NBER WORKING PAPER SERIES

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Working Paper 33916
http://www.nber.org/papers/w33916

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2025

Dylan Black and Andrea Franco Hernandez provided excellent research assistance. Christine Durrance, Yang Wang, and Barbara Wolfe gratefully acknowledge funding from the large, anonymous, philanthropic family organization that supports the Collaborative for Reproductive Equity at the University of Wisconsin-Madison. The views and opinions expressed are those of the authors, and do not necessarily represent the views and opinions of the National Bureau of Economic Research.

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NBER Working Paper No. 33916
June 2025
JEL No. I11, I12, I18, J13, K23

ABSTRACT

In overturning Roe v. Wade and triggering laws in many states that ban or severely restrict abortions, the Supreme Court's landmark 2022 *Dobbs* decision dramatically altered the landscape for reproductive health in the U.S. Prior research has highlighted the far-reaching impact of abortion restrictions for women and families, which extend beyond their proximate effects on abortions, births, and fertility. We provide some of the first causal evidence on how abortion restrictions in the post-*Dobbs* era have impacted women's risk of exposure to intimate partner violence (IPV). IPV is the most common form of violence experienced by women, and changes in access to abortion may generate unintended effects on various inputs (economic resources, stress, intra-household bargaining) that could affect relationship dynamics and raise the risk of IPV. Leveraging information on IPV incidents reported to law enforcement from 2017-2023 combined with post-*Dobbs* changes in county-level travel distance to abortion facilities, analyses are based on a generalized difference-in-differences approach. We find that abortion restrictions – alternately measured by the increase in travel distance and by the presence of a near-total ban – significantly increased the rate of IPV for reproductive-aged women in treated counties on the order of about seven to 10 percent. These estimates imply at least 9,000 additional incidents of IPV among women in the treated "trigger ban" states, which would be predicted to add over $1.24 billion in social costs.

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1 Introduction

The landscape of reproductive health care in the United States underwent a profound transformation following the Supreme Court's decision in *Dobbs v. Jackson Women's Health Organization* (2022), which overturned *Roe v. Wade* (1973) and removed federal protections for abortion rights. This landmark ruling activated trigger bans in 14 states and ushered in a wave of new restrictions, including gestational age limits ranging from 6 to 18 weeks in seven states. In many cases, these restrictions forced women to travel hundreds of miles to access abortion care, substantially increasing the time, cost, and logistical complexity of obtaining services (Myers et al. 2025). Prior research has shown that long travel distances are a major barrier to care, particularly for those with limited financial resources (Fischer et al. 2018; Lindo and Pineda-Torres 2021; Venator and Fletcher 2021; Myers 2024a; Myers et al. 2025). Although the national number of abortions increased in the year following *Dobbs*—driven in part by expanded access to medication abortion via telehealth and the growing role of abortion funds and shield laws (Maddow-Zimet and Gibson 2024; Society of Family Planning 2024)—recent studies documented that states enacting abortion bans experienced an average 2.3% increase in births relative to states that did not restrict abortion access post-*Dobbs* (Dench et al. 2024) and these effects are more pronounced in locations with greater increases in travel distance to nearest abortion clinics (Myers et al. 2025).

While the direct reproductive consequences of these policies are well-documented, less attention has been paid to their broader social and interpersonal impacts. Given recent policy changes, there is a pressing need to explore the potential influence of abortion policy on women's risk of intimate partner violence (IPV) victimization in the post-*Dobbs* landscape. IPV is the most common form of violence experienced by women, imposing adverse consequences for the physical and mental health of victims and their children. According to the National Intimate Partner and Sexual Violence Survey (NISVS), 6.6% of women report experiencing IPV in the past 12 months, with a lifetime prevalence of 37% (Smith et al. 2017). Over half of all female homicide victims are murdered by a current or former intimate partner (Ertl 2019), and the total lifetime health and economic costs of IPV in the U.S. exceed $4.1 trillion (in 2021 dollars), including $1.5 trillion in lost productivity and earnings (Peterson et al. 2018).

State policy surrounding access to abortion may have unintended effects on the family environment and various forms of stress, that could result in IPV. First, restrictions could lead to substantial financial strains, including increased costs of transportation, time away from work, and childcare, which could delay or prevent individuals from accessing care. Carrying a pregnancy, having a baby, and raising a child is costly. Additional financial pressures may serve to exacerbate the potential for IPV (Benson and Fox 2002). Those denied an abortion had higher rates of poverty, less resources to provide for existing children at home, and lower credit scores (Foster

et al. 2018; Roberts et al. 2014; Miller et al. 2023). Second, restrictions in access could affect health outcomes, which may alter relationship quality and the potential risk for IPV (Herd et al. 2016). Adverse (physical or mental) health consequences could be experienced with an unintended or high-risk pregnancy (Herd et al. 2016). Finally, abortion restrictions could lead to changes in intra-household power dynamics, increasing the risk of IPV, either through reproductive coercion (Miller et al. 2010) or by binding women to violent partners (Roberts et al. 2014). Evidence from the Turnaway Study illustrated that those denied an abortion (relative to those who received an abortion just prior to a facility's gestational age limit) were more likely to experience IPV (Roberts et al. 2014). Recent evidence also shows that abortion restrictions, including mandatory waiting periods (MWPs) for abortion (Durrance et al. 2024) and TRAP laws (Muratori 2025), are associated with increases in IPV. An important point that follows from this literature is that abortion restrictions need not affect pregnancy resolution in order to impact risk of IPV exposure; pathways operating through delays, financial strains, stress, and prolonged engagement with violent partners, for instance, can all affect relationship quality and increase the risk of interpersonal violence, even if an abortion is obtained.

In this study, we provide the first causal evidence on how policies regulating abortion access post-*Dobbs* have impacted women's risk of exposure to IPV. We combine data on reported IPV incidents to law enforcement agencies from the National Incident-Based Reporting System (NIBRS) from 2017 to 2023 with a newly developed dataset on travel distances to the closest abortion facility, compiled by Myers (2024b). We supplement this variation with state policy decisions around *Dobbs* (Dench et al. 2024). The *Dobbs* decision resulted in immediate trigger bans in 14 states, discretely increasing travel distances to the nearest clinic in those states. Several other states passed total bans after that. Using these changes in county-level travel distances to the nearest abortion facility, we implement a continuous difference-in-differences (DID) research design to measure the effects of driving distance to the nearest abortion facility on IPV rates, comparing exposed counties where travel distance increased to counties with low (or no) increases in travel distance between 2017 and 2023. We complement this analysis with a synthetic difference-in-differences (SDID) approach, that is robust to potential biases from spatial and dynamic heterogeneity, by leveraging the adoption of state abortion bans following *Dobbs* as a dichotomous treatment. We show that our results are robust to controlling for time-varying demographic and socioeconomic characteristics at the county level, and time-varying state policies, and proxies for time-varying spatial heterogeneity.

Our findings consistently show that abortion restrictions (both increases in the distance traveled to the nearest abortion facility and state abortion bans) increase the rate of exposure to IPV. This evidence is consistent with recent work showing that travel distance plays an important barrier to abortion care in the post-*Dobbs* period despite expanded access through telehealth and travel

assistance (Myers et al. 2025). We find that abortion restrictions – alternately measured by increase in travel distance and by the presence of a near-total ban – significantly increased the rate of IPV for reproductive-aged women in treated counties on the order of about seven to 10 percent. Specifically, our SDID event study estimates suggest an increase in the IPV rate by approximately 6.1–7.5 percent. Given that the average resident in a treated "trigger ban" state experienced an increase of 241 miles in distance to the nearest abortion provider after *Dobbs*, our distance-based estimates suggest treatment dose is associated with approximately 10 percent rise in the IPV rate in these states on average. These estimates imply at least 9,000 additional incidents of IPV among women in the treated "trigger ban" states, which would be predicted to add over $1.24 billion in social costs.

Our study contributes to a growing body of research examining how reproductive rights policies affect broader social and economic dynamics beyond their immediate effects on fertility and health.[1] A number of empirical studies have demonstrated that access to abortion and contraception has significant consequences for women's educational and labor market outcomes (Goldin and Katz 2002; Bailey 2006; Jones and Pineda-Torres 2024) as well as their access to credit markets and financial outcomes (Foster et al. 2022; Miller et al. 2023). Related studies found substantial effects of these policies on children's well-being, life circumstances, and the likelihood of exposure to child maltreatment (Ananat and Hungerman 2012; Foster et al. 2018; Bailey et al. 2019; Durrance et al. 2025; Aslim et al. 2024). Our paper is also closely related to prior studies that examined the effects of mandatory waiting periods on IPV in the US and the effects of improved access to abortion on IPV in Mexico (Durrance et al. 2024; Garcia-Ramos and Pineda-Torres 2023). Building on this literature, our study provides new evidence on how abortion restrictions following the *Dobbs* decision affect women's risk of IPV. By examining the effects of increased travel distance to abortion facilities on IPV exposure, we extend the scope of prior work by highlighting a critical, yet underexplored, interpersonal dimension of reproductive policy: its capacity to affect women's vulnerability to violence through economic stress, deteriorating health, and constrained bargaining power in relationships.

