Benefits from Delay?
The Effect of Abortion Availability on Young Women*

by

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Abstract

While much is now known about the effects of the arrival of the contraceptive pill on the fertility choices and other outcomes of women, there has been less study of the effects of abortion availability. Abortion was made widely available to teenage women in Oslo several years before the rest of Norway. We use a differences-in-differences approach to examine the effects on teen childbearing, fertility at older ages, educational attainment, and labor market outcomes of the affected women. We also study several outcomes for the first-born children of these women. We find that abortion availability delayed fertility but did not reduce completed family size. It also resulted in higher educational attainment. Children of mothers who had access to abortion are also found to have better outcomes.

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1. Introduction

The 1960s and 70s were a transformative time for women, characterized by periods of increased labor force participation and economic opportunities. One key explanation for this has been access to new contraceptive technology—including access to abortion and the birth control pill—that gave women more control over the timing of their fertility. While there has been a substantial literature on the role of access to the birth control pill and abortion, the abortion literature has largely focused on effects on fertility and on children's outcomes arising from selection. Because we have education data and panel data on women from ages 25 to 55, we can trace out the full life-cycle effects of teenage abortion access on human capital accumulation and labor market participation and earnings. Additionally, unlike most of the literature, we can match affected women to their children as adults and so study the full intergenerational effects of abortion access. Our strategy is to use a unique feature of the introduction of abortion access in Norway whereby one county (Oslo) obtained access approximately 4 years prior to the rest of Norway.

There are a number of mechanisms by which access to abortion as a teenager would affect long-run outcomes of women and, ultimately, their children. As described by Goldin and Katz (2002) in their discussion of the birth control pill, there can be both direct and indirect effects. Women are able to time their fertility decision and can thus delay marriage and instead invest more in their own human capital—this is the direct effect. However, even those individuals who might never use these new technologies might change their own behavior—as more women delay marriage, the pool of eligible men for older women improves; thereby incentivizing other women to delay as well—this is the indirect effect.

To estimate the effect of early abortion access, we use the exogenous increase in access to abortion for teenagers experienced in Oslo, Norway, approximately 4 years prior to the increase

in access for the rest of Norway. In the mid 1960s in Norway, abortion access was quite restrictive and subject to approval by both a local physician and a two-doctor committee. Oslo was the first location to loosen these restrictions, making it easier for women to obtain abortions. As a result, we see a dramatic decline in teenage fertility in Oslo beginning in 1969. It is only in 1972 that other counties became more open in their access to abortion and became more similar to Oslo.

Importantly for our work, access to birth control pills does not confound our results. Prior to 1972 in Norway, it was believed that birth control pills were harmful for teenagers and, as a result, they were not prescribed for use by those under 20. Because of this, we are able to separate out the effect of abortion access from that of the pill, something that previous research has found difficult to do in a convincing way.

Using a differences-in-differences strategy, we first show a dramatic decline in teenage fertility with the expansion of abortion access to teenagers in Oslo relative to other parts of the country. We then show that, although short-run teenage fertility rates are dramatically reduced, there is little evidence of any long-run negative effect on completed fertility. We also find effects on educational attainment and labor market outcomes of women—importantly, while education increased overall, the labor market effects are more ambiguous, with improvements in attachment and earnings earlier in the life cycle and reductions later, perhaps as a result of delayed fertility.

Finally, we also examine the effects of this delayed fertility on the outcomes of the children of these women. While existing research on abortion has examined how access to abortion has affected the marginal child born (Gruber et al., 1999; Ananat et al., 2009), we are able to move beyond this and examine how the outcomes of the first-born children changed when their mothers gained access to abortion. We find that these children experience more positive

outcomes, including increased education, increased employment, and reduced use of welfare. We also examine the selection effects of teenage access to abortion by studying the outcomes of children born to teenage mothers. We provide a number of robustness checks, including testing for differential trends prior to the introduction of abortion access and the use of the synthetic control method as an alternative estimation strategy. Our conclusions are robust to these checks.

The paper unfolds as follows. Section 2 presents a review of the relevant literature, and Section 3 presents the institutional framework that serves as our source of identification. Section 4 presents our empirical strategy, Section 5 describes our data, and Section 6 presents results on fertility, human capital and labor market outcomes for women. Section 7 presents intergenerational results and section 8 contains our robustness tests. Section 9 then concludes.

2. Relevant Literature

The research on the effects of abortion access is closely related to that examining the effect of access to the birth control pill, in the sense that both give a woman more power in timing fertility. As a young woman, the ability to control the timing of fertility—either through contraception or access to abortion—reduces the cost and increases the return to investment in human capital and thereby increases long-run labor market opportunities of these women (Goldin and Katz, 2002).

There has been substantial work done examining the role of increased access—legal or financial--to birth control pills.[1] Most relevant to our work is the research that uses variation in access among young women. In one of the earliest papers on the topic, Goldin and Katz (2002) use variation in age of majority laws across states to identify the effect of access to the pill as a

[1] See Guldi (2011) for a comprehensive review of this literature.

teenager on women's marriage and education decisions. Bailey (2006) uses similar variation to examine the effect of access to the Pill on women's labor market outcomes. The literature provides compelling evidence that legal access to the Pill led to later marriages and childbearing, increased educational investment, and improved labor force participation and labor market outcomes for women. The children of these women also experienced improved economic outcomes.[2]

The largest component of the literature on the effects of abortion access focuses on the effect on fertility of women. For the U.S., Levine et al. (1999) show using state-level variation in the timing of abortion legalization that legal abortion led to a 4% decline in the birth rate, with a larger reduction for teenagers. Ananat et al. (2007) use a similar source of variation to demonstrate that reductions in early fertility due to abortion translate into reductions in completed family size at the extensive margin with an increase in the number of women who remain childless but no effect on the intensive margin.[3] A number of other papers examine the effect of abortion access on female fertility, both in the U.S. and in other (most often European) countries, and these papers tend to find that increased access leads to lower fertility at earlier ages. (Levine, 2004, provides a nice summary of the literature). However, much less is known about the effects on education and labor market outcomes of these affected women.[4] Our paper proposes to help fill this gap.

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[2] Other related papers on the topic include Ananat and Hungerman (2012) who show that pill availability decreased the proportion of children born to poor mothers. There is also a significant literature on the effect of access to subsidized contraception on fertility decisions (Bailey 2012). Other studies of pill impacts include Guldi(2008), Hock (2007), Pantano (2007) and Edlund and Machado (2009).

[3] They also study the effects of abortion availability on education and labor market outcomes of women but find inconclusive results.

[4] One exception is Angrist and Evans (1996) who find positive labor market and schooling effects of abortion availability, especially for black females.

There is also a limited amount of work examining the effect of abortion access on the children of the exposed cohorts. This literature focuses on the selection effects of abortion and generally finds that abortion availability leads to better outcomes for children because poorer or less-prepared mothers are more likely to have abortions. Ananat et al. (2009) find that, in the U.S., increased abortion access led to the children of the exposed cohorts having a higher rate of college graduation, lower likelihood of welfare receipt, and lower odds of becoming a single parent.[5] There is also much research on the selection issue using other child outcomes such as crime, substance use, teen pregnancy and other outcomes.[6]

In this paper, we follow this literature by examining the selection effect of abortion availability. However, our major contribution to understanding the intergenerational effects of abortion availability is to study the overall effect on child outcomes by examining the effects on first-born children, irrespective of the age the mother was when the child was born.[7] If abortion availability mostly causes women to postpone rather than reduce fertility, this will provide a much better assessment than simply studying the selection effects on outcomes of children born to teenagers immediately after abortion legalization.

3. Institutional Background

[5] Some of the most compelling work on the effects of access to abortion has come from developing or transition economies. Work by Pop-Eleches (2006) examines the effect of the implementation of an abortion ban in Romania in 1966. He finds that this led to a large increase in the number of births in 1967 and that children born after the abortion ban had better schooling and labor market outcomes, on average. In a later paper, he studies the effects of the lifting of the ban on abortion in Romania in 1989 and finds positive effects on cognitive scores and better educational outcomes for children born after the lifting of the ban (Pop-Eleches, 2009).

[6] This literature includes contributions from Gruber et al. (1999), Grossman and Joyce (1990), Donohue and Levitt (2001) Foote and Goetze (2005), Joyce (2009a, 2009b), Charles and Stephens (2006), Ozbeklik (2007), Donohue, Grogger and Levitt (2009), and Lin and Pantano (2013).

[7] Note that this is not feasible using U.S. Census data as adult children cannot be matched to their parents.

Although abortion became legal in Norway in 1964[8], access was extremely limited prior to 1969—in 1968, the abortion rate was 8 per 1000 unmarried women in Oslo and even lower than that in other parts of Norway. Between 1969 and 1972, Oslo liberalized abortion access and, as a result, more women were able to obtain abortions. The abortion rate for unmarried women in Oslo rose to about 27 per thousand in 1972 and to over 30 per thousand in 1975. In stark contrast, because of a more strict interpretation of the law, the abortion rate in the rest of Norway did not increase beyond 10 per thousand until 1972.

Under the 1964 Norwegian abortion law, abortion was legal for medical, criminal or eugenic reasons. This includes cases where the mother's life was at risk, pregnancies that resulted from rape, and situations where the baby was unlikely to survive. Under this law, women could not make the decision to have an abortion on their own—a woman could apply to her doctor for an abortion and the doctor could choose to send the case to a two-doctor committee. Women applying for an abortion then had to meet with the committee to present and defend their case.[9] If the application was refused, the woman could file an appeal, which again had to be made by the woman's doctor. Because of social norms, unmarried women generally faced greater difficulties obtaining a legal abortion; as a result, teenagers were quite susceptible to the prevailing views of doctors in the areas in which they lived.

While abortion laws were changed at the same time throughout Norway, there was differential enforcement in Oslo relative to the rest of the country. Doctors were generally more liberal in Oslo and were more likely to approve abortion requests by unmarried women

[8] "Lov om svangerskapsavbrot i visse høve – 11. nov nr. 2 1960". (Law on abortion under certain conditions), and it was enacted February 1. 1964.

