Power of the Pill? Re-Examining the Effect of Birth Control and Abortion Access on Educational Outcomes for Women

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October 2, 2024

Abstract

Using the rollout of early access to birth control laws across states and time, previous work has suggested that birth control increases educational attainment and entry into professional occupations for women, likely due to the ability to delay childbirth and marriage (Goldin and Katz, 2002; Hock et al., 2007; Bailey et al., 2012). I revisit this work using newly defined legal coding and heterogeneous robust estimators to determine whether reproductive control has an effect on educational outcomes for women. With American Community Survey data, I find the effect of early access to birth control on college completion is not robust to the use of event studies or heterogeneous robust estimators in contrast with other TWFE methods. I also do not find sufficient evidence early access to birth control affects the propensity to select male-dominated college majors. Finally, there are significant pre-trends in abortion laws that question the use of their rollout across states and time in differences-in-differences specifications.

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1 Introduction

Relative to previous generations, women coming of age in the 1960s-70s had higher rates of college enrollment and completion, had stronger attachment to the workforce and different views of marriage and divorce (Goldin and Katz, 2002; Bailey, 2006; Stevenson and Wolfers, 2007). Some commentators and researchers have claimed that early access to oral birth control (the Pill), which became widely available in the 1960s-1970s, contributed greatly to women's ability to delay childbirth and pursue increased educational attainment (Goldin and Katz, 2002; Myers, 2017b). In fact, previous research using variation in the rollout of early access to (oral) birth control across states has found women delay childbirth, increase college enrollment and completion, change professions, and improve their long-run wages as a result of having access to birth control during crucial periods of adolescence and young adulthood (Goldin and Katz, 2002; Hock et al., 2007; Ananat and Hungerman, 2012; Bailey et al., 2012).

However, there have been recent advancements in the literature that may call into question prior conclusions. First, there has been a substantial revision of legal dates for early access to birth control and abortion laws. Using sources that include annotated statutes, judicial rulings, attorney general opinions, and advisory articles in state medical journals and law reviews, newspaper accounts of legal changes, clinic openings, hospital policies, and enforcement actions, Myers (2017b) and corresponding paper, Myers (2017a), provide a revised version of birth control laws that are substantially different than those used in prior work. Once accounting for the revised early access to birth control laws as well as abortion, previous research on delayed marriage and childbirth is no longer robust with respect to birth control. Instead, Myers (2017b) finds abortion plays a much larger role of shaping fertility and family formation. These findings alone suggest other economic outcomes for women should be revisited using the revised legal codings and additionally include abortion laws.

Second, there have been substantial empirical advancements in designs that use variation in treatment across units and time. In particular, recent work shows that under heterogeneous treatment effects, the traditional TWFE models may not uncover the true treatment

effect (De Chaisemartin and d'Haultfoeuille, 2022). This is pertinent in the case of early access to birth control and abortion which both rollout at different times in different states. Previous research uses the variation in the timing of these laws to identify an effect using TWFE models. Thus, it is important to determine if prior work is robust to alternative estimators.

Using American Community Survey (ACS) data from 2009-2019 and Myers (2017b)'s legal coding, I test whether access to birth control and abortion in late adolescence has an effect on college completion and majoring in a male-dominated field. Like previous work, I rely on the variation across states and time in access to birth control or abortion as an adolescent. I use standard differences-in-differences techniques from OLS regressions to compare and test the robustness of my results to previous work on college completion under the revised legal codings and with the addition of comprehensive abortion access. I also consider an entirely new outcome relative to previous work - college major choice. Finally, I use event studies and heterogeneous robust estimators including Callaway and Sant'Anna (2021) and Sun and Abraham (2021) to compare to standard TWFE OLS models used previously.

I find neither college completion nor pursuit of male-dominated college majors are robustly associated with early birth control or abortion access. Under the revised legal coding and TWFE methods, I find that birth control increases college completion, consistent with the findings in Hock et al. (2007) and Ananat and Hungerman (2012). However, this effect is not robust using event studies or heterogeneous robust estimators. I do not find any consistent evidence that birth control increased women's propensity to select male-dominated college majors. This contrasts with previous work suggesting that women increasingly entered "professional" work (Goldin and Katz, 2002; Bailey et al., 2012). Finally, I find pre-trends when using the rollout of abortion laws on both bachelor's completion and choice of majoring in male-dominated fields, suggesting these results do not follow required assumptions for a causal interpretation.

While I do not find robust results on educational outcomes, I cannot rule out definitively that birth control and abortion access have no effect on educational attainment for women. In light of recent changes to women's access to both birth control and abortion, this work

highlights the need for additional research in this area (Lindo et al., 2020). Future work may benefit from additionally considering anticipatory effects of birth control or alternative natural experiments, for instance.

2 Background and Empirical Framework

Despite the introduction of the Pill in 1960, unmarried women under the age of 21 - an age when many women would still be in school or college - were often not allowed access to birth control without parental consent under state law (Goldin and Katz, 2002). Over the course of the following decades, states updated laws regarding legal age of majority both across the board and with a specific focus on medical consent. These legal changes provided women ages 18-20 with differential access to birth control across states and time. For instance, at age 18 a woman born in 1951 living in Maine would have access to birth control legally while the same aged woman born in 1951 living in Maryland would have to wait two more years before legally accessing birth control at age 20. A similar phenomena occurred with abortion access.

The ages these law changes affected, 18-20, are particularly important for making long-term educational decisions, and the ability to delay family formation could allow young women to pursue more education or different careers that may have more demanding requirements. Thus, laws providing early access to reproductive control have the possibility to influence long-term educational decisions for women in young adulthood. These early access laws were staggered across states and time which provides variation that previous research has used to test whether access to reproductive control during this crucial age has any influence on the ultimate educational decisions of women.

I also use this framework to test similar conclusions using recently revised legal coding for when women gained access to birth control and abortion. I define my legal coding off of Myers (2017b) Table 1 because this is the most thorough and comprehensive investigation into early access to birth control and abortion to date - also see accompanying article Myers (2017a).

My empirical analysis takes two forms to study reproductive control on educational out-

comes for women. In what follows, I outline a TWFE model that is similar to previous works' empirical analysis. This allows for a straightforward comparison of previous studies work on college completion using 1) updated legal coding and 2) an additional focus on abortion rights. Next, I estimate both event studies and recently developed heterogeneous robust estimators. This allows comparison of the traditional TWFE models to updated methodological approaches to see if there is consistency across the two empirical methods. All of the following empirical analyses take advantage of the variation in access to both birth control and abortion on educational outcomes using the staggered rollout of early access to reproductive control across states and cohorts. Finally, I also consider a new outcome: share of college majors completed in male-dominated fields.

2.1 TWFE

I begin with the following OLS TWFE model:

$$O_{sy} = \alpha + \beta_{bc}\text{BirthControl}_{sy} + \beta_{ab}\text{Abortion}_{sy} + \gamma_s + \eta_y + \theta X_{sy} + \ _{sy} \tag{1}$$

Outcome, O_{sy}, is either share of the population completing at least a bachelor's degree or share of bachelor's degrees completed in male-dominated college majors in birth state s and birth year y for women. The treatment variables are $BirthControl_{sy}$ and $Abortion_{sy}$ which take value of one for the first and subsequent birth cohorts y in birth state s that have access to birth control/abortion when they are 18 years old and zero otherwise. I choose age 18 opposed to age 20 (most laws reduced the age of consent from 21+ to 18+) because ages 18 and younger are likely more influential on post-secondary educational outcomes. In fact, 20 year olds may already have completed their educational attainment before obtaining access to legal birth control or abortion. I also include state fixed effects γ_s and year fixed effects η_y. Additional controls, X_{sy}, include state-specific linear trends, indicators for equal pay law and the fair employment practices act, as well as the share of women who are white, Black, and Hispanic.[1] I weight these regressions by cell size. The weighting makes my regressions

[1]I obtain the equal pay law, and fair employment practices act from Myers (2017b). I set these equal to 1 if these laws were in affect at age 18 for a cohort. When there are missing values for these laws, I opt to count them as not in effect (0 for all cohorts).

comparable to previous work using linear probability models. I cluster my standard errors at the state-of-birth level.

I run all models on data pulled from American Community Surveys (ACS) for years 2009-2019. I collapse down to birth cohort and state-of-birth cells using the person weight and aggregating across all survey years. I use the ACS because it started reporting college majors beginning with the 2009 survey. Male-dominated majors include physics, chemistry/pre-med, geology/other sciences, engineering, economics, finance/business/accounting, pre-pharmacy, pre-law, actuarial science, agriculture, and architecture.[2] For the primary analysis, I keep birth cohorts that range from 1930 to 1958 to keep with previous work - see Table 1.

