Contraception Responses to Abortion Bans

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Abstract

When access to abortion becomes limited, the potential negative consequences of pregnancy become more significant for women of reproductive age. Economic theory suggests that women might respond by increasing their use of contraception to prevent pregnancy. Using longitudinal data from commercial health insurance plans and a stacked difference-in-differences approach, we examine whether state abortion bans following the *Dobbs* decision led to changes in prescription contraception use. We focus on two main questions. First, among women who were not previously using contraception, do abortion bans increase take up of short-acting contraception (like birth control pills) or long-acting contraception (like IUDs)? Second, among women already using short-acting methods, do abortion bans induce a shift to long-acting options? Our analysis of the claims data reveals that contraception use has been gradually declining nationwide over the study period. Importantly, we find little evidence of meaningful increases in contraceptive initiation among prior non-users. Among women already using short-acting hormonal contraception, we find a small, statistically significant increase in switching to LARC, though event-study patterns and a 2020 divergence that coincides with the COVID-19 period suggest caution in attributing this to *Dobbs*. We also examine heterogeneity by geographic access to abortion providers, measured by driving distance to the nearest abortion facility. These findings suggest that behavioral responses to abortion restrictions, where present, are modest and uneven across margins of contraceptive choice, underscoring that changes in abortion access do not necessarily translate into large shifts in claims-based contraceptive use.

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1 Introduction

Following the Supreme Court's 2022 decision in *Dobbs v. Jackson Women's Health Organization*, access to abortion services changed substantially across many states. Within days of the decision, 19 states moved to ban or severely restrict abortion by activating trigger laws, enforcing pre-*Roe* statutes, or passing new legislation (McCann and Walker, 2022). There was an immediate contraction in abortion access: 66 clinics across 15 states ceased providing abortion services within 100 days, representing an 8% reduction in the number of clinics nationwide (Nash et al., 2024). Average travel time to the nearest abortion facility rose from 28 to 100 minutes (Rader et al., 2022). While clinic-based services may have contracted, self-managed abortions supported by medication and telehealth visits may have become more available(Aiken et al., 2024). This shift in how abortion services are accessed appears to have influenced overall utilization: research suggests that the total number of abortions reported nationally did not decline following *Dobbs*, and may have increased modestly (Society of Family Planning, 2024).

These changes in reproductive policy have likely altered the consequences of pregnancy for many women and may have influenced how individuals make contraceptive decisions. This paper examines whether abortion bans lead to changes in contraceptive behavior across several key decision points. When abortion access is restricted, the stakes of contraceptive failure become higher, particularly for those who would have previously relied on abortion services to address an unintended pregnancy. In response, individuals may adapt their contraceptive strategies in several ways: increasing overall contraceptive use, switching to more effective or longer-lasting methods, or beginning contraception earlier in their reproductive years.

The presence and magnitude of such behavioral responses, however, depend on real-world frictions in access and decision-making. Access to contraception is uneven across the United States. Many rural and low-income areas face shortages of women's health and primary care providers, limiting access to contraceptive counseling, long-acting reversible contraceptive (LARC) insertions, and prescription renewals. In states where scope-of-practice regulations are more flexible, pharmacists are authorized to prescribe hormonal methods (Grossman et al., 2025), which may alleviate some barriers. In others, such policies are absent, and provider access is more constrained. From a behavioral perspective, even among those with access, demand for preventive contraception may remain limited by myopic time horizons, low perceived risk, or inertia—particularly for individuals who previously viewed abortion as a fallback option. Contraceptive method choice may also be shaped by trust in providers and life stage considerations.

These mechanisms are supported by early empirical research. Dench et al. (2024) find that abortion bans led to a 2.3% increase in births nationally—approximately 32,000 additional births—which suggests that contraception responses were either limited or insufficient to offset declines in abortion access. Qato et al.

(2024) find a substantial decline in emergency contraceptive fills in abortion ban states, while Xu et al. (2024) report a 3% monthly increase in sterilization procedures following the *Dobbs* decision. Requests for abortion pills through telehealth services also rose sharply post-*Dobbs*, particularly in states that enacted full bans (Aiken et al., 2024). Evidence on health system and provider responses has been more mixed: while some companies introduced abortion travel policies to retain workers (Adrjan et al., 2023), and clinicians reported widespread uncertainty around managing pregnancy complications (Roper et al., 2024), there has been no evidence of an exodus of obstetricians or gynecologists from ban states (Staiger et al., 2025).

People of reproductive age continuously make decisions about family planning and fertility management. Their decisions are not only limited to preventing unintended pregnancies but deciding whether and when to pursue a pregnancy and how to manage pregnancy risks and deal with complications. Individuals have different preferences over family size, timing, allowable risks, and the costs of different interventions. The tools to manage fertility vary in efficacy, access, cost, and reversibility. Not using contraception is an option. Over-the-counter products like condoms, spermicides, internal condoms, and sponges are readily available and do not require a physician to access. The birth control pill was historically only available by prescription, but as of 2024, a progestin-only version called the Opill became available over-the-counter. Prescription birth control pills are still used, as are other short-acting hormonal methods like patches or vaginal rings. Long-acting reversible contraceptives–the implant or intrauterine devices (IUDs)–are highly effective but require surgical insertion and removal. Sterilization procedures such as tubal ligation cause permanent infertility. These methods are imperfect substitutes, and people will have different risk tolerances and preferences over side effects and costs, as well as different access. We motivate our paper with a simple model in which reduced abortion access raises the marginal value of preventing pregnancy in the first place, creating a demand-side incentive to adopt more effective contraceptive methods. Whether this incentive is realized in observed contraceptive claims, however, depends on access costs, inertia, and other frictions that the model also allows for.

This paper contributes to the literature by evaluating whether the post-*Dobbs* abortion bans affect the extensive and intensive margins of contraception use. We use health insurance claims data from the Merative MarketScan Commercial Database from 2016 to 2024 to track contraceptive behavior across millions of women of reproductive age. These data contain longitudinal information on prescription drug fills, outpatient visits, and procedural claims—including insertions and removals of LARC devices, and sterilization surgeries. Unlike prior studies that examine changes in aggregate use, our approach uses detailed panel data and a difference-in-differences (DID) design to isolate effects across decision cohorts. This design allows us to examine whether women who were not previously using contraception initiate contraception after the *Dobbs* decision, an extensive margin response. We also examine intensive margin choices by studying the effects of

abortion bans on transitions from short to long-acting contraception among women who were already using short-acting contraception.

Our initial results suggest limited behavioral response on the extensive or intensive margin. Among women who were not using contraception in the year prior, we find no statistically significant increase in uptake of either short-acting hormonal methods or LARCs following abortion bans. Among current short-acting prescription contraceptive users, we estimate a small, statistically significant shift toward long-acting methods. However, the event-study pattern is not as clear, with treated and control states diverging in 2020 during the COVID-19 period. Taken together, our results suggest that abortion bans did not induce large-scale behavioral changes in contraceptive choice among commercially insured women, but further work is needed to understand heterogeneous effects by age and by local health care market conditions. Our work provides new evidence on how individuals respond to shifts in the reproductive policy landscape. It also informs ongoing debates about the substitutability of contraception for abortion, and whether increased regulatory restrictions lead to compensatory behaviors or instead create additional burdens for women navigating reproductive health choices.

The paper proceeds as follows: Section 2 presents a simple model of contraception decision-making that accounts for method effectiveness. Section 3 describes the data used in the analysis and 4 describes the research design. We present the results in 5 and conclude in 6.

2 Conceptual Framework

To fix ideas about why and for whom abortion bans may alter contraceptive behavior, our conceptual framework provides a simple model of abortion as a state-contingent fallback against contraceptive failure. We draw on models from Levine and Staiger (2002), Kane and Staiger (1996), and Miller and Valente (2016) to formalize our model. Women of reproductive age choose a contraceptive method from the basket of options, including the option to use no contraception, anticipating that if the method fails, she then faces a decision to continue the pregnancy or terminate it. An abortion ban or an increase in the distance to the nearest provider because of state bans or restrictions will raise the cost of the terminate option. This higher cost will influence the contraception decision, raising the marginal value of preventing a pregnancy in the first place.

Women choose from a set of methods $m \in \{0, 1, ..., M\}$ where $m = 0$ is no contraception and higher m values index more effective and longer-lasting methods. Each method will have a failure probability f_m that is decreasing in m (that is, $f_0 > f_1 > ... > f_M$). Each method will have a per-period cost of c_m which includes money, side effects, hassle, and provider access. Methods include no contraception, over-the-counter products, short-acting prescription methods, long-acting reversible contraceptives, and permanent methods.

Conditional on contraceptive failure, the woman experiences an unintended pregnancy and can resolve it by choosing the less costly of two options: carrying to term with the net value of V_b or terminating at the cost p. The value of the failure state is $\max\{V_b, -p\}$, and the woman chooses m to minimize the expected cost:

$$\min_m \; c_m + f_m \cdot \big[-\max\{V_b, \, -p\} \big].$$

An abortion ban will raise the cost of terminating, p, which raises the cost of the failure state. This in turn raises the marginal return to a lower f_m which will push women towards a higher m. A ban can also impact V_b, the net value of the unintended birth, as bans are accompanied by clinician uncertainty over managing pregnancy complications and concerns about obstetric care supply in affected states. These will raise the risk and cost of carrying a pregnancy to term, which will lower V_b. This will also raise the cost of the failure state which will similarly raise the marginal return to a lower f_m which will push to a higher m.

The price channel, p, will bind for those where $-p > V_b$; these are reproductive age women where abortion was an option pre-ban, or those who would have terminated rather than carrying an unintended pregnancy to term. The V_b channel can be binding for anyone who would carry a pregnancy, regardless of whether they would consider an abortion.

This framework also allows us to specify the intensive and extensive margins. Considering that short-acting hormonal methods are less effective than long-acting methods, we define the intensive margin as switching from short-acting methods to longer-acting methods among current users. That is, $m_{pre} \rightarrow m_{post} > m_{pre}$ where the method adopted after the abortion ban is a higher effectiveness. The extensive margin is adoption among non-users [1]. For this, we consider those who are $m_{pre} = 0 \rightarrow m_{post} > 0$.

The model also implies that the effect of abortion restrictions on contraception is ambiguous if the policy shock also raises the cost of accessing some contraceptive methods. This is particularly relevant for LARC, which requires in-person clinical insertion. If abortion clinic closures or broader reproductive-care disruptions increase the access cost of LARC (c_m rising for higher m), then the same shock that raises the value of pregnancy prevention may also reduce the feasibility of initiating or switching to more effective methods. The net effect on LARC initiation is therefore theoretically ambiguous and is ultimately an empirical question.

3 Data

Insurance Claims We use insurance claims data from Merative MarketScan Commercial Database from 2016 to 2024. Merative MarketScan provides longitudinal health insurance claims data for employees and

[1] For our purposes, we define the non-users as those who do not have prescription contraceptive claims. While the model is inclusive of all methods, including non-prescription methods, the results from the extensive margin should be considered as those going from no prescription method to a prescription method

their dependents. Each claim record contains information on patient demographics, medical diagnoses, procedures, inpatient and outpatient visits, and prescription drug fills. The claim records have patient IDs which allow us to track individuals over time.Contraception use is defined by NDC, procedure, or diagnosis codes from the U.S. Department of Health and Human Services.[2] Prescriptions for short-acting contraception —including oral contraceptive pills, contraceptive patches, and vaginal rings — are identified using NDC codes in the prescription claims data. Insertion and removal of long-acting reversible contraceptives (LARCs) like intrauterine devices (IUDs) and implants are captured using CPT/HCPCS codes.

A strength of the MarketScan data is that it allows us to observe detailed contraceptive method use based on both pharmaceutical and procedural claims in addition to demographic characteristics, provider type, and geographic location. However, the data are limited to individuals with employer-sponsored insurance, and may not generalize to populations with public insurance or no coverage. Regardless, the internal validity of the study should not be impacted as this limitation will affect selection of individuals in both our control and treatment states. If anything, finding impacts on this group with potentially higher income and economic stability could indicate the far reaching impacts of these policies beyond the most vulnerable groups we likely cannot observe with this data.

Abortion Laws The regulation of abortion services changed quickly after the Dobbs decision. The Guttmacher Institute tracked the changes and found that 13 states had "trigger bans" on abortion following the *Dobbs* Decision. They identify the states whose trigger bans went into effect upon the decision (Kentucky, Louisiana, and South Dakota), those that went into effect after 30 days (Idaho, Tennessee, Texas), and those which required certification (Arkansas, Mississippi, Missouri, North Dakota, Oklahoma, Utah, Wyoming). Missouri's ban was initially blocked so that only 12 of the trigger bans came into force immediately. In the following months, additional states altered access to abortion through bans or increased restrictions.

After comparing reports on state abortion regulations from the Guttmacher Institute, the New York Times, the Center for Reproductive Rights, and the Kaiser Family Foundation, we categorized states into three categories: trigger ban states that implemented a near ban on abortion after the decision, clean control states that did not further restrict access, and an excluded other group of states that made partial changes to their abortion policies during the period that do not fit cleanly as a control or treated state. [3] Table 1 shows which states belong to each of our three categories; this classification reflects each state's realized near-total

[2]For more information, see
https://opa.hhs.gov/research-evaluation/title-x-services-research/contraceptive-care-measures/claims-data-sas-program-instructions

[3]For more information on state policy timelines, see:
Guttmacher - http://states.guttmacher.org/policies/south-dakota/abortion-policies,
New York Times - https://www.nytimes.com/interactive/2024/us/abortion-laws-roe-v-wade.html,
Center for Reproductive Rights - https://reproductiverights.org/maps/abortion-laws-by-state/,
Kaiser - https://www.kff.org/womens-health-policy/dashboard/abortion-in-the-u-s-dashboard/

abortion ban status rather than strict trigger-law mechanics, which is why it includes Alabama and West Virginia (which restricted access through other means) and excludes North Dakota, Utah, and Wyoming (whose trigger laws were enjoined, delayed, or otherwise not enforced during our study period). Table 2 shows an alternative classification used in recent work by Dench et al. (2024). This scheme groups states into those with a near total abortion ban, protected abortion rights, and excludes states with less clear categorization. We conducted our analysis using both classification systems and found that the results were very similar, suggesting that the distinctions are not central to measuring possible contraception responses to the policy changes.