[9] There was great scope for individual doctors to influence abortion decisions either in the initial application stage or through their membership of the abortion committee. Grünfeld (1973) has documented that, in practice, similar cases were often treated very differently by doctors and by abortion committees and the doctors' personal views on abortion had great influence on the final outcome of the process.

(Grünfeld, 1973). To facilitate abortion access, pro-choice doctors opened two clinics in Oslo in 1970/1971 that provided free assistance to women to help them apply for abortions (Sarpsborg Arbeiderblad, 1970).[10] In addition, because there were more doctors per capita in Oslo, it was easier for women in Oslo to find a doctor to accommodate them in Oslo there were 379 inhabitants per physician, while in Bergen and Hordaland the equivalent number was 709, and this number was 694, on average, for Norway as a whole. (Norwegian Official Statistics, "Statistics for Physicians", 1970).[11]

Figure 1 shows abortion rates for unmarried women, a proxy for teenagers, for selected counties in Norway.[12] As is clear from the figure, Oslo experienced a much greater growth in abortions among unmarried women between 1969 and 1972. Figures 2 and 3 contrast the unmarried women's abortion rates in Oslo/Akershus (Akershus is the county that contains Oslo) with the average over the rest of the country. Again, we see a large relative increase in Oslo/Akershus until 1971 and then the difference flattens out. Bergen, the second biggest city in Norway, is an interesting comparison city for Oslo and it (see Figure 4) had a very small increase in the unmarried abortion rate over this period (Bergen is in Hordaland and so that county's abortion rate is a good reflection of that of Bergen).

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[10] In February 1971 a group of doctors opened a clinic in Oslo and stated publicly that they would help any woman apply for an abortion and give contraceptive guidance; the help was given for free (Ganes, 1975). The clinic was called "clinic for sexual information", though it is claimed that it could just as well have been called "aborthjelpen" (the abortion helper) as everyone knew that was why it was established (Reigstad & Ganes, 1981). From experience, the doctors at the clinic knew where to send particular abortion applications to increase the probability that they would be approved. The clinic had as its objective that every woman who wanted her pregnancy terminated would get help from the clinic; this information was also given through the newspapers (Reigstad & Ganes, 1981). In 1971/1972, it forwarded almost half of all applications in Oslo (Reigstad & Ganes, 1981).

[11] Bergen is the second largest city in Norway. Since 1972, Bergen and Hordaland have been one county, and in this paper we treat them as one county.

[12] We do not have information on abortion rates by county for teenagers over this period. However, abortions by unmarried women have been shown to be a reasonable proxy for teenage abortions. Grünfeld (1973) studied the combination of marital status and age for 5 counties representing different parts of Norway. He found that increases in the number of unmarried women getting an abortion were to a large extent due to more teenagers accessing abortion.

A key advantage of our study is that we can estimate the effect of abortion access without having our results confounded by the introduction of the birth control pill. Although the birth-control pill was introduced as a contraceptive in Norway in 1967, it was contraindicated for teenagers before 1972. As a result, doctors were reluctant to give it to young women prior to 1972.[13] In general, pill use in Norway was very low in this period; sales statistics indicate only around 100 user doses per 1000 women aged 18 to 44, between one third and one half of the take up rate in Sweden in the same period (Sakshaug, 1983).

As one way to verify that access to the Pill is not driving the changes we observe, we can look at the total pregnancy rate of teenagers—with the introduction of the birth control pill, we might expect pregnancy rates to decline. Although data on abortion among teenagers are limited, Figure 5 presents information about abortion among teenagers in Oslo (excluding Akershus) beginning in 1966. We then use this information to construct the pregnancy rate among teenagers each year. The total number of pregnancies is the total number of abortions registered among teenagers added to the total numbers of births registered by teenage mothers (before the month they turn 20).[14] We see that, if anything, pregnancy rates are increasing in Oslo.

4. Empirical Approach

Our empirical approach is a simple difference-in-differences strategy. Between 1969 and 1972, teenagers in Oslo had more liberal access to abortions than those in the rest of Norway; prior to that time, no one had access to abortions in Norway. As a result, we compare the change

[13] Appendix Figure1A uses data from a fertility survey in Norway carried out in 1977 to show the patterns of pill use by cohort, demonstrating that Oslo was very similar to the rest of Norway in terms of use of the birth control pill. This question asks if the individual has ever taken the birth control pill, and it is only asked to individuals who have been sexually active. In total 1727 answered that they had used the method, while 2164 answered that they had not used the method, 7 said they did not remember and 239 have missing information.

[14] One limitation here is that we do not have any information about illegal abortions.

in the outcomes (and those of their children) for those individuals who were teenagers in Oslo prior to abortion access to the outcomes of those who were teenagers just after access became available. We then compare that difference to the same difference in other parts of Norway, thereby differencing out national trends.

We consider cohorts born in Oslo between 1951 and 1954--individuals who had access to abortion when they were 18—as being treated, although in some specifications we allow the intensity of exposure to vary based on the number of teenage years during which an individual had access to abortion.[15] Because abortion rates were very low throughout Norway up to and including 1968, we assume that teenagers born prior to 1951 did not have abortion access in any part of Norway. After that, there was rapidly increasing access for the 1951 through 1956 cohorts in Oslo. On the other hand, in Bergen and in other parts of Norway, the increased availability of abortion only began with the 1954 cohort and was significantly less than in Oslo until at least the 1956 cohort. We consider only cohorts born prior to 1955 because, for that cohort and later cohorts, teenagers had access to the birth control pill (as the contraindication of the pill for teenagers ended in 1972.

There are 19 counties in Norway, including Oslo and Akershus. Since Akershus is the county surrounding Oslo, we refer to these two counties as Oslo hereafter. This leaves 17 counties as potential control counties. Our primary control group includes women from all the other 17 different counties, but we also test the robustness of our estimates to using Bergen as a single control county.

We estimate the following equation:

$$y_{ict} = \alpha_0 + \mu_c + \delta_t + \alpha_1 I(Cohort > 1950) * Oslo + \varepsilon_{ict}$$

[15] In previous work (Black, Devereux and Salvanes, 2008), we found the median age at birth for teenagers in Norway to be 18.

Here, y_{ict} is the outcome variable, either for the women or her first-born child *Oslo* indicates if a person lives in Oslo as a teenager, μ_c refers to a full set of county dummies and δ_t refers to a full set of indicator variables for birth cohort. The coefficient of interest is α_1, the interaction between Oslo and being from a post-1950 cohort. This represents the treatment effect of having access to abortion as a teenager.

Inference

Given the treatment (early abortion access) applies at the county level, variance estimates using the individual-level data would tend to understate uncertainty about parameter values.[16] Therefore, we calculate our standard errors using the method proposed by Donald and Lang (2007): We first take means to reduce the dimension of our dataset to 180 observations (18 counties by 10 cohorts (1945-1954)). Then, we do all estimation using these 180 county-by-cohort averages by regressing the average value of each dependent variable on a full set of county dummies, a full set of cohort dummies, and the treatment indicator (whether born in Oslo after 1950), weighting by the number of underlying individual observations in the cell. Donald and Lang (2007) find that, in situations with small numbers of treatment and control groups, standard asymptotics provide poor approximations but, in many circumstances, the distribution of the t-statistic is well-described by a t-distribution with *S-2* degrees of freedom (where *S* is the number of groups). Therefore, when doing inference we compare our estimated t-values to critical values from a t(16) distribution. In practice, this means that we require a t-statistic of at least 2.12 for significance at the 5% level and 1.746 for significance at the 10% level.

Even after aggregating to the county-year level, standard errors may be biased because of serial correlation. To address this issue, we also report estimates where we aggregate the data

[16] It is well-known that the standard approach of doing analysis at the individual-level and clustering by county leads to under-estimated variances when the number of groups is small as in our case (Donald and Lang, 2007).

further to 36 cells by taking the mean value of each variable before and after 1950 for each county. This leaves us with two observations per county.[17]

Bergen as the Control Group

When including all other counties as the control group, one might worry that the control group may have different trends than the treatment group. While we provide evidence that this is not the case (and, in fact, it seems like the assumption of equal trends does hold), we also estimate a specification where we limit our control group to Bergen, the second largest city in Norway (with Oslo being the largest).[18] When using Bergen as a control county, we aggregate the data to 20 observations (2 counties by 10 cohorts) and use a t(8) for inference.[19]

Mobility

One issue is whether teenagers from other parts of Norway could have gained access to abortion by traveling to Oslo. From 1968 on, we have data on where the women applying for an abortion are, and there is little evidence that women travelled to obtain an abortion during the period we are studying. The only exception is that it was common for unmarried women in

[17] We have carried out a set of placebo experiments to verify that our standard errors are reasonable. The idea is that, if standard errors are appropriate and the true treatment effect is zero, the rejection rate of the null hypothesis of zero effect should be equal to the significance level. To implement this, we exclude Oslo from the sample and then pretend that the treatment was implemented in 1951 for each of the other 17 counties in turn. For each county and each outcome variable we estimate a placebo treatment effect. We know that the true effect is zero and test this null hypothesis. Over 15 adult outcomes and 17 counties, this gives 255 placebo coefficients. The 15 adult outcomes used in the placebo tests are the 5 fertility outcomes in Table 1, the 4 educational outcomes in Table 2, plus full time employment at ages 30, 40, 50 and log(earnings) at ages 30, 40, and 50. Using the standard errors from the county-cohort (170 in this case as there are 17 counties) cell regressions, we find that 44 of these are significant at the 5% level giving a rejection rate of 17%. However, using the standard errors from the county-before-after (34 in this case, 2 observations for each of 17 counties) cell regressions we find that only 12 of them are significant at the 5% level, a rejection rate of 4.7%. This implies that the standard errors from the most aggregated approach are of correct size. For child outcomes (these are the outcomes we study in Table 3), we find equivalent rejection rates of 7.8% (for the 170 observation regression) and 3.9% for the 34 observation regression. Our conclusion from this placebo exercise is that we will tend to over-reject the null of no-effect when using the 180 observation approach but this will not be a problem when we use the 36 observation sample. If anything, our inference with that sample is likely to be conservative.

