

Abortion Restrictions and Electoral Turnout: The Effect of *Dobbs v. Jackson* on Women's Voting*

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Abstract

The overturning of *Roe v. Wade* in June 2022 constituted the largest policy change to abortion access in nearly fifty years. Prior to the 2022 elections, many predicted that abortion restrictions would especially motivate women to vote. This expectation is consistent with a well-established theory in political science that beneficiaries of government policies – in this case, women – will turn out to vote at higher rates. However, beneficiaries may not support a policy on ideological grounds, as not all women support abortion access. Using national voter registration and turnout data alongside geographic variation in abortion laws, I assess whether reduced abortion access increased political participation among women, the group most directly affected. I find that abortion restrictions do not substantially increase women's participation. I also present suggestive evidence that Democrats who viewed abortion as an important issue were already very likely to vote, so their turnout decision was likely unaffected by the ruling.

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1 Introduction

Access to goods, services, and resources has historically served as a driver of political participation among the American public (Mettler, 2005; Campbell, 2003; Chen, 2013; Verba and Nie, 1987). In line with this phenomenon, both election forecasters and political leaders anticipated increased turnout among women in the 2022 midterm elections as a backlash to the Supreme Court’s decision in *Dobbs v. Jackson (Dobbs)*, which removed federal protections for abortion access. Survey evidence further suggests that abortion was a highly salient issue in the 2022 midterms: women reported higher motivation to vote due to abortion concerns (Frey, 2022), and an Economist/YouGov poll conducted prior to the 2022 midterms indicated that 60 percent of women reported that views on abortion mattered “a lot” in their vote choice, compared to 40 percent of men.

On the other hand, individuals do not always align their policy preferences with the benefits they receive. For example, some low-income voters may be advocates of eliminating education subsidies on ideological grounds, even though these subsidies would benefit their families. This paper investigates whether a political response to *Dobbs* exists, and whether it is best understood as gender-based mobilization or as a reflection of broader partisan and ideological dynamics. How do increased obstacles to accessing an abortion (and abortion-related care), as typified by abortion restrictions imposed by the *Dobbs* decision, impact women’s political participation?

I leverage national voter registration and turnout from 2014 to 2022, which includes turnout and demographic information for all registered voters in the US, to implement a series of tests. Using electoral turnout during midterm and general elections as the dependent variable, I construct a panel dataset suited for a Difference-in-Differences (DiD) analysis. I use the removal of federal protections around abortion in June 2022 as an opportunity to exploit a policy shock to reproductive healthcare. This policy shift triggered the reversion of state-level abortion regulations to preexisting statutes – some dating back nearly a century – which may not accurately reflect the contemporary policy preferences of the affected constituencies.¹ While many policy shocks yield diffuse or uneven effects across demographic groups, the consequences of *Dobbs* disproportionately affect women of

¹For example, after the passage of *Dobbs*, Michigan’s legislation around abortion would automatically revert to its 1931 pre-*Roe v. Wade* ban on abortion.

reproductive age. As such, I define women as the primary beneficiaries of abortion access, and thus the affected group, for the purposes of this study. Therefore, this policy shock presents a unique opportunity to examine how the abrupt loss of access to a health service influences political behavior within a clearly identifiable segment of the electorate.

First, I investigate whether state-level restrictions to abortion access affect the electoral turnout of women and the gender turnout gap in the 2022 midterm elections by exploiting state-level variation in abortion access and the timing of *Dobbs v. Jackson*. I compare individuals residing in states where abortion legislation changed post-Dobbs to those in states where it remained unchanged. Second, I turn to a more granular analysis within the state of Indiana, which was the first US state to pass abortion restrictions after *Dobbs v. Jackson*. I compare Indiana counties on the border with Illinois to counties inland Indiana, as a test between turnout in places at risk of increased travel and financial costs to accessing an abortion to counties relatively unaffected.

In both research designs, I obtain null estimates, and find no evidence that the loss of reproductive healthcare access alone had a significant effect on women’s political participation in the 2022 midterm elections. The estimates are sufficiently precise to rule out increases in the turnout gap larger than about 1 percentage point and decreases larger than about 0.1 percentage points, indicating that any effects of abortion restrictions on the gender turnout gap are likely to be very small in magnitude.² Instead, I present two alternative explanations: one, individuals favoring abortion access increase turnout when restrictions occur, and two, individuals who hold abortion with sufficient importance are more likely to be mobilized. Applying several research designs and incorporating survey results, my findings support the latter: voter mobilization appears to be driven not by abortion attitudes alone, nor by direct policy benefits, but by the importance of those attitudes.

My findings challenge key assumptions in the policy feedback and self-interest literature, which posit that individuals are mobilized by the loss of access to valued resources or goods (Mettler, 2005; Campbell, 2003). I also do not find evidence for the demobilizing effects predicted by stigmatization of policy benefits (as seen with means-tested programs (Bruch et al., 2010), nor the resource effects as predicted by the loss of healthcare literature (Cox et al., 2024). Instead, abortion access has

²This calculation is based on the estimate presented in Column 5 of Table 1.

become intertwined with ideology to the point at which access to reproductive healthcare may no longer be experienced uniformly by women as a material loss.

2 Theory and Previous Research: Why would abortion clinic access affect political participation?

Across a range of policy areas, individuals who benefit from government programs are more likely to participate politically (Campbell, 2003; Mettler, 2005; Wolfinger and Rosenstone, 1980; Verba and Nie, 1987; Chen, 2013). This pattern has been observed among recipients of Social Security (Campbell, 2003), the GI Bill (Mettler, 2005), agricultural subsidies (Wolfinger and Rosenstone, 1980), and Medicaid (Haselswerdt, 2017), however less is known about political participation in response to highly politicized policies.

In some cases, beneficiaries of policies or programs triggered responsiveness specifically when access to a valued good or resource is threatened (e.g see Green and Gerken (1989)). These studies suggest that individuals can be mobilized not only by receiving benefits, but also by their loss — a theoretical perspective that aligns with self-interest theories of voting, all of which rest on the foundational assumption that individuals respond to perceived costs and benefits associated with policies that affect them (Green and Gerken, 1989). Extending this logic, one might expect women, whose bodies are primarily affected, to respond to new restrictions with increased political participation. This expectation was widely voiced ahead of the 2022 midterm elections, including by former President Biden, who publicly predicted a surge in women’s turnout following *Dobbs* (Gambino, 2022), as backlash to the Supreme Court decision.³ Women’s voter registration spiked after the *Dobbs* decision (Catalist, 2022), especially in states that had pivotal midterm elections for abortion access (Paris and Cohn, 2022). However, the turnout results following *Dobbs* present mixed evidence as to whether the decision mobilized voters, and if so, who.⁴

³The decision was leaked in May, but became official and public on June 24, 2022.

