Playing it safe?
The effect of abortion bans on sexual behavior

James Flynn* David Munro* Caitlin Myers*

June 5, 2026

Abstract

We estimate the effect of total abortion bans enacted after Dobbs v. Jackson Women's Health on sexual behavior. Using synthetic and standard difference-in-differences designs, we analyze gonorrhea rates and over-the-counter contraceptive purchases as indicators of sexual activity and contraceptive use. We find that total abortion bans reduced gonorrhea rates by 21% among the population aged 15–44 and increased condom purchases by 5.4%. County-level analyses suggest the response reflects awareness of state policy rather than changes in travel distance to providers. These findings are consistent with a framework in which abortion restrictions increase the perceived cost of unprotected sex, inducing more cautious behavior.

Keywords: Abortion, Dobbs, Risky Sexual Behavior
JEL Codes: I11, I12, I18, J13, K23.

*Flynn: Miami University and IZA. Munro: Middlebury College. Myers: Middlebury College, IZA, and NBER. We are grateful to the comments of seminar participants at the Ohio Policy Evaluation Network (OPEN), Middle Tennessee State University, Miami University, the Association of Public Policy Analysis and Management (APPAM) annual meeting, the Fresh Faces in Health Economics conference at Ball State University, and the Society of Labor Economists (SOLE) annual meeting. This research was supported by the Society of Family Planning grant SFP18-TTC8 and the Middlebury Initiative for Data and Digital Methods (midd.data). Researchers' own analyses calculated in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researchers and do not reflect the views of NielsenIQ. NielsenIQ is not responsible, had no role in, and was not involved in analyzing and preparing the results reported herein.

1. Introduction

Reproductive decisions unfold as a sequence: whether to have sex, whether and what form of contraception to use, and, conditional on pregnancy, whether to carry to delivery (Levine, 2004). A substantial literature has studied the last nodes in this decision tree, estimating the effects of abortion policy on abortion rates and births (see, e.g., Levine et al., 1999; Joyce et al., 2013; Quast et al., 2017; Lindo et al., 2020). While this work has established that abortion restrictions result in fewer abortions and more births, less attention has been paid to whether the same restrictions shape the choices that precede pregnancy. We examine these earlier nodes, estimating the effect of the total abortion bans enacted after the Supreme Court's 2022 decision in Dobbs v. Jackson Women's Health Organization on sexual activity and the use of short-acting contraception.

Whether bans shape behavior upstream of pregnancy depends on when individuals become aware of the relevant policy changes and on where in the decision sequence they respond. Decisions about sex and short-acting methods are made *ex ante*, before any pregnancy is realized, and hinge on the perceived probability that abortion would be available if needed—a perception that updates discretely with a salient policy shock but may be little moved by marginal changes in travel distance that are invisible to most individuals. Decisions about whether to carry a pregnancy to delivery, by contrast, are made *ex post*, once pregnancy has occurred, and respond to the realized cost of reaching an open provider. This distinction organizes the pre-Dobbs evidence. Studies of parental involvement laws, Texas House Bill 2, and other low-salience, access-reducing policies find little if any response on *ex ante* margins (Levine, 2004; Klick and Stratmann, 2008; Sabia and

Anderson, 2016; Fischer et al., 2018); where effects have been identified, they have taken the form of shifts toward permanent or long-acting methods (Crowe et al., 2025; Pennington and Venator, 2024). Surveys from the same period underscore the point, documenting that most women were unaware of the abortion laws in their own states (Swartz et al., 2020).

The Dobbs decision and the total abortion bans that ensued represent a qualitatively different shock. These bans shuttered every abortion facility across a wide swath of the South and Midwest, attracting sustained national attention that earlier, more modest restrictions did not—though surveys continue to document substantial uncertainty about the laws in effect in individuals' own states (Jozkowski et al., 2023; Gomez et al., 2024). Early research on post-Dobbs behavior documents increases in tubal sterilizations, vasectomies, and other permanent and long-acting methods (Strasser et al., 2025; Ellison et al., 2024; Bole et al., 2024; Liang et al., 2023; Mitchell et al., 2024; Gallen and Lu, 2025), alongside modest declines in oral contraceptive dispensing that may reflect reduced clinic capacity (Qato et al., 2024). Whether bans have also affected sexual activity and the use of short-acting contraception—margins the framework above predicts should respond only to a sufficiently salient shock—remains open.

We assemble three data sources to answer this question. Surveillance data on gonorrhea diagnoses from the NCHHSTP (2025) serve as a biological marker of unprotected sexual contact (Carpenter, 2005; Markowitz et al., 2005); retail scanner data from Nielsen (NielsenIQ, 2025) capture purchases of condoms; and the Youth Risk Behavior Survey (YRBS, 2025) provides self-reported sexual activity and contraceptive use among high school students. Because the three differ in sampling, outcomes, and modes of measurement, consistent patterns across them are less easily attributed to any single source of bias. We further exploit county-level variation in distance to the nearest provider to distinguish behavioral responses driven by awareness of state policy from those driven by changes in access—a distinction that speaks directly to the salience question raised above.

Our identification strategy combines difference-in-differences with synthetic difference-in-differences (SDID), comparing outcomes in states that enacted total abortion bans to outcomes in states that protected abortion access; most specifications exclude states with intermediate restrictions. Event-study estimates confirm parallel pre-trends through 2021 and reveal divergences beginning in 2022. Total bans reduced gonorrhea rates among the population aged 15–44 by 21% ($p = 0.02$) in our preferred SDID specification, with estimates negative across every alternative estimator and functional form we consider. A reduction of this magnitude is large but plausible: it tracks the behavioral shifts high school students report in the YRBS—more consistent condom use alongside substantial reductions in sexual activity—and matches the transmission dynamics of gonorrhea, which responds roughly proportionally to the rate of unprotected contact. Placebo tests on chlamydia and tuberculosis argue against a contraction in STI surveillance capacity as the driver.

Retail scanner data corroborate the gonorrhea result: condom purchases in ban states rose by 5.4% ($p < 0.01$), robust across specifications. YRBS data tell the same story—high school students in ban states report being 21% less likely to have ever had sex, having 32% fewer recent sexual partners, and being 28.8% less likely to have had unprotected sex. With only four ban states and seven protected states in the YRBS sample, we treat these estimates as corroboration of the gonorrhea and scanner results rather than stand-alone evidence. County-level analyses that separately identify ban enactment and distance to the nearest provider show the policy itself drives the response, not the accompanying increase in travel distance—consistent with a salience channel rather than an access channel. Section 5 combines these behavioral estimates with prior evidence on post-Dobbs fertility to bound the share of the resulting births that sexual and contraceptive change could have averted.

2. Data and descriptive evidence

We combine state-level classifications of post-Dobbs abortion policy with three sources of outcome data: administrative records on sexually transmitted infections, which serve as a biological marker of risky sexual behavior; retail scanner data on condom purchases, which provide a direct measure of protective behavior; and survey data on self-reported sexual behavior among high school students, available for a smaller set of states. This section describes each source and presents descriptive evidence motivating our empirical approach.

2.1. State abortion bans

The United States Supreme Court decision in Dobbs v. Jackson Women's Health, issued on June 24, 2022, overturned prior precedents set in Roe v. Wade (1973) and Planned Parenthood v. Casey (1992), ruling that the United States Constitution does not confer a right to abortion. In doing so, the Court returned the power to enact substantial restrictions on abortion access to individual states. Within 6 months of the ruling, a dozen states were enforcing near-total abortion bans prohibiting abortion in nearly all circumstances, with limited exceptions typically for serious risk to the pregnant person's life or physical health. As a result, abortion facilities in these states ceased providing services, causing 16 million women of reproductive age to experience increases in distance to the nearest facility, leaving the average affected woman more than 300 miles from the nearest facility (Dench et al., 2025).

Following Dench et al. (2025) and as documented in Appendix A and Table A.1, we categorize states as enforcing a total or near-total abortion ban, being hostile to abortion, or protecting abortion access, based on each state's policy environment from June 2022 through December 2024, the latest period observed for any of our outcome variables. As depicted in Figure 1, we classify 12 states as enforcing near-total bans.[1] Another 14 states enforced total bans for a portion of this period or implemented other highly restrictive measures such as gestational age limits. These states are classified as "hostile." The remaining 25 states, which neither enforced gestational limits nor were considered at risk of doing so, are classified as "protected."

2.2. The incidence of gonorrhea and other infections

We use data from the National Center for HIV, Viral Hepatitis, STD, and Tuberculosis Prevention (NCHHSTP, 2025) on state-level diagnoses of sexually transmit-

[1] Three states enforced total bans for only part of the post-Dobbs period: Indiana from August 2023 onward, North Dakota from April 2023 through September 2024, and again from November 2025 to present, and Wisconsin from June 2022 through September 2023. Because our primary SDID specification requires absorbing treatment, we classify these three states as "hostile" in the main analyses and exclude them from the ban-versus-protected comparison. Appendix B reports sensitivity analyses using two-way fixed effects DiD specifications that reclassify Indiana, North Dakota, and Wisconsin as ban states and allow treatment status to vary with actual enforcement in each state-year or state-quarter.

ted and other infections. The NCHHSTP data provide annual counts of reported diagnoses by state, separately for men and women and for five-year age groups from 15–19 through 40–44. Following CSTE guidelines, cases are attributed to the case-patient's state of usual residence rather than the state of diagnosis, so cross-state travel for care does not mechanically reassign cases across our treatment and control groups.[2,3] We combine these counts with population estimates from SEER (2025) to construct rates per 1,000 people in each age-sex cell. Throughout our analyses we aggregate to the population aged 15–44, the conventional reproductive-age window used in federal fertility and reproductive-health statistics and the age range over which gonorrhea incidence is heavily concentrated. This choice also aligns the denominator across our two primary outcomes—STI rates and per-capita condom purchases—with the population whose behavior is most plausibly responsive to abortion policy. Our analytical sample includes annual data from 2019 through 2023 for the District of Columbia and all 50 states except Maryland, which is missing data for 2021. The NCHHSTP also provides county-level counts, which we use to construct county-year panels for secondary analyses that exploit within-county variation in distance to the nearest abortion provider.

We focus on gonorrhea as a biological marker of risky sexual behavior, following precedent in the economics literature (e.g., Carpenter, 2005; Markowitz et al.,

[2] Centers for Disease Control and Prevention (2024b); Council of State and Territorial Epidemiologists (2005).

[3] The NCHHSTP suppresses county-level infection counts when cell sizes are small enough to pose confidentiality concerns. We take a conservative approach: if any age-sex cell within a county-year is suppressed, we treat the county's total infection count for that year as missing. This primarily affects small rural counties and results in 13% of county-year observations being excluded from county-level analyses of gonorrhea outcomes. State-level counts, which aggregate across counties, are subject to negligible suppression.

2005; Koppa, 2018; Corno and Paula, 2019). Gonorrhea is caused by the bacterium *Neisseria gonorrhoeae* and is spread almost exclusively through unprotected sexual contact, including vaginal, anal, and oral sex.[4] Several features make reported diagnoses a credible proxy for true incidence. Symptoms—which include painful urination, discharge, and genital inflammation—typically appear within two to five days of exposure, and the infection is quickly diagnosed via urine specimen. Men are especially likely to seek diagnosis, as 90–95% of male infections are symptomatic compared with 50–70% of female infections (Farley et al., 2003). Because gonorrhea rarely resolves without antibiotic treatment, symptomatic individuals have strong incentives to present for care. Using CDC modeling results, Pollock et al. (2023) estimate that in 2019, 44% of infections among men and 28% among women were diagnosed and reported through surveillance systems. Gonorrhea is also notable for the responsiveness of its aggregate incidence to changes in protective behavior. Transmission models describe gonorrhea as sustained near the threshold of endemic transmission, with population-level incidence roughly proportional to underlying rates of unprotected contact (Hethcote and Yorke, 1984; Chen et al., 2008). Even moderate shifts in contact rates or contraceptive use can therefore produce meaningful changes in aggregate incidence—a property that makes gonorrhea a sensitive marker of underlying behavior.

As a simple descriptive analysis, Figure 1 illustrates the change in gonorrhea rates across the three state policy environments between 2021 and 2023. We use 2021 as the baseline—the last full pre-Dobbs calendar year and the first year unaffected by pandemic-era distortions—and 2023 as the first full post-Dobbs year.

[4]Our review of the transmission, symptoms, and treatment of gonorrhea draws heavily from Jones and Lopez (2014), Workowski et al. (2021), and Bennett et al. (2014).

Rates fell in all three groups of states, but the decline was largest in states that enacted total abortion bans, where rates fell by 26.6% compared to 18.2% in hostile states and 13.5% in protected states. The difference between total-ban and protected states is statistically significant ($p = 0.01$).[5] Table 1 provides pre-treatment summary statistics for protected and ban states, illustrating that gonorrhea rates were also significantly higher in ban states than protected states at baseline: 5.4 versus 4.2 cases per 1,000 people aged 15–44 ($p < 0.01$). This difference in levels motivates our use of SDID, which re-weights control units to better match the pre-treatment trends of treated units.

In the analyses that follow, we supplement our gonorrhea analyses with results for chlamydia and primary and secondary syphilis, two additional sexually transmitted infections that we expect to respond less strongly to abortion policy than gonorrhea. While chlamydia is transmitted through the same contact patterns as gonorrhea, its biology makes it a less sensitive marker of short-run behavioral change: long asymptomatic windows and lower per-act transmission probability imply that equilibrium incidence adjusts slowly to shifts in unprotected contact. Because most chlamydia cases are diagnosed through routine screening rather than self-presentation for symptoms (Farley et al., 2003), reported diagnoses also reflect screening practices, which further weakens the mapping from current behavior to observed cases. Primary and secondary syphilis, in contrast, is dis-

[5] One might reasonably wonder whether abortion facility closures reduced STI testing capacity in ban states, artificially lowering observed STI rates. Using the Myers Abortion Facility Database, we find that of the 52 abortion facilities operating in ban states on January 1, 2022, more than half remain open and continue to offer non-abortion reproductive healthcare services, including STI testing. Of the 24 that closed entirely, only 7 had offered STI testing; the remaining 17 did not advertise STI testing services. The loss of 7 testing sites across 12 states is negligible relative to the broader STI surveillance infrastructure.

proportionately concentrated among men who have sex with men (Centers for Disease Control and Prevention, 2024a). Because pregnancy risk is not a factor for this population, syphilis rates should be less responsive to abortion policy. Finally, we examine tuberculosis, a non-sexually-transmitted infection, as a placebo outcome.

2.3. Purchases of condoms and other products

We use retail scanner data from the NielsenIQ Retail Measurement Services (RMS), which record weekly sales at the barcode level for a large panel of U.S. stores spanning grocery, drug, mass merchandise, and warehouse club channels (NielsenIQ, 2025). Each store is linked to a county identifier, allowing us to construct county-level measures of product sales over time, covering 2019 through 2024. Our primary outcome is condom purchases, identified through the Male Contraception product module. We construct two measures of condom purchases: total units sold, computed using package-size information in the UPC-level product attributes (e.g., a 10-pack counts as 10 units), and total revenue. In addition to the state-level aggregates used in our primary analyses, we construct county-quarter panels for secondary analyses of the relationship between distance to the nearest provider and product purchases. We exclude Alaska and Hawaii from our Nielsen-based analyses due to sparse retailer coverage in those states.

Our primary analyses use the full unbalanced sample of stores. Because stores enter and exit the Nielsen panel over time, we also construct a balanced subset of stores that report at least 26 weeks of data per year and appear in the panel continuously from 2019 through 2024 as a check on compositional changes.[6] Results

[6]If a store sells zero units of condoms in a given week, it is missing from the product-specific

are similar across the two samples. We aggregate weekly transaction data to the state-quarter level for analysis.

As Table 1 illustrates, baseline condom purchases were substantially lower in ban states than in protected states during the pre-treatment period: 85.4 versus 149.9 units per 1,000 people aged 15–44 per quarter ($p < 0.01$). As with the gonorrhea data, this difference in pre-treatment levels further motivates our use of SDID. Turning to the descriptive bar chart in Figure 1, condom sales rose by 2.9% in total-ban states between 2021 and 2023 while falling in protected states (−1.6%), a difference of 4.5 percentage points that is statistically significant ($p < 0.01$). This descriptive evidence is consistent with a shift toward protective behavior in states that enforced total bans.

While condom purchases provide the most direct measure of protective behavior, we also examine purchases of emergency contraception, pregnancy tests, and ovulation tests as supplementary outcomes. The expected direction of these effects is theoretically ambiguous. Emergency contraception purchases could rise if individuals seek additional insurance against the heightened cost of an unintended pregnancy, or fall if the shift toward pre-coital methods such as condoms reduces the need for post-coital methods as back-up. Ovulation test purchases may serve as a proxy for pregnancy intentions—a decline would suggest that fewer individuals are actively trying to conceive—but could also reflect increased use of fertility awareness as a contraceptive strategy. Pregnancy test purchases could rise with increased vigilance about pregnancy risk or fall if more consistent contraceptive use reduces the frequency of pregnancy scares. Finally, we treat purchases of ath-

transaction data in that week.

lete's foot and toothpaste products as placebo outcomes that should be unaffected by abortion policy and pregnancy intention.

2.4. Self-reported sexual behavior among high school students

We supplement our primary analyses with data from the Youth Risk Behavior Survey (YRBS), which captures self-reported sexual behavior among high school students. The YRBS is coordinated by the CDC and administered by state education and health agencies during the spring semester of odd-numbered years. The survey is designed to be representative of students in grades 9 through 12 in each state, is self-administered and anonymous to limit social desirability bias, and is weighted for nonresponse and probability of selection. Appendix Table B.1 provides pre-period summary statistics.

We restrict the sample to states that report data on these questions in every survey wave from 2017–2023, yielding 429,843 observations from seven states that protected abortion access (Hawaii, Maryland, Michigan, Montana, New Hampshire, New Mexico, and Nevada) and four states that enacted total bans (Arkansas, Kentucky, Oklahoma, and Texas). We examine whether the respondent has ever had sexual intercourse, how many sexual partners the respondent has had in the past three months, and whether the respondent or their partner used a condom the last time they had sex. We also construct an indicator for unprotected sex, coded as one for respondents who report not using a condom at last intercourse and zero for anyone who either used a condom or has never had sex, capturing both the extensive margin of sexual activity and the intensive margin of contraceptive use.[7]

[7]Because the condom question is conditional on having had sex, respondents who report never having had intercourse are coded as missing for the condom outcome but as zero for the unprotected sex indicator.

Because states' decisions to field the YRBS sexual-behavior items are not random, the resulting sample of eleven states is not nationally representative, and we treat the YRBS results as suggestive evidence that complements our primary state-level analyses.

2.5. Economic, demographic, and policy controls

Our county-level specifications and several robustness checks draw on standard external data sources. Driving distance to the nearest abortion-providing facility comes from the Myers Abortion Facility Database (Myers, 2021; Dench et al., 2025), which tracks the operating status and location of every publicly identifiable abortion provider in the United States and provides county-month measures of driving distance from the population centroid of each county to the coordinates of the nearest open abortion facility. We obtain county-year and state-year unemployment rates from the Bureau of Labor Statistics Local Area Unemployment Statistics (U.S. Bureau of Labor Statistics, 2025), and poverty rates from the Census Bureau's Small Area Income and Poverty Estimates (U.S. Census Bureau, 2025).[8] County population counts and age-by-sex-by-race shares come from SEER (2025), which we use both as regression denominators for our primary outcomes and as the source of demographic controls (race-by-age shares interacted with time fixed effects). Finally, we use state-month counts of Medicaid and CHIP enrollment from the Centers for Medicare and Medicaid Services Performance Indicator reports (Centers for Medicare and Medicaid Services, 2025), combined with SEER denominators, to construct the share of the adult population (ages 19–64) enrolled

[8]Connecticut's planning regions replaced counties in the SAIPE series beginning in 2022, leaving three historical CT counties without 2022–2024 values. We impute those years by applying the state-wide percent change from 2021 to each missing county.

in Medicaid in each state-year.

3. Empirical Strategy

Our identification strategy compares changes in proxies for sexual behavior in states that enacted total abortion bans to changes in states that protected abortion access, before and after the Dobbs decision. We implement this comparison using three approaches tailored to the structure of each data source: SDID for state-level STI and consumer purchase outcomes, two-way fixed effects Poisson models for county-level analyses that incorporate distance to the nearest provider, and two-way fixed effects event studies for individual-level survey data.

3.1. SDID

Our primary analyses use the SDID estimator of Arkhangelsky et al. (2021), which combines features of the synthetic control method (Abadie et al., 2010) with traditional difference-in-differences. SDID selects unit weights to minimize differences in pre-treatment *trends* between treated and control units, rather than minimizing differences in levels as in the standard synthetic control approach, addressing concerns about bias when the pre-treatment fit in levels is imperfect (Ferman and Pinto, 2021). SDID also selects time weights so that the pre-treatment periods used to construct the counterfactual reflect time-varying shocks similar to those in the post-treatment period. These features are well-suited to our setting, where Table 1 documents substantial differences in pre-treatment levels of both gonorrhea rates and condom purchases between ban and protected states.

We estimate SDID event studies following Clarke et al. (2024), which compares treated units to their synthetic control at each event time and calculates confidence intervals using a placebo bootstrap procedure with 500 replications. For STI outcomes, the unit of observation is the state-year and the sample spans 2019–2023, with treatment defined as enforcement of a total ban beginning in 2022. For consumer purchase outcomes, the unit of observation is the state-quarter and the sample spans 2020–2024, with treatment beginning in the third quarter of 2022; quarter dummies are included as projected covariates to account for seasonality.[9] In both cases, the estimation sample includes only total-ban and protected states, excluding hostile states, consistent with the binary absorbing treatment required by the SDID framework. Outcomes are measured in logs, and we report the average treatment effect on the treated (ATT) for each post-treatment period. Appendix B presents robustness checks that replicate these analyses using OLS, population-weighted OLS, and Poisson estimators, and presents results using level rates as well as logs.

3.2. County-level two-way fixed effects models

While the SDID estimates above capture the combined effect of bans and the distance increases that accompanied them, they cannot separate behavioral responses driven by the *ex ante* salience of legal prohibition from those driven by the *ex post* cost of reaching a provider—a distinction we return to in Section 4.4. Following the approach in Myers (2024) and Dench et al. (2025), we estimate Poisson regression models at the county level that include both a binary indicator for total ban enforcement and a quadratic in distance to the nearest provider (measured in hundreds of miles):

[9]We drop 2019 from the Nielsen sample in both the SDID and the county-level Poisson specifications: event studies on the 2019–2024 sample display unstable pre-trends that stabilize once 2019 is removed.

$$E[y_{ct}] = \exp\left(\beta_b B_{ct} + \beta_d D_{ct} + \beta_{dd} D_{ct}^2 + \beta_6 6\text{wk}_{ct} + X_{ct}'\gamma + \delta_c + \theta_t\right) \cdot \text{Pop}_{ct} \quad (1)$$

where y_{ct} is the count of gonorrhea cases or product units sold in county c at time t; B_{ct} is an indicator for a total abortion ban; D_{ct} is distance to the nearest provider in hundreds of miles; 6wk_{ct} is an indicator for a six-week gestational ban; X_{ct} includes poverty rates, unemployment rates, the share of adults (ages 19–64) enrolled in Medicaid, and detailed age-by-ethnicity population shares, each interacted with time fixed effects; δ_c and θ_t are county and time fixed effects; and Pop_{ct} is the population aged 15–44, which enters as the exposure variable. Standard errors are clustered at the state level. Gonorrhea models are estimated at the county-year level over 2019–2023; consumer purchase models are estimated at the county-quarter level over 2020–2024. Appendix Table B.2 reports pre-period summary statistics for the county sample.

Unlike the SDID analyses, these models include all states—total ban, hostile, and protected—allowing us to estimate the marginal effect of the average ban, evaluated at the average post-ban increase in driving distance from approximately 50 to 300 miles. The marginal effect is calculated as $100 \times (\exp(\hat{\beta}_b + \hat{\beta}_d(3 - 0.5) + \hat{\beta}_{dd}(3^2 - 0.5^2)) - 1)$, representing the combined percentage effect of a ban and its associated distance increase. Appendix B presents robustness checks that replicate these models using weighted OLS on the log rate and with and without the covariates in X_{ct} as well as models that use only the set of ban and protected states analyzed in the SDID specifications.

3.3. YRBS event studies

The YRBS sample includes only four total-ban states and seven protected states, too few control units to construct reliable synthetic weights and conduct placebo inference. We therefore estimate standard two-way fixed effects event studies:

$$y_{ist} = \sum_{\tau=-n}^{m} \alpha^{\tau} \Big(\mathbf{1}[t - 2021 = \tau] \times BanState_s \Big) + X'_{ist}\beta + \delta_s + \gamma_t + \varepsilon_{ist} \tag{2}$$

where y_{ist} is an outcome for individual i in state s in survey year t. The reference year is 2021, the last pre-Dobbs survey wave. Pre-treatment leads allow us to assess the plausibility of the parallel trends assumption, and post-treatment lags trace the evolution of effects after Dobbs. We include controls for the respondent's age, race, and sex, as well as state and year fixed effects. Because the eleven-state sample provides too few clusters for conventional cluster-robust inference to be reliable (Cameron et al., 2008; MacKinnon and Webb, 2018), we report wild cluster bootstrap p-values with Webb weights and 9,999 replications (Roodman et al., 2019), clustered at the state level.

4. Results

4.1. Gonorrhea

Figure 2 presents SDID event-study estimates of the effect of total abortion bans on gonorrhea rates among the population aged 15–44. The event-study coefficients show little evidence of differential trends between ban and protected states prior to Dobbs: estimates for 2019 and 2020 are close to zero, consistent with the parallel trends assumption underlying our identification strategy. A small and statistically insignificant decline appears in 2022, the year the Dobbs decision was issued, followed by a large and statistically significant reduction in 2023. The 2023 ATT represents a 0.233 log point reduction ($p = 0.02$), consistent with a 20.8% decline in gonorrhea rates in states that enacted total bans relative to states that protected access.[10]

Figure 3 disaggregates this result by sex and five-year age group, plotting the 2023 ATT from separate SDID event-study specifications for men and women aged 15−44 overall and for each five-year age cell from 15−19 through 40−44. For men aged 15−44, the estimated reduction in gonorrhea is 21.3% ($p < 0.01$), with estimates by age group for males ranging from 20.4% to 30.1%. The largest estimated effects are among men aged 15−19, for whom gonorrhea rates fell by 30.1% ($p = 0.02$)—a finding consistent with the expectation that younger individuals, who face higher costs of unintended pregnancy relative to their resources and who are less likely to use long-acting contraception, may be most responsive to changes in abortion access.

The reductions for women are slightly smaller in magnitude and less precisely estimated, a pattern that is expected given the epidemiology of gonorrhea: whereas 90−95% of male infections produce noticeable symptoms that prompt diagnosis, only 50−70% of female infections are symptomatic (Farley et al., 2003), making reported female gonorrhea rates a noisier proxy for changes in risky sexual behavior. The 2023 ATT for all women aged 15−44 is a 17.8% reduction ($p = 0.05$), and estimates for individual female age groups range from 13.8% to 25.3%. The full set of sex and age-specific event-study graphs underlying Figure 3, along with cor-

[10]Throughout the paper, we convert log point estimates to percent changes using the formula $\%\Delta = (e^{\beta} - 1) \times 100$.

responding estimates and coefficient plots using level rates rather than log rates, are presented in Appendix Figures B.1 through B.5; in each case, the pre-treatment coefficients are close to zero, with divergences emerging only after Dobbs.