[18] We actually do the analysis as the county level so the control county is Hordaland. The population of this county is mostly in Bergen so, for convenience, we refer to the control group as being Bergen.

[19] Note that when using only one control county, we cannot further aggregate to before/after as this would leave us with only 4 cells and the resulting regression would have a perfect fit, making inference impossible.

Akershus to apply for abortions in Oslo—this is because Oslo is located inside Akershus, so the commuting distance is short. As mentioned earlier, we treat Oslo and Akershus as one county for the purpose of the analysis.

5. Data

Our data are from a comprehensive data set of the population of Norway compiled from different administrative registers and census data from Statistics Norway and include information on family background, age, marital status, educational history, and employment. We include cohorts of women born between 1945 and 1954 in our sample; this means that the youngest women are 56 years old when we last observe them in 2010. We can therefore study the effect of abortion access on completed fertility, defined as the number of children they have before age 55, and the timing of their fertility decisions, as well as long run education and labor market outcomes.

In order to link each woman to the county where she grew up, we match the administrative data to the 1960 census. From the 1960 census, we know the municipality in which the woman's mother lived in 1960, at which time our cohorts of women were between 6 and 15 years old. Mobility at any point after we assign location may imply that a teenager we consider as living in Oslo is not actually living in Oslo (or vice versa). This creates a measurement error problem that will tend to bias our estimates of the effects of abortion availability towards zero.

We have information about education from two different registers. One has information on the years of schooling obtained by every individual from 1986 onwards and the other source gives us information about level of schooling. Combining these two datasets we can look at the

likelihood of finishing high school, total years of education and whether the woman has a college or advanced degree (a masters degree or PhD).

From the earnings register, we also have the earnings history of every woman in our sample; hence we can study their labor market outcomes at different stages in life. Following Havnes and Mogstad (2011), we define women as working full time if their earnings were more than 4 times the threshold of the Norwegian Social Insurance Scheme.[20]

Appendix Table 1 provides descriptive statistics for the outcomes in our analysis sample. Teenage fertility is quite high for these cohorts, with about 18% of women having a child before age 20. Completed fertility (number of children by age 55) averages 2.07 per woman with about 12% remaining childless, and the average age at first birth for those who have children is about 24.

We are also able to link these women to their children and examine the outcomes of the first-born children of the treated women. Appendix Table 2 provides descriptive statistics for the first-born children of these women.

6. Effects on Outcomes of Women

Fertility

Since fertility is the primary mechanism through which abortion is likely to affect women, we first examine the effects of having access to abortion as a teenager on later fertility outcomes. Figure 6 illustrates the share of each birth cohort that becomes a teenage mother in Oslo, Bergen, and in the rest of Norway. Teenage fertility rates are very similar in Oslo and Bergen for cohorts up to 1950 and then start to diverge, with the rate starting to fall in Oslo but rising in Bergen.

[20] This value increases every year, in 1990 when the women in the sample were between 36 and 45 years old it was approximately $5,700.

Teenage fertility rates are higher in the rest of the country than in either of the two big cities. However, like in Bergen, they continue to rise after 1950. Figure 6 provides compelling evidence that the expansion of abortion access in Oslo for cohorts born after 1950 led to a relative reduction in teenage fertility rates. Importantly, it also demonstrates that Bergen may be a good choice as a control county for Oslo.

Table 1 provides regression estimates for several fertility-related outcome variables. The first outcome is a dummy variable for whether the woman gave birth as a teenager. The next two outcomes are the number of children that a woman has before age 20 and their completed fertility (births by age 55). The fourth outcome variable is age at first birth and the fifth outcome is a dummy variable for whether the woman was childless at age 55.

The first two rows of the table present estimates using our preferred differences-in-differences method using the rest of Norway as the control group. As expected, the choice between aggregating to 36 or 180 groups has very little impact on the coefficient estimates but does affect the size of the t-statistics. However, for these fertility outcomes, the level of aggregation has no affect on which coefficients are statistically significant.

Access to abortion as a teenager reduces the likelihood of becoming a teenage mother. The coefficient of -.03 implies that abortion access reduces the probability by 3 percentage points. This is a substantial effect, given that the baseline teenage fertility rate is about 18%. Abortion access also causes these women to postpone when they have their first child by almost a half year and reduces the total number of children before age 20 by about .03.

Interestingly, there is no evidence of a negative effect of teenage abortion access on completed fertility; if anything, completed family size is higher by about .06 for women who had abortion access. These findings imply that abortion access causes women to postpone fertility rather than reduce it. This is consistent with the fact that the abortion access is at a sufficiently

young age that a lack of access does not prevent most women from attaining their desired family size. The effect on the probability of remaining childless is small and statistically insignificant.

Row 3 reports differences-in-differences estimates using only Bergen as the control county. The results are generally similar to those using the rest of Norway as the control. The only substantive difference is that we find a positive effect of abortion access as a teenager on the probability of remaining childless. This finding is consistent with the idea that abortion access allows women who have a desired family size of zero to attain that goal. The results for teenage fertility and age at first birth are robust across all specifications.

Education

Table 2 presents the effect of abortion access on education outcomes including the likelihood of finishing high school, years of education, and the probability the women have obtained a college or advanced degree. Once again, the estimates using the rest of Norway as the control group are in the first two rows. We do not find any effect on the likelihood of finishing high school or on years of education, but we do find a positive effect on obtaining a college degree that is about 1.8% and statistically significant at the 5% level. This is substantial, given the baseline probability of achieving a college degree is only 24% (Appendix Table 1). We find a smaller positive effect (also significant at the 5% level) on obtaining an advanced degree of about .8 of a percentage point. Given that only about 3% of these cohorts obtain a higher degree, this is a large effect, but the confidence interval is quite large. As with the fertility outcomes, the findings using Bergen as the control county are similar. We conclude that abortion access led to increases in educational investments for these women.

Labor Market Outcomes

Given that we observe a delay in childbearing and increased education for those women who obtained access to abortion as a teenager, we next turn to how abortion access affected labor market outcomes of these women. We have information about labor market variables from the age of 25 until the women are 55 years old; hence we can study their labor market attachment at different stages in life. We first study the probability that the woman is employed full-time. Rather than choose particular ages to study, we run separate regressions for each age and plot the estimates and confidence intervals in Figure 7. This provides an almost complete picture of the effects of abortion access on labor market participation over the life-cycle. We see that abortion access leads to higher attachment until about age 35 with the magnitude being about 3% in the late 20s and early 30s. However, affected women have lower attachment in their late 30s and during their 40s with the size of the effect being about 2 or 3%.

This result is consistent with the idea that women who had abortion access delayed their fertility and was more attached to the labor market before age 35. However, because they delay childbearing, they were more likely to have young children in their late 30s and 40s and, so, are less likely to work full-time. The effect sizes get smaller as women get towards their 50s but remain about 2% and statistically significant up to age 55.

We have also studied the effect on log income among those women that have income greater than zero. In figure 8, we see a life-cycle progression that is somewhat similar to that for participation. Women who had abortion access have higher earnings in their mid 20s but have lower earnings subsequently. The earnings differences are rarely statistically significant until women reach between about 35 and 45 at which point there is a persistent negative effect of about 5%. After age 45 the earnings penalty to early abortion access gets smaller and generally becomes statistically insignificant.

7. Intergenerational Effects

A key advantage of our data is that we are able to identify the children of the affected cohorts of women and examine how the ability to delay fertility affected their first-born children.[21] As before, we study mothers born between 1945 and 1954. In order to improve the precision of our estimates, we pool daughters and sons in the analysis.[22] The first-born children we study are born between 1960 and 2008, with the majority being born between 1965 and 1980. Because we have data up to 2010, we are able to study longer term outcomes, like labor market outcomes at the age of 30 as well as completed education, for the vast majority of first-born children. The results are in Table 3.[23]

Teen Fertility

The first outcome we study is whether the child becomes a teen mother herself (available only for girls). We study this for all first-born daughters born before 1990 (99% of the total sample of first-born girls). While the coefficients are negative, they are not statistically significant so there is little evidence for an effect of abortion availability on teenage fertility in the next generation.

Education

We next examine various educational outcomes for first-born children. The Norwegian high school system is divided into two main tracks; an academic track that leads to higher education and a vocational track. Typically the vocational track is comprised of two years of schooling followed by two years of apprenticeship and a test. We define students who either

[21] We restrict our sample to first-born children both because many women have only one child and because of the large birth order effects that have been found in Norway (Black, Devereux, Salvanes, 2005).
[22] When we estimate separately for sons and daughters, we cannot reject that the coefficient estimates are the same.
[23] We have also tried augmenting the intergenerational specification by including additional controls for year of birth of the child and completed family size, as these are likely to have independent effects on outcomes, and found similar estimates. We prefer the specification without these controls as both of them are likely to be affected by abortion access.

complete the academic high school track or obtain a certificate of completed apprenticeship to have obtained a high school degree. Because we measure this at age 21, we drop children born after 1989 from the sample (we lose 0.8% of first-borns). As we see in the second column of Table 4, there is no evidence for any effect on high school graduation. However, children of mothers who had access to abortion are about 2% more likely to take the academic track rather than the vocational track

When looking at the effect on having a college degree we drop children born after 1985 from the sample (2.8% of first-borns). We also have information about everyone who has entered into college and use this information to construct an indicator variable that is equal to one if the child has some college. This enables us to include some extra cohorts -- we drop children born after 1988 when we look at the likelihood of having some college (1.2% of first-borns). For both these outcomes, we see little evidence for a positive effect of maternal abortion availability on education of their children.