4 Research Design

Our analysis is built around two main elements: (i) a DID design created because a group of 13 states had "trigger bans" and pre-existing state laws that became binding after the *Dobbs* decision, and (ii) a large database of longitudinal health insurance claims data. The claims data allows us to move beyond simple measures of contraception utilization and to take advantage of the DID to study responses to abortion regulations on several different margins of contraception choice, across geographic areas with different health care supply conditions, and across sub-populations who exhibit different levels of engagement with preventive health care.

4.1 Sample Construction

We begin with the *Enrollment Detail* dataset from MarketScan for the years 2016 through 2024. We construct two-year decision cohorts using baseline years 2016 through 2023 and follow-up (lead) years 2017 through 2024. In each year, we restrict the sample to women aged 15 to 49. To ensure continuous coverage and accurate measurement of contraceptive use, we further require individuals to be continuously enrolled in an insurance plan for all 12 months of the calendar year. In 2016, the full enrollment file included $33,559,337$ individuals. Restricting to women aged 15âĂŞ49, reduced the sample $9,635,368$. Applying the 12 month continuous enrollment criterion reduced the sample further to $6,447,129$ individuals. The restrictions had a similar effect on sample sizes in the 2017-2023 data as well.

Enrollment Cohorts We use the overall analytic sample to construct decision cohorts that allow us to study extensive margin contraception choices among people who were previously not using prescription contraception, and intensive margin choices among people who were already using short-acting contraception. We construct these cohorts from people who were enrolled in a plan for two consecutive years. To see the idea,

let $i = 1, \ldots, N$ index the individuals in our full analytic file. For each person, let E_i^t be an indicator that person i was enrolled in a commercial plan in calendar year t. For example, a person with $E_i^{2016} = E_i^{2017} = 1$ was enrolled in both 2016 and 2017. Now let $c = 2016, \ldots, 2023$ represent a sequence of two-year enrollment cohorts, where c is the baseline year and $c+1$ is the lead (follow-up) year. Person i is included in the data set for cohort c if she was enrolled in both year c and year $c+1$. Formally, person i is included in cohort c if $E_i^c = E_i^{c+1} = 1$.

Contraception Outcomes Let S_{ic}^{c+1} be a binary indicator that person i in cohort c filled at least one prescription for short acting contraception in year $c+1$. Likewise, let S_{ic}^{c} indicate whether she filled at least one prescription for short acting contraception in year c. Similarly, L_{ic}^{c} and L_{ic}^{c+1} are binary indicators that the person had a LARC insertion in year c and $c+1$.

Extensive Margin Cohort To study take up of contraception on the extensive margin, we restrict the cohort c sample to people with $S_{ic}^{c} = L_{ic}^{c} = 0$. This is the subset of cohort c that was "contraception naive" at baseline in the sense that they did not have any claims related to contraception in period c. After constructing the extensive margin sample for each cohort, we use a difference in difference strategy to study take up using S_{ic}^{c+1} and L_{ic}^{c+1} as outcomes.

Intensive Margin Cohort To study transitions to LARC among people who are already using short acting contraception, we restrict cohort c to people with $S_{ic}^{c} = 1$. In this case, the study population consists of people who had a least one claim for short acting contraception in period c and we study their continuation of short acting contraception and transition to LARC using difference in difference designs.

Table 3 reports summary statistics for the studied cohort by treatment status. The cohort sample includes individuals with two years of claims beginning in years 2017 through 2024 and contains 5,081,588 observations (2,010,994 unique women) in states with trigger bans and 10,354,706 observations (3,929,137 unique women) in our clean control states. The extensive-margin sample contains 3,860,583 observations in trigger-ban states and 7,781,533 observations in clean-control states, while the intensive-margin switcher sample contains 1,068,725 observations in trigger ban states and 2,242,605 observations in clean control states. Age and prior contraceptive use are broadly similar across groups, but geographic composition differs substantially because trigger-ban states are concentrated in the South whereas our clean-control states are disproportionately located in the Northeast and West.

4.2 Difference in Differences

Notation and Assumptions Use $s(i)$ to represent the state where patient i resides. We limit the sample to people residing in either a trigger ban state or a clean control state. Let A_s represent the cohort year when state s first adopts a near ban on abortion.

We adopt potential outcomes notation to represent causal relationships. Focusing on short acting contraception choices, let $S_{ic}^{c+1}(0)$ represent the contraception use of person i in cohort c in the absence of an abortion ban. $S_{ic}^{c+1}(a)$ represents the same person's contraception use in period $c+1$ under a scenario where the state adopts an abortion ban in period a. The person's realized contraception use is $S_{ic}^{c+1} = S_{ic}^{c+1}(0) + \sum_a \mathbf{1}\{A_{s(i)} = a\}\theta_{ic}^{c+1}(a)$, where $\theta_{ic}^{c+1}(a) = S_{ic}^{c+1}(a) - S_{ic}^{c+1}(0)$ is the person specific causal effect of banning abortion for the first time in period a on contraception use for person i in period $c+1$. In our case, $A_s = 2022$ in the trigger ban states, and we set $A_s = \infty$ for clean control states.

Our identification strategy rests on two main assumptions.

Assumption 1 (Common Trends)**.** *In the absence of treatment exposure, trends in contraception utilization rates would follow a common trend in trigger ban states and clean control states. Specifically, let* $\Delta(c, c')^b = \Pr\left(S_{i,c}^{c+1}(0) \mid Ban_{s(i)} = b\right) - \Pr\left(S_{i,c'}^{c'+1}(0) \mid Ban_{s(i)} = b\right)$ *represent the change in contraception use between cohort* c' *and* c *for* $c > c'$ *for* $b \in \{0, 1\}$*. Under the common trend assumption* $\Delta_{c,c'}^{Ban} = \Delta_{c,c'}^{Control}$ *so that change over time is the same for both groups.*

Assumption 2 (No Anticipation)**.** *The no anticipation assumption requires that the abortion bans adopted in 2022 did not affect contraception choices people made in periods before 2022. Specifically, we assume that* $S_{ic}^{c+1}(a) = S_{ic}^{c+1}(0)$ *for all* $c < a$*.*

Estimation We estimate stacked DID models (Wing et al., 2024), comparing changes in contraceptive behavior in states that implemented abortion bans to changes in control states that did not restrict abortion access after *Dobbs*. Letting $Ban_{ic} = \mathbf{1}\{c \geq A_{s(i)}\}$ indicate whether person i from cohort c is subject to an abortion ban. The simple two period DID regression is:

$$S_{ic}^{c+1} = \beta_{DID} Ban_{ic} + a_s + b_c + e_{ic}$$

In the model, a_s and b_c represent state and cohort fixed effects. β_{DID} measures the differential change in the contraception outcome among people in Trigger Ban states in the post period. We fit this regression to data on short acting and long acting contraception in period $c+1$ in both the intensive and extensive margin

samples.

We also estimate event study regressions with the following form:

$$S_{ic}^{c+1} = \sum_{h=-5;h\neq-1}^{1} \delta_h \mathbf{1}\{c - A_{s(i)} = h\} + a_s + b_c + e_{ic}$$

This specification allows the effect of the abortion bans to change over time in the post period. δ_0 measures average effect of the abortion ban on contraception use in the year of the ban, and δ_1 measures the effect of the ban after one year. The event study specification also provides a partial test of the common trend and no-anticipation assumptions. For $h \in \{-2, -3, -4, -5\}$, δ_h is a placebo DID comparison the measures deviations in contraception use between the trigger ban and control states prior to the implementation of the abortion bans. These coefficients should equal 0 under the common trend and no-anticipation assumptions.

We will expand the basic model to an event study specification to test for differential pre-trends, use covariate matched samples to improve the credibility of the core assumptions, and fit separate models by age group and geography to explore heterogeneity across sub-populations.

Age Standardization To address potential confounding due to changes in the age composition of cohorts over time, we implement an age standardization procedure. We calculate age weights for each decision-year cohort c and age a as follows:

$$W_{c,a} = \frac{1}{s_{c,a}} \times S_{2016,a}$$

where $s_{c,a}$ denotes the share of individuals of age a in the follow-up year $t+1$ of cohort c. $S_{2016,a}$ represents the corresponding age share in the 2016 cohort which serves as the reference population. These weights adjust for any cohort-specific shifts in age distribution by rescaling each cohort to match the age distribution in 2016. We apply the resulting weights in computing age-standardized outcomes across all decision margins. The resulting estimates are nearly identical to the unweighted versions.

4.3 Heterogeneity by Distance to Abortion Provider

To study heterogeneity in the magnitude of the access shock, we attach an area-level measure of travel distance to the nearest abortion provider using patient geography. The distance measure is assigned at the patient's state-by-metropolitan-area geography rather than the individual home-address level. For each state-by-metropolitan-area geography, we measure the minimum driving distance to the nearest abortion provider in 2021 and in 2023, and then compute the change between those two years.

The current distance-heterogeneity design groups trigger-ban geographies into three bins based on that 2021 to 2023 change: a 0–<150 mile increase, a 150–<400 mile increase, and a 400+ mile increase. These geographic bins are then assigned to women based on residence. For each distance bin, we estimate the same DID and event-study specifications described above, comparing women in trigger-ban geographies within that bin to the common pool of clean-control women. Thus, the by-distance analysis is a series of separate treated-versus-clean-control comparisons rather than direct comparisons across treated bins. All by-distance models use state and cohort fixed effects, state-clustered standard errors, and age-standardization weights.

5 Results

To contextualize our DID and event study results, we begin by examining trends in any kind of contraceptive use. Figure 1 displays the annual share of individuals using either short-acting hormonal contraceptive or LARC procedures. Across all states, there is a steady decline in the use of these contraceptive methods from 2019 through 2023.

Figures 2 and 3 show the percent of women aged 15–49 in MarketScan by abortion ban status using any short-acting hormonal contraceptive by year and the percent using any LARC by year, respectively. Across all states, the share using short-acting methods declines sharply beginning in 2019. LARC uptake follows a different pattern: increasing from 2017 to 2019, falling in 2020—likely reflecting pandemic-related care disruptions—then rebounding modestly in 2021 before entering a gradual decline.

We first examine new contraceptive uptake at the extensive margin, focusing on women with no observed contraception use in the prior year (the "naive" sample). Figure 4 shows the share of the contraceptive naive women who initiate short-acting hormonal contraceptive by year and by abortion ban states while Figure 6 shows the share initiating LARCs. Figures 4 and 6 are unweighted and Figures 5 and 7 are weighted by age. Between 2017 and 2023, the share of women initiating short-acting methods declines steadily in both treatment and control states, with no visible divergence following the *Dobbs* decision. For LARC uptake, both groups exhibit modest increases from 2017 to 2019. Beginning in 2020, however, treated states experience a continuous decline, while control states show a temporary dip in 2020 followed by a brief rebound in 2021 before declining again.

Figure 8 presents event study estimates of short-acting hormonal contraceptive take-up, comparing treatment and control states. Estimates in the pre-*Dobbs* period are stable and centered around zero, suggesting no differential pre-trend. Table 4 shows the DID regression estimates, which are statistically zero. Post-*Dobbs*, we find no statistically significant increase in short-acting contraceptive uptake in treatment states.

Figure 9 presents analogous estimates for LARC initiation among the naive sample. The event-study estimates do not show a clear post-*Dobbs* increase. Instead, they include a spike in 2020 in treatment states, potentially reflecting anticipatory behavior or pandemic-related changes. Table 4 shows that the pooled DID estimate for LARC initiation is small and negative (-0.12 percentage points), with marginal statistical significance at the 10 percent level. Overall, these findings point to limited responsiveness at the extensive margin, with no evidence of a meaningful increase in LARC initiation following abortion bans.

Transition to LARC Next, we examine LARC uptake at the intensive margin. Figure 11 plots the share of LARC uptake among women who use short-acting hormonal contraceptive in the year before, separately for treatment states and control states. Figure 11 is unweighted and Figure 12 is age-weighted. Both treatment states and control states exhibit a general decline in the share switching to LARCs, though with notable differences in timing and levels. In 2017, control states began with higher rates of LARC uptake than treatment states. However, by 2019, the gap narrowed, and both groups experienced a decline in 2020, likely reflecting pandemic-related disruptions in access to in-person care. After 2020, control states showed a brief rebound in 2021 before continuing to decline, while LARC uptake in treatment states remained relatively flat through 2021 before declining.

Figure 10 reports event study estimates for LARC uptake among women already using short-acting hormonal contraceptive. Estimates in the pre-*Dobbs* period are increasing over time yet statistically indistinguishable from zero until 2020, during which treatment states have a significant increase compared to control states. This difference may reflect pandemic-related disruptions or early anticipatory shifts. Table 5 does find a very small and statistically significant increase in switching at the intensive margin: a 0.18 percentage point increase in the rate of switching to a LARC from a short-acting hormonal contraceptive after *Dobbs* in treated states. However, the event studies do suggest that the pretrends differed slightly across treated and control states, and there may be anticipatory or pandemic-related changes in access before *Dobbs*.

