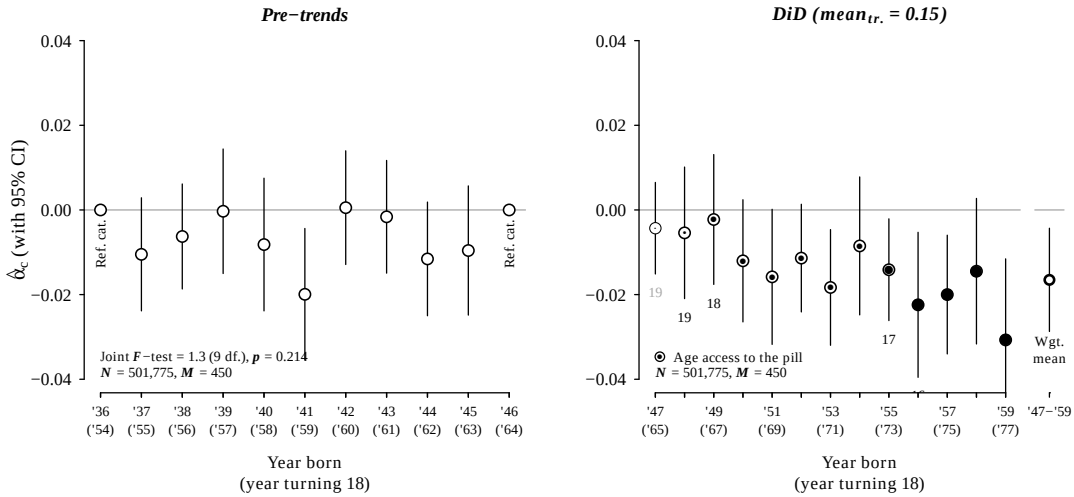


Figure A.7: The municipality distribution of driving time to the nearest pharmacy from the municipality administrative center.

A.5 Difference-in-differences results for teenage motherhood without the Oslo and Akershus region and with including abortion rates as a covariate



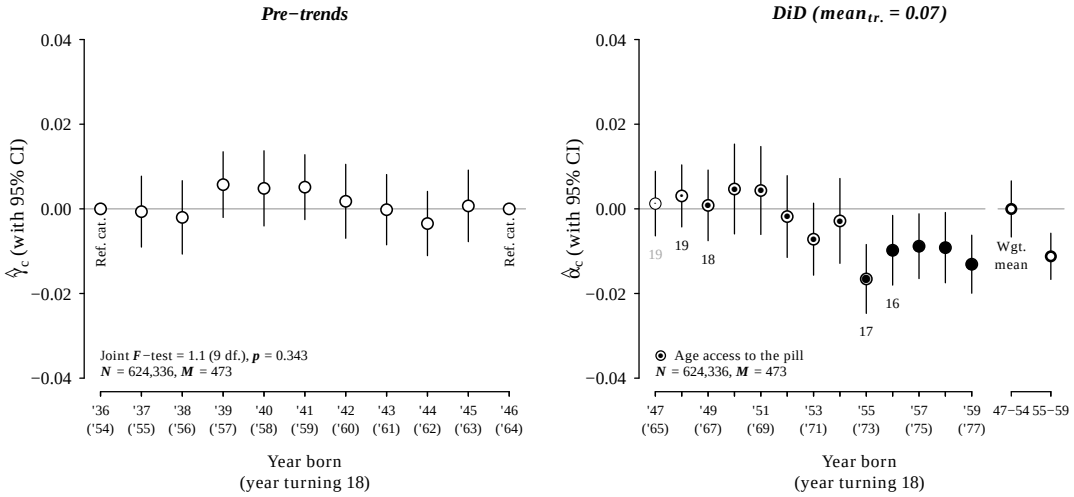
(a) Mother before 19 years old.