Labor Market Outcomes

Unfortunately, we are not able to look at the income profile for these children during their entire working life, but we have information at age 30 for the majority of first-borns; for this analysis we include all first-born children born in 1980 or earlier (90.4% of all first-borns). We find that children whose mothers had early access to abortion are about 1% more likely to be employed at age 30. However, the estimates provide little evidence of any effect on full-time employment. There is also no evidence of any effect on earnings at age 30.

Welfare Use

Using register data we can also look at the probability that the child is on welfare. These data are available from 1992 until 2010. We construct an indicator variable indicating if the child has received welfare at any point between 1992 and 2010. Children born after 1990 are dropped

from the sample. We find a negative effect on the likelihood of receiving welfare payments of about 3%.

Alternative specification for intergenerational effect: Selection

A key contribution of our research is our ability to follow the outcomes of the children of women who had access to abortion as a teenager; because of data limitations, the existing research has focused instead on the likely outcomes of the marginal children who are not born to teenagers as a result of abortion access. To provide a comparison, we can also replicate the methodology used earlier—in this case, we are looking at the cohorts of children born when access to abortion was introduced. We are thereby comparing the outcomes for those born before and after abortion was available along the lines done in the previous US studies (Ananat, Gruber, Levine, and Staiger, 2009, Ananat and Hungerman. 2012). This means in our setting that we do the results by the year of birth of the child instead of the cohort of the mother. More precisely, children born in 1969 or after to teenage Oslo mothers are treated, while those born to teenage mothers before that or in counties other than Oslo are not treated.

In Table 4, we present the result. Irrespective of specification, we find evidence of lower educational attainment for children born to teenage mothers who had access to abortion. This is most likely a selection effect – the teen births that would have occurred in the absence of abortion may have been ones that would have resulted in relatively highly educated children. This is the opposite of what Ananat et al. (2009) found in the U.S. but is plausible as abortion availability may have caused more able young women to postpone fertility and invest more in education.

8. Robustness Checks

Intensity of Treatment

While we find evidence that access to abortion at age 18 affected the long-run outcomes of affected women and their children, we are not using all the information available to us; while some women were 18 when they received access to abortion, others were 16 when they received access. To take advantage of this additional information, we allow coefficients to vary depending on the intensity of treatment—we would expect larger effects for those individuals who had more years of access as a teenager. As a result, we would expect to find larger effects for the 1952, 1953, and 1954 cohorts than for the 1951 cohort. To assess this "intensity of treatment effect", we redo the regressions adding an additional explanatory variable which is zero for the control group and for Oslo cohorts born before 1951, 1 for the 1951 Oslo cohort, 2 for the 1952 Oslo Cohort, 3 for the 1953 Oslo cohort, and 4 for the 1954 Oslo cohort. The coefficient on the treatment dummy tells us whether there was a jump for the 1951 cohort, and the coefficient on the newly added variable tells us whether the effect of the treatment increases for later cohorts.

The estimates for the fertility variables are in Table 5. We don't learn a lot from these results as both the main effects and the intensity of treatment effects are often statistically insignificant. One exception is that both the main effect and intensity of treatment effects are positive for age at first birth. [24] Unfortunately, the estimates suggest that we have limited power to move beyond the simple differences-in-differences specification used earlier.

Education Reform

During this period, Norway experienced an education reform, where mandatory years of schooling were increased from 7 to 9 years.[25] The reform was implemented in different

[24] We do not report estimates for the intensity analysis for the educational and intergenerational analysis as the coefficients are generally insignificant.
[25] See Black, Devereux, and Salvanes (2008) for more details about this reform.

municipalities at different times and, in most municipalities, the first cohorts affected by the reform were born between 1947 and 1956. In Oslo, the first cohort affected by the reform was the 1953 cohort. In contrast, Bergen adopted the reform early so that the first cohorts affected there were born in 1947. Other municipalities implemented the reform at other times.

We have done a number of things to check whether the education reform is confounding our results. The first is that we have re-estimated the difference-in-differences estimates after dropping women born in 1953 or after. This ensures that none of the Oslo women were affected by the reform. As can be seen in Appendix Table 3, the estimates are similar to those using the full set of cohorts.

As a second approach, we have re-estimated the specifications with an additional control variable for whether the woman was subject to the reform (this differs by municipality and cohort). Once again, we find estimates that are almost identical to those without this additional control.[26]

Pre-Treatment Trends

One threat to the validity of the differences-in-differences estimator is that the pre-treatment trends may have been different in Oslo compared to the rest of Norway. The fact that our estimates are generally similar when we use Bergen as the control group is reassuring. However, we have tested directly for this possibility by allowing the trend to differ between Oslo and the rest of Norway for each dependent variable. We only find statistically significant differences for one variable – income at age 30. This suggests that diverging pre-treatment trends are not an important source of bias.

[26] For parsimony, we do not report these estimates.

As a final check, we use the synthetic control method of creating an artificial control group that closely proxies the trends in Oslo prior to increased abortion access. The procedure of constructing a synthetic control group is introduced in Abadie and Gardeazabal (2003) and was furthered developed and formalized in Abadie, Diamond, and Hainmueller (2010).

The idea of the synthetic control approach is that a weighted average of the 17 other counties may be a better control for Oslo than any one particular county. This weighted average is chosen so that the pre-treatment behavior of the outcome variable in the synthetic control matches that of Oslo as closely as possible. The estimator involves first calculating the time-invariant weight for each county and, then, comparing the outcome variable for Oslo in each year to that from the weighted average of the other counties (the synthetic Oslo).

Given that the weights are county- and outcome-specific, we first take the means of all variables by county and cohort. This reduces the dimension of the dataset to 180 observations (18 counties and 10 cohorts). To make sure that we match on trends rather than levels, we take the deviation of the outcome variable from the value of that variable in that county for the 1950 cohort (the last pre-treatment cohort) and we construct weights to match the pre-treatment values of this demeaned variable. We use lagged values of the demeaned outcome variable as predictor variables (for cohorts 1945-1949) so that the weights are chosen to match the pre-treatment values of the (demeaned) outcome variable for Synthetic Oslo as closely as possible with Oslo. Once we have the time-invariant county weights, we aggregate to create our Synthetic Oslo for each year.

Appendix Figure 2A and appendix Figure 3A include pictures of various outcomes by cohort for Oslo and its synthetic control.

9. Conclusions

Starting in 1969, teenagers in Oslo had access to abortion services that were generally denied to teenagers in other parts of Norway. Mapping into birth cohorts, this implies that cohorts born between 1951 and 1954 had much better access as teenagers in Oslo than in the rest of the country. We find that this translated into lower rates of teen fertility and a higher age at first birth but had no negative effect on completed family size. We find evidence that greater abortion access led to higher educational attainment. We also find interesting effects on labor market attachment with teen abortion access leading to higher attachment at younger ages but lower attachment after about age 35. These findings are consistent with abortion leading to a delay in fertility that increases labor market attachment at younger ages but decreased it later. Finally, we study the effects on the first-born offspring of the affected cohorts and find that early abortion access led to lower teen pregnancy rates and welfare use in the next generation. It is also associated with a more academic high school track and increased college-going. So, there appear to be positive spillover effects to the next generation. Interestingly, these positive intergenerational effects occur despite evidence for a negative selection effect – the teenage births that occur when there is abortion access lead to children with lower educational attainment.

The interpretation of these empirical findings is quite complicated. All the cohorts we study had access to the contraceptive pill once they reached age 20. They also all had reasonable access to abortion after 1972 so even the pre-treatment cohorts we study had access to abortion when they were in their 20s. The fundamental effect of the treatment was to provide certain Oslo cohorts with access to abortion as teenagers at a time when other Norwegians the same age had very limited access. So, our estimates should be interpreted as the effects of teenage access to fertility control for a group of women who has access to this later on in their 20s and subsequently. Our finding of effects on educational and labor market variables are consistent with

those of Bailey (2006) who finds similar effects from teenage access to the pill in the U.S. in a context where all women have access to the pill once they turn 21.

Our findings of positive effects on the next generation may arise for many reasons. A plausible interpretation is that these reflect the fact that abortion access allows women greater freedom to time births – a woman who had abortion access is more likely to have her first child at her desired age-at-first-birth. So, it is more likely that she is prepared for the child and in a good position to invest in the human capital of the child. Also, it is more likely that the mother, herself, has invested in her own human capital and so may be better able to transmit human capital to the child. While we have identified these intergenerational effects, further work will be required to determine the exact channels through which they occur.