⁴Turnout data from the 2022 midterms show that while overall participation was relatively high (52.5 percent), young people did not exhibit disproportionately higher turnout compared to other groups (Frey, 2023). In particular, the Republican Party - often associated with efforts to restrict abortion access - received increased support from women in 2022 (48 percent) compared to 2018 (40

The loss of reproductive care may also have demobilizing effects on political participation. For example, individuals who lose access to healthcare, either through hospital closures or health insurance dis-enrollment, are less likely to vote (Cox et al., 2024; Haselswerdt and Michener, 2019). These effects are often attributed to “resource models,” whereby the financial and logistical burdens associated with lost benefits increase the cost of political engagement (Brady et al., 1995). In the case of rural hospital closures, the deleterious consequences are well-documented: longer travel times, reduced access to care, and higher mortality risk (Gujral and Basu, 2019; McCarthy et al., 2021; Miller et al., 2020). Abortion clinic closures may produce comparable outcomes, as women lose access to essential healthcare services such as screenings for cervical and breast cancer, access to contraceptives, free screening for sexually transmitted diseases, and providing care for miscarriages (Lawrence and Ness, 2017).⁵

Interactions with representatives responsible for benefit allocation of a resource can also politically demobilize affected individuals through negative interactions with providers (Mettler, 2002; Haselswerdt, 2017; Campbell, 2003; Bruch et al., 2010; Campbell, 2012), instead of the political engagement typically associated with receiving policy benefits (Chen, 2013; Clinton and Sances, 2018). Abortion access is often stigmatized due to its associations with religion, ideology, and socioeconomic status. Individuals seeking abortion services may face intrusive or discouraging interactions with government institutions or healthcare providers, potentially generating negative interpretive effects. Such experiences can undermine political trust and engagement, particularly among those most directly affected. Descriptively, this mechanism may help explain the decline in turnout among young people and women in the 2022 midterms compared to 2018, with some state-level exceptions (Frey, 2023).

Alternatively, if attitudes toward abortion are the primary determinant of political participation, percent). However, voters in Kansas turned out in record-breaking numbers to protect their right to an abortion (Sirin and Villalobos, 2024), indicating at least some semblance of political motivation amongst constituencies.

⁵Following Hernandez et al. (2025), accessible abortion also offers a kind of “insurance” against unplanned transition to parenthood, especially for individuals that face higher costs to parenthood, for whom abortion’s prospective insurance value is high. Unplanned parenthood imposes significant personal and economic costs that can shape long-term well-being and life outcomes (e.g., (Williams and Shames, 2003; Herd et al., 2016; Johansen et al., 2020; Mark and Cowan, 2022)).

then proximity or access to abortion clinics should have little explanatory power. Abortion attitudes are strongly aligned with ideology and partisanship, suggesting that opposition to abortion restrictions may have mobilized individuals primarily based on their attitudes.⁶ Strong ingrained attitudes and emotions can overcome self-interest oriented responses to certain policies – as seen in the opposition (by White survey respondents) to the expansion of buses to school districts in the 1950s and 1960s (Sears et al., 1979). A similar example of this phenomenon comes from the Affordable Care Act (ACA), or Obamacare, in a Southern state like Kentucky. Despite the program’s clear success in regards to enrollment rates, these policy benefits did not translate into political support. For example, 82 percent of Kentucky’s Whitley county voters supported Donald Trump in the presidential election, even though he campaigned on repealing Obamacare (Kliff, 2016). This pattern illustrates that even direct policy benefits may fail to shift voting behavior when ideology and attitudes are stronger drivers, particularly once the policy itself becomes politicized. In this view, political mobilization around abortion would reflect attitudes about abortion rather than loss of access to reproductive care. If attitudes are indeed the primary driver of voter behavior on this issue, I should obtain null estimates for direct beneficiaries (e.g., women) of abortion access, as women are only slightly more supportive of abortion access than men.

Despite these competing hypotheses, there remains a notable gap in recent literature on how the loss of access to abortion and abortion-related care affects political participation. While Sirin and Villalobos (2024) examine the relationship between attitudes toward the *Dobbs* decision, empathy, and voting behavior, their analysis relies on survey data, which is known to significantly overestimate actual voter turnout (Ansolabehere and Hersh, 2012). Similarly, Mutz and Mansfield (2024) find that abortion attitudes prompted some survey respondents to switch their support from one party’s candidate to another in the 2022 midterms, however they rely on survey data and examine partisan shifts, rather than actual mobilization. This study addresses that limitation by leveraging a national voter file, offering a precise estimate of the relationship between the *Dobbs* decision and political participation.

⁶Descriptively, this held true leading into the 2022 midterms, with Democratic voters identifying abortion as a top policy priority, according to survey data from Pew Research Center (Schaeffer and Green, 2022).

3 The effect of state-level abortion restrictions on political participation

In this section, I examine the effect of abortion access on voter turnout at the state level. In section 3.1, I describe the variation in abortion policy across states following the *Dobbs* decision and explain how this heterogeneity informs the empirical strategy. This approach allows for a comparison of political participation among women in states that experienced policy changes versus those where abortion access remained protected. In section 3.2, I detail the data sources and construction. In Section 3.3, I explain the research design, employing a Difference-in-Differences model to estimate the effect of changes in abortion access on voter turnout. I then present graphical evidence suggesting little change in participation based on access to abortion care in section 3.4, followed by formal estimates that confirm the null effects in section 3.5.

3.1 Using state-level variation in abortion restrictions

I use the removal of federal protections around abortion due to *Dobbs v. Jackson* and regression back to previously passed state-level legislation as an opportunity to exploit a policy shock to reproductive healthcare. This policy change fundamentally and heterogeneously reshaped the landscape of reproductive healthcare access in the US, and allows for the comparison of states that experienced changes in access to those that did not. Moving forward, states had the power to restrict or protect abortion rights without federal interference, raising questions about the seemingly incongruous relationship between current American abortion policy and nationwide public opinion (Pew Research Center, 2022b).⁷

The immediate reactivation of pre-existing state-level laws, many of which date back decades or even a century, led to the implementation of highly restrictive abortion policies that did not necessarily reflect contemporary public preferences. For example, the state of Michigan reverted back to its previous 1931 ban on abortion, which was temporarily halted via legislative battle, and

⁷According to Pew reports, the majority of Americans disapproved of the decision to overturn *Roe v. Wade*, and the majority of Americans believe that abortion rights should be protected (Pew Research Center, 2022b).

eventually overturned after the November 2022 midterms. As such, this abrupt and externally imposed legal shift constitutes a shock to reproductive healthcare access, as residents were suddenly subject to laws that had not been recently democratically reconsidered. No other policy shock occurred in the post treatment period (between June and November 2022) that would lead to differential effects by gender on political participation.