The gonorrhea result is robust across a range of additional alternative specifications. Appendix Table B.3 reports estimates from seven specifications that vary the estimator (SDID, OLS, weighted OLS, Poisson) and the functional form (log rate and level rate). The gonorrhea coefficient is negative in all seven, and statistically significant in four. A leave-one-out analysis (Appendix Figure B.6) confirms that no single state drives the result: all 36 estimates remain negative and statistically significant at the 5% level. A randomization inference exercise (Appendix Figures B.7 through B.9) places our estimate in the extreme tail of the distribution of placebo effects, with the pattern holding across sex and age groups, and Appendix Figure B.10 shows that the decline in gonorrhea rates is concentrated in total-ban states, with smaller and statistically insignificant effects in hostile states—a pattern consistent with a dose-response relationship between the severity of abortion restrictions and behavioral change. Finally, Appendix Table B.5 addresses the concern that the post-Dobbs contraction in adult Medicaid enrollment, triggered by the end of COVID-19 continuous-coverage provisions in April 2023, could explain the gonorrhea result by reducing access to STI testing and diagnosis in ban states. Residualizing the outcome on state-year adult Medicaid share (following the projected-covariate method of Clarke et al. 2024) leaves the 2023 ATT essentially unchanged, and if anything slightly more negative (−0.251 versus −0.233 in the main SDID specification). Dropping the three ban states with the largest adult Medicaid declines between the 2022 peak and the 2023–2024 trough (Idaho, Oklahoma, and Texas) also leaves the estimate largely unchanged (−0.216). As a whole, these estimates indicate that total abortion bans were associated with substantial reductions in gonorrhea rates across all age groups and for both sexes.

Appendix Figures B.11 and B.12 present analogous estimates for chlamydia and primary and secondary syphilis, respectively. We find no evidence of an effect on either infection: point estimates are small, mixed in sign, and statistically insignificant across nearly all subgroups. The null result for chlamydia is consistent with the weaker signal-to-noise ratio for an infection that is far more likely to be asymptomatic and thus underdiagnosed. The null result for syphilis is expected given that transmission is disproportionately concentrated among men who have sex with men, for whom pregnancy risk is not a factor in sexual decision-making. Appendix Figure B.13 presents estimates for tuberculosis, a non-sexually-transmitted infection that serves as a placebo outcome; we find no effect, as expected.

4.2. Purchases of condoms and other products

Figure 4 presents SDID event-study estimates of the effect of total abortion bans on quarterly sales of condoms and emergency contraception, the two methods of contraception observable in the Nielsen data. Pre-treatment coefficients are imprecisely estimated during 2020—a year in which the COVID-19 pandemic disrupted retail purchasing patterns, store operations, and social behavior—but are generally small and statistically indistinguishable from zero, consistent with parallel trends. Post-Dobbs, condom sales rise by 5.4% ($p < 0.01$) in ban states relative to protected states, pooled across 2022Q3–2024Q4, while emergency contraception sales show no systematic change. This contrast is consistent with substitution from *ex post* to *ex ante* contraception: individuals facing a more costly alternative to abortion may shift toward more consistent condom use, reducing both the need for emergency contraception after unprotected intercourse and the frequency of such episodes.

Both findings—the increase in condom sales and the null result for emergency contraception—are robust across a wide range of specifications. Appendix Table B.6 and Table B.8 report ATTs across four estimators (SDID, OLS, population-weighted OLS, and Poisson), two store panels (balanced and unbalanced), and two outcome measures (unit sales and sales revenue); Appendix Figures B.14 through B.21 plot the corresponding event studies in both logs and levels. The condom coefficient is positive and statistically significant in nearly every combination. A leave-one-out analysis (Appendix Figure B.22) confirms that no single state drives the result: all 35 estimates remain positive and statistically significant at the 5% level. The emergency contraception coefficient is small and insignificant under SDID throughout. The condom result is also robust to accounting for post-Dobbs variation in adult Medicaid enrollment. Appendix Table B.10 shows that adding state-year adult Medicaid share as a time-varying covariate leaves the pooled ATT essentially unchanged (0.051 versus 0.052 in the main specification), and dropping the three ban states with the largest adult Medicaid declines (Idaho, Oklahoma, and Texas) yields a slightly larger estimate (0.057, $p < 0.01$).

Turning to the remaining products in the Nielsen data, pregnancy tests and ovulation tests generally show no significant SDID effects. The absence of a clear pattern is not unexpected: as discussed in Section 2, the direction of these effects is theoretically ambiguous. The fact that the point estimates are generally positive rather than negative does, however, cut against the alternative interpretation that the rise in condom sales reflects a broader decline in the desire to conceive. Neither of our two placebo products—athlete's foot medication and toothpaste—shows a systematic effect in the unbalanced SDID specifications, consistent with the assumption that abortion policy should not affect purchases of unrelated consumer goods.

4.3. Self-reported sexual behavior

Figure 5 displays two-way fixed effects event-study estimates of the effect of total abortion bans on self-reported sexual behavior among high school students, drawing on the Youth Risk Behavior Survey. Each specification includes controls for respondent age, race, and sex, as well as state and year fixed effects. Statistical inference is based on the wild cluster bootstrap with Webb weights and 9,999 replications, clustered at the state level. With only eleven state clusters, the bootstrap-based p-values are noticeably more conservative than those produced by conventional cluster-robust standard errors; we treat the YRBS results as suggestive rather than dispositive corroboration of the gonorrhea and scanner-data findings.

We begin with two measures of sexual activity. The top-left panel of Figure 5 shows estimates for whether the respondent has ever had sexual intercourse. Pre-treatment coefficients are close to zero from 2019 through 2021; a negative lead in 2017 is statistically significant under cluster-robust standard errors but not under wild cluster bootstrap inference, and the 2019 and 2021 coefficients—immediately preceding Dobbs—are close to zero in either case. The 2023 coefficient indicates a 7.0 percentage point reduction in the share of respondents reporting ever having had sex, which represents 21% of the pre-treatment base rate of 33.3% ($p = 0.16$ via wild cluster bootstrap). Appendix Figure B.23 shows that this point estimate is present for both sexes, with a somewhat larger estimate for young men (8.3 percentage points) than for young women (5.7 percentage points), though the difference is not statistically significant ($p = 0.65$). The top-right panel examines the intensive margin: the number of sexual partners in the past three months. The 2023 estimate is a reduction of 0.11 partners ($p = 0.13$), approximately 32% of the pooled pre-period base rate of 0.348. Point estimates are again larger for young men (0.15 partners, or 40% of the male base rate) than for young women (0.07 partners, or 23% of the female base rate), and a test of equal coefficients does not reject equality ($p = 0.37$).

Turning to contraceptive behavior, the bottom-left panel of Figure 5 presents estimates for condom use at last intercourse, conditional on having had sex. Pre-treatment trends are largely parallel, and the 2023 coefficient suggests a 4.0 percentage point increase in condom use, though the estimate is imprecise ($p = 0.37$). Appendix Figure B.24 reveals that this suggestive pattern is concentrated among young women, for whom the point estimate is a 10.1 percentage point increase ($p = 0.19$); the corresponding estimate for young men is close to zero ($p = 0.34$), and a test of equal coefficients returns $p = 0.09$. The bottom-right panel combines both margins by examining our composite measure of unprotected sex, coded as one for respondents who had sex without a condom and zero for those who either used a condom or never had sex. In 2023, respondents in ban states were 4.1 percentage points less likely to have had unprotected sex ($p = 0.20$), a reduction of 28.8% relative to the pooled pre-period base rate of 14.2%. The pattern is concentrated among young women, for whom unprotected sex fell by 5.9 percentage points ($p = 0.14$); the corresponding reduction for young men is 2.4 percentage points and not statistically distinguishable from zero ($p = 0.44$).

Appendix Figure B.25 presents a leave-one-out analysis in which each of the 11 states in the YRBS sample is iteratively dropped. The estimates depend on the inclusion of Texas: dropping Texas attenuates the reductions in ever having had sex (from 7.0 to less than 0.1 percentage points), recent sexual partners (from 0.11 to 0.03 partners), and unprotected sex (from 4.1 to 0.3 percentage points) to magnitudes indistinguishable from zero, while dropping any of the other ten states leaves the estimates largely unchanged. The small number of ban states in the YRBS sample—Arkansas, Kentucky, Oklahoma, and Texas—limits our ability to assess whether this pattern reflects features specific to Texas or a broader behavioral response that happens to be most pronounced there.

4.4. Is distance the mechanism?

Dench et al. (2025) find that the effect of abortion bans on births operates primarily through increases in driving distance rather than the legal prohibition itself: fertility rose most in counties where bans produced the largest distance increases, while counties already close to a remaining out-of-state provider saw less change. If behavioral responses are similarly access-driven, reductions in risky sexual behavior should also be larger in counties where distance increased the most. If instead total bans act *ex ante* through the salience of the policy rather than *ex post* through changes in access, the behavioral response should not depend on distance to the nearest provider. We test this prediction using the county-level Poisson specification described in Equation 1, which includes all states in the contiguous United States and exploits the continuous variation in distance that bans induced.

Table 2 presents these estimates. For each of our three outcomes—gonorrhea rates, condom sales, and emergency contraception sales—we report two specifications. The odd columns include only the binary indicator for total ban enforcement; the even columns add a quadratic in distance to the nearest abortion provider. Comparing the two allows us to assess how much of the estimated effect loads on the legal prohibition itself and how much on the induced changes in access. If distance is the operative mechanism, the ban coefficient should attenuate when distance is included, and the marginal effect evaluated at the average post-ban distance increase should capture most of the total response.

Table 2 presents the results. The behavioral response does not scale with distance. Across all three outcomes, the distance coefficients are small and not statistically significant at conventional levels, and the marginal effect of the average ban, evaluated at the observed post-Dobbs distance increase, differs from the ban-only estimate by less than half of a percentage point in every case. This suggests that distance is not the mechanism through which bans influence these sexual behaviors.

The county-level point estimates are broadly consistent with our state-level results for condom sales: a 2.8% to 2.9% increase that is statistically significant in both specifications of Table 2 and directionally aligned with the 5.4% SDID estimate. Results for the other two outcomes are more sensitive to specification. The 4% estimated reduction in gonorrhea rates is well below the 21% estimated 2023 ATT presented in Figure 2. The gap is driven by weighting. SDID's synthetic weights target pre-treatment trend similarity rather than population, whereas the Poisson models give larger-population states and counties more influence. A comparison of weighted and unweighted specifications in Table B.3 demonstrates the point:

unweighted OLS on pooled state-level data (Panel B) yields estimates similar to SDID's, while population-weighted variants (Panels C–D) reduce the magnitudes by more than half. We view the equal-state weighting as the more defensible target for the average state-level treatment effect, given that the policy operates at the state level and SDID's pre-trend correction is well-suited to a setting with twelve treated units. Emergency contraception, which is null in the state-level SDID, is negative and significant at the county level—a pattern that parallels the balanced-panel TWFE estimates discussed in Section 4.2. These patterns persist across five alternative specifications reported in Appendix Tables B.11 through B.15, which restrict the sample to ban and protected states to match the SDID, replace Poisson with weighted OLS on the log rate, or strip the covariate set to only the six-week ban indicator. The condom effect is positive in every specification (ranging from 2.8% to 6.8%), emergency contraception is negative in every specification (ranging from −3.1% to −4.8%), and the distance coefficients are generally small, with the most consistent pattern of statistical significance appearing on the gonorrhea coefficient in specifications that omit all controls. Taken together, the behavioral response to total bans appears to operate through awareness of the policy itself rather than through the access costs it imposes.

5. Assessing plausibility and reconciling with fertility effects

Dench et al. (2025) estimate that total abortion bans increased births in affected states by approximately 2.2%. One might reasonably wonder whether this figure is consistent with our finding that risky sexual behaviors declined in the same states over the same period. We argue that it is, and that together—subject to the caveats we detail below—the two findings are suggestive about the relative magnitudes of the direct and behavioral channels through which abortion bans affect fertility.

We first consider a back-of-the-envelope calculation estimating the change in pregnancies that might result from the estimated increase in condom use. As a starting point, we assume that every additional condom user substitutes from using no method of contraception to condom-protected sex. Let $\Delta P/P^0$ represent the causal effect of a total ban on the pregnancy rate, where P^0 denotes the counterfactual rate that would have prevailed absent the ban. Under the further simplifying assumption that this change is generated solely by the estimated 5.4% increase in condom sales (treated as a one-for-one proxy for use),

$$\frac{\Delta P}{P^0} = \frac{0.054\,\pi_c(f_u - f_c)}{P^0}, \tag{3}$$

where π_c is the share of women using male condoms as their primary contraceptive method, f_c is the failure rate of condoms, and f_u is the failure rate of using no method.

We proxy these parameters as follows. For π_c, we use 0.084 for women aged 15–49 from the 2017–2019 National Survey of Family Growth (Daniels and Abma, 2020). For the failure rates f_c and f_u, we use typical-use first-year pregnancy probabilities from Trussell (2011), setting $f_c = 0.18$ for condoms and $f_u = 0.85$ for no method. For the counterfactual pregnancy rate P^0, we use 82 per 1,000 women 15–44—the 2020 population-weighted average across the 12 total-ban states, computed from Kost et al. (2023). Plugging these values into the expression above yields an implied pregnancy reduction of approximately 3.7%.

The 3.7% figure is an upper bound, because substitution from no method yields by far the largest pregnancy reduction per switcher. Substitution from less-effective methods such as fertility awareness or withdrawal (typical-use first-year pregnancy rates of 24% and 22%, respectively) to condoms yields implied pregnancy reductions of only 0.2–0.3% of baseline pregnancies, an order of magnitude smaller than the 3.7% upper bound. Similarly, adding condoms as a second method would also yield much smaller declines in pregnancies than substituting condoms for unprotected sex.

Appendix C provides a simple accounting that reconciles our findings that pregnancies decline in response to total bans with the 2.2% increase in births estimated by Dench et al. (2025). For births to rise even as pregnancies fall, the share of pregnancies ending in abortion must fall by more—in other words, the access response (fewer abortions because women who seek them cannot obtain one) must outweigh the behavioral response (fewer pregnancies to begin with). Under reasonable assumptions about method substitution and about the share of behaviorally averted pregnancies that would have ended in abortion, our accounting exercise implies that roughly 16 to 20% of women who become pregnant and want an abortion in ban states are prevented from obtaining one. The behavioral response tempers the fertility consequences of the ban but does not come close to neutralizing them.

6. Conclusion

In defending abortion restrictions, some policymakers and litigants have argued that individuals can adjust their behavior to avoid unintended pregnancy (see, e.g., Lambert et al., 2023; Moon and Krems, 2025). As the Texas Right to Life amicus brief in *Dobbs* put it, “One can imagine a scenario in which a woman has chosen to engage in unprotected (or insufficiently protected) sexual intercourse on the assumption that an abortion will be available to her later. But when this Court announces the overruling of Roe, that individual can simply change her behavior in response to the Court’s decision if she no longer wants to take the risk of an unwanted pregnancy” (Texas Right to Life, 2021). This reasoning implies a testable prediction: that abortion bans induce substitution toward safer or less frequent sex.

We test this prediction using three markers of risky sexual behavior and find evidence consistent with it. Using difference-in-differences methods that compare states enacting total bans to states preserving access, we estimate that bans reduced gonorrhea rates by 21% and increased condom purchases by 5%. Self-reported sexual behavior in the YRBS points the same direction—high-school students in ban states report 20–32% reductions in sexual activity, though imprecisely estimated given the eleven-state sample. Taken together, the evidence indicates that some individuals in ban states have indeed shifted toward the safer behaviors abortion opponents anticipated.

Yet the accounting exercise in Section 5 indicates that these behavioral channels are unlikely to substantially reduce unintended pregnancies and hence the demand for abortions. Coupled with estimates that abortion bans raise births by 2–3% (Dench et al., 2024; Dench et al., 2025), our results imply that the modest reduction in unintended pregnancies is outweighed by a rise in unwanted births among women who wish to obtain an abortion but cannot.

References

(NCHHSTP), National Center for HIV, Viral Hepatitis, STD, and Tuberculosis Prevention (2025). *AtlasPlus: STD Surveillance Data.*

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller (2010). “Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California’s Tobacco Control Program”. In: *Journal of the American Statistical Association* 105.490, pp. 493–505.

Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager (Dec. 2021). “Synthetic Difference-in-Differences”. In: *American Economic Review* 111.12, pp. 4088–4118. ISSN: 0002-8282.

Bennett, John E., Raphael Dolin, and Martin J. Blaser (Aug. 2014). *Mandell, Douglas, and Bennett’s Principles and Practice of Infectious Diseases.* Vol. 1-2. Elsevier Inc., pp. 1–3697. ISBN: 9996096742.

Bole, Raevti, Scott D. Lundy, Evonne Pei, Petar Bajic, Neel Parekh, and Sarah C. Vij (May 2024). “Rising vasectomy volume following reversal of federal protections for abortion rights in the United States”. en. In: *International Journal of Impotence Research* 36.3. Publisher: Nature Publishing Group, pp. 265–268. ISSN: 1476-5489.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller (2008). “Bootstrap-Based Improvements for Inference with Clustered Errors”. In: *Review of Economics and Statistics* 90.3, pp. 414–427.

Carpenter, Christopher (May 2005). “Youth alcohol use and risky sexual behavior: evidence from underage drunk driving laws”. In: *Journal of Health Economics* 24.3, pp. 613–628. ISSN: 0167-6296.

Centers for Disease Control and Prevention (2024a). *Sexually Transmitted Infections Surveillance, 2023.* Accessed: 2026-03-05. Atlanta, GA: U.S. Department of Health and Human Services.

— (2024b). *STD Surveillance Tips.*

Centers for Disease Control and Prevention (CDC) (2025). *Youth Risk Behavior Survey (YRBS) Data, 2015–2023.*

Centers for Medicare and Medicaid Services (2025). *Medicaid and CHIP Performance Indicator Data.* [dataset], September 2025 release.

Chen, Mark I., Azra C. Ghani, and W. John Edmunds (2008). “A metapopulation modelling framework for gonorrhoea and other sexually transmitted infections in heterosexual populations”. In: *Journal of the Royal Society Interface* 6.38, pp. 775–791.

Clarke, Damian, Daniel Pailañir, Susan Athey, and Guido W. Imbens (2024). “Synthetic Difference-in-Differences Estimation”. In: *The Stata Journal* 24.4, pp. 557–598.

Corno, Lucia and Áureo de Paula (2019). “Risky Sexual Behaviours: Biological Markers and Self-reported Data”. en. In: *Economica* 86.342, pp. 229–261. ISSN: 1468-0335.

Council of State and Territorial Epidemiologists (2005). *CSTE Guidelines for Determining Residency for Disease Reporting Purposes.*

Crowe, Bradley, Graham Gardner, and Cara Haughey (Sept. 2025). “The effects of restricted abortion access on IUDs and vasectomies: Evidence from Texas”. In: *Journal of Health Economics* 103, p. 103041. ISSN: 0167-6296.

Daniels, Kimberly and Joyce C. Abma (2020). *Current Contraceptive Status Among Women Aged 15–49: United States, 2017–2019.* NCHS Data Brief 388. Hyattsville, MD: National Center for Health Statistics.

Dench, Daniel, Mayra Pineda-Torres, and Caitlin Myers (June 2024). "The effects of post-Dobbs abortion bans on fertility". In: *Journal of Public Economics* 234, pp. 105–124. ISSN: 0047-2727.

Dench, Daniel L., Caitlin K. Myers, and Mayra Pineda-Torres (Mar. 2025). *The Road Not Taken: How Driving Distance and Appointment Availability Shape the Effects of Abortion Bans*. en. Tech. rep. w33548. Cambridge, MA: National Bureau of Economic Research.

Dobbs v. Jackson Women's Health (2022). *No. 19-1392, 597 U.S. 215.*

Ellison, Jacqueline E., Brittany L. Brown-Podgorski, and Jake R. Morgan (Apr. 2024). "Changes in Permanent Contraception Procedures Among Young Adults Following the Dobbs Decision". In: *JAMA Health Forum* 5.4, e240424. ISSN: 2689-0186.

Farley, Thomas A., Deborah A. Cohen, and Whitney Elkins (Apr. 2003). "Asymptomatic sexually transmitted diseases: The case for screening". In: *Preventive Medicine* 36.4, pp. 502–509. ISSN: 00917435.

Ferman, Bruno and Cristine Pinto (2021). "Synthetic Controls with Imperfect Pretreatment Fit". In: *Quantitative Economics* 12.4, pp. 1197–1221.

Fischer, Stefanie, Heather Royer, and Corey White (Nov. 2018). "The impacts of reduced access to abortion and family planning services on abortions, births, and contraceptive purchases". In: *Journal of Public Economics* 167, pp. 43–68. ISSN: 0047-2727.

Gallen, Yana and Daisy Lu (2025). *Post-Roe Planning: The Effect of Dobbs v. Jackson on Contraceptive and Sterilization Choices*. SSRN Working Paper 5389330. SSRN.

Gomez, Ivette, Karen Diep, Brittni Frederiksen, Usha Ranji, and Alina Salganicoff (2024). *Abortion Experiences, Knowledge, and Attitudes Among Women in the U.S.: Findings from the 2024 KFF Women's Health Survey*. Tech. rep. KFF (Kaiser Family Foundation).

Hethcote, Herbert W. and James A. Yorke (1984). *Gonorrhea Transmission Dynamics and Control*. Vol. 56. Lecture Notes in Biomathematics. Berlin, Heidelberg: Springer-Verlag.

Jones, Richard E. and Kristin H. Lopez (2014). "Sexually Transmitted Diseases". In: *Human Reproductive Biology*, pp. 323–347.

Joyce, Ted, Ruoding Tan, and Yuxiu Zhang (2013). "Abortion before & after Roe". In: *Journal of Health Economics* 32.5, pp. 804–815. ISSN: 0167-6296.

Jozkowski, Kristen N., Xiana Bueno, Ronna C. Turner, Brandon L. Crawford, and Wen-Juo Lo (2023). "People's knowledge of and attitudes toward abortion laws before and after the Dobbs v. Jackson decision". In: *Sexual and Reproductive Health Matters* 31, p. 2233794. ISSN: 2641-0397.

Klick, Jonathan and Thomas Stratmann (May 2008). "Abortion Access and Risky Sex Among Teens: Parental Involvement Laws and Sexually Transmitted Diseases". In: *The Journal of Law, Economics, and Organization* 24.1, pp. 2–21. ISSN: 8756-6222.

Koppa, Vijetha (May 2018). "The Effect of Alcohol Access on Sexually Transmitted Diseases: Evidence From the Minimum Legal Drinking Age". In: *American Journal of Health Economics* 4.2. Publisher: The University of Chicago Press, pp. 164–184. ISSN: 2332-3493.

Kost, Kathryn, Isaac Maddow-Zimet, and Alex Arpaia (2023). *Pregnancies, Births and Abortions in the United States, 1973–2020: National and State Trends by Age*. Tech. rep. New York: Guttmacher Institute.

Lambert, Victoria C., Emily E. Hackworth, and Deborah L. Billings (2023). "Qualitative analysis of anti-abortion discourse used in arguments for a 6-week abortion ban in South Carolina". In: *Frontiers in Global Women's Health* 4, p. 1124132. ISSN: 2673-5059.

Levine, Phillip (2004). *Sex and Consequences: Abortion, Public Policy, and the Economics of Fertility*. Princeton University Press. ISBN: 978-0-691-13045-3.

Levine, Phillip, Douglas Staiger, Thomas Kane, and David Zimmerman (1999). “Roe v Wade and American fertility.” In: *American Journal of Public Health* 89.2, pp. 199–203.

Liang, Angela C., Catherine Gliwa, Charisse M. Loder, Vanessa K. Dalton, and Roger D. Smith (July 2023). “Tubal Sterilization Requests at a Single Institution Following the Supreme Court Decision to Overturn the Constitutional Right to Abortion”. In: *JAMA* 330.4, pp. 374–375. ISSN: 0098-7484.

Lindo, Jason M., Caitlin Knowles Myers, Andrea Schlosser, and Scott Cunningham (Sept. 2020). “How Far Is Too Far? New Evidence on Abortion Clinic Closures, Access, and Abortions”. In: *Journal of Human Resources* 55.4, pp. 1137–1160. ISSN: 15488004.

MacKinnon, James G. and Matthew D. Webb (2018). “The Wild Bootstrap for Few (Treated) Clusters”. In: *Econometrics Journal* 21.2, pp. 114–135.

Markowitz, Sara, Robert Kaestner, and Michael Grossman (May 2005). “An Investigation of the Effects of Alcohol Consumption and Alcohol Policies on Youth Risky Sexual Behaviors”. en. In: *American Economic Review* 95.2, pp. 263–266. ISSN: 0002-8282.

Mitchell, Jameson A., Meng Yao, Rie Maeda, Justin R. Lappen, and Ashley R. Brant (Sept. 2024). “Permanent and long-acting reversible contraception volumes at a multihospital system in Ohio before and after *Dobbs*”. In: *Contraception* 137, p. 110471. ISSN: 0010-7824.

Moon, Jordan W. and Jaimie Arona Krems (2025). “Pro-Life Policy Preferences Partly Reflect Desires to Suppress Casual Sexual Behavior, Not Solely Sanctity of Life Concerns”. In: *Social Psychological and Personality Science*, p. 19485506251320681. ISSN: 1948-5506.

Myers, Caitlin (2021). *Myers Abortion Facility Database*.

— (Jan. 2024). “Forecasts for a post-Roe America: The effects of increased travel distance on abortions and births”. In: *Journal of Policy Analysis and Management* 43.1, pp. 39–62. ISSN: 1520-6688.

NielsenIQ (2025). *Retail Scanner Dataset*.

Pennington, Kate and Joanna Venator (2024). “Reproductive policy uncertainty and defensive investments in contraception”. Working paper.

Pollock, Emily D, Patrick A Clay, Kristen M Kreisel, and Ian H Spicknall (Apr. 2023). “Estimated Incidence and Prevalence of Gonorrhea in the United States, 2006–2019”. In: *Sexually transmitted diseases* 50.4, pp. 188–195. ISSN: 0148-5717.

Qato, Dima M., Rebecca Myerson, Andrew Shooshtari, Jenny S. Guadamuz, and G. Caleb Alexander (2024). “Use of Oral and Emergency Contraceptives After the US Supreme Court’s Dobbs Decision”. In: *JAMA Network Open* 7.6, e2418620.

Quast, Troy, Fidel Gonzalez, and Robert Ziemba (2017). “Abortion Facility Closings and Abortion Rates in Texas”. In: *Inquiry: A Journal of Medical Care Organization, Provision and Financing* 54, p. 46958017700944. ISSN: 1945-7243.

Roodman, David, James G. MacKinnon, Morten Ørregaard Nielsen, and Matthew D. Webb (2019). “Fast and Wild: Bootstrap Inference in Stata Using `boottest`”. In: *Stata Journal* 19.1, pp. 4–60.

Sabia, Joseph J. and D. Mark Anderson (Jan. 2016). “The effect of parental involvement laws on teen birth control use”. In: *Journal of Health Economics* 45, pp. 55–62. ISSN: 0167-6296.

Strasser, Julia, Ellen Schenk, Sara Luckenbill, Danielle Tsevat, Lauryn King, Qian Luo, and Julie Maslowsky (Jan. 2025). "Tubal Sterilization And Vasectomy Increased Among US Young Adults After The Dobbs Supreme Court Decision In 2022". eng. In: *Health Affairs (Project Hope)* 44.1, pp. 99–107. ISSN: 2694-233X.