Furthermore, our study relates to the extended literature on the public policy changes that affect the prevalence of IPV, ranging from mandatory arrest laws (Iyengar 2009; Chin and Cunningham 2019), unilateral divorce laws (Stevenson and Wolfers 2006), reformulation of prescription opioids and introduction of prescription drug monitoring programs (Dave et al. 2023, 2024), conditional cash transfers (Bobonis et al. 2013), educational reforms (Erten and Keskin 2018), and other policy changes. Our study contributes to this literature by examining the effects of more stringent abortion

[1] For a review of this literature, see Bailey and Lindo (2018). Myers et al. (2025) provide evidence on the effects of the most recent abortion restrictions on fertility in the post-*Dobbs* era. Related recent studies include Lu and Slusky (2019); Fischer et al. (2018); Lindo et al. (2020); Dench et al. (2024); Flynn (2024).

restrictions introduced in the *post-Dobbs* era on the prevalence of IPV.

The remainder of this article is organized as follows. Section 2 discusses the data. Section 3 presents the identification strategy we employ in our analysis. Section 4 presents our findings and robustness checks, and Section 5 concludes the paper.

2 Data

2.1 Intimate Partner Violence

We use data from the National Incident-Based Reporting System (NIBRS), a law enforcement-administered system of crime data collection maintained by the Federal Bureau of Investigation (FBI). NIBRS provides detailed, incident-level information on crimes reported to police across the United States, including the characteristics of the victim (e.g., age, gender, race, ethnicity, and relationship to the offender), the offender (e.g., age, gender, race, ethnicity), and the incident itself (e.g., date/time, type of offense, injuries, and arrests). NIBRS represents a substantial improvement over traditional survey-based measures of crime, particularly for IPV, as it is less susceptible to underreporting due to recall or social desirability biases. It also enables the systematic identification of IPV cases based on both the nature of the offense (e.g., simple and aggravated assault, intimidation, and sexual assault) and the reported relationship between the victim and perpetrator (e.g., spouses, ex-spouses, boyfriends/girlfriends, common-law partners, and same-sex partners). Nonetheless, it is important to recognize that many crimes, including IPV, are never reported to law enforcement, and even when reported, not all are documented as IPV. Consequently, IPV rates derived from NIBRS data likely understate the true extent of IPV.

Although reporting of agencies to NIBRS is voluntary and not all agencies participate, coverage has expanded substantially over time, with 79% of U.S. law enforcement agencies across 50 states and District of Columbia participating as of 2023. In order to construct a consistent panel of counties observed both before and after state abortion restrictions, we use a fully balanced sample at the county level from 2017-2023 (as done in prior studies (Dave et al. 2023; Durrance et al. 2024).[2]

We focus on IPV experienced by female victims of reproductive age (15-44), including relationships that consist of spouses, common-law spouses, boyfriends/girlfriends, homosexual partners, ex-spouses, and ex-boyfriends/girlfriends. The incidents considered are aggravated assaults, simple assaults, sexual assault, and intimidation. Our primary measure is the annual IPV rate per 10,000 reproductive-age female population at the county level. When we examine results by

[2]Our sample accounts for about 40 percent of counties in the US, covering over 30 percent of the U.S. population. In robustness checks, presented in Section 4, we show that our estimates are not sensitive to alternate sample restrictions to address any residual measurement errors and/or inconsistent reporting.

victim characteristics (e.g., age, race), we adjust the denominators accordingly to reflect the population at risk using using population data from the Surveillance, Epidemiology, and End Results (SEER) program. In cases where population denominators are unavailable, we retain the broader reproductive-age female population as the denominator. Our additional outcome measures the injury rate per 10,000 reproductive age women, which is the count of injuries resulting from an IPV incident at the county level divided by the reproductive age female population at the county; and the arrest rate per 10,000 reproductive age women, which is the count of arrests due to an IPV incident at the county level divided by the reproductive age female population at the county.

2.2 Travel Distance to the Nearest Abortion Facility

We use travel distance data from the Myers Abortion Facility Database (Myers 2024b), a novel and comprehensive dataset that tracks the availability of abortion services across the United States. This database draws on a wide array of sources, including state licensing records, historical and current abortion facility websites, media reports, Planned Parenthood directories, and National Abortion Federation (NAF) listings. It aims to comprehensively identify abortion facility openings, closures, relocations, and service suspensions, covering all providers—such as private physician offices, hospitals, and clinics—operating in the continental U.S. since 2009 (Myers et al. 2025).

The data include monthly driving distances from the population-weighted centroid of each county to the nearest operating abortion facility, measured as of the first day of each month, resulting in a county-by-month panel covering the period from January 2009 through October 2024, which continues to be updated as new data become available. Figure 1 shows a map of the US identifying maximum distance changes over our panel 2017-2023, where counties included in the NIBRS are identified and coded based on their distance changes. Red shaded counties indicate increases in distance, while blue shaded counties indicate decreases in distance. Grey shaded counties are those which are identifiable in the NIBRS but did not experience any distance changes. White areas are counties/states that did not report to NIBRS during our data period. Some areas in the western US (Oregon, Idaho), the south (Texas, Oklahoma, Louisiana, Arkansas, and Tennessee), and Midwest (Wisconsin) experienced relatively large increases in distance.

For our analysis, we restrict the sample to the period 2017 to 2023 to ensure compatibility with the NIBRS dataset. We then aggregate the monthly data to create county-by-half-year averages of travel distance to the nearest abortion provider. We then merge the travel distance data with the NIBRS data using the county identifier.

2.3 Control Variables

We use multiple sources of data to account for time-varying county characteristics and state policies in our analysis. First, we incorporate demographic measures, including racial, ethnic, and age composition of reproductive-age women, from the SEER Program. Second, we include annual average unemployment rates reported by the Bureau of Labor Statistics, and official poverty rate data reported by the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) program to account for socioeconomic conditions at the county level. Third, we add initial educational attainment and rural shares interacted with half-year fixed effects to account for differences in initial gender norms and abortion access. Specifically, we include the initial (2017) shares of the population with more than a high school education and the share of rural population from the SEER Program, both interacted with half-year fixed effects. Finally, we incorporate state-level policy changes and indicators. The data on minimum wage, maximum welfare benefits for three-person families, and state Medicaid enrollment rate come from the UKCPR National Welfare Data (University of Kentucky Center for Poverty Research 2023).

3 Empirical Strategy

We leverage changes in the distance to the nearest abortion facility across counties and over time using a difference-in-differences (DID) framework. Specifically, we aggregate the data to a county-half-year panel and implement continuous difference-in-difference (DID) and event study (ES) research designs using the spatial and temporal variation in abortion access proxied by travel distance to the nearest abortion provider. We estimate the following two-way fixed effects (TWFE) specification:

$$Y_{c,t} = \beta_0 + \beta_1 distance_{c,t} + \beta_2 \mathbf{X_{c,t}} + \sum_t \theta_t 1\{half\text{-}year = t\} \times Z_c + \delta_c + \delta_t + \varepsilon_{c,t}, \tag{1}$$

where $Y_{c,t}$ is the rate of IPV, defined as the number of reported IPV incidents per 10,000 (respective) capita of reproductive age females in county c in half-year t (or variants of this outcome). The treatment variable, $distance_{c,t}$, represents the travel distance (in 100s of miles) to the nearest abortion facility in county c at time t. The travel distance serves as a proxy for access to abortion services, with longer distances representing stronger barriers against abortion access.

The vector $\mathbf{X_{c,t}}$ includes time-varying county demographic and socioeconomic characteristics and the state policies. The county characteristics include the shares of women aged 15-44 categorized by race/ethnicity and five-year age groupings, unemployment rate, and poverty rate. The state controls include the minimum wage, the maximum welfare benefits for a three-person house-

hold, and state Medicaid enrollment rate. Initial county characteristics Z_c include the initial (2017) share of the population with more than a high school education and the share of the population that is rural. We interact them with half-year fixed effects to proxy for time-varying spatial heterogenetiy and flexibly control for county features that can affect the evolution of our outcome variable differentially over the sample period.

All models include county fixed effects, δ_c, to control for unobserved, time-invariant county characteristics that may influence IPV rates, and half-year fixed effects, δ_t, to account for nationwide shocks or trends that affect all counties uniformly over time. We cluster standard errors at the county level to account for serial correlation in errors within counties, and weight all regressions with initial (2017) county female population between the ages of 15 and 44.

Because the bulk of the distance changes occur following the *Dobbs* decision in June 2022, we also directly model the reduced-form effect of a near-total abortion ban, that is a dichotomous treatment indicator which takes on a value of one for all sates with trigger bans as of June 2022. The only exception is Texas, which was partially treated in the pre-period due to the implementation of SB8 in September 2021, and is therefore coded as being treated as of the second half of 2021. SB8, or the Texas Heartbeat Act, prohibited all abortions in the State of Texas after fetal cardiac activity. Appendix Table A1 lists this classification as Approach 1. We also provide an alternative classification using Approach 2 (which further captures state bans—total and gestational—that occurred after June 2022) as provided in Appendix Table A1. The results are robust to these different treatment definitions.

We implement a synthetic difference-in-differences (SDID) approach to estimate the average causal effect of the abortion bans on IPV rates (outcome Y_{it} in county i at time period t) by obtaining:

$$\left(\hat{\tau}^{SDID}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{n} \sum_{t=1}^{t} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{SDID} \hat{\lambda}_t^{SDID} \right\}$$

where W_{it} denotes the abortion ban, and the causal effect of this treatment is estimated from a fixed effects model with optimally chosen weights $\hat{\omega}_{ij}^{SDID}$ and $\hat{\lambda}_t^{SDID}$. As in standard DID estimation, the time fixed effects (β_t) control for all unmeasured shared temporal shocks and the county fixed effects (α_i) control for all time invariant country-specific confounding factors. In contrast to the standard DID estimation, however, SDID more flexibly reweights and matches pre-treatment trends by selecting a weighted set of control units that minimizes the trend differences in the pre-exposure periods. Specifically, optimal unit-specific weights $\hat{\omega}_i^{SDID}$ are chosen to align pre-treatment trends across outcomes in the control vs. treated units, subject to a regularization parameter which prevents overfitting while increasing the variance and uniqueness of the weights. These features improve the robustness and precision of the SDID estimator, in addition to making the model more flexible in generating credible counterfactual comparisons (Arkhangelsky et al.