The variation in abortion legislation introduces substantial differences in the cost of accessing reproductive healthcare. Abortion restrictions lead to increases in driving distance (Myers et al., 2025), and driving distance is a significant barrier to individuals accessing reproductive healthcare (Quast et al., 2017; Fischer et al., 2018; Dench et al., 2023). This variation in costs to accessing abortions and abortion-related care can be exploited to test whether increased barriers to access serve as a driver of political mobilization.

3.2 Data

For the purpose of this analysis, I assembled a novel dataset linking individual-level voter turnout data with detailed information on state-level abortion restrictions. To calculate voter turnout, I use a national voter file between the years of 2014 and 2022,⁸ which contains turnout and demographic information for all registered voters in the US. I use estimated state-level voting age population by gender from the US Census for each respective year as the denominator for my calculations. Electoral turnout is averaged at the state level by gender and year, and the gender turnout gap is computed as the difference between men’s and women’s turnout rates (where turnout gap = men – women). The analysis includes five election cycles.

To create a dataset of abortion restrictions, I rely on the Guttmacher Institute’s classification system, which I cross-reference with state-level legislative changes compiled from FindLaw and the Center for Reproductive Rights. This approach enables the identification of each state’s abortion policy landscape between June 24, 2022, and the November 8, 2022 midterm elections, as well as whether a change in policy occurred during that period. For example, a state like Illinois — where

⁸This is a cumulative voter file, and turnout is not calculated retrospectively. As such, over-time attrition due to changing addresses, death, name changes, and etc. all are accounted for.

abortion rights are constitutionally protected at the state level — would be coded as experiencing ‘no change,’ given that access to abortion remained protected both before and after Dobbs. I consider ‘control’ states to be states that had no changes to their abortion policy: the obstacles or costs of obtaining abortion care did not change. A state is classified as treated if abortion access became more restricted than under *Roe v. Wade*, including through the activation of trigger bans, or remain legally contested at the time of the November 2022 elections. A full list of each state’s categorization with reference to its legal landscape is provided in the Appendix.

The logic for coding contested states as treated builds on the concept of insurance: while contested states may not have experienced abortion clinic closures yet, residents of those states are in danger of losing access to healthcare. As such, they are grouped empirically with residents in other states that have lost access to healthcare going into the 2022 midterm elections.

3.3 Design

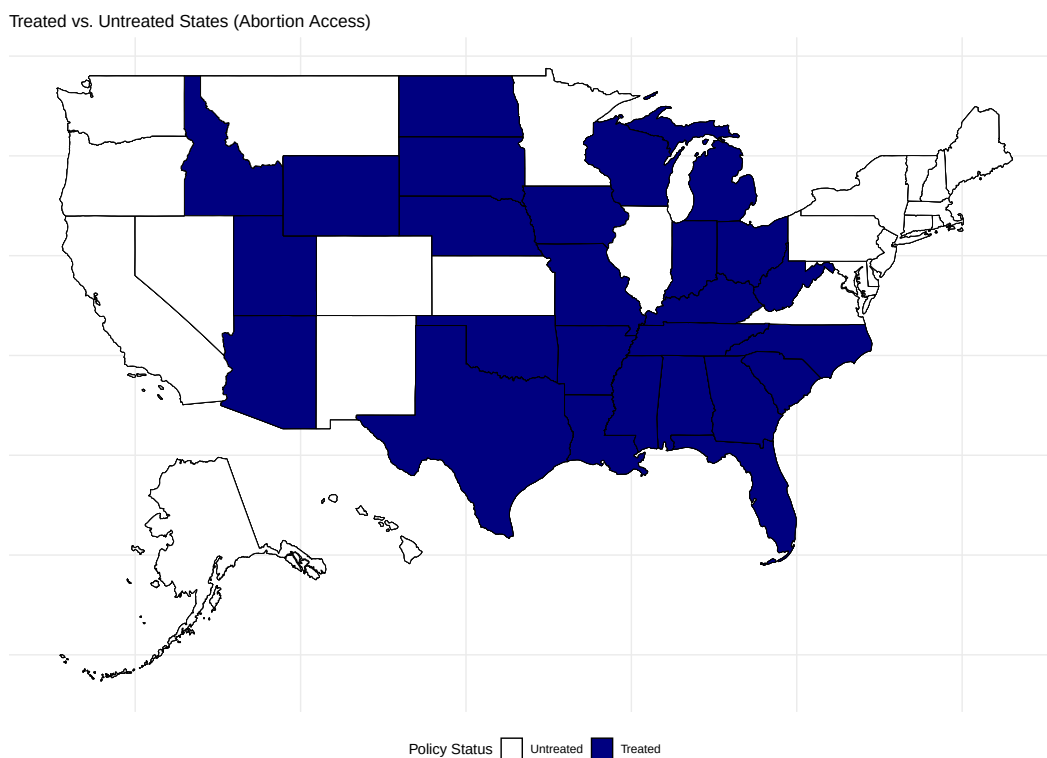
I estimate the effect of changes in access to abortion with the following equation:

$$Y_{st} = \beta_0 + \beta_1(\text{Post}_t \times \text{Treated}_s) + \gamma_s + \delta_t + \epsilon_{st} \quad (1)$$

where Y_{st} represents the outcome variable, the turnout gap or women’s turnout in state s at time t . The variable Post_t is an indicator for the post-treatment period, where year = 2022. There is only one post-treatment election in this dataset. Treated_s is a binary indicator for whether the state implemented or was in the process of implementing abortion restrictions post *Dobbs v. Jackson*. The coefficient of the interaction between $\text{Post}_t \times \text{Treated}_s$, β_1 , represents the change in women’s voting or the change in the turnout gap for treated states, or the Average Treatment Effect on the Treated (ATT), as estimated by the DiD model. I include state and election-year fixed effects, represented by γ_s and δ_t , respectively. I cluster all standard errors at the state level.⁹

⁹As part of the DiD design, time-invariant differences between states and across years are accounted for with state fixed effects and (election) year fixed effects. If the association between state-level abortion protections and gendered political participation would not have changed absent *Dobbs v. Jackson*, then this model should identify the causal effect of abortion access on women’s political participation.

Figure 1: US Map of Abortion Restrictions



I use elections between 2014 and 2022, using both women's turnout and the turnout gap. The sole treated period is 2022. The benefit of using the turnout gap is that it lends more plausibility to the comparability of this dependent variable across states, as the turnout gap between genders is plausibly similar state to state. If overall turnout was higher in certain states due to the heightened salience of a particular election, examining women's turnout in isolation could mistakenly attribute those increases to the *Dobbs* decision. By instead including the gender turnout gap as a dependent variable, this analysis accounts for broader shifts in electoral participation and better isolates the potential effect of abortion policy changes.