Surveillance, Epidemiology, and End Results Program (SEER) (2025). *SEER*Stat Database: Populations-State-level population files (1973-2023) Vintage 2023*. [dataset].

Swartz, Jonas J., Carly Rowe, Jessica E. Morse, Amy G. Bryant, and Gretchen S. Stuart (Nov. 2020). "Women's knowledge of their state's abortion regulations. A national survey". eng. In: *Contraception* 102.5, pp. 318–326. ISSN: 1879-0518.

Texas Right to Life (2021). *Brief of Texas Right to Life as Amicus Curiae in Support of the Petitioners, Dobbs v. Jackson Women's Health Organization, 597 U.S. ___*. Amicus Brief 19-1392. Supreme Court of the United States. Texas Right to Life.

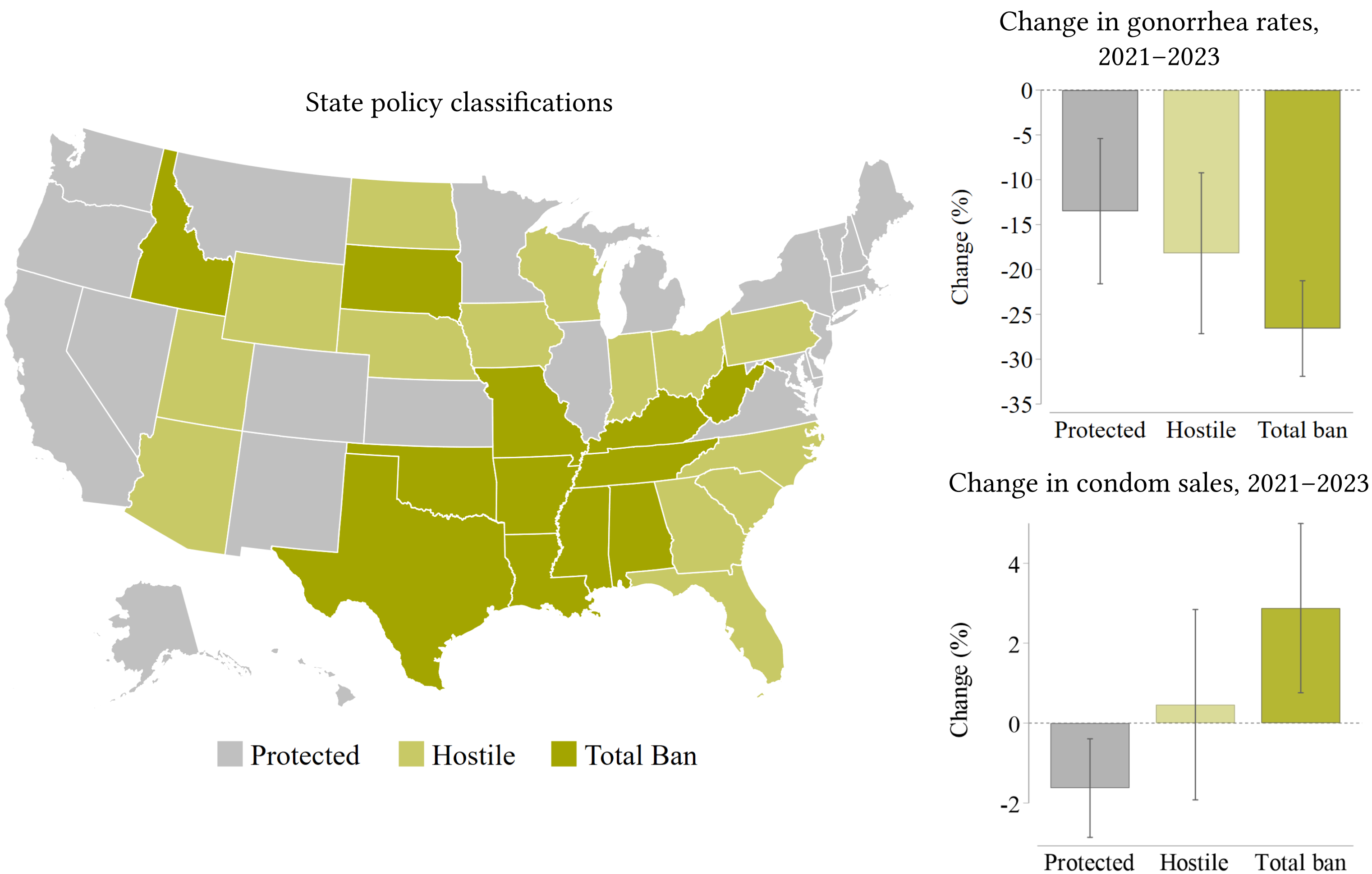
Trussell, James (2011). "Contraceptive Failure in the United States". In: *Contraception* 83.5, pp. 397–404.

U.S. Bureau of Labor Statistics (2025). *Local Area Unemployment Statistics: County Data*. [dataset].

U.S. Census Bureau (2025). *Small Area Income and Poverty Estimates (SAIPE)*. [dataset].

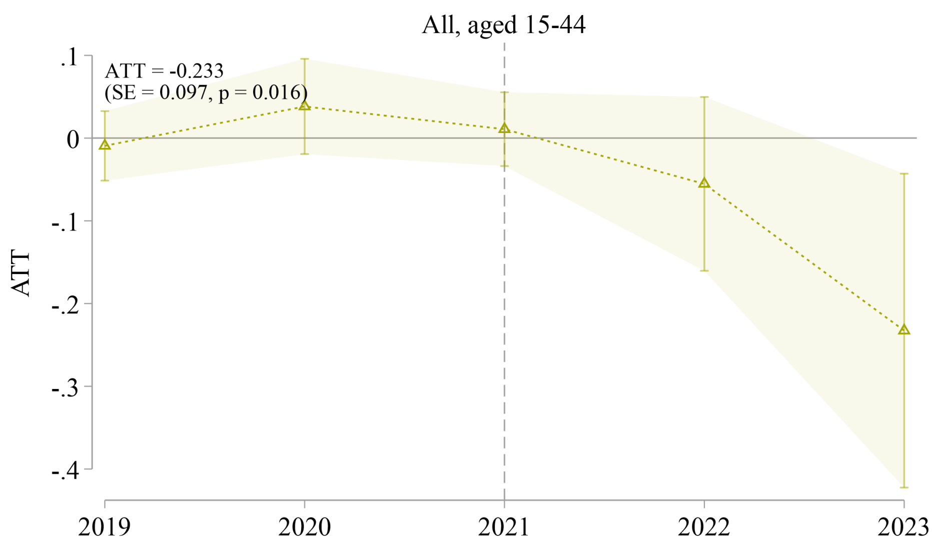
Workowski, Kimberly A., Laura H. Bachmann, Philip A. Chan, Christine M. Johnston, Christina A. Muzny, Ina Park, Hilary Reno, Jonathan M. Zenilman, and Gail A. Bolan (2021). "Sexually Transmitted Infections Treatment Guidelines, 2021". In: *MMWR Recommendations and Reports* 70.4, pp. 1–187. ISSN: 15458601.

Figure 1 — Abortion bans are associated with relative declines in gonorrhea and increases in condom sales



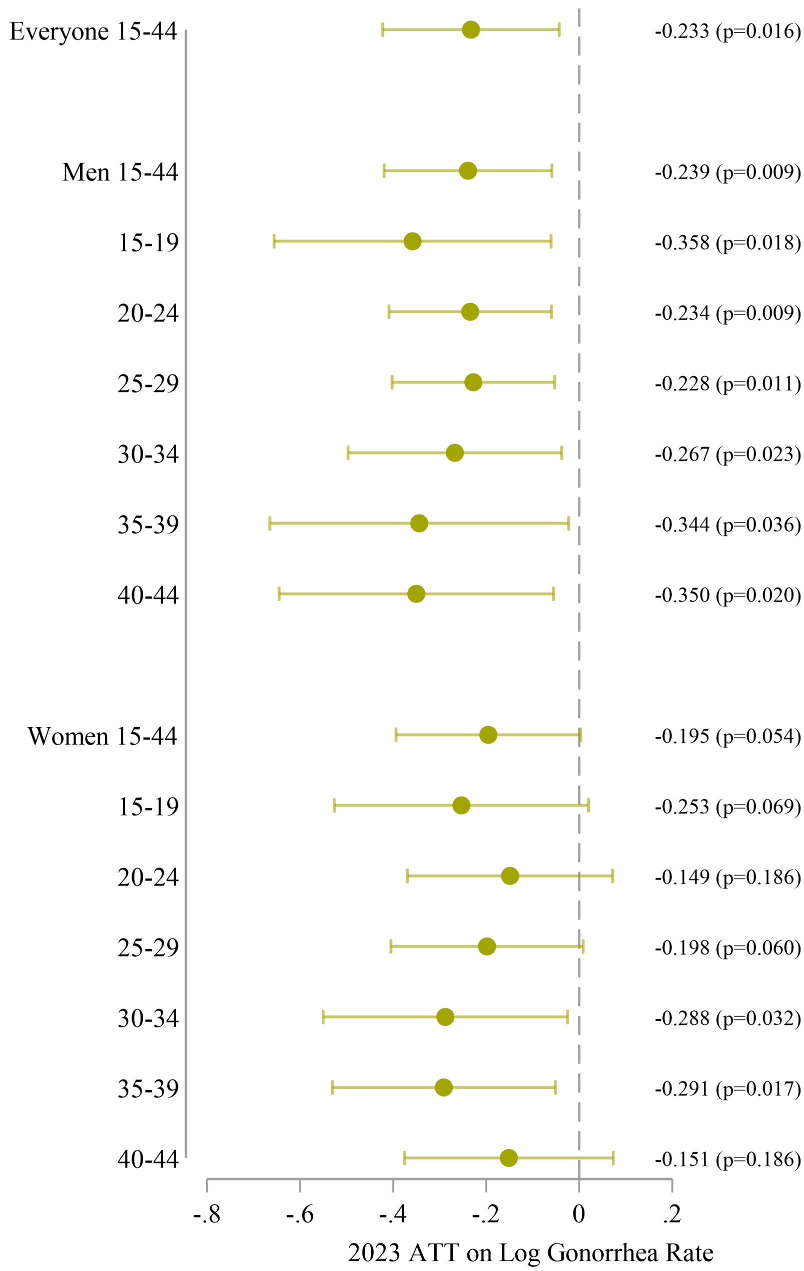
Notes: State policy classifications are described in Appendix A. Gonorrhea rates are cases per 1,000 people aged 15–44. Condom sales are units sold per 1,000 people aged 15–44. Changes are 2021–2023 percentage changes, population-weighted across states within each policy group. Bars represent 95% confidence intervals. Data sources: NielsenIQ (2025), SEER (2025), NCHHSTP (2025).

Figure 2 — SDID event study estimates of the effect of a total abortion ban on gonorrhea rates



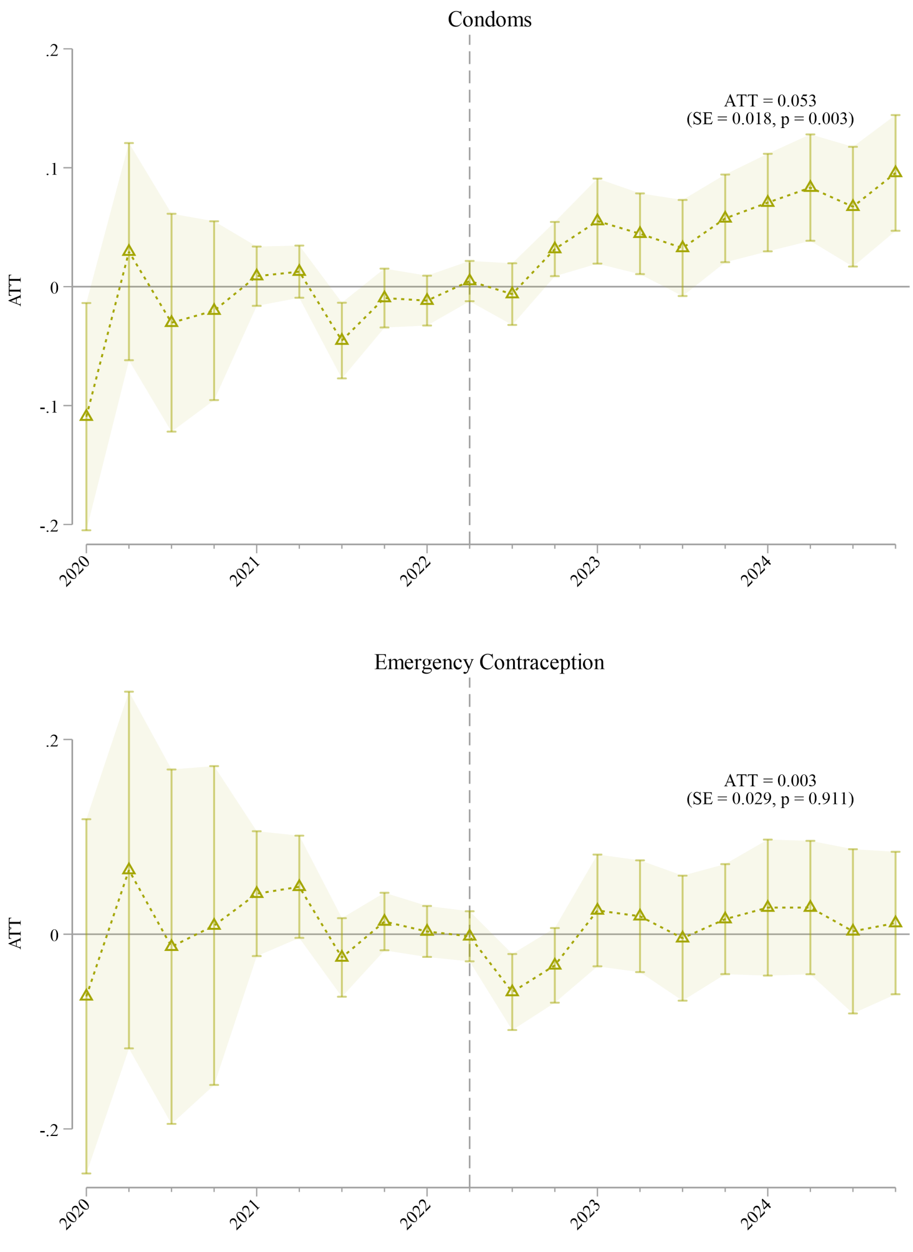
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcome is the log rate of gonorrhea cases per 1,000 people aged 15–44. The sample includes 12 total-ban states and 24 protected states, excluding hostile states. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks 2022, the year of Dobbs v. Jackson Women's Health (June 2022). Data: NCHHSTP (2025), SEER (2025).

Figure 3 — SDID event study estimates of the effect of a total abortion ban on gonorrhea rates



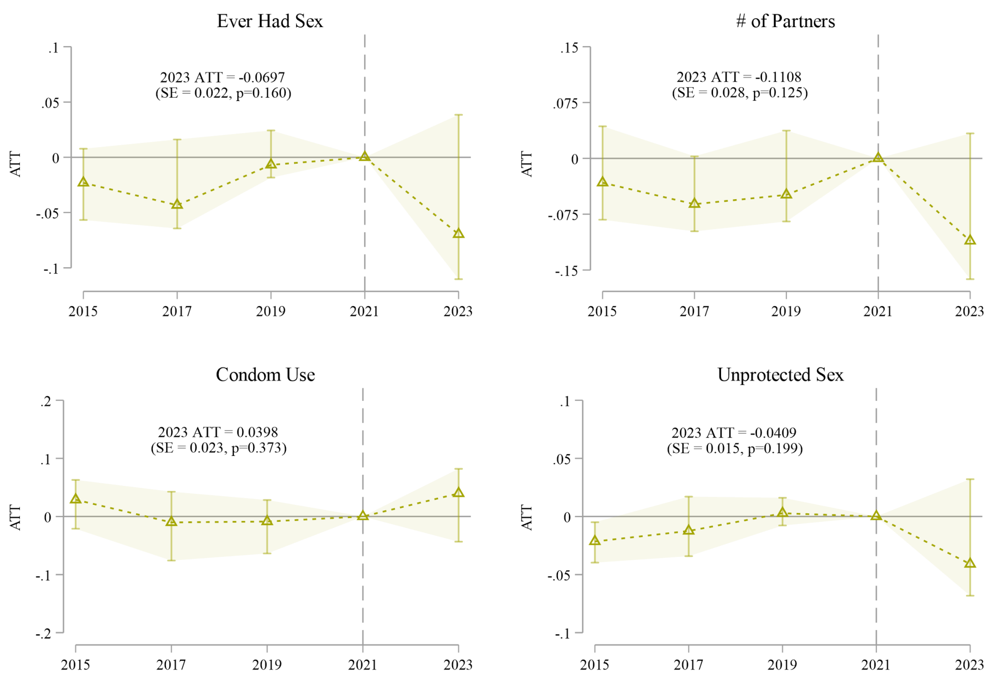
Notes: Each point shows the estimated 2023 ATT from a SDID event study of the log gonorrhea rate per 1,000 population in the indicated subgroup. Horizontal bars indicate 95% confidence intervals based on placebo inference with 500 bootstrap replications. Rates are computed using SEER population denominators. The sample includes 12 total-ban states and 24 protected states, excluding hostile states. States with incomplete panels for a given subgroup are excluded from that subgroup's estimation. Data: NCHHSTP (2025), SEER (2025).

Figure 4 — SDID event study estimates of the effect of a total abortion ban on condom and EC sales



Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are log quarterly condom sales volume per capita (Panel A) and emergency contraception sales volume per capita (Panel B) observed at all stores and aggregated to the state level. The sample includes 12 total-ban states and 23 protected states, excluding hostile states. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure 5 — TWFE event-study estimates of the effect of a total abortion ban on responses to questions about sexual behavior in YRBS



Notes: Difference-in-differences event study estimates comparing states with total abortion bans to protected states. The outcomes are individual responses of high school students to questions about sexual behavior and the models are linear probability models with age, sex, race, state, and year fixed effects. Shaded areas and vertical bars show 95% confidence intervals computed via the wild cluster bootstrap with Webb weights and 9,999 replications, clustered at the state level. The dashed line marks 2022, the year of Dobbs v. Jackson Women's Health (June 2022). Data: CDC (2025).

Table 1 — Pre-period summary statistics by state policy category

	Ban states		Protected states	
	Mean	(SD)	Mean	(SD)
Panel A: STI outcomes (state × year)				
Gonorrhea rate, total	5.36	(1.86)	4.19	(1.16)
Gonorrhea rate, male	5.58	(1.77)	4.92	(1.57)
Gonorrhea rate, female	5.14	(2.01)	3.44	(1.04)
State × year observations	36		74	
States	12		25	
Panel B: Purchase outcomes (state × quarter)				
Condom units	85.44	(18.82)	149.91	(25.08)
Emergency contraception units	4.92	(2.04)	4.79	(2.05)
Pregnancy test units	19.08	(5.50)	22.86	(5.91)
Ovulation test units	2.83	(0.95)	4.43	(1.38)
Athlete's foot units (placebo)	15.32	(5.19)	24.58	(10.14)
Toothpaste units (placebo)	676.83	(160.59)	1182.16	(220.91)
State × quarter observations	144		276	
States	12		23	

Notes: This table reports pre-period (2019–2021) means and standard deviations for states classified as ban states and protected states for the SDID estimation strategies employed in this paper. Hostile states are excluded, consistent with these specifications. Panel A reports gonorrhea rates per 1,000 population aged 15–44 at the state × year level. Panel B reports product units sold per 1,000 population aged 15–44 at the state × quarter level. Alaska and Hawaii are excluded from our Nielsen-based analyses due to sparse retailer coverage, resulting in 2 fewer protected states. All statistics are weighted by the state population aged 15–44. See Appendix A for state policy classifications. Data: NielsenIQ (2025), SEER (2025), NCHHSTP (2025).

Table 2 — Two-way fixed effects Poisson estimates of the effect of bans and driving distance on gonorrhea and sales of condoms and EC

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	-0.043	-0.084	0.028***	0.041***	-0.036***	-0.025*
	(0.040)	(0.060)	(0.011)	(0.016)	(0.012)	(0.014)
Distance		0.009		0.001		-0.004
		(0.026)		(0.009)		(0.010)
Distance^2		0.002		-0.002*		-0.000
		(0.004)		(0.001)		(0.002)
Marginal effect	-4.2%	-4.3%	2.9%**	2.8%***	-3.5%***	-3.6%***
of average ban	(3.9)	(3.7)	(1.1)	(1.0)	(1.1)	(1.1)
Observations	13,581	13,581	47,020	47,020	32,480	32,480
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	yes	yes	yes	yes	yes	yes

Notes: This table presents coefficients from Poisson models of county-level gonorrhea rates (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6). The population aged 15–44 is the exposure variable. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. The sample covers counties in the contiguous United States. All models include county fixed effects, controls for unemployment rates, poverty rates, the share of adults (ages 19–64) enrolled in Medicaid, and detailed age-by-ethnicity population shares (each interacted with time fixed effects), and an indicator for 6-week gestational age bans. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100\times(\exp(\hat{\beta}_b)-1)$ in odd columns and $100\times(\exp(\hat{\beta}_b+\hat{\beta}_d(3-0.5)+\hat{\beta}_{dd}(3^2-0.5^2))-1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

A. State abortion bans

We rely on the review and classification of state abortion policies published by Dench et al., 2025. Table A.1 summarizes states classified as having enacted or enforced pre-viability abortion bans since Dobbs, along with the dates of enforcement.

Following Dench et al., 2025, we classify states into three groups. States that enforced a near total ban on abortion are classified as "total ban" states. States that enforced a pre-viability gestational age ban (at 6, 12, 15, or 18 weeks) as well as states with pending litigation or a political environment favoring future restrictions are classified as "hostile." All remaining states are classified as "protected." This classification is assigned based on total ban enforcement between June 2022 and December 2024. Three states that enforced total bans for a portion of this period—Indiana, North Dakota, and Wisconsin—are classified as "hostile" and excluded from the SDID analyses that require absorbing treatment. However, we note that several robustness checks as well as the county-level analyses relying on models with TWFE allow for non-absorbing treatment and include these states.

See Dench et al., 2025 for a detailed state-by-state review of policies and enforcement dates.

Table A.1 — Summary of pre-viability abortion bans

State	Classification	Enforcement Dates
Alabama	Total ban	6/24/2022–present
Arizona	Hostile	15-week ban 9/25/2022–12/5/2024. Facilities also closed 6/24/2022–7/11/2022 & 9/24/2022–10/7/2022 due to perceived enforcement risks.
Arkansas	Total ban	6/24/2022–present
Florida	Hostile	15-week ban 7/1/2022–4/30/2024; 6-week ban 5/1/2024–present
Georgia	Hostile	6-week ban 7/20/2022–11/15/2022; 11/21/2022–9/30/2024; 10/7/2024–present
Idaho	Total ban	6-week ban 8/19/2022–8/24/2022; Total ban 8/25/2022–present
Indiana	Hostile	Total ban 8/21/2023–present
Iowa	Hostile	6-week ban 7/29/2024–present
Kentucky	Total ban	6/24/2022–present
Louisiana	Total ban	6/24/2022–present
Mississippi	Total ban	7/7/2022–present
Missouri	Total ban	Total ban 6/24/2022–12/23/2024; 5/27/2025–7/3/2025
Nebraska	Hostile	12-week ban 5/22/2023–present
North Carolina	Hostile	12-week ban 7/1/2023–present
North Dakota	Hostile	Total ban 4/23/2023–9/12/2024; 11/21/2025–present
Ohio	Hostile	6-week ban enforced 6/24/2022–9/14/2022. Voters approved a constitutional amendment to protect abortion rights on 11/7/2023.
Oklahoma	Total ban	6-week ban 5/3/2022–5/25/2022; Total ban 5/26/2022–present.
Pennsylvania	Hostile	No pre-viability ban enforced; classified as hostile based on political environment.
South Carolina	Hostile	6-week ban 6/24/2022–8/17/2022; 8/23/2023–present
South Dakota	Total ban	6/24/2022–present
Tennessee	Total ban	6-week ban 6/28/2022–8/24/2022; total ban 8/25/2022–present
Texas	Total ban	6-week ban 9/1/2021–6/23/2022; total ban 6/24/2022–present
Utah	Hostile	Total ban 6/24/2022–6/27/2022; 18-week ban 6/26/2022–present
West Virginia	Total ban	6/24/2022–7/20/2022; 9/16/2022–present
Wisconsin	Hostile	Total ban 6/24/2022–9/18/2023
Wyoming	Hostile	6-week ban 3/9/2026–present

Notes: Classifications reflect each state's policy environment from June 2022 through December 2024, the latest period observed for any outcome in the paper, corresponding to the policy map in Figure 1. The "Enforcement Dates" column reports the period(s) during which each restriction was in effect. Protected states (no pre-viability ban enforced) are omitted. Source: Dench et al., 2025.

Appendix B: Additional results

Throughout the paper, we allude to various additional results and alternative specifications of our primary models. These include the following.

B.1. Additional summary statistics

- Table B.1 presents summary statistics for the Youth Risk Behavior Survey (YRBS) data used to estimate results in Figure 5 and Appendix Figure B.23 and Figure B.24.

- Table B.2 presents summary statistics for the county-level data used to estimate results in Table 2.

B.2. Additional analyses of STI outcomes

- Figure B.1 presents SDID event study estimates of the effect of a total abortion ban on log male gonorrhea rates by five-year age group, disaggregating Figure 2 by subpopulation.

- Figure B.2 presents the corresponding estimates for log female gonorrhea rates by five-year age group.

- Figure B.3 and Figure B.4 present the corresponding estimates using level gonorrhea rates (rather than log rates) as the outcome, for men and women respectively.

- Figure B.5 plots the 2023 ATT from SDID by age group and sex using level rates as the outcome, rather than the log rates reported in Figure 3.

- Figure B.6 presents a 'leave-one-out' analysis, where we iteratively drop each of the 36 states from our main SDID specification and reestimate the model. This figure plots the 2023 ATTs from our SDID event-study when each state is dropped.

- Figure B.7 presents the distribution from a randomization inference where we compare our estimate of the effect of abortion restrictions on gonorrhea for the entire population to placebo estimates where we randomly select treated and control states and then run our main SDID specification.

- Figure B.8 presents distributions from a randomization inference where we compare our estimate of the effect of abortion restrictions on gonorrhea for men in each five-year age group to placebo estimates where we randomly select treated and control states and then run our main SDID specification.

- Figure B.9 presents distributions from a randomization inference where we compare our estimate of the effect of abortion restrictions on gonorrhea for women in each five-year age group to placebo estimates where we randomly select treated and control states and then run our main SDID specification.

- Figure B.10 presents a comparison of the 2023 ATTs from SDID event-study specifications comparing total-ban states to protected states, while also comparing hostile states to protected states.

- Figure B.11 plots the 2023 SDID ATT on log chlamydia rates by age group and sex, analogous to the gonorrhea estimates in Figure 3.

- Figure B.12 plots the 2023 SDID ATT on log syphilis rates (primary and secondary) by age group and sex, analogous to the gonorrhea estimates in Figure 3.

- Figure B.13 plots the 2023 SDID ATT on log tuberculosis rates by age group, analogous to the gonorrhea estimates in Figure 3. Note that tuberculosis rates are not reported by sex.