Heterogeneity by Distance to Abortion Provider

Within trigger-ban states, the change in access to abortion services after *Dobbs* varied across geographic regions. For some women in ban states, the nearest abortion provider remained right across the state border as their neighboring state maintained abortion access while others may have faced a serious constriction in the availability of any nearby abortion providers because neighboring states also banned abortions. We examine whether the contraceptive response differs by the magnitude of the change in travel distance to the nearest abortion clinic. We split women in trigger ban states into three subgroups based on the change in minimum driving distance to the nearest abortion-providing facility between 2021 and 2023: women in areas whose

nearest clinic distance increased by less than 150 miles (0–149 miles), those in areas with a distance increase of 150 to 399 miles, and those whose access worsened by 400 miles or more. Each subgroup is compared against the same pooled sample of clean-control state women. The estimation uses the same TWFE event study specification as the main results. The by-distance event studies and TWFE estimates reported here use state and cohort-year fixed effects, standard errors clustered at the state level, and age-standardization weights. We present the event studies as primary evidence with corresponding trend figures for context of levels and pre-*Dobbs* trajectories.

Figures 22, 24, and 26, together with Tables 6, 7, and 8, show event study estimates and TWFE coefficients for the extensive margin or the short-acting hormonal contraceptive take-up among prescription contraception-naive women across the three distance-change groups. In the 0–149 mile group, the TWFE estimate is −0.34 percentage points (SE 0.28), statistically indistinguishable from zero. The 150–399 mile group yields +0.46 percentage points (SE 0.53), also null. In the 400+ mile group, the estimate is +0.11 percentage points (SE 0.22). Pre-period coefficients in all three event studies are generally close to zero, providing little evidence of differential pre-trends. The corresponding trend figures (Figures 13, 15, and 17) confirm no visible divergence across any distance bin following the *Dobbs* decision.

Figures 23, 25, and 27 present the corresponding estimates for LARC initiation at the extensive margin. Estimates in the 0–149 and 150–399 mile groups are statistically zero throughout, with no systematic pre-trend divergence. The 2020 spike observed in the pooled LARC event study (Figure 9) appears across all distance subgroups, suggesting it reflects a broader common pattern unrelated to abortion access. In the 400+ mile group, however, the TWFE estimate is a statistically significant −0.11 percentage points (SE 0.05, $p < 0.05$; Table 8): naive women in the areas with the largest access loss were *less* likely to initiate LARC after the ban relative to clean-control states. This finding is consistent with the idea that many abortion clinics also provided other reproductive care, including LARC insertions and prescription birth control, and when clinics close and the closest available provider is hundreds of miles away, the ability to access long-acting reversible contraceptives is diminished, just as the ability to access an abortion is diminished when a clinic closes. In addition, LARC insertion does require in-person care, as opposed to abortions which can be managed medically and from telehealth providers. The trend figures (Figures 14, 16, and 18) show that this negative divergence is concentrated in the 400+ mile group.

Figures 28, 29, and 30, and Tables 9, 10, and 11 examine LARC take-up at the intensive margin by distance-change group. Among women already using short-acting hormonal contraceptives, the TWFE estimate for the 0–149 mile group is +0.17 percentage points (SE 0.07, $p < 0.05$) and the estimate for the 400+ mile group is +0.16 percentage points (SE 0.07, $p < 0.05$), both statistically significant. The 150–399 mile group yields a similar point estimate of +0.17 percentage points but with a wider confidence interval (SE

0.16), reflecting the smaller sample of 211,000 women in that bin. The pattern does not follow a dose-response gradient: the middle distance bin is no more responsive than the smallest, and precision rather than effect size appears to drive the significance pattern. The trend figures (Figures 19, 20, and 21) show broadly parallel trajectories across bins and the clean-control group, with no bin displaying a sharply distinct post-2021 acceleration in LARC switching.

The results from the distance analysis suggest that there is not a linearly increasing or decreasing change in the contraceptive response to abortion restrictions and abortion clinic closures. For the extensive margin, there is no change in the initiation of short-acting hormonal prescription methods among women who were previously not using prescription contraception. Taken together, the by-distance results do not support a monotone dose-response account of the contraceptive response to abortion restrictions. For LARC insertions at the extensive margin, there is a statistically significant decrease in likelihood of a new LARC insertion of 0.11 percentage points, which could be due to the broader contraction in the availability of reproductive care that results from abortion clinic closures. For the intensive margin, we find small and positive effects for those in the 0–149 mile and 400+ mile distance change groups, but no effect for those in the 150–399 mile group. These results align with the main analysis in showing no large, consistent, or monotone contraceptive response to post-*Dobbs* abortion bans.

6 Conclusion

This study examines whether abortion bans following the 2022 *Dobbs v. Jackson Women's Health Organization* decision changed contraceptive behavior in the United States. Using longitudinal claims data from Merative MarketScan, we analyze changes in contraception use along both the extensive and intensive margins, comparing outcomes in states that enacted bans to those that did not.

Our findings suggest limited behavioral response to the contraction in abortion access. Among contraceptive naive women, we observe no significant increase in the uptake of short-acting hormonal methods or LARCs after *Dobbs*. These null effects hold across the regression estimates and raw descriptive trends, which show parallel downward movement in short-acting contraceptive use across treatment and control states. For LARC uptake at the extensive margin, we see an increase in 2020 followed by decline in treatment states, suggesting the observed trends may have been driven by factors unrelated to abortion policy, such as pandemic-era disruptions. At the intensive margin—among existing contraceptive users—we find a small, statistically significant increase after *Dobbs*, though the event studies are statistically zero, and there may be pandemic-related or anticipatory behavior changes in 2020.

Our analysis is limited to commercially insured women, a population that likely has better baseline

access to contraception and more financial resources to navigate reproductive health care changes. Future research should examine the effects of abortion bans on lower-income populations, Medicaid beneficiaries, and uninsured individuals, who may be more vulnerable to both abortion access loss and contraceptive supply constraints.

Our analysis also focuses on the near-term aftermath of *Dobbs*. Behavioral responses may take time to materialize, especially if they depend on accumulating awareness, changes in provider behavior, or shifts in social norms. We exclude states where abortion access was in question, as many state bans or legislation was challenged in court. Additional analyses examining the precise timing of abortion bans using staggered DID or synthetic control designs may provide a more precise estimate of how abortion bans impact the extensive and intensive margins.

We also plan to examine heterogeneity in contraceptive responses by age, access to care, and indicators of planning behavior. Younger women may be more responsive to changes in perceived pregnancy risk, particularly if they face a longer time horizon of potential fertility and family planning. Geographic variation in provider availability and scope-of-practice regulations may moderate the ability to switch methods, especially toward LARCs that require in-person visits. Finally, we will use engagement with other forms of preventive care—such as routine screenings or annual wellness visits—as proxies for time preferences and health system engagement, which may help distinguish forward-looking individuals from those more likely to delay care.

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Tables and Figures

Tables

Table 1: State Abortion Policy Classification: Trigger Ban, Clean Control, and Other

Abortion Status	**States**
Trigger Ban	AL, AR, ID, KY, LA, MS, MO, OK, SD, TN, TX, WV
Clean Control	AK, CA, CO, CT, DE, DC, HI, IL, ME, MD, MA, MN, MT, NV, NH, NJ, NM, NY, OR, PA, RI, VT, VA, WA
Other	AZ, GA, IN, IA, KS, MI, NE, NC, ND, OH, SC, UT, WI, WY, FL

Table 2: Dench et al. State Abortion Policy Classification: Total Ban, Protected, and Excluded

Abortion Status	**States**
Total Ban	AL, AR, ID, KY, LA, MS, MO, OK, SD, TN, TX, WI, WV
Protected	AK, CA, CO, CT, DE, DC, HI, IL, KS, ME, MD, MA, MI, MN, MT, NV, NH, NJ, NM, NY, OR, RI, VT, VA, WA
Excluded	AZ, FL, GA, IN, IA, NE, NC, ND, OH, PA, SC, UT, WY

Table 3: Cohort Sample Summary Statistics by Treatment Status (Lead Years 2017–2024)

	Trigger Ban	Clean Control	SMD
Panel A: Full Cohort Sample Size			
Cohort observations (all lead years)	5,081,588	10,354,706	
Unique women (all lead years)	2,010,994	3,929,137	
Pre-Dobbs cohort observations	3,389,530	6,906,571	
Pre-Dobbs unique women	1,565,313	2,995,656	
Naive observations (all lead years)	3,860,583	7,781,533	
Naive unique women (all lead years)	1,689,859	3,286,279	
Switcher observations (all lead years)	1,068,725	2,242,605	
Switcher unique women (all lead years)	542,478	1,086,839	
States	12	24	
Panel B: Pre-Dobbs Baseline, Full Cohort			

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	Trigger Ban	Clean Control	SMD
Mean age	33.4	33.6	-0.02
Age 15–19	11.2	11.1	0.00
Age 20–29	25.7	24.9	0.02
Age 30–39	29.1	29.3	-0.00
Age 40–49	34.0	34.7	-0.02
Metropolitan area	81.5	86.3	**-0.13**
Northeast	0.0	41.7	**-1.20**
North Central	11.4	11.2	0.01
South	87.4	13.2	**2.21**
West	1.2	33.8	**-0.95**
Region missing	0.0	0.0	NA
Panel C: Pre-Dobbs Baseline, Naive Sample			
Mean age	34.7	34.9	-0.02
Age 15–19	10.7	10.8	-0.00
Age 20–29	20.8	19.7	0.03
Age 30–39	29.0	29.1	-0.00
Age 40–49	39.5	40.4	-0.02
Metropolitan area	81.5	86.8	**-0.15**
Northeast	0.0	41.0	**-1.18**
North Central	11.2	11.1	0.00
South	87.6	12.9	**2.25**
West	1.2	35.0	**-0.98**
Region missing	0.0	0.0	NA
Panel D: Pre-Dobbs Baseline, Switcher Sample			
Mean age	29.5	29.7	-0.02
Age 15–19	13.3	12.4	0.03
Age 20–29	40.5	40.6	-0.00

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	Trigger Ban	Clean Control	SMD
Age 30–39	28.7	29.1	-0.01
Age 40–49	17.5	18.0	-0.01
Metropolitan area	81.2	84.7	-0.09
Northeast	0.0	44.7	**-1.27**
North Central	12.0	11.5	0.01
South	87.0	14.4	**2.11**
West	1.0	29.4	**-0.86**
Region missing	0.0	0.0	NA
Panel E: Pre-Dobbs Prior Contraceptive Use, Full Cohort			
Lag STHC	22.7	23.5	-0.02
Lag LARC	3.2	3.3	-0.01
Lag any contraceptive	25.1	26.0	-0.02

Notes: Sample restricted to women ages 15–49 in the final two-year cohort file and limited to trigger-ban and clean-control states. The cohort year variable indexes the lead year; observed lead years run from 2017 to 2024. Panel A reports both cohort-year observations and unique women. Panels B–E use pre-Dobbs baseline cohorts with lead year $\leq$ 2021. Shares are reported in percent. Metropolitan area is defined as an MSA code not equal to 0 and not missing. Lag contraceptive flags are filled to 0 when missing. SMD reports the standardized mean difference; absolute values above 0.10 indicate meaningful imbalance.

Table 4: Effect of Dobbs on Contraceptive Take-Up (Extensive Margin: Contraceptive Naive)

Dependent Variables:	STHC	LARC
	STHC	LARC
Model:	(1)	(2)
Variables		
treat_st	-0.0002	-0.0012*
	(0.0024)	(0.0006)
Fixed-effects		
State	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	11,642,116	11,642,116
R^2	0.00441	0.00171
Within R^2	4.17×10^{-8}	2.49×10^{-6}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 5: Effect of Dobbs on Contraceptive Switching (Intensive Margin: STHC Switchers)

Dependent Variable:	LARC
	LARC
Model:	(1)
Variables	
treat_st	0.0018**
	(0.0008)
Fixed-effects	
State	Yes
Year	Yes
Fit statistics	
Observations	3,311,330
R^2	0.00166
Within R^2	3.87×10^{-6}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 6: Effect of Dobbs on Contraceptive Take-Up: Naive, 0-<150 mi increase vs Clean Controls (Age-Weighted)

Dependent Variables:	STHC	LARC
Model:	(1)	(2)
Variables		
treat_st	-0.0034	-0.0008
	(0.0028)	(0.0006)
Fixed-effects		
State	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	8,404,893	8,404,893
R^2	0.00454	0.00226
Within R^2	2.97×10^{-6}	3.09×10^{-7}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 7: Effect of Dobbs on Contraceptive Take-Up: Naive, 150-<400 mi increase vs Clean Controls (Age-Weighted)

Dependent Variables:	STHC	LARC
Model:	(1)	(2)
Variables		
treat_st	0.0046	-0.0002
	(0.0053)	(0.0007)
Fixed-effects		
State	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	8,158,848	8,158,848
R^2	0.00442	0.00231
Within R^2	3.31×10^{-6}	9.79×10^{-9}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8: Effect of Dobbs on Contraceptive Take-Up: Naive, 400+ mi increase vs Clean Controls (Age-Weighted)

Dependent Variables:	STHC	LARC
Model:	(1)	(2)
Variables		
treat_st	0.0011	-0.0011**
	(0.0022)	(0.0005)
Fixed-effects		
State	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	8,681,835	8,681,835
R^2	0.00438	0.00221
Within R^2	4.15×10^{-7}	9.16×10^{-7}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 9: Effect of Dobbs on LARC Take-Up: Switcher, 0-<150 mi increase vs Clean Controls (Age-Weighted)

Dependent Variable:	LARC
Model:	(1)
Variables	
treat_st	0.0017**
	(0.0007)
Fixed-effects	
State	Yes
Year	Yes
Fit statistics	
Observations	2,442,398
R^2	0.00201
Within R^2	1.28×10^{-6}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 10: Effect of Dobbs on LARC Take-Up: Switcher, 150-<400 mi increase vs Clean Controls (Age-Weighted)

Dependent Variable:	LARC
Model:	(1)
Variables	
treat_st	0.0017
	(0.0016)
Fixed-effects	
State	Yes
Year	Yes
Fit statistics	
Observations	2,346,993
R^2	0.00208
Within R^2	6.67×10^{-7}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 11: Effect of Dobbs on LARC Take-Up: Switcher, 400+ mi increase vs Clean Controls (Age-Weighted)

