The Impact of Home Pregnancy Testing on Fertility and Women's Later-Life Outcomes

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Abstract

Home pregnancy tests give early fertility information and help women make timely family-planning decisions. This paper studies how the introduction of home pregnancy tests in the US in 1977 impacted fertility, early prenatal care, and later-life outcomes. Using county-level drugstore accessibility to approximate test availability, I document significant trend breaks in fertility rates after 1977 among women who had access to drugstores. The effects are the strongest for those aged 15–29 and concentrated among those with access to abortion services. In the long run, women exposed to home pregnancy tests were more likely to delay childbirth, participate in the labor force, and never marry; these women were also less likely to divorce.

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1 Introduction

Women in the US did not have broad access to fast and private pregnancy confirmation—and hence early fertility information—until the Food and Drug Administration (FDA) approved home pregnancy tests at the end of 1977. Now home pregnancy tests are widely used: nearly eight million women in the US used this over-the-counter device in 2020.[1] The fertility information allows women to make timely family-planning decisions, increasing assessing abortion services.[2] In this paper, I investigate whether living close to a drugstore, the primary point of access for home pregnancy tests at the time, affects fertility and women's later-life outcomes.

Little work has been done estimating the impact of home pregnancy testing on fertility or women's outcomes. Indeed, testing might seem relatively unimportant given that it became available only after two other powerful reproductive technologies: birth control pills in 1960 and legalized abortion in 1973. However, by providing timely and accurate information, home pregnancy tests indirectly reduce the costs of abortion. Specifically, early pregnancy confirmation gives women more time to make decisions, making them more likely to meet the gestational limits on abortion and giving them more and safer abortion options.[3] Greater ability to control fertility may allow women to invest in their own human capital, benefiting them in the short run and long run.

In the first part of this paper, I answer two questions: when earlier pregnancy confirmation became more accessible, (1) did fertility rates decline through the resulting increase in access to abortion and (2) did women begin prenatal care earlier? I focus on children born between 1974 and 1984 to women aged 15–39. My event-study identification strategy relies on information about when home pregnancy tests emerged combined with information on geographic access to drugstores.

[1] Based on *"Usage of home pregnancy tests in the U.S. 2020,"* published by Statista Research Department, July 2, 2021. The estimation is calculated using United Nations data and Simmons National Consumer Survey (NHCS).

[2] As showcased in early advertisements, proponents have suggested women could improve their reproductive health by having an abortion or initiating prenatal care earlier (Oakley 1976, Boston Women's Health Book Collective 1984, Leavitt 2006). I discuss the advertising strategies in Appendix C.1.

[3] The Supreme Court in *Roe v. Wade* ruled that states could not unduly restrict women's access to abortions within the first trimester of pregnancy and allowed states to prohibit abortion only after fetal viability. However, both undue restrictions and viability were not defined. Thus, many states still passed laws to regulate abortion after 1973.

I further test the complementarity between access to home pregnancy tests and abortion services directly by incorporating geographic access to abortion and state-level parental-consent variation in my estimation.

I find that the availability of home pregnancy tests reduced fertility rates but did not change prenatal-care timing. Specifically, I find that for women aged 15–29 with access to drugstores, prior to 1978, fertility rates were increasing, but, starting in 1978, these upward rates immediately plateaued. I document that the changes in trends only occurred among women living in counties with greater access to both drugstores and abortion providers. Furthermore, while providing minors with abortion access reduced fertility rates by 8%, minors with access to both abortion and home pregnancy tests experienced an additional 5% decline. That is, access to both early detection and abortion services was responsible for the decline in fertility rates.

In the second part of this paper, I investigate the long-term impacts of home pregnancy testing on delaying childbirth and women's later-life outcomes. Relying on variations in geographic access to drugstores and abortion providers, I compare cohorts with different levels of exposure to home pregnancy tests. For delaying childbirth, I examine fertility rates for three stages of reproductive years. For educational outcomes, I examine the impact of combined access to abortion providers and drugstores on high school dropout and college. For labor market outcomes, I focus on labor force participation and current employment status. Finally, I estimate the impact on women's marital outcomes, including never married, currently married, and currently divorced.

My estimation shows that, after the introduction of home pregnancy tests, women were more likely to delay childbirth. Specifically, I find that the combined access to drugstores and abortion providers had no effects on fertility rates for women aged 20–26 and 27–33. However, it increased fertility rates by 6% and lowered birth orders by 6% between ages 34 and 40 among women who turned 16 after home pregnancy tests became available. For women's later-life outcomes, I find that labor force participation increased significantly by 0.43% among women with early access to home pregnancy tests in areas with access to abortion and drugstores; these women were also 4% less likely to be currently divorced and 3% more likely to be never married. The effects on marital outcomes can be explained by better marriage-matching quality.

This paper contributes to an expansive body of research on information shocks. Researchers have found that providing (accurate) information leads to behavioral changes in educational decisions and choices (e.g., Jensen (2010), Wiswall & Zafar (2015), Andrabi et al. (2017)), consumption behaviors (e.g., Garmaise & Moskowitz (2004), Aker (2010), Allcott (2011)), sexual and reproductive behavior (e.g., Dupas (2011)), job searching and satisfaction (e.g., Card et al. (2012)), among other things. My findings highlight the importance of access to information in family-planning decisions. The extent to which new home testing technologies provide timely and accurate information increases the uptake of another technology: abortion services.

In addition, this paper complements the literature on reproductive technologies and their consequences. Previous literature has found positive long-term impacts of access to birth control pills (e.g., Goldin & Katz (2002), Bailey (2006), Hock et al. (2007), Pantano (2007), Ananat & Hungerman (2012)) and access to legal abortion (e.g., Gruber et al. (1999), Donohue III & Levitt (2001), Ananat et al. (2007), Foote & Goetz (2008), Ananat et al. (2009), Mølland (2016)) for both women and their offspring.[4] By delaying childbirth and marriage, women complete more schooling and are less likely to receive welfare and be single parents. In this paper, I show that, when complemented by abortion services, access to home pregnancy tests makes women more likely to delay pregnancies and thus has some of the same consequences for women's later-life outcomes.

This paper also relates to the literature on home testing. Existing studies on home pregnancy testing have mainly focused on its immediate impacts, including its uptake, its accuracy, and follow-up actions (Valanis & Perlman 1982, Shew et al. 2000, Estes et al. 2008). My findings suggest the importance of the complementarity between technologies. Moreover, in addition to convenience, these tests have significant implications for when and how people access health care. In the pregnancy-confirmation context, privacy and the timing of testing are crucial.

Finally, my empirical results provide generalizable insights that are policy-relevant. Previous work has relied on randomized controlled trials (RCTs) to examine the short-run consequences of early pregnancy recognition. It finds that early preg-

[4]Although recent studies by Myers (2017) and Myers (2022) find that most long-term consequences were results of legal and confidential access to abortion and not access to the pill, it is still consistent with previous studies that women and their children were better off because mothers were more likely to delay childbearing and marriage.

nancy confirmation reduces late-term abortions and late prenatal care (Morroni & Moodley 2006, Andersen et al. 2013, Comfort et al. 2019). These RCTs do not necessarily generalize to the population at large, though.[5] By contrast, this paper has two significant implications for how access to fertility information complements abortion services at the population level. First, for at-risk populations and areas that have not yet seen wide access to pregnancy confirmation, providing early pregnancy confirmation is a low-cost method with immediate and long-term consequences. Second, this complementarity allows women to delay fertility. Delayed childbirth may potentially increase the age at first marriage and improve the quality of marriage matches. These results suggest that efforts to reduce access to reproductive services are likely to harm women's later-life circumstances, generating long-term costs.

2 How Did Home Pregnancy Tests Arrive in Women's Homes?

Although pregnancy tests have been available at clinics and hospitals since the '60s, many women did not use the tests. Women still diagnosed pregnancies by relying on morning-sickness symptoms and experiences before the introduction of home pregnancy testing for two reasons. First, pregnancy confirmation was costly, and women lacked privacy in the male-dominant laboratory environment. Second, the need for early confirmation was stigmatized and associated with promiscuity (Better Homes and Gardens Baby Book 1966, *Consumer Reports* 1978).

At the end of 1977, the emergence of home pregnancy tests made pregnancy confirmation private, fast, and accurate.[6][7] Women could purchase and use this over-the-counter product easily without involving other people in the process and

[5]Most RCT studies conduct small-scaled intervention in developing countries.Andersen et al. (2013) recruits 1,683 women from Nepal; Morroni & Moodley (2006) studies 322 women in Cape Town, South Africa; and Comfort et al. (2019) follows 706 women in Eastern Madagascar.

[6]In 1972, scientists successfully distinguished human chorionic gonadotropin (hCG), a protein-based hormone that the body produces during pregnancy, and applied that knowledge to early tests for pregnancy (Vaitukaitis et al. 1972). For a history of the pregnancy test, please see the "A Timeline of Pregnancy Testing" web page from the Office of NIH History and Stetten Museum.

[7]When home pregnancy test kits first made it to the Canadian market in the early '70s, they might have become available to a small US market. Nevertheless, the product and its impacts seemed not to draw much attention among US medical professionals and in the health care market (Field 1971).

learn the results right away. In a survey by Coons (1989), above 90% of users purchased this product without pharmacists' intervention. The same survey finds that most users used the tests because of "the speed of obtaining results." The tests also provided accurate results without requiring advanced education to use them correctly. Early research concludes that accuracy rates from testing by a physician in a laboratory and by an average woman at home were not statistically different.[8]

Some research has studied the prevalence of home pregnancy testing. Many such studies conduct small-scale surveys among women visiting ob-gyns or family-planning clinics and find that between 28.5% and 40% of their sample have used this device (Valanis & Perlman 1982, Coons 1989). The National Maternal and Infant Health Survey (NMIHS) of 1988 is the first nationally representative dataset covering questions specifically about home pregnancy test utilization.[9] Its the raw data show that 25.3% of the pregnant women it surveyed had used over-the-counter home pregnancy tests for their current pregnancies; Jeng et al. (1991) finds that the adjusted share of use was approximately 33%. These samples could be selective because of the particular location or condition in which the survey sample was recruited.

Critically, drugstore accessibility did not correspond to the prevalence of other reproductive products. Two reproductive technologies, the oral contraceptive pill and abortion, became available well before home pregnancy tests entered the US market. The pill arrived in US drugstores as an over-the-counter product in 1960, which makes it possible that access to drugstores impacted its uptake. Based on the National Survey of Family Growth, Cycle III, 1982, Figure 1a shows the fraction of women taking the pill at time of first intercourse across different years.[10] For

[8]Family Planning Perspectives (1979) states that the four home-pregnancy-test products available in the late '70s had a very similar accuracy rate: more than 97% of first positive results were accurate, while 80% of first negative results were accurate. Women who got negative results usually had a second test to increase the accuracy rate to 91%. Family Planning Perspectives (1979) writes that the research cited mainly comes from *H. G. McQuarrie and A. D. Flanagan, "Accuracy of Early Pregnancy Testing At Home," paper presented at the annual meeting of the Association of Planned Parenthood Physicians, San Diego, Oct. 24-27; and data from a study by Veasy Butram, Jr., cited in A. D. Flanagan, "Review: e.p.t.-In-Home Early Pregnancy Test," Warner/Chilcott Medical Dept. (mimeo)."*

[9]The NMIHS of 1988 contains a sample of 9,953 women who had live births, 3,309 who had late fetal deaths, and 5,332 who had infant deaths in 1988.

[10]Another way to present pill use is to collect information on pill usage from different cycles of the NSFG. The drawbacks are that this covers a shorter period, the sample composition changes in each cycle, and the questions were asked differently, which might bias the estimation. I discuss

all women, the fraction taking the pill increased substantially from 5% to 11% in the early '60s and stayed between 11% and 15% from 1965 to 1982 (Pratt et al. 1984, Mosher & Bachrach 1987). The trends are similar among women who were unmarried at first intercourse. Thus, uptake did not change significantly in my study period. Meanwhile, abortion became legal nationwide in 1973. To exclude the possibility that access to drugstores correlated with access to abortion providers, I provide a county-level scatter plot of drugstore accessibility (drugstores per 10,000 residents) and number of abortion providers in Figure 1b. No evidence suggests that counties with greater accessibility to drugstores also had better access to abortion providers.[11]

If most users obtained the test from drugstores, local-drugstore numbers would be an indicator of its availability. Indeed, in a survey, Coons (1989) finds that about 90% of users made their purchases from drugstores. In addition, noting the prevalence of individuals buying home tests in drugstores, in 1978 the American Pharmaceutical Association (APhA) finally "recognized that the pharmacist is a widely available and qualified health professional to advise patients in the use of the more complex home-use, in vitro diagnostic and monitoring products" (*APhA House of Delegates Actions* 1987). This evidence supports using county-level drugstore numbers as a proxy for the availability of home pregnancy tests to examine its effects on fertility rates and timing of first prenatal-care visit.

3 Data

3.1 Drugstore and Abortion Provider Data

Information on county-level drugstore numbers comes from County Business Patterns (CBP). To identify accessibility to abortion providers in each county, I use county-level abortion-provider data from the Guttmacher Institute.

My primary data source on annual county-level drugstore numbers comes from

this alternative in Appendix C.2.

[11] I use number of abortion providers and not abortion cases because only 30% of counties had abortion providers in the late '70s, and many people traveled across counties to get an abortion. So the relationship between the number of abortion cases and drugstore accessibility at the county level might not precisely reflect the association between the prevalence of abortion and drugstore accessibility.

CBP between 1974 and 1984. CBP provides annual series of numbers of various types of establishments at the county level by Standard Industrial Classification (SIC) during my study period.[12] I define "drugstores" using establishments categorized as "Drug Stores and Proprietary Stores," where the SIC codes are 5910 and 5912.[13] Assuming that the number of drugstores predicts the sales of home pregnancy tests in a local neighborhood, I use the average county-level drugstore numbers per 10,000 residents between 1974 and 1984 as a proxy for test use.[14]

Importantly, the average county-level drugstore numbers per 10,000 residents are not highly correlated with the urban status of a county. Thus, my identification strategy of relying on the variation in drugstore levels is not merely a comparison between urban and rural areas. In Figure 2, I plot the correlation between average drugstores per 10,000 residents and log population densities in 1977 at the county level. The figure shows that drugstore accessibility varies among counties of similar population densities, especially when these counties are less dense. Importantly, drugstore accessibility does not predict the urban status of a county. If anything, the two measures are weakly but negatively correlated.