- Table B.3 reports ATT estimates for gonorrhea, chlamydia, syphilis, and tuberculosis rates across seven specifications. Panels A–C vary the estimator (SDID, OLS, weighted OLS) using the log rate as the outcome; Panel D estimates a Poisson model with raw case counts and population exposure; Panels E–G repeat the SDID, OLS, and weighted OLS specifications using the level rate.

- Table B.4 reports non-absorbing sensitivity estimates that reclassify Indiana, North Dakota, and Wisconsin as ban states and replace the binary ban indicator with E_{st}, the fraction of state-year st during which a total ban was in effect. The coefficient is interpretable as the effect of a full year under ban. Panels A–C report OLS, weighted OLS, and Poisson estimates of the log-rate and count specifications; Panels D–E repeat OLS and weighted OLS using the level rate. SDID panels are omitted because SDID requires absorbing treatment.

- Table B.5 reports the pooled SDID ATT on log gonorrhea rates under three specifications that address the concern that the post-Dobbs contraction in adult Medicaid enrollment could have reduced STI diagnosis in ban states. Column (1) reports the main specification without additional covariates. Column (2) adds state-year adult Medicaid share as a time-varying covariate. Column (3) drops the three ban states with the largest adult Medicaid declines between the 2022 peak and the 2023–2024 trough (Idaho, Oklahoma, and Texas). The main conclusion is unchanged across all three specifications.

B.3. Additional analyses of consumer purchase outcomes

- Figure B.14 presents SDID event study estimates of the effect of a total abortion ban on quarterly unit sales per capita of six consumer products: condoms, emergency contraception, pregnancy tests, ovulation tests, and two placebo products (antifungal medication and toothpaste). Unit sales are aggregated across all stores in the Nielsen panel at the state-quarter level. The condom and emergency contraception panels are identical to those in Figure 4.

- Figure B.15 present corresponding estimates to Figure B.14, aggregating sales only over stores that appear at the beginning and end of the sample period.

- Figure B.16 presents corresponding estimates using log sales revenue per capita rather than units sold as the outcome.

- Figure B.17 presents corresponding estimates using log sales revenue per capita as the outcome and aggregating sales only over stores that appear at the beginning and end of the sample period.

- Figure B.18 presents corresponding estimates to Figure B.14 using units sold per 1,000 population rather than the log rate as the outcome.

- Figure B.19 presents the balanced-panel analog of Figure B.18.

- Figure B.20 presents corresponding estimates using sales revenue per 1,000 population rather than the log rate as the outcome.

- Figure B.21 presents the balanced-panel analog of Figure B.20.

- Figure B.22 presents a 'leave-one-out' analysis, where we iteratively drop each of the 35 states from our main SDID specification for condom purchases and reestimate the pooled ATT. This figure plots the resulting estimates when each state is dropped.

- Table B.6 reports ATT estimates from eight specifications for each product, where sales volume is the outcome. Panels A–B use SDID; Panels C–D use unweighted OLS; Panels E–F use population-weighted OLS; and Panels G–H use Poisson with population exposure. Within each estimator, odd panels use the full unbalanced store panel and even panels use the balanced panel.

- Table B.8 reports corresponding estimates using sales revenue as the outcome.

- Table B.7 and Table B.9 report non-absorbing sensitivity estimates for units sold and sales revenue that reclassify Indiana, North Dakota, and Wisconsin as ban states and replace the binary ban indicator with E_{sq}, the fraction of state-quarter sq during which a total ban was in effect. The coefficient is interpretable as the effect of a full quarter under ban. SDID panels are omitted because SDID requires absorbing treatment.

- Table B.10 reports the pooled SDID ATT on log condom units under three specifications that address the concern that the post-Dobbs contraction in adult Medicaid enrollment could confound the condom result. Column (1) reports the main specification (quarter dummies as covariates). Column (2) adds state-year adult Medicaid share as a time-varying covariate. Column (3) drops the three ban states with the largest adult Medicaid declines between the 2022 peak and the 2023–2024 trough (Idaho, Oklahoma, and Texas). The main conclusion is unchanged across all three specifications.

B.4. Additional analyses of self-reported sexual behavior

- Figure B.23 reports TWFE event study estimates of two outcomes—ever had sex and number of partners—for all respondents (corresponding to Figure 5) and by respondent sex.

- Figure B.24 reports TWFE event study estimates of two outcomes—use of a condom at last sexual intercourse and no contraception use at last sexual intercourse—for all respondents (corresponding to Figure 5) and by respondent sex.

- Figure B.25 displays results of a leave-one-out analysis, where each state is iteratively dropped from our event-study specifications in order to see if any one state is driving our results.

B.5. Additional county-level analyses

- Table B.11 replicates the main Poisson specification from Table 2 but omits the covariates in X_{ct} (unemployment rates, poverty rates, the share of adults enrolled in Medicaid, and age-by-ethnicity population shares), testing sensitivity to conditioning on these time-varying county-level controls.

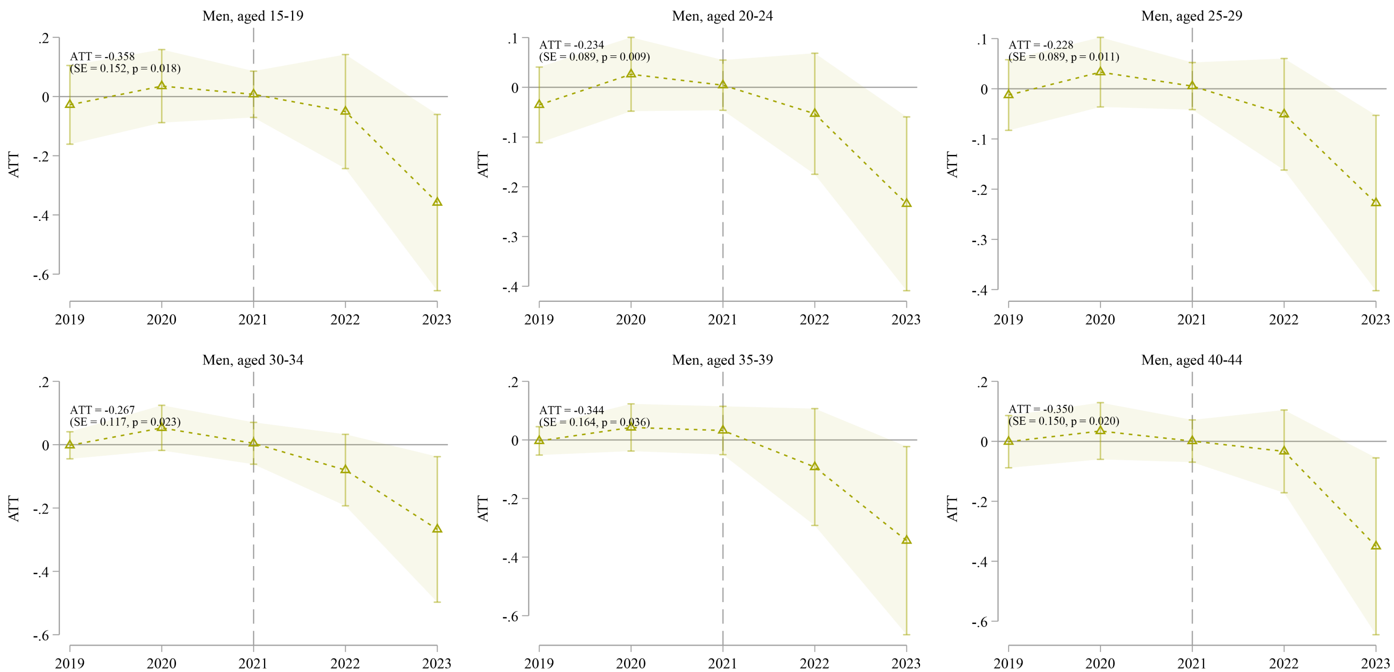
- Table B.12 replicates the main Poisson specification but restricts the sample to counties in ban and protected states, excluding the 14 hostile states to match the sample used in the state-level SDID analyses.

- Table B.13 replicates the main Poisson specification excluding hostile states and omitting the covariates in X_{ct}.

- Table B.14 estimates the model by weighted OLS on the log outcome rate, using population weights and dropping observations with zero counts where the log is undefined, with the full set of demographic controls retained. This tests sensitivity to the Poisson functional form assumption.

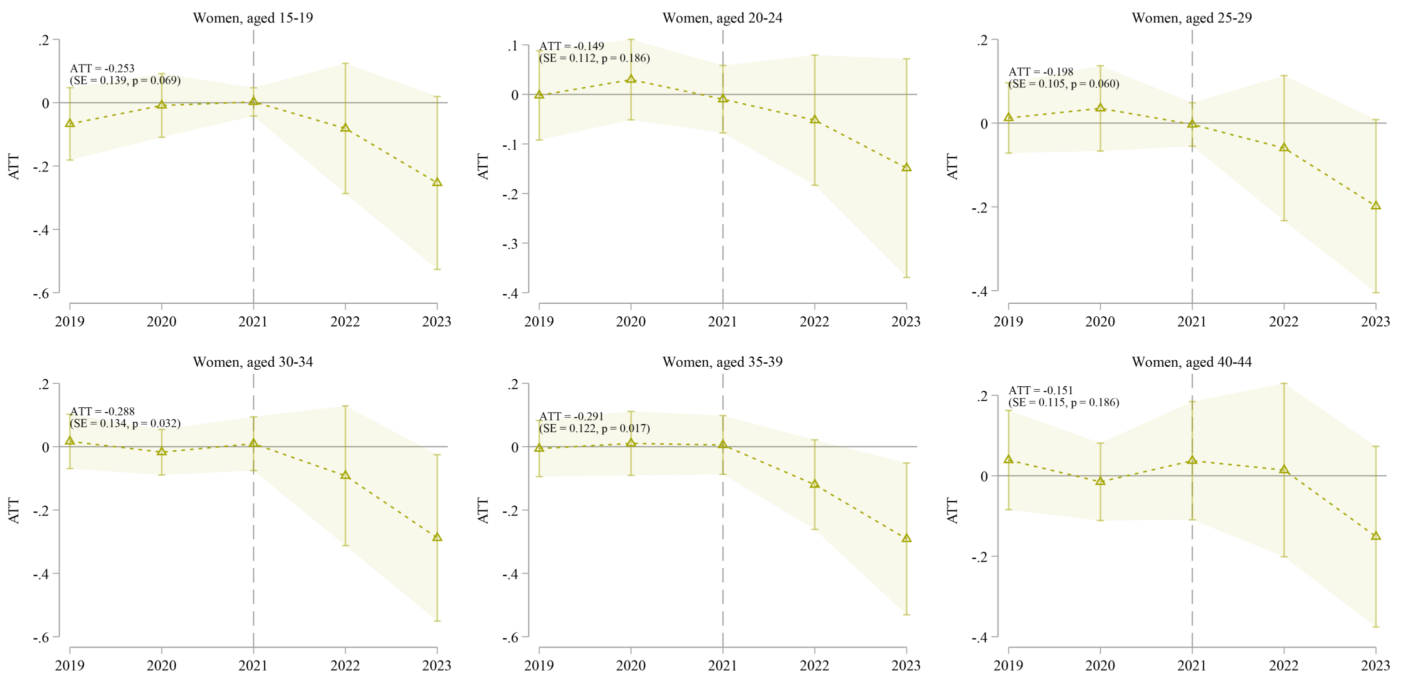
- Table B.15 estimates the weighted OLS log-rate model without the covariates in X_{ct}.
- Table B.16 replicates the weighted OLS specification but restricts the sample to counties in ban and protected states, excluding the 14 hostile states to match the sample used in the state-level SDID analyses.
- Table B.17 replicates the weighted OLS specification excluding hostile states and omitting the covariates in X_{ct}.

Figure B.1 — SDID event study estimates of the effect of a total abortion ban on male gonorrhea rates, by age group



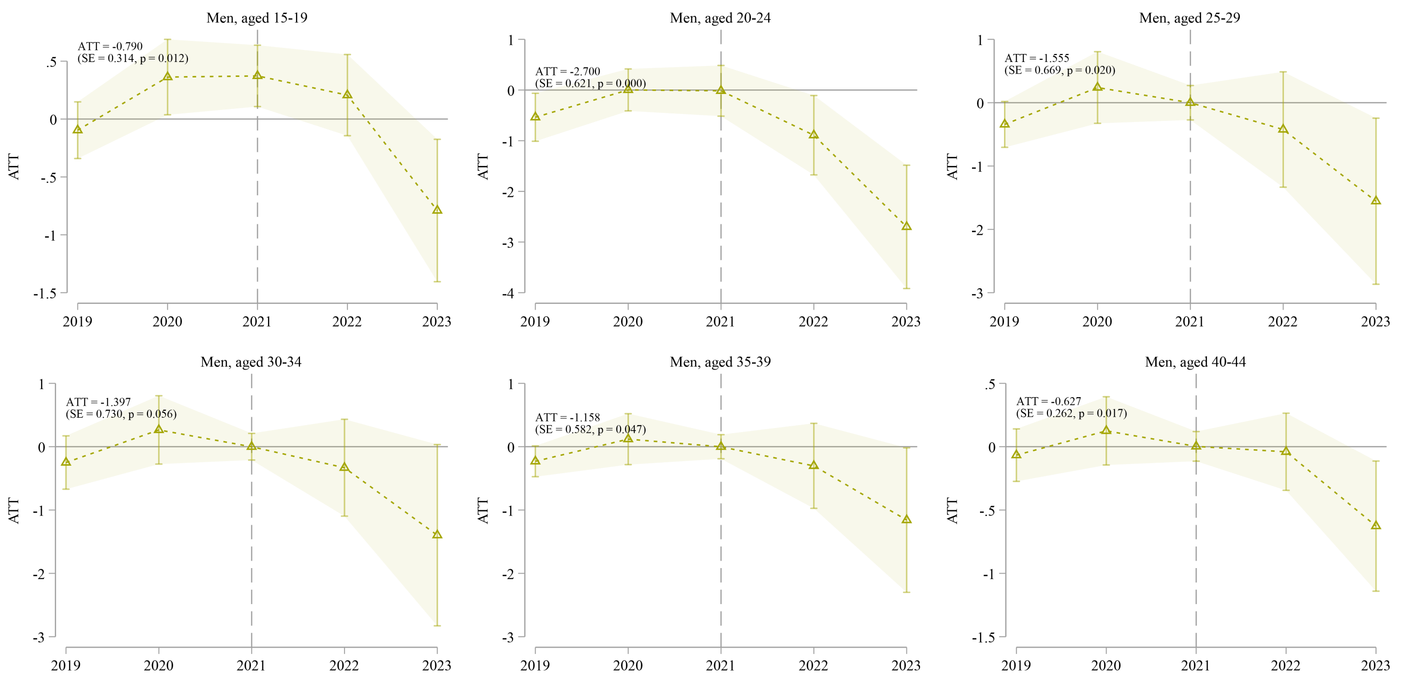
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcome is the log gonorrhea rate per 1,000 men of the indicated age group. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks 2022, the year Dobbs v. Jackson Women's Health was decided (June 2022). Data: NCHHSTP (2025), SEER (2025).

Figure B.2 — SDID event study estimates of the effect of a total abortion ban on female gonorrhea rates, by age group



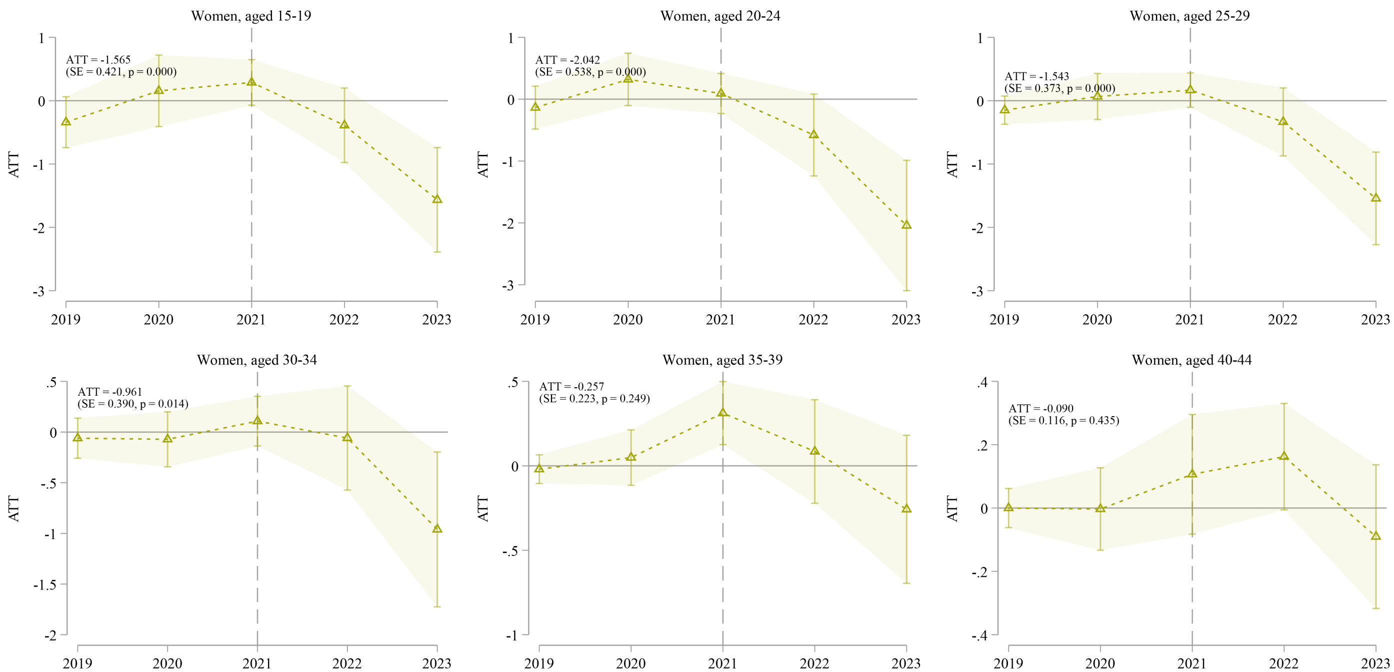
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcome is the log gonorrhea rate per 1,000 women of the indicated age group. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks 2022, the year Dobbs v. Jackson Women's Health was decided (June 2022). Data: NCHHSTP (2025), SEER (2025).

Figure B.3 — SDID event study estimates of the effect of a total abortion ban on male gonorrhea rates, by age group (level)



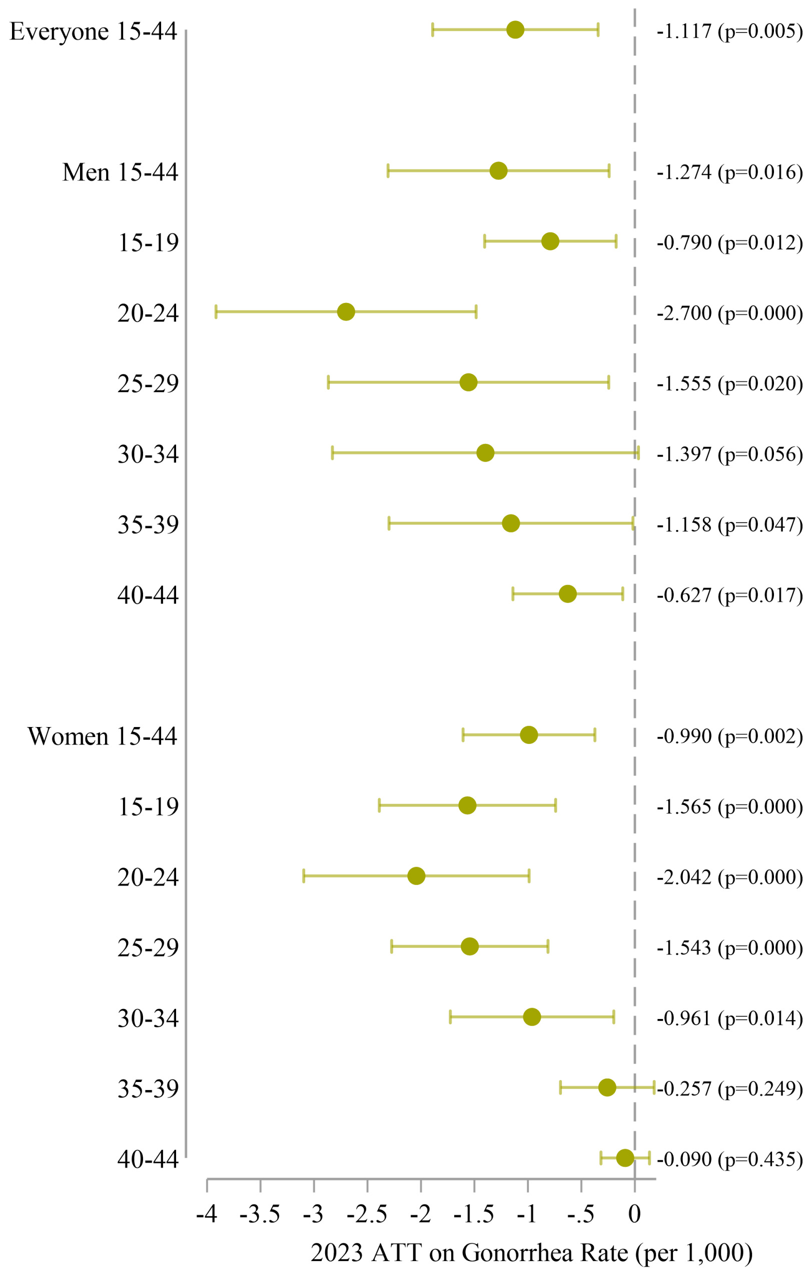
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcome is the level gonorrhea rate per 1,000 men of the indicated age group. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks 2022, the year Dobbs v. Jackson Women's Health was decided (June 2022). Data: NCHHSTP (2025), SEER (2025).

Figure B.4 — SDID event study estimates of the effect of a total abortion ban on female gonorrhea rates, by age group (level)



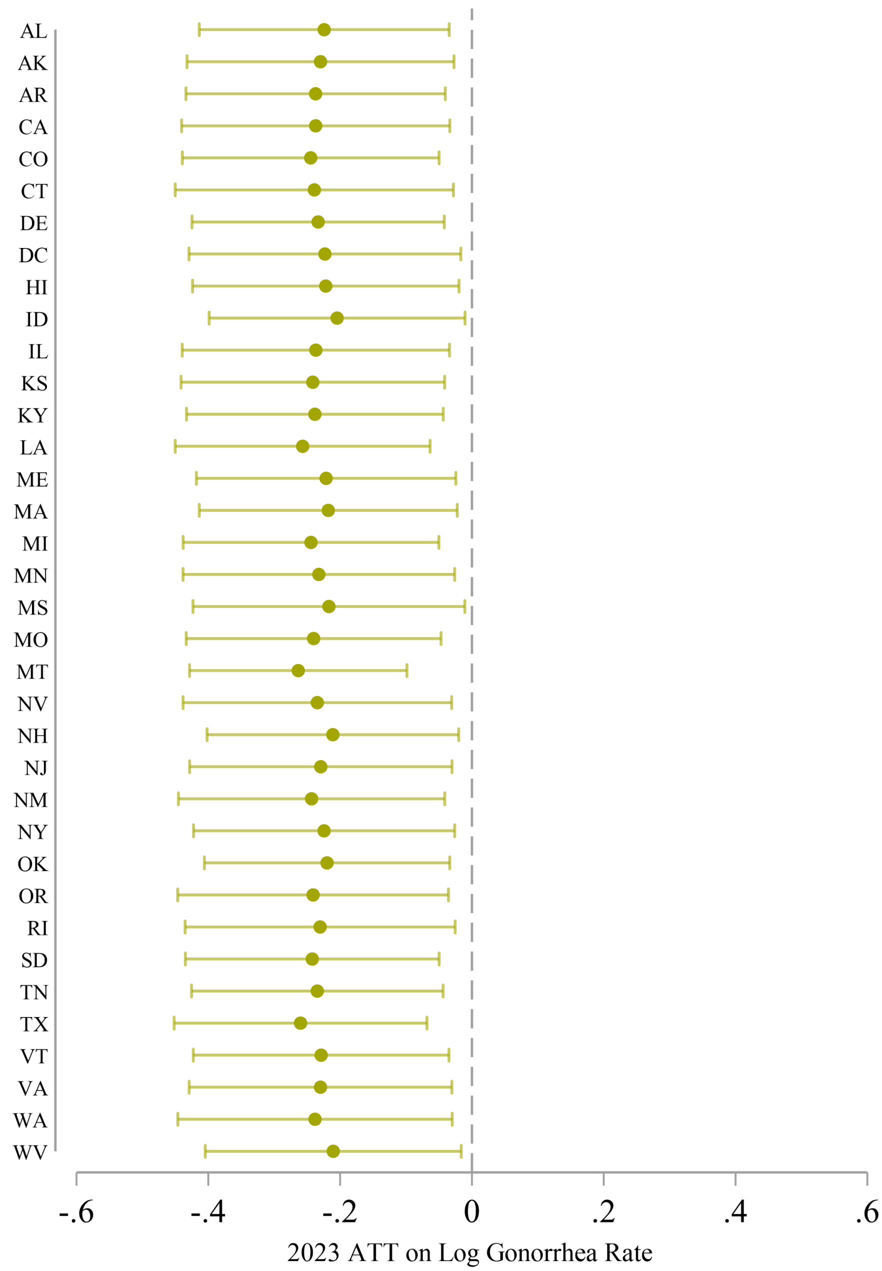
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcome is the level gonorrhea rate per 1,000 women of the indicated age group. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks 2022, the year Dobbs v. Jackson Women's Health was decided (June 2022). Data: NCHHSTP (2025), SEER (2025).

Figure B.5 — SDID event study estimates of the effect of a total abortion ban on gonorrhea rates (level)



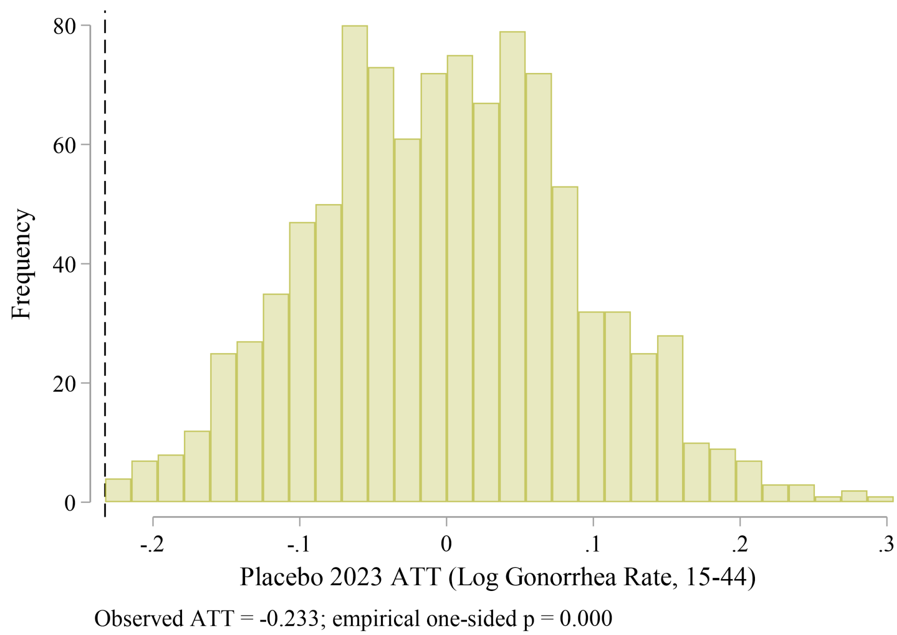
Notes: Each point shows the estimated 2023 ATT from a SDID event study of the gonorrhea cases per 1,000 population in the indicated subgroup. Horizontal bars indicate 95% confidence intervals based on placebo inference with 500 bootstrap replications. Rates are computed using SEER population denominators. The sample includes 12 total-ban states and 24 protected states, excluding hostile states. States with incomplete panels for a given subgroup are excluded from that subgroup's estimation. Data: NCHHSTP (2025), SEER (2025).