Dependent Variable:	LARC
Model:	(1)
Variables	
treat_st	0.0016**
	(0.0007)
Fixed-effects	
State	Yes
Year	Yes
Fit statistics	
Observations	2,478,307
R^2	0.00199
Within R^2	1.15×10^{-6}

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Figures

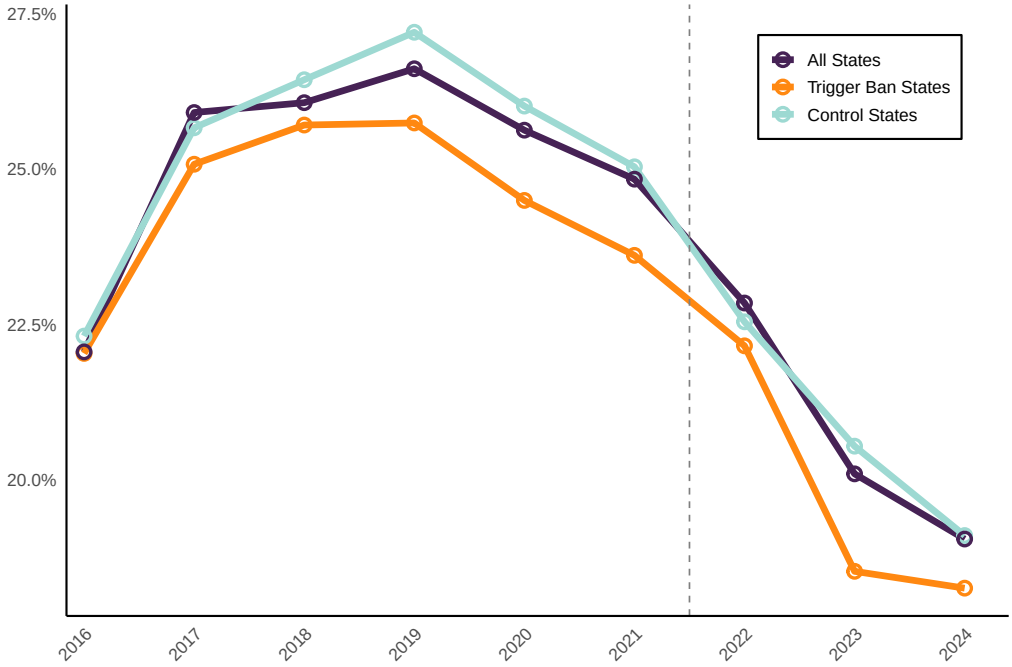


Figure 1: Annual Trends in Any Prescription Contraceptive Use

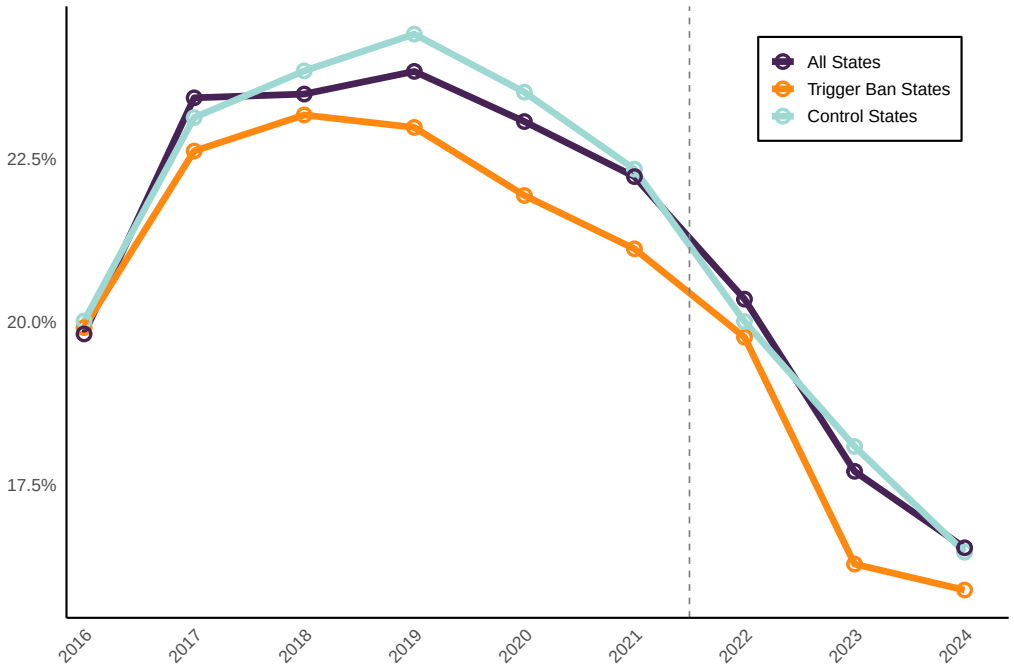


Figure 2: Annual Trends in Short-Acting Hormonal Contraceptive Use

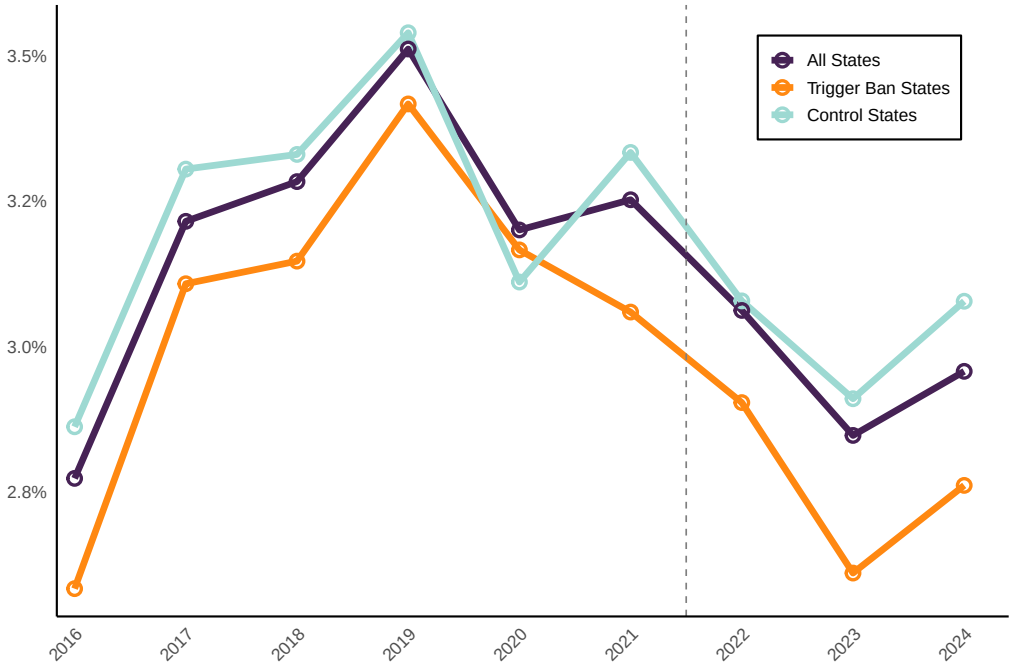


Figure 3: Annual Trends in LARC Procedure Use

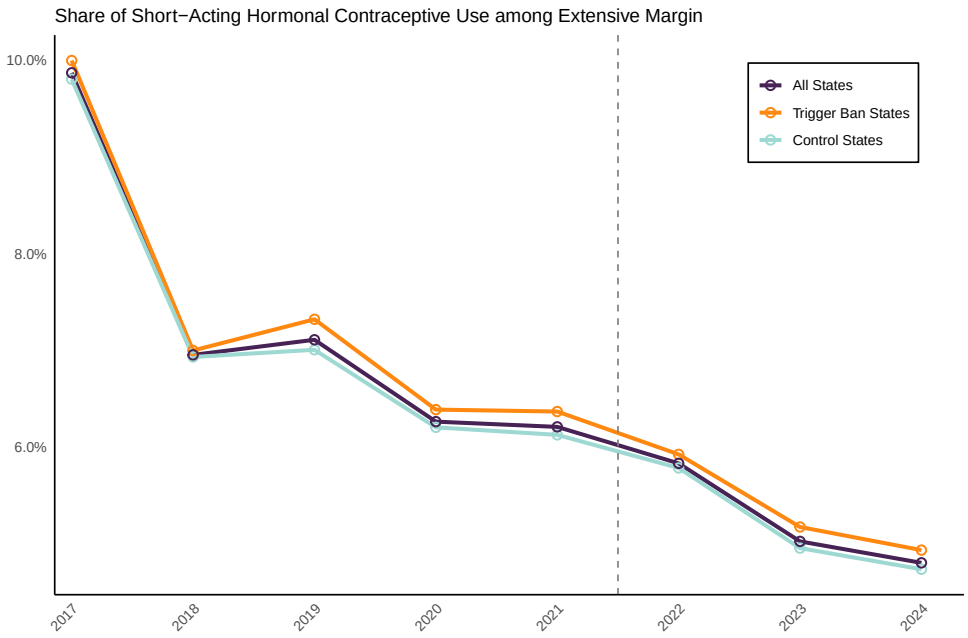


Figure 4: Extensive-Margin Short-Acting Hormonal Contraceptive Trends (Unweighted)

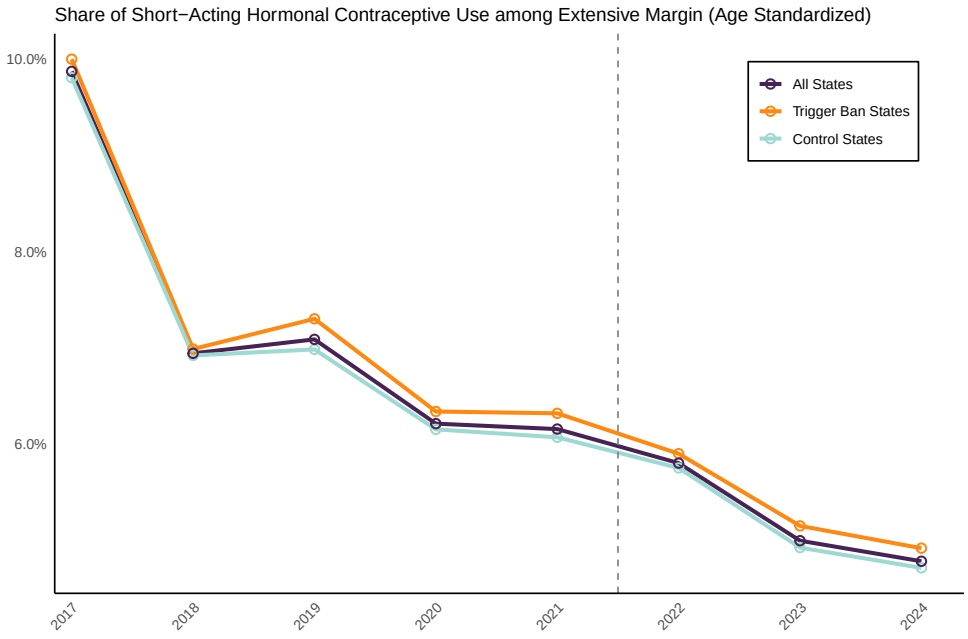


Figure 5: Extensive-Margin Short-Acting Hormonal Contraceptive Trends (Age Standardized)

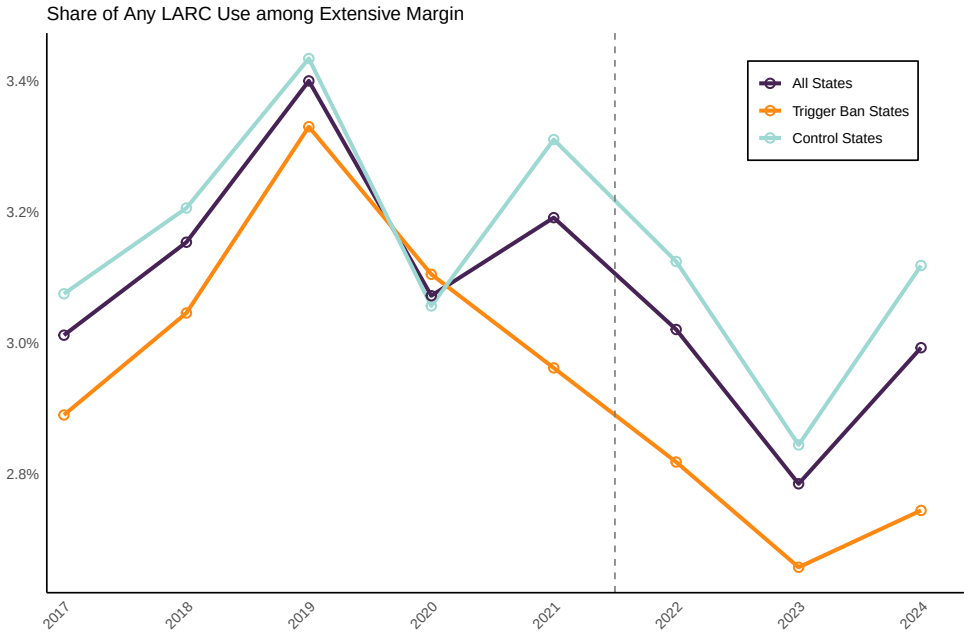


Figure 6: Extensive-Margin LARC Trends (Unweighted)