Unlike some prior research, my empirical analysis does not use age fixed-effects. Because I use recent ACS data and not older Census samples, women in my sample relative to previous works' samples are well past the typical age of college completion. In particular, the oldest cohorts affected by early birth control laws are those born in 1942 - see Table 2. These women are aged 67-77 in the ACS samples. Meanwhile, the youngest cohorts affected by early birth control (born in 1958 for my sample) are ages 51-61. In either case, individuals do not typically complete college degrees past their 50s. For this reason, I do not see a need to account for college completion-by-age like previous work which studied women who were young at the time of the survey. One potential drawback of using these ACS samples relative to the older Census samples is survivorship bias. It is possible, especially considering the share of the population with a bachelor's degree, that more educated women live longer than less educated women and as such the ACS disproportionately surveys older, educated women than older, uneducated women. In all, this should inflate my estimates for bachelor's share more than previous work, but I find the opposite. This may suggest the bias is minimal.

Alternatives to equation 1 replace the binary indicators for early access to birth control and abortion with the exposure to birth control laws and abortion laws between ages 14 and 18, based on Myers (2017b). Specifically, it is the number of years between ages 14 and 18 that a cohort y would have lived in their birth state s with legal birth control but no access for their age; lived in a state s with legal birth control for their age; lived in a state s with

[2]This is based off of whether males made up 70 percent or more of the college major based on birth cohorts from 1920-1944.

legal abortion but no access for their age; lived in a state s with legal abortion for their age - see Myers (2017b) for more detail.[3] These alternative variables provide a few additional advantages. First, they include changes to the law that expand access to birth control and abortion to those younger than 18 years old. Second, they account for cumulative exposure to these laws which may matter if we think there are effects of spreading information or slower shifting social acceptance. Finally, they document differing events - those exposed (or not at all) to any birth control/abortion, and those exposed at an earlier age (Myers, 2017b).

The main identifying assumptions in both of these TWFE specifications require that women who obtain access to birth control or abortion in a given year would have experienced the same changes to their educational outcomes as women who have not-yet-gained access to birth control or abortion if they had not experienced a change in the law (parallel trends). Because many of the early access laws were due to changes in the legal age of majority (21 to 18), it may be plausible these policies were truly "randomly" placed among states that would have otherwise experienced similar national trends (Myers, 2017b). On the other hand there were several policy changes occurring in the 1960s and 1970s that may have been packaged together with birth control (Bailey et al., 2012; Myers, 2017b). Like previous work, I try to capture some of these changes by controlling for anti-discrimination laws and abortion in the case of birth control and vice versa (Myers, 2017b).

One way to test these trends is through event studies. I focus on either birth control or abortion treatment effects in an event study framework while controlling for the other as a covariate. The event study specifications depicted below also control for state-specific linear trends, indicators for equal pay law and the fair employment practices act, and the share of women who are white, Black, and Hispanic. I bin any years more than seven years prior to the treatment and any years more than 10 years post-treatment for birth control. I bin any years more than 15 years prior to the treatment and any years more than six years post-treatment for abortion effects. For birth control, I run the following equation where I

[3] I obtain these variables from Myers (2017b)'s replication package found here: https://www.openicpsr.org/openicpsr/project/136181/version/V1/view. I say "lived" in a state by instrumenting birth state for where a person resided their adolescence.

is the period in relationship to the first treated birth year and the rest is as in equation 1:

$$\mathrm{O}_{sy} = \sum_{l=-7/-1}^{10} \beta_{bc}^{l} I\,(\text{l periods from BC})_{sy}^{l} + \beta_{ab}\mathrm{Abortion}_{sy} + \gamma_s + \eta_y + \theta X_{sy} + \quad _{sy} \qquad (2)$$

This can be re-written similarly when considering abortion as the primary treatment outcome with modified bins. All regressions cluster standard errors at the state-of-birth level.

Ideally these event studies will trace out the dynamic effects of pre- and post-law changes for both birth control and abortion. This provides a test of whether the parallel trends assumption is satisfied and additionally provides comparability with the alternative estimators described below; however, event studies still suffer from the same potential biases as equation 1.

2.2 Heterogeneous Robust Estimators

Recent work in econometrics has shown that under heterogeneous or dynamic treatment effects, TWFE models may produce incorrect estimates of treatment effects, even to the point of sign flipping (De Chaisemartin and d'Haultfoeuille, 2022). Thus, I provide results from alternative estimators that are heterogeneous robust and which differ from work investigating the introduction of birth control on educational outcomes. Heterogeneous robust estimators should provide a more accurate picture of underlying causal effects under similar identifying assumptions, namely parallel trends.

Specifically, I begin by estimating treatment effects proposed in Callaway and Sant'Anna (2021). Their estimator is ideal to use here because these birth control/abortion laws are staggered over time, meaning that states are "treated" in different years and once they become treated, they stay treated. The basic intuition behind their approach is to estimate each group-specific and time average treatment effect, $\mathrm{ATT}_{g,t}$, using "clean" comparisons, where group g is defined by the first year treated. Under unconditional parallel trends assumptions and no anticipation, the $\mathrm{ATT}_{g,t}$'s take the form of:

$$\mathrm{ATT}_{g,t} = E[O_t - O_{g-1}|\text{Group = g}] - E[O_t - O_{g-1}|\text{Groups not yet treated by time g}] \qquad (3)$$

Equation 3 is modified to incorporate a situation in which the parallel trends assumption is only satisfied after controlling for additional variables - see equations 2.5-2.7 in Callaway and Sant'Anna (2021) for details. Using specific weights, $\text{ATT}_{g,t}$'s can be averaged to other ATT's of interest including an overall, group-specific overall, calendar year, and event study average treatment on the treated effects. In particular, event study estimates with treatment effect for time l relative to first treatment can be constructed as a weighted average for some weights, w_g:

$$\text{ATT}_l = \sum_g w_g \text{ATT}_{g,g+l} \tag{4}$$

In what follows, I only report the event study ATT's. I consider both a case in which birth control/abortion laws satisfy assumptions with and without additional controls.[4] When I condition on controls, I include equal pay laws, the fair employment practices act, and demographics described above. When examining birth control with additional covariates, I also control for abortion laws at ages 18+. When examining abortion with additional covariates, I also control for birth control laws at ages 18+.

I additionally consider a heterogeneous robust estimator from Sun and Abraham (2021). Their estimator is intuitively similar to Callaway and Sant'Anna (2021)'s estimator. The event study treatment effects at period l in relation to treatment are given by:

$$v_l = \sum_g \hat{\delta}_{g,l} \hat{w}_{g,l} \tag{5}$$

Where g, again, defines when a unit is first treated. Weight $\hat{w}_{g,l}$ is $\hat{Pr}[\text{Group} = g | \text{Group} \in [-l, T-l]]$ and $\hat{\delta}_{g,l}$ is estimated from the following regression:

$$Y_{i,t} = \alpha_i + \gamma_t + \sum_{g \in C} \sum_{l \neq -1} \delta_{g,l} I(\text{Group g}) I(\text{i,t is l periods from treatment}) + \varepsilon_{i,t} \tag{6}$$

where C represents control groups. In practice control groups can be any never-treated units or, in this case, the last-to-be-treated units excluding that year's data in estimation. For

[4] In practice, I use the `csdid` program in Stata which estimates the sample analog to equation 3 and its other variants. I use the doubly robust estimator based on stabilized inverse probability weighting and ordinary least squares estimator (default). Alternatives to the default method paint a similar picture - there are no clear trends in post-treatment outcomes. Results available upon request.

this paper, the last-treated units with respect to birth control are birth cohort 1959 in states that passed an early access law in 1977. These then act as "control" groups, and equation 6 is run on units excluding birth cohorts born after 1958 (as in the rest of the regressions). For abortion, the last-treated birth cohort is 1956 in states that passed early access abortion laws in 1974. Thus, unlike prior regressions I estimate this one on women born from 1935-1955 and let the 1956 cohorts in those states be the "control".[5] For birth control as the focused treatment, I additionally control for abortion laws. For abortion as the focused treatment, I additionally control for birth control laws.

3 E□ect of Birth Control and Abortion on College Completion and Majoring in Male-Dominated Fields

3.1 Descriptive

Figure 1 plots the share of bachelor's degrees completed and share of college majors in male-dominated fields by birth cohort for men and women. For each plot, I additionally provide the first birth cohort affected by early access to birth control laws at age 18 denoted by a vertical line. I also draw a vertical line at the birth cohort that had the most states adopting early access birth control laws - see Table 2 for dates.

For the share of population earning bachelor's degrees, there is a steep incline for both men and women pre- and post-introduction of the first early access birth control law. For women, this incline seems to flatten for those born in 1950, about the time that several states enacted early access to birth control for 18 - 20 year olds. There does not appear to be a sharp break in trends around law changes. In fact, Figure 2 plots the share of bachelor's completions using the time relative to the passage of a law in their birth state instead of birth year. There does not appear to be a break in the share of each cohort obtaining a bachelor's degree with respect to changes in early access to birth control.

[5] I use the Stata command `eventstudyinteract` to estimate the v_l's and corresponding standard errors. I choose default settings and additionally control for demographics, equal pay laws, and the fair employment practices act.