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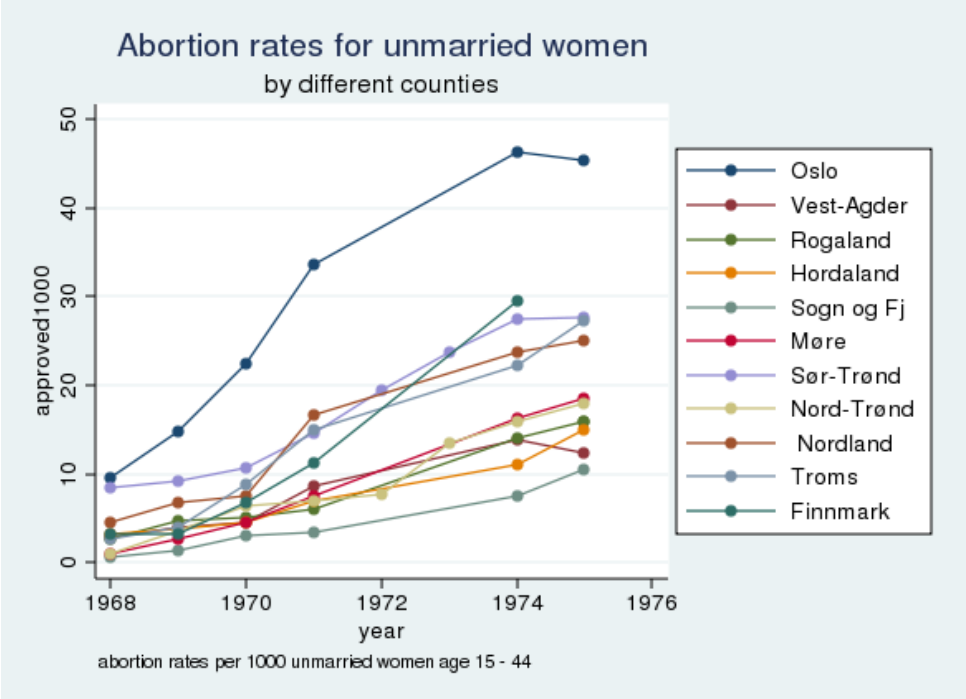
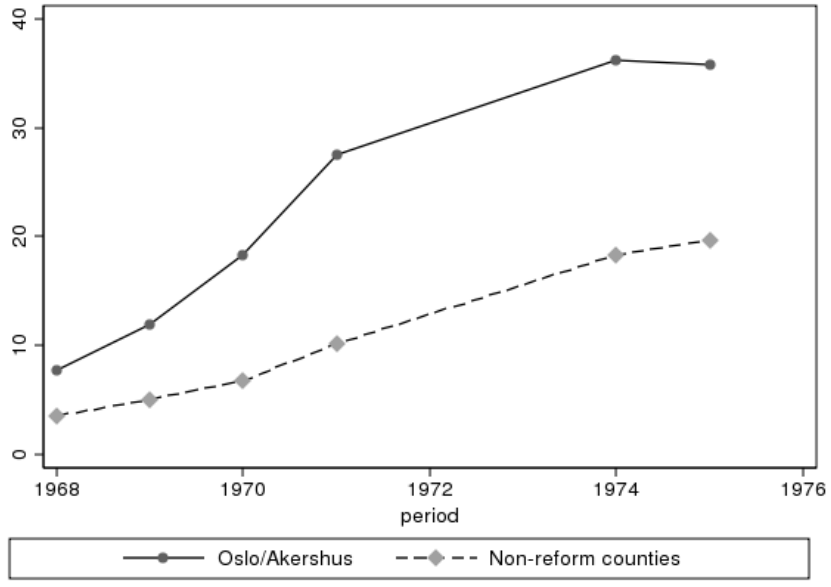
Figure 1: Abortions per 1000 unmarried women in different counties



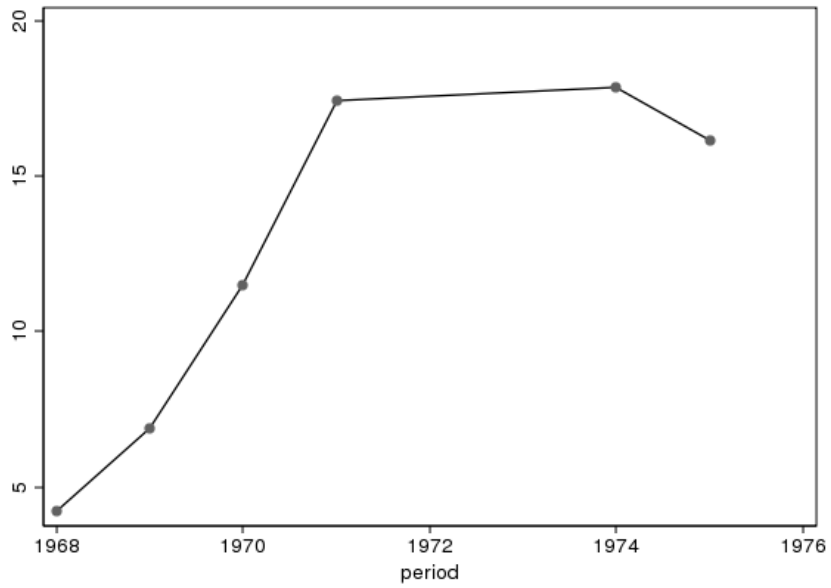
Notes: The figure plots apportion per 1000 unmarried women for selected counties in Norway. Source: Achieves after the Norwegian Directorate of Health, office for psychiatry.

Figure 2: Abortion rates for unmarried women in Oslo/Akershus versus the rest of Norway



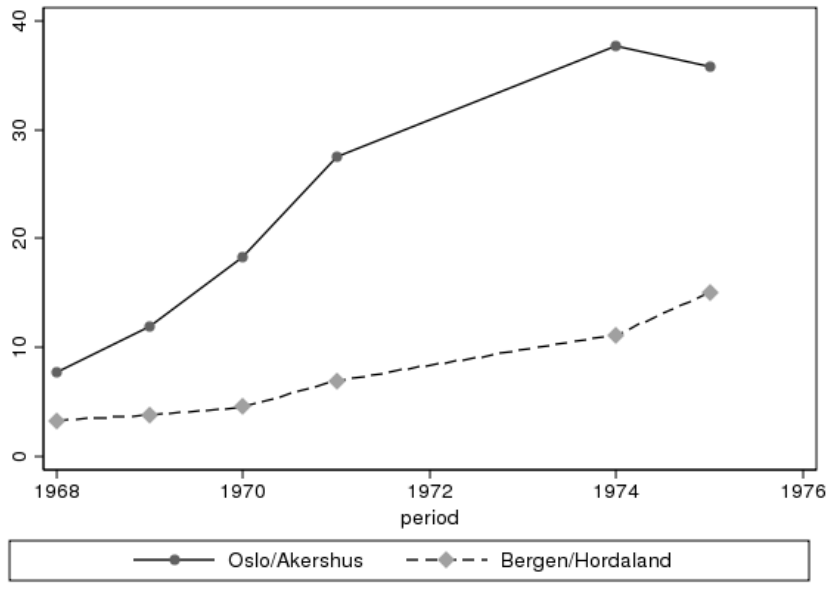
Note: The figure plots abortion rates for unmarried women in Oslo/Akershus versus the rest of Norway in the period 1968-1975. Source: as for figure 1.

Figure 3: Difference in abortion rates for unmarried women in Oslo/Akershus and the rest of Norway



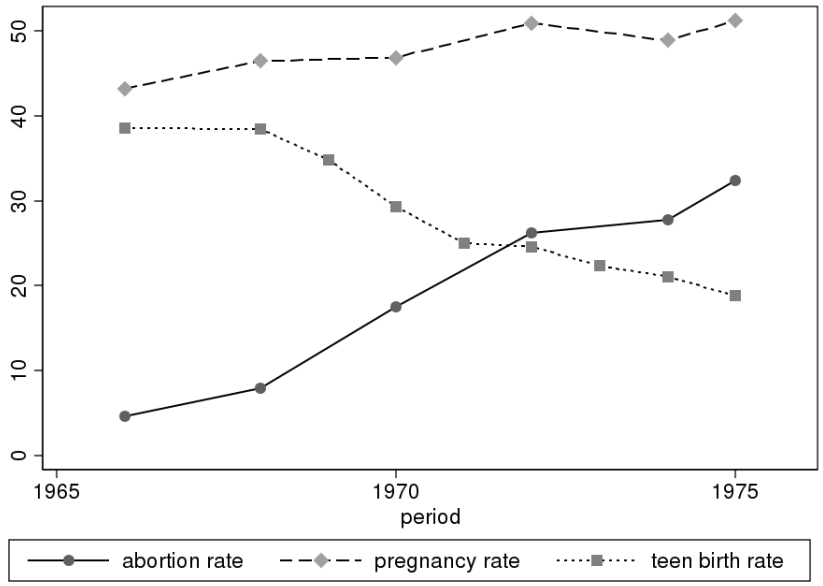
Note: The figure plots the difference in abortion rates for unmarried women in Oslo/Akershus versus the rest of Norway in the period 1968-1975. Source: as for figure 1.

Figure 4: Abortion rates for unmarried women in Oslo/Akershus and Bergen/Hordaland



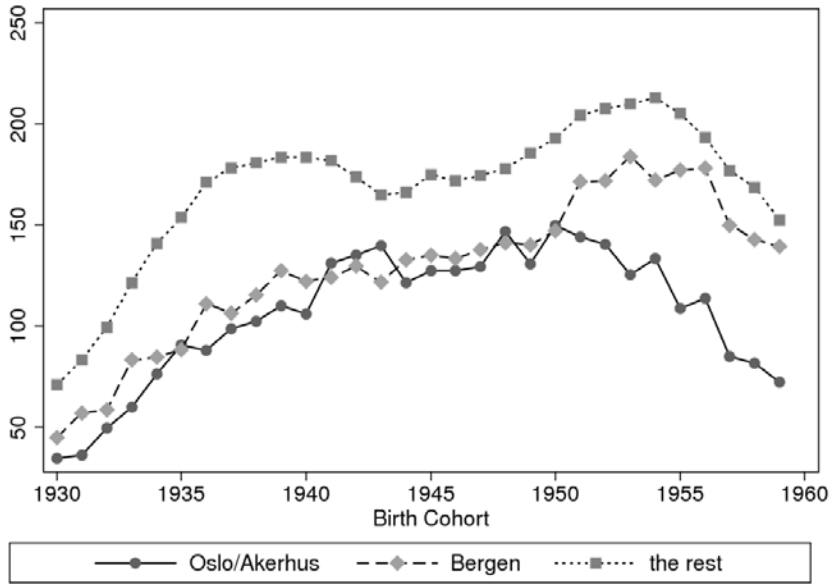
Note: The figure plots the difference in abortion rates for unmarried women in Oslo/Akershus versus Bergen/Hordaland in the period 1968-1975. Source: as for figure 1.

Figure 5: Teen birth, abortion, and pregnancy rates in Oslo



Notes: The figure plots teen birth, abortion and pregnancy rates in Oslo in the period 1966-1975.