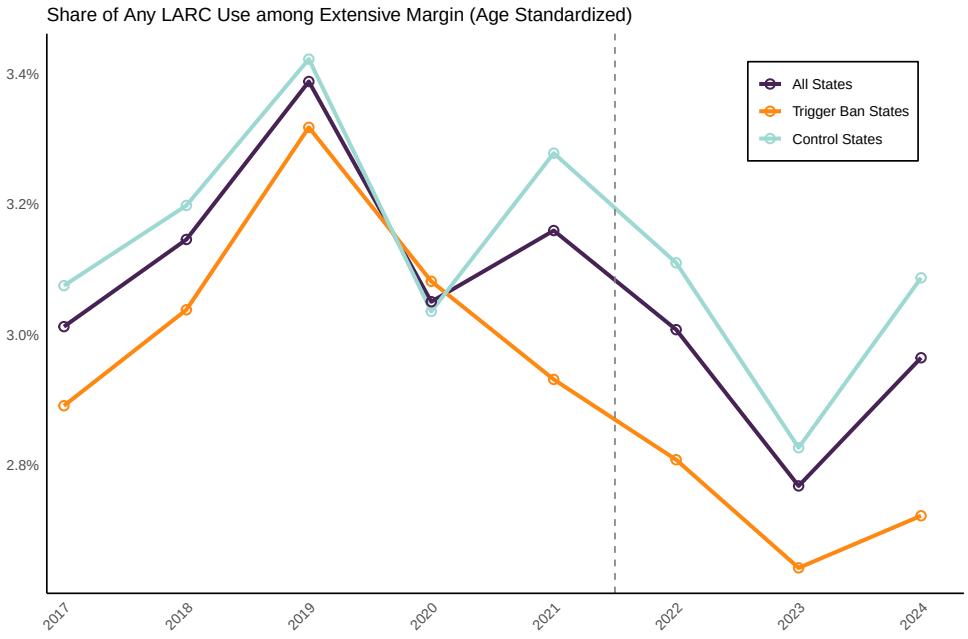


Figure 7: Extensive-Margin LARC Trends (Age Standardized)

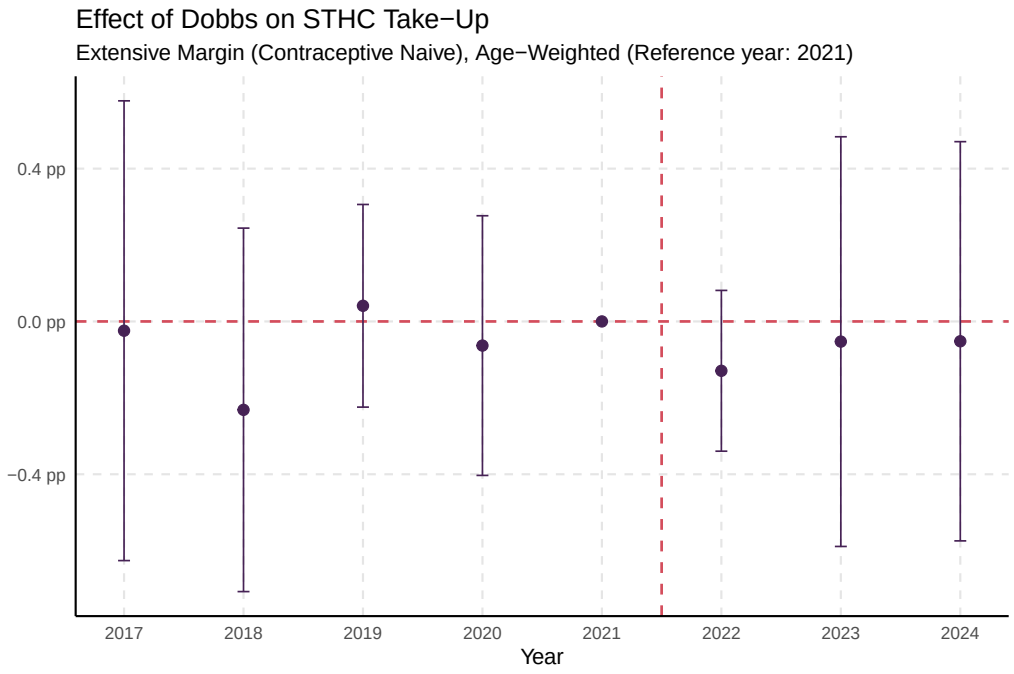


Figure 8: Event Study Estimates: Extensive-Margin Short-Acting Hormonal Contraceptive Take-Up

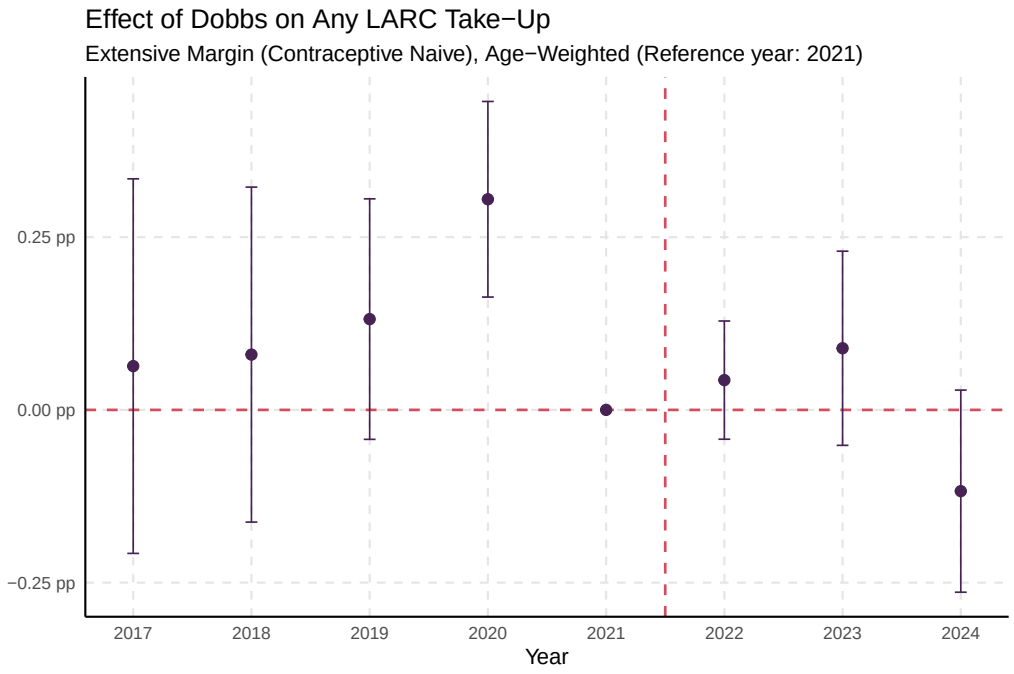


Figure 9: Event Study Estimates: Extensive-Margin Any LARC Take-Up

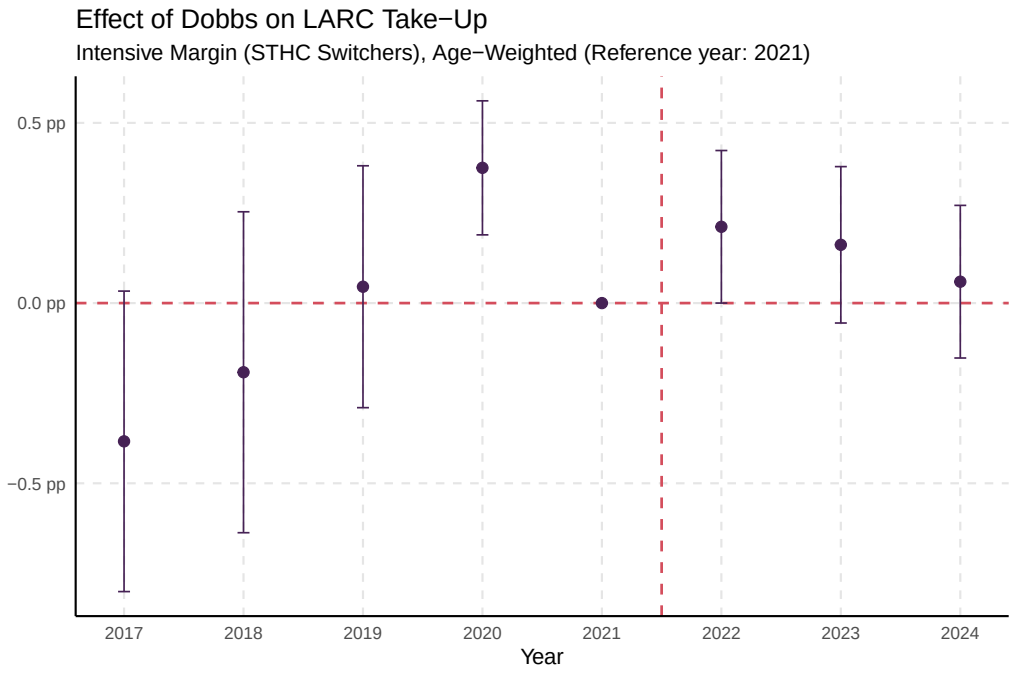


Figure 10: Event Study Estimates: Intensive-Margin LARC Take-Up

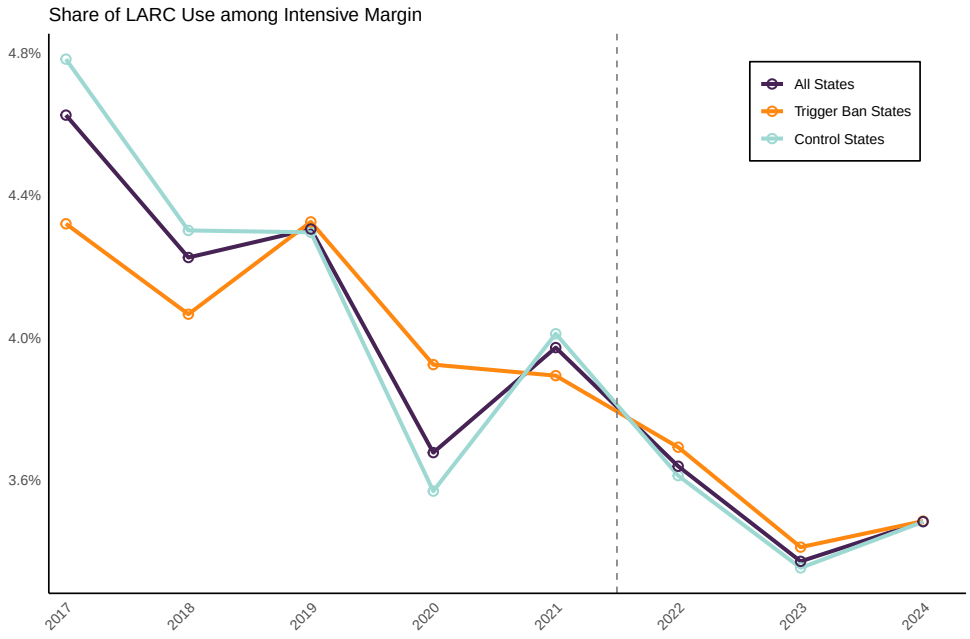


Figure 11: Intensive-Margin LARC Trends (Unweighted)

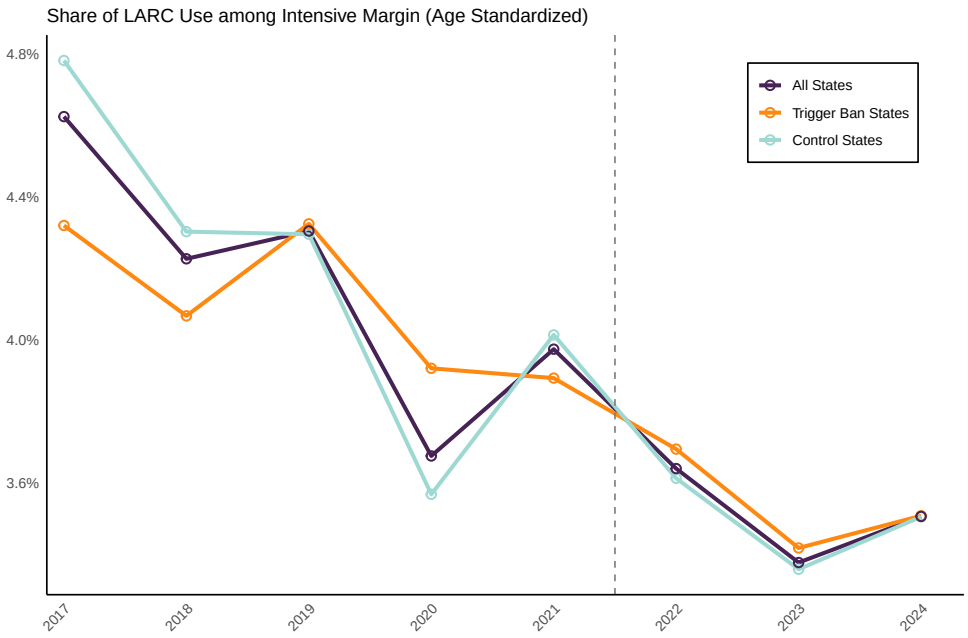


Figure 12: Intensive-Margin LARC Trends (Age Standardized)