The share of all majors in male-dominated fields appear to tell a slightly different story. There is a clear break in trends towards majoring in male-dominated fields immediately after many states implement early access to birth control (second vertical line). This break also seems to occur similarly for men and women. Figure 2 graphs the share of male-dominated majors in time relative to state law changes in access to birth control. There is a clear increase post-law passage; however, there also appears to be a pre-trend as well.

Now considering abortion's effect on either college completion or major choice, Figure 3 plots the time relative to abortion laws passed for 18 year olds in a given state. Like birth control, the pre- and post-law changes in abortion do not appear to be related to the share of women completing a bachelor's degree. Similar to birth control again, the share of women and men completing male-dominated majors in college does increase through the passage of abortion laws. However, again, there seem to be pre-trends.

None of the descriptive figures illustrate clear deviations with respect to state law changes for early birth control or abortion access. This questions the validity of the parallel trends assumptions required to accurately identify the effect of birth control or abortion on bachelor's completion or field of study. I turn next to a more formal empirical analysis.

3.2 Bachelor's Completion

Table 3 presents the output for the TWFE models (equation 1) with varying controls and specifications for birth control or abortion laws as described in Section 2. In general, early access to abortion is never significantly related to the share of women who have completed bachelor's degrees. Using a binary indicator for the cohorts in a state with access to birth control at age 18 (columns 1-4), there does appear to be a small and statistically significant relationship. Specifically, these TWFE modes would suggest a .5 percentage point increase in women completing a bachelor's degree when a law changes in their state. Relative to the mean over this period that's approximately a 2 percent increase. This is close to the 2 percent and 3 percent increase in college completion found in Ananat and Hungerman (2012) and Hock et al. (2007), respectively.

How well do the results for birth control's effect on bachelor's completion hold up across different models? First, the exposure method for birth control - column 6 in Table 3 -

demonstrates that these results may be sensitive as the exposure variables are insignificant. Figure 4 plots the event studies for the effect of early access to birth control and abortion on the share of bachelor's completions. Statistically, there seems to be no discernible effect from the event study for either birth control or abortion.

Alternatives to the TWFE methods include Callaway and Sant'Anna (2021) - results ploted in Figure 6 - and Sun and Abraham (2021) - results plotted in Figure 8. Neither Callaway and Sant'Anna (2021) nor Sun and Abraham (2021) estimators demonstrate a discernible effect of early access to birth control on women's completion of bachelor's degrees. Interestingly, the event studies and the heterogeneous robust estimators for birth control do not demonstrate observable pre-trends either. The abortion treatment in both the event studies and in Sun and Abraham (2021)'s estimator show a clear negative pre-trend. This questions the validity of abortion's treatment effects using differences-in-differences styled estimators.

While the simple TWFE models show early access to birth control statistically significantly increased women's college completion, other methods do not. On net, the evidence does not support the conclusion that early access to birth control leads to significant increased educational attainment.[6]

Finally, there is no evidence presented here that would suggest early access to abortion increases college attainment. If anything the event studies and Sun and Abraham (2021) estimates would suggest that there are observable pre-trends that would caution the use of differences-in-differences techniques.

3.3 Male-Dominated Majors

The TWFE methods in Table 4 using binary treatment effects for early access to birth control and abortion do not suggest a causal effect of either on the choice to major in a male-dominated field for women. However, Myers (2017b)'s exposure variables suggest both birth control and abortion access may make women more likely to major in male-dominated

[6]Lindo et al. (2020) recently replicated Bailey et al. (2012) and Myers (2017b) using the Health and Retirement Study data instead. While they find directionally similar results, it is not statistically significant. I take this to be another test that may not support early access to birth control's effect on college completion.

fields.

Again turning to event studies, there is weak evidence that either birth control or abortion had any effect on increased selection of male-dominated college majors - see Figure 5. There appears to be no deviations pre- or post- early access to birth control on the share of women selecting male-dominated fields. Abortion, however, has clear negative trends. This would caution any interpretation of abortion's effect on college major selection using differences-in-differences techniques as in equation 1

Estimators derived from Callaway and Sant'Anna (2021) - Figure 7 - and Sun and Abraham (2021) - Figure 9 - both repeat the findings of the event studies for birth control. That is, there are no pre- or post- trends for birth control's effect on selecting male-dominated majors. Unlike the event studies, Callaway and Sant'Anna (2021) estimators - Figure 7 - show no pre-trends for early access to abortion. Most of the post-trends for abortion are non-negative and, in some cases, large positives. However, these post-trends for abortion are not typically statistically significant. For the Sun and Abraham (2021) estimators, again there are no pre-trends in abortion unlike the event studies. However, there is no evidence of abortion's positive effects on selecting male-dominated fields either.

As in the bachelor's completion results, the male-dominated outcomes do not seem to have a significant association with either early access to birth control or abortion. In some cases there is suggestive evidence of a positive effect for both treatments - Table 4 column 6 and Figure 7, but all the evidence taken together does not provide sufficient robustness.

4 Comparison to Other Work and Robustness

What are some plausible explanations for discrepancies across prior work that found significant effects of birth control on educational attainment and this work which did not? One obvious reason is the differences in legal coding across states. If Myers (2017b) is considered the "true" legal coding, then this may suggest previous papers were incorrect in their definition of treatment effects. However, I still was able to nearly replicate Hock et al. (2007)'s and Ananat and Hungerman (2012)'s findings on college completion using a similar TWFE model. In other words, even under the newly defined legal coding, I still obtain similar

results in the most simplistic model.[7]

Despite the replication in simple models, the fact that previous researchers disagreed over when women ages 18-20 would have had access to birth control questions how salient these laws could have been. If it is difficult for researchers to understand state statutes, it may be unreasonable to suppose the average person knew their rights regarding access to birth control. Further, using "early access" may provide an explanation for finding a weak relationship. Early access only grants women use of the Pill or abortion during ages 18-20. Because the Pill became available to women 21 or older in 1960, women 18-20 needed to only wait a few years to get access to oral birth control, for instance. Thus, not finding a robust association may be more indicative that the *timing* of access matters less than access at all. Taken together, it may not be surprising that further probing suggests a null relationship between these narrowly defined measures of reproductive control and educational outcomes.

Another potential difference between this study and previous work relates to data sample choices. I follow most other work by restricting my youngest cohort to be women born no later than 1958. The last state to change laws that would allow 18-20 year olds to access birth control actually began with birth cohort 1959. It is possible extending forward at least an additional year or removing some of the pre-treatment cohorts would change the results found in this paper. However, the underlying results are unchanged with respect to selecting different cohorts - see Tables A1 and A2. For bachelor's degree completion and male-dominated major choice, early access to birth control and abortion have nearly identical point estimates when extending the cohorts to include those born from 1935 through 1966, for instance.

Further, there is reason to believe parallel trends may not have been satistified, and descriptively there did not appear to be trend changes resulting from laws. - see Figures 2 and 3. These figures additionally show men's educational decisions were similarly affected as women, which we may or may not expect with birth control and abortion access for women.[8] This may imply that there were other external factors at play resulting in increased

[7]That being said, Hock et al. (2007) definition of legal coding was the most similar to Myers (2017b).

[8]On the one hand, men typically are not as held back by children and are not as socially reproached for childbirth out of wedlock. However, men may have to put their educational aspirations aside to care for a new family.

educational attainment in early adopting states versus late adopting states that was unrelated to the Pill rollout.

These reasons alone place doubt on the use of early access expansion as a plausible natural experiment. This particular time period is difficult to study given omitted factors that contributed to tremendous social attitude shifts and environmental changes for these particular cohorts. While this paper and previous work have tried to account for some of those differences, this may place doubt on using these laws as a clean experiment in this case.

However, a legitimate concern and limitation to using event studies and the heterogeneous robust estimators is that they are extremely demanding and sometimes imprecise. This may partly explain why Myers (2017b)'s exposure specification can detect effects of birth control and abortion on male-dominated college major selection. Simply put, exposure variables use more variation. These limitations may be particularly concerning given the intuitive nature of how reproductive control could influence long-term decision making.

Some other alternative robustness: Tables A3 and A4 provide different specifications for anti-discrimination laws and additionally include no fault divorce laws. The estimates using these alternative covariates is nearly identical in every single regression, as expected. Additionally, the results within the main output tables also do not significantly change with addition of state linear trends or controls. This means within the TWFE models, the output is relatively robust to additional controls.

5 Conclusion

The increase in college enrollments and workforce attachment among women in the 1960s-70s, along with changing views on marriage and divorce, has been attributed by some to the widespread availability of oral birth control, the Pill. Previous research has found that access to birth control during adolescence and young adulthood has allowed women to delay childbirth, increase college enrollment and completion, and improve their long-term wages. However, recent revisions to early access to birth control and abortion laws, as well as advancements in empirical designs, have called into question these previous conclusions.