3.4 Graphical Evidence

The outcome variables are plotted over time below to provide a visual summary of the results, and are consistent with the assumption that treated and untreated states followed similar trajectories prior to the 2022 midterms. Figure 2a shows that women’s turnout trends in treated and untreated states remain closely aligned, with only a minimal widening in 2022. Figure 2b shows no visually discernible difference in the turnout gap trends over time.

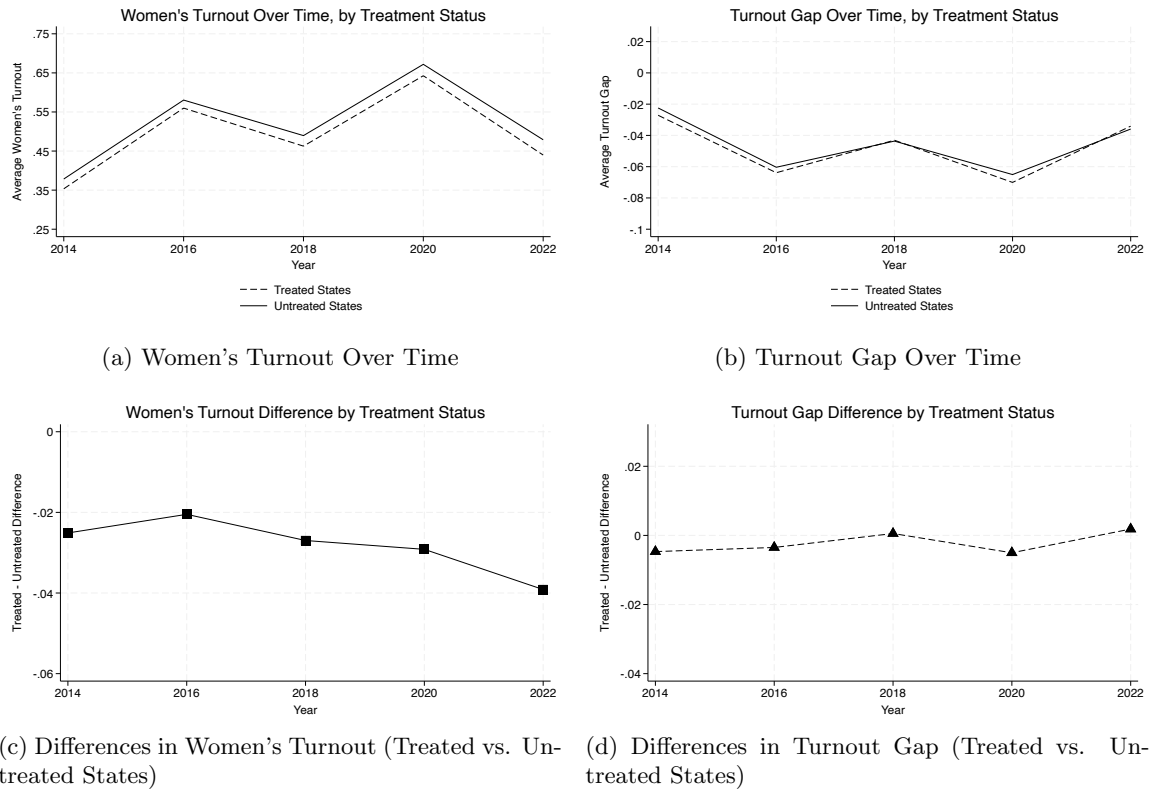
To further illustrate pre- and post-treatment trends, Figures 2c and 2d plot the differences between treated and control states over time for both women’s turnout and the gender turnout gap. The turnout gap remains consistently near zero across all periods, while women’s turnout is consistently lower in treated states compared to controls, with this difference becoming more negative in the post-treatment period. This pattern may suggest a modest demobilizing effect, assuming women’s turnout remained stable in control states following Dobbs. However, Figure 2c also indicates a downward trend in the pre-treatment period, suggesting that treated states may have been experiencing relatively lower levels of women’s turnout or that untreated states were experiencing relatively higher levels prior to 2022. I adjust the analysis below to account for this potential pre-trend by incorporating state-specific time trends, which helps isolate the treatment effect from background variation in political participation across states.

3.5 Results

Table 1 presents formal estimates corresponding to the graphical evidence presented above, confirming that the observed differences are minimal (and null). The analysis considers both women’s turnout and the gender turnout gap, examining all elections as well as restricting to midterms only. Although aggregating individuals to the state level reduces statistical power, the estimates across all specifications provide no consistent evidence that changes in abortion access affected political participation. In particular, the results do not support widespread media narratives suggesting that abortion was a politically mobilizing force for women in the 2022 midterms.

While the point estimates in Columns 1 and 3 suggest a modest demobilizing effect of about one

Figure 2: Women's Turnout (left) and Turnout Gap (right), Over Time (top) and Differences (bottom).



percentage point, these estimates are not statistically significant.¹⁰ Though substantively small – about half the demobilizing effect of hospital closures (Cox et al., 2024), half the mobilizing effect of Medicaid expansion (Baicker and Finkelstein, 2019), and roughly one-sixth the mobilizing effect of Alaska’s universal basic income (Loeffler, 2023) – such differences could matter in tightly contested elections. Columns 5 and 7 show a non-significant increase in the turnout gap (0.5 and 0.4 percentage points, respectively), indicating that women turned out at lower rates relative to men, consistent with the negative point estimates in Columns 1 and 3. Nevertheless, the findings offer no reliable support for the hypothesis that abortion policy changes impacted women’s political participation, as the results are statistically indistinguishable from zero.

As demonstrated in Figure 4, there is a slight downward trend in the women’s turnout difference prior to 2022. This suggests that either untreated states began experiencing increases in turnout prior to 2022, or treated states were already seeing declines, indicating a potential violation of the parallel trends assumption. To address this, I re-estimate the Difference-in-Differences model using a more conservative specification that includes an adjustment for state-specific time trends. This approach allows each state to follow its own linear trajectory over time, controlling for state-level changes in turnout that may be unrelated to changes in abortion policy.

Once these state-specific linear trends are added to the model, the estimated treatment effects decline in magnitude across all outcomes, and standard errors increase slightly. This reduction suggests that the effects identified in the baseline model were at least partially driven by preexisting nonparallel trends in turnout between treated and control states, where treated states were already experiencing slight declines in political participation before abortion access was restricted.

Substantively, these null results run contrary to prevailing public opinion and media narratives, which suggested that women (especially in states imposing new restrictions on abortion access) would be politically mobilized to defend reproductive rights. Instead, the estimates provide little evidence of such a response.

¹⁰Power calculations indicate that this analysis has 80 percent power to detect effects as small as three percentage points.