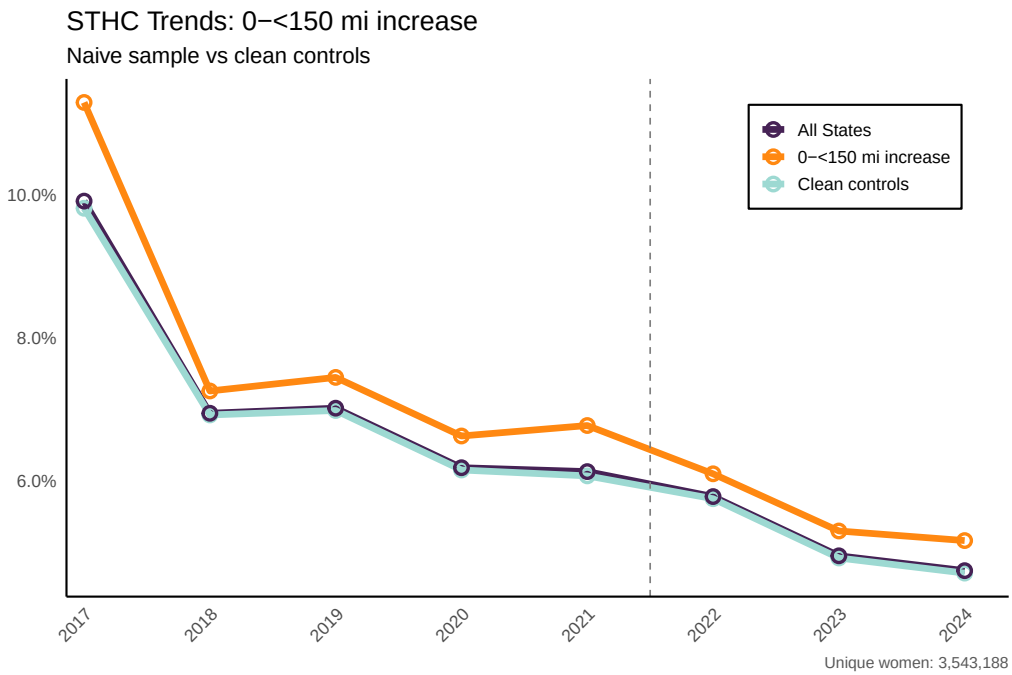


Figure 13: Extensive-Margin Short-Acting Hormonal Contraceptive Trends: 0–149 Mile Increase

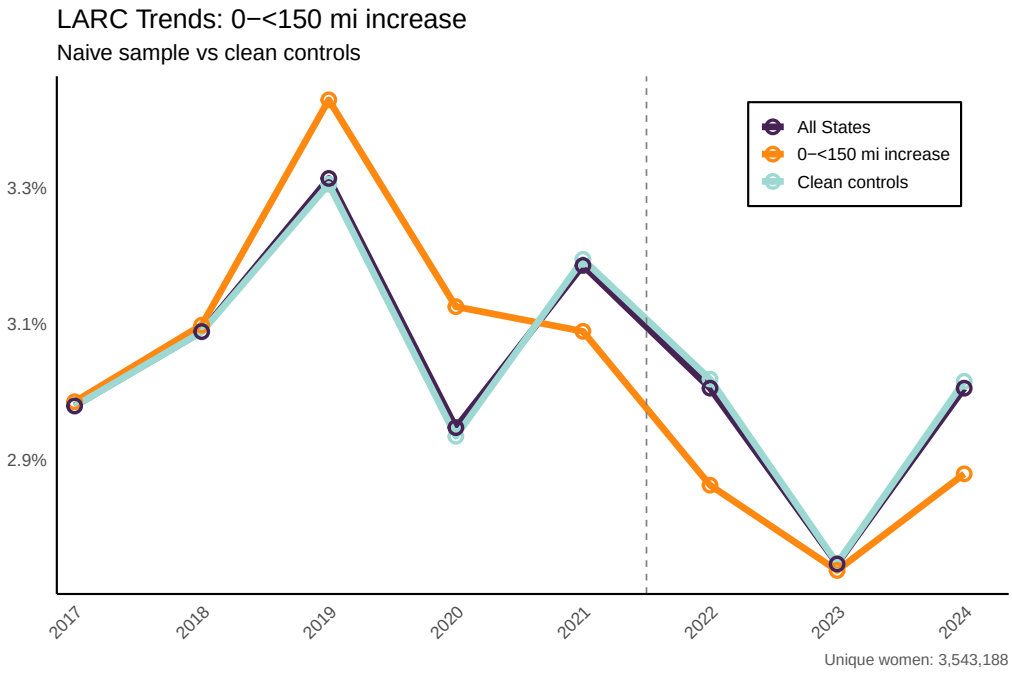


Figure 14: Extensive-Margin LARC Trends: 0–149 Mile Increase

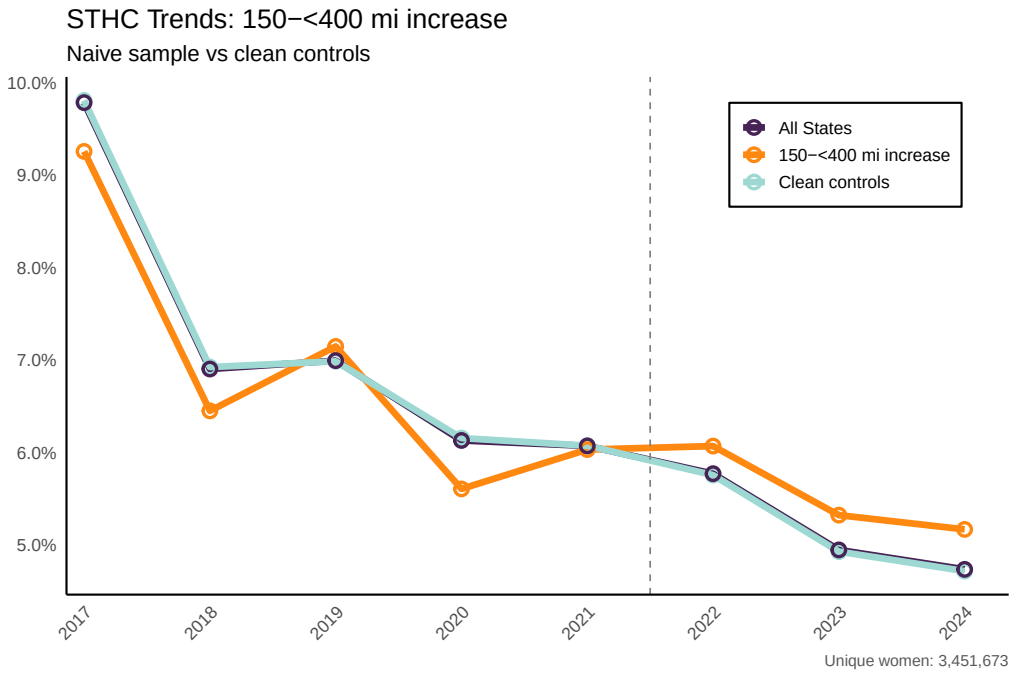


Figure 15: Extensive-Margin Short-Acting Hormonal Contraceptive Trends: 150–399 Mile Increase

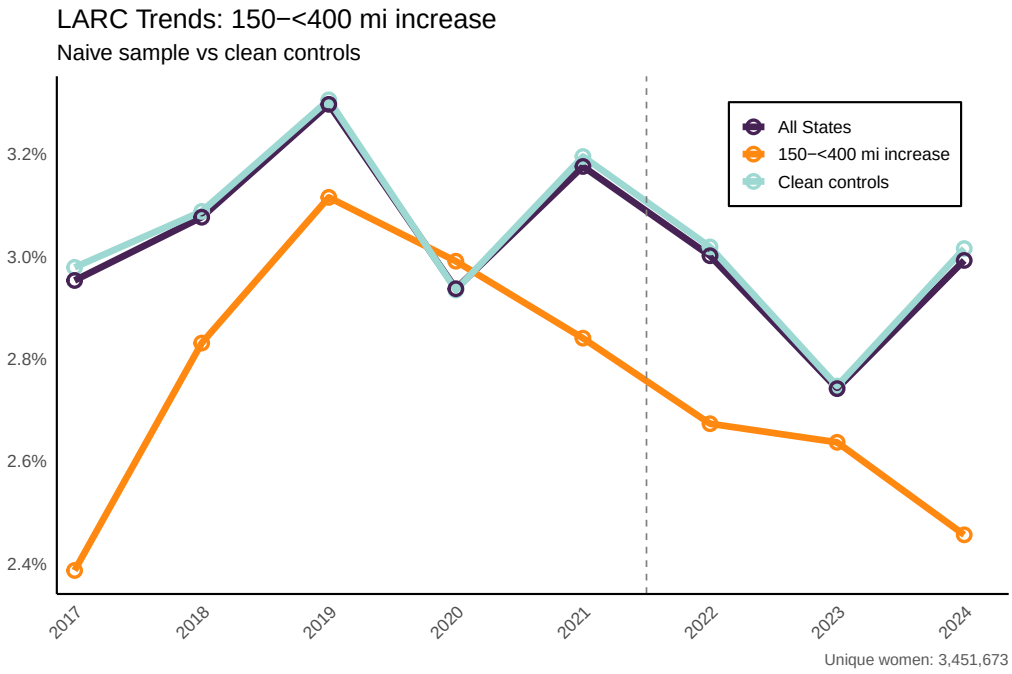


Figure 16: Extensive-Margin LARC Trends: 150–399 Mile Increase

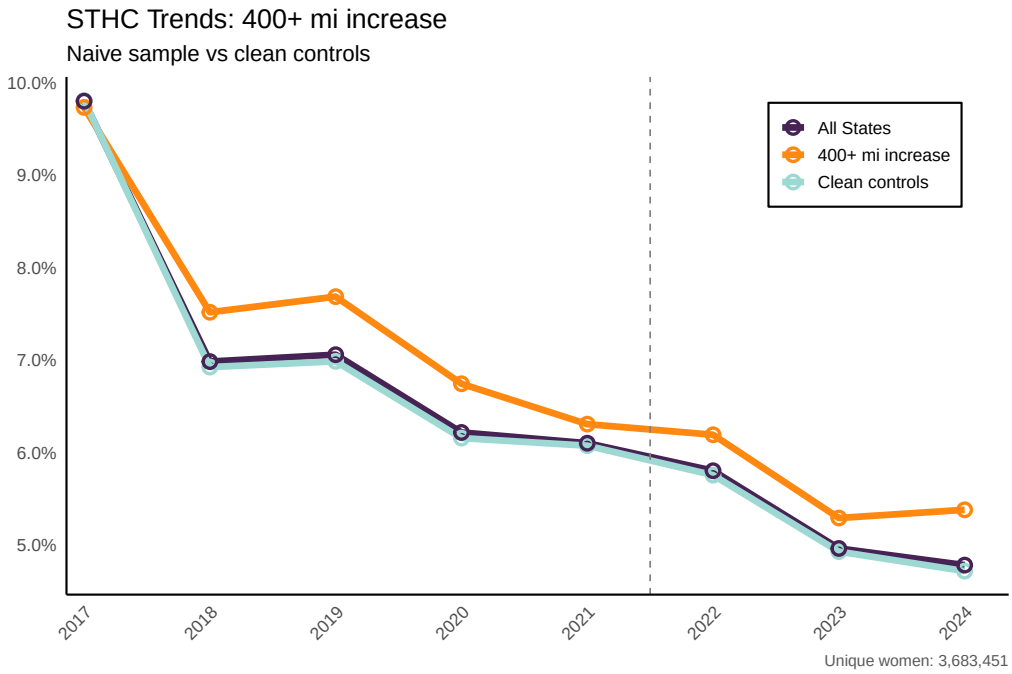


Figure 17: Extensive-Margin Short-Acting Hormonal Contraceptive Trends: 400+ Mile Increase