Using ACS data from 2009-2019 and the revised legal codings, I test the relationship between access to birth control and abortion and college completion and majoring in male-dominated fields. I find neither of these outcomes are robustly associated with early access to birth control or abortion. The effect of birth control on college completion, a positive association found in some studies, is not robust when using event studies or heterogeneous robust estimators. There is also no consistent evidence that birth control increases women's propensity to select male-dominated college majors. Additionally, the rollout of abortion laws does not have a significant effect on either college completion or choice of majoring in male-dominated fields and has clear pre-trends.

While I do not definitively rule out the effects of birth control and abortion access on educational attainment for women, this work highlights the need for additional research in this area, especially given recent changes in access to both birth control and abortion.

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6 Tables

Table 1: Literature Review

Paper	Cohorts	Sample	Main Outcomes	Main Specification for BC	Additional Controls
Hock et al. (2007)	1940-1959	1990 and 2000 Census	College Completion (and enrollment)	Binary	State linear trend, race/ethnicity, Vietnam related college enrollment controls
Bailey et al. (2012)	1943-1954	NLS-YW 1968 ages 14 to 24 + additional surveys of the same women	Wages (mechanisms: occupation - nonteacher or nonnurse)	Binary	None
Ananat and Hungerman (2012)	1920-1961	1970, 1980, 1990 Census women aged 30-49	College Completion (actual paper focuses on fertility)	Binary	Percentage of the state population that is black and percentagethat is other nonwhite
Myers (2017b)	1935-1958	1979–95 CPS June Fertility (at least 22 during survey year)	Fertility and Marriage	Exposure to legal pill/abortion and exposure to early consent of pill/abortion	State linear trend, race/ethnicity, anti-discrimination laws and no fault divorce laws
Lindo et al. (2020)	1935-1958	Health and Retirement Study	Years of Education and Earnings	Follow Bailey 2012 and modify based on Meyers 2017	
This paper	1935-1958	ACS 2009-2019	Completion and Male-dominated college major	See text	See text

Table 2: Number of States Passing Birth Control or Abortion Laws for 18+ Year Olds

Year Passed	Number of States
Birth Control	
1960	8
1961	1
1963	1
1965	3
1969	2
1970	3
1971	15
1972	10
1973	5
1974	2
1977	1
Abortion	
1970	4
1971	1
1973	42
1974	4

Source: Myers (2017b) Table 1.

Table 3: OLS: Share of Bachelor's Degrees with Early Access Birth Control and Abortion Laws

	Treatment is Binary					Myer (2017)
Birth Control (18+)	0.004	0.005	0.005	0.005		
	(0.003)	(0.003)	(0.002)	(0.002)		
Abortion (18+)	-0.004	-0.001	-0.001		-0.000	
	(0.008)	(0.004)	(0.004)		(0.004)	
Exp. BC Legal						-0.005
						(0.003)
Exp. BC Consent						-0.004
						(0.005)
Exp. AB Legal						-0.004
						(0.004)
Exp. AB Consent						-0.006
						(0.007)
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
State Lin. Trend	no	yes	yes	yes	yes	yes
Controls	no	no	yes	yes	yes	yes
N	1224	1224	1224	1224	1224	1224
Dep. Var. Mean	0.263	0.263	0.263	0.263	0.263	0.263

Notes: Estimates from TWFE models of equation 1. Outcome is the share of women who have earned at least a bachelor's degree. Controls include share of white, Black and Hispanic women in each birth year and birth state, and whether an equal pay law and the fair employment practices act was enacted at age 18 or later within a state. Weighted by the total number of women in a birth year and state of birth. Standard errors are clustered at the cohort level and * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Data: ACS and Myers (2017b).

Table 4: OLS: Share of Bachelor's Degress in a Male-Dominated Field with Early Access Birth Control and Abortion Laws

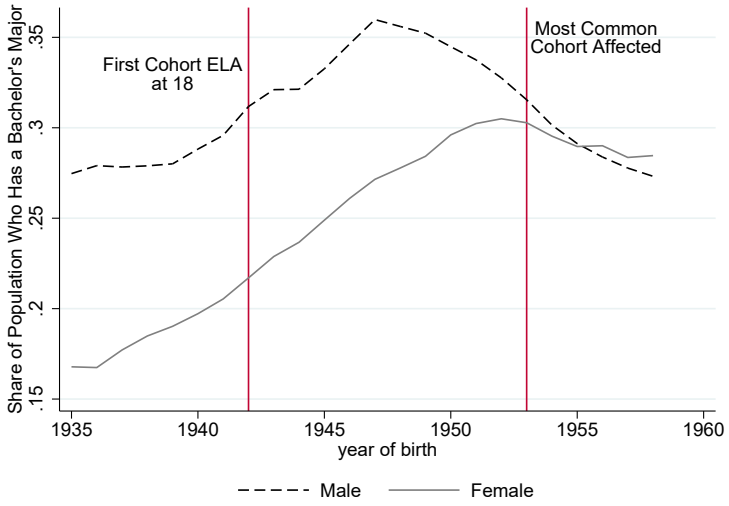
	Treatment is Binary					Myer (2017)
Birth Control (18+)	-0.004	-0.003	-0.003	-0.002		
	(0.003)	(0.003)	(0.002)	(0.002)		
Abortion (18+)	0.001	0.001	0.001		0.001	
	(0.004)	(0.003)	(0.003)		(0.003)	
Exp. BC Legal						0.006
						(0.003)
Exp. BC Consent						-0.002
						(0.006)
Exp. AB Legal						0.011
						(0.004)
Exp. AB Consent						0.021
						(0.012)
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
State Lin. Trend	no	yes	yes	yes	yes	yes
Controls	no	no	yes	yes	yes	yes
N	1218	1218	1218	1218	1218	1218
Dep. Var. Mean	0.193	0.193	0.193	0.193	0.193	0.193

Notes: Estimates from TWFE models of equation 1. Outcome is the share of women who have majored in a male-dominated field out of all women who have completed at least a bachelor's degree. Controls include share of white, Black and Hispanic women in each birth year and birth state, and whether an equal pay law and the fair employment practices act was enacted at age 18 or later within a state. Weighted by the total number of women who completed a bachelor's degree in a birth year and state of birth. Standard errors are clustered at the cohort level and * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Data: ACS and Myers (2017b).

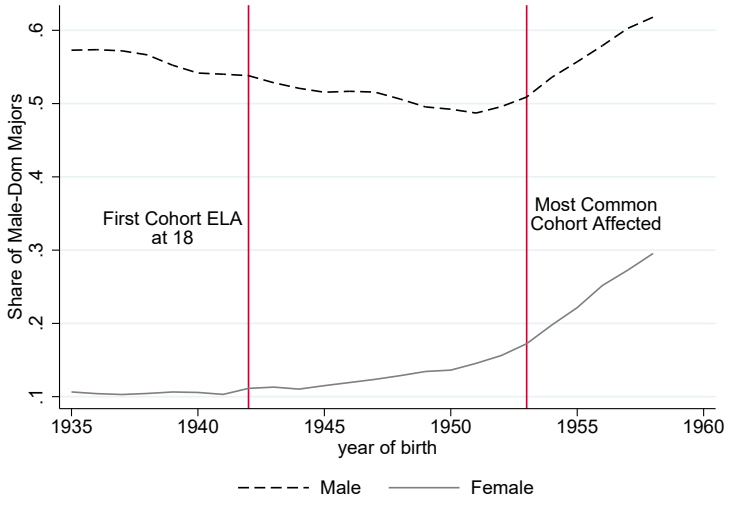
7 Figures

Figure 1: Share of Bachelor's Earners and Share of Bachelor's in Male-Dominated Fields for Men and Women by Birth Cohort

Panel A - Share of Population with Bachelor's Degree



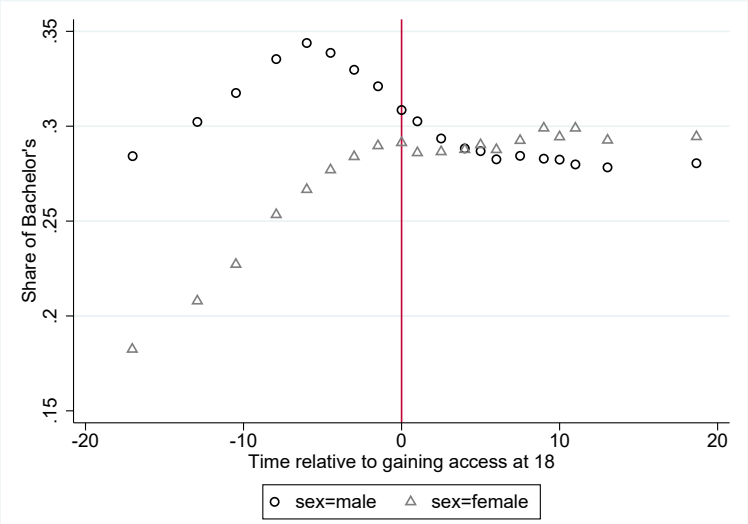
Panel B - Share of Male-Dominated College Majors