Table 1: Difference-in-Differences Results: US States

	Women's Turnout				Turnout Gap			
	All Years (1)	All Years (2)	Midterms Only (3)	Midterms Only (4)	All Years (5)	All Years (6)	Midterms Only (7)	Midterms Only (8)
Post*Treated	-0.014 (0.011)	-0.009 (0.018)	-0.013 (0.011)	-0.010 (0.027)	0.005 (0.003)	0.004 (0.005)	0.004 (0.002)	-0.004 (0.006)
State-Specific Trend	No	Yes	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	255	255	153	153	255	255	153	153
Number of States	51	51	51	51	51	51	51	51

Notes: Standard errors in parentheses. Standard errors clustered at the state level. Fixed effects for state and year included. Post = 1 if year = 2022, 0 otherwise. Treatment = abortion restrictions present or at risk of enactment.

3.6 Validation

3.6.1 Demand-side Differences

One concern with the DiD strategy is whether there is comparable demand for abortions and abortion-related care across states. If abortions were not sought after, or viewed as a valuable procedure, then why should a change in political participation follow? Given pre-Dobbs data on annual abortion rates for 46 reporting states by the the Center of Disease Control (CDC)(2023), the differences in abortion rates between the treated and control states do not appear large enough to explain heterogeneity in abortion policy. Of the top five states with the highest abortion rates, three are in the treated group - Georgia, Florida, and Michigan. Pre-Dobbs, Florida's abortion rate exceeded that of New York's, and the average abortion rate across treated states is approximately 8 abortions per 1,000 women, while the average across untreated states is approximately 12 per 1,000 women (Kortsmit et al., 2023).¹¹ Therefore, given the similarities in abortion rates across treated and untreated groups pre-Dobbs, I have plausible evidence to dismiss these concerns.

¹¹The number of abortions obtained in 2021 across treated states is 351,690, versus 274,288 across control states (Kortsmit et al., 2023)

3.6.2 Parallel Trends Checks

To validate the state-level results presented in Section 3.5, I first conduct placebo tests by reassigning 2018 and 2020 as pseudo-treatment years while retaining the treated $\times$ post interaction for 2022 (Table A.6, Appendix). Placebo tests assess the core parallel trends assumption: if treated and control states were already diverging prior to the intervention, the DiD estimator may confound these pre-existing differences with the causal effect of abortion policy changes. The placebo results show null estimates for women’s turnout in both years, although the 2018 placebo test yields a small but statistically significant estimate for the turnout gap with the same sign as the 2022 estimate. This raises concerns about violations of the parallel trends assumption. However, in this case, the magnitude of the coefficient is very small (well below one percentage point), the 2020 placebo test is null, and the 2022 estimates are themselves null. This pattern suggests that the 2018 result likely reflects noise specific to 2018 (perhaps due to issues comparing midterm to presidential elections) rather than a systematic divergence and violation of parallel trends, and it does not substantively affect the interpretation of the main findings.

As a second robustness check, I re-estimate the models using a synthetic DiD design (Table A.5, Appendix). This approach mitigates the risk that differences in baseline characteristics or pre-treatment trends between treated and control states drive the results by constructing a weighted synthetic control group whose pre-treatment outcomes closely replicate those of the treated states. The synthetic DiD estimate for women’s turnout is nearly identical to the baseline specification, while the estimated turnout gap increases modestly and reaches statistical significance. This change is consistent with improved pre-treatment fit: by better aligning control states with treated states prior to treatment, the synthetic DiD design reduces bias from differential trends and isolates the post-treatment divergence more cleanly. As a result, the estimated gap is slightly larger and more precisely estimated. Importantly, the synthetic DiD estimates are similar in magnitude and share the same sign as those reported in Table 1, alleviating concerns that the baseline results are driven by systematic differences in pre-treatment trends.

Thus, the placebo tests and synthetic DiD validation provide complementary evidence that the

null results are not due to systematic pre-treatment divergence. While the statistically significant 2018 placebo result cautions against assuming perfectly parallel trends, the small size of the gap, the absence of consistent evidence across pre-treatment years, and the replication of null results under synthetic DiD collectively support the conclusion that state-level abortion restrictions neither mobilized nor demobilized women voters.

3.6.3 Reclassifying treatment

It is possible that individuals most directly affected by abortion access, namely women, do not conceptualize access as a form of insurance. Instead, women may only perceive a threat to abortion access when restrictions or bans are actively in place. If so, states in which abortion policy remained legally contested may not mobilize women in the same way as states that implemented immediate bans, where access was directly and visibly restricted. In the baseline DiD specification, contested states are grouped with banned states. To assess whether this classification obscures heterogeneous treatment effects, I conduct two additional robustness checks.

First, I exclude contested states from the analysis and restrict the sample to states that implemented abortion bans, following the approach in Dench et al. (2023). Under this specification, the sample includes 40 states. The results, reported in Table A.1 of the Appendix, closely mirror those in Section 3.5. The estimated effects remain similar in magnitude and null for both women’s turnout and the turnout gap.

Second, I reclassify treatment using a non-binary design to capture variation in the intensity of abortion restrictions. Contested states are assigned a treatment value of one, while states that implemented full abortion bans are assigned a value of two. The results, presented in Table A.2 of the Appendix, again yield null estimates comparable to those reported in Section 3.5.

Overall, these findings suggest that neither the severity of abortion restrictions nor the legal clarity of the policy environment meaningfully mobilized women’s turnout during this period. The persistence of null effects across alternative treatment definitions indicates that legal uncertainty in contested states is unlikely to explain the baseline results, or comparisons between ban states and protective states would be expected to produce substantively different estimates.

3.6.4 Sub-setting to women of reproductive age

Finally, I further refine the definition of policy beneficiaries by restricting the analysis to women of reproductive age, defined as ages 18 to 45. This subgroup is most directly affected by changes in access to reproductive healthcare and therefore most likely to respond to abortion policy shifts. The results, reported in Table A.3 of the Appendix, closely align with those in Section 3.5.

These findings suggest that even among women most likely to benefit from access to reproductive healthcare, abortion policy changes did not meaningfully mobilize turnout. The estimates remain small, retain the same sign as in the baseline specification, and are not statistically significant.

4 The effect of abortion restrictions on political participation within counties in Indiana

As a further test, I examine the effect of abortion access on political participation at the county level within Indiana. I begin by describing the variation in clinic access within the state, focusing on border versus inland counties.

4.1 County-level variation in abortion clinic access

Indiana provides a unique opportunity to observe the effects of abortion restrictions due to its proximity to Illinois, where abortion access is protected (McCann, 2022), and the abortion restrictions of Indiana’s neighbor states. Spillover effects and cross-state mobility present challenges for the state-level DiD design, as some residents may have traveled to neighboring states to access abortion clinics. As such, it is plausible that many residents in the treated states of this analysis may not have felt the effects of changes to their state’s abortion policy. To overcome this issue, I use the state of Indiana as a case study to assess whether proximity to abortion clinics affected women’s turnout.