Figure 6: Teen birth rate, by cohort

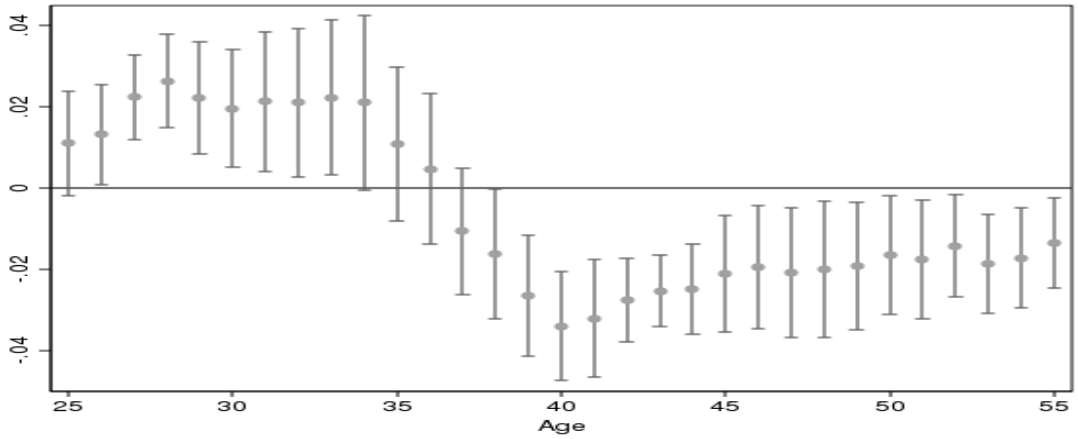


Note: The figure plots the rate of women in each birth cohorts who becomes teenage mothers in Oslo/Akershus, Bergen and the rest of Norway. The teen birth rate is plotted by cohorts born in 1930-1959.

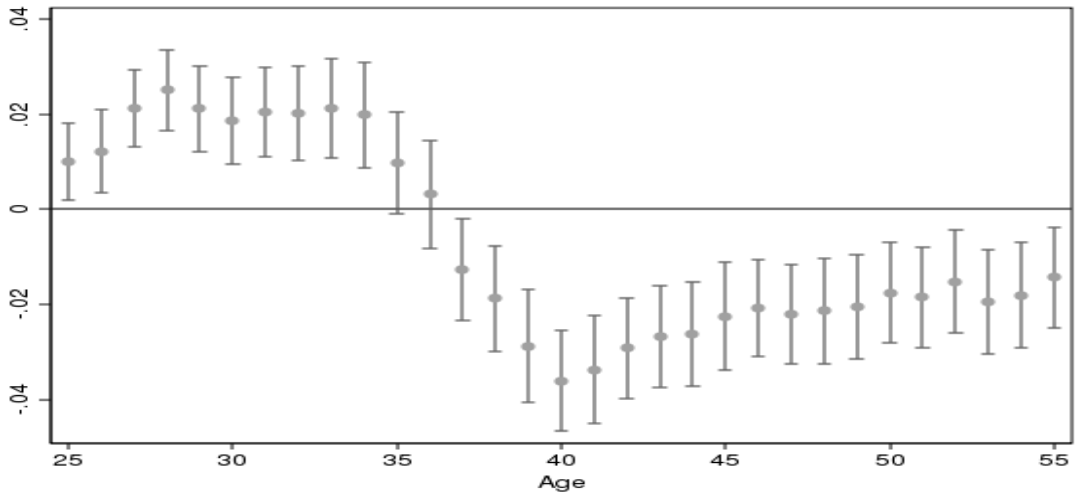
Figure 7

Effects of Abortion Access on whether Full time employed at various ages

Using all other counties in Norway as control, 36 observations



Using all other counties in Norway as control, 180 observations



Using Hordaland/Bergen as control, 20 observations

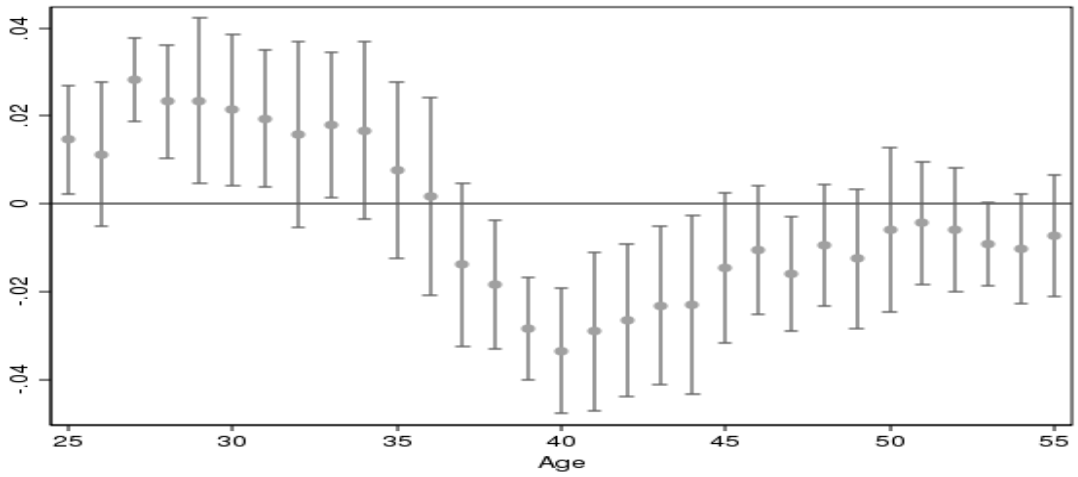
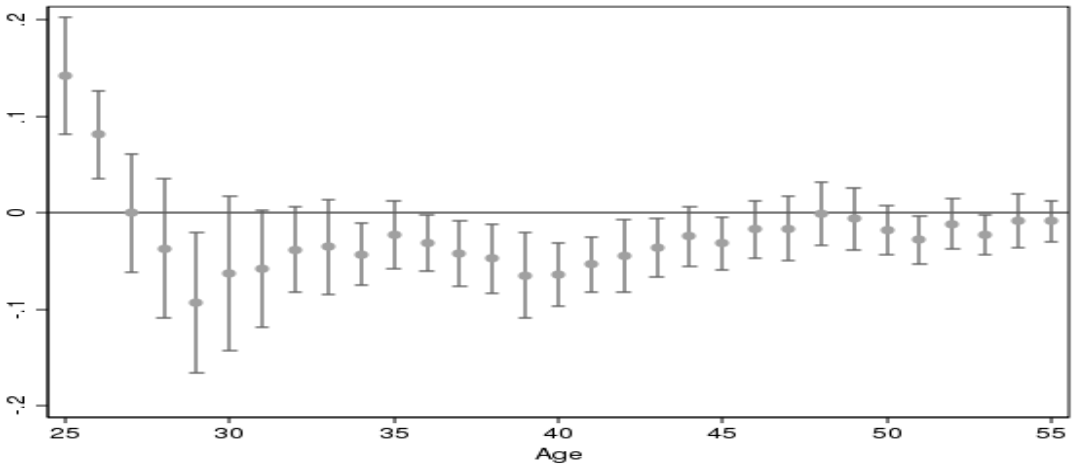
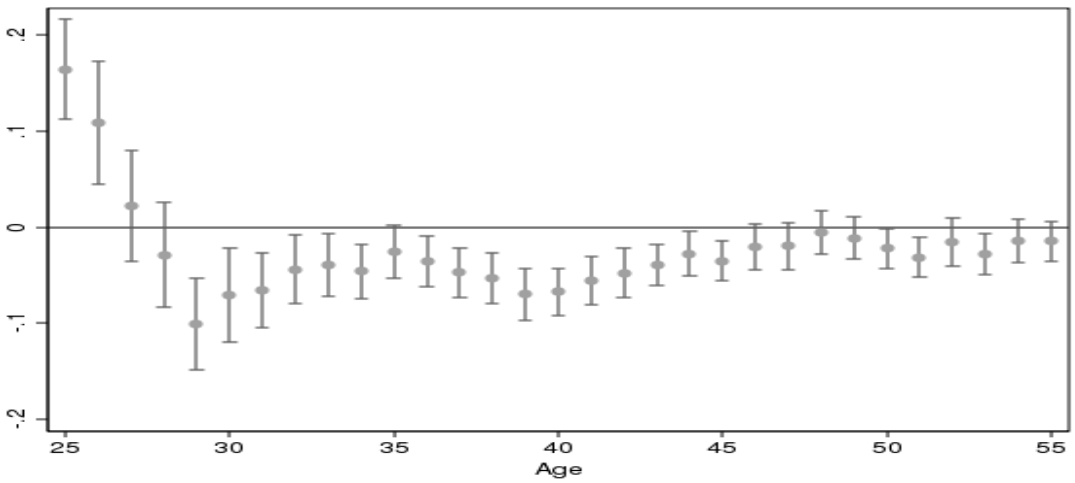


Figure 8
Effect of Abortion Access on Log (income) at various ages

Using all other counties in Norway as control, 36 observations



Using all other counties in Norway as control, 180 observations



Using Hordaland/Bergen as control, 20 observations

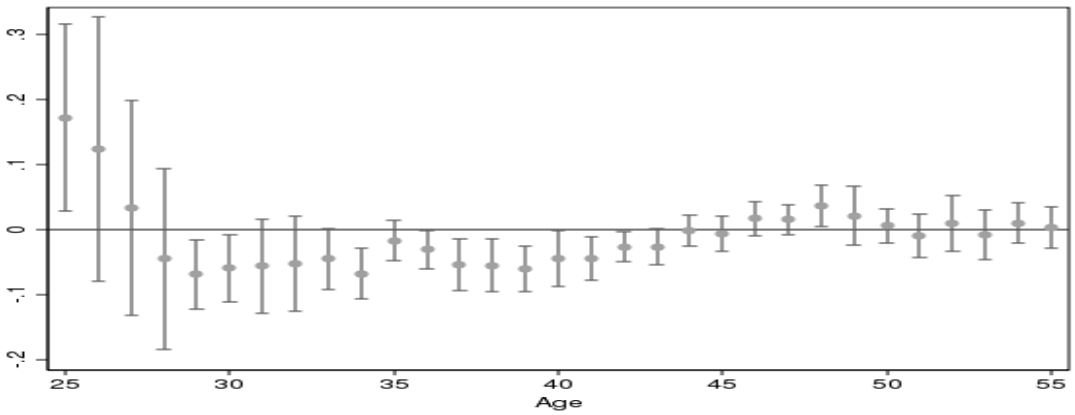


Table 1: Fertility Outcomes-Multiple Specifications
Effects of Early Abortion Access on Women's Outcomes

	Teen Mother	# of Children before 20	# of Children before 55	Age 1st birth	Childless
Control Group					
All Other Counties in Norway	-0.029**	-0.032**	0.062**	0.478**	0.005
	0.009	0.009	0.028	0.077	0.006
	-3.405	-3.440	2.245	6.174	0.833
	36	36	36	36	36
All Other Counties in Norway	-0.030**	-0.032**	0.065**	0.475**	0.005
	0.005	0.006	0.016	0.054	0.004
	-6.205	-5.787	4.158	8.857	1.251
	180	180	180	180	180
Bergen	-0.035**	-0.038**	0.004	0.580**	0.014**
	0.006	0.008	0.022	0.084	0.004
	-5.502	-4.727	0.172	6.937	3.696
	20	20	20	20	20

Standard errors are calculated using Donald and Lang standard errors. Each coefficient is from a different regression. Each regression uses aggregated observations formed from the population data; sample sizes are reflected in Appendix Table 1. The other variables included in the regressions are county dummies and birth cohort dummies. * $p < 0.10$, ** $p < 0.05$.