To estimate the complementarity between home pregnancy tests and access to abortion as influences on fertility, I obtain abortion-provider numbers of each county from Guttmacher Institute abortion data.[15] Kane & Staiger (1996) discusses the issue that the estimated number of abortion providers in each county can be imprecise because a smaller provider or a provider in a small county may have few cases in a year, and the authors note that annual changes of abortion provider numbers may be endogenous. To minimize these concerns, I create an indicator for whether a county had at least one abortion provider between 1979 and 1981. Given that distance to abortion providers has considerable influence on fertility and abortion rates, I calculate the distance between each county and the county with the nearest

[12]For CBP, the SIC was revised for 1988–97 data, and the North American Industry Classification System (NAICS) was adopted for data on 1988 and later years.

[13]For the detailed SIC code list, see Appendix B of Technical Documentation for County Business Patterns, 1974–86.

[14]To clarify, my main samples on prenatal care and fertility rates cover 1974–84. I fix county-level drugstore numbers as the average numbers for years between 1974 and 1984 from CBP for consistent data quality.

[15]The Guttmacher Institute reports the number of legal abortion providers with at least 20 cases in each county between 1979 and 1981.

provider, where distance serves as the measure of county-level accessibility.[16][17] To be precise, it is measured as the distance of between counties' centroids.

3.2 Other Data Sources

I use different sources of data for different outcomes and control variables. The birth-related information—including number of births, first month of prenatal care, counties of residence, and demographic background—comes from Vital Statistics Natality Birth Data. Women's later-life outcomes come from the 1990 5% Census. The Census data provides information on education attainment, labor market outcomes, marital status, poverty, state and Public Use Microdata Area (PUMA) of residence, and demographic background.

To consider local economic status in my estimation, I use county-level total employment, income per capita, and fraction of unemployment insurance income over total income from the Regional Economic Information System from the United States Bureau of Economic Analysis.[18] I also use SEER US County Population Data to calculate fertility rates and control for county-level demographic composition by gender, age, and race.

[16] Akerlof et al. (1996) and Kane & Staiger (1996) have proposed theories to connect the emergence of new reproductive technologies and fertility rates. Other papers study recent policies that increased abortion costs in some states. Most of them find that the increases—including increased distance to providers, new mandatory waiting periods, more clinic violence, and reduction in reduced funding—reduced abortion rates. The findings for fertility rates are less conclusive (Jacobson & Royer 2011, Fischer et al. 2018, Lindo et al. 2020, Lindo & Pineda-Torres 2021).

[17] Regarding the distance measure, it is at the county level because the exact addresses of residency and abortion providers are unavailable. Therefore, I drop counties with land areas in the top 15 percentiles to lessen the underestimation of the distance to the nearest abortion provider. My data do not allow me to identify cases in which the nearest provider to an individual living near the county border is in the neighboring county.

[18] When examining birth-related outcomes, I follow Kane & Staiger (1996) controlling for the same set of local economy variables in the year of conceptions. They discuss the concerns that using the unemployment rate, instead of total employment may introduce a bias because the population estimates are imprecise.

4 Short-Term Impacts: Fertility Rates and Early Prenatal Care

4.1 Sample Construction

I construct county-level measured fertility (birth rates) for women aged 15-39 using birth records from Vital Statistics Natality Birth Data between 1974 and 1984.[19] I also construct average month of pregnancy when initiating prenatal care. Table 1, Column (1) reports the summary statistics.[20][21] I restrict my sample to singleton births to white and black mothers, and I exclude births with unknown race, county of residency, or age of mother.[22] I define fertility rates as numbers of singleton live births per 1,000 women in a given age group.[23] Because women in different stages of life may behave differently, I create six age groups: age 15–39, 15–19, 20–24, 25–29, 30–35, and 35–39. Figure 4 shows that abortion ratios, defined as abortions per 1,000 births, varied across age groups between 1973 and 1984. Unsurprisingly, women aged 15–19, the group most likely to have unintended pregnancies, had the highest abortion ratio.

Figure 3 presents smooth trends in the two outcome variables. Figure 3a shows

[19] One concern regarding the study period is that a wave of Dalkon Shield IUD (intrauterine devices) removals might bias my estimation. The Dalkon Shield was one important contraceptive tool in the '70s. Because it increased risks for pelvic inflammatory diseases, it was only on the market between 1970 and 1974. However, it was not recalled until September 1980, and a media campaign advising women to remove it only began in October 1984 (Goodhue 1983, Horwitz 2018). Given that home pregnancy tests went on the market in 1977, the IUD removals that started in the mid-1980s should not affect my analyses.

[20] Regarding cutoff dates, home pregnancy tests were approved by the FDA in late 1977. Taking into account that the typical gestational length is between 9 and 10 months, I set the cutoff of for each year at August. The study period runs from August 1974 to July 1985. This should not cause issues because the cutoff is March 1 for CBP data and July 1 for the population data from the Census Bureau.

[21] In data from Vital Statistics Natality Birth Data, not all states consecutively collected the month prenatal care began between 1974 and 1984. To make a balanced sample, I drop data on Alabama, Alaska, Arkansas, Idaho, Massachusetts, New Mexico, Pennsylvania, and Virginia because the information on prenatal care in those states was not collected for selected years.

[22] These restrictions exclude about 3% of the observations.

[23] Some states only report 50% of the births in some years in the study period. Therefore, I weigh birth numbers for these states for these years with the weight factor provided in the Vital Statistics Natality Birth Data. For states with 100% (50%) of births reported, a weight factor of 1 (2) appears in that dataset. Appendix C.3 reports on states whose data are based on 100% of births between 1974 and 1984. This should not affect the results but should prevent large variations for counties with smaller population sizes.

fertility rates of the national average and the five age groups between 1974 and 1984, and Figure 3b shows the month of first prenatal care in each county. Nationally, the first month pregnant women began prenatal care smoothly decreased from around month 3 to 2.8 in my study period. Although using the first month of prenatal care as the measure could be too coarse for detecting impacts, another available prenatal-care-related statistic is total prenatal care visits, which usually correlate with other confounding factors (e.g., maternal health and risk aversion).

I create a subsample that contains counties with information on abortion-provider accessibility. This subsample focuses on women between the ages 15 and 29 because such women are more likely to have unintended pregnancies. Geographic variation in abortion-provider numbers is relevant for patients because women traveled for abortion, especially with limited accessibility in the '70s and '80s (Shelton et al. 1976, Kane & Staiger 1996, Henshaw 1991). I limit the study period to 1974–81 to avoid biases from abortion-clinic closures starting in 1982.[24][25] Using the distance between county of residency and county with the nearest abortion provider, I stratify counties into two groups: those with and without at least one provider within 20 miles.[26] I provide robustness checks using 15 and 25 miles as the distance cutoff in Appendix B.4. Table 1 reports the summary statistics of both samples, where the characteristics are comparable, including the employment–population ratios.

4.2 Empirical Framework

Using an event-study estimation, the main source of variation comes from the average county-level drugstore numbers per 10,000 residents during my study period; the numbers indicate the availability of home pregnancy tests.

Assuming that women living in counties with more drugstores had greater access

[24] Previous studies suggest nonlinear relationships between distance to abortion services and abortion rates (Joyce et al. 2012, Lindo et al. 2020).

[25] A wave of restrictions on Medicaid abortion funding and a wave of abortion-clinic closures took place in the late '70s and early '80s. Nevertheless, the data show that the distance remained stable until 1981 (Kane & Staiger 1996).

[26] To avoid endogeneity issues, I identify counties as having a provider as long as they had at least one provider with at least 20 cases between 1979 and 1981.

to home pregnancy tests, I estimate the following model:

$$Y_{cy} = \alpha + D_c \times \sum_{t=1974}^{1984} \beta_t \times \mathbf{1}_{y=t} + \gamma D_c + \boldsymbol{X}_{cy}\delta + \tau_{s\times y} + \tau_{c\times y} + \epsilon_{cy} \tag{1}$$

Here, Y_{cy} denotes the outcome variables—fertility rates and first month of prenatal-care visit—for county c in year y. D_c represents the average number of drugstores per 10,000 residents of county c during the study period. Fixing access to drugstores over time at the county level helps avoid the endogeneity between drugstore numbers and the outcome variables.[27] The coefficient β_{1977} is normalized to 0, as births after 1978 were exposed to the home pregnancy test. $\boldsymbol{X}_{cy}$ is a vector of county characteristics—including demographic composition, income per capita, total employment, and fraction of unemployment-insurance income—because the local economy affects pregnancy intention (Dehejia & Lleras-Muney 2004). $\tau_{c\times y}$ represents county-specific time trend effects, $\tau_{s\times y}$ state-by-year fixed effects, and ϵ_{cy} idiosyncratic shocks, which I assume to be independent of all other terms in Equation 1. All estimates are weighted using the number of women in the given group and clustered at the county level.

Two assumptions are made in the analyses. First, I assume that there were no underlying trends correlated with home-pregnancy-test uptake. The approval of home pregnancy tests occurred at the same time nationally, which made it an exogenous shock and not associated with any local trends. Second, I assume that there is no reverse causality between changes in drugstore numbers and home-pregnancy-test availability. Given that the home pregnancy test is one of the thousands of products a drugstore carries, it is unlikely that the sales of home pregnancy tests in the previous year could determine the opening or closure of a drugstore in a county.

[27] I examine fertility rates using two additional definitions of D_c in Appendix B.1. I first adopt raw county-level drugstore numbers to allow for within-county variation, and then I use the average of county-level drugstore numbers acorss the pre-period (1974–77) as the proxy. The results are robust.

4.3 Did Home-Pregnancy-Test Availability Affect Fertility Rates?

Figure 5 reports the results for impact on fertility rates with 95% confidence intervals estimated using the event study shown in Equation 1. For comparison purposes, I also plot predicted fertility rates based on the trends before 1977 in each figure. Figure 5a reports results of the final sample covering women between ages 15 and 39, while in Figures 5b to 5f, I break the sample down by age group. The coefficient estimates corresponding to each panel in Figure 5 are presented in Table 2.

Trend breaks in fertility around 1977 occurred among women aged 15–39, 15–19, 20–24, and 25–29 (Figures 5a, 5b, 5c, and 5d). The point estimates started to deviate from the linear prediction and stopped increasing after 1977.[28] The gaps between yearly estimates and the linear prediction were widening, implying that the effects were getting stronger over time. This might be because women were taking up home pregnancy tests gradually. The effects concentrated among younger women, and women aged 15–19 experienced the strongest effects when accounting for the fact that they had the lowest mean fertility rates.[29] In Figure 5b, taking as an example 1982, which was five years after home pregnancy tests entered the market, moving to a county with one more drugstore per 10,000 residents led on average to about 2 fewer births for every 1,000 women aged 15–19. The estimates are comparable to Ananat & Hungerman (2012) findings that pill access reduced births by 2 to 6 for every 1,000 women aged 14–20. Given that abortion is the channel causing the breaks, the findings are consistent with the fact that women aged 15–19 also had the highest abortion ratio, shown in Figure 4. This is consistent with the finding of Ananat et al. (2007) that when the cost of abortion decreased, younger women between 16 and 26 years old were more likely to be affected.

Figures 5a to 5d present increasing trends before 1977 among younger groups. These upward trends reflect the demographic transition in the '70s. The overall fertility rates were decreasing in the early '70s, especially for relatively younger

[28] Appendix B.1 provides robust results using two other definitions of drugstore accessibility.

[29] In Appendix B.2, I rule out the possibility that the trend break among women aged 15–19 results from changes in parental-consent laws in the '70s. Using legal coding from Myers (2017) and Myers (2022), I show that the results are robust when stratifying the sample of 15- to- 19-year-olds into two groups based on whether, in a given state, they had legal and confidential access to abortion. Importantly, in Section 4.4, I show that the effects of such access on fertility rates are stronger when it is complemented by access to home pregnancy tests.

women. However, trends in fertility rates varied among women of different demographic backgrounds. I discuss two examples that could explain the increasing trends. First, among younger women, Black women experienced a sharper decline in fertility rates compared to their white counterparts in the early '70s (Westoff et al. 1983). Second, immigrant births rose rapidly starting in 1970 because of the increase in immigration and in the share of females in their reproductive years among immigrants (Camarota 2005). In these cases, the fertility rates would appear to trend upward if white women and immigrants were more likely to live in counties with higher drugstore levels.

Given that immigrants in the '70s mainly came from Latin America and Asia, immigrants made up a tiny share of the Black population (Martin 2013).[30] This implies that there were relatively fewer confounding factors among Black women. Therefore, I stratify my sample by race to validate that the increasing trends before 1977 were mainly driven by the demographic transition in the early '70s as discussed above.[31] If race and immigration status played a part in explaining the increasing trends, we should expect to see a more flat trend before 1977 among Black women while the trend break remains. Figure 6 reports the event-study result that the estimates in both groups present significant trend breaks around 1977. As expected, the fertility rates among white women show the same trend as the main results in Figure 5, while the trend in fertility rates among Black women was flat before 1977 and started to decrease after 1977.[32] The same patterns persist among Black women when I further divide them into different age groups. These findings support the conjecture that racial and immigration compositions in the population are important sources of heterogeneity that explain the increasing trends before 1977 in Figures 5a to 5d. Importantly, the trend breaks persist within the population.

To better quantify the trend breaks, I estimate the difference in trends (or slopes)

[30] My sample only includes Black and white women because only a small share of birth records consisted of races other than Black and white in the study period. Thus, my sample contains immigrants from Latin America but not from Asia.

[31] Characteristics such as immigrant status and religion at the county level are only available in decennial reports.

[32] The estimates are noisier for Black women because the sample of Blacks is smaller. Note that my sample is balanced: the populations of 2,076 counties include whites over the entire study period, while only 1,204 include Blacks every year over the same period.

following Equation 2.

$$Y_{cy} = \alpha + \beta D_c \times YEAR_y \times \mathbf{1}_{y \geq 1978} + \gamma D_c + \boldsymbol{X}_{cy}\delta + \tau_{s\times y} + \tau_{c\times y} + \epsilon_{cy} \quad (2)$$

$YEAR_y$ is a year running variable, and $\mathbf{1}_{y \geq 1978}$ is an indicator for $y \geq 1978$. Other notations follow Equation 1. Table 2 reports the coefficients of interest, β, in the upper row. The difference in trends is consistently significant and negative for the full sample: the fertility rates fell by 0.6% or 0.8% one year after home pregnancy tests became available. The estimates are significant only among women aged 15–29.