In the state of Indiana, seven abortion clinics were operating in June 2022. Shortly afterward, Indiana became the first state in the US to approve abortion restrictions post-*Dobbs v. Jackson*,

with a near-total ban signed by the Republican governor (Rodgers, 2022b), which went into effect September 15, 2022. On September 22, 2022, this ban was temporarily halted via court injunction, due to a lawsuit filed by the American Civil Liberties Union (ACLU) of Indiana (Rodgers, 2022a), allowing abortions to be legal up to 20 weeks post-fertilization. Whether abortion clinics would lose their licenses under the law would rest on a decision from the Indiana Supreme Court after the November 2022 midterm elections.¹²

West of Indiana, Illinois became a haven for abortion care in the Midwest. After the *Dobbs v. Jackson* decision, abortions in Illinois increased by 9 percent in 2022, with a total of 56,457 in 2022 and 51,797 in 2021 (McCann, 2022). The number of abortions obtained by out-of-state women increased by 49 percent in 2022, and abortion clinics anticipated heightened demand in the face of neighboring abortion restrictions (Steupert and Cox, 2022; McCann, 2022). For example, the state of Ohio immediately defaulted to its previous 2019 abortion ban at six weeks, which was temporarily halted prior to the 2022 midterms (ACLU, n.d.). To the south, Kentucky returned to its former trigger ban (ACLU, 2025).

4.2 Data

I use the L2 (national) Voter File within the state of Indiana, which contains the electoral turnout and demographic information of all registered voters in the state, and merge it into a county-level dataset of abortion clinic locations. I use the L2 dataset for November midterm and presidential elections between the years of 2010 and 2022, with 2022 again coded as the sole treated year. Aggregating individuals by county, I calculate average electoral turnout by gender for each of the elections, using population estimates of the voting-age population (over 18) from the US Census.¹³ Each county is coded with a binary measure indicating the county’s border status. Of 92 counties in Indiana, eleven are coded as border counties.¹⁴

¹²By the end of 2022, some abortion clinics halted in-person appointments completely, and full closures of clinics began in 2023 (Callahan, 2023; Rodgers, 2022a).

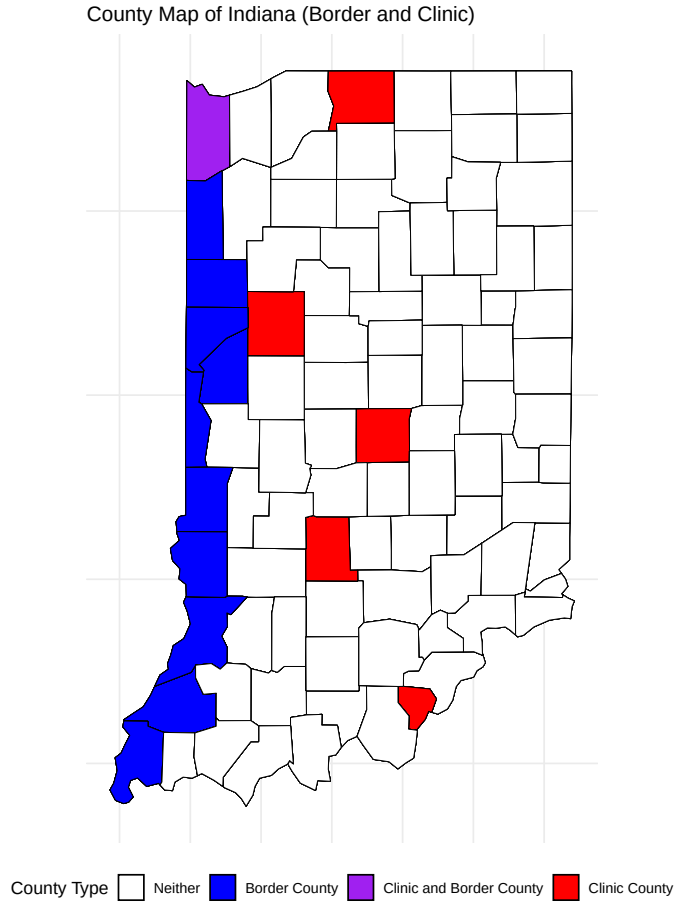
¹³For simplicity, I drop individuals that do not identify with the gender-binary before calculating the gender gap.

¹⁴Within the state of Indiana, 10 counties border Illinois, however one of the counties is extraordinarily narrow. I code the neighboring county as a border county as well, which is located the same distance from the Illinois border as the remaining counties, for consistency with distance from Illinois.

4.3 Research Design

First, I investigate whether abortion access affects electoral turnout for women by estimating a DiD model that compares counties in Indiana that are bordering Illinois, to those that are not, pre and post *Dobbs v. Jackson*. A county map is provided below (Figure 3), with color coding for treatment. The border counties (in blue) are approximately equidistant from in-state clinic locations (in red) and clinics located in Illinois.

Figure 3



Below, equation 2 presents the statistical model used to estimate the difference-in-differences model for the border county analysis. As part of the DiD model, county γ_c and year δ_t fixed effects

are included, which will take into account county-level and election year-specific trends in electoral turnout within Indiana. Y_{ct} represents the outcome variable, the turnout gap or women’s turnout in county c at time t . The variable Post_t is an indicator for the post-treatment period, where year = 2022. Treated_c is a binary indicator for whether the county was at risk of losing access to an abortion clinic after the *Dobbs v. Jackson* decision.

$$Y_{ct} = \beta_0 + \beta_1(\text{Post}_t \times \text{Treated}_c) + \gamma_c + \delta_t + \epsilon_{ct} \quad (2)$$

The coefficient of the interaction term, β_1 , represents the change in women’s voting or the change in the turnout gap for treated counties. I cluster all standard errors at the county level.

4.3.1 Border county analysis

I code counties that are on the border with Illinois as ‘control,’ since residents of these counties will likely experience no interruptions in their access to abortions due to *Dobbs v. Jackson*. Residents of these counties are located almost equidistantly from clinics in Indiana as compared to clinics in Illinois. Indiana counties that are located further inland are coded as ‘treated,’ since their access to abortion care will be interrupted if abortion restrictions are adopted in Indiana, and the possibility of restrictions was looming at the time of the 2022 midterm elections.

4.4 Graphical Evidence

Visual evidence in favor of the parallel trends assumption is demonstrated in Figures 4a and 4b, which demonstrate that the treated and control counties followed similar trajectories throughout the study period. The lines for both groups remain parallel, with only minor deviations in the post-treatment period, indicating that should an effect exist, it is slight. Figures 4c and 4d plot the differences between treated and untreated counties for women’s voting and the turnout gap. Differences between treated and untreated groups are generally close to zero.