2021). For statistical inference, we rely on block bootstrap methods. To estimate SDID event-study coefficients with confidence intervals, we follow Clarke et al. (2024). Specifically, we compute the difference between treated and control groups in each period, relative to the average difference in the time-weighted pre-treatment period. We use bootstrap inference to construct 95 percent confidence intervals and present half-yearly event-study estimates.

4 Results

Summary statistics are presented in Table 1. The mean IPV rate over the full analysis period was 53.2 reported incidents per 10,000 women of reproductive age, with about 56 percent of these incidents resulting in an arrest and over 55 percent resulting in some injury. Exposure is not uniform across all sub-populations. The IPV rate exhibits a strong gradient vis-à-vis age, increasing with age through the mid-30s and then declining; the rate is highest for women between the ages of 25-34 (approximately 70 incidents per 10,000 women). Exposure to IPV is highest for non-Hispanic Blacks (73.4) followed by non-Hispanic Whites (43.6) and lowest for Hispanics (32.0). Table 1 also reports the average driving distance to the nearest abortion provider based on the county centroid, which shows a sharp increase nationally on average from 65.48 miles to 123.5 miles post-*Dobbs*. Among the counties in the states that were enforcing near-total abortion bans with very limited exceptions ("ban states"), the increase in travel distance post-*Dobbs* was 241 miles.

The main findings are reported in Tables 2 through 7 and Figure 2. Supplemental analyses and robustness checks are presented in the Appendix.

Table 2 presents estimates of the effects of travel distance on women's exposure to intimate partner violence, as captured by the IPV rate, based on equation 1.[3] We start with a parsimonious specification that includes only the county and period fixed effects, and progressively add time-varying controls to assess the importance of these controls and gauge the sensitivity of our estimates to concurrent economic and policy shifts that may also have impacted IPV. Estimates in column (1) suggest that an increase in travel distance by 100 miles results in a statistically significant increase in the IPV rate by 4.12 incidents per 10,000 reproductive age women per half year. Although adding time-varying county-level economic and sociodemographic controls (column 2) somewhat moderates the estimated effect by about 30 percent, we continue to find a statistically significant increase in IPV on the order of 2.86 incidents per 10,000 reproductive age women. The next two columns, respectively, include interactions between baseline county characteristics and period fixed effects (column 3), as proxies for unmeasured confounding trends across local areas, and include state-level controls (column 4). These extensions do little to alter the estimated

[3]We also considered quadratic and non-parametric binned specifications; however, our findings mostly identify linear distance effects, and as such, we present linear specifications in what follows.

effects, indicating that longer travel distances raise exposure to IPV by between 2.36 and 2.90 (approximately 4.4 to 5.5 percent of the outcome mean).

We note that these effects represent a reduced-form effect operating on the net through all reinforcing and counteracting mechanisms discussed earlier. These effects should also be interpreted as intention-to-treat (ITT), since not all women of reproductive age are affected by abortion restrictions. To frame the spillover effects of the post-*Dobbs* increase in travel distance on IPV exposure, and gauge the plausibility of the effect magnitudes, we can combine them with the "first stage" effects on abortions and births from the literature to impute a back-of-the-envelope version of the treatment-on-the-treated (TOT) effect. Myers (2024a) estimated that an increase in distance by 100 miles reduces surveilled abortions by about 19 percent;[4] births are predicted to increase by between 1.8 and 2.2 percent as a result of a ban that causes a corresponding increase in distance (Myers 2024a; Myers et al. 2025). These estimates suggest an increase of approximately 18,170 to 22,208 births (associated with a 100-mile increase in average travel distance post-*Dobbs*). Estimates from Table 2 (columns 3 and 4) suggest an increase in IPV-related incidents between 3,999 and 4,902 among the ban states, for a corresponding increase in travel distance. Combining these estimates implies a TOT of about 0.18 to 0.27. In other words, about one out of every four to five women, who on the margin carried their pregnancy to term because of abortion restrictions and the increase in travel distance, was exposed to IPV. These estimates should be interpreted with caution and are meant to be suggestive since TOT estimates rescaled in this manner can be sensitive to relatively small changes in the underlying "first stage" estimates. Moreover, they are also very likely an overestimate since they load all of the mechanisms through which abortion restrictions may affect IPV on women who could not obtain an abortion and carried their pregnancy to term. Note that, as discussed earlier, abortion restrictions can impact a woman's exposure to IPV, conditional on births and abortions. That is, even if a woman ultimately obtained an abortion, economic, financial, and mental or physical health-related stressors triggered by any delays in accessing services or navigating the restrictions may independently increase the risk of IPV. To place these estimated "marginal" impacts (of carrying an unintended pregnancy to term on women's exposure to IPV) in context, estimates of IPV prevalence among women who gave births is approximately six percent (D'Angelo et al. 2023), and approximately seven to 11 percent of abortion patients and pregnant women with an unintended pregnancy report being a victim of IPV (Marshall et al. 2011; Saftlas et al. 2010; Woo et al. 2005).

In the remaining panels in Table 2, we present the findings for IPV incidents that involve an injury (injury rate) and lead to an arrest (arrest rate) as a share of reproductive age women, and consistently find that abortion restrictions that increase distance also increase the prevalence of

[4]This decline may be overstated since surveilled abortions generally do not include medication abortions from states where such telehealth is restricted (Myers et al. 2025).

both types of IPV incidents, by between 1.64 and 1.98 incidents involving an injury (5.6 – 6.8 percent) and by between 1.09 and 1.39 incidents involving an arrest (3.7 – 4.7 percent). These corollary increases in arrest and injury rates add a degree of confidence that the overall increase in IPV incidents that we find is not an artifact of increased reporting behavior of victims to law enforcement. Moreover, since victims may be more likely to report more serious IPV incidents that involve an injury or ultimately result in an arrest, these outcomes may also help to smooth out variation in reporting behaviors across women, sub-populations, and areas.

Figure 2 visually presents estimates from the event study analysis, based on the synthetic difference-in-differences (SDID) approach. The SDID approach is robust to potential bias from heterogeneity in the treatment effects across treated units and over time. With the closure of clinics providing abortion and reproductive health services as a result of the post-*Dobbs* abortion bans, in the models presented above we had parameterized the shift in access through changes in the driving distance to the nearest provider. Distance has been found to be a significant barrier for women seeking abortions (Myers et al. 2025). The event study analysis models the reduced-form DiD effects of the abortion bans, capturing any and all pathways including but not limited to the increase in distance. These analyses help assess the common trends assumption underlying the identification strategy while also exploring dynamic effects, and highlight three points. First, the estimates in the pre-*Dobbs* period do not indicate any significant deviations in outcome trends between counties across the treated and control states; all of the pre-treatment estimates are close to zero and statistically insignificant. Second, any discernible increase in IPV materializes only subsequent to the treatment. These coefficient patterns add a degree of confidence underlying the credibility of the counterfactuals employed.[5] Third, the estimated average treatment effect of the bans on the treated (ATT) counties in Figures 2 and A1 suggest an increase in the IPV rate of between 3.2 and 3.9 incidents (6.1 - 7.5 percent, relative to the sample means of 52.2 and 52.1 leaving out excluded states in Table A1), in line with the estimates presented in Table 2 based on distance as the continuous treatment measure. Multiplying the IPV estimate (Table 2 model 4) with the mean treatment dose (the average resident in a "treated" ban state experienced an increase of 241 miles in distance to the nearest abortion provider post-*Dobbs*) reassuringly yields an average treatment effect of an increase in 5.69 IPV incidents (about 10 percent relative to the mean), in the ballpark of the SDID-based ATT estimates.[6]

In Tables 3 and 4, we assess heterogeneity in our main effects across victim characteristics.

[5]Appendix Figure A1 shows that the event study estimates are robust to the alternative classification of bans (approach 2), and similar patterns for the event study analyses of IPV incidents that involve an injury (reported in Appendix Figures A2 and A3).

[6]We also find that the abortion bans increased IPV incidents involving an injury by between 2.0 and 2.4 incidents, and an increase in incidents involving an arrest by between 1.4 and 2.0 incidents. All of the SDID-based estimates of the ATT are statistically significant at 5% level.

Exposure to IPV exhibits a concave relationship with age, increasing through the late 20's and mid 30's and then declining (Table 3), a pattern that reflects variation in fertility, union formation and relationship risk factors, as well as women's earnings capacity and economic independence. Coefficient estimates of the treatment effect across age groups largely mirror this pattern as well (Table 3). We find that increased distance to an abortion provider significantly raises the risk of IPV exposure for women of all ages; these effects, in an absolute sense, are strongest (2.94 to 3.29 incidents) for women between the ages of 25-29 and 30-34 and then monotonically smaller for younger and older age groups. Relative to the sub-group means, however, the largest impacts are realized for teens (5.2 percent) and for older women ages 40-44 (5.0 percent).