I provide falsification tests in Appendix B.3 by substituting drugstore accessibility with access to apparel and accessory stores, eating and drinking places, and home furniture and furnishings stores. Access to these four categories of retail stores is unlikely to correlate with drugstore accessibility nor with reproductive behaviors. The falsification tests present robust findings and rule out the possibility that my main findings are driven by an arbitrary choice of the proxy for home pregnancy test availability.

4.4 The Role of Access to Abortion

In this section, I explore the complementarity between home pregnancy tests and access to abortion among younger women because trend breaks in fertility around 1977 occurred among this group. To estimate whether the impact of home-pregnancy-test availability on fertility increases with access to abortion, I exploit the geographic variation of abortion providers and the changes in parental-consent laws in the late 1970s.

Distance to Abortion Providers

I limit my sample to women aged 15 to 29 and stratify my sample by distance to the nearest abortion providers. Table A3 provides the summary statistics of these two groups. Around 27% of the observations had access to abortion providers within 20 miles (also reported in Table 1), and these tend to be counties with a larger population and lower fertility rates.

Figure 8 reports the results with 95% confidence intervals and full specification following Equation 1. Among women with access to drugstores, trends of fertility rates between counties with and without abortion providers were similar before 1977, the base year; and they deviated from each other afterward. Figure 8a shows that in counties with no access to abortion clinics within 20 miles, the fertility rates were increasing and presented no trend breaks between 1974 and 1981. While Figure 8b shows that in counties with providers within 20 miles, a trend break appeared in the year when home pregnancy tests became available and the trend became significantly different from the linear prediction. Notably, the pretrends before 1978 are similar in both areas with and without an abortion provider nearby.

For falsification test, I change the cutoff of group assignment from 20 miles to 15 and 25 miles, respectively (Figure B.4). When changing the cutoff to 15 miles (20 miles), observations in the group with access to abortion providers decrease (increase) from 27% to 21% (40%). The results are robust.

Changes in Parental-Consent Law for Abortion

Previous research finds that access to abortion granted by lifting parental consent laws reduced fertility rates among minors (Levine 2003, Guldi 2008). I further test whether the effects were stronger when minors had access to both abortion services and drugstores. I focus on births to mothers aged 15 to 21 between 1974 and 1980 and construct county-year-level fertility rates by single year of age following Guldi (2008).[33] I estimate the following equation:

$$\begin{aligned} ln(Fertility)_{ysca} = & \beta_0 + \beta_1 Abortion\ Access_{(y-1)sa} \times D_{sc} \times Post_y + \\ & \beta_2 Abortion\ Access_{(y-1)sa} + \boldsymbol{X}_{(y-1)sc}\beta_4 + \boldsymbol{V}_{(y-1)sca}\beta_5 + \\ & \tau_{s\times y} + \tau_{sc\times y} + \tau_a + \epsilon_{ysca} \end{aligned} \tag{3}$$

$ln(Fertility)_{ysca}$ denotes log fertility rates of women aged a residing in state s and county c in year y.[34] $Abortion\ Access_{(y-1)sa}$ is an indicator for whether women aged a in state s had access to abortion services without requiring parental consent in the year of conception $y-1$.[35] D_{sc} denotes average county-level drugstore accessibility

[33] Guldi (2008) constructs her observations at the state-year-age-race level. I construct mine at the county-year-age level instead because my treatment, drugstore accessibility, is at the county level. And to reduce the bias from the population estimates for the denominator, I do not further stratify births by race. Instead, I control for share of Blacks in each cell.

[34] $ln(Fertility)_{ysca} = ln(1,000 \times Number\ of\ Births_{ysca}/Population_{ysca})$

[35] I use codes from Myers (2017).

across my study period. $Post_y = 1$ if $y \geq 1978$, as home pregnancy tests entered the US market in late 1977. $\boldsymbol{X}_{(y-1)sc}$ is a vector of time-varying county characteristics; $\boldsymbol{V}_{(y-1)sca}$ is a vector of age-specific time-varying characteristics.[36] τ_a denotes age fixed effects. The notations of other fixed effects and of the error term ϵ_{ysca} follow the previous definitions. All estimates are weighted using the number of women in the given group and clustered at the county level.

Table 3 reports the results. Column (1) shows that access to abortion resulting from lifting parental-consent laws reduced fertility rates by 8%. This is comparable to Guldi (2008)'s estimation. In Column (2), when interacting *Abortion Access* with *Post*, the effects of *Abortion Access* become significantly weaker in the post-period. This may imply that there were more and more ways to circumvent the parental consent laws. For instance, traveling to other states might become more manageable over time. The coefficient of *Abortion Access* thus increased (in absolute value). In Column (3), I further consider access to home pregnancy tests. The estimates show that compared to women who only had access to abortion, women who had access to both abortion and home pregnancy tests experienced an additional 5% decline in fertility rates.

Finally, I stratify my sample into two groups based on whether there was at least one abortion provider within 20 miles. In Table 3, Column (4) reports that the effects of access to abortion (resulting from the change in parental-consent laws) accompanied by access to home pregnancy tests almost double (from a 4.8% decline to a 9.2% decline) when there is an abortion provider nearby. Column (5) shows no effects for minors who lived far away from abortion providers. These findings confirm that home pregnancy tests affect fertility rates when they are accompanied by access to abortion services.

[36] $\boldsymbol{X}_{(y-1)sc}$ comprises share of Blacks in the population, income per capita, total employment, and fraction of unemployment-insurance income out of all income. $\boldsymbol{V}_{(y-1)sca}$ comprises the interaction terms $Abortion\ Access_{ysa} \times D_{sc}$ and $Abortion\ Access_{ysa} \times Post_y$ as well as the variable $Pill\ Access_{ysa}$, which indicates whether women had access to birth control pills without requiring parental consent. To construct $Pill\ Access_{ysa}$, I follow Myers (2017)'s coding.

4.5 Did Home-Pregnancy-Test Availability Affect Early Prenatal Care?

Figure 7 reports the results of impacts on the first- month of prenatal care with 95% confidence intervals following Equation 1.[37] The figure shows a trend break around 1977, where women had their first prenatal care earlier and earlier before 1977 but later and later afterward. Although Vital Statistics Natality Birth Data specifies that a pregnancy test should not be counted as the first prenatal care, these data are self-reported by women after labor. In addition, women can always initiate a consultation right after learning the pregnancy-test results. In this case, women would meet their physicians for the first consultation earlier when pregnancy tests were only available in clinics and hospitals. If most women used their first prenatal care for pregnancy confirmation, it is reasonable to expect that women had their first prenatal-care visit later after home pregnancy testing became available. Indeed, Table A2 reports that the difference in trends is significant with a magnitude of 0.01 months.

However, 0.01 months is equivalent to 0.3 days, which is economically insignificant. Moreover, Figure 7 shows that most point estimates are not significantly different from the reference year, 1977. Although β_{1974}, the point estimate for 1974, is significantly different from 0, the effect size is 0.019 months (reported in Figure 7 and Table A2), which is equivalent to 0.57 days. I conclude that I find no effects on prenatal care initiation. Notably, data limitations prevent me from observing a finer measure of prenatal-care initiation than the monthly measure. It could be that prenatal care did start earlier, but these data do not allow one to measure it.

[37] Following the same logic, mothers who recognized their pregnancy status earlier may have changed their behavior earlier in ways that improved birth outcomes. Given that information on tobacco use only began to be recorded in mid-1980, instead of that measure, I examine birth weight, preterm-birth status, and Apgar scores as outcomes of behavioral changes. I do not find significant impacts, which is to be expected because I do not find that women initiate prenatal care earlier.

5 Long-Term Impacts: Delaying Childbirth and Women's Later-Life Outcomes

5.1 Sample Construction

To explore how the complementarity between home pregnancy tests and access to abortion affects long-term outcomes, I focus on women's fertility across reproductive years and their later-life outcomes. My sample covers three groups of women with different levels of exposure to home pregnancy tests between ages 16 and 18 (the last three years of high school) when the product hit the market in 1977: (1) no exposure (cohorts 1956–58), (2) partial exposure (cohorts 1959–61), and (3) full exposure (cohorts 1962–64). I use average local-level drugstore numbers per 10,000 residents aged between 16 and 18 to proxy for home-pregnancy-test availability.

To test whether women were more likely to delay childbirth when exposed to home pregnancy tests and abortion services earlier, I compare fertility rates of cohorts over their reproductive years. Using the Vital Statisitcs Natality Birth Data from 1976 to 2002, I calculate fertility rates and average birth orders for three maternal age groups: age 20–26, 27–33, and 34–40.[38] Fertility rates are defined as numbers of births at a given age in a given county and year. I also create an indicator for whether there was at least one abortion provider within 20 miles between 1979 and 1981 at the county level. Table 4 provides the summary statistics. The younger cohorts had higher fertility rates and lower birth orders. However, the table shows no significant difference across the full sample and the three groups.

For women's later-life outcomes of interest, I focus on educational attainment (high school dropout and college), labor market outcomes (labor force participation and current employment status), and marital status (never married, currently divorced, and currently married) from the 5% 1990 Census. I also observe race, state of birth, urban status of residency, migration within five years, and PUMA of residency. Although data on the residence while attending high school and on people's entire migration history do not exist, most people attend high schools in their home county. Thus, to minimize biases, I restrict my sample to women who

[38] For years 1989–2002, I include only counties with more than 100,000 residents because smaller counties cannot be identified from the data. Nineteen percent of the births in these years are thus eliminated. To have a balanced panel, I restrict the sample in the same way when examining outcomes for maternal ages 27–33.

still lived in their birth states and did not move within the last five years.[39]

The smallest geographic level in the public Census data is the PUMA, and thus I merge the county-level information on access to drugstores and abortion providers to the PUMA level. In particular, I use the population-weighted number of abortion providers in a PUMA as a proxy for accessibility of abortion providers.[40] Table 5 presents the summary statistics. The summary statistics display no significant difference across the sample.

5.2 Empirical Framework

My identification strategy relies on different levels of exposure to home-pregnancy-test availability between ages 16 and 18. I create an indicator for birth cohorts 1956–58 ($b = 0$), 1958–61 ($b = 1$), and 1962–64 ($b = 2$). To estimate whether women exposed to home pregnancy tests in a county with access to abortion providers were delaying childbirth, I focus on fertility rates and birth orders for women aged 20–26, 27–33, and 34–40 separately. I adopt a difference-in-differences (DID) specification at the the age-county-year level:

$$\begin{aligned} Y_{acy} =& \alpha + \sum_{t=0}^{2} \mathbf{1}_{b=t} \times \left[\beta_b \times \overline{D}_{ac} \times Access_c + \gamma_b \overline{D}_{ac} + \delta_b Access_c\right] + \\ & \kappa \overline{D}_{ac} + \lambda Access_c \times \overline{D}_{ac} + \boldsymbol{X}_{c(y-1)}\eta + \tau_y + \tau_c + \tau_{state \times b} + \epsilon_{acy} \end{aligned} \tag{4}$$

Y_{acy} denotes the average birth order and fertility rates among women of age a residing in county c in year y. $\overline{D}_{ac}$ denotes drugstore accessibility—county-level drugstore numbers per 10,000 residents—when a girl was between ages 16 and 18. $Access_c$ denotes whether there was at least one abortion provider within 20 miles between 1979 and 1981, when home pregnancy tests first entered the market. $\boldsymbol{X}_{c(y-1)}$ is a vector of time-varying county characteristics in the year of conception. τ_y denotes year fixed effects, τ_c county fixed effects, $\tau_{state \times b}$ state-by-cohort fixed effects, and ϵ_{acy} idiosyncratic shocks. Estimation is weighted by numbers of women in a given

[39]This restriction has minimal effects on my estimates. I remove this residency constraint and obtain robust results in Appendix B.5.

[40]Here I use population-weighted abortion-provider numbers but not distance to the nearest abortion provider because of the data constraint—the information on numbers of abortion providers is recorded at the county level, while the Census data are at the PUMA level. Given that in most cases a PUMA contains many counties, I cannot assess the distance at the PUMA level.

group and two-way clustered at the county-cohort level.

For women's later life outcomes, I estimate the impacts of exposure in a PUMA with greater access to abortion providers using a DID specification at the individual level:

$$\begin{aligned} Outcome_{ipb} =& \alpha + \sum_{t=0}^{2} \mathbf{1}_{b=t} \times \left[\beta_b \times \overline{D}_{pi} \times Access_p + \gamma_b \overline{D}_{pi} + \delta_b Access_p\right] + \\ & \kappa \overline{D}_{pi} + \lambda Access_p \times \overline{D}_{pi} + \boldsymbol{X}_i \eta + \boldsymbol{\Pi}_{pi} \theta + \tau_p + \tau_{s \times b} + \epsilon_{ipb} \end{aligned} \tag{5}$$

Here, i denotes the individual, p the PUMA, s the state of birth (also the state of residence in 1990), and b the birth cohort. $\overline{D}_{pi}$ represents the population-weighted PUMA-level number of drugstores per 10,000 residents in a woman's state of birth when she was aged 16 to 18. *Access* is the population-weighted number of abortion providers in a PUMA. $\boldsymbol{X}_i$ denotes race and the urban status of residence; $\boldsymbol{\Pi}_{pb}$ denotes population-weighted PUMA-level economic status between ages 16 and 18, including total employment and income per capita. τ_p indicates PUMA fixed effects, $\tau_{s \times b}$ state-of-birth-specific time fixed effects, and ϵ_{ipb} idiosyncratic shocks. Estimation is two-way clustered at the PUMA-cohort level.

Different birth cohorts corresponding to different levels of exposure to home pregnancy tests are the key to my identification. I assign cohorts 1956–58 as the reference group (or $\beta_0 = 0$). The coefficients of interest are β_1 and β_2, which estimate how the outcomes were affected when women had the same accessibility to abortion providers and drugstores but different exposure levels to home pregnancy tests.

5.3 Did Home-Pregnancy-Test Availability Delay Childbirth?

Home pregnancy tests increased the uptake of abortion services by providing early fertility information. Access to abortion providers does not necessarily affect long-term fertility. Instead, it may delay the timing of childbirth, childbearing, and marriage (Goldin & Katz 2002, Mølland 2016). I thus test whether the combined access to home pregnancy tests and abortion providers had similar effects on the timing of childbirth. Specifically, I focus on fertility rates and birth orders for three reproductive age groups: age 20–26, 27–33, and 34–40.