Figures 4c and 4d demonstrate that women’s political participation in 2022 does not differ significantly from pretreatment periods. However, Figure 4c also reveals a slight downward trajectory in

the difference in women’s turnout between treated and untreated counties prior to 2022. I account for this by adjusting for potential pre-trends in the analysis below.

4.5 Results

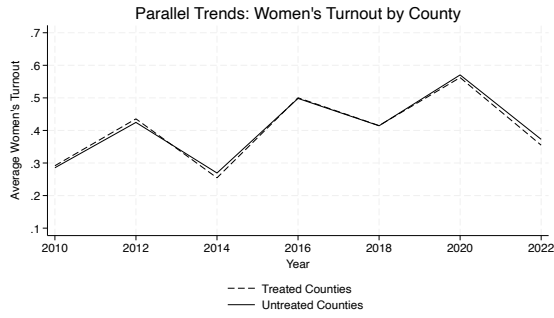
The border county analyses confirm the null estimates obtained with the state-level model. In Table 2, I present results from the border-county analysis within the state of Indiana. This design compares electoral turnout for women and the gender gap (men’s voting - women’s voting) in the elections prior to the *Dobbs v. Jackson* decision, to the November 2022 midterm election after the decision. As the DiD model includes fixed effects for counties and years, I am able to account for unobserved differences across counties that do not vary over time, in addition to unobserved differences across election years.

According to Column 1, turnout for women decreased by about 2 percentage points for treated counties, which were located further away from Illinois. Sub-setting to midterms changes the estimate only slightly, from about -1.7 percentage points to -1.5 percentage points. The purpose of sub-setting to midterm elections is because electoral turnout tends to be higher in presidential elections, which is immediate pre-treatment period in 2020. These results are statistically significant, and though small – approximately the same size as the state-level analysis, which are about half of the demobilizing effect observed in hospital closure research (Cox et al., 2024) – could make a difference in close general elections. As a point of reference, a simple back-of-the-envelope calculation indicates a decline of 1.7 percentage points in turnout corresponds to roughly 150,000 fewer voters in Indiana’s 2022 midterm elections, a substantively meaningful reduction given the state’s overall turnout of about 2 million voters in the 2022 midterms.¹⁵

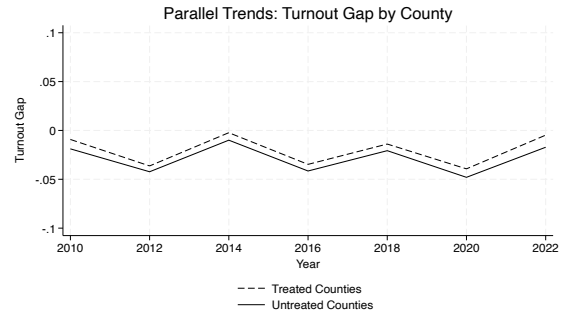
I also examine the gender turnout gap, since differences between men’s and women’s voting habits are more stable and comparable over time than turnout levels. If testing for the effects of abortion access, this should impact women more than men, and thus decreasing the turnout gap. The results provide little indication that proximity to Illinois clinics altered the turnout gap between

¹⁵Indiana’s turnout rate in the 2022 midterms was 22 percent overall, which amounts to approximately 2 million individuals that registered and voted (Hotchkiss, 2023).

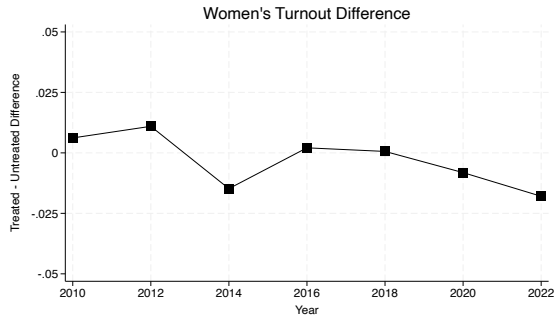
Figure 4: Turnout Trends and Differences between Treated and Untreated Groups [Border Counties]



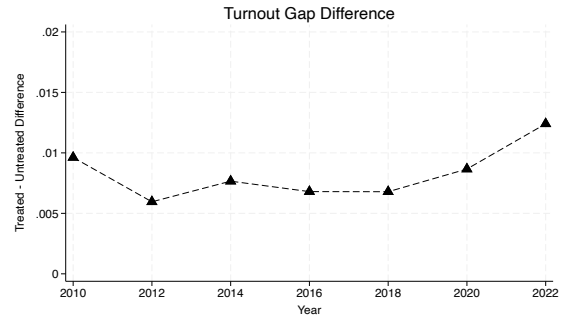
(a) Women's Turnout by Treatment Status [Border Counties]



(b) Turnout Gap by Treatment Status [Border Counties]



(c) Differences in Women's Turnout [Border Counties]



(d) Differences in Turnout Gap [Border Counties]

men and women, either in all elections or when focusing solely on midterms.

As with the national analysis, it is important to test the robustness of the Indiana border county results to pre-treatment trends. Figures 4c and 4d suggests the possibility of modest differences in women’s turnout trajectories between treated and control counties prior to the abortion policy change. Thus, I re-estimate the Difference-in-Differences model using a specification that includes county-specific linear time trends (Columns 2, 4, 6, and 8), allowing each county to follow its own gradual path over time. This adjustment controls for any underlying time-varying factors at the county level that might otherwise bias the estimated treatment effect.

Incorporating these county-specific trends leads to a noticeable decline in the estimates for women’s turnout, suggesting that the estimates derived in Columns 1 and 3 were in part driven by pre-existing downward trends in the treated counties. The estimates for women’s turnout also lose statistical significance, which is expected given that the effect size is smaller, requiring greater statistical power to detect with precision. Additionally, the inclusion of county-specific linear trends reduces the residual degrees of freedom, further limiting the model’s ability to detect statistically significant effects. In contrast, the estimated effect on the turnout gap remains virtually unchanged, both in magnitude and statistical significance. This asymmetry implies that while part of the decline in women’s turnout may have been due to unrelated trends in specific counties, the turnout gap between women’s and men’s turnout was not driven by those same trends.

As a robustness check, I re-estimate the model using men’s turnout rather than women’s. The results, presented in Table A.4 of the Appendix, indicate that the estimates for men’s turnout are similar to those for women in Columns 1 and 3 of Table 2. These findings suggest that the costs of abortion care and access to clinics for affected voters (i.e. women) were not the primary direct drivers of political participation. If this were the case, no relationship should be observed for men’s turnout. The absence of such a distinction indicates that other factors likely played a more substantial role in influencing voter behavior. Thus, I do not find substantive evidence that distance to clinics had an impact on women’s political participation.