Figure B.6 — Leave-one-out analysis of the effect of a total abortion ban on gonorrhea rates



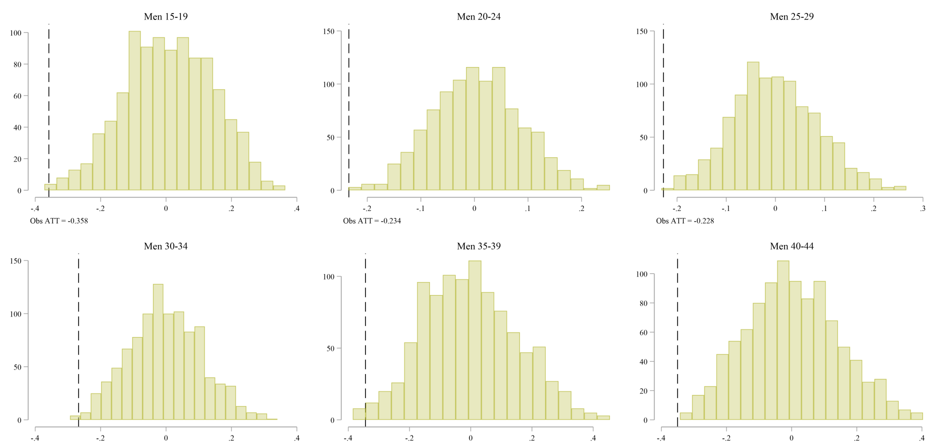
Notes: Leave-one-out analysis investigating whether the effect of a total abortion ban on gonorrhea is sensitive to dropping individual states. Each coefficient represents the 2023 ATT from a synthetic difference-in-difference where one of the 36 states included in our primary analysis is dropped before the specification is estimated. Data: NCHHSTP (2025), SEER (2025).

Figure B.7 — Distribution of placebo estimates of the effect of a total abortion ban on gonorrhea rates



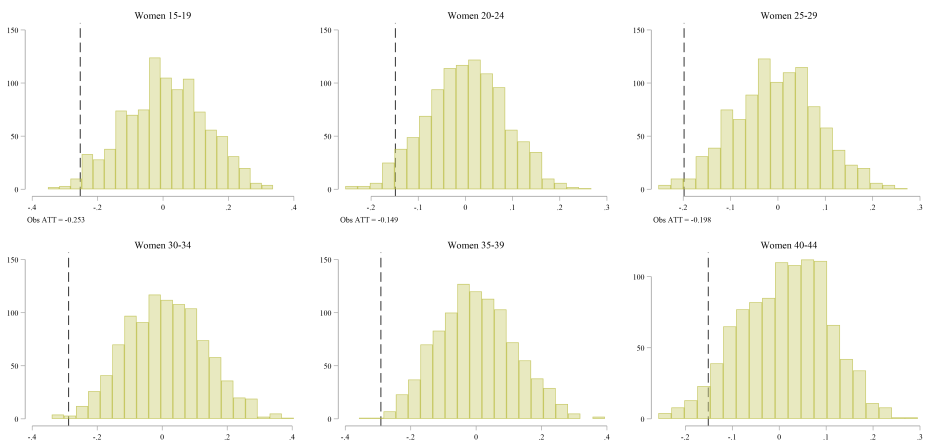
Notes: Randomization inference comparing our SDID estimate of the effect of abortion restrictions on gonorrhea to placebo specifications where treatment and control states were chosen at random. This graph displays a histogram of 1,000 placebo estimates, with a vertical line representing our actual SDID estimate. Data: NCHHSTP (2025), SEER (2025).

Figure B.8 — Distribution of placebo estimates of the effect of a total abortion ban on gonorrhea rates in men aged 15-44



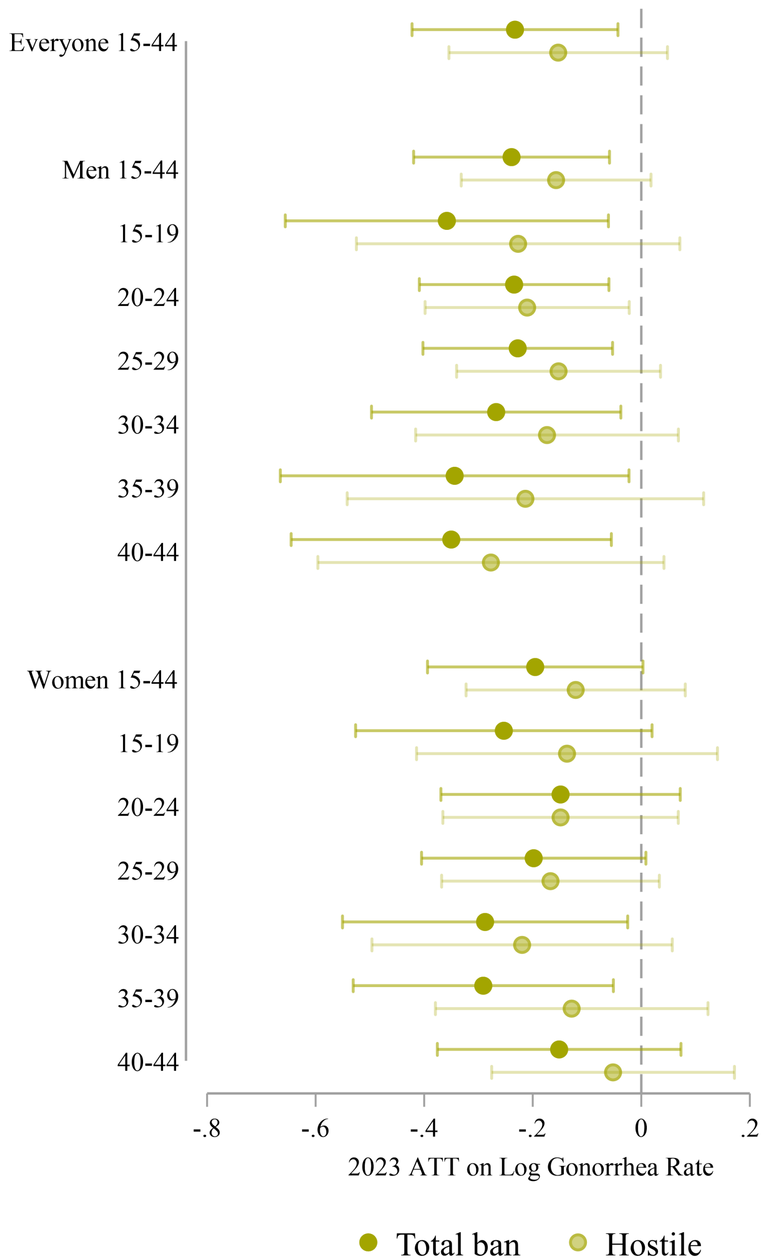
Notes: Randomization inference comparing our SDID estimate of the effect of abortion restrictions on gonorrhea in men to placebo specifications where treatment and control states were chosen at random. This graph displays histograms of 1,000 placebo estimates for each five-year age group from 15-19 through 40-44, with a vertical line representing our actual SDID estimate. Data: NCHHSTP (2025), SEER (2025).

Figure B.9 — Distribution of placebo estimates of the effect of a total abortion ban on gonorrhea rates in women aged 15-44



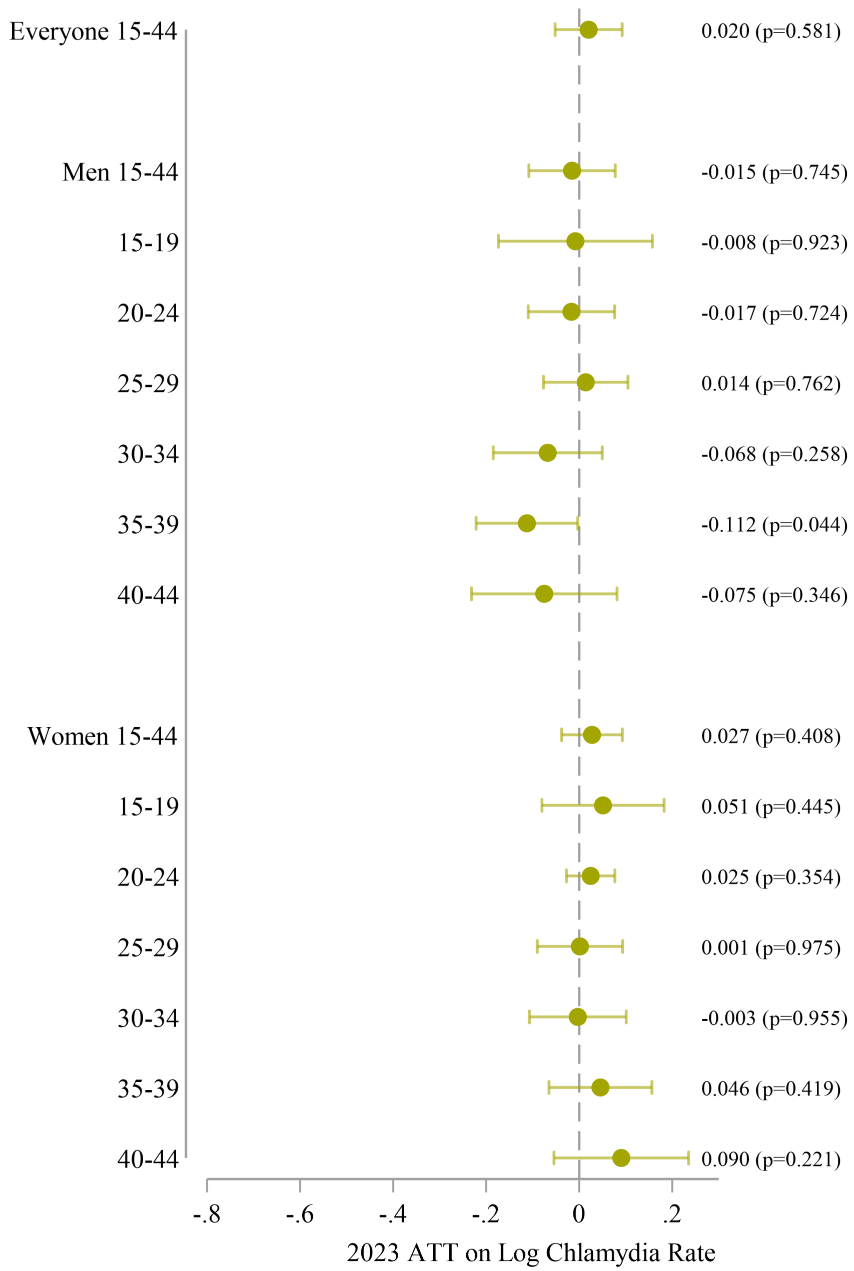
Notes: Randomization inference comparing our SDID estimate of the effect of abortion restrictions on gonorrhea in women to placebo specifications where treatment and control states were chosen at random. This graph displays histograms of 1,000 placebo estimates for each five-year age group from 15-19 through 40-44, with a vertical line representing our actual SDID estimate. Data: NCHHSTP (2025), SEER (2025).

Figure B.10 — Comparison of SDID event-study estimates of the effect of abortion restrictions on gonorrhea - ‘total ban’ states versus ‘hostile’



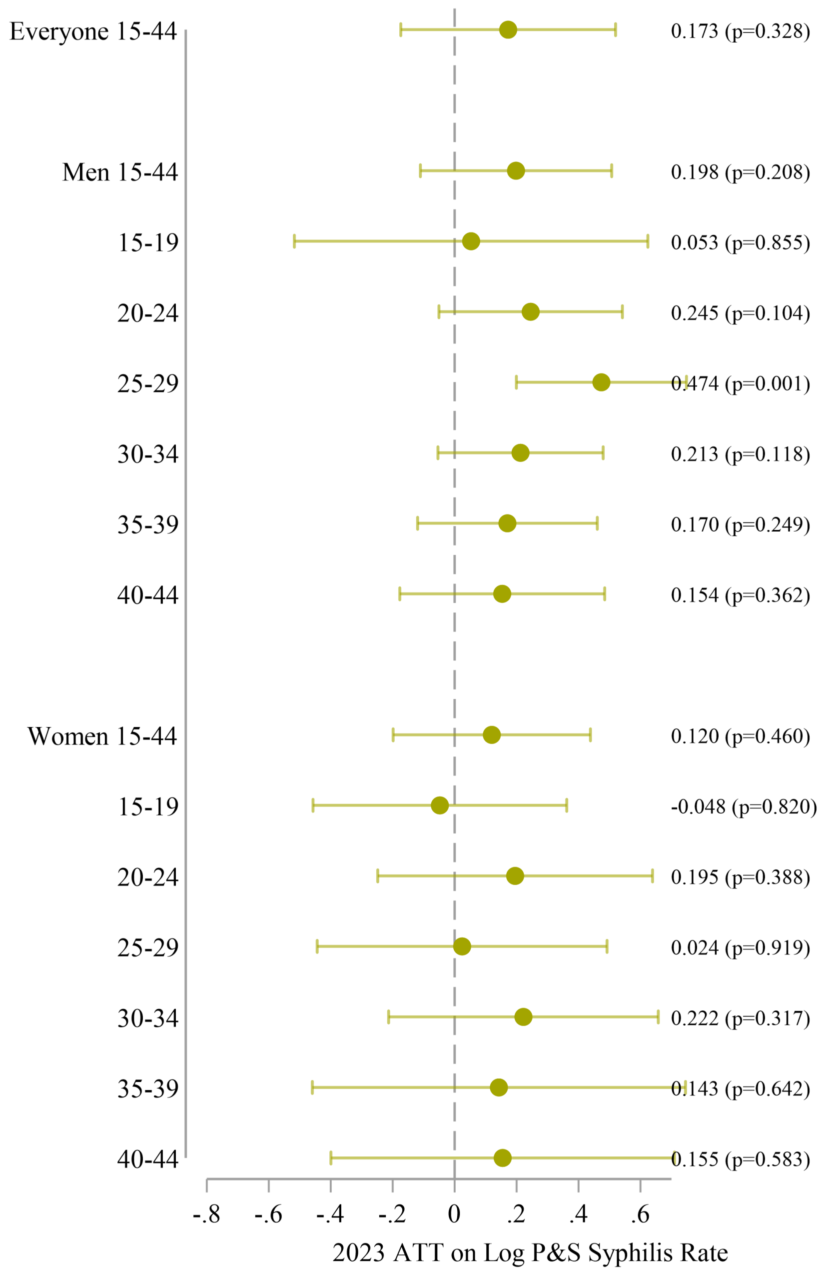
Notes: Each point shows the estimated 2023 ATT from a SDID event study of the log gonorrhea rate per 1,000 population in the indicated subgroup. Horizontal bars indicate 95% confidence intervals based on placebo inference with 500 bootstrap replications. The darker points and confidence intervals represent the estimate comparing total-ban states to protected states, while the lighter points and confidence intervals represent the estimate comparing hostile states to protected states. Rates are computed using SEER population denominators. The sample includes 12 total-ban states, 14 hostile states, and 24 protected states. States with incomplete panels for a given subgroup are excluded from that subgroup’s estimation. Data: NCHHSTP (2025), SEER (2025).

Figure B.11 — SDID event study estimates of the effect of a total abortion ban on chlamydia rates



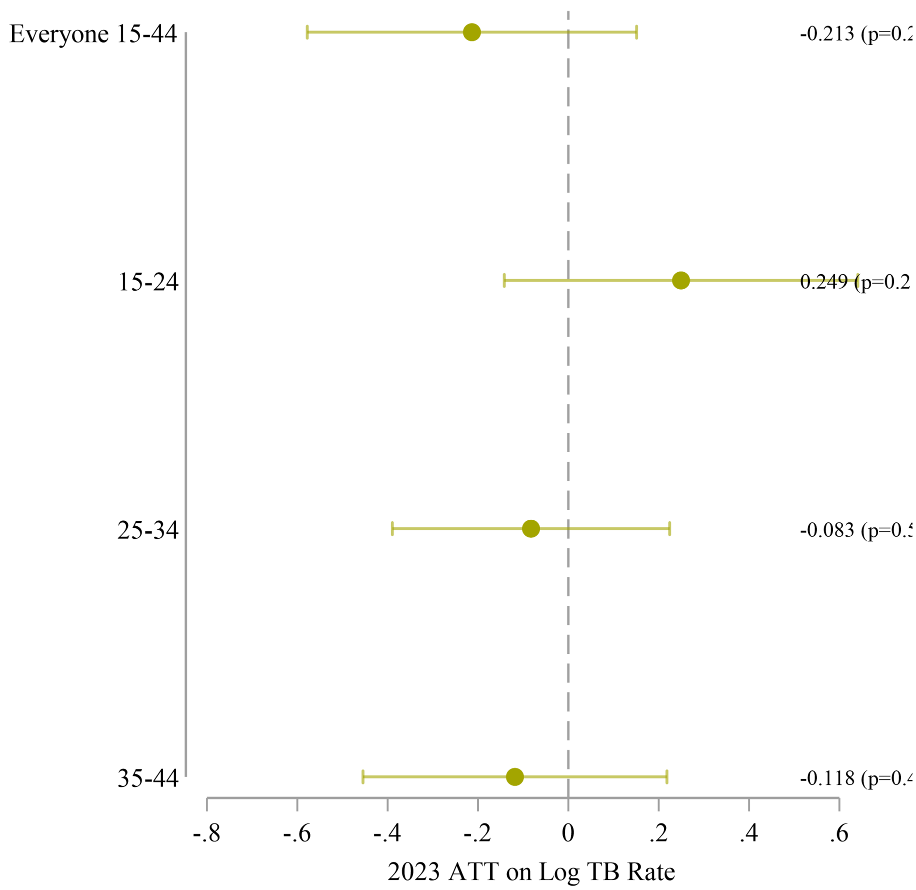
Notes: Each point shows the estimated 2023 ATT from a SDID event study of the log chlamydia rate per 1,000 population in the indicated subgroup. Horizontal bars indicate 95% confidence intervals based on placebo inference with 500 bootstrap replications. Rates are computed using SEER population denominators. The sample includes 12 total-ban states and 23 protected states, excluding hostile states. Maryland and Maine are excluded due to missing 2021 chlamydia data. States with incomplete panels for a given subgroup are excluded from that subgroup's estimation. Data: NCHHSTP (2025), SEER (2025).

Figure B.12 — SDID event study estimates of the effect of a total abortion ban on syphilis rates



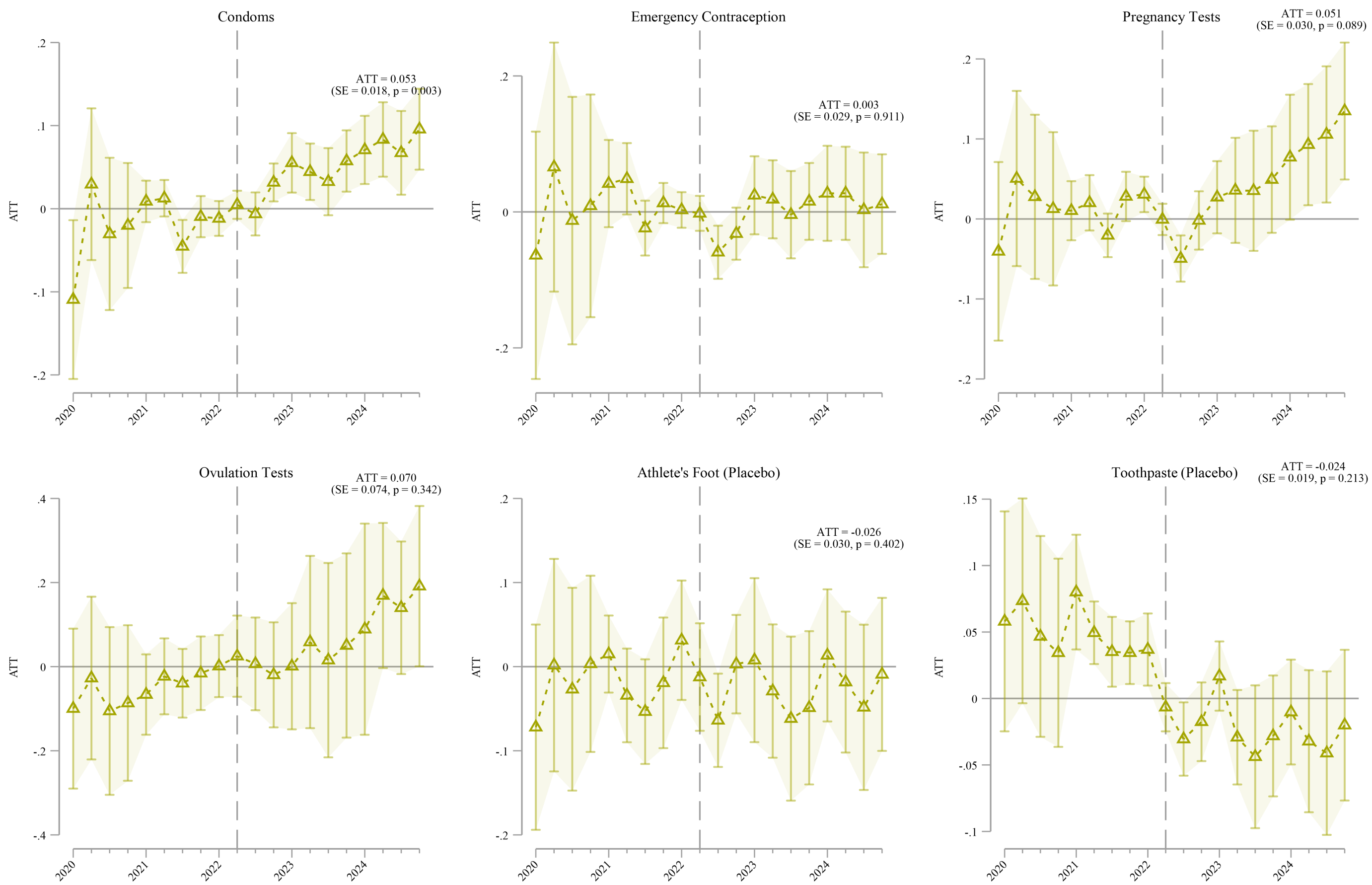
Notes: Each point shows the estimated 2023 ATT from a SDID event study of the log primary and secondary syphilis rate per 1,000 population in the indicated subgroup. Horizontal bars indicate 95% confidence intervals based on placebo inference with 500 bootstrap replications. Rates are computed using SEER population denominators. The sample includes 12 total-ban states and 24 protected states, excluding hostile states. States with incomplete panels for a given subgroup are excluded from that subgroup's estimation. Syphilis rates not reported in 2020. Data: NCHHSTP (2025), SEER (2025).

Figure B.13 — SDID event study estimates of the effect of a total abortion ban on tuberculosis rates



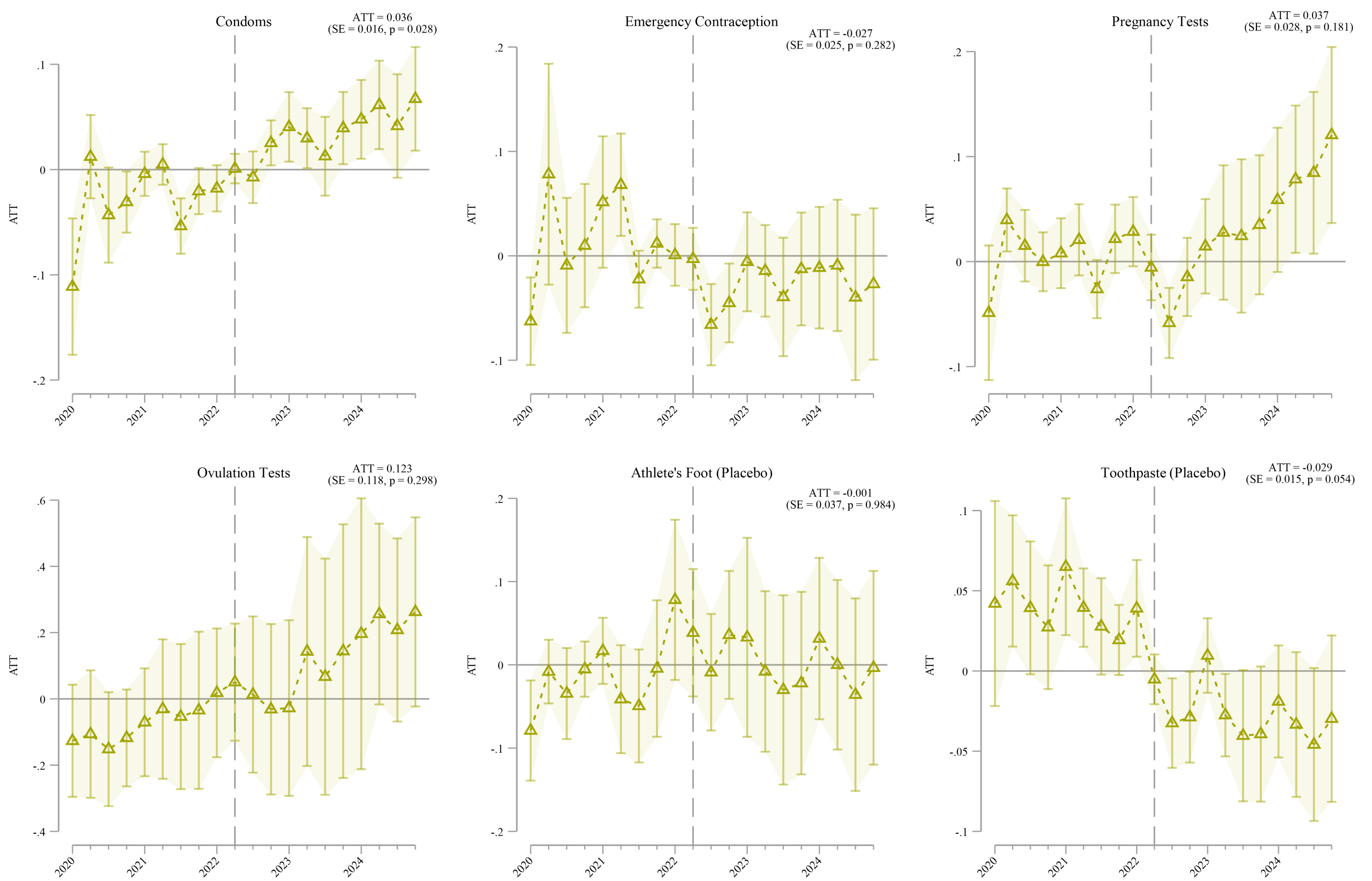
Notes: Each point shows the estimated 2023 ATT from a SDID event study of the log tuberculosis rate per 1,000 population in the indicated subgroup. Horizontal bars indicate 95% confidence intervals based on placebo inference with 500 bootstrap replications. Rates are computed using SEER population denominators. The sample includes 12 total-ban states and 25 protected states, excluding hostile states. States with incomplete panels for a given subgroup are excluded from that subgroup's estimation. Tuberculosis rates are not reported by sex. Data: NCHHSTP (2025), SEER (2025).