Table 6 reports the results. Columns (1) to (4) show that among women with the same access to abortion services and drugstores in the late 70s, fertility rates and birth orders at a younger age had no significant change across exposure levels. Meanwhile, Column (5) shows that fertility rates between ages 34–40 increased by 6% among women with full exposure to home pregnancy tests between ages 16 and 18. In addition, Column (6) indicates that their birth parities were significantly lower (6%). Notably, births between maternal ages 34 and 40 are more likely to be their last births. The lowered birth order among the increased fertility rates at older ages and not effects at younger ages implies that within this population, women exposed to home pregnancy tests complemented with abortion access while young delayed childbirth.

5.4 Did Home-Pregnancy-Test Availability Affect Women's Later-Life Outcomes?

Table 7 reports the results of estimating Equation 5. The findings show that exposure to home pregnancy tests accompanied by access to abortion providers while young significantly increased the likelihood of labor force participation and being never married and decrease the odds of being currently divorced. The coefficient estimates of other outcomes are imprecise.

Column (3) shows that with the same accessibility to drugstores and abortion, the probability of participating in the labor force increased by 0.32 percentage points (or 0.4%) for cohorts 1962–64 compared to the reference group. The magnitude of the effects is small but significant. Nevertheless, I do not find significant effects on currently being employed (Column (4)). This may imply that although women at the margin became more likely to join the labor force, these women might not be as qualified as those who would always join the labor force in the labor market.

Columns (5) to (8) report that effects concentrated on marital outcomes. In specific, cohorts 1962–64 experienced a 0.43 percentage-point (4.3%) reduction in current divorce. Among women ever married, the same cohorts experienced a 0.58 percentage-point (4.1%) reduction in current divorce. Meanwhile, the odds of never marrying increased by 0.68 percentage points (3.1%). My findings on marital outcomes are qualitatively similar to estimates of Goldin & Katz (2002). There are two channels by which access to home pregnancy tests accompanied by access to abortion services could lead to a reduction in divorce. First, all women who would have been divorced did not enter marriage. Second, the likelihood of divorce decreased among those ever-married women. Both channels can be explained by better marriage matching when women delayed age at first birth and marriage (Goldin & Katz 2002, Ananat & Hungerman 2012).

6 Conclusion

This paper has documented the short- and long-term consequences of timely fertility information provided by a widely used test– home pregnancy tests. I use local-drugstore numbers to approximate the home-pregnancy-test availability. I examine the impacts of availability on fertility rates, early prenatal care, and women's outcomes in later life.

I find that the availability of home pregnancy tests stopped the increasing trends in fertility rates among women aged 15–29 who had access to drugstores, and the effects are the strongest among those aged 15–19. Evidence also suggests that access to abortion explains the trend breaks in fertility rates, as timely information made women more likely to meet the gestational limits on abortion. For long-term effects on fertility, I find evidence that women exposed to home pregnancy tests and abortion services while young were more likely to delay childbirth. For women's later-life outcomes, I find a 0.4% increase in labor market participation, a 4% reduction in current divorce (among all women and ever-married women), and a 3% increase in never entering marriage when home pregnancy tests were introduced in areas with access to abortion providers and drugstores.

This paper only estimates intent-to-treat (ITT) effects. I cannot estimate treatment-on-the-treated (TOT) effects directly because no data on home-pregnancy-test use can be linked to specific pregnancies, births, or abortions. To obtain the TOT, by definition, one would have to divide ITT by the share of compliers. The share of women who took up home pregnancy tests in 1988, the first year for which this measure is available, was 33% (Jeng et al. 1991).[41] If we assume 33% accurately reflects the takeup in 1978, the TOT effects are three times as large. In addition, reductions in the cost of abortion also decrease the cost of having sex and thus may increase sexual activity (Akerlof et al. 1996). Taking these concerns into consideration, this paper's estimates are lower bounds.

[41] The smallest geographic level in the NMIHS of 1988 is the region, which does not allow for comparison to the county-level variation in this paper.

Home pregnancy tests are a complementary tool to other reproductive technology that allow women to take marginally more control over their own reproductive process. By incorporating access to home pregnancy testing, this paper complements the literature on reproductive technologies and their consequences and allows us to more accurately estimate the impacts of access to abortion. Providing access to early pregnancy confirmation can be an effective and low-cost method to prevent late-term abortion and improve marriage matching quality among at-risk populations and in resource-limited areas. It also suggests that reducing access to reproductive services, such as abortion, could cause adverse outcomes.

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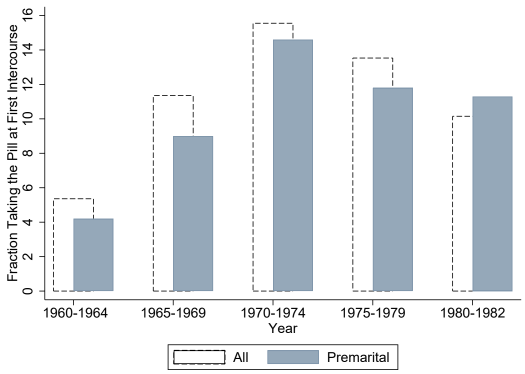
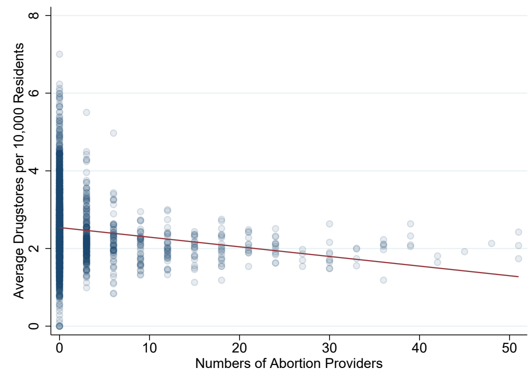


(a) Usage of the Pill at First Intercourse



(b) Abortion Providers

Figure 1: Correlation Between Accessibility to Drugstores and Other Reproductive Technologies

To zoom in, I exclude counties with abortion provider numbers at the top 1% level in Figure 1b. *Source:* National Survey of Family Growth, Cycle III, 1982. Mosher & Bachrach (1987). County Business Patterns. Guttmacher Institute.

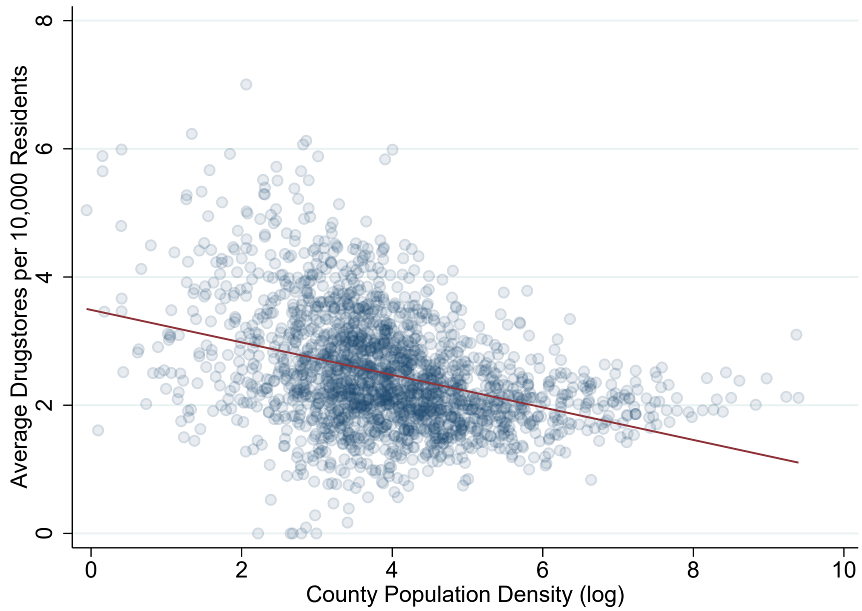
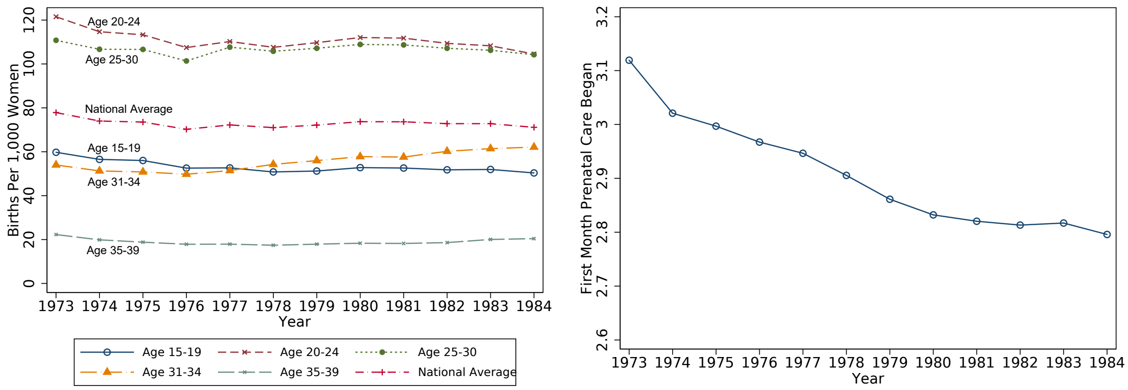


Figure 2: Correlation Between Drugstore Accessibility and County Population Density, 1977

I define county-level drugstore numbers as the average numbers among years between 1974 and 1984 from CBP for consistent data quality. Log county population density is defined as the population size of 1977 per square mile.

Source: County Business Patterns 1974–84. U.S. Census Bureau, Estimates of the intercensal population of counties 1970–79. U.S. Census Bureau, Land Area, 1980.



(a) Fertility Rates by Age Groups (b) First Month Prenatal Care Began

Figure 3: Trends in Fertility Rates and First Month Prenatal Care Began 1974–84

List of states with missing prenatal care adoption by years is provided in Appendix C.3. Fertility rates are defined as numbers of births per 1,000 women in each age block at the national level. Cutoffs of years are set to be August, i.e., the year of 1978 ranges from August 1978 to July 1979. *Source:* Vital Statistics Natality Birth Data between August 1974 and July 1985. Census Bureau between 1974 and 1984.

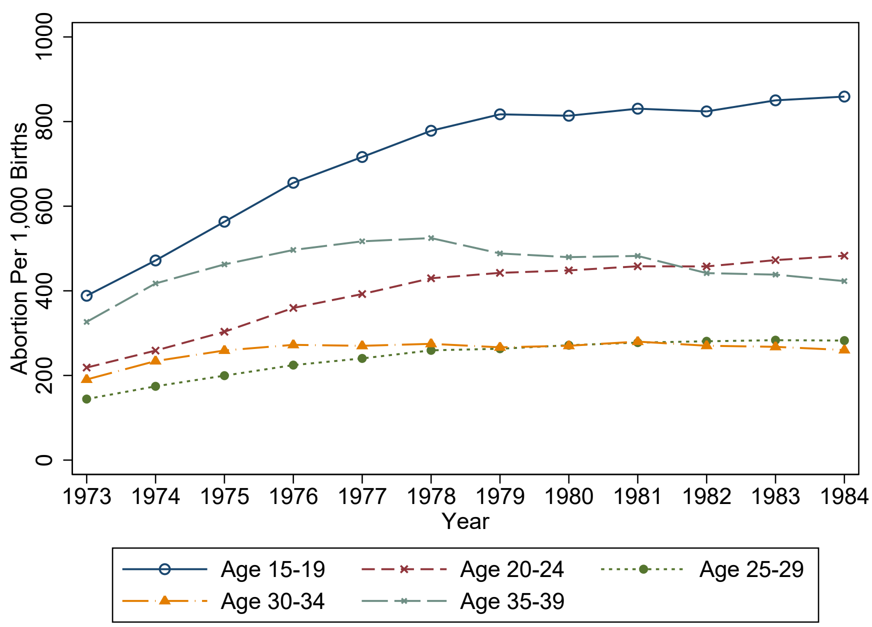
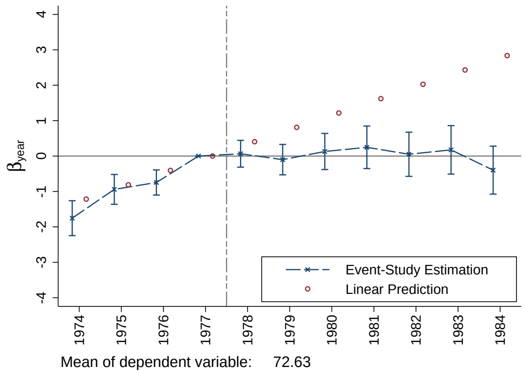
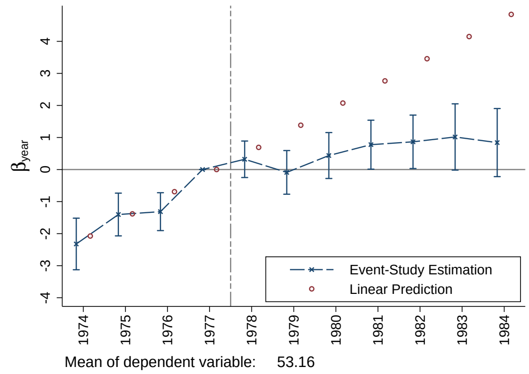


Figure 4: Abortion ratio by age groups

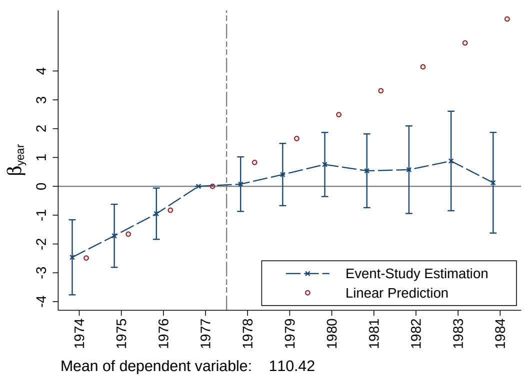
Source: Pregnancies, Births and Abortions in the United States: National and State Trends by Age, 1973-2017.