We further consider heterogeneity across race/ethnicity (Table 4) given pre-existing racial/ethnic differences in fertility, abortion, and IPV prevalence. Reporting behavior with respect to IPV also varies significantly by race/ethnicity, partly driven by cultural and community norms; conditional on experiencing IPV, Black women, for instance, have been found to be more likely to report an IPV incident to police, particularly when the incident involves an injury and Hispanic women are somewhat less likely, possibly due to immigration concerns and linguistic barriers (Holliday et al. 2020; Erten et al. 2022). Estimates indicate that abortion restrictions have some of the largest (imprecisely estimated) impacts on IPV incidents among non-Hispanic Black women (2.43 incidents per 10,000 women of reproductive age; 3.3 percent relative to the mean). This is consistent with non-Hispanic Black women having the highest abortion rate (approximately 38-40 percent of surveilled abortions), implying that more women of reproductive age in this sub-population are "treated." Moreover, the "first-stage" response of more restrictive abortion policies on births has also been found to be larger for non-Hispanic Black women (Myers et al. 2025), which plausibly translates into stronger downstream effects on IPV risk. For non-Hispanic White and Hispanic women, point estimates indicate an increase in IPV exposure on the order of about 1.17 and 1.15 incidents (2.7 percent and 3.6 percent), respectively. Differentials across race-ethnic subpopulations are imprecisely estimated, and we are not able to reject the null of equal-sized treatment effects. We, therefore, view these patterns as suggestive, and relegate more definitive conclusions regarding heterogeneity and the underlying drivers for future research.

One limitation of the NIBRS is that it does not contain socioeconomic information on victims (or perpetrators). To explore heterogeneity along these dimensions, we turn to area-level variation in education and poverty. Results reported in Table 5 indicate larger effects in areas with a greater share of disadvantaged individuals, measured by educational attainment and poverty status. This is in line with prior research pointing to abortion restrictions being more salient and binding for less-educated and more disadvantaged women, and eliciting a stronger fertility response among these women. Moreover, IPV risk (both in terms of victimization and perpetration) is higher among less-educated and more economically disadvantaged populations. Next, we assess the differential

effects between urban and rural areas. While rates of IPV are generally similar across rural, urban and suburban areas, some evidence points to IPV perpetration in rural locales as being more chronic and severe, possibly driven by higher rates of unemployment and substance use (Edwards 2014); victim reports also tend to differ, with women in urban areas more likely to report IPV. Results in Table 5 show a substantially larger response magnitude among urban counties (2.35 more incidents; 4.1 percent) compared to rural counties where the point estimate amounts to a one percent increase in IPV which is not statistically distinguishable from zero.

The results thus far consistently indicate that the overall risk of IPV exposure increased significantly for women of reproductive age in relation to the increased distance to abortion providers in the post-*Dobbs* era. In general, administrative reports of IPV to law enforcement reflect more severe IPV as evidenced by the fact that over 54 percent of victim-reported IPV incidents to police involve some injury (Table 1). In order to further inform whether abortion restrictions impacted relatively more vs. less severe forms of IPV, in Table 6 we specifically disaggregate effects across the main offense categories. These results suggest a substantially larger impact on IPV incidents involving simple assault (1.83 incidents; 4.9 percent), a category which constitutes the majority of reported IPV incidents; effects are somewhat more moderated though still significant for more severe IPV incidents involving aggravated assault (3.0 percent) and sexual violence (4.6 percent). We note that the latter component is relatively rare and constitutes only two to three percent of reported IPV incidents.

Finally, in the results reported in Table 7, we undertake various supplementary analyses to gauge the robustness of our findings to: 1) additional controls; 2) sample restrictions; 3) alternative standard error adjustment; 4) incorporating the impacts of the COVID-19 public health crisis; and 5) alternate functional forms. Notably, our results are not sensitive to adding further controls for Medicaid expansions under the Affordable Care Act (ACA) and state Earned Income Tax Credit (EITC). In our main analyses, we adjusted standard errors by clustering at the county level – the level of variation in our treatment; statistical inferences remain robust to alternately clustering standard errors at the state level. While agency coverage in the NIBRS improved substantially over the past decade, reporting and participation was still voluntary at the agency level prior to 2021 when it became the national standard for law enforcement crime data. As such, to adjust for reporting quality and inconsistency, we control for seasonality, control for the county-level coverage rate (percent of county population that is represented by the reporting agencies), alternately exclude counties that do not achieve at least 65 percent coverage, and limit analyses to large counties (which also tend to have more consistent and relatively more complete agency reporting).[7]

We acknowledge that some of our time period is impacted by the COVID-19 public health cri-

[7]Seasonality is adjusted by including county-by-half year interactions. We follow Fone et al. (2023) and use 65% coverage rate, although results are similar for alternative cutoffs. The coverage indicator shows the effective coverage of reporting of IPV by agencies for a county in a given year, with greater values representing more complete coverage.

sis. In Table 7, we separately control for the COVID-19 mortality rate and exclude the year 2020, the start of the public health crisis. Our findings are not materially altered in any of these specifications. Finally, our results are also robust to alternate functional forms, including estimating the models via fixed effects Poisson regression and utilizing the log of the IPV rate.[8]

Lastly, we also consider the policy context in Texas in Table 7. While a post-*Dobbs* trigger ban took effect in August of 2022 following the *Dobbs* decision, the Texas Heartbeat Act (Texas Senate Bill 8; SB-8) went into effect a year earlier in September of 2021, prohibiting physicians from performing an abortion after a fetal heartbeat is detected under the threat of a civil liability. Failing to account for this earlier treatment could lead to potential confounding in modeling the effects of the distance-based abortion restrictions that materialized with the trigger ban.[9] In fact, when we explicitly model SB-8 by including an interaction between Texas and the pre-*Dobbs* period over which SB-8 was in effect, the estimated treatment effects expectedly become stronger; an increase in distance by 100 miles is found to significantly raise IPV by 3.43 incidents, and those involving injury or arrest by between 2.38 and 1.50 incidents.

Our study has some important limitations. While the NIBRS data has much more complete coverage in recent years, it is not representative of the entire US. Moreover, the NIBRS represents IPV incidents that are known to the police, and as such, may underestimate true prevalence of IPV. Our proxy for abortion access (distance from the county-centroid to the nearest abortion provider) may not fully capture an individual's access to reproductive health care, which could also be affected by factors affecting individual agency including informational, financial, and interpersonal barriers. We also are unable to account for access to medication abortion (e.g., misoprostol with or without mifepristone) or abortion funds. Our calculation of travel distance assumes the use of a personal car (as opposed to public transit), which may underestimate actual travel times for those

The coverage indicator is calculated following Fone et al. (2023) in the following way:

$$CI_{c,t} = (1 - \sum_{a=1}^{n_{c,t}} \left(\frac{A_{a,c,t}}{T_{c,t}} \cdot \frac{12 - M_{a,t}}{12} \right)) \times 100 \tag{2}$$

where $CI_{c,t}$ is the coverage indicator for county c in year t; $n_{c,t}$ is the number of agencies in county c at time t; $A_{a,c,t}$ is the population of agency a in county c in year t; $T_{c,t}$ is the total population in county c in year t; and $M_{a,t}$ is the number of months agency a reported in year t.

[8]Estimates from the Poisson regression model indicate an increase in the IPV rate on the order of approximately 4.1 percent, which compares to the 4.4 percent increase implied by the estimate reported in Table 2 (when expressed relative to the mean; model 4). Estimates based on the log transformation of the IPV rate point to larger effect sizes, on the order of approximately 8 percent.

[9]While the law did not make performing an abortion a crime, it authorized the public to bring a civil lawsuit against any provider who performs such an illegal abortion. Since a fetal heartbeat is usually detected within five to six weeks of the pregnancy, the law effectively restricts abortion access beyond this period except in the case of medical emergencies. Note that distance to the nearest abortion provider was mechanically not impacted by the Texas Heartbeat Act; however, at the time the law made accessing abortions within the state very difficult and costly, and in a sense increased the 'effective' distance to the nearest provider. Failure to model this pre-*Dobbs* shift is essentially akin to misclassifying the timing of the distance-based treatment.

who rely on public transportation.

5 Conclusion

Following the *Dobbs* decision in June 2022, access to reproductive health care changed dramatically with changes in state policy towards abortion and physical access to abortion care. While a considerable amount of research has focused on births and abortions, here we study an important social outcome that has impacts on families and children, in particular, intimate partner violence. We use administrative data that capture intimate partner violence against women (NIBRS) and county-level variation in distances to nearest abortion provider for a period before and after the *Dobbs* decision, 2017-2023. Applying DiD and event study methods, we find that abortion restrictions increase the risk of IPV on the order of nearly 3 incidents per 10,000 women of reproductive age, and that these effects are mainly driven by simple assaults. Furthermore, we find that increases in IPV are greatest for women between the ages of 25-34 compared to younger and older women, although the highest percentage increases are for teens and women 40-44. Risks for IPV also differ by race/ethnicity with the largest impacts of abortion restrictions on IPV incidents among non-Hispanic Blacks (2.43 incidents per 10,000 women of reproductive age; or 3.3 percent relative to the mean). We also asked if increases in IPV differ by socioeconomic status of the county. Here, we found evidence of greater increases in IPV for counties with populations that have more limited years of schooling. Our findings are important for policy at the state and federal level, both around reproductive health policies but also in supporting vulnerable populations at risk of violence.