Figure B.14 — SDID event study estimates of the effect of a total abortion ban, by product (log units sold per capita, unbalanced panel)



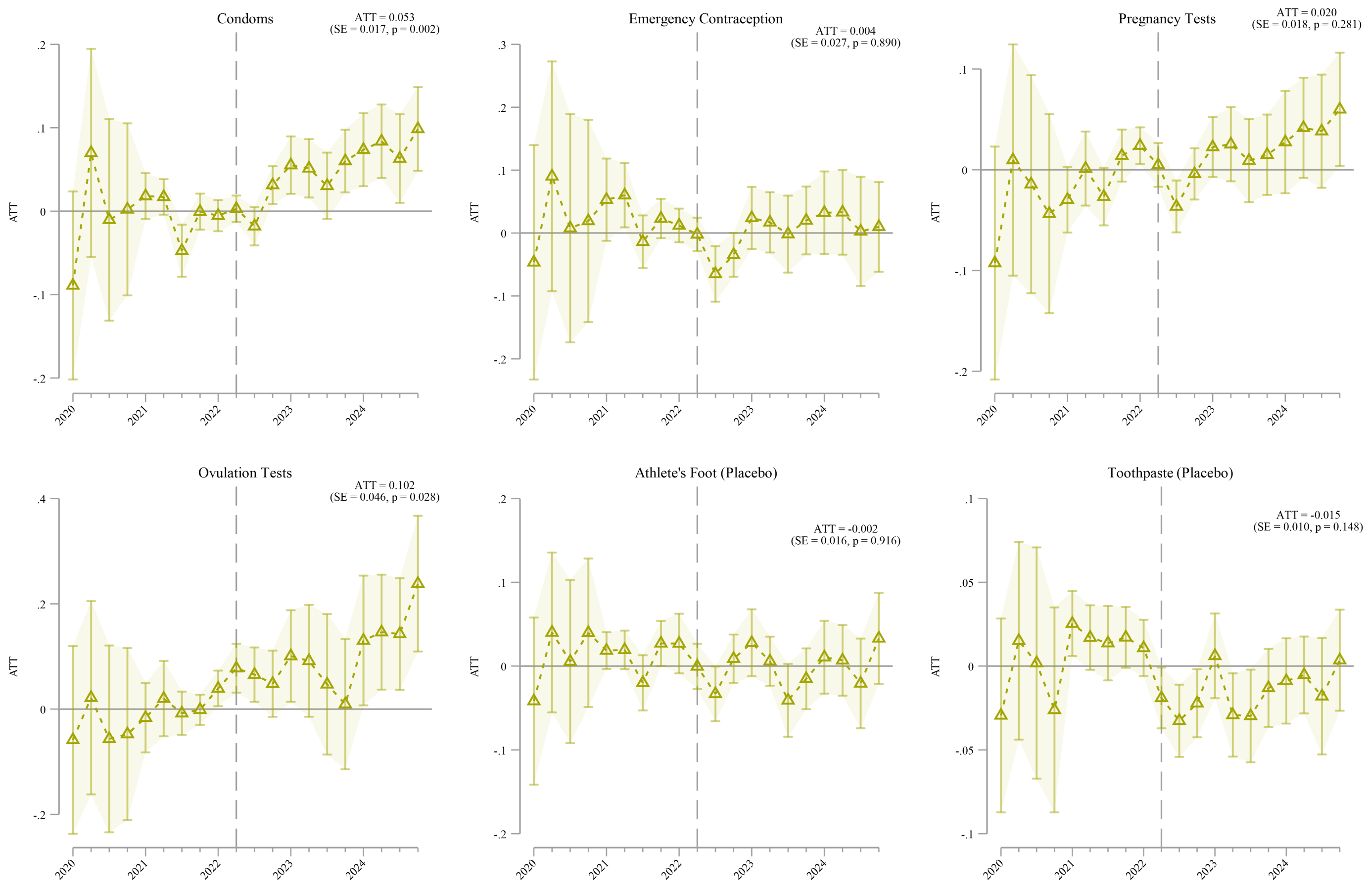
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are log quarterly units sold for each indicated product, aggregated from all stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.15 — SDID event study estimates of the effect of a total abortion ban, by product (log units sold per capita, balanced panel)



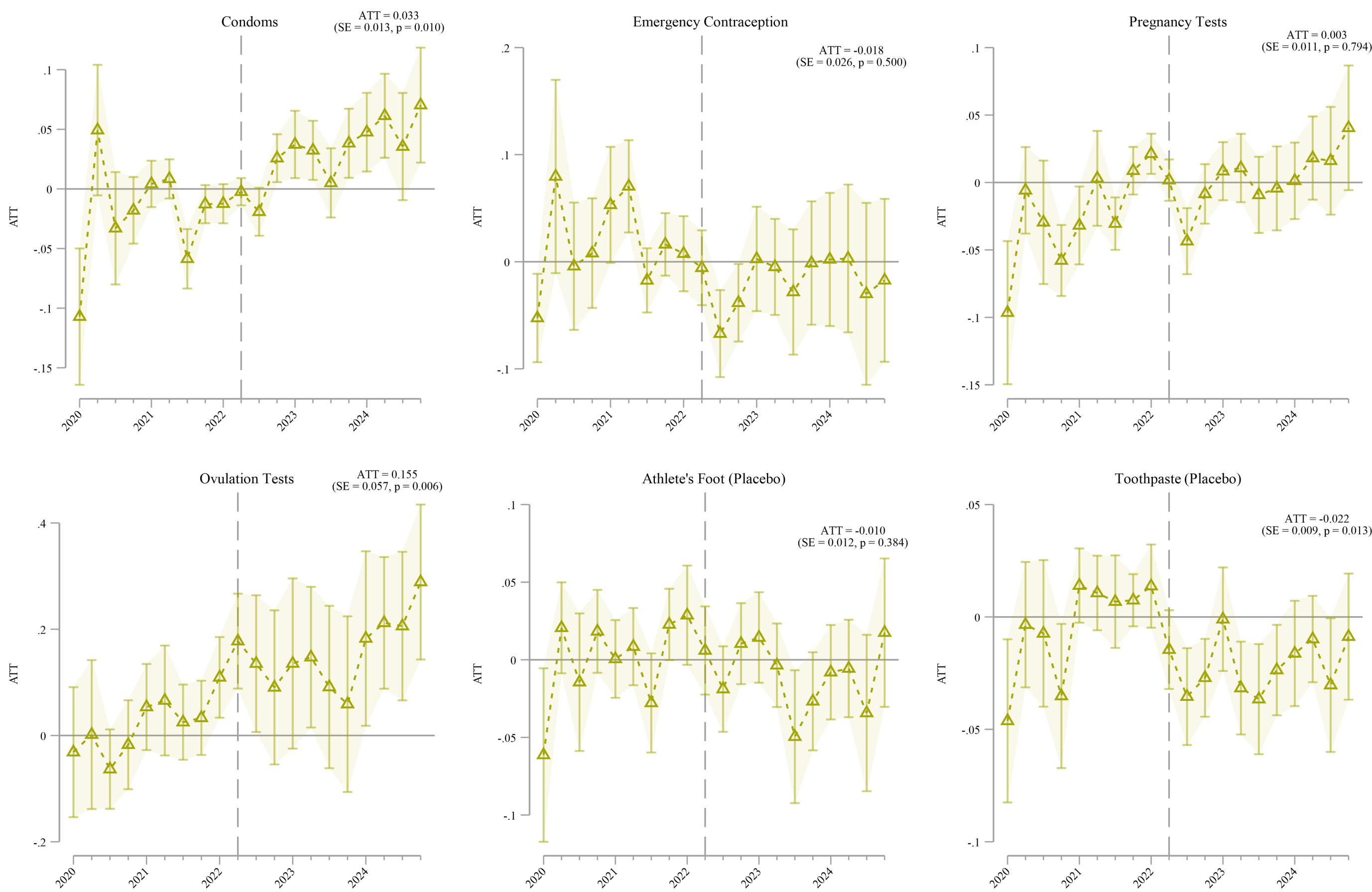
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are log quarterly units sold for each indicated product, aggregated from a balanced panel of stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.16 — SDID event study estimates of the effect of a total abortion ban, by product (log revenue per capita, unbalanced panel)



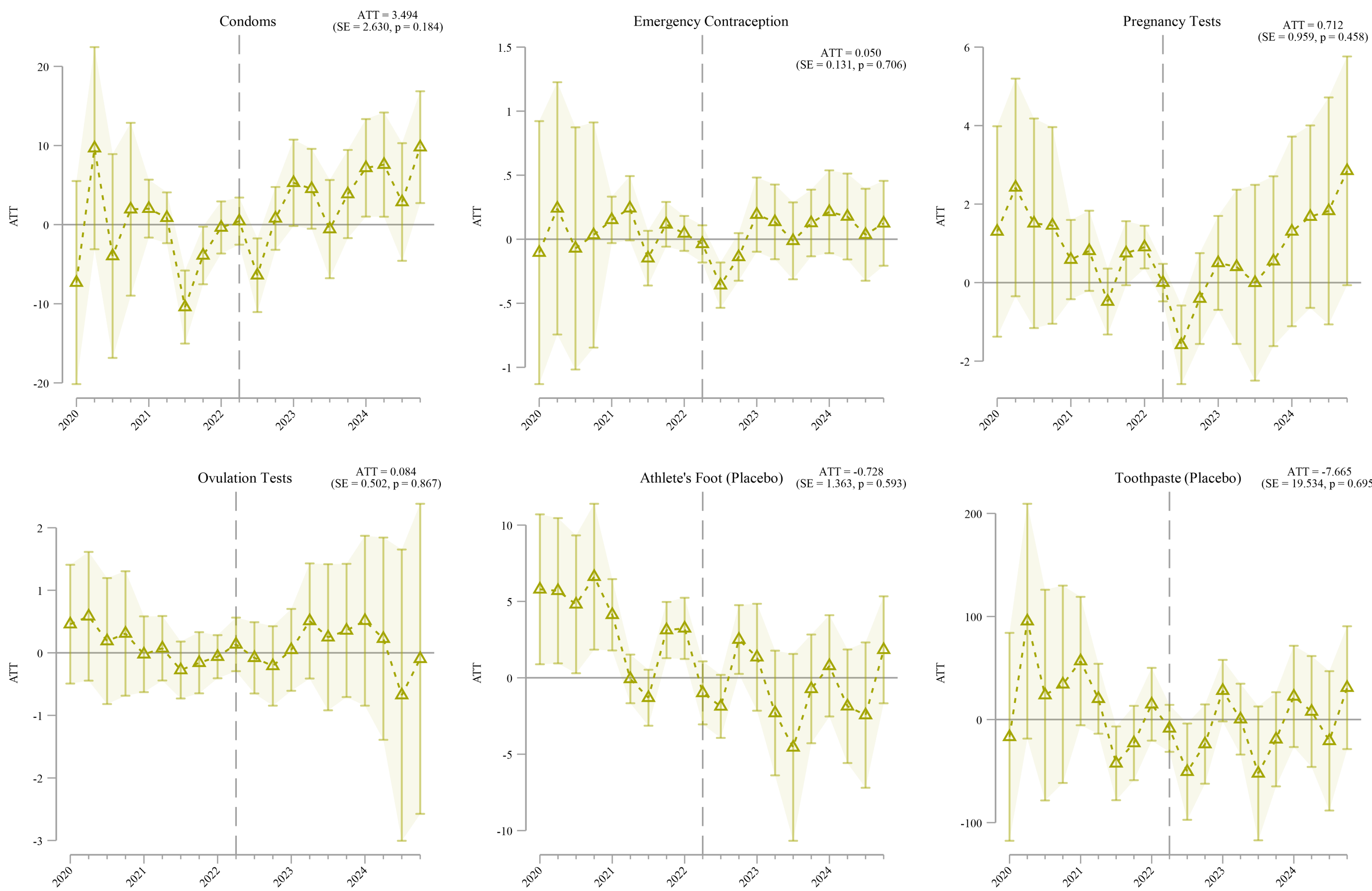
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are log quarterly sales revenue for each indicated product, aggregated from all stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.17 — SDID event study estimates of the effect of a total abortion ban, by product (log revenue per capita, balanced panel)



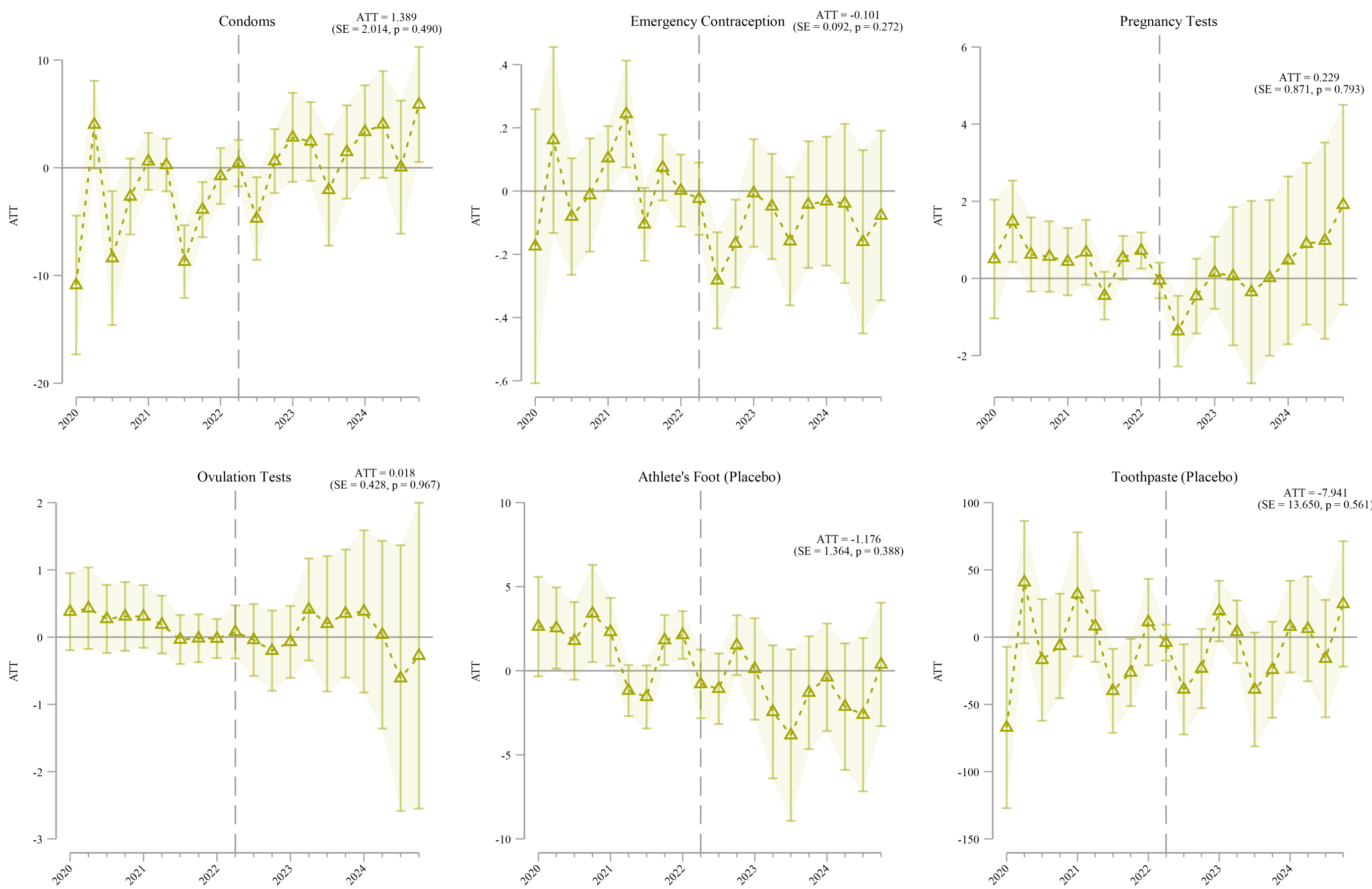
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are log quarterly revenue for each indicated product, aggregated from a balanced panel of stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.18 — SDID event study estimates of the effect of a total abortion ban, by product (level units sold per capita, unbalanced panel)



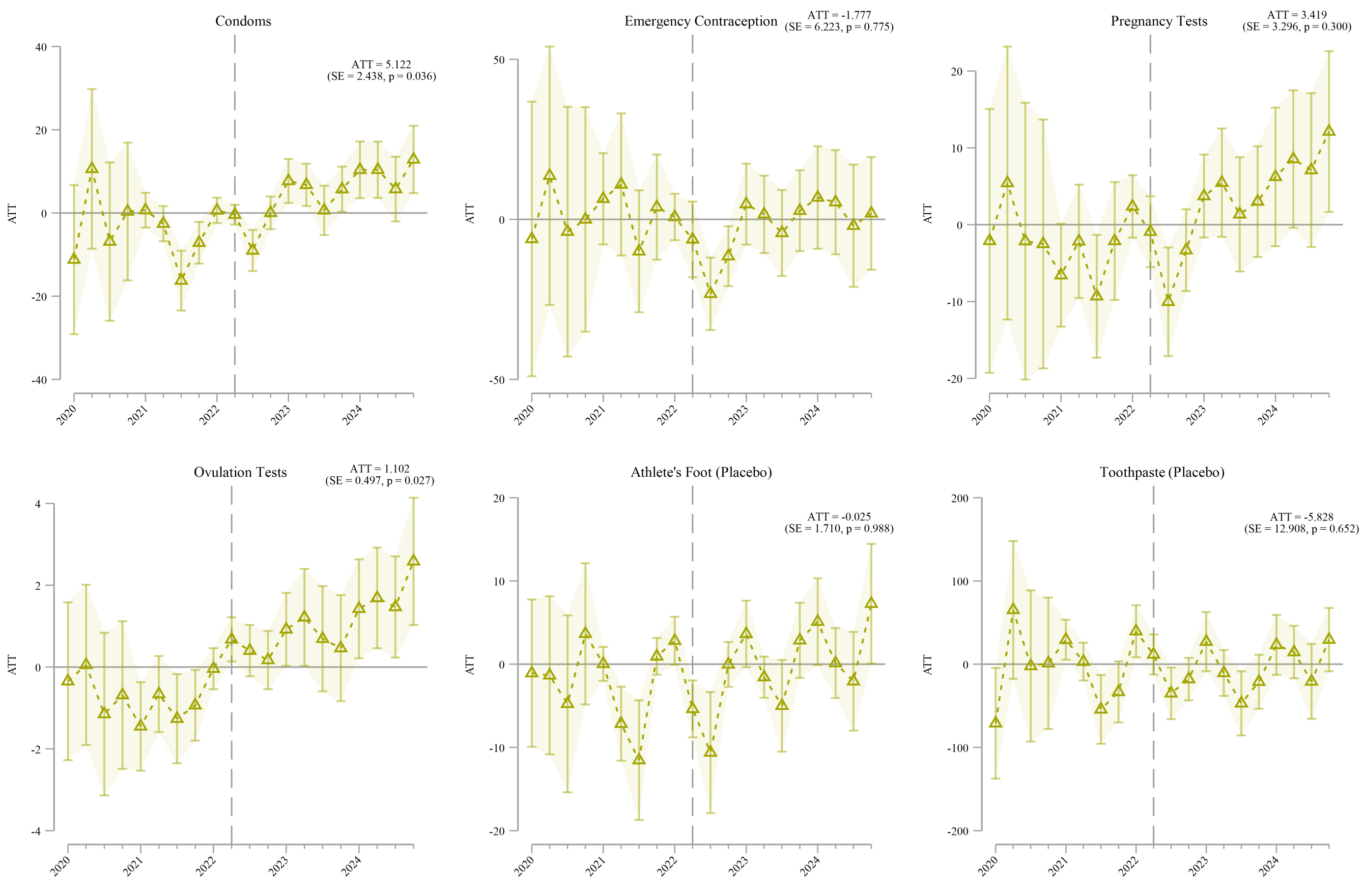
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are quarterly units sold per 1,000 population aged 15–44 for each indicated product, aggregated from all stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.19 — SDID event study estimates of the effect of a total abortion ban, by product (level units sold per capita, balanced panel)



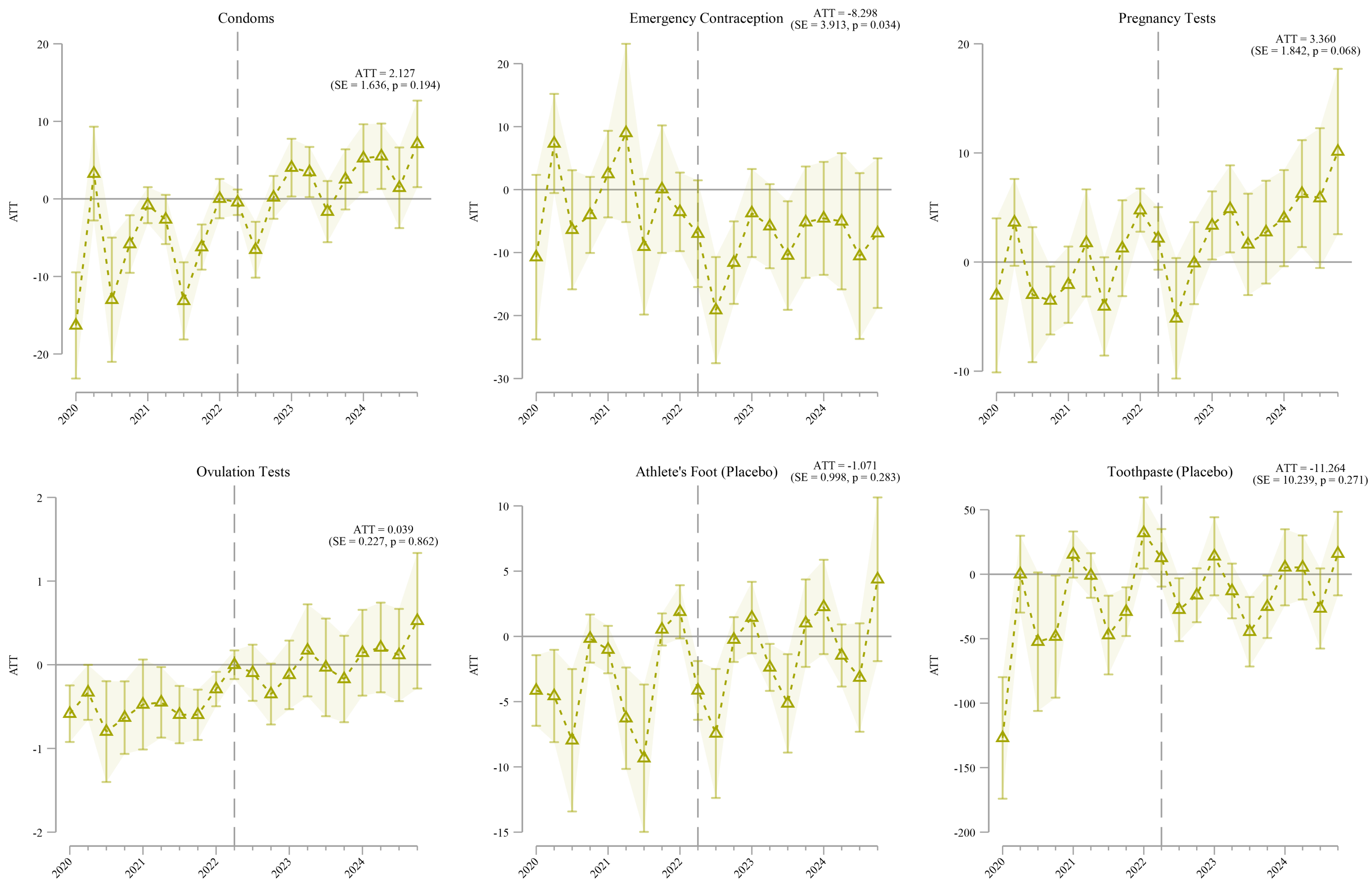
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are quarterly units sold per 1,000 population aged 15–44 for each indicated product, aggregated from a balanced panel of stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.20 — SDID event study estimates of the effect of a total abortion ban, by product (level revenue per capita, unbalanced panel)



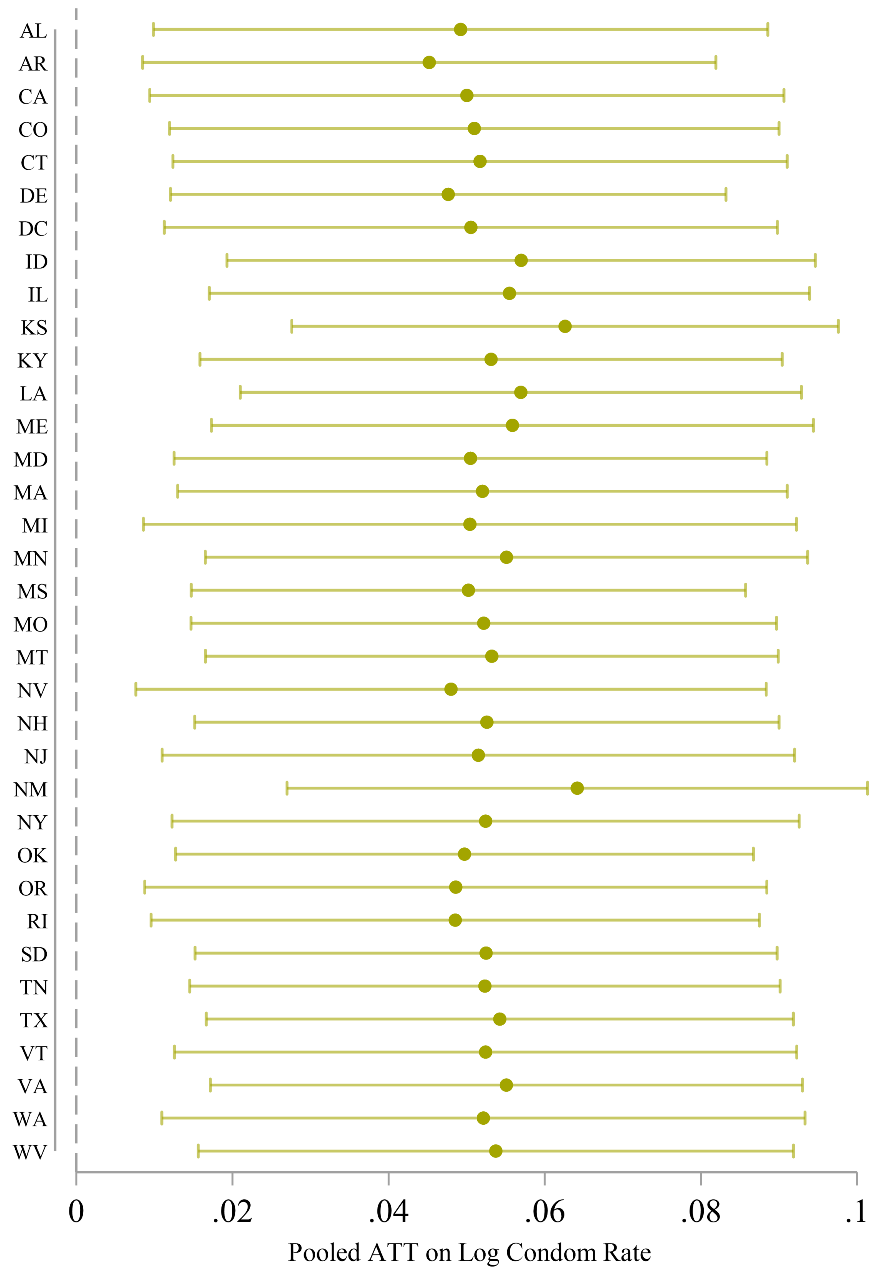
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are quarterly sales revenue per 1,000 population aged 15–44 for each indicated product, aggregated from all stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.21 — SDID event study estimates of the effect of a total abortion ban, by product (level revenue per capita, balanced panel)



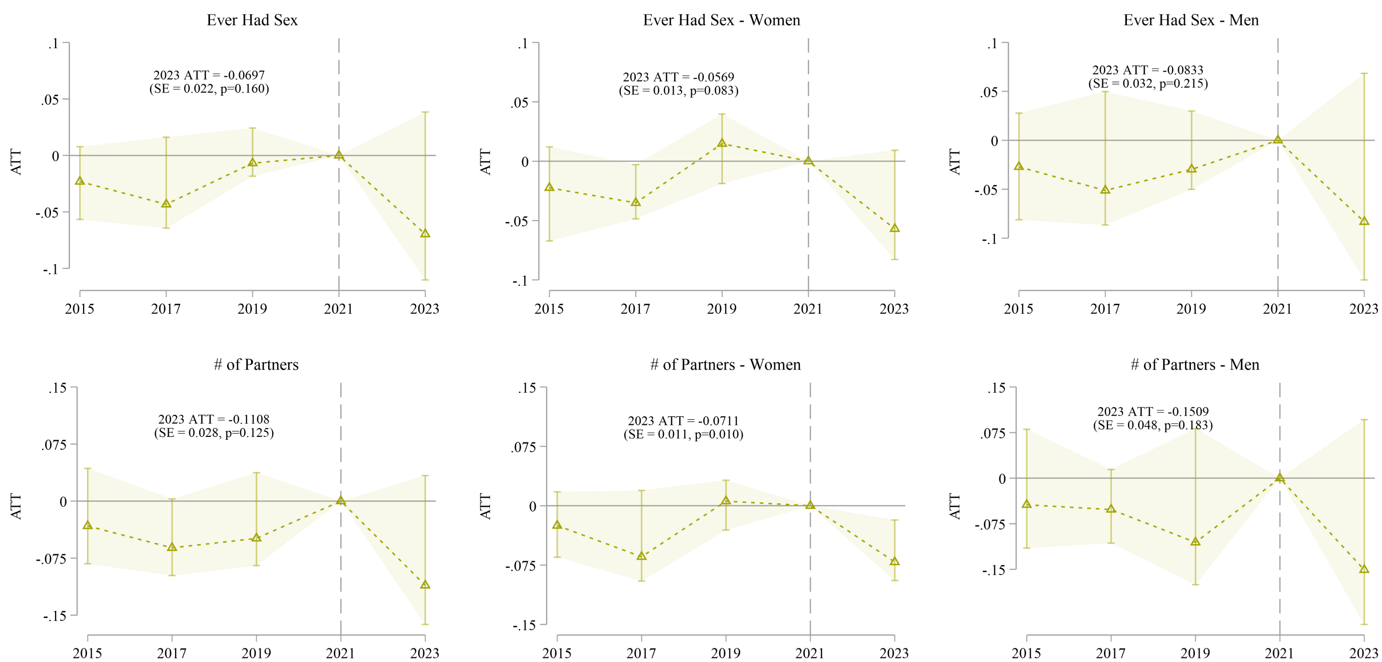
Notes: SDID event study estimates comparing states with total abortion bans to protected states. The outcomes are quarterly sales revenue per 1,000 population aged 15–44 for each indicated product, aggregated from a balanced panel of stores. Shaded areas and vertical bars show 95% confidence intervals from placebo inference (500 replications). The dashed line marks Q2 2022, the last quarter before Dobbs v. Jackson Women's Health. Quarter dummies included as projected covariates. Data: NielsenIQ (2025).