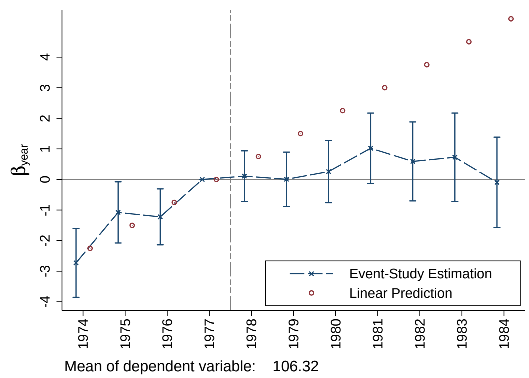
(a) All: Age 15–39



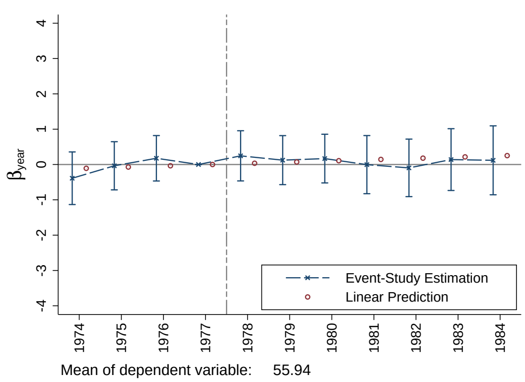
(b) Age 15–19



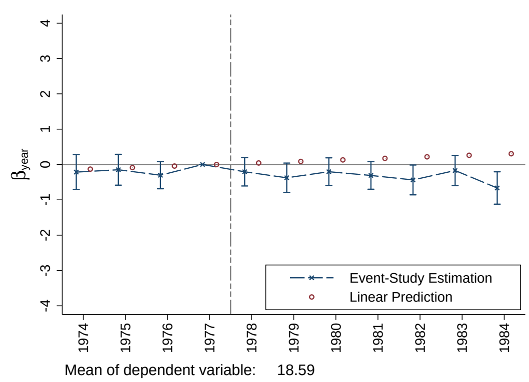
(c) Age 20–24



(d) Age 25–29



(e) Age 30–34



(f) Age 35–39

Figure 5: Impacts on County-Level Fertility Rates

The full set of specifications are reported in Table 2. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero.

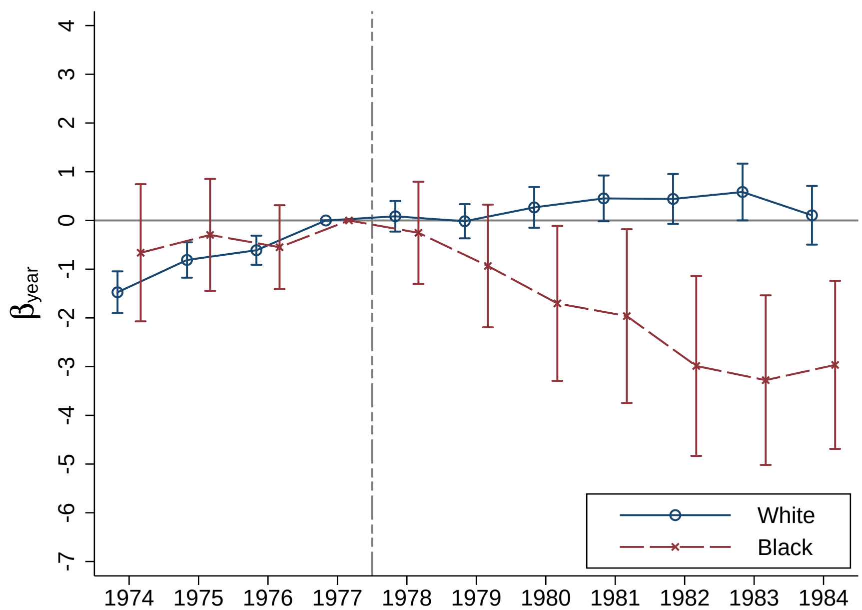


Figure 6: Impacts on Fertility Rates by Race

The full set of specifications are reported in Table A1. 2,076 counties had White population over my study period, while only 1,204 had black population every year over the same period. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero. List of states with missing prenatal care adoption by years is provided in Appendix C.3.

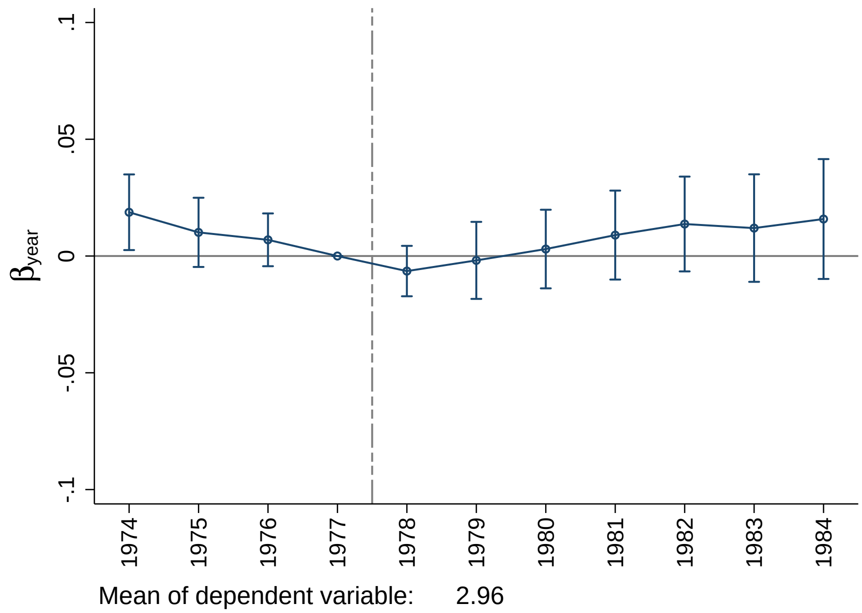
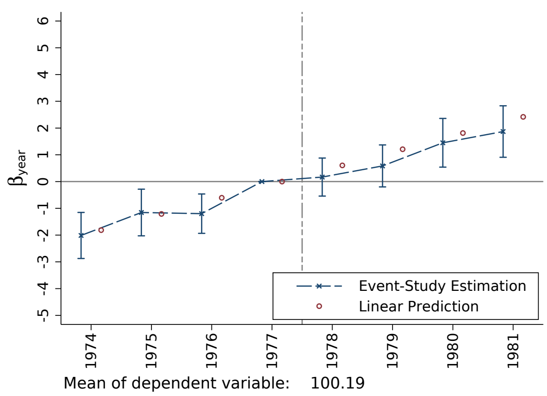
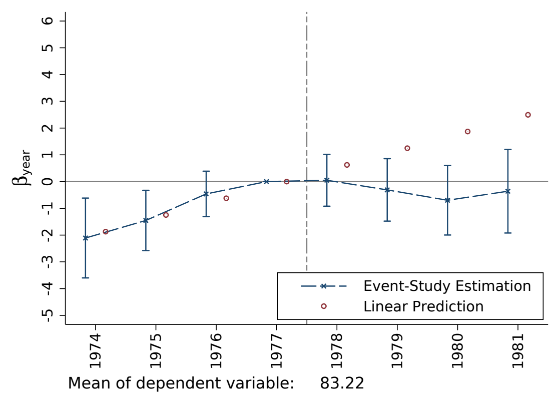


Figure 7: Impacts on First Month Prenatal Care Began

The full set of specifications are reported in Table A2. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero. List of states with missing prenatal care adoption by years is provided in Appendix C.3.



(a) No Access in 20mi



(b) With Access in 20mi

Figure 8: Impacts of Home Pregnancy Test Availability on Fertility Rates By Abortion Provider Accessibility

The full set of specifications are reported in Table A4. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero.

Table 1: Summary Statistics of County-Year Level Observations

	(1) Main Sample	(2) Subsample
Number of counties	2,079	1,815
Observations (county-year level)	22,869	14,520
Year	1974-1984	1974-1981
Age range	15-39	15-29
Births per 1,000 women	80.13	100.46
	(15.93)	(19.32)
Month prenatal care began	2.96	3.01
	(0.37)	(0.37)
Black	0.11	0.13
	(0.17)	(0.20)
Marital Status*	0.84	0.80
	(0.13)	(0.15)
High School Diploma*	0.78	0.73
	(0.11)	(0.12)
Drugstore numbers per 10,000 residents†	2.48	2.47
	(0.90)	(0.90)
Total employment†	41,054	37,114
	(142,632)	(108,259)
Income per capita†	7,243	6,184
	(2,513)	(1,826)
Fraction of Unemployment Insurance Income†	0.0082	0.0074
	(0.0066)	(0.0063)
County population†	83,821	77,208
	(254,521)	(199,191)
County demography†		
Percent of female population in the give age range that is		
black and age 15–19	0.026	0.061
white and age 15–19	0.217	0.475
black and age 20–24	0.022	0.049
white and age 20–24	0.197	0.415
black and age 25–29	0.017	0.036
white and age 25–29	0.185	0.385
black and age 30–34	0.013	
white and age 30–34	0.168	
black and age 35–39	0.011	
white and age 35–39	0.144	
Distance to abortion providers		0.21
At least 1 in county $\leq$ 15 miles		(0.41)
Distance to abortion providers		0.27
At least 1 in county $\leq$ 20 miles		(0.44)
Distance to abortion providers		0.39
At least 1 in county $\leq$ 25 miles		(0.49)

* For selected stated that reported the information. † Calculated for the year of conceiving. List of states with missing prenatal care information by years is provided in Appendix C.3. Cutoffs of years are set to be August, i.e., the year of 1978 ranges from August 1978 to July 1979.
Source: Vital Statistics Natality Birth Data between August 1974 and July 1985. Census Bureau between 1974 and 1984. County Business Patterns between 1974 and 1984. Guttmacher Institute abortion data 1979-1981. United States Bureau of Economic Analysis, Regional Economic Information System 1969-1992.

Table 2: Impacts of Home Pregnancy Test Availability on Fertility Rates Among Age Groups

	(1) age 15–39	(2) age 15–19	(3) age 20–24	(4) age 25–29	(5) age 30–34	(6) age 35–39
Trend Break						
Post1978	-0.578***	-0.555***	-0.785**	-0.762***	-0.152	-0.095
	(0.094)	(0.149)	(0.249)	(0.205)	(0.138)	(0.082)
Event Study						
β_{1974}	-1.754***	-2.323***	-2.463***	-2.728***	-0.389	-0.215
	(0.251)	(0.410)	(0.664)	(0.574)	(0.380)	(0.253)
β_{1975}	-0.941***	-1.404***	-1.717**	-1.078*	-0.036	-0.148
	(0.215)	(0.340)	(0.558)	(0.510)	(0.348)	(0.223)
β_{1976}	-0.746***	-1.314***	-0.950*	-1.223**	0.176	-0.303
	(0.181)	(0.301)	(0.453)	(0.467)	(0.328)	(0.196)
β_{1977}	0	0	0	0	0	0
β_{1978}	0.065	0.320	0.076	0.109	0.245	-0.205
	(0.193)	(0.290)	(0.482)	(0.421)	(0.362)	(0.205)
β_{1979}	-0.101	-0.089	0.408	0.006	0.124	-0.376
	(0.219)	(0.347)	(0.550)	(0.453)	(0.354)	(0.212)
β_{1980}	0.129	0.437	0.758	0.257	0.168	-0.204
	(0.260)	(0.365)	(0.567)	(0.519)	(0.351)	(0.200)
β_{1981}	0.248	0.776*	0.538	1.021	-0.004	-0.310
	(0.306)	(0.389)	(0.652)	(0.586)	(0.420)	(0.199)
β_{1982}	0.051	0.865*	0.576	0.590	-0.095	-0.437*
	(0.318)	(0.425)	(0.774)	(0.658)	(0.415)	(0.215)
β_{1983}	0.175	1.016	0.879	0.727	0.139	-0.170
	(0.349)	(0.527)	(0.880)	(0.737)	(0.446)	(0.219)
β_{1984}	-0.397	0.840	0.125	-0.095	0.119	-0.664**
	(0.345)	(0.542)	(0.891)	(0.754)	(0.497)	(0.233)
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes
County-specific time trend effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-State FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	72.63	53.16	110.42	106.32	55.94	18.59
Adjusted R^2	0.930	0.927	0.931	0.821	0.833	0.771
Observations	22,869	22,869	22,869	22,869	22,869	22,869

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. The coefficients of 1977 are normalized as zero.

Table 3: Impacts of Access to Abortion Granted by Lifting Parental Consent Law Complemented with Home Pregnancy Tests Availability on Log Fertility Rates

	(1) All Sample	(2) All Sample	(3) All Sample	(4) Dis: below 20mi	(5) Dis: above 20mi
Abortion Access	-0.079***	-0.101***	-0.125**	-0.216**	0.016
	(0.024)	(0.023)	(0.049)	(0.103)	(0.045)
Abortion Access × Post		0.064***	0.159***	0.259***	0.048
		(0.017)	(0.041)	(0.084)	(0.036)
Abortion Access × Drugstore × Post			-0.048***	-0.092**	-0.014
			(0.015)	(0.040)	(0.012)
Time-varying controls	Yes	Yes	Yes	Yes	Yes
County-specific time trend effects	Yes	Yes	Yes	Yes	Yes
Year-by-State FE	Yes	Yes	Yes	Yes	Yes
Mean of dep var	3.913	3.913	3.913	3.847	4.080
Adjusted R^2	0.901	0.901	0.901	0.930	0.828
Observations	136,151	136,151	136,151	33,576	102,575

Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Time-varying controls include the share of black population, total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Age-varying controls including access to birth control pills, $Abortion\ Access_{ysa} \times Drugstore_{sc}$, $Abortion\ Access_{ysa} \times Post_y$ are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. $Post = 1$ if $year \geq 1978$. I assign $ln(Fertility) = ln(1/2)$ when $Fertility\ Rate = 0$ in the cell.

Table 4: Summary Statistics Across Birth Cohorts

	(1) All	(2) Cohort 1956–58	(3) Cohort 1959–61	(4) Cohort 1962–64
Maternal Age 20–26				
N of Counties		2,960	2,978	2,978
Age	22.95	23.17	23.02	22.63
Births per 1,000 women	138.68	136.74	138.58	140.86
	(64.29)	(65.25)	(63.44)	(64.14)
Birth order	1.83	1.86	1.84	1.80
	(0.36)	(0.37)	(0.37)	(0.34)
Maternal Age 27–33				
N of Counties		406	457	458
Age	29.98	29.93	29.94	30.08
Births per 1,000 women	94.97	90.57	96.50	98.02
	(24.47)	(23.08)	(24.36)	(25.31)
Birth order	2.23	2.24	2.23	2.21
	(0.28)	(0.29)	(0.28)	(0.28)
Maternal Age 34–40				
N of Counties		458	460	454
Age	36.90	37.08	37.01	36.56
Births per 1,000 women	35.09	31.14	34.12	40.61
	(18.35)	(17.12)	(17.89)	(18.87)
Birth order	2.74	2.77	2.73	2.69
	(0.46)	(0.49)	(0.45)	(0.42)

Counties with population below 100,000 cannot be identified between 1989 and 2002. To have a balanced panel, I restrict the sample in the same way when examining outcomes for maternal ages 27–33. *Source:* Vital Statistics Natality Birth Data between 1976 and 2002. Census Bureau between 1976 and 2002. United States Bureau of Economic Analysis, Regional Economic Information System.