Intimate partner violence imposes substantial costs on victims and society, with its lifetime economic burden estimated to exceed $4.5 trillion (inflated to 2023$). The bulk of this burden comprises health care costs ($2.7 trillion) as well as productivity losses and reductions in lifetime earnings capacity ($1.7 trillion) (Peterson et al. 2018). Moreover, almost 40 percent of the total economic burden is borne by the public sector. Our estimates indicate that the post-*Dobbs* increase in abortion restrictions resulted in additional intimate partner violence among female victims, on the order of between 9,271 and 13,998 incidents across the treated (ban) states; monetizing this total implied rise in IPV is predicted to add between $1.24 billion and $1.87 billion to social costs.[10]

[10]For this exercise, we utilize the range of estimates reported in Table 7 for the total IPV rate, in conjunction with the following inputs: i) estimated "treatment dose" as measured by the population-weighted increase in distance for the average county in the ban states between 2021 and end of 2023 is 241.1 miles; ii) total population of reproductive-aged women (ages 15-44) is 16.92 million in 2023 for the ban states; iii) estimated lifetime cost per female IPV victim is $133,559 (deflated to 2023$, based on Peterson et al. (2018)). Monetizing the increase in IPV incidents is an inherently noisy endeavor, and these estimates should be interpreted as crude "ballpark" estimates. On the one hand, our estimates of the added social economic burden are potentially conservative due to various considerations. First, these estimates do not include intangible costs to victims and family members (i.e., pain and suffering), which can amount to more than double the tangible costs (Zhang et al. 2012). Second, while our focus on IPV incidents reported to law enforcement likely captures more serious and/or frequent incidents (i.e. 55 percent of all such reported

In light of such costs, our results underscore the importance of assessing more far-reaching, downstream effects of abortion restrictions beyond their proximate effects on fertility, births, and abortions. Understanding and quantifying policy spillovers on broader measures of the well-being of women, families, and children – including the risk of exposure to violence – is vital for weighing the full costs and benefits of such policies, and for identifying vulnerable populations who may be further at-risk of experiencing the negative effects of abortion restrictions.

incidents involve physical injury), many incidents do not get reported; by some accounts, approximately 40 percent of domestic violence incidents remain unreported (Wheeler and Piquero 2024; Durose et al. 2005). Our imputation of the added social burden does not include IPV incidents that are not reported to law enforcement. On the other hand, our figures of this added economic cost may also be over-estimated since the cost estimates in Peterson et al. (2018) correspond to the lifetime costs per victim, which may involve multiple incidents, as opposed to the cost per incident. If we employ only the increase in IPV incidents that involved physical injury (Table 7), added economic costs amount to between $0.9 and $1.3 billion.

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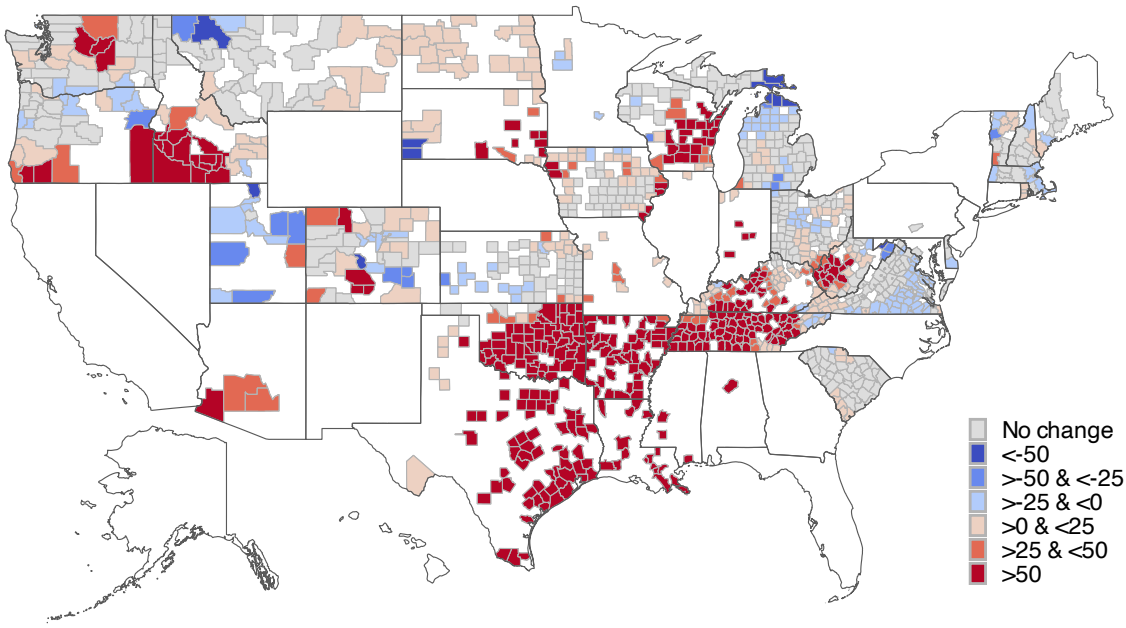
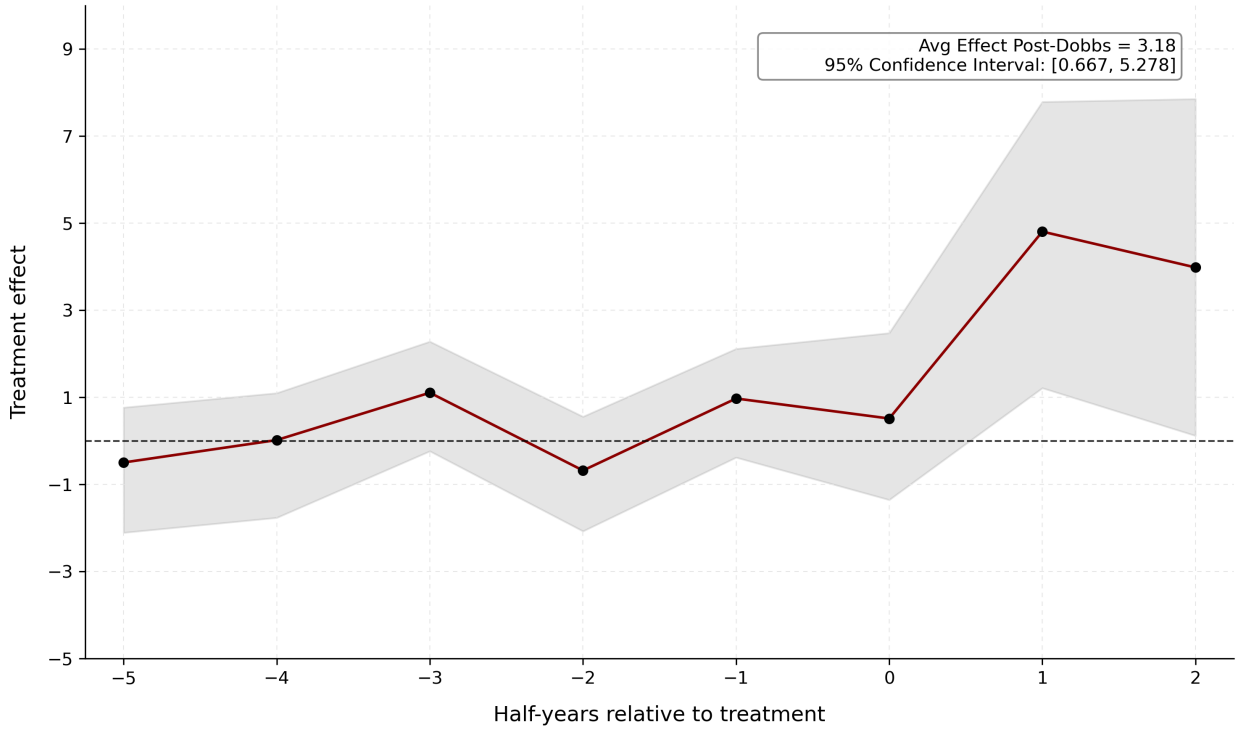
FIGURE 1: The maximum change in distance to nearest abortion provider for counties in the NIBRS sample, 2017–2023



Note: Counties in the NIBRS sample are shaded by changes in driving distance from the county population centroid to the nearest abortion facility, measured between 2017 and 2023. Blank areas represent no data availability.

FIGURE 2: Synthetic Difference-in-differences event study estimated effects of a ban on intimate partner violence rate



Note: Estimated treatment effects produced with the implementing the event study estimator proposed by Clarke et al. (2024). The outcome is intimate partner violence rate per 10,000. Treatment and control are defined by state ban classifications listed in Table A1. We use Approach 1 classification in this specification. The shaded lines show 95% confidence intervals.