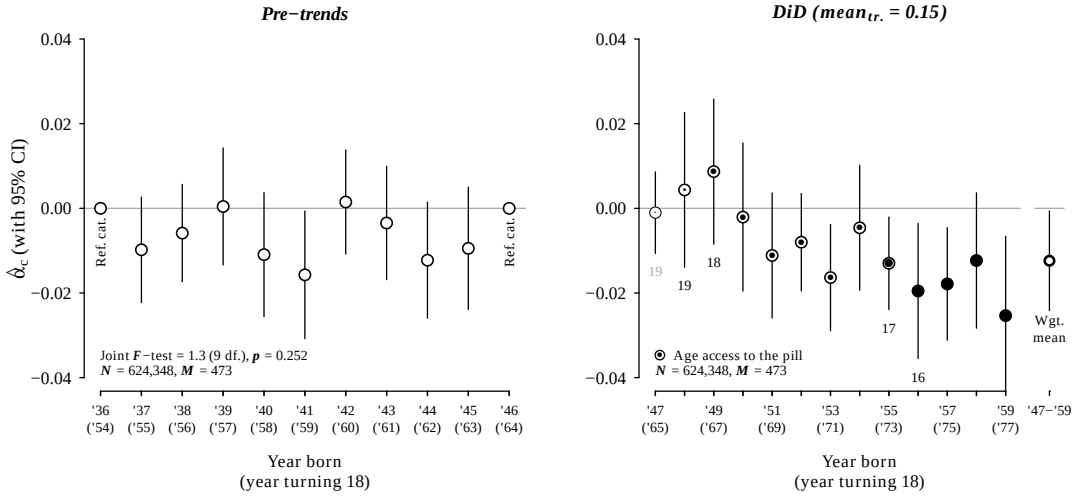
(b) Mother as a 19 or 20 year old.

Figure A.8: Results from Figure 4 *without the Oslo and Akerhus region*.

Note: See Figure 4 for details.



(a) Mother before 19 years old.



(b) Mother as a 19 or 20 year old.

Figure A.9: Results from Figure 4 *with abortion rate covariate*.

Note: From 1964 and onwards, we have collected data on induced abortions accepted by the abortion committees for each of the 19 Norwegian counties from Statistics Norway's Health Statistics annual yearbooks between 1964 and 1978 (before 1969, the numbers are available for regions only). To get the abortion rate, we then divide the number of accepted abortions by the female population age 15-49 in the county. For each individual in each cohort we then calculate the average abortion rate in their county of residence while they were 16 to 18 years old. Echoing the results in Mølland (2016, p. 12), we find that, in a model with county- and cohort-fixed effects, a one standard deviation increase in the abortion rates decreases the chance of teenage motherhood by 1.6 percentage points (p <0.001).

A.6 Additional difference-in-differences results for fertility, marriage, education, and labor market outcomes

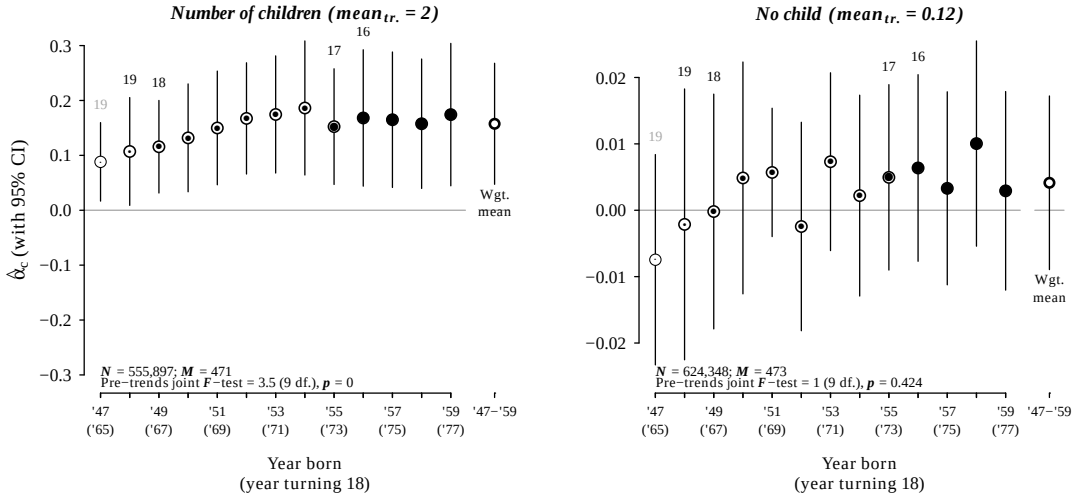


Figure A.10: Treatment effect for the cohorts with legal access to the pill, i.e., changes in the estimated probability of becoming a mother before turning 19 years old (left panel) and as a 19 or 20 year old (right panel).