Table 5: Summary Statistics (Women Born 1956–64 from 1990 Census)

	(1) All	(2) Cohort 1956–58	(3) Cohort 1959–61	(4) Cohort 1962–64
High School Dropout	0.085	0.081	0.085	0.089
	(0.279)	(0.272)	(0.279)	(0.285)
College	0.173	0.177	0.170	0.173
	(0.379)	(0.382)	(0.376)	(0.378)
Labor Force Participation	0.747	0.747	0.740	0.754
	(0.435)	(0.435)	(0.439)	(0.431)
Currently Employed	0.940	0.945	0.940	0.934
	(0.238)	(0.227)	(0.237)	(0.249)
Never Married	0.218	0.153	0.205	0.301
	(0.413)	(0.360)	(0.404)	(0.459)
Currently Married	0.640	0.685	0.652	0.580
	(0.480)	(0.464)	(0.476)	(0.494)
Currently Divorced	0.128	0.139	0.127	0.115
	(0.334)	(0.346)	(0.333)	(0.319)
Currently Divorced among Ever Married	0.100	0.117	0.101	0.080
	(0.300)	(0.322)	(0.301)	(0.272)
Drugstores per 10,000†	2.151	2.163	2.127	2.163
	(0.521)	(0.518)	(0.515)	(0.531)
Weighted Numbers of Abortion Providers†	1.340	1.318	1.337	1.364
	(1.842)	(1.835)	(1.839)	(1.852)
Urban Status of Residency	0.635	0.618	0.635	0.653
	(0.481)	(0.486)	(0.481)	(0.476)
Black	0.136	0.132	0.135	0.141
	(0.343)	(0.339)	(0.344)	(0.348)
Income per capita†	8972	6641	8915	11512
	(2,954)	(1,539)	(2,123)	(2,787)
Total employment†	366,303	332,003	371,192	397,543
	(719,868)	(656,864)	(728,979)	(770,760)
Number of Observations	510,916	173,329	174,605	162,982

† Calculated when one was between ages 16 and 18. *Source:* 1990 US Census. Census Bureau between 1974 and 1984. County Business Patterns between 1974 and 1984. Guttmacher Institute abortion data 1979-1981. United States Bureau of Economic Analysis, Regional Economic Information System 1969-1992.

Table 6: DID, Impacts on Delaying Childbirth (Reference Cohort: 1956–58)

	Maternal Age 20–26		Maternal Age 27–33		Maternal Age 34–40	
	(1)	(2)	(3)	(4)	(5)	(6)
	Fertility Rates	Birth Order	Fertility Rates	Birth Order	Fertility Rates	Birth Order
$BC_{5961} \times Access \times \overline{D_{1618}}$	-0.413	0.004	-1.287	-0.005	0.341	-0.017
	(0.831)	(0.004)	(1.298)	(0.013)	(0.645)	(0.026)
$BC_{6264} \times Access \times \overline{D_{1618}}$	-0.888	-0.002	0.491	-0.005	2.055***	-0.045*
	(1.084)	(0.005)	(1.502)	(0.013)	(0.789)	(0.027)
$\overline{D_{1618}}$	-1.194**	-0.005*	3.758	-0.012	1.201	0.081*
	(0.544)	(0.003)	(2.683)	(0.027)	(2.073)	(0.044)
$BC_{5961} \times \overline{D_{1618}}$	0.503	0.002	-1.732	0.022*	-1.202**	0.022
	(0.325)	(0.002)	(1.082)	(0.011)	(0.479)	(0.024)
$BC_{6264} \times \overline{D_{1618}}$	1.182***	-0.001	-2.120*	0.011	0.621	0.016
	(0.419)	(0.002)	(1.253)	(0.011)	(0.568)	(0.025)
$Access \times \overline{D_{1618}}$	-4.262***	0.022*	-4.522	0.009	-0.909	-0.064
	(1.527)	(0.012)	(3.366)	(0.033)	(2.835)	(0.049)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × State FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	138.945	1.832	94.599	2.239	35.092	2.735
Adjusted R^2	0.673	0.688	0.728	0.867	0.804	0.730
Observations	172,064	172,064	24,907	24,907	25,769	25,769

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. BC_{5658} is an indicator for birth cohorts 1956–58, BC_{5961} 1959–61, and BC_{6264} 1962–64. *Access* is a dummy variable indicating having at least one abortion provider within 10 miles in a given county assigned using county-level provider data. $\overline{D}$ denotes the average drugstore numbers per 10,000 residents between ages 16 and 18. Time-varying controls include share of black, total employment and fraction of unemployment income in the conceiving year at the county levels. Estimates are two-way clustered at the county-cohort levels. Coefficients of $BC_{5658} \times Access \times \overline{D}$ are normalized as zero.

Table 7: DID, Impacts on Women's Later Life Outcomes (Reference Cohorts: 1956–58)

	(1) High School Dropout	(2) College	(3) Labor Force Participation	(4) Currently Employed	(5) Never Married	(6) Currently Married	(7) Currently Divorced	(8) Currently Divorced among Ever Married
$BC_{5961} \times Access \times \overline{D}$	0.0010	-0.0007	-0.0008	-0.0014	0.0020	-0.0003	-0.0008	-0.0006
	(0.0009)	(0.0016)	(0.0014)	(0.0009)	(0.0016)	(0.0016)	(0.0011)	(0.0013)
$BC_{6264} \times Access \times \overline{D}$	0.0005	0.0011	0.0032**	0.0013	0.0068***	-0.0004	-0.0043***	-0.0058***
	(0.0010)	(0.0016)	(0.0015)	(0.0010)	(0.0019)	(0.0018)	(0.0011)	(0.0015)
$\overline{D}$	0.0041	0.0022	0.0178**	-0.0051	0.0127	-0.0065	-0.0088**	-0.0083*
	(0.0038)	(0.0042)	(0.0073)	(0.0034)	(0.0082)	(0.0063)	(0.0038)	(0.0043)
$BC_{5961} \times \overline{D}$	-0.0021	0.0025	-0.0028	-0.0001	0.0043	-0.0003	-0.0015	-0.0019
	(0.0019)	(0.0025)	(0.0031)	(0.0020)	(0.0026)	(0.0029)	(0.0021)	(0.0024)
$BC_{6264} \times \overline{D}$	-0.0009	-0.0044*	-0.0110***	-0.0022	-0.0004	-0.0009	0.0043**	0.0040
	(0.0020)	(0.0026)	(0.0034)	(0.0022)	(0.0030)	(0.0032)	(0.0022)	(0.0026)
$BC_{5961} \times Access$	0.0005	0.0036	0.0059**	0.0047***	0.0050*	0.0021	-0.0060***	-0.0097***
	(0.0017)	(0.0028)	(0.0027)	(0.0018)	(0.0030)	(0.0029)	(0.0020)	(0.0027)
$Access \times \overline{D}$	-0.0019	0.0039	-0.0000	0.0002	-0.0037	-0.0018	0.0052**	0.0056**
	(0.0018)	(0.0024)	(0.0028)	(0.0016)	(0.0041)	(0.0034)	(0.0024)	(0.0027)
Individual-level control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying PUMA-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	0.085	0.173	0.747	0.940	0.218	0.640	0.100	0.128
Adjusted R^2	0.0324	0.0735	0.0230	0.0268	0.1302	0.1140	0.0125	0.0173
Observations	501,130	510,916	510,916	381,448	510,916	510,916	510,916	399,513

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. BC_{5658} is an indicator for birth cohorts 1956–58, BC_{5961} 1959–61, and BC_{6264} 1962–64. *Access* denotes population-weighed abortion provider numbers. $\overline{D}$ denotes the average drugstore numbers per 10,000 residents between ages 16 and 18. Time-varying controls include mean total employment and income per capita between ages 16 and 18 at the PUMA levels. Estimates are two-way clustered at the PUMA-cohort levels. Coefficients of $BC_{5658} \times Access \times \overline{D}$ are normalized as zero.

Appendices

A Additional Tables

Table A1: Impacts on Fertility Rates by Race

	(1) Black	(2) White
Trend Break	-0.411***	-0.676**
	(0.084)	(0.267)
Event Study		
β_{1974}	-0.663	-1.474***
	(0.718)	(0.219)
β_{1975}	-0.296	-0.811***
	(0.585)	(0.185)
β_{1976}	-0.549	-0.611***
	(0.439)	(0.152)
β_{1977}	0	0
β_{1978}	-0.254	0.085
	(0.535)	(0.160)
β_{1979}	-0.934	-0.016
	(0.641)	(0.179)
β_{1980}	-1.702**	0.268
	(0.810)	(0.213)
β_{1981}	-1.963**	0.453*
	(0.909)	(0.240)
β_{1982}	-2.986***	0.441*
	(0.941)	(0.261)
β_{1983}	-3.277***	0.584**
	(0.887)	(0.297)
β_{1984}	-2.965***	0.106
	(0.878)	(0.307)
County-level controls	Yes	Yes
County-specific time trend effects	Yes	Yes
Year × State FE	Yes	Yes
Mean of dep var	72.64	58.74
Adjusted R^2	0.779	0.926
Observations	12,518	22,869

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are clustered at the county levels. The coefficients of 1977 are normalized as zero.

Table A2: Impacts on First Month Prenatal Care Began

	(1)
Trend Break	0.010***
	(0.004)
Event Study	
β_{1974}	0.019**
	(0.008)
β_{1975}	0.010
	(0.008)
β_{1976}	0.007
	(0.006)
β_{1977}	0
β_{1978}	-0.006
	(0.006)
β_{1979}	-0.002
	(0.008)
β_{1980}	0.003
	(0.009)
β_{1981}	0.009
	(0.010)
β_{1982}	0.014
	(0.010)
β_{1983}	0.012
	(0.012)
β_{1984}	0.016
	(0.013)
County-level controls	Yes
County-specific time trend effects	Yes
Year × State FE	Yes
Mean of dep var	2.96
Adjusted R^2	0.858
Observations	22869.000

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are clustered at the county levels. The coefficients of 1977 are normalized as zero.

Table A3: Summary Statistics by Abortion Provider Accessibility

	(1) Access in 20 mi	(2) No access in 20 mi
Number of counties	487	1,328
Observations (county-year level)	3,896	10,624
Year	1974-1981	1974-1981
Age range	15-29	15-29
Births per 1,000 women	93.08	112.83
	(24.71)	(22.77)
Month prenatal care began	3.06	3.12
	(0.33)	(0.40)
Black	0.16	0.12
	(0.19)	(0.20)
Marital Status*	0.77	0.81
	(0.15)	(0.16)
High School Diploma*	0.73	0.73
	(0.09)	(0.12)
Drugstore numbers per 10,000 residents†	2.17	2.58
	(0.61)	(0.96)
Total employment†	102,722	13,054
	(193,087)	(13,809)
Income per capita†	6,859	5,936
	(2,007)	(1,689)
Fraction of Unemployment Insurance Income†	0.0075	0.0074
	(0.0062)	(0.0064)
County population†	202,258	31,350
	(351,854)	(31,589)
County demography†		
Percent of female population aged 15-29 that is		
black and age 15-19	0.065	0.060
	(0.081)	(0.098)
white and age 15-19	0.448	0.485
	(0.092)	(0.105)
black and age 20-24	0.057	0.047
	(0.069)	(0.077)
white and age 20-24	0.430	0.409
	(0.079)	(0.083)
black and age 25-29	0.045	0.033
	(0.055)	(0.055)
white and age 25-29	0.396	0.381
	(0.091)	(0.089)

* For selected stated that reported the information. † Calculated for the year of conceiving. List of states with missing prenatal care information by years is provided in Appendix C.3. Cutoffs of years are set to be August, i.e., the year of 1978 ranges from August 1978 to July 1979.

Source: Vital Statistics Natality Birth Data between August 1974 and July 1985. Census Bureau between 1974 and 1984. County Business Patterns between 1974 and 1984. Guttmacher Institute abortion data 1979-1981. United States Bureau of Economic Analysis, Regional Economic Information System 1969-1992.

Table A4: Impacts on Fertility Rates by Abortion Provider Accessibility

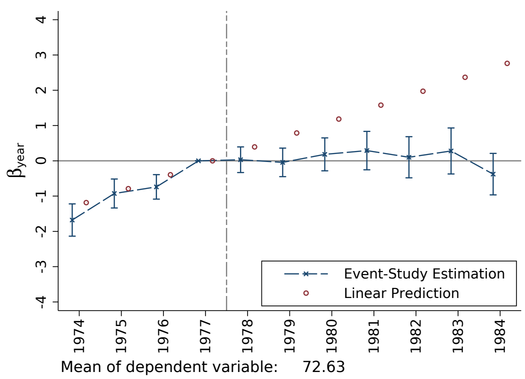
	(1) No Access	(2) Access in 20mi
Trend Break	-0.892**	-0.004
	(0.348)	(0.203)
Event Study		
β_{1974}	-2.013***	-2.109***
	(0.439)	(0.759)
β_{1975}	-1.156***	-1.453**
	(0.444)	(0.574)
β_{1976}	-1.199***	-0.461
	(0.375)	(0.433)
β_{1977}	0	0
β_{1978}	0.169	0.049
	(0.362)	(0.493)
β_{1979}	0.585	-0.311
	(0.400)	(0.594)
β_{1980}	1.450***	-0.698
	(0.464)	(0.661)
β_{1981}	1.870***	-0.361
	(0.490)	(0.795)
Time-varying controls	Yes	Yes
County-specific time trend effects	Yes	Yes
Year × State FE	Yes	Yes
Mean of dep var	100.19	83.22
Adjusted R^2	0.872	0.972
Observations	10,608	3,888

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. The coefficients of 1977 are normalized as zero.

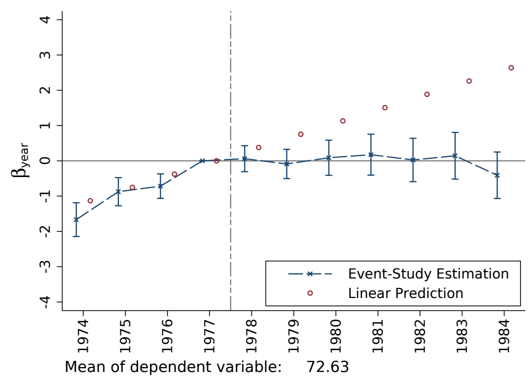
B Additional Analyses

B.1 Additional Definitions for the Drugstore Accessibility

In this section, I provide additional analyses adopting two other different definitions for drugstore accessibility while examining fertility rates. First, instead of taking the average over my study period, I introduce within-county variation and use raw county-level drugstore numbers as the proxy to home pregnancy test availability. Second, I take the average over only the pre-period (1974-1977) to further prevent reverse causality. Figures B1a and B1b display robust results for both proxies. Table B1 also presents robust and detailed estimates.