TABLE 1: SUMMARY STATISTICS

	Pre-*Dobbs* (2017h1–2022h1)	Post-*Dobbs* (2022h2–2023h2)	Full Sample (2017h1–2023h2)
IPV outcomes			
Intimate partner violence per 10k	54.48	48.36	53.17
	(36.79)	(30.30)	(35.59)
Arrests per 10k	30.36	26.82	29.60
	(21.80)	(16.68)	(20.86)
Injury per 10k	29.96	26.84	29.29
	(21.42)	(17.91)	(20.75)
Abortion access			
Distance to nearest abortion provider (miles)	65.48	123.5	77.91
	(53.34)	(132.0)	(80.85)
Economic conditions			
Unemployment rate (%)	4.627	3.618	4.411
	(1.881)	(1.152)	(1.799)
Poverty rate (%)	13.94	13.91	13.93
	(5.174)	(4.977)	(5.132)
Unemployment rate (%)	4.627	3.618	4.411
	(1.881)	(1.152)	(1.799)
Minimum wage	8.357	9.269	8.552
	(1.646)	(2.432)	(1.880)
Maximum welfare benefit for a family of three	945.1	1192.8	998.2
	(149.1)	(161.1)	(182.7)
Medicaid coverage (%)	22.23	26.56	23.16
	(6.120)	(6.050)	(6.359)
Initial population with more than a HS education (%)	54.33	54.33	54.33
	(10.89)	(10.89)	(10.89)
Initial rural population (%)	50.68	50.68	50.68
	(29.11)	(29.11)	(29.11)
Demographics (% of total population of women aged 15-44)			
White & non-Hispanic	85.63	85.38	85.58
	(14.73)	(14.56)	(14.69)
Black & non-Hispanic	9.135	9.242	9.158
	(13.64)	(13.43)	(13.60)
Hispanic	9.790	10.33	9.905
	(12.10)	(12.25)	(12.14)
Age 15–19	17.79	17.29	17.69
	(2.485)	(2.510)	(2.499)
Age 20–24	16.72	16.90	16.76
	(3.960)	(3.985)	(3.966)
Age 25–29	16.36	15.73	16.22
	(1.762)	(1.551)	(1.738)
Age 30-34	16.30	16.86	16.42
	(1.774)	(1.804)	(1.795)
Age 35-39	16.70	16.50	16.66
	(1.801)	(1.817)	(1.806)
Age 40–44	16.13	16.73	16.26
	(2.101)	(2.028)	(2.100)

Notes: Data are from the 2017–2023 NIBRS. The table presents the means and standard deviations of variables for the pre-*Dobbs*, post-*Dobbs*, and full sample period at the county level (N=16,646 county – half-years).

TABLE 2: BASELINE RESULTS

	(1)	(2)	(3)	(4)
Intimate partner violence rate	4.12***	2.86**	2.90**	2.36*
	(0.93)	(1.02)	(0.95)	(0.97)
Observations	16,646	16,646	16,646	16,646
Outcome Mean	53.17	53.17	53.17	53.17
Injury rate	2.74***	2.01**	1.98**	1.64*
	(0.64)	(0.68)	(0.63)	(0.64)
Observations	16,646	16,646	16,646	16,646
Outcome Mean	29.29	29.29	29.29	29.29
Arrest rate	1.96***	1.33***	1.39***	1.09**
	(0.31)	(0.34)	(0.35)	(0.36)
Observations	16,646	16,646	16,646	16,646
Outcome Mean	29.60	29.60	29.60	29.60
County and half-year fixed effects	✓	✓	✓	✓
Time-varying county controls	×	✓	✓	✓
Initial county characteristics x half-year fixed effects	×	×	✓	✓
Time-varying state controls	×	×	×	✓

Notes: Data are from the 2017–2023 NIBRS. Estimates of the effects of a 100 mile increase in distance from abortion facility on the IPV rate, injury rate, and arrest rate per 10,000 population reported by female victims at the county level (N=16,646 county-half-years). All specifications include county and half-year fixed effects. Column (2) adds county-level demographic and economic controls (percent of female population between 15 and 19, 20 and 24, 25 and 29, 30 and 34, and between 40 and 44; percent of female 15–44 year old population Black, White, Hispanic; unemployment rate; labor force participation rate; poverty rate). Column (3) controls for initial (2017) shares of the population with more than a high school education and percent of rural population, both interacted with half-year fixed effects. Column (4) controls for state-level minimum wage, maximum welfare for a family of three, and proportion of the population with Medicaid insurance. Outcome means for the full period are listed in rows under the standard errors. Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 3: HETEROGENEITY BY VICTIM'S AGE

	15 to 19 (1)	20 to 24 (2)	25 to 29 (3)	30 to 34 (4)	35 to 39 (5)	40 to 44 (6)
Distance (100s miles)	1.15**	2.37	3.29*	2.94*	2.26*	2.16**
	(0.41)	(1.35)	(1.33)	(1.16)	(0.94)	(0.81)
Observations	16,646	16,646	16,646	16,646	16,646	16,646
Outcome Mean	21.99	60.39	69.58	69.94	58.50	42.82

Notes: Data are from the 2017–2023 NIBRS. Estimates of the effects of a 100 mile increase in distance from abortion facility on the IPV rate per 10,000 (N=16,646 county-half-years) separated by age. All specifications include county and half-year fixed effects, county-level demographic and economic controls (percent of female population between 15 and 19, 20 and 24, 25 and 29, 30 and 34, and between 40 and 44; percent of female 15–44 year old population Black, White, Hispanic; unemployment rate; labor force participation rate; poverty rate), initial (2017) shares of the population with more than a high school education and percent of rural population, both interacted with half-year fixed effects, and state-level minimum wage, proportion of the population with Medicaid insurance, and maximum welfare for a family of three. Outcome means for the full period are listed under the standard errors. Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 4: HETEROGENEITY BY VICTIM'S RACE

	Non-Hispanic White (1)	Non-Hispanic Black (2)	Hispanic (3)
Distance (100s miles)	1.17**	2.43	1.15
	(0.36)	(1.67)	(0.91)
Observations	16,646	16,646	16,646
Outcome Mean	43.62	73.42	32.04

Notes: Data are from the 2017–2023 NIBRS. Estimates of the effects of a 100 mile increase in distance from abortion facility on the IPV rate per 10,000 (N=16,646 county-half-years) separated by race. All specifications include county and half-year fixed effects, county-level demographic and economic controls (percent of female population between 15 and 19, 20 and 24, 25 and 29, 30 and 34, and between 40 and 44; percent of female 15–44 year old population Black, White, Hispanic; unemployment rate; labor force participation rate; poverty rate), initial (2017) shares of the population with more than a high school education and percent of rural population, both interacted with half-year fixed effects, and state-level minimum wage, proportion of the population with Medicaid insurance, and maximum welfare for a family of three. Outcome means for the full period are listed under the standard errors. Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 5: HETEROGENEITY BY COUNTY CHARACTERISTICS

	Sample split at median of the following county characteristic:					
	A. More than HS Education		B. Rural Share		C. Household Income	
	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median
	(1)	(2)	(3)	(4)	(5)	(6)
Distance (100s miles)	1.32	6.13***	0.18	2.35*	1.92	4.18*
	(0.98)	(1.79)	(0.81)	(1.06)	(1.01)	(2.08)
Observations	8,330	8,316	8,316	8,330	8,316	8,330
Outcome Mean	50.44	55.91	49.14	57.20	45.92	60.41

Notes: Data are from the 2017–2023 NIBRS. We divide the sample by above and below the median initial (2017) share of the population with more than a high school education, share of the population living in a rural county, and median household income at the county. Estimates of the effects of a 100 mile increase in distance from abortion facility on the IPV rate per 10,000 (N=16,646 county-half-years). All specifications include county and half-year fixed effects, county-level demographic and economic controls (percent of female population between 15 and 19, 20 and 24, 25 and 29, 30 and 34, and between 40 and 44; percent of female 15–44 year old population Black, White, Hispanic; unemployment rate; labor force participation rate; poverty rate), initial (2017) shares of the population with more than a high school education and percent of rural population, both interacted with half-year fixed effects, and state-level minimum wage, proportion of the population with Medicaid insurance, and maximum welfare for a family of three. Outcome means for the full period are listed under the standard errors. Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

TABLE 6: HETEROGENEITY BY IPV COMPONENTS

	(1) Simple assault rate	(2) Aggravated assault rate	(3) Sexual violence rate	(4) Intimidation rate
Distance (100s miles)	1.83*	0.27*	0.06**	0.20
	(0.80)	(0.12)	(0.02)	(0.14)
Observations	16,646	16,646	16,646	16,646
Outcome Mean	37.19	8.96	1.31	6.36

Notes: Data are from the 2017–2023 NIBRS. Estimates of the effects of a 100 mile increase in distance from abortion facility on the rate of simple assault, aggravated assault, sexual violence, and intimidation per 10,000 (N=16,646 county-half-years). All specifications include county and half-year fixed effects, county-level demographic and economic controls (percent of female population between 15 and 19, 20 and 24, 25 and 29, 30 and 34, and between 40 and 44; percent of female 15–44 year old population Black, White, Hispanic; unemployment rate; labor force participation rate; poverty rate), initial (2017) shares of the population with more than a high school education and percent of rural population, both interacted with half-year fixed effects, state-level minimum wage, proportion of the population with Medicaid insurance, and maximum welfare for a family of three. Outcome means for the full period are listed under the standard errors. Standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