Table 2: Education Outcomes-Multiple Specifications
Effects of Early Abortion Access on Women's Outcomes

	Completed high school	Years of education	College degree	Advanced degree
Control Group	b/se/t	b/se/t	b/se/t	b/se/t
All Other Counties in Norway	-0.003	0.021	0.019**	0.009**
	0.008	0.055	0.009	0.002
	-0.404	0.376	2.134	4.967
	36	36	36	36
All Other Counties in Norway	-0.005	0.009	0.018**	0.008**
	0.005	0.031	0.005	0.002
	-0.906	0.303	3.733	5.447
	180	180	180	180
Bergen	-0.007	0.013	0.011*	0.008**
	0.010	0.036	0.005	0.003
	-0.736	0.356	2.204	2.574
	20	20	20	20

Standard errors are calculated using Donald and Lang standard errors. Each coefficient is from a different regression. Each regression uses aggregated observations formed from the population data; sample sizes are reflected in Appendix Table 1. The other variables included in the regressions are county dummies and birth cohort dummies. * $p < 0.10$, ** $p < 0.05$.

Table 3: Effect of Early Access to Abortion on Outcomes of 1st born child-Multiple Specifications

	Teen Mother	Academic track	High school degree	College degree	Started College	Full time employed age 30	Employed age 30	Log income age 30[27]	Welfare Benefits
Control Group	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t
All Other Counties in Norway	-0.005	0.017**	-0.012	0.003	0.004	0.002	0.012**	0.001	-0.030**
	0.006	0.007	0.008	0.008	0.008	0.006	0.005	0.011	0.006
	-0.930	2.529	-1.451	0.422	0.481	0.274	2.449	0.127	-4.905
	36	36	36	36	36	36	36	36	36
All Other Counties in Norway	-0.005	0.018**	-0.013**	0.003	0.003	0.002	0.012**	-0.000	-0.031**
	0.004	0.006	0.006	0.006	0.006	0.005	0.004	0.010	0.004
	-1.196	2.944	-2.247	0.422	0.524	0.405	2.765	-0.036	-6.880
	180	180	180	180	180	180	180	180	180
Bergen	-0.011	0.021**	-0.009	0.014*	0.012	0.015**	0.023**	0.019	-0.034**
	0.005	0.008	0.006	0.007	0.007	0.005	0.007	0.012	0.005
	-2.199	2.674	-1.436	2.016	1.766	2.948	3.271	1.661	-6.349
	20	20	20	20	20	20	20	20	20

Standard errors are calculated using Donald and Lang standard errors. Each coefficient is from a different regression. Each regression uses aggregated observations formed from the population data; sample sizes are reflected in Appendix Table 2. The other variables included in the regressions are county dummies and birth cohort dummies.

All regressions pool boys and girls except the Teen Mother regression which only includes girls.

* $p < 0.10$, ** $p < 0.0$

[27] Conditional on having income greater than zero

Table 4: Outcomes for sample all children born between 1963 and 1972 for teenage mothers after abortion available

	Academic track	High school degree	College degree	Started College	Full time employed age 30	Employed age 30	Log income age 30[28]	Welfare Benefits
Control Group	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t
All Other Counties in Norway	0.025	-0.047**	-0.021*	-0.014	0.010	0.011	0.015	-0.014
	0.022	0.015	0.012	0.014	0.014	0.012	0.024	0.017
	1.146	-3.133	-1.752	-0.957	0.684	0.880	0.621	-0.872
	36	36	36	36	36	36	36	36
All Other Counties in Norway	0.026	-0.046**	-0.021*	-0.014	0.010	0.012	0.019	-0.013
	0.017	0.014	0.012	0.013	0.013	0.011	0.025	0.015
	1.490	-3.227	-1.780	-1.063	0.722	1.096	0.745	-0.879
	180	180	180	180	180	180	180	180
Bergen	-0.004	-0.053**	-0.033*	-0.028	0.020	0.024	0.035	-0.037
	0.012	0.014	0.018	0.021	0.017	0.017	0.036	0.024
	-0.318	-3.761	-1.860	-1.334	1.199	1.413	0.969	-1.523
	20	20	20	20	20	20	20	20

Standard errors are calculated using Donald and Lang standard errors. Each coefficient is from a different regression. Each regression uses aggregated observations formed from the population data. 2. The other variables included in the regressions are county dummies and birth cohort dummies. All regressions pool boys and girls

* $p < 0.10$, ** $p < 0.0$

[28] Conditional on having income greater than zero

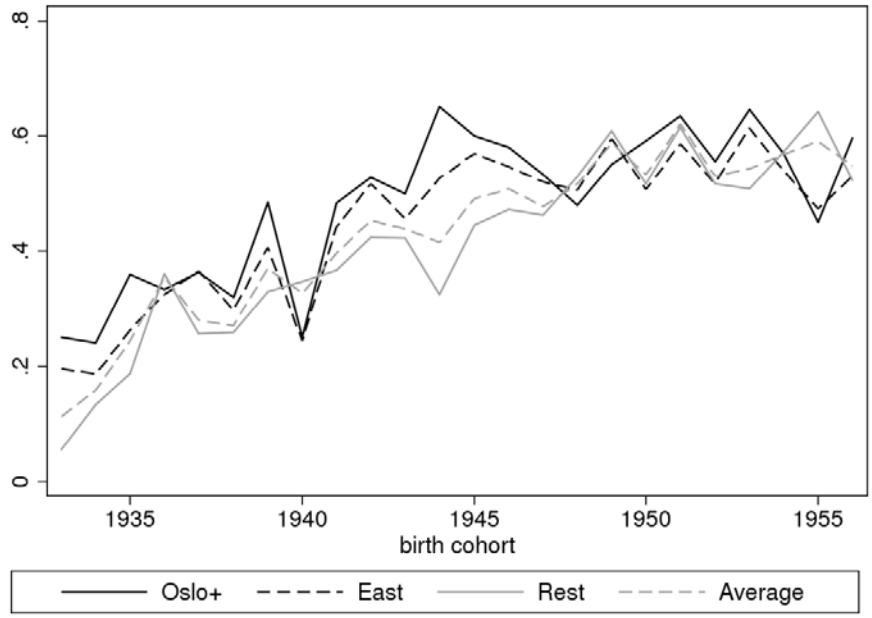
Table 5: Treatment Intensity
Estimates using the Rest of Norway as the Control Group for Oslo

Fertility Outcomes

	Teen Mother	Children before 20	Children before 55	Age 1st birth	Childless
	-0.007*	-0.007	0.029**	0.090**	-0.003
Treated intensity	0.004	0.004	0.012	0.040	0.003
	-1.925	-1.721	2.483	2.229	-1.131
	-0.011	-0.013	-0.011	0.248**	0.013
Treated dummy	0.011	0.013	0.034	0.117	0.008
	-0.988	-1.010	-0.329	2.132	1.541
	90	90	90	90	90
Observations					
Treated intensity	-0.007**	-0.007*	0.029**	0.090**	-0.003
	0.003	0.004	0.011	0.037	0.003
	-2.187	-1.929	2.709	2.403	-1.247
Treated dummy	-0.011	-0.013	-0.009	0.248**	0.013
	0.010	0.011	0.031	0.108	0.007
	-1.160	-1.168	-0.291	2.296	1.700
Observations	180	180	180	180	180
BERGEN					
Treated intensity	-0.006	-0.006	0.026*	0.085	-0.000
	0.004	0.006	0.013	0.053	0.003
	-1.483	-1.131	1.985	1.609	-0.068
Treated dummy	-0.020	-0.023	-0.062	0.367**	0.014
	0.012	0.016	0.038	0.153	0.008
	-1.666	-1.425	-1.619	2.404	1.788
	20	20	20	20	20

Notes: All estimation carried out as detailed in the text.