Figure B.22 — Leave-one-out analysis of the effect of a total abortion ban on condom purchases



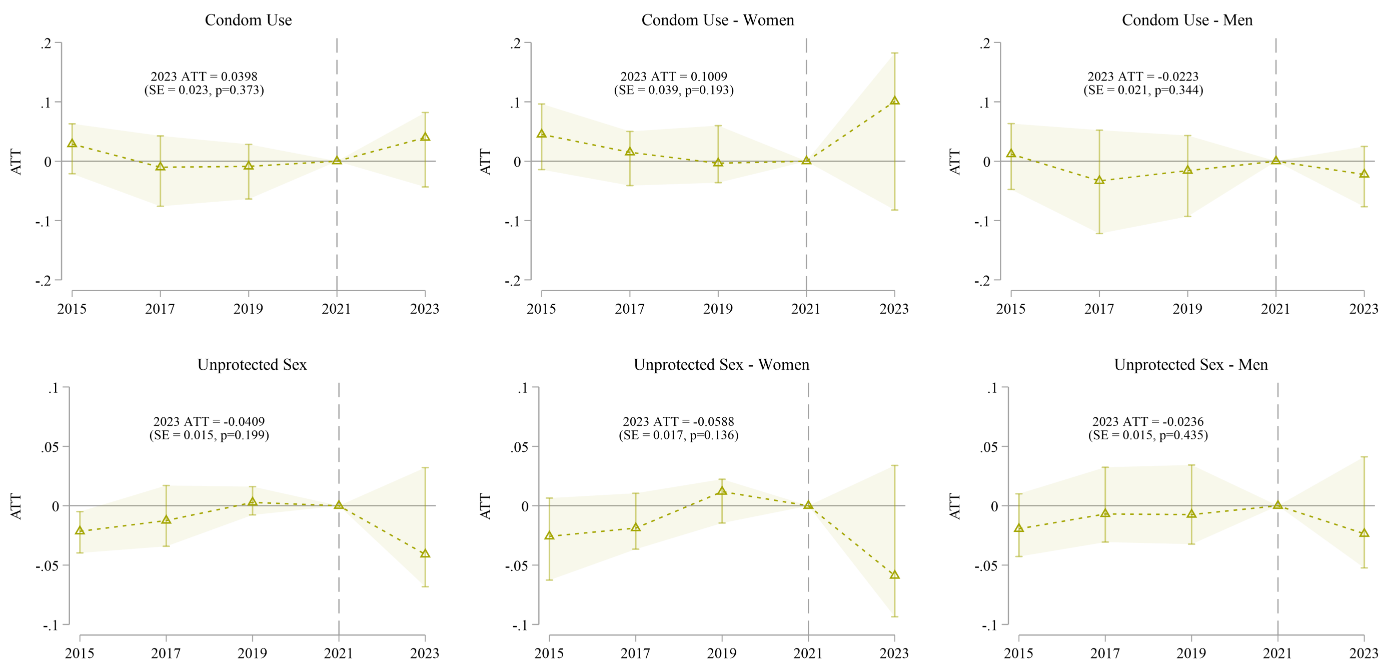
Notes: Leave-one-out analysis investigating whether the effect of a total abortion ban on condom purchases is sensitive to dropping individual states. Each coefficient represents the pooled ATT from a synthetic difference-in-differences specification where one of the 35 states included in our primary analysis is dropped before the specification is estimated. The outcome is the log of quarterly condom units sold per 1,000 population aged 15–44, aggregated across all stores in the Nielsen panel at the state-quarter level. Horizontal bars show 95% confidence intervals from placebo inference (500 replications). Quarter dummies included as projected covariates. Data: NielsenIQ (2025), SEER (2025).

Figure B.23 — TWFE event-study estimates of the effect of a total abortion ban on responses to questions about sexual partners in YRBS, total and by sex



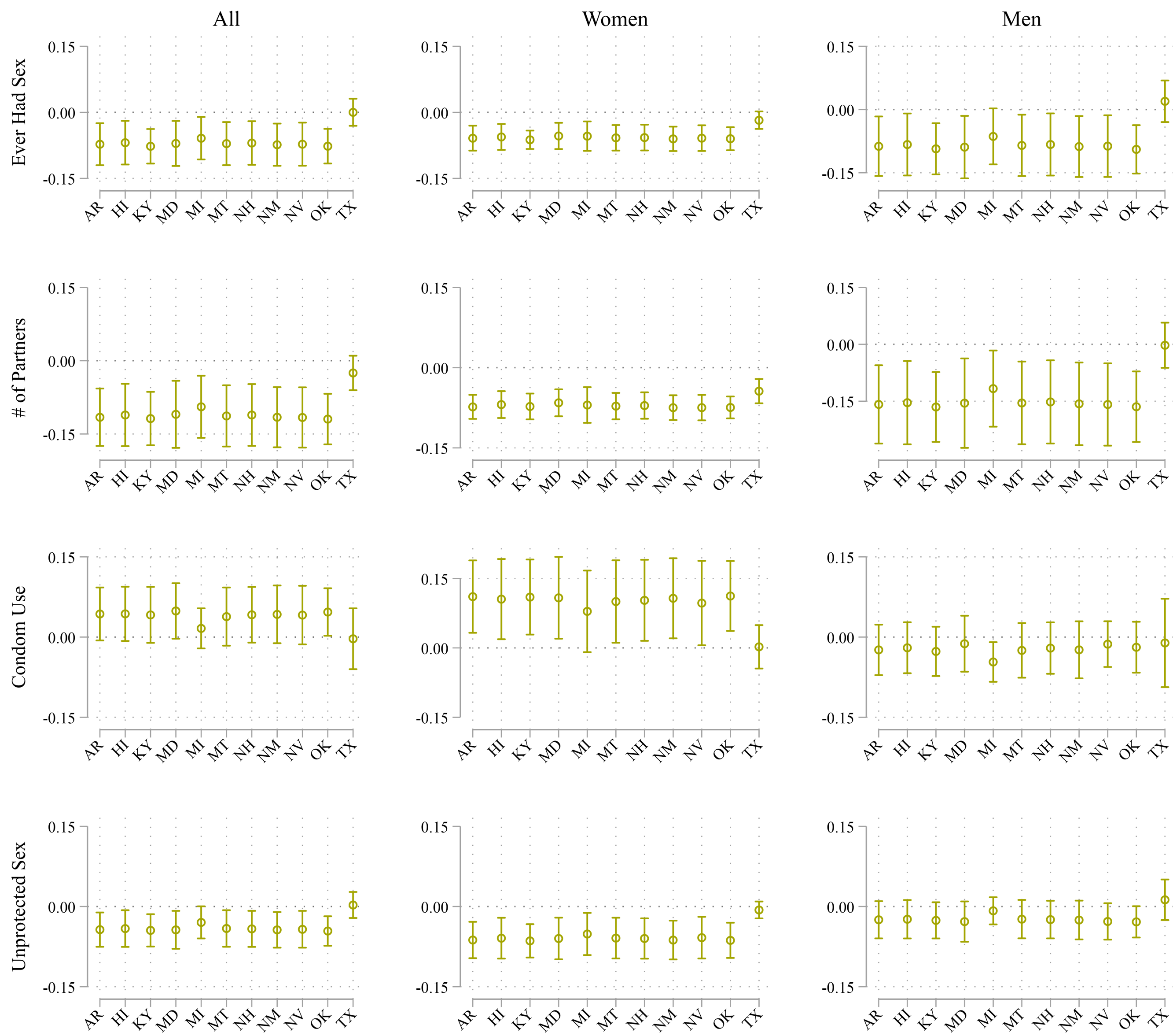
Notes: Difference-in-differences event study estimates comparing states with total abortion bans to protected states. The outcomes are individual responses of high school students to questions about sexual behavior and the models are linear probability models with age, sex, race, state, and year fixed effects. Shaded areas and vertical bars show 95% confidence intervals computed via the wild cluster bootstrap with Webb weights and 9,999 replications, clustered at the state level. The dashed line marks 2022, the year of Dobbs v. Jackson Women's Health (June 2022). Data: CDC (2025).

Figure B.24 — TWFE event-study estimates of the effect of a total abortion ban on responses to questions about contraception use in YRBS, total and by sex



Notes: Difference-in-differences event study estimates comparing states with total abortion bans to protected states. The outcomes are individual responses of high school students to questions about sexual behavior and the models are linear probability models with age, sex, race, state, and year fixed effects. Shaded areas and vertical bars show 95% confidence intervals computed via the wild cluster bootstrap with Webb weights and 9,999 replications, clustered at the state level. The dashed line marks 2022, the year of Dobbs v. Jackson Women's Health (June 2022). Data: CDC (2025).

Figure B.25 — Leave-One-Out analysis of the effect of a total abortion ban on responses to questions about contraception use in YRBS



Notes: Leave-one-out analysis investigating whether the effect of a total abortion ban on self-reported sexual behavior is sensitive to dropping individual states. Each coefficient represents the 2023 ATT from an event-study specification where one of the 11 states included in our primary YRBS

Table B.1 — Pre-period summary statistics by state policy category, Youth Risk Behavior Survey

	Ban states		Protected states	
	Mean	(SD)	Mean	(SD)
Panel A: Dependent Variables				
% Ever Had Sex	0.389	0.487	0.334	0.472
# of Sexual Partners	0.396	0.860	0.330	0.775
% Used Condom	0.526	0.499	0.574	0.494
% Had Unprotected Sex	0.182	0.386	0.141	0.348
Panel B: Other Covariates				
% Female	0.490	0.500	0.491	0.500
% White	0.400	0.490	0.526	0.499
% Black	0.126	0.332	0.161	0.368
% Hispanic	0.389	0.488	0.185	0.389
Age	16.004	1.250	15.835	1.254
Grade	10.415	1.116	10.431	1.119
Observations	29,671		326,125	

Notes: This table reports pre-period (2015–2021) means and standard deviations for the Youth Risk Behavior Survey (YRBS) analyses employed in the paper. States are classified by policy environment as of December 2022. Panel A reports responses to our dependent variable questions. Panel B reports means for other covariates included in our main specifications. All statistics are weighted by the YRBS provided survey weights. See Appendix A for state policy classifications. Data: Centers for Disease Control and Prevention (CDC) (2025).

Table B.2 — Pre-period summary statistics by state policy category, estimated over counties

	Ban states		Hostile states		Protected states	
	Mean	(SD)	Mean	(SD)	Mean	(SD)
Panel A: STI outcomes (county × year)						
Gonorrhea rate, total	5.33	(3.51)	4.88	(3.13)	4.18	(2.66)
Gonorrhea rate, male	5.61	(3.77)	5.28	(3.47)	4.98	(3.50)
Gonorrhea rate, female	5.08	(3.52)	4.49	(3.07)	3.39	(2.33)
County × year observations	3,101		2,810		2,710	
Counties	1,114		1,003		1,000	
Panel B: Purchase outcomes (county × year, balanced)						
Condom units	903.59	(540.34)	1220.28	(573.69)	1551.21	(572.08)
Emergency contraception units	49.07	(27.62)	41.67	(22.34)	41.89	(20.92)
Pregnancy test units	200.81	(100.19)	219.42	(89.95)	229.42	(86.48)
Ovulation test units	14.29	(13.87)	18.62	(17.25)	25.18	(18.58)
Athlete's foot units (placebo)	148.87	(84.65)	177.50	(87.06)	232.36	(105.97)
Toothpaste units (placebo)	7447.73	(4518.47)	10128.04	(4777.31)	12600.04	(4601.08)
County × year observations	3,342		3,009		2,973	
Counties	1,114		1,003		991	
Panel C: Additional covariates						
Distance to provider (mi.)	48.32	(57.98)	27.67	(36.44)	13.15	(23.76)
Unemployment rate	5.30	(2.04)	5.20	(2.10)	6.32	(2.90)
Poverty rate	14.58	(5.05)	12.53	(4.10)	11.44	(4.10)
White (%)	55.14	(23.66)	61.68	(20.33)	52.23	(21.24)
Black (%)	16.78	(14.62)	17.01	(13.98)	11.73	(10.38)
Hispanic (%)	22.91	(22.06)	16.26	(14.19)	25.02	(17.17)
County × year observations	3,342		3,009		2,973	
Counties	1,114		1,003		1,000	

Notes: This table reports pre-period (2019–2021) means and standard deviations for the county-level analyses employed in the paper. States are classified by policy environment as of December 2024. Panel A reports gonorrhea rates per 1,000 population aged 15–44 at the county × year level. Panel B reports product units sold per 1,000 population aged 15–44 at the county × year level for the balanced panel of counties with non-missing sales in all years. Panel C reports county × year treatment and control variables. Distance is measured in miles to the nearest abortion provider. Demographic shares are computed from SEER population estimates for ages 15–44. All statistics are weighted by the county population aged 15–44. See Appendix A for state policy classifications. Data: NielsenIQ (2025), SEER (2025), NCHHSTP (2025).

Table B.3 — Effect of Total Abortion Bans on STI and TB Rates: Robustness to Estimator and Functional Form

	Gonorrhea	Chlamydia	Syphilis	TB
Panel A: Log rate per 1,000, ages 15–44 (SDID)				
ATT	-0.144*	0.051	0.139	-0.062
	(0.074)	(0.034)	(0.169)	(0.137)
Panel B: Log rate per 1,000, ages 15–44 (OLS)				
ATT	-0.181***	0.003	0.304**	-0.043
	(0.059)	(0.032)	(0.137)	(0.092)
Panel C: Log rate per 1,000, ages 15–44 (weighted OLS)				
ATT	-0.062	0.032	0.253***	-0.006
	(0.069)	(0.039)	(0.051)	(0.053)
Panel D: Cases, ages 15–44 (Poisson)				
ATT	-0.065	0.034	0.233***	0.008
	(0.062)	(0.035)	(0.060)	(0.045)
Panel E: Rate per 1,000, ages 15–44 (SDID)				
ATT	-0.639**	0.508	0.130**	-0.002
	(0.307)	(0.372)	(0.052)	(0.002)
Panel F: Rate per 1,000, ages 15–44 (OLS)				
ATT	-0.892***	0.068	0.169*	-0.002
	(0.292)	(0.377)	(0.103)	(0.001)
Panel G: Rate per 1,000, ages 15–44 (weighted OLS)				
ATT	-0.401	0.358	0.103***	-0.000
	(0.345)	(0.443)	(0.029)	(0.002)

Notes: This table reports the effect of total abortion bans on STI and tuberculosis (TB) rates at the state-year level, 2019–2023. The treatment indicator equals one for states with a total abortion ban in effect following the Dobbs decision in June 2022. States with partial restrictions (hostile states) are excluded from the comparison group. Panel A uses the synthetic difference-in-differences (SDID) estimator of Arkhangelsky et al. (2021), with inference based on placebo variance estimation using 500 replications. Panels B–C report two-way fixed effects (TWFE) difference-in-differences estimates with state and year fixed effects; Panel B uses unweighted OLS and Panel C uses OLS weighted by state population. In Panels A–C, the dependent variable is the natural log of diagnosed cases per 1,000 population aged 15–44. Panel D estimates Poisson models with raw case counts as the outcome and population aged 15–44 as the exposure variable. Panels E–G repeat the SDID, OLS, and WOLS specifications using the level rate (cases per 1,000) as the dependent variable. Maryland is excluded from gonorrhea and chlamydia samples and Maine from chlamydia samples due to missing 2021 data. Syphilis estimates exclude 2020 due to COVID-related reporting disruption. Standard errors in Panels B–D and F–G are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4 — Non-absorbing DiD sensitivity: STI and TB rates with Indiana, North Dakota, and Wisconsin reclassified as ban states

	Gonorrhea	Chlamydia	Syphilis	TB
Panel A: Log rate per 1,000, ages 15–44 (OLS)				
ATT	-0.261***	-0.019	0.360**	-0.176*
	(0.073)	(0.037)	(0.147)	(0.104)
Panel B: Log rate per 1,000, ages 15–44 (weighted OLS)				
ATT	-0.119	0.011	0.289***	-0.077
	(0.087)	(0.049)	(0.055)	(0.072)
Panel C: Cases, ages 15–44 (Poisson)				
ATT	-0.123	0.014	0.257***	-0.031
	(0.081)	(0.045)	(0.063)	(0.067)
Panel D: Rate per 1,000, ages 15–44 (OLS)				
ATT	-1.302***	-0.229	0.181*	-0.003**
	(0.367)	(0.448)	(0.101)	(0.002)
Panel E: Rate per 1,000, ages 15–44 (weighted OLS)				
ATT	-0.681	0.091	0.110***	-0.002
	(0.432)	(0.568)	(0.030)	(0.002)

Notes: This table reports two-way fixed effects DiD estimates of the effect of total abortion bans on STI and tuberculosis (TB) rates at the state-year level, 2019–2023, using a non-absorbing definition of treatment. Indiana, North Dakota, and Wisconsin—reclassified as "hostile" and excluded from Table B.3 because their total bans did not remain in force for the full post-Dobbs period—are here reclassified as ban states. The treatment variable is E_{st}, the fraction of state-year st during which a total ban was in effect; the reported ATT is interpretable as the effect of a full year under ban. Other hostile states remain excluded from the comparison group. Panels A–C report results for the log-rate and count specifications; Panels D–E report results for the level rate. SDID panels are omitted because SDID requires absorbing treatment. Maryland is excluded from gonorrhea and chlamydia samples and Maine from chlamydia samples due to missing 2021 data. Syphilis estimates exclude 2020 due to COVID-related reporting disruption. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5 — Robustness of the Gonorrhea Result to Post-Dobbs Medicaid Unwinding

	(1) Main	(2) + Medicaid share	(3) Drop ID, OK, TX
2023 ATT on ln(gonorrhea rate)	-0.233**	-0.251**	-0.216**
	(0.097)	(0.103)	(0.100)
Implied % change	-20.8%	-22.2%	-19.4%
Ban states	12	12	9
Protective states	24	24	24

Notes: This table reports the 2023 ATT of a total abortion ban on the natural log of the gonorrhea rate per 1,000 population aged 15–44, from SDID event studies estimated on 2019–2023 state-year data. Column (1) reproduces the main specification from Figure 2 without additional covariates. Column (2) applies the projected-covariate method of Clarke et al. (2024) to adjust for state-year adult Medicaid share: the outcome is first residualized on the covariate via a two-way fixed-effects regression estimated on pre-treatment (2019–2021) observations, and the SDID event study is then estimated on the residualized outcome. Adult Medicaid share is computed as the annual mean of monthly adult Medicaid enrollment reported by the Centers for Medicare & Medicaid Services through the Performance Indicator project, divided by the state population aged 19–64 from SEER (2025). Where adult enrollment is not directly reported by CMS (months prior to July 2024), it is derived from the identity that Medicaid + CHIP enrollment minus child enrollment equals adult enrollment, because CHIP eligibility is restricted to children. Column (3) reestimates the main specification excluding Idaho, Oklahoma, and Texas, the three ban states with the largest declines in adult Medicaid enrollment between the 2022 peak and the April 2023 to December 2024 trough. The implied percent change is computed as $100 \cdot (\exp(\text{ATT}) - 1)$. Inference uses placebo variance estimation with 500 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6 — Effect of Total Abortion Bans on Consumer Product Purchases: Units Sold

	Condoms	Emergency Contraception	Pregnancy Tests	Ovulation Tests	Athlete's Foot (Placebo)	Toothpaste (Placebo)
Panel A: Log units sold per capita, unbalanced panel (SDID)						
ATT	0.052***	0.005	0.050*	0.070	-0.026	-0.023
	(0.019)	(0.030)	(0.029)	(0.078)	(0.034)	(0.019)
Panel B: Log units sold per capita, balanced panel (SDID)						
ATT	0.036**	-0.025	0.034	0.123	-0.001	-0.028*
	(0.017)	(0.025)	(0.029)	(0.130)	(0.038)	(0.016)
Panel C: Log units sold per capita, unbalanced panel (OLS)						
ATT	0.078***	-0.004	0.039	0.115*	-0.009	-0.070***
	(0.022)	(0.031)	(0.027)	(0.066)	(0.027)	(0.021)
Panel D: Log units sold per capita, balanced panel (OLS)						
ATT	0.070***	-0.041**	0.032	0.186*	0.008	-0.064***
	(0.017)	(0.020)	(0.028)	(0.099)	(0.030)	(0.015)
Panel E: Log units sold per capita, unbalanced panel (weighted OLS)						
ATT	0.064***	-0.004	0.010	0.114***	0.037	-0.057***
	(0.016)	(0.030)	(0.024)	(0.043)	(0.045)	(0.018)
Panel F: Log units sold per capita, balanced panel (weighted OLS)						
ATT	0.054***	-0.039*	-0.006	0.192***	0.077**	-0.051**
	(0.015)	(0.022)	(0.032)	(0.073)	(0.033)	(0.020)
Panel G: Units sold, unbalanced panel (Poisson)						
ATT	0.061***	0.009	0.009	0.120***	0.053	-0.063***
	(0.015)	(0.021)	(0.023)	(0.041)	(0.047)	(0.018)
Panel H: Units sold, balanced panel (Poisson)						
ATT	0.050***	-0.037*	-0.010	0.201**	0.089**	-0.053***
	(0.013)	(0.021)	(0.031)	(0.079)	(0.036)	(0.020)

Notes: This table reports the effect of total abortion bans on Nielsen consumer product unit sales (volume) at the state-quarter level, 2020Q1–2024Q4. The treatment indicator equals one for states with a total abortion ban in effect following the Dobbs decision in June 2022. States with partial restrictions (hostile states) are excluded from the comparison group. Panels A–B use the synthetic difference-in-differences (SDID) estimator of Arkhangelsky et al. (2021), with inference based on placebo variance estimation using 500 replications. Panels C–F report two-way fixed effects (TWFE) difference-in-differences estimates with state and quarter fixed effects. Panels C–D use unweighted OLS; Panels E–F use OLS weighted by state population. In Panels A–F, the dependent variable is the natural log of per capita units sold, where the population denominator is ages 15–44. Panels G–H estimate Poisson models with raw unit counts as the outcome and population aged 15–44 as the exposure variable. Within each estimator, odd panels use the full unbalanced panel and even panels use a balanced panel of stores with non-missing sales data throughout the sample period. Standard errors in Panels C–H are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7 — Non-absorbing DiD sensitivity: Units Sold with Indiana, North Dakota, and Wisconsin reclassified as ban states

	Condoms	Emergency Contraception	Pregnancy Tests	Ovulation Tests	Athlete's Foot (Placebo)	Toothpaste (Placebo)
Panel A: Log units sold per capita, unbalanced panel (OLS)						
ATT	0.070***	0.011	0.044*	0.118**	-0.019	-0.065***
	(0.020)	(0.030)	(0.025)	(0.060)	(0.027)	(0.019)
Panel B: Log units sold per capita, balanced panel (OLS)						
ATT	0.062***	-0.031	0.039	0.177**	-0.008	-0.061***
	(0.016)	(0.020)	(0.026)	(0.086)	(0.030)	(0.013)
Panel C: Log units sold per capita, unbalanced panel (weighted OLS)						
ATT	0.059***	0.003	0.011	0.102**	0.031	-0.052***
	(0.016)	(0.027)	(0.022)	(0.041)	(0.042)	(0.017)
Panel D: Log units sold per capita, balanced panel (weighted OLS)						
ATT	0.049***	-0.037*	-0.004	0.178***	0.068**	-0.048***
	(0.013)	(0.021)	(0.029)	(0.062)	(0.031)	(0.018)
Panel E: Units sold, unbalanced panel (Poisson)						
ATT	0.056***	0.013	0.011	0.104***	0.043	-0.058***
	(0.015)	(0.019)	(0.021)	(0.037)	(0.043)	(0.017)
Panel F: Units sold, balanced panel (Poisson)						
ATT	0.044***	-0.036*	-0.007	0.190***	0.074**	-0.051***
	(0.012)	(0.020)	(0.027)	(0.064)	(0.036)	(0.018)