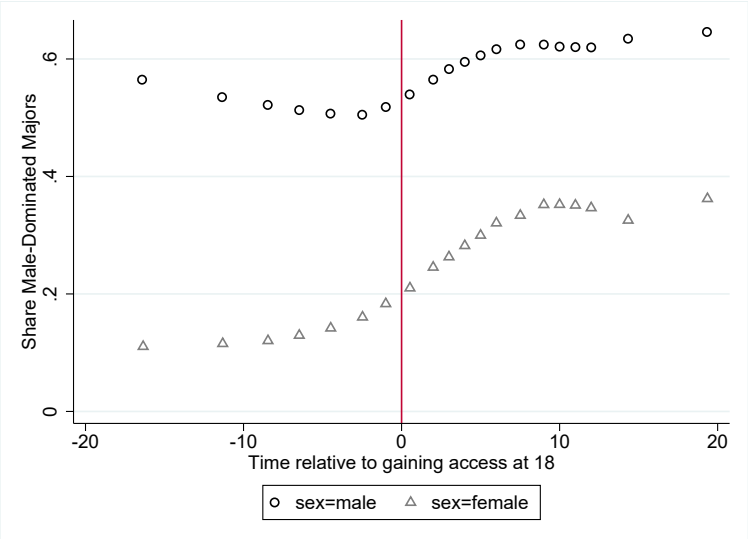
Note: Share of men and women who have at least a bachelor's degree by birth year (top) and share of male-dominated college majors of men and women (bottom) by birth year; Red lines are for birth cohorts 1942 and 1953, the first and most common birth control law passed allowing 18+ year olds early access, respectively. Data: ACS and Myers (2017b).

Figure 2: Share of Bachelor's Earners and Share of Bachelor's in Male-Dominated Fields for Men and Women by Time Relative to Passage of Birth Control Law in State of Birth

Panel A - Share of Population with Bachelor's Degree



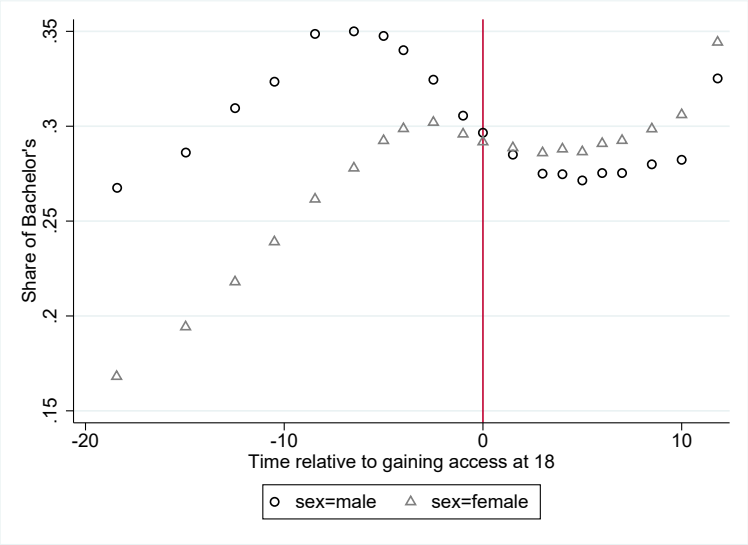
Panel B - Share of Male-Dominated College Majors



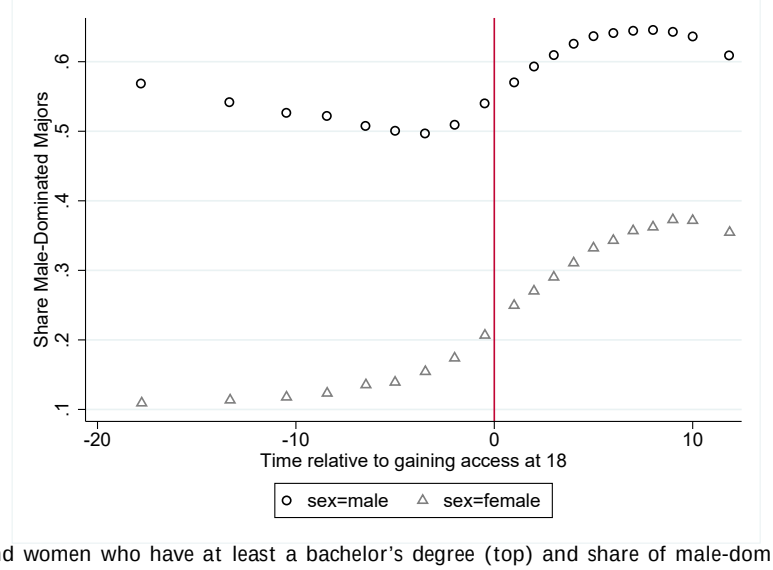
Note: Share of men and women who have at least a bachelor's degree (top) and share of male-dominated college majors of men and women (bottom) by year relative to the passage of early access birth control in their state of birth; Data: ACS and Myers (2017b).

Figure 3: Share of Bachelor's Earners and Share of Bachelor's in Male-Dominated Fields for Men and Women by Time Relative to Passage of Abortion Law in State of Birth