Appendix: Figure 1A: Share Ever Used the Pill, by Birth Cohort, Region of Living, and Age

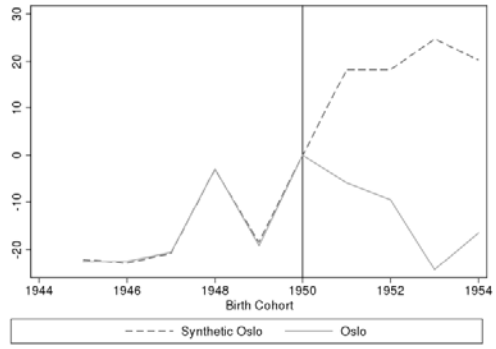


Notes: The figure plots the share ever having used the pill in the different regions across women born in 1933-1956. The regions are grouped in the following way: *Oslo+* is women growing up in urban areas of east Norway. *East* includes all women growing up in the eastern part of Norway, including the Oslo+ citizens. *Average* includes all women and *Rest* is all women not living in Oslo+.
Source: These data follows from the fertility survey conducted in Norway in 1977.[29]

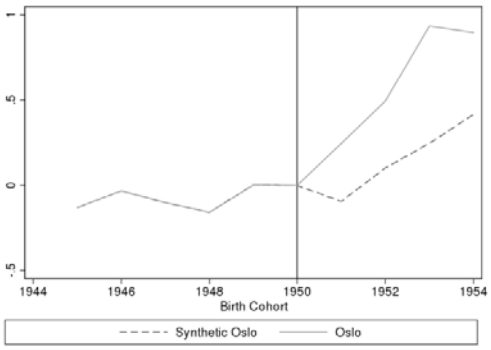
[29] The data are prepared and made available in anonymized from by Norwegian social science data services (NSD). Neither Statistics Norway nor NSD are responsible for the interpretations and analysis made in this paper. For more details about these data please see Noack and Østby (1981) and Østby (1977)

Figure 2A: Synthetic Control Mothers' Outcome

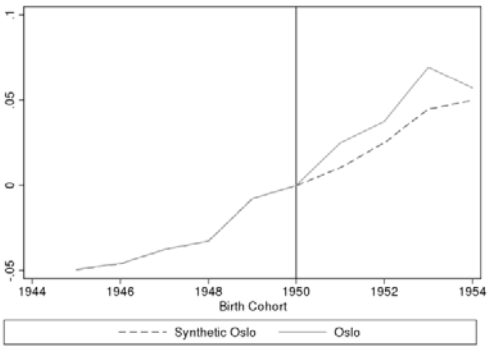
Teen Mother



Age 1st Birth



College Degree



Years of Education

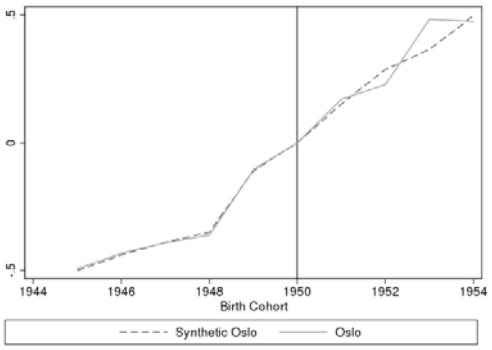
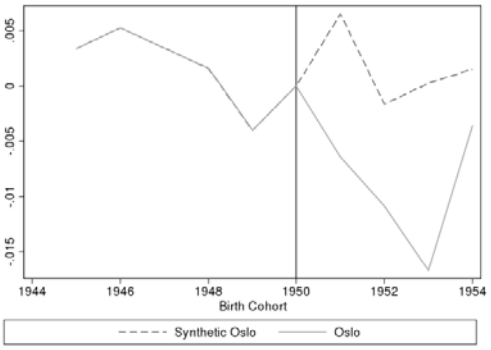
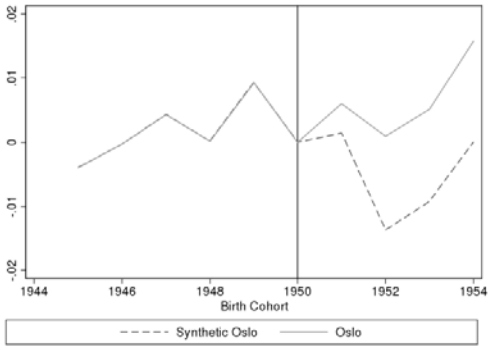


Figure 3A: Synthetic Control Children's Outcomes

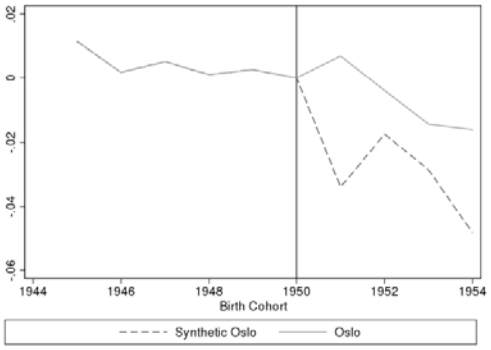
Teenage Mom



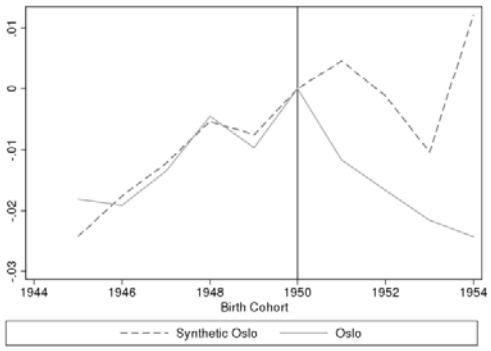
Fulltime Emploued age 30



Academic Track in High School



Welfare Benefits



Notes: The figures plot the demeaned trend in various outcomes by cohort of Oslo and its synthetic control (dotted line). The horizontal axis gives the women's/mothers' year of birth (1945-1954).

Appendix Table 1: Descriptive statistics for Women born 1945-1954

	Total
Children before 20	0.204
[N=296531]	(0.464)
Children before 55	2.074
[N=296531]	(1.158)
Childless	0.115
[N=296531]	(0.319)
Teenage mom	0.178
[N=296531]	(0.383)
Age 1st Birth	23.67
[N=262544]	(4.453)
College Degree	0.240
[N=290107]	(0.427)
Advanced Degree	0.0319
[N=290107]	(0.176)
Years of Education	11.36
[N=290107]	(2.687)
Completed High School	0.426
[N=290107]	(0.495)
Full Time Employed Age 30	0.186
[N=291806]	(0.389)
Full Time Employed Age 40	0.389
[N=291806]	(0.488)
Full Time Employed Age 50	0.501
[N=291806]	(0.500)
Log income30	11.30
[N=284268]	(1.346)
Log income40	11.92
[N=284268]	(0.966)
Log income50	12.25
[N=284268]	(0.884)

mean coefficients; sd in parentheses

Appendix Table 2: Descriptive statistics for First-Born Children of Women born 1945-54

Outcome Variable	Entire Sample	Daughters	Sons
Teenage Fertility		0.0617	
		(0.241)	
Academic track	0.809	0.887	0.729
	(0.393)	(0.316)	(0.445)
High school degree	0.651	0.684	0.619
	(0.477)	(0.465)	(0.486)
College degree	0.453	0.527	0.383
	(0.498)	(0.499)	(0.486)
Started college	0.569	0.636	0.504
	(0.495)	(0.481)	(0.500)
Full time employed age 30	0.637	0.506	0.761
	(0.481)	(0.500)	(0.427)
Employed age 30	0.819	0.770	0.866
	(0.385)	(0.421)	(0.340)
Income age 30	12.45	12.24	12.64
	(0.873)	(0.916)	(0.785)
Welfare benefits	0.200	0.207	0.194
	(0.400)	(0.405)	(0.395)
Observations	260600	126393	134061

mean coefficients; sd in parentheses

Appendix Table 3: Difference-in-differences estimates dropping women born in 1953 and 1954

Fertility Outcomes

	Teen Mother	# of Children before 20	# of Children before 55	Age 1st birth	Childless
Control Group	-0.020**	-0.022**	0.034	0.347**	0.008
All Other Counties in Norway	0.009	0.009	0.027	0.087	0.005
	-2.333	-2.286	1.242	3.968	1.402
	36	36	36	36	36
All Other Counties in Norway	-0.020**	-0.022**	0.036*	0.347**	0.008
	0.006	0.007	0.019	0.065	0.004
	-3.518	-3.297	1.867	5.304	1.730
	144	144	144	144	144
Bergen	-0.025**	-0.027**	-0.025	0.456**	0.015**
	0.005	0.006	0.026	0.067	0.005
	-5.204	-4.332	-0.983	6.850	3.232
	16	16	16	16	16

Appendix Table 3: Difference-in-differences estimates dropping women born in 1953 and 1954 (continued)

Education Outcomes

	Completed high school	Years of education	College degree	Higher college degree
Control Group				
All Other Counties in Norway	-0.008	0.003	0.015	0.008**
	0.008	0.061	0.010	0.002
	-0.909	0.055	1.564	4.003
	36	36	36	36
All Other Counties in Norway	-0.009	-0.005	0.015**	0.008**
	0.007	0.039	0.006	0.002
	-1.302	-0.133	2.425	3.871
	144	144	144	144
Bergen	-0.020*	-0.029	0.007	0.009**
	0.010	0.041	0.004	0.003
	-2.067	-0.709	1.553	2.778
	16	16	16	16

Appendix Table 3: Difference-in-differences estimates dropping women born in 1953 and 1954 (continued)

1st Born Childs Outcomes

	Teenage Mother	Academic track	High school degree	College degree	Started College	Full time employed age 30	Employed age 30	Log income age 30[30]	Welfare Benefits
Control Group	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t	b/se/t
All Other Counties in Norway	-0.007	0.018**	-0.008	0.011	0.008	-0.001	0.016**	0.010	-0.017**
	0.005	0.007	0.008	0.009	0.008	0.007	0.004	0.012	0.006
	-1.326	2.473	-1.079	1.290	1.005	-0.197	4.011	0.810	-2.930
	36	36	36	36	36	36	36	36	36
All Other Counties in Norway	-0.008	0.020**	-0.004	0.016*	0.013*	0.000	0.017**	0.013	-0.019**
	0.005	0.008	0.006	0.007	0.007	0.007	0.005	0.013	0.005
	-1.577	2.554	-0.654	2.106	1.815	0.014	3.163	1.006	-3.644
	144	144	144	144	144	144	144	144	144
Bergen	-0.008	0.024*	-0.013*	0.020	0.019*	0.011	0.023**	0.026	-0.027**
	0.006	0.011	0.006	0.011	0.009	0.008	0.004	0.017	0.007
	-1.379	2.176	-2.159	1.840	2.069	1.475	6.074	1.539	-3.867
	16	16	16	16	16	16	16	16	16

Notes: All estimation carried out as detailed for the equivalent outcomes in Tables 1 to 3.

[30] NB- Conditional on having income greater than zero