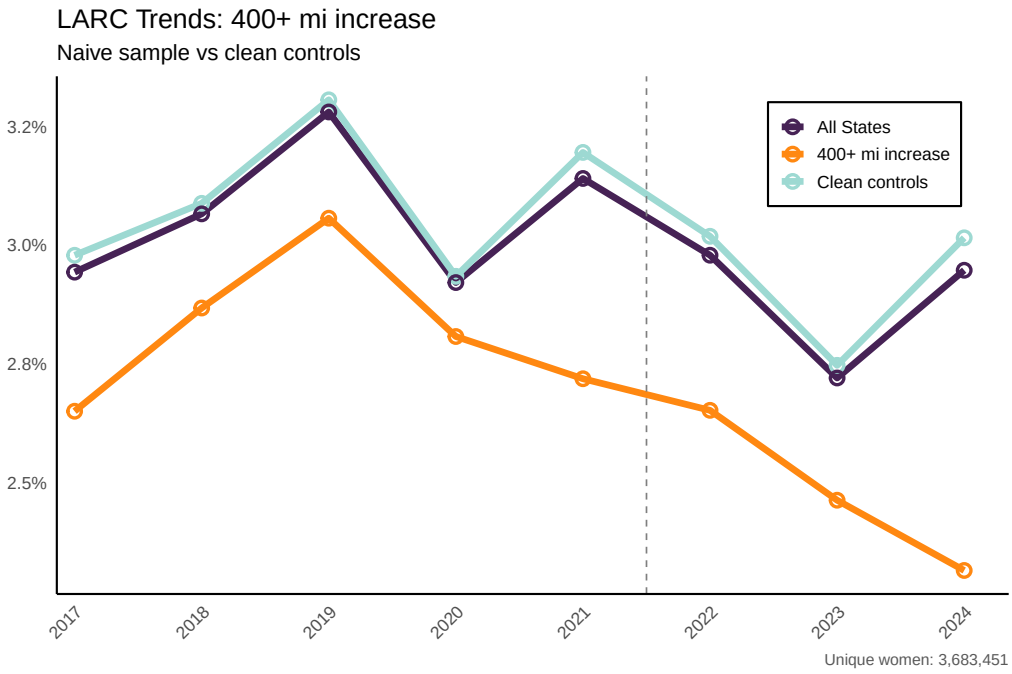


Figure 18: Extensive-Margin LARC Trends: 400+ Mile Increase

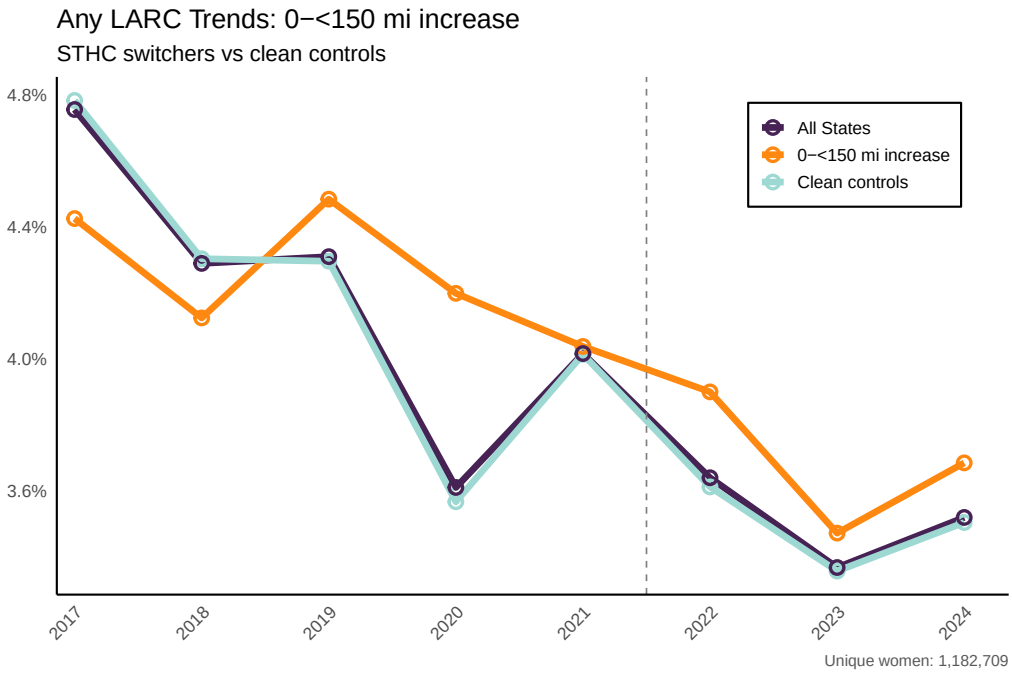


Figure 19: Intensive-Margin Any LARC Trends: 0–149 Mile Increase

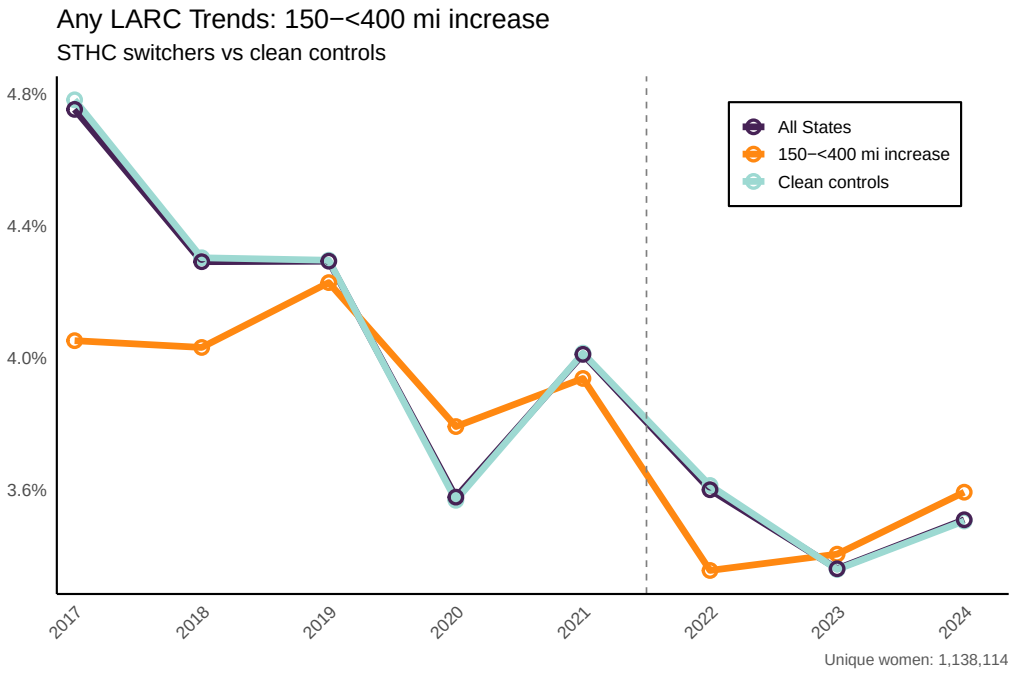


Figure 20: Intensive-Margin Any LARC Trends: 150–399 Mile Increase

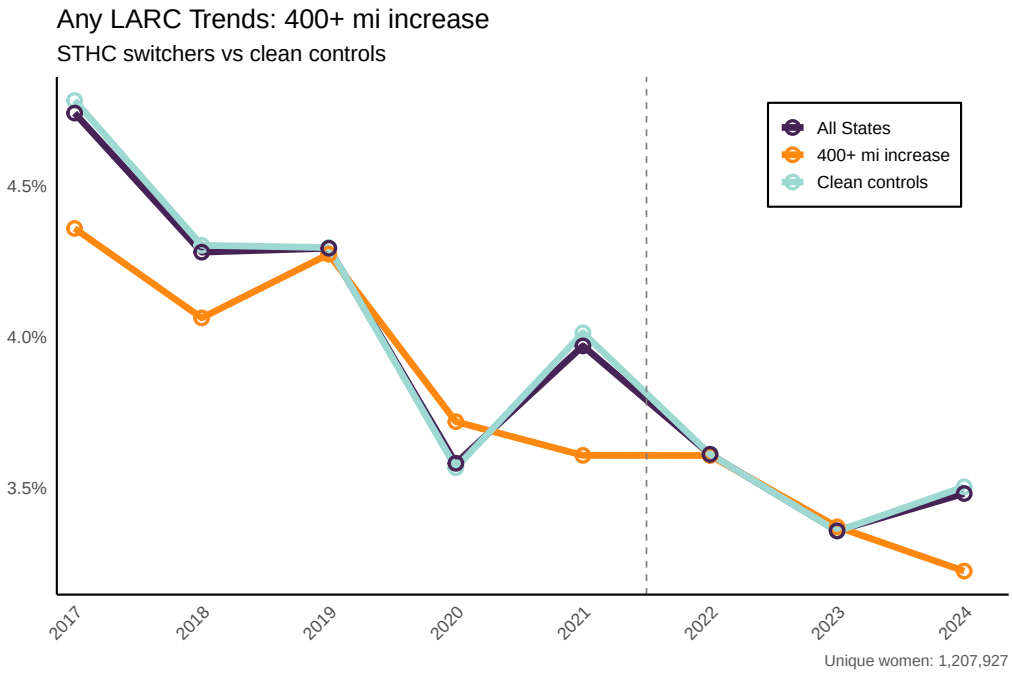


Figure 21: Intensive-Margin Any LARC Trends: 400+ Mile Increase

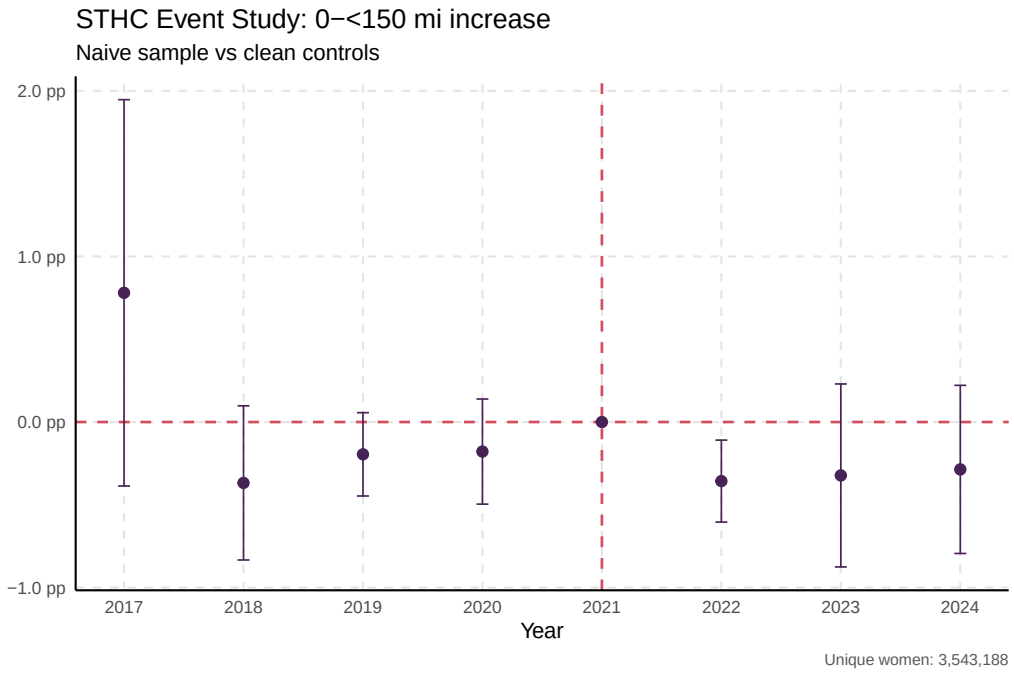


Figure 22: Extensive-Margin Short-Acting Hormonal Contraceptive Event Study: 0–149 Mile Increase

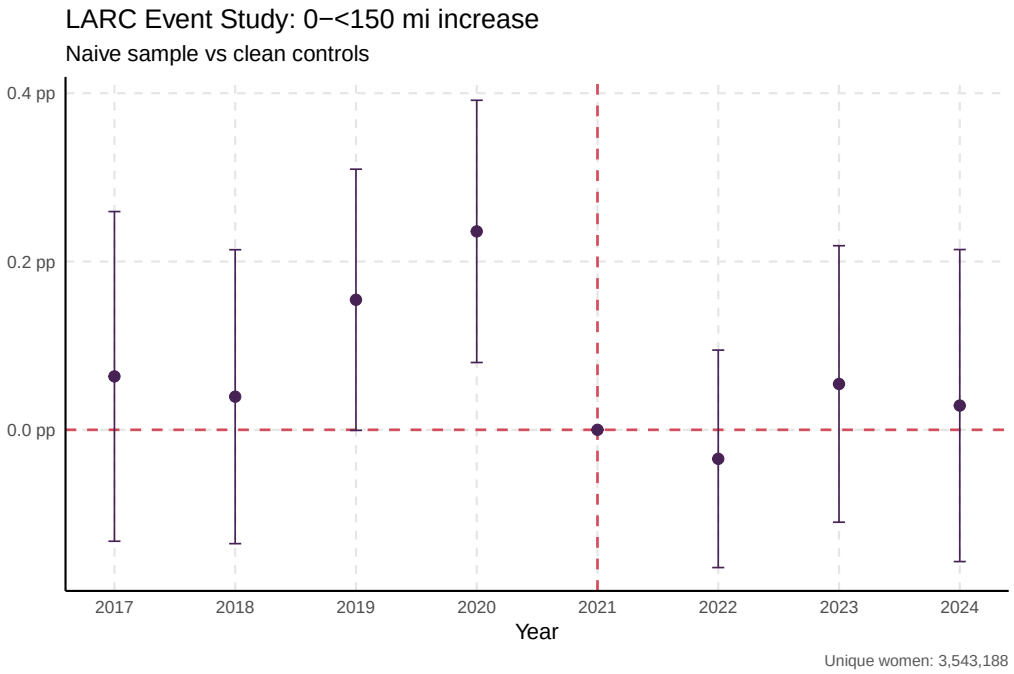


Figure 23: Extensive-Margin LARC Event Study: 0–149 Mile Increase

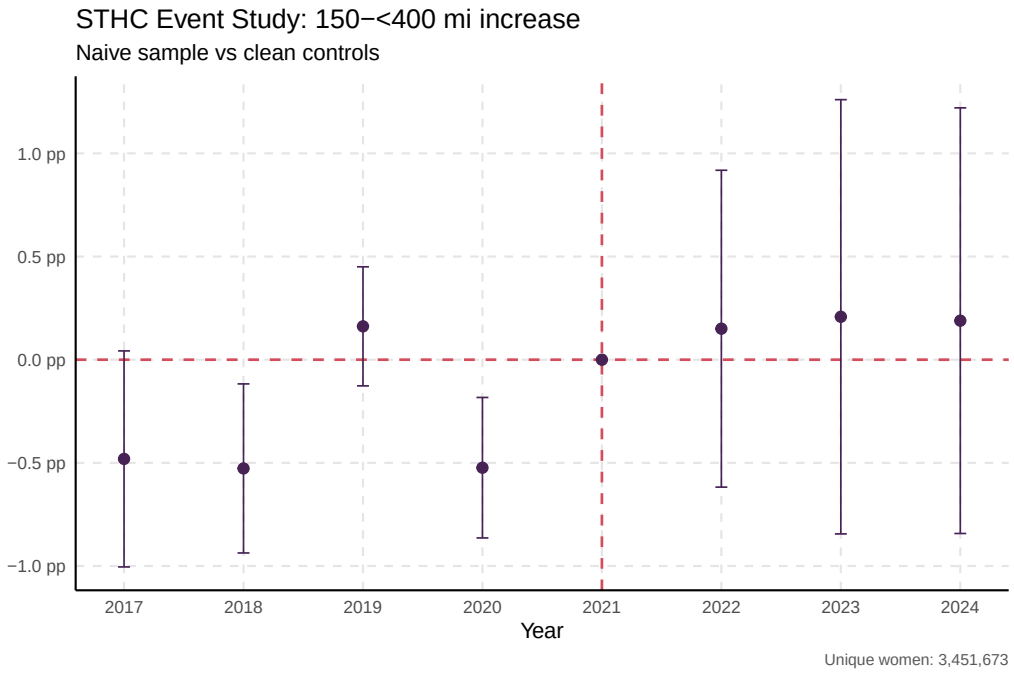


Figure 24: Extensive-Margin Short-Acting Hormonal Contraceptive Event Study: 150–399 Mile Increase