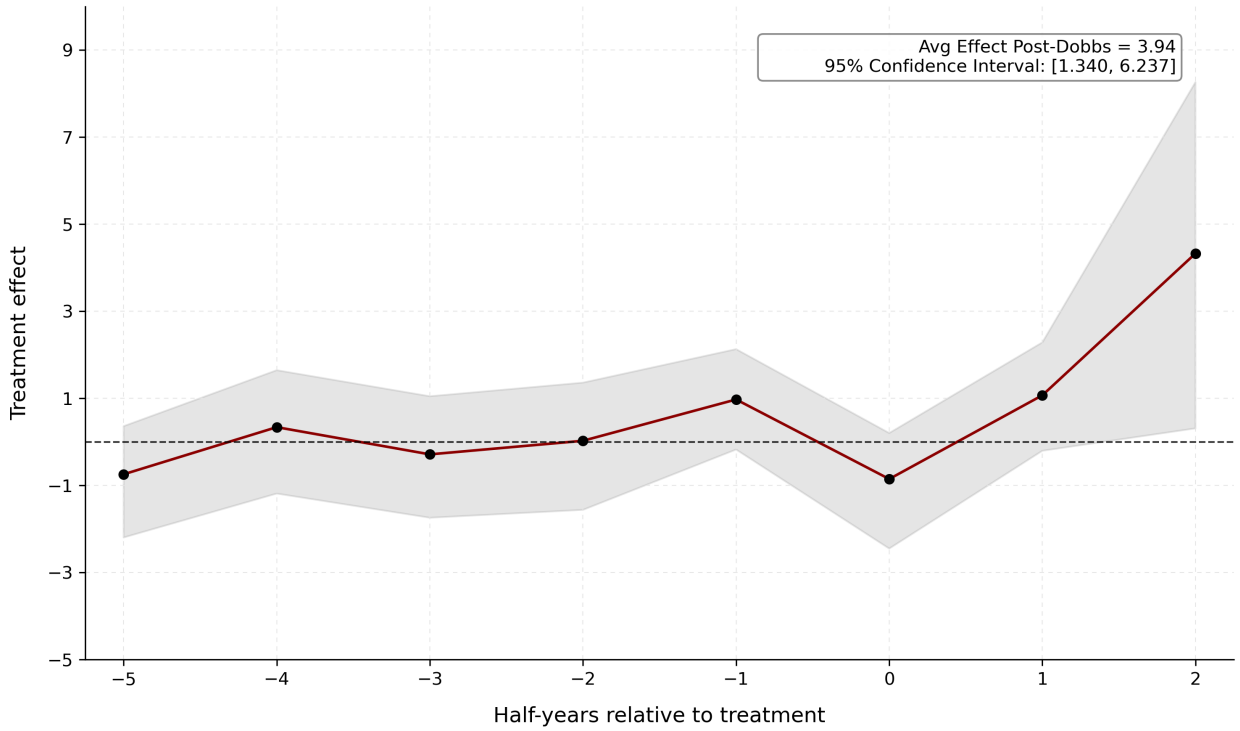
TABLE 7: ROBUSTNESS TO ALTERNATIVE SPECIFICATIONS

	IPV rate	Injury rate	Arrest rate
Controlling for ACA Expansion	2.38*	1.65**	1.10**
	(0.96)	(0.63)	(0.35)
Controlling for EITC Policy	2.27*	1.57*	1.07**
	(0.98)	(0.64)	(0.37)
Clustering at the State Level	2.36**	1.64***	1.09**
	(0.69)	(0.41)	(0.38)
Controlling for Coverage Rate	2.36*	1.63*	1.08**
	(0.98)	(0.65)	(0.37)
Controlling for Seasonality	2.29**	1.60**	1.06***
	(1.06)	(0.69)	(0.38)
Dropping counties below 65% coverage rate	2.35*	1.59*	1.09**
	(1.03)	(0.67)	(0.38)
Only counties with population $\geq$ 10,000	2.35*	1.63*	1.08**
	(0.98)	(0.64)	(0.36)
Excluding 2020	2.73***	1.83***	1.20***
	(0.78)	(0.52)	(0.32)
Controlling for COVID-19 Mortality Rate	2.37*	1.65**	1.10**
	(0.96)	(0.63)	(0.35)
Poisson specification using incidents	0.04**	0.05**	0.06***
	(0.02)	(0.02)	(0.02)
Log of IPV rates	0.08**	0.09**	0.09***
	(0.03)	(0.04)	(0.03)
Controlling for Texas Senate Bill 8 (Texas Heartbeat Act)	3.43**	2.38**	1.50***
	(1.13)	(0.75)	(0.40)

Notes: Data are from the 2017–2023 NIBRS. Estimates of a 100 mile increase in distance from abortion facility on the IPV rate per 10,000, injury rate per 10,000, and arrest rate per 10,000 (N=16,646 county-half-years) subject to various controls. Policy controls include expansion of the Affordable Care Act as well as tax rates under the Earned Income Tax Credit policy. We also control for the period between Texas' passing of S.B. 8 ("The Heartbeat Bill") and *Dobbs*. Further changes to the standard specification include clustering standard errors at the state level, controlling for county coverage rate, adding an indicator for half-of-year to control for seasonality, dropping counties with a minimum coverage rate below 65%, and restricting the sample to counties with populations over 10,000. All regressions include county and half-year fixed effects, county-level demographic and economic controls (percent of female population between 15 and 19, 20 and 24, 25 and 29, 30 and 34, and between 40 and 44; percent of female 15–44 year old population Black, White, Hispanic; unemployment rate; labor force participation rate; poverty rate), initial (2017) shares of the population with more than a high school education and percent of rural population, both interacted with half-year fixed effects, state-level minimum wage, proportion of the population with Medicaid insurance, and maximum welfare for a family of three. Unless otherwise stated, standard errors in parentheses are clustered at the county level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Appendix Tables and Figures

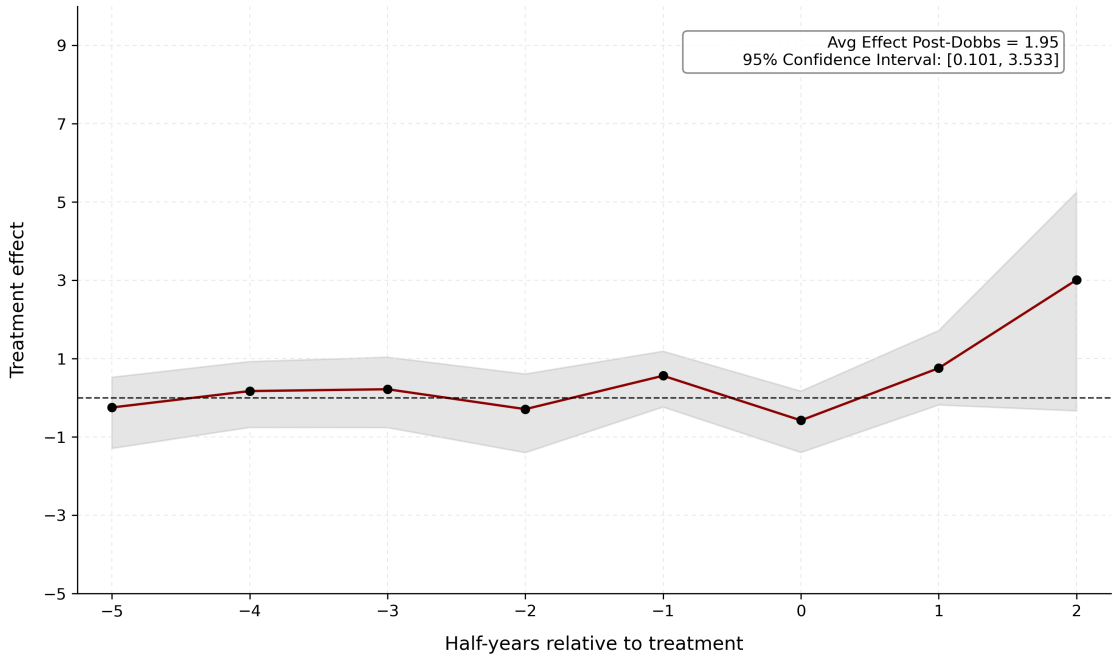
FIGURE A1: Synthetic Difference-in-differences event study estimated effects of a ban on intimate partner violence rate using an alternative total ban definition



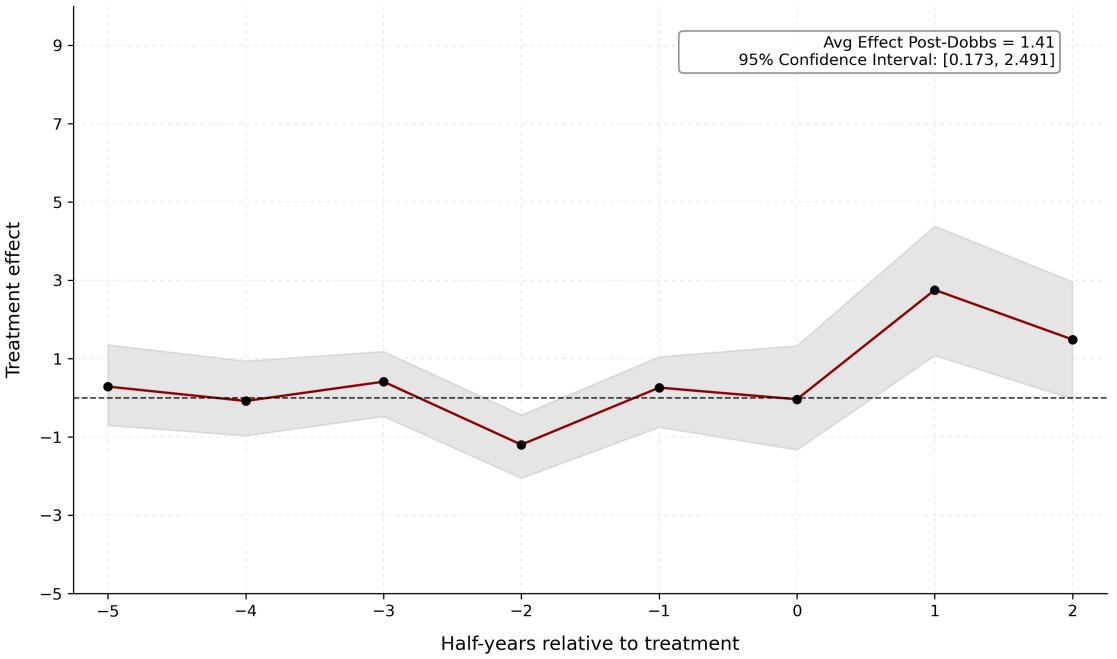
Note: Estimated treatment effects produced with the implementing the event study estimator proposed by Clarke et al. (2024). The outcome is intimate partner violence rate per 10,000. Treatment and control are defined by state ban classifications listed in Table A1. We use Approach 2 classification in this specification. The shaded lines show 95% confidence intervals.