Notes: This table reports two-way fixed effects DiD estimates of the effect of total abortion bans on Nielsen consumer product unit sales (volume) at the state-quarter level, 2020Q1–2024Q4, using a non-absorbing definition of treatment. Indiana, North Dakota, and Wisconsin—reclassified as "hostile" and excluded from Table B.6 because their total bans did not remain in force for the full post-Dobbs period—are here reclassified as ban states. The treatment variable is E_{sq}, the fraction of state-quarter sq during which a total ban was in effect; the reported ATT is interpretable as the effect of a full quarter under ban. Other hostile states remain excluded from the comparison group. In Panels A–D, the dependent variable is the natural log of per capita units sold, where the population denominator is ages 15–44. Panels E–F estimate Poisson models with raw unit counts as the outcome and population aged 15–44 as the exposure variable. Within each estimator, odd panels use the full unbalanced panel and even panels use a balanced panel. SDID panels are omitted because SDID requires absorbing treatment. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8 — Effect of Total Abortion Bans on Consumer Product Purchases: Sales Revenue

	Condoms	Emergency Contraception	Pregnancy Tests	Ovulation Tests	Athlete's Foot (Placebo)	Toothpaste (Placebo)
Panel A: Log sales revenue per capita, unbalanced panel (SDID)						
ATT	0.053***	0.005	0.020	0.101**	-0.002	-0.015
	(0.018)	(0.029)	(0.020)	(0.047)	(0.016)	(0.012)
Panel B: Log sales revenue per capita, balanced panel (SDID)						
ATT	0.033**	-0.017	0.003	0.155**	-0.010	-0.022**
	(0.014)	(0.027)	(0.012)	(0.064)	(0.013)	(0.009)
Panel C: Log sales revenue per capita, unbalanced panel (OLS)						
ATT	0.059**	-0.017	0.038*	0.115**	-0.013	-0.016
	(0.026)	(0.028)	(0.020)	(0.052)	(0.022)	(0.014)
Panel D: Log sales revenue per capita, balanced panel (OLS)						
ATT	0.053***	-0.038**	0.027*	0.120***	-0.011	-0.017*
	(0.016)	(0.019)	(0.015)	(0.045)	(0.012)	(0.009)
Panel E: Log sales revenue per capita, unbalanced panel (weighted OLS)						
ATT	0.044*	-0.015	0.024	0.116***	-0.030**	-0.017*
	(0.023)	(0.026)	(0.015)	(0.035)	(0.015)	(0.009)
Panel F: Log sales revenue per capita, balanced panel (weighted OLS)						
ATT	0.034***	-0.037	0.003	0.104**	-0.027	-0.023**
	(0.013)	(0.024)	(0.026)	(0.049)	(0.020)	(0.010)

Notes: This table reports the effect of total abortion bans on Nielsen consumer product sales revenue at the state-quarter level, 2020Q1–2024Q4. The treatment indicator equals one for states with a total abortion ban in effect following the Dobbs decision in June 2022. States with partial restrictions (hostile states) are excluded from the comparison group. Panels A–B use the synthetic difference-in-differences (SDID) estimator of Arkhangelsky et al. (2021), with inference based on placebo variance estimation using 500 replications. Panels C–F report two-way fixed effects (TWFE) difference-in-differences estimates with state and quarter fixed effects. Panels C–D use unweighted OLS; Panels E–F use OLS weighted by state population. In Panels A–F, the dependent variable is the natural log of per capita sales revenue (in dollars), where the population denominator is ages 15–44. Within each estimator, odd panels use the full unbalanced panel and even panels use a balanced panel of stores with non-missing sales data throughout the sample period. Standard errors in Panels C–F are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9 — Non-absorbing DiD sensitivity: Sales Revenue with Indiana, North Dakota, and Wisconsin reclassified as ban states

	Condoms	Emergency Contraception	Pregnancy Tests	Ovulation Tests	Athlete's Foot (Placebo)	Toothpaste (Placebo)
Panel A: Log sales revenue per capita, unbalanced panel (OLS)						
ATT	0.054**	0.001	0.036**	0.100**	-0.007	-0.011
	(0.023)	(0.028)	(0.018)	(0.045)	(0.020)	(0.013)
Panel B: Log sales revenue per capita, balanced panel (OLS)						
ATT	0.047***	-0.029	0.023*	0.102***	-0.008	-0.012
	(0.014)	(0.019)	(0.013)	(0.040)	(0.011)	(0.008)
Panel C: Log sales revenue per capita, unbalanced panel (weighted OLS)						
ATT	0.041*	-0.007	0.024*	0.103***	-0.024*	-0.013
	(0.021)	(0.024)	(0.014)	(0.034)	(0.015)	(0.010)
Panel D: Log sales revenue per capita, balanced panel (weighted OLS)						
ATT	0.031***	-0.035	0.001	0.093**	-0.024	-0.018*
	(0.011)	(0.022)	(0.022)	(0.041)	(0.019)	(0.010)

Notes: This table reports two-way fixed effects DiD estimates of the effect of total abortion bans on Nielsen consumer product sales revenue at the state-quarter level, 2020Q1–2024Q4, using a non-absorbing definition of treatment. Indiana, North Dakota, and Wisconsin—reclassified as "hostile" and excluded from Table B.8 because their total bans did not remain in force for the full post-Dobbs period—are here reclassified as ban states. The treatment variable is E_{sq}, the fraction of state-quarter sq during which a total ban was in effect; the reported ATT is interpretable as the effect of a full quarter under ban. Other hostile states remain excluded from the comparison group. The dependent variable is the natural log of per capita sales revenue (in dollars). Within each estimator, odd panels use the full unbalanced panel and even panels use a balanced panel. SDID panels are omitted because SDID requires absorbing treatment. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10 — Robustness of the Condom-Purchase Result to Post-Dobbs Medicaid Unwinding

	(1) Main	(2) + Medicaid share	(3) Drop ID, OK, TX
Pooled ATT on ln(condom units)	0.052^{***}	0.051^{***}	0.057^{***}
	(0.020)	(0.019)	(0.018)
Implied % change	5.4%	5.3%	5.9%
Ban states	12	12	9
Protective states	23	23	23

Notes: This table reports the pooled SDID ATT of a total abortion ban on the natural log of condom units sold per 1,000 population aged 15–44 at the state-quarter level, 2020Q1–2024Q4, under three specifications. Column (1) reproduces the main specification from Figure 4 with quarter dummies as projected covariates. Column (2) adds state-year adult Medicaid share as an additional projected covariate (broadcast from annual values to all four quarters of the same calendar year). Adult Medicaid share is defined as the annual mean of monthly adult Medicaid enrollment from the CMS Performance Indicator file divided by the state population aged 19–64 from SEER (2025); see notes to Table B.5 for details. Column (3) reestimates the main specification excluding Idaho, Oklahoma, and Texas, the three ban states with the largest declines in adult Medicaid enrollment between the 2022 peak and the April 2023 to December 2024 trough. Pooled ATT estimates average the post-treatment quarters (2022Q3–2024Q4). The implied percent change is computed as $100 \cdot (\exp(\mathrm{ATT}) - 1)$. Inference uses placebo variance estimation with 500 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11 — Two-way fixed effects Poisson estimates without covariates

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	-0.108	-0.290***	0.043***	0.051**	-0.033*	-0.004
	(0.080)	(0.072)	(0.011)	(0.020)	(0.017)	(0.016)
Distance		0.086***		0.000		-0.011
		(0.031)		(0.011)		(0.010)
Distance^2		-0.004		-0.001		0.000
		(0.004)		(0.001)		(0.002)
Marginal effect	-10.2%	-10.1%**	4.4%***	4.6%***	-3.2%*	-3.1%**
of average ban	(7.2)	(4.9)	(1.2)	(1.2)	(1.7)	(1.6)
Observations	13,581	13,581	47,020	47,020	32,480	32,480
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	no	no	no	no	no	no

Notes: This table presents coefficients from Poisson models of county-level gonorrhea rates (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6). The population aged 15–44 is the exposure variable. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. The sample covers counties in the contiguous United States. All models include county fixed effects and an indicator for 6-week gestational age bans. Unlike Table 2, this specification omits all other controls: unemployment rates, poverty rates, the share of adults enrolled in Medicaid, and age-by-ethnicity population shares. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100 \times (\exp(\hat{\beta}_b) - 1)$ in odd columns and $100 \times (\exp(\hat{\beta}_b + \hat{\beta}_d(3 - 0.5) + \hat{\beta}_{dd}(3^2 - 0.5^2)) - 1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

Table B.12 — Two-way fixed effects Poisson estimates excluding hostile states, with demographic controls

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	0.027	-0.022	0.047***	0.073***	-0.045***	-0.027
	(0.048)	(0.059)	(0.014)	(0.016)	(0.016)	(0.019)
Distance		-0.001		-0.006		-0.009
		(0.027)		(0.007)		(0.008)
Distance^2		0.005		-0.001		0.000
		(0.004)		(0.001)		(0.001)
Marginal effect	2.7%	2.1%	4.8%***	4.6%***	-4.4%***	-4.6%***
of average ban	(4.9)	(4.6)	(1.4)	(1.2)	(1.5)	(1.4)
Observations	9,189	9,189	31,120	31,120	21,380	21,380
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	yes	yes	yes	yes	yes	yes

Notes: This table presents coefficients from Poisson models of county-level gonorrhea rates (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6). The population aged 15–44 is the exposure variable. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. Unlike Table 2, this specification restricts the sample to counties in ban and protected states, excluding the 14 hostile states, to match the sample used in the state-level SDID analyses. All models include county fixed effects, controls for unemployment rates, poverty rates, the share of adults (ages 19–64) enrolled in Medicaid, and detailed age-by-ethnicity population shares (each interacted with time fixed effects), and an indicator for 6-week gestational age bans. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100 \times (\exp(\hat{\beta}_b) - 1)$ in odd columns and $100 \times (\exp(\hat{\beta}_b + \hat{\beta}_d(3 - 0.5) + \hat{\beta}_{dd}(3^2 - 0.5^2)) - 1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

Table B.13 — Two-way fixed effects Poisson estimates excluding hostile states, without covariates

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	-0.109	-0.298***	0.054***	0.082***	-0.034	-0.001
	(0.082)	(0.082)	(0.015)	(0.021)	(0.022)	(0.022)
Distance		0.087**		-0.010		-0.015**
		(0.034)		(0.011)		(0.008)
Distance^2		-0.004		-0.000		0.001
		(0.004)		(0.001)		(0.001)
Marginal effect	-10.4%	-10.7%**	5.6%***	5.7%***	-3.3%	-3.2%*
of average ban	(7.3)	(5.2)	(1.5)	(1.4)	(2.1)	(1.9)
Observations	9,189	9,189	31,120	31,120	21,380	21,380
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	no	no	no	no	no	no

Notes: This table presents coefficients from Poisson models of county-level gonorrhea rates (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6). The population aged 15–44 is the exposure variable. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. Unlike Table 2, this specification restricts the sample to counties in ban and protected states, excluding the 14 hostile states, to match the sample used in the state-level SDID analyses, and also omits all other controls (unemployment rates, poverty rates, the share of adults enrolled in Medicaid, and age-by-ethnicity population shares). All models include county fixed effects and an indicator for 6-week gestational age bans. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100 \times (\exp(\hat{\beta}_b) - 1)$ in odd columns and $100 \times (\exp(\hat{\beta}_b + \hat{\beta}_d(3-0.5) + \hat{\beta}_{dd}(3^2 - 0.5^2)) - 1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

Table B.14 — Two-way fixed effects weighted OLS estimates with demographic controls

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	-0.085**	-0.141***	0.031**	0.047***	-0.037***	-0.028*
	(0.039)	(0.049)	(0.012)	(0.017)	(0.013)	(0.016)
Distance		0.007		-0.001		-0.004
		(0.023)		(0.009)		(0.010)
Distance^2		0.004		-0.002		-0.000
		(0.004)		(0.001)		(0.002)
Marginal effect	-8.1%**	-8.1%**	3.1%**	3.1%***	-3.6%***	-3.8%***
of average ban	(3.6)	(3.5)	(1.2)	(1.1)	(1.3)	(1.3)
Observations	13,134	13,134	46,769	46,769	32,424	32,424
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	yes	yes	yes	yes	yes	yes

Notes: This table presents coefficients from weighted OLS regressions of the log rate of county-level gonorrhea (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6), weighted by the population aged 15–44. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. The sample covers counties in the contiguous United States. Counties with zero counts are excluded because the dependent variable is undefined at zero. All models include county fixed effects, controls for unemployment rates, poverty rates, the share of adults (ages 19–64) enrolled in Medicaid, and detailed age-by-ethnicity population shares (each interacted with time fixed effects), and an indicator for 6-week gestational age bans. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100\times(\exp(\hat{\beta}_b)-1)$ in odd columns and $100\times(\exp(\hat{\beta}_b+\hat{\beta}_d(3-0.5)+\hat{\beta}_{dd}(3^2-0.5^2))-1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

Table B.15 — Two-way fixed effects weighted OLS estimates without covariates

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	-0.121	-0.351***	0.056***	0.073***	-0.033*	-0.004
	(0.103)	(0.061)	(0.012)	(0.022)	(0.019)	(0.017)
Distance		0.102***		-0.009		-0.013
		(0.021)		(0.013)		(0.011)
Distance^2		-0.003		0.000		0.000
		(0.003)		(0.001)		(0.002)
Marginal effect	-11.4%	-11.6%**	5.7%***	5.5%***	-3.3%*	-3.4%**
of average ban	(9.1)	(4.7)	(1.3)	(1.3)	(1.8)	(1.6)
Observations	13,134	13,134	46,769	46,769	32,424	32,424
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	no	no	no	no	no	no

Notes: This table presents coefficients from weighted OLS regressions of the log rate of county-level gonorrhea (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6), weighted by the population aged 15–44. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. The sample covers counties in the contiguous United States. Counties with zero counts are excluded because the dependent variable is undefined at zero. All models include county fixed effects and an indicator for 6-week gestational age bans. Unlike Table B.14, this specification omits all other controls: unemployment rates, poverty rates, the share of adults enrolled in Medicaid, and age-by-ethnicity population shares. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100 \times (\exp(\hat{\beta}_b) - 1)$ in odd columns and $100 \times (\exp(\hat{\beta}_b + \hat{\beta}_d(3 - 0.5) + \hat{\beta}_{dd}(3^2 - 0.5^2)) - 1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

Table B.16 — Two-way fixed effects weighted OLS estimates excluding hostile states, with demographic controls

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	-0.051	-0.123**	0.045***	0.072***	-0.047**	-0.029
	(0.053)	(0.056)	(0.015)	(0.018)	(0.017)	(0.019)
Distance		0.014		-0.007		-0.009
		(0.028)		(0.008)		(0.009)
Distance^2		0.005		-0.001		0.000
		(0.004)		(0.001)		(0.002)
Marginal effect	-4.9%	-4.6%	4.6%***	4.5%***	-4.6%***	-4.8%***
of average ban	(5.0)	(4.9)	(1.5)	(1.3)	(1.6)	(1.5)
Observations	8,874	8,874	30,962	30,962	21,344	21,344
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	yes	yes	yes	yes	yes	yes

Notes: This table presents coefficients from weighted OLS regressions of the log rate of county-level gonorrhea (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6), weighted by the population aged 15–44. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. Counties with zero counts are excluded because the dependent variable is undefined at zero. Unlike Table B.14, this specification restricts the sample to counties in ban and protected states, excluding the 14 hostile states, to match the sample used in the state-level SDID analyses. All models include county fixed effects, controls for unemployment rates, poverty rates, the share of adults (ages 19–64) enrolled in Medicaid, and detailed age-by-ethnicity population shares (each interacted with time fixed effects), and an indicator for 6-week gestational age bans. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100\times(\exp(\hat{\beta}_b)-1)$ in odd columns and $100\times(\exp(\hat{\beta}_b+\hat{\beta}_d(3-0.5)+\hat{\beta}_{dd}(3^2-0.5^2))-1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

Table B.17 — Two-way fixed effects weighted OLS estimates excluding hostile states, without covariates

	Gonorrhea		Condoms		Emergency contraception	
	(1)	(2)	(3)	(4)	(5)	(6)
Total ban	-0.116	-0.350***	0.066***	0.102***	-0.038	-0.002
	(0.101)	(0.068)	(0.015)	(0.024)	(0.024)	(0.021)
Distance		0.105***		-0.020		-0.018*
		(0.021)		(0.013)		(0.009)
Distance^2		-0.004		0.001		0.001
		(0.003)		(0.002)		(0.002)
Marginal effect	-10.9%	-11.7%**	6.8%***	6.5%***	-3.7%	-3.8%*
of average ban	(9.0)	(5.0)	(1.6)	(1.4)	(2.3)	(2.0)
Observations	8,874	8,874	30,962	30,962	21,344	21,344
County FE	yes	yes	yes	yes	yes	yes
Time FE	year	year	quarter	quarter	quarter	quarter
Controls	no	no	no	no	no	no

Notes: This table presents coefficients from weighted OLS regressions of the log rate of county-level gonorrhea (Columns 1–2), condom units sold (Columns 3–4), and emergency contraception units sold (Columns 5–6), weighted by the population aged 15–44. Gonorrhea models are estimated at the county-year level over 2019–2023; condom and EC models are estimated at the county-quarter level over 2020–2024. Counties with zero counts are excluded because the dependent variable is undefined at zero. Unlike Table B.14, this specification restricts the sample to counties in ban and protected states, excluding the 14 hostile states, to match the sample used in the state-level SDID analyses, and also omits all other controls (unemployment rates, poverty rates, the share of adults enrolled in Medicaid, and age-by-ethnicity population shares). All models include county fixed effects and an indicator for 6-week gestational age bans. Gonorrhea models include year fixed effects; condom and EC models include year-quarter fixed effects. Standard errors are clustered at the state level. The marginal effect of the average ban is calculated as $100 \times (\exp(\hat{\beta}_b) - 1)$ in odd columns and $100 \times (\exp(\hat{\beta}_b + \hat{\beta}_d(3 - 0.5) + \hat{\beta}_{dd}(3^2 - 0.5^2)) - 1)$ in even columns, where the latter evaluates the combined effect of a ban at the average post-ban distance increase (from 50 to 300 miles).

Appendix C: Decomposing the birth change into access and behavioral channels

This appendix extends the back-of-the-envelope exercise in Section 5 by combining our bounds on the pregnancy response with the 2.2% estimated increase in births (Dench et al., 2025) to back out an implied abortion change and to offer a tentative decomposition into access and behavioral channels. The exercise is instructive but rests on meaningful inferential leaps, and the bounds that follow should be read as plausibility checks rather than precise estimates.

The starting point is the accounting identity $B = P - A - M$: births equal pregnancies net of abortions and miscarriages. Assuming for simplicity that the ban leaves miscarriages unchanged ($\Delta M = 0$), taking changes, and dividing by baseline births gives

$$\frac{\Delta B}{B^0} = \frac{P^0}{B^0} \cdot \frac{\Delta P}{P^0} - \frac{A^0}{B^0} \cdot \frac{\Delta A}{A^0}. \tag{C-1}$$

The first inferential leap is that we do not observe the counterfactual ratios P^0/B^0 and A^0/B^0—what pregnancies, abortions, and births in the 12 total-ban states would have looked like in 2022–2024 had Dobbs not happened. We substitute the observed 2019–2020 pooled resident ratios from Kost et al. (2023): $P^0/B^0 = 1.36$ and $A^0/B^0 = 0.15$. These are imperfect stand-ins. The counterfactual ratios may have differed from their 2019–2020 values due to continuing trends in contraceptive use, COVID-era disruptions, pre-Dobbs changes in abortion access (notably Texas's SB 8), and secular demographic shifts. With that caveat, substituting these ratios and the observed $\Delta B/B^0 = 0.022$ into the identity gives

$$0.022 = 1.36 \cdot \frac{\Delta P}{P^0} - 0.15 \cdot \frac{\Delta A}{A^0}. \tag{C-2}$$

Combined with our bounds on the behavioral pregnancy response, $\Delta P/P^0 \in [-0.037, -0.002]$, this implies a relative abortion change of

$$\frac{\Delta A}{A^0} = \frac{1}{0.15}\left[1.36 \cdot \frac{\Delta P}{P^0} - 0.022\right] \in [-0.48,\ -0.16], \tag{C-3}$$

that is, something on the order of a 16% to 48% reduction in abortions among residents of ban states. This range depends on the pre-Dobbs-ratio substitution above but is otherwise independent of how the abortion change is partitioned between access and behavior; it follows from the identity, the observed birth response, and the two bounds on the pregnancy response alone.

To decompose the abortion change into access and behavioral components, let θ denote the fraction of women who become pregnant, wish to obtain an abortion, and are prevented from doing so by the ban—that is, those who are neither able to travel out of state nor able to obtain an abortion through mail-order medication or other means. Let α denote the share of behaviorally averted pregnancies that, absent the ban, would have ended in abortion. Then

$$\Delta A = \alpha \Delta P - \theta \left(A^0 + \alpha \Delta P \right) . \tag{C-4}$$

The first term is abortions averted by behavior: among the $|\Delta P|$ averted pregnancies, the α share that would have ended in abortion would no longer have occurred. The second term is abortions lost to the access channel, with the ban-induced prevention rate θ operating on the post-behavior pool of would-be abortion seekers, $A^0 + \alpha \Delta P = A^0 - \alpha |\Delta P|$.

The parameter α is not observed and must be bracketed. Under proportional scaling—behavior averts pregnancies uniformly across desired and unwanted categories—$\alpha = A^0/P^0 \approx 0.11$. One might reasonably wonder whether α could fall below this value: if women who would have aborted were already the most-motivated contraceptors pre-Dobbs, the marginal post-Dobbs adopter might be drawn disproportionately from the would-have-continued pool, yielding $\alpha < A^0/P^0$. We treat A^0/P^0 as a lower bound on the assumption that this selection is dominated by the opposite force: the ban imposes its sharpest *ex post* costs on women who would have sought abortion, giving them the strongest incentive to adjust their behavior in response. Under such selection α is higher, approaching 1 in the extreme case where behavior is drawn entirely from the would-have-aborted group.

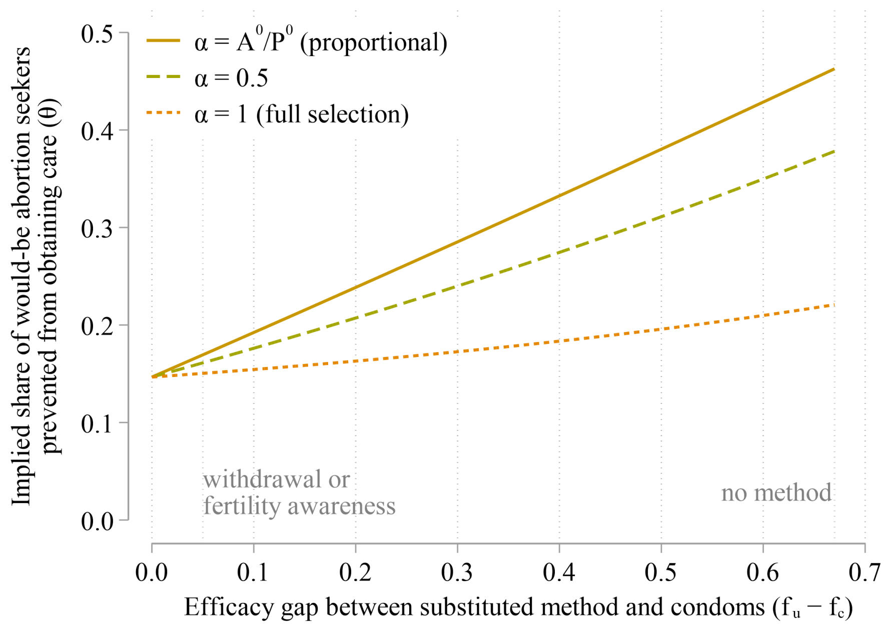
Figure C.1 plots the implied θ across the full continuum of efficacy gaps for three illustrative values of α: proportional scaling ($\alpha = A^0/P^0$) as a lower bound, full selection ($\alpha = 1$) as an upper bound, and $\alpha = 0.5$ as an intermediate case. The x-axis is the efficacy gap ($f_u - f_c$), which determines the implied pregnancy response.

Reference lines mark substitution from withdrawal or fertility awareness ($f_u - f_c \approx 0.05$) and from no method ($f_u - f_c = 0.67$).

Two features of the figure are worth emphasizing. First, at the low-gap end of the range (substitution from less-effective methods), the three lines converge near $\theta \approx 0.15$—selection barely moves θ when the pregnancy response is small. Second, at the no-method extreme, the lines fan out to $[0.22, 0.46]$—selection matters most when the pregnancy response is large. Our judgment about where on the figure the realized θ sits thus depends jointly on how much of the condom response reflects substitution from no method versus from less-effective methods, and on how concentrated the behavioral response is among would-have-aborted pregnancies.

Taken together, this is a plausibility check rather than a point estimate, but it suggests that our behavioral findings and the existing post-Dobbs fertility estimates are jointly consistent. A pregnancy response well below the 3.7% upper bound—as is likely once we allow for substitution from less-effective methods—combined with behavior plausibly concentrated among would-have-aborted pregnancies (α meaningfully above A^0/P^0) places the share of would-be abortion seekers prevented from obtaining care (θ) in the neighborhood of 15 to 20%—that is, roughly one in seven to one in five women who become pregnant and want an abortion in ban states are unable to obtain one. Under this anchoring, the behavioral channel accounts for roughly 15 to 35% of the implied decline in abortions, with the remaining 65 to 85% reflecting the access channel—women who become pregnant, want an abortion, and are prevented by the ban from obtaining one. Women who travel out of state or obtain medication abortion by mail continue to end their pregnancies and thus do not contribute to the abortion decline. The 2.2% birth increase documented by Dench et al. (2025) thus reflects primarily the access channel—unwanted births among women prevented from obtaining abortions—rather than the behavioral channel. The 5.4% increase in condom purchases should not be read as a one-for-one reduction in pregnancies at risk of being aborted; it is more consistent with a broad statewide shift toward more protected—or less frequent—sex.

Figure C.1 — Implied share of would-be abortion seekers prevented from obtaining care, by efficacy gap and selection regime



Notes: The x-axis shows the efficacy gap ($f_u - f_c$) between condoms and the method substituted away from, which determines the implied pregnancy response $\Delta P/P^0$. The y-axis shows the implied share θ of would-be abortion seekers prevented from obtaining care. α denotes the share of behaviorally averted pregnancies that, absent the ban, would have ended in abortion; its value is unobserved and plausibly varies continuously between A^0/P^0 (proportional scaling, lower bound) and 1 (full selection, upper bound). The three lines shown are illustrative values bracketing this range: $\alpha = A^0/P^0 \approx 0.11$, $\alpha = 0.5$, and $\alpha = 1$. Vertical reference lines mark substitution from withdrawal or fertility awareness ($f_u - f_c \approx 0.05$) and from no method ($f_u - f_c = 0.67$). Data inputs are pre-Dobbs 2019–2020 ratios for the 12 total-ban states: $P^0/B^0 = 1.36$ and $A^0/B^0 = 0.15$ from Kost et al. (2023); $\Delta B/B^0 = 0.022$ from Dench et al. (2025); $\pi_c = 0.084$ from Daniels and Abma (2020); $f_c = 0.18$ from Trussell (2011).