Panel A - Share of Population with Bachelor's Degree

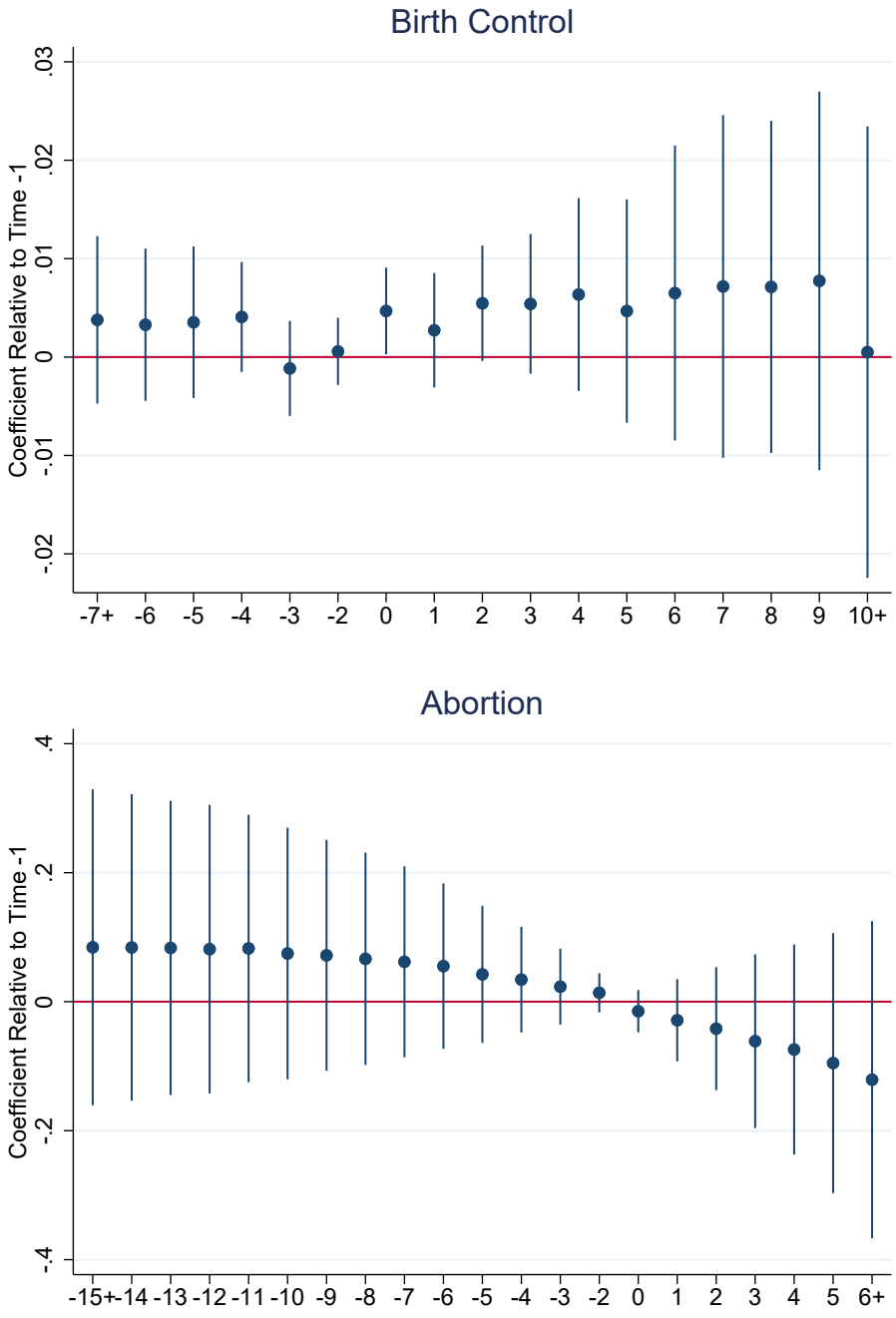


Panel B - Share of Male-Dominated College Majors



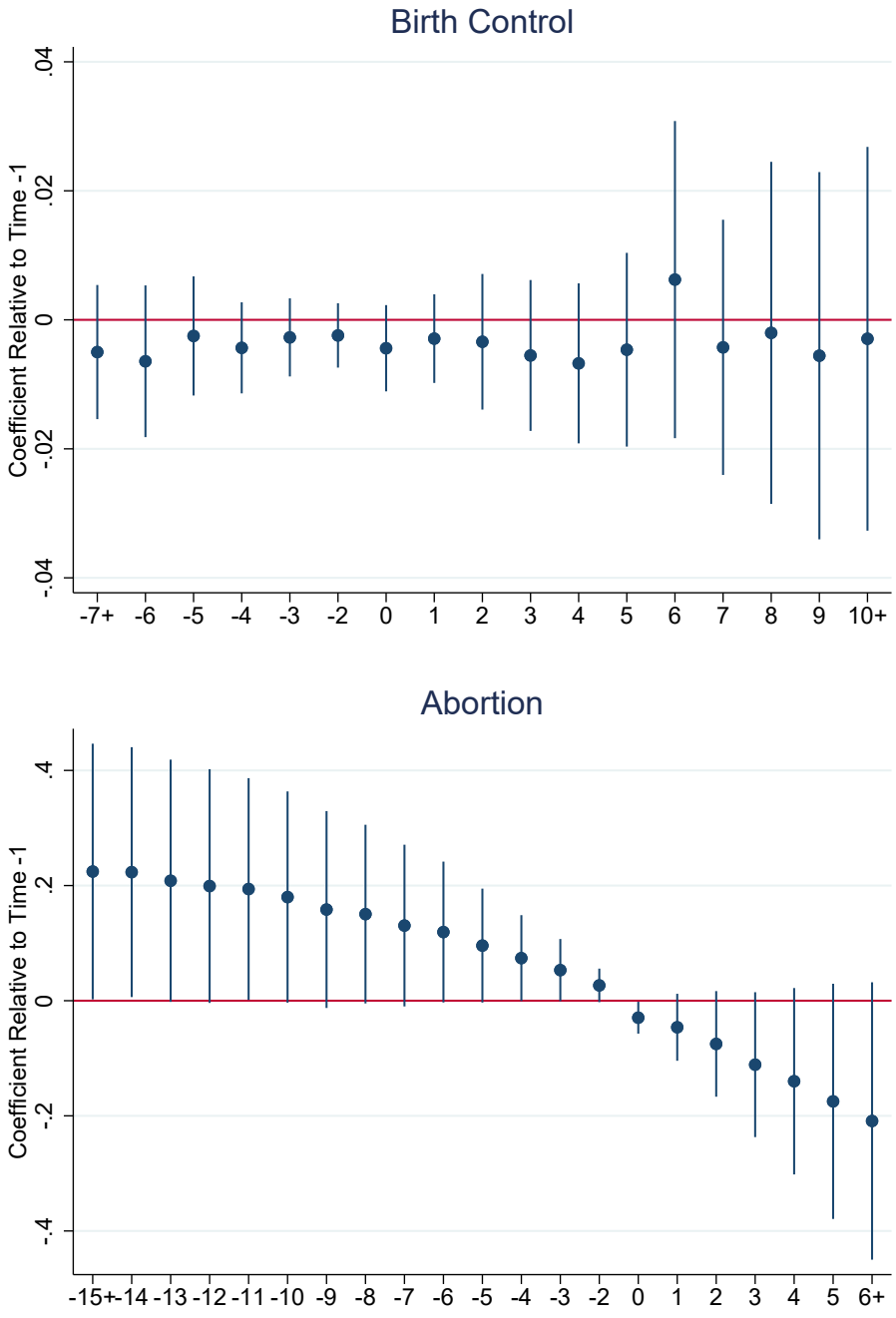
Note: Share of men and women who have at least a bachelor's degree (top) and share of male-dominated college majors of men and women (bottom) by year relative to the passage of early access abortion in their state of birth; Data: ACS and Myers (2017b).

Figure 4: Event Study: Effect of Birth Control and Abortion Laws Available for 18+ Year Olds on Share of Bachelor's Degrees for Women



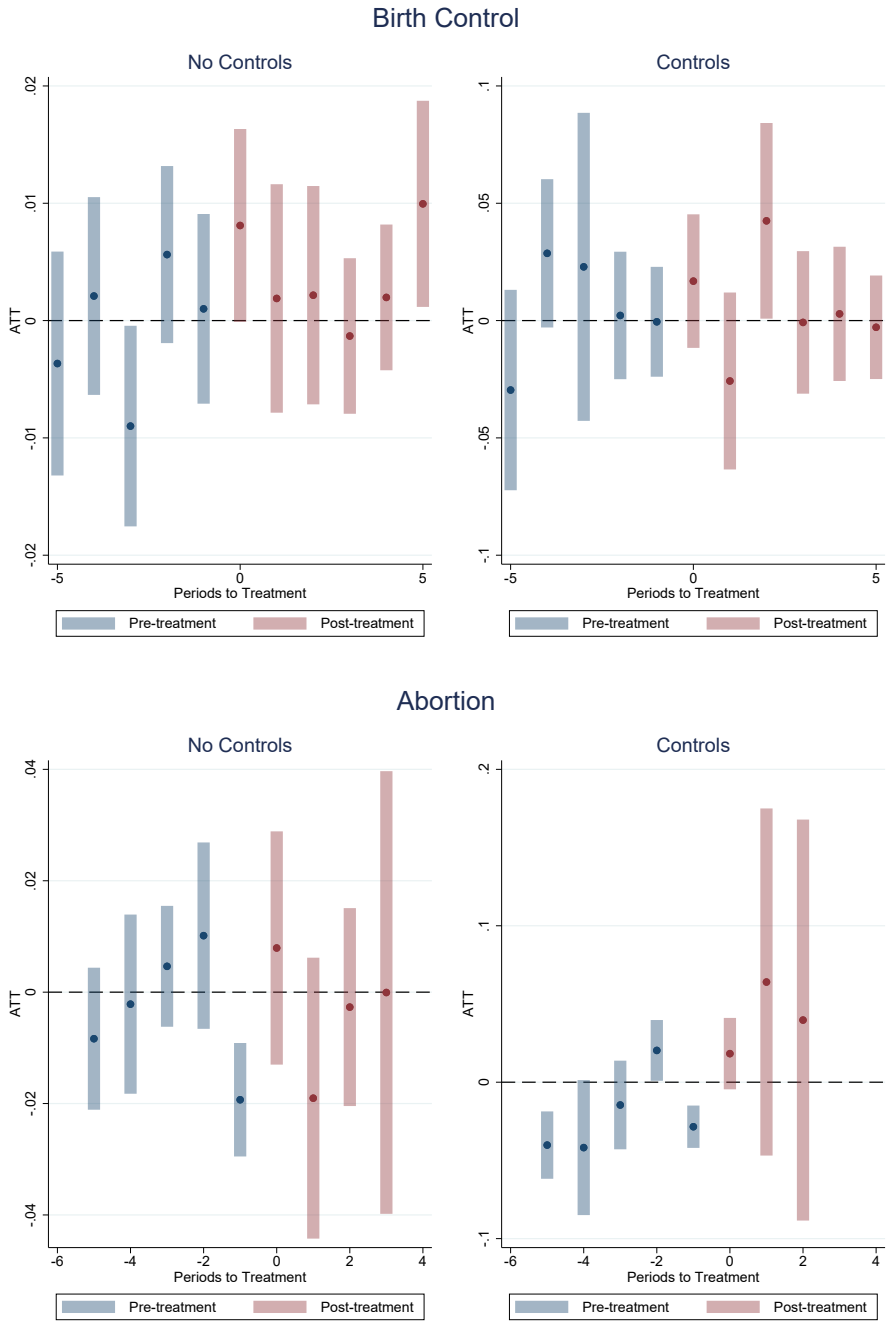
Note: Point estimates and confidence intervals of the event studies described in text. Top panel uses birth control as treatment and bottom uses abortion as treatment. Both have full set of controls. Data: ACS and Myers (2017b).