Note: For details on the estimation, see the note to Figure 4 in the main text.

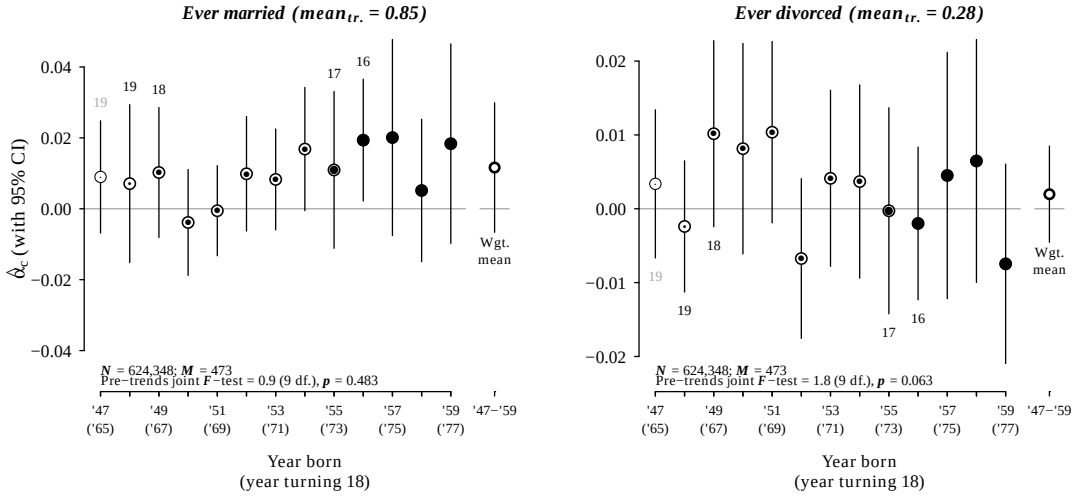


Figure A.11: Treatment effect for the cohorts with legal access to the pill, i.e., changes in the estimated probability of becoming a mother before turning 19 years old (left panel) and as a 19 or 20 year old (right panel).

Note: For details on the estimation, see the note to Figure 4 in the main text.

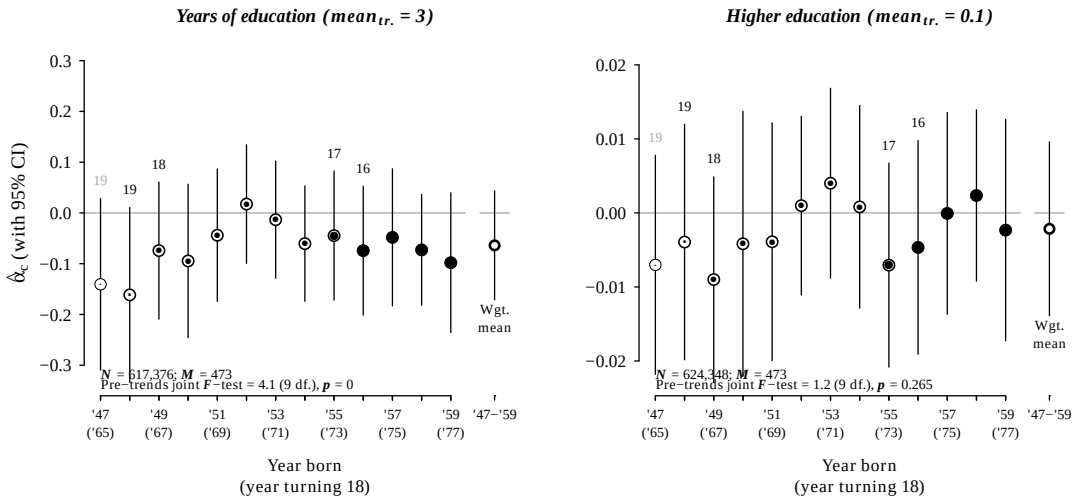


Figure A.12: Treatment effect for the cohorts with legal access to the pill, i.e., changes in the estimated probability of becoming a mother before turning 19 years old (left panel) and as a 19 or 20 year old (right panel).