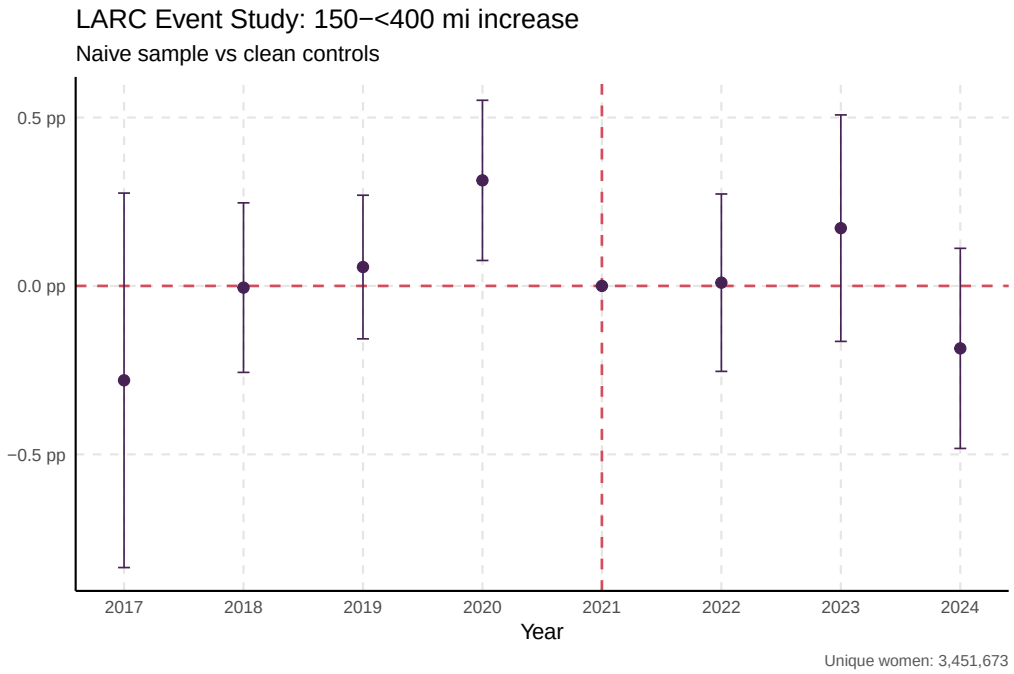


Figure 25: Extensive-Margin LARC Event Study: 150–399 Mile Increase

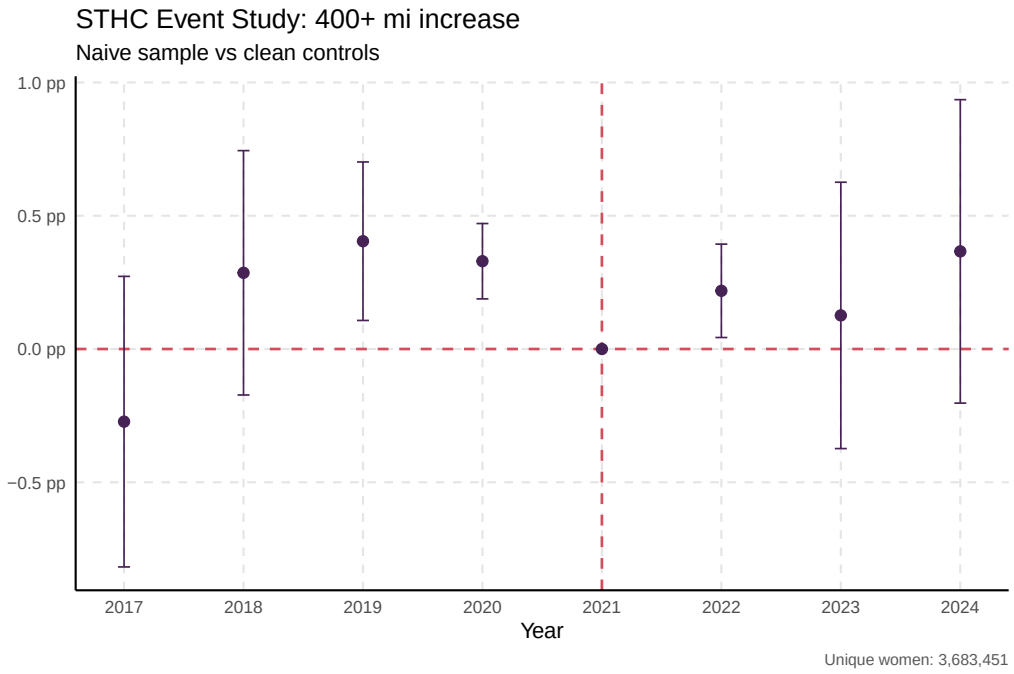


Figure 26: Extensive-Margin Short-Acting Hormonal Contraceptive Event Study: 400+ Mile Increase

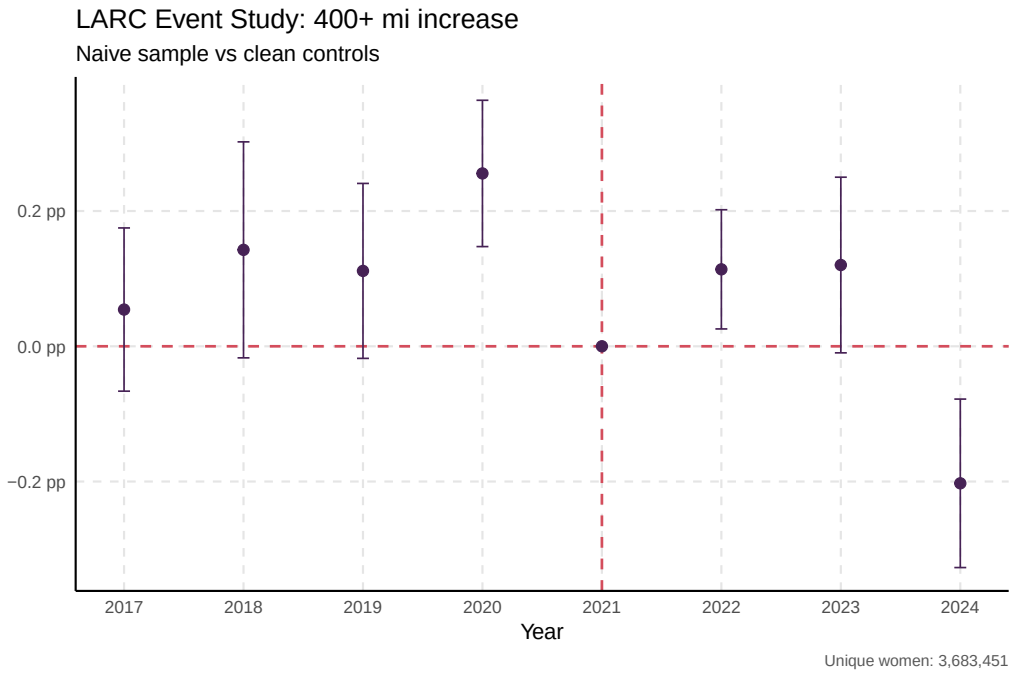


Figure 27: Extensive-Margin LARC Event Study: 400+ Mile Increase

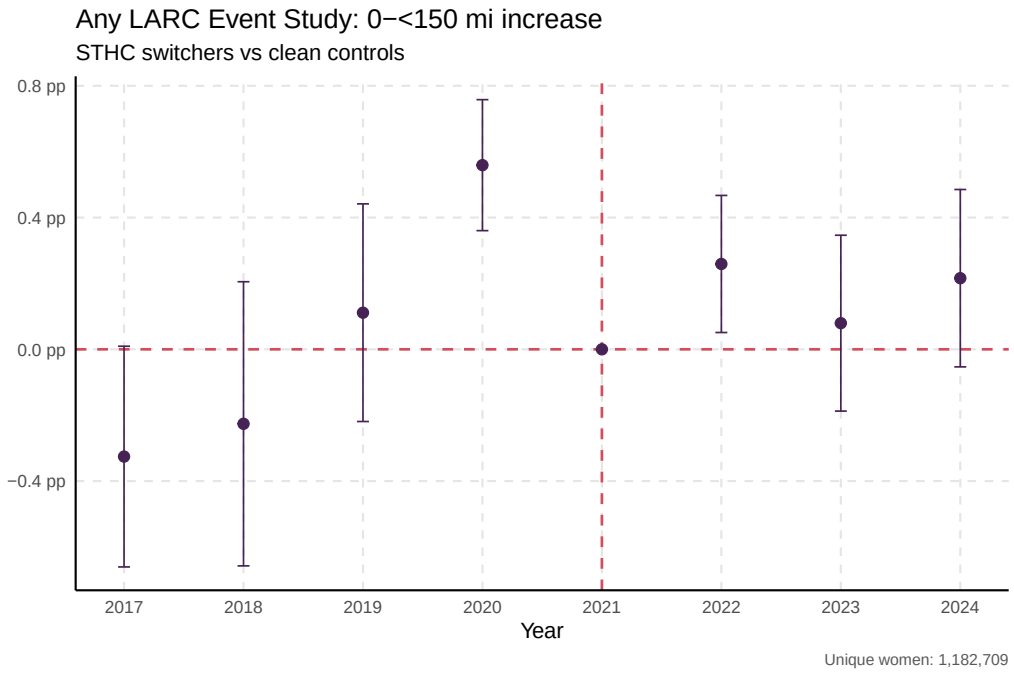


Figure 28: Intensive-Margin Any LARC Event Study: 0–149 Mile Increase

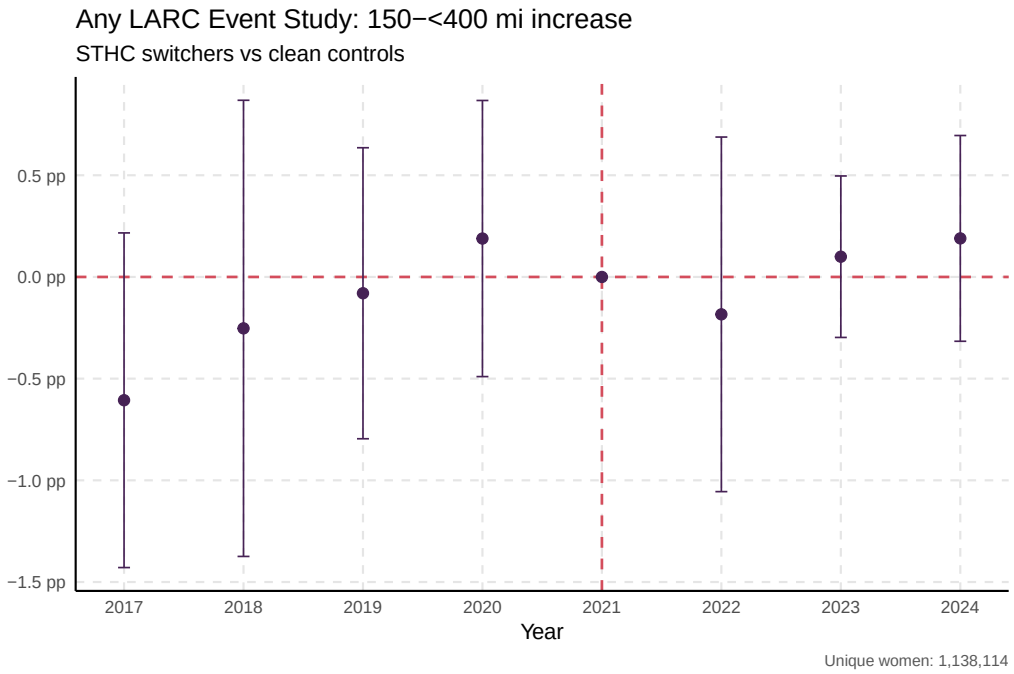


Figure 29: Intensive-Margin Any LARC Event Study: 150–399 Mile Increase

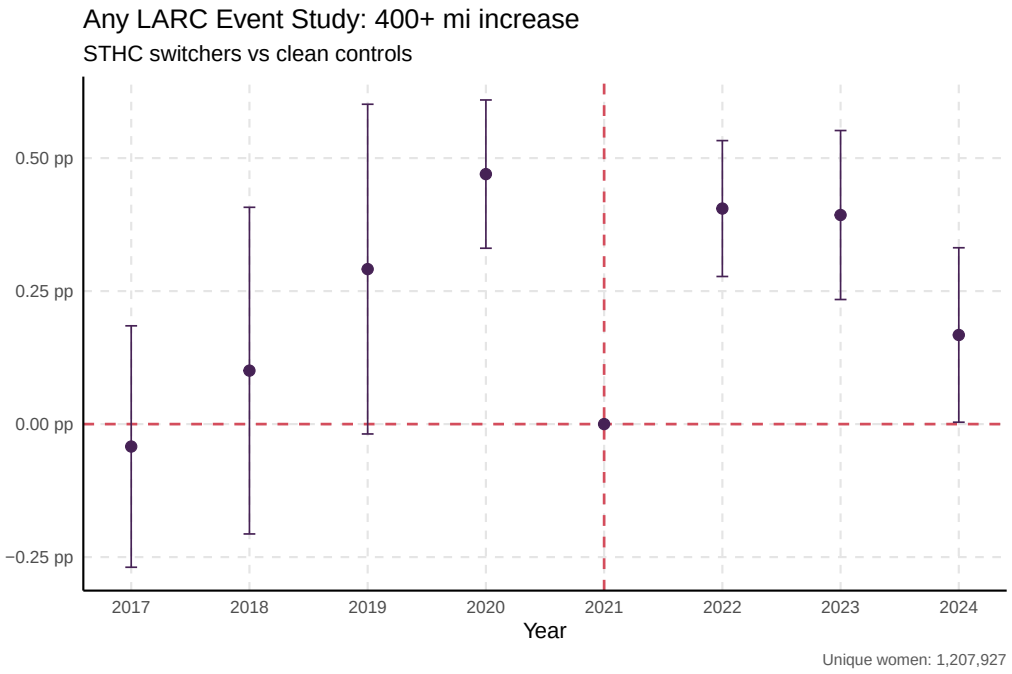


Figure 30: Intensive-Margin Any LARC Event Study: 400+ Mile Increase