Figure 5: Event Study: Effect of Birth Control and Abortion Laws Available for 18+ Year Olds on Share of Male-Dominated College Majors for Women



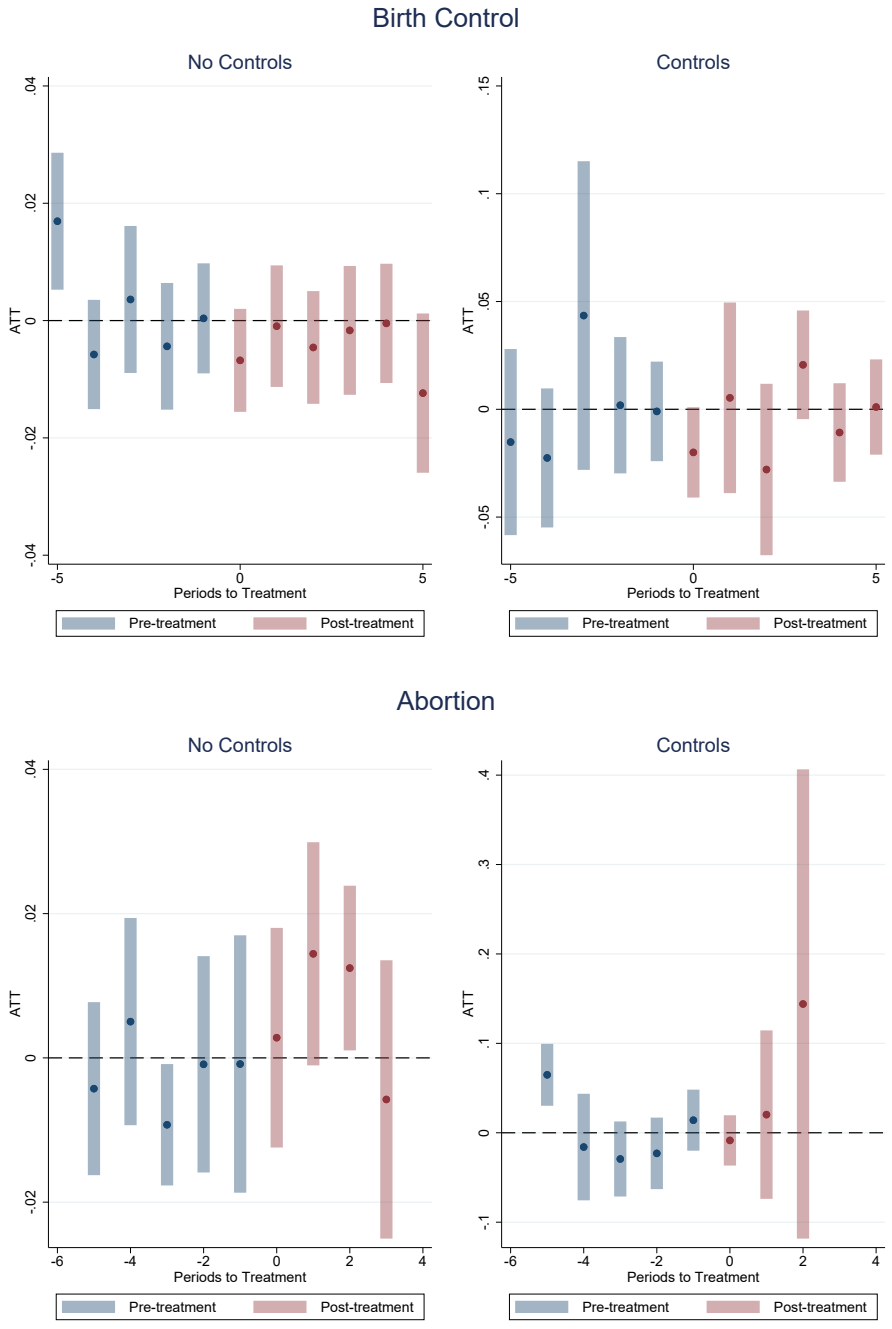
Note: Point estimates and confidence intervals of the event studies described in text. Top panel uses early access to birth control as treatment and bottom uses early access to abortion as treatment. Both have full set of controls. Data: ACS and Myers (2017b).

Figure 6: Callaway and Sant'Anna (2021): Effect of Birth Control and Abortion Laws Available for 18+ Year Olds on Share of Bachelor's Degrees for Women



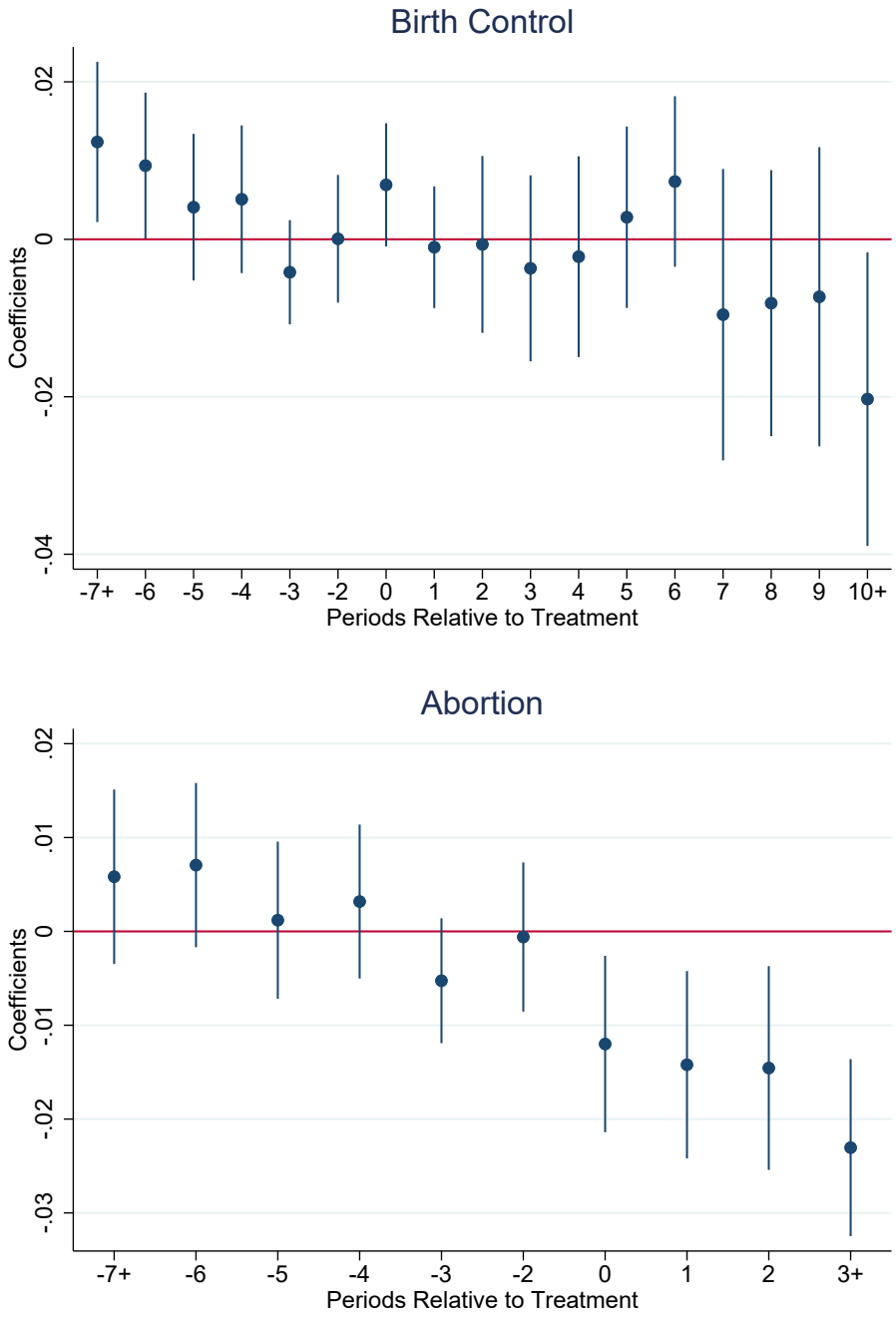
Note: Output from Callaway and Sant'Anna (2021) event study estimators and confidence intervals with and without controls. The top panel uses early access to birth control as primary treatment while the bottom uses early access to abortion as primary treatment.