Note: For details on the estimation, see the note to Figure 4 in the main text.

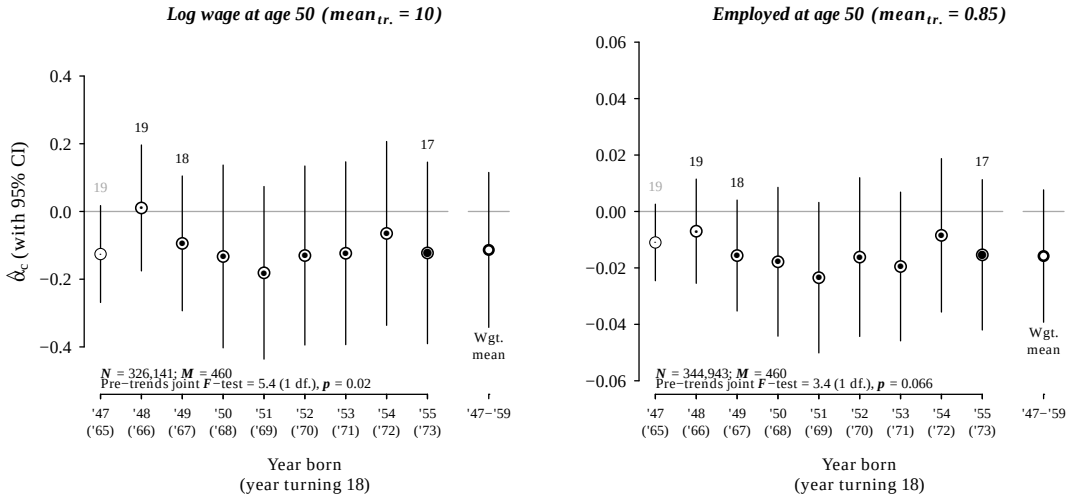
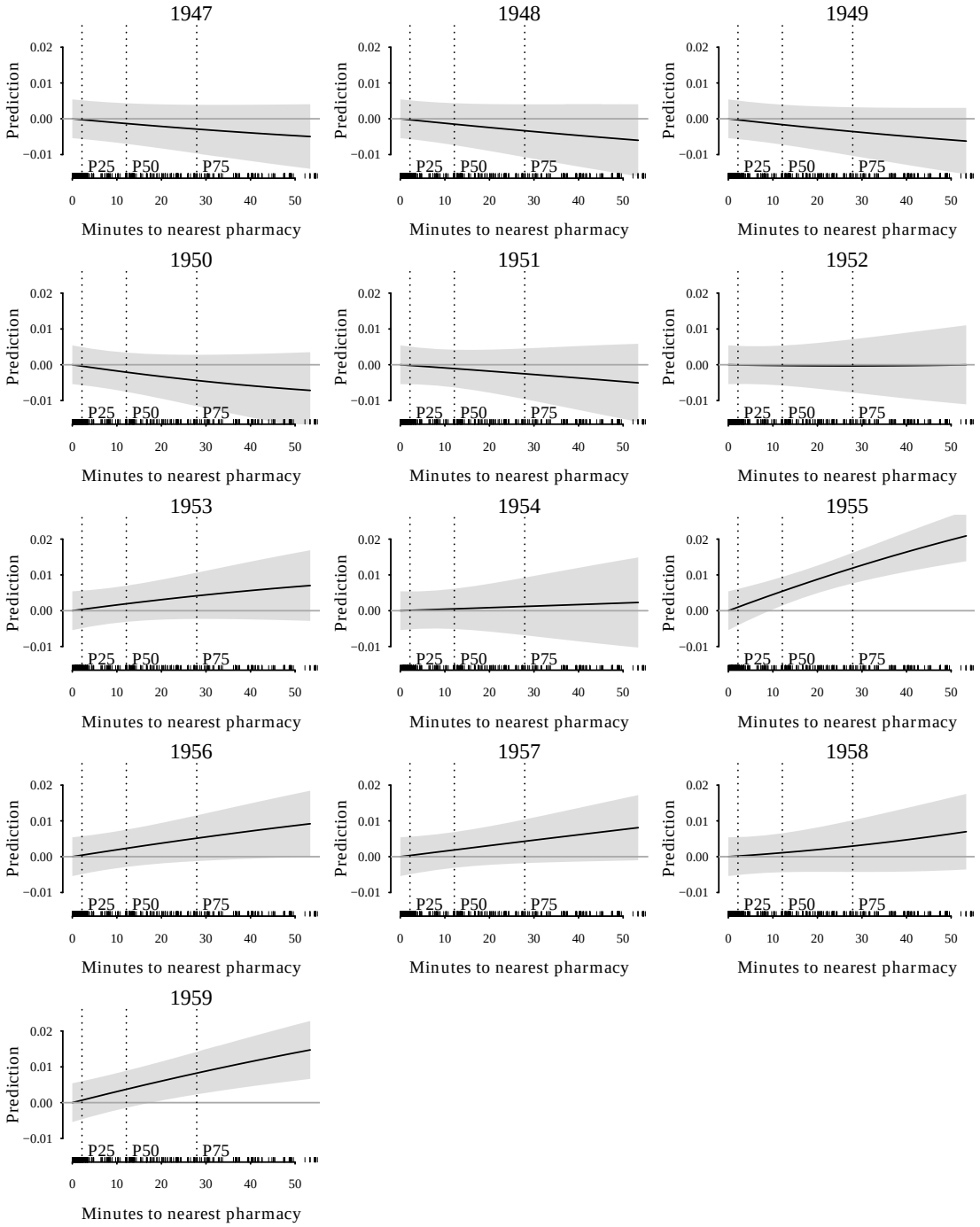