(a) Within-county Yearly Variation



(b) Pre-period (1974-1977) Average

Figure B1: Impacts on Fertility Rates Using Additional Definitions for Drugstore Accessibility

The estimation follows Equation 1. The full set of specifications are reported in Table B1. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero.

B.2 Parental Consent for Abortion and Impacts on Fertility Rates among Women Aged 15-19

The variation in ages where minors could get an abortion without parental consent across the 70s impacted fertility rates among young women (Hock et al. 2007, Guldi 2008, Myers 2017, 2022). In July 1976, the Supreme Court ruled parental consent laws without health exceptions for abortion to be unconstitutional in the case of

Planned Parenthood of Central Missouri v. Danforth. In July 1979, the Supreme Court further invalidated parental consent laws with health exceptions but without judicial-bypass features in the *Bellotti v. Baird*. The increased access to abortion among young women due to law changes might result in the trend break found among women aged 15 to 19 in Figure 5b. This is likely especially when many states started to change parental consent laws after 1976, only one year before the FDA approved home pregnancy tests.

To exclude this concern, I stratify my sample of women aged 15-19 into two groups by whether unmarried women aged 15-17 could get access to abortion after 1976 in their states of residence using Myers's legal coding (Myers 2017, 2022).[1] Figure B2 displays that the two groups had similar trends across my study period, which implies the trend breaks were not driven by changes in legal and confidential access to abortion among minors. The findings are consistent with Figure 8 where there was no trend break in abortion ratio among women aged 15-19 around 1976. Moreover, in Section 4.4, I show that the effects of confidential access to abortion on reducing fertility rates are stronger when complemented by access to home pregnancy tests.

B.3 Falsification Test: Substituting Drugstore Accessibility with Access to Other Retail Stores

I present a falsification test to rule out the possibility that the trend breaks I find in Figure 5 are driven by an arbitrary choice of the proxy to home pregnancy test accessibility. Namely, the falsification test aims to show that substituting drugstore accessibility with access to other retail stores does not lead to the same findings. I choose three categories of retail stores that cover a wide range of varieties—apparel and accessory stores, home furniture and furnishings stores, and eating and drinking places because they are unlikely to correlate with the drugstore accessibility and thus home pregnancy test availability.[2] I define their accessibility likewise: the average number of each store type per 10,000 residents in each county between 1974 and

[1] The law changes in the late 70s did not affect women above the age of 18. See Table 1 of Myers (2017) and Table 2 of Myers (2022).

[2] I identify retail apparel and accessory stores using SIC codes 5600, 5690, and 5699, home furniture and furnishings stores 5700, 5710, 5712, and 5720, and eating and drinking places 5800, 5812, and 5813. For the detailed SIC code list, see Appendix B of Technical Documentation for County Business Patterns (CPB), 1974-1986.

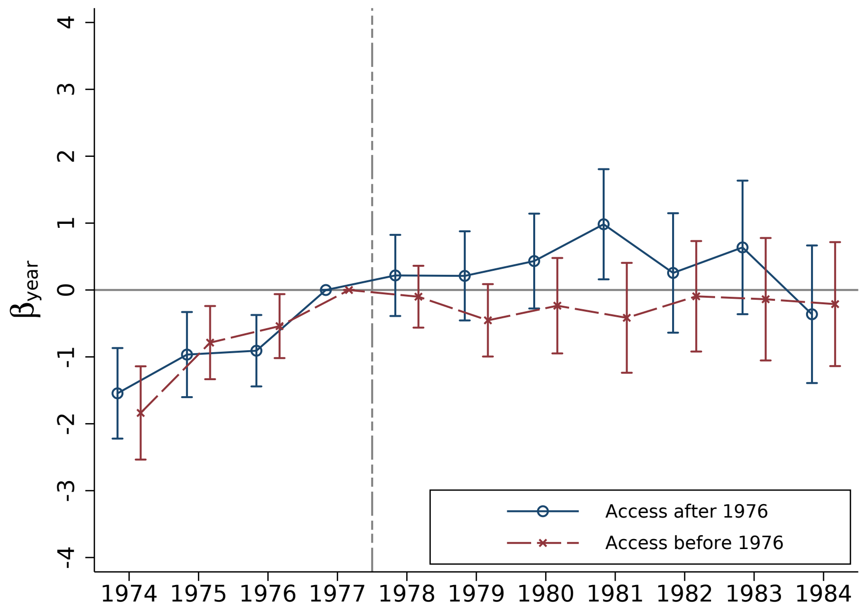


Figure B2: Impacts on Fertility Rates by Legal and Confidential Access to Abortion

The full set of specifications are reported in Table B2. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero. List of states with missing prenatal care adoption by years is provided in Appendix C.3.

1984.

Figures B3a to B3c display no trend break around 1977 when I conduct the same analysis following Equation 1 and substitute the drugstore accessibility with access to these four other retail stores. Although in Figure B3b, there seems to be a jump in 1978 when using home furniture and furnishings store accessibility as the proxy, none of the point estimates are significantly different from the reference year (β_{1977}). The falsification test confirms that my main findings are not the results of cherry-picking a proxy for home pregnancy test availability.

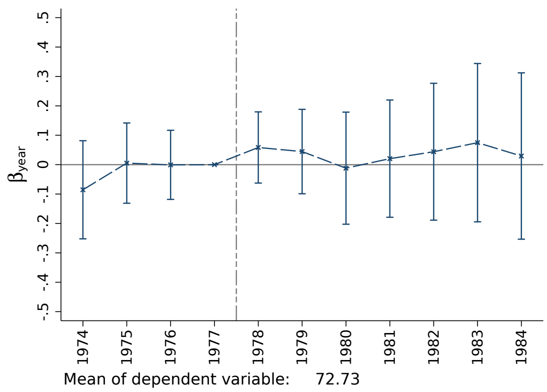
B.4 Mechanism of Fertility Trend Break: Abortion Provider Accessibility in 15 and 25 Miles

To examine whether abortion was the channel through which home pregnancy tests caused the trend break in fertility rates, I divide sample women aged 15–29 into two groups by the distance to the nearest abortion providers at the county level. When using 20 miles as the distance cutoff, I find that the trend break only persists among women in counties with greater access to abortion, as shown in Figure 8. In this section, I further change the distance cutoff for the group assignment from 20 miles to 15 and 25 miles. Figure B4 shows robust results where impacts of home pregnancy tests only persist when women had better access to abortion.

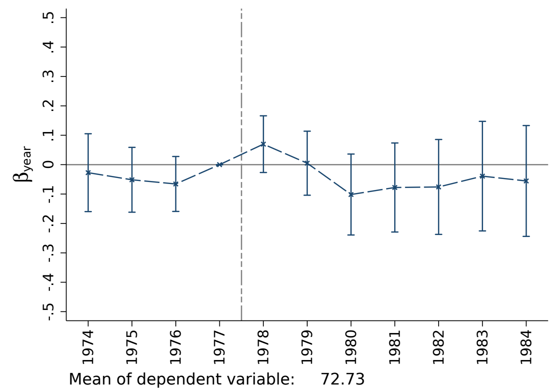
B.5 Relaxing Sample Restriction on Birth Place and Current Residence

As described in Section 5.1, I use the 1990 Census to test women's later-life outcomes in my main analyses, and I restrict my sample to women who still lived in their birth states and did not move within the last five years. This is because following Equation 5, the drugstore and abortion provider accessibility is defined as the exposure around 1977 at the PUMA level. Not restricting my sample based on residency could introduce bias and lead to a less precise estimation. However, this restriction can also make my sample selective. For comparison, here I release the sample restrictions on women's birth state, current residence, and moving history.

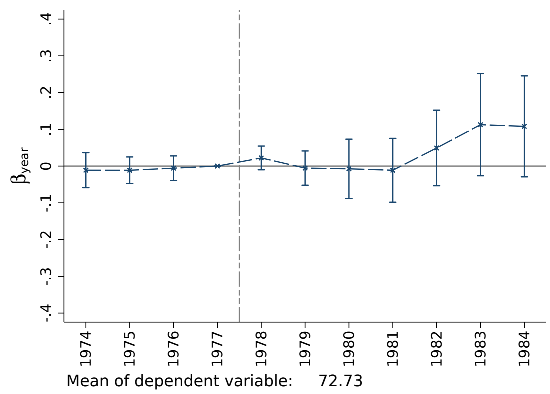
Table B5 reports the results. Consistently, access to home pregnancy tests significantly increased the likelihood of obtaining labor market participation and never entering marriage and decreased the odds of divorce among women in areas with access to abortion services. The effect sizes are comparable to the estimates from



(a) Apparel and Accessory Stores



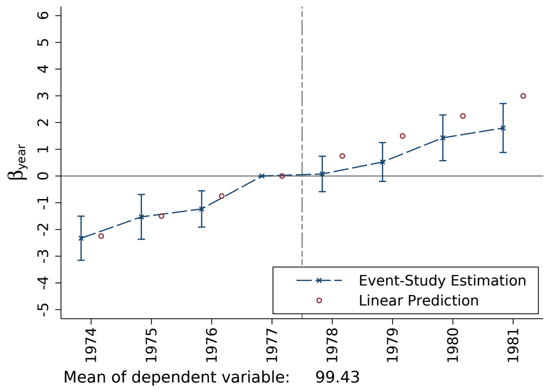
(b) Home Furniture and Furnishing Stores



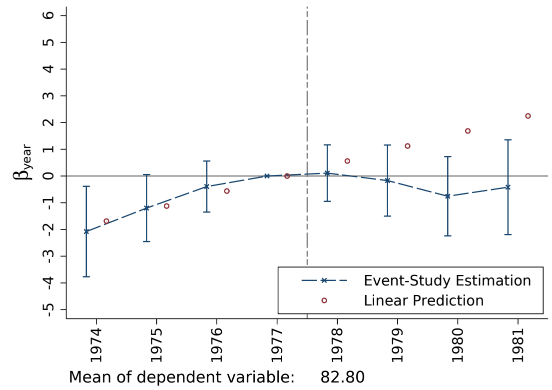
(c) Eating and Drinking Places

Figure B3: Impacts on Fertility Rates Substituting Drugstore Accessibility with other Retail Store Accessibility

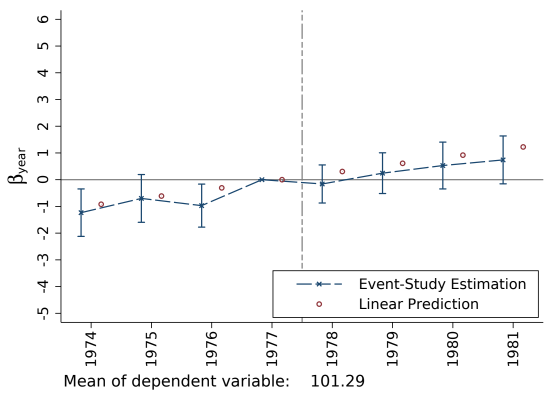
The full set of specifications are reported in Table B3. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero.



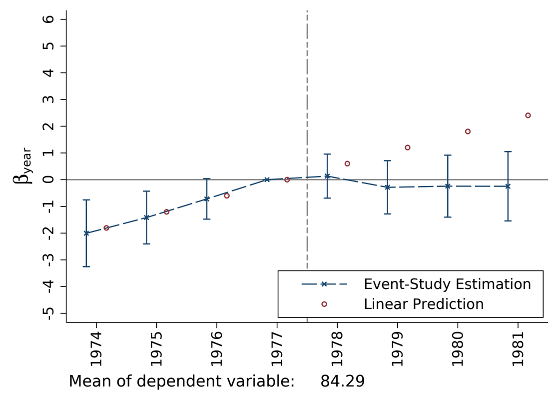
(a) No Access in 15mi



(b) With Access in 15mi



(c) No Access in 25mi



(d) With Access in 25mi

Figure B4: Impacts of Home Pregnancy Test Availability on Fertility Rates By Abortion Provider Accessibility in 15 and 25 Miles

The full set of specifications are reported in Table B4. Total employment, income per capita, fractions of unemployment insurance income, and demographic composition in the year of conception at the county levels are controlled. County and state-by-year fixed effects are controlled. Estimates are weighted by the number of women in the given age range residing in the county and clustered at the county levels. Confidence intervals are at the 95% level. The coefficients of 1977 are normalized as zero.

Tables 7. Notably, Column (2) shows positive and significant impact on obtaining college degrees. The findings are robust.

Table B1: Impacts on Fertility Rates Using Additional Drugstore Accessibility Definitions

	(1) Yearly Variation	(2) Pre-period Mean
Trend Break	-0.550***	-0.552***
	(0.088)	(0.089)
Event Study		
β_{1974}	-1.678***	-1.666***
	(0.233)	(0.244)
β_{1975}	-0.926***	-0.875***
	(0.209)	(0.203)
β_{1976}	-0.738***	-0.717***
	(0.177)	(0.177)
β_{1977}	0	0
β_{1978}	0.031	0.061
	(0.185)	(0.188)
β_{1979}	-0.044	-0.090
	(0.206)	(0.211)
β_{1980}	0.183	0.087
	(0.238)	(0.254)
β_{1981}	0.290	0.174
	(0.277)	(0.296)
β_{1982}	0.101	0.024
	(0.297)	(0.313)
β_{1983}	0.279	0.144
	(0.332)	(0.337)
β_{1984}	-0.377	-0.409
	(0.300)	(0.336)
Time-varying controls	Yes	Yes
County-specific time trend effects	Yes	Yes
Year-by-State FE	Yes	Yes
Mean of dep var	72.63	72.63
Adjusted R^2	0.930	0.930
Observations	22,869	22,869

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimation follows Equation 1. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are clustered at the county levels. The coefficients of 1977 are normalized as zero.