FIGURE A2: Synthetic Difference-in-Differences event study estimated effects of a ban on injury and arrest rate

(a) Injury rate



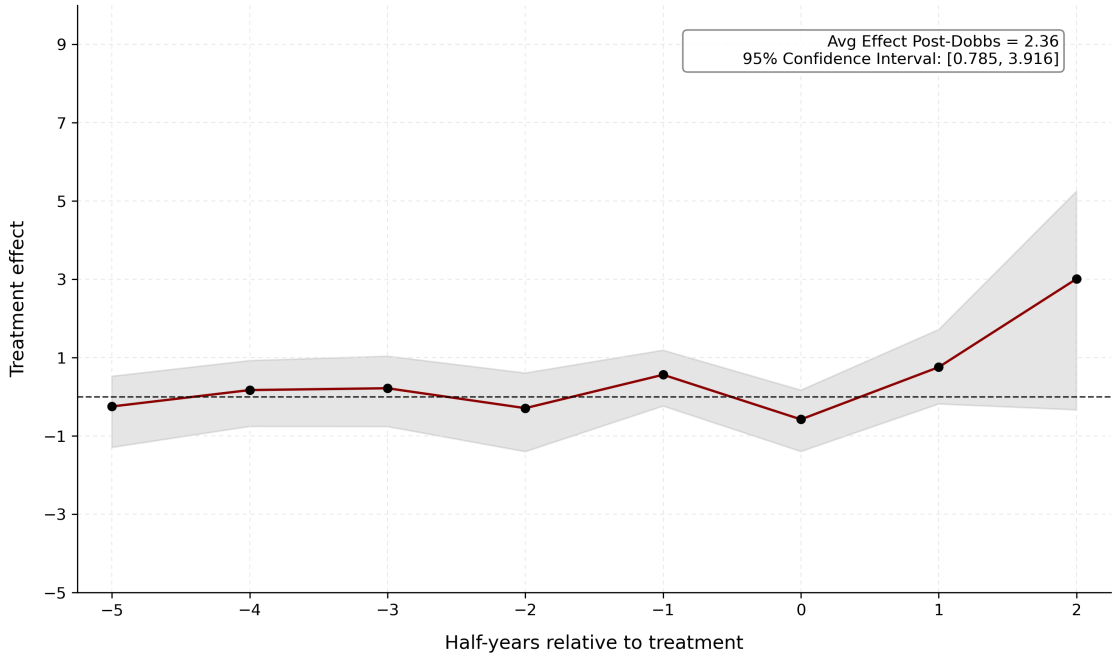
(b) Arrest rate



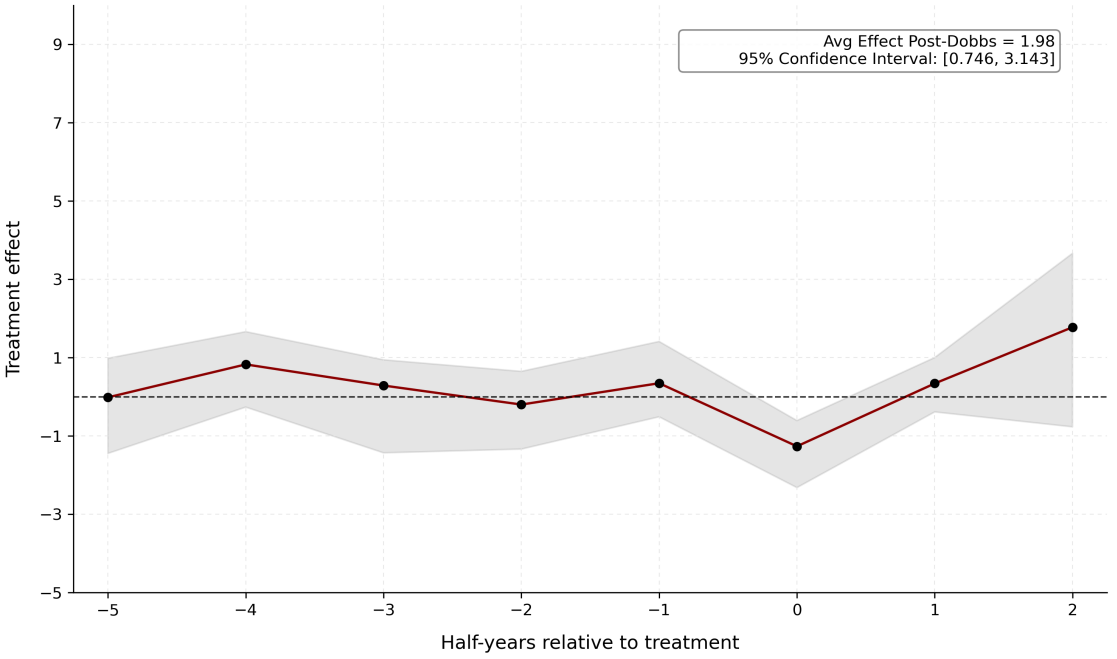
Note: Estimated treatment effects produced with the event study estimator proposed by Clarke et al. (2024). The outcome is injury rate per 10,000 in Panel (a) and arrest rate per 10,000 in Panel (b). Treatment and control are defined by state ban classifications listed in Approach 1 in Table A1. The shaded lines show 95% confidence intervals.

FIGURE A3: Synthetic Difference-in-Differences event study estimated effects of a ban on injury and arrest per 10k using an alternative total ban definition

(a) Injury rate



(b) Arrest rate



Note: Estimated treatment effects produced with the event study estimator proposed by Clarke et al. (2024). The outcome is injury rate per 10,000 in Panel (a) and arrest rate per 10,000 in Panel (b). Treatment and control are defined by state ban classifications listed in Approach 2 in Table A1. The shaded lines show 95% confidence intervals.

TABLE A1: Table A1: State Laws around Abortion Bans and the June 2022 *Dobbs* Decision

Panel A: Approach 1

Total Ban States	**Protective States**	**Excluded States**
Alabama	Alaska	Arizona
Arkansas	California	Florida
Idaho	Colorado	Georgia
Indiana	Connecticut	Iowa
Kentucky	Delaware	Nebraska
Louisiana	DC	North Carolina
Mississippi	Hawaii	North Dakota
Missouri	Illinois	Ohio
Oklahoma	Kansas	South Carolina
South Dakota	Maine	Utah
Tennessee	Maryland	Wyoming
Texas*	Massachusetts	
Wisconsin	Michigan	
West Virginia	Minnesota	
	Montana	
	Nevada	
	New Hampshire	
	New Jersey	
	New Mexico	
	New York	
	Oregon	
	Pennsylvania	
	Rhode Island	
	Vermont	
	Virginia	
	Washington	

Note: This approach is derived directly from Dench et al., 2024, with some small modifications: *Texas passed SB8 in September 2021, which effectively banned abortion after fetal cardiac activity; Pennsylvania is considered protective.

TABLE A1: State Laws around Abortion Bans and the June 2022 *Dobbs* Decision (continued)

Panel B: Approach 2

Any Ban States	Protective States	Excluded States
Alabama (June 2022) – total ban	Alaska	Iowa
Arkansas (June 2022) – total ban	California	Ohio
Arizona (Sept 2022) – 15 wk ban (2024 legal)	Colorado	South Carolina
Florida (July 2022) – 15 wk ban (2024 6 wk ban)	Connecticut	Wyoming
Georgia (Nov 2022) – 6 wk ban	Delaware	
Idaho (June 2022) – total ban	DC	
Indiana (October 2023) – total ban	Hawaii	
Kentucky (June 2022) – total ban	Illinois	
Louisiana (June 2022) – total ban	Kansas	
Mississippi (June 2022) – total ban	Maine	
Missouri (June 2022) – total ban (legal 2024)	Maryland	
North Carolina (July 2023) – 12 wk ban	Massachusetts	
North Dakota (April 2023) – total ban	Michigan	
Oklahoma (June 2022) – total ban	Montana	
South Dakota (June 2022) – total ban	Nevada	
Tennessee (June 2022) – total ban	New Hampshire	
Texas (Sept 2021; June 2022) – total ban*	New Jersey	
Utah (June 2022) - 18 wk ban	New Mexico	
Wisconsin (June 2022) – total ban, lifted Sept 2023	New York	
West Virginia (June 2022) – total ban	Oregon	
	Pennsylvania	
	Rhode Island	
	Vermont	
	Virginia	
	Washington	

Note: Panel B approach adds those states which had any bans (total or partial) after the 2022 *Dobbs* decision. *Texas passed SB8 in September 2021, which effectively banned abortion after fetal cardiac activity. **Excluded states in Panel B:** Iowa (July 2023), 6 wk ban; enjoined; Ohio (June 2022) – 6 wk ban; enjoined; South Carolina (June 2022) – 6 wk ban 6-22 to 8-22, enjoined; again 8-23; Wyoming (June 2022) – total, enjoined.