Figure A.13: Treatment effect for the cohorts with legal access to the pill, i.e., changes in the estimated probability of becoming a mother before turning 19 years old (left panel) and as a 19 or 20 year old (right panel).

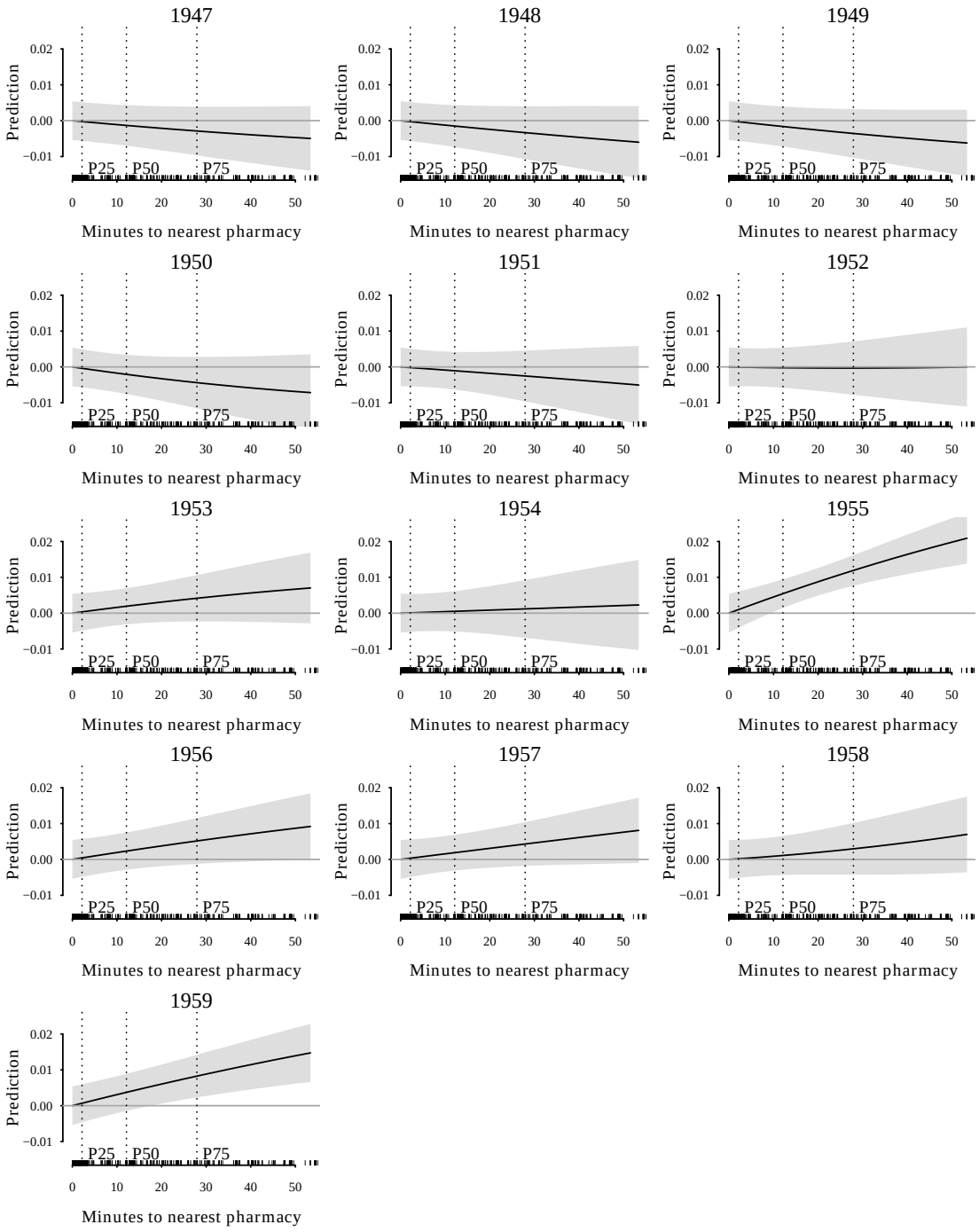
Note: For details on the estimation, see the note to Figure 4 in the main text.

A.7 Re-estimating the DiD results in Figure 4 with polynomials

Whereas the binary distance-to-pharmacy variable takes the value 1 if the distance is *below* the median value, the continuous measure goes in the other direction, with larger values indicating longer traveling time to the nearest pharmacy. Largely mirroring the results in Figure 4a (though somewhat more imprecise), Figure A.14a shows that starting with the 1955 cohort, the probability of giving birth before turning 19 *increases* with *increasing* driving time to the nearest pharmacy. The same picture emerges in Figure A.14b, where giving birth at age 19 or 20 is the dependent variable. Also the results in this figure echoes the conclusions drawn from Figure 4b in the main text.



(a) Mother before 19 years old.



(b) Mother as a 19 or 20 year old.

Figure A.14: Re-estimating the DiD results in Figure 4 (Equation 1) with polynomials for distance to nearest pharmacy.

Note: The figures shows the predicted difference (with 0 minutes distance set to 0). The vertical lines give the percentiles of the minutes-to-nearest-pharmacy variable. The x axis displays values up to the 90th percentile of the minutes-to-nearest-pharmacy variable (see Figure A.7 for the distribution of the variable).

A.8 Details on the matching approach

The matching was carried out separately for respondents with sexual debut age below 18 and 18 or above. The samples were matched on the following covariates: upbringing in densely vs. sparsely populated area, region of upbringing (north vs. non-north), mother worked (seldom/never vs. more than seldom/never), education (less than high school vs. one or more years of high school), religious sentiment (weak vs. medium/strong), and abortion opinion (not pro-choice vs. pro-choice). Based on the results in Figure 7, we simplify some of the covariates before matching: for *region*, we compare the northern region to being brought up in any of the other regions; for *mother worked*, we compare "seldom or never" to being in any of the other categories; and for *religious sentiments*, we collapse "medium" and "strong" to one category. To deal with missing values, we also match on missingness for each covariate.

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