Figure 7: Callaway and Sant'Anna (2021): Effect of Birth Control and Abortion Laws Available for 18+ Year Olds on Share of Male-Dominated College Majors for Women



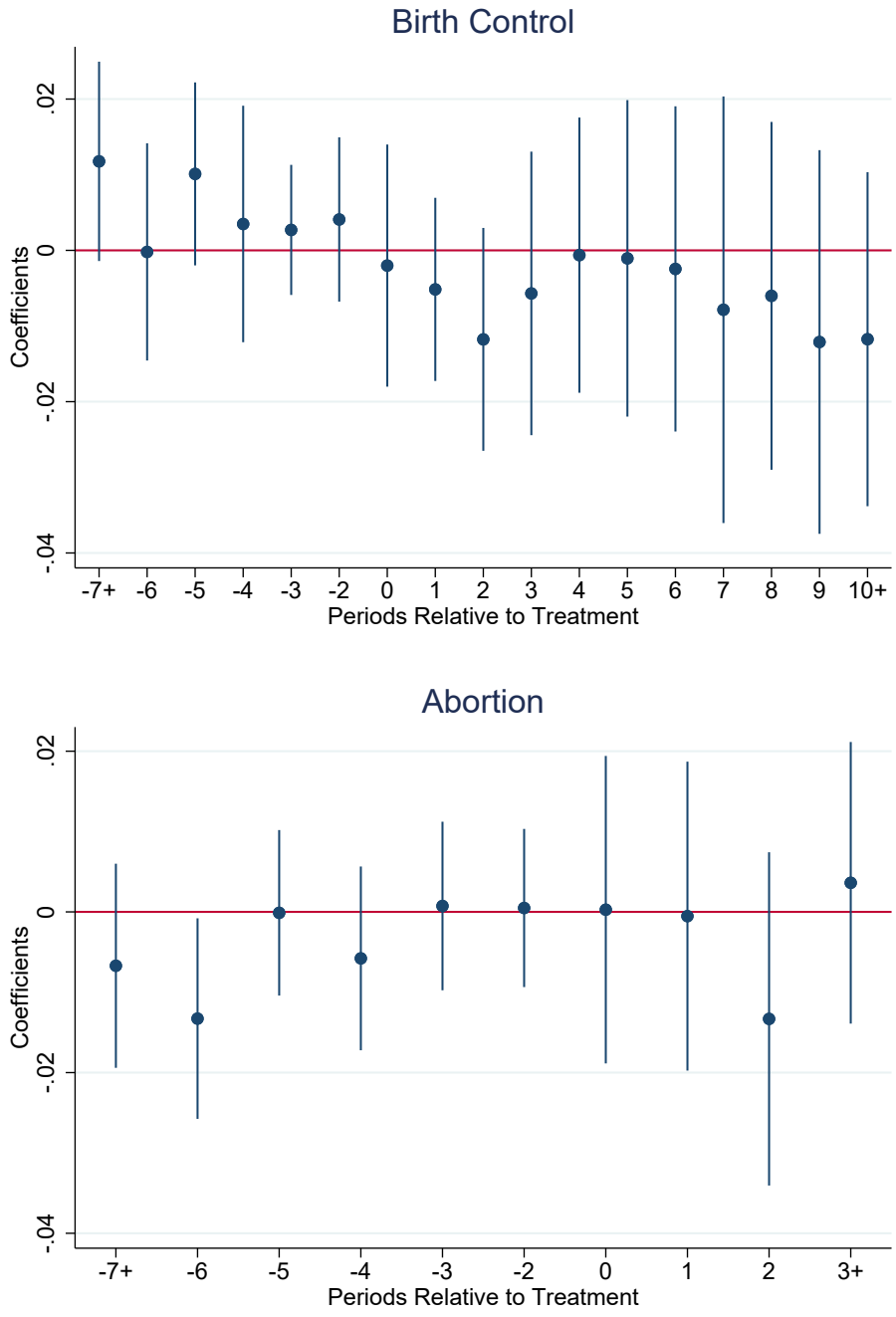
Note: Output from Callaway and Sant'Anna (2021) event study estimators and confidence intervals with and without controls. The top panel uses early access to birth control as primary treatment while the bottom uses early access to abortion as primary treatment.

Figure 8: Sun and Abraham (2021): Effect of Birth Control and Abortion Laws Available for 18+ Year Olds on Share of Bachelor's Degrees for Women



Note: Output from Sun and Abraham (2021) event study estimators and confidence intervals. The top panel uses early access to birth control as primary treatment while the bottom uses early access to abortion as primary treatment. Both control for demographics, equal pay laws and fair employment acts as well as early access to abortion (birth control) when early access to birth control (abortion) is the primary treatment variable.

Figure 9: Sun and Abraham (2021): Effect of Birth Control and Abortion Laws Available for 18+ Year Olds on Share of Male-Dominated College Majors for Women



Note: Output from Sun and Abraham (2021) event study estimators and confidence intervals. The top panel uses early access to birth control as primary treatment while the bottom uses early access to abortion as primary treatment. Both control for demographics, equal pay laws and fair employment acts as well as early access to abortion (birth control) when early access to birth control (abortion) is the primary treatment variable.

Appendices

A Tables and Figures

Table A1: OLS: Share of Bachelor's Degrees with Early Access Birth Control and Abortion Laws: Cohorts 1935-1966

	Treatment is Binary				
Birth Control (18+)	0.004	0.004	0.005	0.004	
	(0.003)	(0.003)	(0.003)	(0.003)	
Abortion (18+)	0.001	0.001	-0.002		-0.001
	(0.006)	(0.006)	(0.006)		(0.006)
State FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
State Lin. Trend	no	yes	yes	yes	yes
Controls	no	no	yes	yes	yes
N	1632	1632	1632	1632	1632
Dep. Var. Mean	0.274	0.274	0.274	0.274	0.274

Notes: Same as Table 3, but with cohorts 1935-1966. Estimates from TWFE models of equation 1. Outcome is the share of women who have earned at least a bachelor's degree. Controls include share of white, Black and Hispanic women in each birth year and birth state, and whether an equal pay law and the fair employment practices act was enacted at age 18 or later within a state. Weighted by the total number of women in a birth year and state of birth. Standard errors are clustered at the cohort level and * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Data: ACS and Myers (2017b).

Table A2: OLS: Share of Bachelor's Degress in a Male-Dominated Field with Early Access Birth Control and Abortion Laws: Cohorts 1935-1966

	Treatment is Binary				
Birth Control (18+)	-0.002	-0.004	-0.004	-0.004	
	(0.004)	(0.003)	(0.002)	(0.003)	
Abortion (18+)	0.004	0.002	0.003		0.002
	(0.009)	(0.006)	(0.005)		(0.005)
State FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
State Lin. Trend	no	yes	yes	yes	yes
Controls	no	no	yes	yes	yes
N	1626	1626	1626	1626	1626
Dep. Var. Mean	0.280	0.280	0.280	0.280	0.280

Notes: Same as Table 4 but estimated on cohorts 1935-1966. Estimates from TWFE models of equation 1. Outcome is the share of women who have majored in a male-dominated field out of all women who have completed at least a bachelor's degree. Controls include share of white, Black and Hispanic women in each birth year and birth state, and whether an equal pay law and the fair employment practices act was enacted at age 18 or later within a state. Weighted by the total number of women who completed a bachelor's degree in a birth year and state of birth. Standard errors are clustered at the cohort level and * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Data: ACS and Myers (2017b).

Table A3: OLS: Share of Bachelor's Degrees with Early Access Birth Control and Abortion Laws: Alternative Definitions Anti-Discrimination Laws and No Fault Divorce

	Treatment is Binary					Myer (2017)
Birth Control (18+)	0.004	0.005	0.005	0.005		
	(0.003)	(0.003)	(0.002)	(0.002)		
Abortion (18+)	-0.004	-0.001	-0.001		-0.000	
	(0.008)	(0.004)	(0.004)		(0.004)	
Exp. BC Legal						-0.007
						(0.003)
Exp. BC Consent						-0.007
						(0.005)
Exp. AB Legal						-0.004
						(0.004)
Exp. AB Consent						-0.004
						(0.006)
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
State Lin. Trend	no	yes	yes	yes	yes	yes
Controls	no	no	yes	yes	yes	yes
N	1224	1224	1224	1224	1224	1224
Dep. Var. Mean	0.263	0.263	0.263	0.263	0.263	0.263

Notes: Controls include share of white, Black and Hispanic women in each birth year and state. Controls for the equal pay laws and the fair employment practices act based on an exposure method as in (Myers, 2017b). Estimates from TWFE models of equation 1. Outcome is the share of women who have earned at least a bachelor's degree. Weighted by the total number of women in a birth year and state of birth. Standard errors are clustered at the cohort level and * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Data: ACS and Myers (2017b).

Table A4: OLS: Share of Bachelor's Degress in a Male-Dominated Field with Early Access Birth Control and Abortion Laws: Alternative Definitions Anti-Discrimination Laws and No Fault Divorce

	Treatment is Binary					Myer (2017)
Birth Control (18+)	-0.004	-0.003	-0.003	-0.002		
	(0.003)	(0.003)	(0.002)	(0.002)		
Abortion (18+)	0.001	0.001	0.003		0.002	
	(0.004)	(0.003)	(0.003)		(0.003)	
Exp. BC Legal						0.008
						(0.003)
Exp. BC Consent						0.002
						(0.005)
Exp. AB Legal						0.012
						(0.004)
Exp. AB Consent						0.019
						(0.012)
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
State Lin. Trend	no	yes	yes	yes	yes	yes
Controls	no	no	yes	yes	yes	yes
N	1218	1218	1218	1218	1218	1218
Dep. Var. Mean	0.193	0.193	0.193	0.193	0.193	0.193

Notes: Controls include share of white, Black and Hispanic women in each birth year and state. Controls for the equal pay laws and the fair employment practices act based on an exposure method as in (Myers, 2017b). Estimates from TWFE models of equation 1. Outcome is the share of women who have majored in a male-dominated field out of all women who have completed at least a bachelor's degree. Weighted by the total number of women who completed a bachelor's degree in a birth year and state of birth. Standard errors are clustered at the cohort level and * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Data: ACS and Myers (2017b).