Table B2: Impacts on Fertility Rates by Legal and Confidential Access to Abortion

	(1) Access before 1976	(2) Access after 1976
Trend Break	-0.557***	-0.511***
	(0.131)	(0.135)
Event Study		
β_{1974}	-1.838***	-1.545***
	(0.355)	(0.345)
β_{1975}	-0.787***	-0.966***
	(0.279)	(0.324)
β_{1976}	-0.540**	-0.908***
	(0.244)	(0.272)
β_{1977}	0	0
β_{1978}	-0.100	0.218
	(0.236)	(0.309)
β_{1979}	-0.453	0.211
	(0.276)	(0.340)
β_{1980}	-0.235	0.433
	(0.363)	(0.361)
β_{1981}	-0.416	0.983**
	(0.419)	(0.420)
β_{1982}	-0.094	0.255
	(0.421)	(0.455)
β_{1983}	-0.138	0.637
	(0.467)	(0.509)
β_{1984}	-0.211	-0.363
	(0.472)	(0.524)
County-level controls	Yes	Yes
County-specific time trend effects	Yes	Yes
Year $\times$ State FE	Yes	Yes
Mean of dep var	70.65	76.17
Adjusted R^2	0.918	0.941
Observations	12,683	10,186

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimation follows Equation 1. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are clustered at the county levels. The coefficients of 1977 are normalized as zero.

Table B3: Impacts on Fertility Rates Substituting Drugstore Accessibility with other Retail Store Accessibility

	(1) Apparel & Accessory	(2) Furniture	(3) Eating and Drinking
Trend Break	-0.023	-0.023	0.016
	(0.036)	(0.029)	(0.015)
Event Study			
β_{1974}	-0.085	-0.027	-0.011
	(0.085)	(0.068)	(0.024)
β_{1975}	0.005	-0.051	-0.011
	(0.070)	(0.056)	(0.018)
β_{1976}	-0.001	-0.066	-0.006
	(0.060)	(0.048)	(0.017)
β_{1977}	0	0	0
β_{1978}	0.058	0.070	0.022
	(0.062)	(0.049)	(0.016)
β_{1979}	0.045	0.005	-0.005
	(0.073)	(0.056)	(0.024)
β_{1980}	-0.012	-0.102	-0.007
	(0.097)	(0.070)	(0.041)
β_{1981}	0.020	-0.078	-0.011
	(0.102)	(0.077)	(0.044)
β_{1982}	0.044	-0.076	0.049
	(0.119)	(0.082)	(0.052)
β_{1983}	0.075	-0.039	0.113
	(0.137)	(0.095)	(0.071)
β_{1984}	0.029	-0.055	0.108
	(0.144)	(0.096)	(0.070)
County-level controls	Yes	Yes	Yes
County-specific time trend effects	Yes	Yes	Yes
Year × State FE	Yes	Yes	Yes
Mean of dep var	72.73	72.73	72.73
Adjusted R^2	0.928	0.928	0.928
Observations	22,869	22,869	22,869

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimation follows Equation 1. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are clustered at the county levels. The coefficients of 1977 are normalized as zero.

Table B4: Impacts on Fertility Rates by Abortion Accessibility in 15 and 25 Miles

	15 miles		25 miles	
	No Access	With Access	No Access	With Access
	(1)	(2)	(3)	(4)
Trend Break	-0.124	-0.918**	-0.047	-0.781***
	(0.195)	(0.397)	(0.219)	(0.290)
Event Study				
β_{1974}	-2.328***	-2.078**	-1.234***	-2.005***
	(0.421)	(0.860)	(0.452)	(0.636)
β_{1975}	-1.529***	-1.199*	-0.701	-1.416***
	(0.427)	(0.637)	(0.456)	(0.502)
β_{1976}	-1.232***	-0.394	-0.971**	-0.719*
	(0.346)	(0.486)	(0.410)	(0.386)
β_{1977}	0	0	0	0
β_{1978}	0.077	0.109	-0.161	0.133
	(0.338)	(0.538)	(0.363)	(0.420)
β_{1979}	0.525	-0.170	0.244	-0.285
	(0.370)	(0.676)	(0.388)	(0.507)
β_{1980}	1.430***	-0.756	0.530	-0.242
	(0.436)	(0.754)	(0.446)	(0.591)
β_{1981}	1.796***	-0.418	0.740	-0.246
	(0.467)	(0.902)	(0.456)	(0.660)
Time-varying controls	Yes	Yes	Yes	Yes
County-specific time trend effects	Yes	Yes	Yes	Yes
Year × State FE	Yes	Yes	Yes	Yes
Mean of dep var	99.43	82.80	101.29	84.29
Adjusted R^2	0.873	0.975	0.862	0.966
Observations	11,440	3,056	8,864	5,616

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The estimation follows Equation 1. Time-varying controls include total employment, income per capita, and fraction of unemployment insurance income in the year of conception at the county levels. Estimates are clustered at the county levels. The coefficients of 1977 are normalized as zero.

Table B5: DID, No Restrictions on Residency, Impacts on Women's Later Life Outcomes (Reference Cohorts: 1956–58)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High School Dropout	College	Labor Force Participation	Currently Employed	Never Married	Currently Married	Currently Divorced	Currently Divorced among Ever Married
$BC_{5961} \times Access \times \overline{D}$	0.0004	-0.0015	0.0011	-0.0007	0.0020	-0.0008	-0.0006	-0.0004
	(0.0006)	(0.0012)	(0.0013)	(0.0006)	(0.0014)	(0.0015)	(0.0008)	(0.0010)
$BC_{6264} \times Access \times \overline{D}$	-0.0007	0.0024*	0.0038***	0.0011	0.0059***	-0.0014	-0.0030***	-0.0038***
	(0.0007)	(0.0013)	(0.0015)	(0.0007)	(0.0017)	(0.0017)	(0.0008)	(0.0011)
$\overline{D}$	-0.0002	0.0065*	0.0111**	-0.0021	0.0047	-0.0043	-0.0024	-0.0016
	(0.0035)	(0.0037)	(0.0056)	(0.0027)	(0.0073)	(0.0054)	(0.0032)	(0.0034)
$BC_{5961} \times \overline{D}$	-0.0011	0.0051**	-0.0033	-0.0009	0.0045*	-0.0031	-0.0003	-0.0003
	(0.0015)	(0.0021)	(0.0027)	(0.0016)	(0.0023)	(0.0024)	(0.0017)	(0.0019)
$BC_{6264} \times \overline{D}$	0.0017	-0.0021	-0.0132***	-0.0019	0.0047*	-0.0078***	0.0043**	0.0055***
	(0.0016)	(0.0023)	(0.0028)	(0.0017)	(0.0027)	(0.0027)	(0.0018)	(0.0020)
$BC_{5961} \times Access$	-0.0006	0.0067***	0.0032	0.0032***	0.0042	0.0009	-0.0039***	-0.0058***
	(0.0012)	(0.0021)	(0.0027)	(0.0012)	(0.0029)	(0.0029)	(0.0014)	(0.0020)
$Access \times \overline{D}$	-0.0010	0.0002	-0.0003	-0.0003	-0.0026	-0.0007	0.0030**	0.0031**
	(0.0012)	(0.0024)	(0.0027)	(0.0010)	(0.0038)	(0.0034)	(0.0014)	(0.0014)
Individual-level control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying PUMA-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	0.077	0.218	0.747	0.941	0.209	0.647	0.102	0.129
Adjusted R^2	0.0315	0.0986	0.0227	0.0249	0.1254	0.1030	0.0116	0.0154
Observations	809,109	822,687	822,687	614,620	822,687	822,687	822,687	650,364

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. BC_{5658} is an indicator for birth cohorts 1956–58, BC_{5961} 1959–61, and BC_{6264} 1962–64. *Access* is a dummy variable indicating greater access to abortion providers within a specific cutoff, assigned using county-level provider data. $\overline{D}$ denotes the average drugstore numbers per 10,000 residents between ages 16 and 18. Time-varying controls include mean total employment and income per capita between ages 16 and 18 at the PUMA levels. Estimates are two-way clustered at the PUMA-cohort levels. Coefficients of $BC_{5658} \times Access \times \overline{D}$ are normalized as zero.

C Supplementary Materials

C.1 The Advertising Strategies of Home Pregnancy Tests

The advertising campaign hit major feminine magazines and popular TV shows nationally in 1978.[3] Leavitt (2006) summarizes that the ads in magazines signaled two targeted customer groups differently. The first targeted group was women who were younger, sexually active, and not planning to conceive. In magazines that targeted young women (e.g., Vogue, Mademoiselle, etc.), the ads were usually found next to birth controls ads and emphasized private, accurate, and fast results with more scientific languages, as shown in Figure C1a. Using this product gave the users more time to make decisions and made them more likely to meet the gestational limits for abortion if they chose to have an abortion, which could affect fertility rates.

The second targeted group was women who were planning to conceive. In magazines that had women planning to conceive or women who were mothers as primary readers (e.g., Ladies' Home Journal, American Journal of Public Health, etc.), the ads were usually right next to baby product ads and emphasized that early recognition could bring earlier prenatal care, shown as Figure C1b. To test its behavioral impacts, I thus examine whether women began their prenatal care earlier after home pregnancy tests became available.

The identification of this paper relies on the fact that most users purchased such products in drugstores. The indication in the ads further supports this identification strategy. The ads directed the customers straight to the local drugstores:

> *The e.p.t In-Home Early Pregnancy Test is a* ***private little revolution*** *any woman can easily buy at her* ***drugstore****...Now its high* ***accuracy rate*** *has been verified here in America by doctors...That means you can confidently do this easy pregnancy test yourself—****privately****—right* ***at home*** *without waiting for appointments or delays...At last early knowledge of pregnancy belongs* ***easily and accurately*** *to us all.* (print advertisement for e.p.t, March 1978.)

To conclude, first, the marketing strategy that signaled two targeted customer

[3] The two leading manufacturers, Ortho Pharmaceuticals and Warner Lambert, were estimated to have spent eight million in total for their advertising campaign in 1986 (Winters 1986).

groups different information supports my hypotheses: the use of home pregnancy tests could affect the timing of prenatal care initiation and fertility rates. Second, the ads directing women to their neighborhood drugstores for their purchases made local drugstore numbers the source of variation for my identification.

(a) Vogue, March 1978

(b) American Journal of Public Health, January 1979

Figure C1: Home Pregnancy Test Ads in Magazines Targeting Two Groups of Customers
Source: The Thin Blue Line, Office of NIH History & Stetten Museum

C.2 Oral Contraceptive Pill Use in the Study Period

In Figure 1a, I use the National Survey of Family Growth, Cycle III, 1982 to show the fraction of taking the pill at one's first intercourse from 1960 to 1982. The uptake trends remained between 11% and 15% after 1970, which implies that drugstore accessibility did not correspond to the prevalence of oral contraceptive use in my study period.

In order to present the prevalence of pill usage over time, an alternative is to collect information on such topics from different cycles of surveys. Using the National Fertility Survey 1965 and National Survey of Family Growth 1973, 1976, and 1982, Figure C2 displays age-adjusted fractions of taking the pill as the main contraception among women 15-44 years of age who were not pregnant nor trying to be pregnant. Consistently, 25% to 33% of the sample used the pill as the main method between 1973 and 1982. Although Figure C2 delivers message similar to Figure 1a, it is still important to keep in mind that some drawbacks make this alternative more biased compared to using the fraction of taking the pill at one's first intercourse as the prevalence. The primary concerns include but not limited to: (1) the sample composition changed in each cycle[4] and (2) the questions asked and the answers to choose from are slightly different across these years.

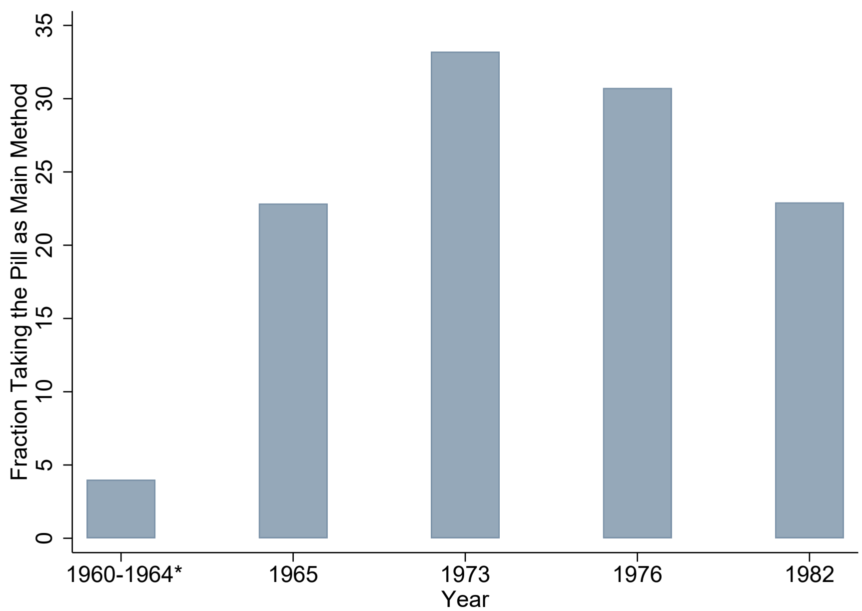


Figure C2: Using Oral Contraceptive Pill as Main Contraceptive Method

Fractions of 1965, 1973, 1976, and 1982 are age-adjusted based on the 1982 sample. Only the 1982 sample contains never-married women. *Pratt et al. (1984) and Mosher & Bachrach (1987) states that the share of the pill usage among women aged 15-44 was around 4%.
Source: National Fertility Survey, 1965. National Survey of Family Growth, Cycle I, 1973; Cycle II, 1976; and Cycle III 1982.

[4] Besides the demogrphic composition, the National Fertility Survey 1965 and National Survey of Family Growth 1973 and 1976 only cover women aged 15-44 who were ever married. While National Survey of Family Growth 1982 interviewed women regardless of marital status.

C.3 Vital Statistics Natality Birth Data State Coverage

I set the cutoff of a year at August because home pregnancy tests entered the US market in November 1977. The nine to ten months gestational length makes August a reasonable cutoff. Precisely, my study period is from August 1974 to July 1985, and year 1974 refers to the period from August 1974 to July 1974. Some states in Vital Statistics Natality Birth Data did not report information on prenatal care yearly between August 1974 and July 1985. Table C1 reports states with missing information by years, where years follow the cutoff that I set. I exclude data of states marked in Table C1 ao that I have balanced sample.

Table C1: States with Missing Information On Prenatal Care by Years

State	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984
Alabama	×	×	×									
Alaska	×	×	×	×	×							
Arkansas	×	×	×	×	×							
Idaho	×	×	×	×	×							
Massachusetts	×	×	×	×	×							
New Mexico	×	×	×	×	×	×	×					
Pennsylvania	×	×	×	×	×							
Virginia	×	×	×	×	×							

Source: Vital Statistics Natality Birth Data between August 1974 and July 1985.