Table 2: Difference-in-Differences Results: Indiana’s Border Counties

	Women’s Turnout				Turnout Gap			
	All	All	Midterms	Midterms	All	All	Midterms	Midterms
	Years	Years	Only	Only	Years	Years	Only	Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-Border	-0.017**	-0.009	-0.015**	-0.010	0.005	0.005	0.004	0.007
County*2022	(0.006)	(0.010)	(0.005)	(0.011)	(0.003)	(0.005)	(0.004)	(0.007)
County-Specific Trend	No	Yes	No	Yes	No	Yes	No	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	644	644	368	368	644	644	368	368
Number of Counties	92	92	92	92	92	92	92	92

Standard errors in parentheses. Standard errors clustered at the county level. Treatment is a binary measure where 1 indicates Non-Border county and 0 indicates a Border county. 2022 is the single Post period.

4.6 Validation

4.6.1 Parallel Trends Checks

To validate the Difference-in-Differences estimates, I conduct a series of robustness exercises that address potential violations of the parallel trends assumption. This is critical to the DiD design because if treated and control counties have diverging pre-trends, the standard DiD estimator can be biased, which could conceal the true treatment effect.

To address this risk, I first estimate placebo effects by interacting the treated indicator with year dummies for the two pre-treatment elections (2018 and 2020), while retaining the original treated $\times$ post (2022) interaction. These placebo coefficients provide a direct test of whether treated counties were already following different trends in electoral turnout from control counties prior to the policy change. If the coefficients on these interactions were significant, it could indicate systemic divergence in pre-treatment paths, suggesting bias in the DiD estimator. By contrast, null results (reported in Table A.7 of the Appendix) imply that the treated and control groups were on parallel trajectories before 2022, allowing for the estimated difference in differences over time and groups to be attributed

to the treatment. Without evidence of parallel pre-trends, null estimates risk being misleading, as underlying differences in pre-treatment trajectories could attenuate a true treatment effect toward zero and create the appearance of no impact.

As a second step, I implement a synthetic DiD design, which complements the placebo test by explicitly constructing a credible counterfactual: the synthetic DiD assigns weights to untreated units so that the weighted average of their pre-treatment outcomes closely replicates the trajectory of the treated units. This approach operationalizes the parallel trends assumption by ensuring that the comparison group not only has similar average levels but also matches the entire pre-treatment trends of the treated group. When re-estimated with this method, the treatment effect remains virtually identical to the baseline DiD estimate (Tables A.8 - A.9, Appendix). The similarity of results across both approaches suggests that pre-treatment mismatch is unlikely to be a source of bias. Thus, the placebo tests and synthetic DiD design provide complementary evidence that the parallel trends assumption holds, reducing concern that violations of this assumption are concealing a true effect.

4.6.2 Continuous Treatment Design

Lastly, I implement a continuous DiD using the actual distance from the centroid of each Indiana county to the Illinois border, rather than a binary treatment that classifies only border counties as treated. This design addresses a key limitation of the binary specification: counties located just beyond the border-county threshold are coded as untreated, even though their geographic and political environments may closely resemble those of the border counties themselves. Treating these “near-border” counties as if they are entirely unaffected introduces the risk of attenuating the treatment effects and biasing the estimates downward. By measuring distance in kilometers, the continuous specification captures meaningful variation in exposure intensity. It operationalizes the idea that the effect of proximity on Indiana’s abortion access should increase as distance from the Illinois border increases.

This approach provides a more nuanced test of whether proximity mobilizes voters, and mitigates the risk that an arbitrary treatment cutoff masks underlying variation in treatment exposure. In the

continuous DiD estimates reported in Table A.9 of the Appendix, I divide the calculated kilometers by 100 as using kilometers yields estimates and standard errors of zero. The results remain null, with an effect size of approximately one percentage point, which translates to a one percentage point change for every additional 100km farther from the Illinois border. This reinforces the conclusion that even when accounting for a gradient of exposure rather than a binary border definition, proximity to clinics did not significantly mobilize women voters in Indiana.

5 Why do women voters not turn out at higher rates when their access to abortion care is restricted?

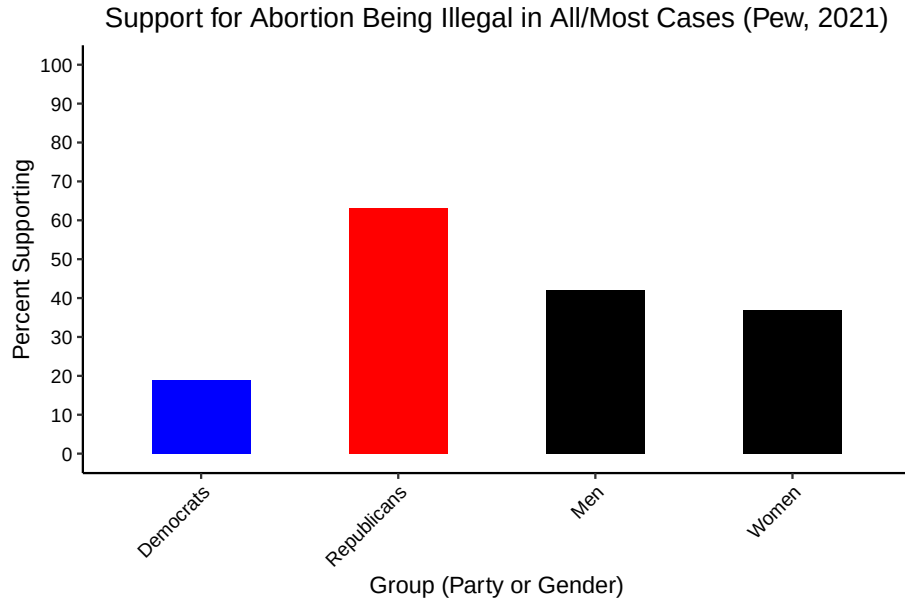
I have shown that access to reproductive healthcare does not substantively mobilize women in comparison to men, a finding that runs contrary to popular expectations and conventional wisdom that individuals should mobilize to defend their policy benefits (Campbell, 2003; Chen, 2013). Why are women not motivated to protect access to reproductive healthcare? In this section, I investigate two alternative explanations: one, individuals favoring abortion access increase turnout when restrictions occur,¹⁶ and two, individuals who hold abortion with sufficient importance are more likely to be mobilized. I do not find substantial evidence that Democrats turned out at higher rates due to abortion restrictions. Instead, I find that Democrats that held abortion as a highly important issue reported in surveys higher levels of motivation to turn out to the polls. But, as these Democrats were already likely politically engaged regular voters, restrictions on abortion did not meaningfully increase their participation.

5.1 Party, not gender, is a predictor of abortion attitudes

Descriptive evidence from Pew Research polling (2021) suggests that abortion opinions are strongly correlated with party rather than gender. Figure 5 shows the percent of men, women, Democrats, and

¹⁶Attitudes toward abortion are more strongly correlated with partisanship than with gender (Hartig, 2022; Hout, 2022), and similar dynamics have been observed in other politicized policy areas, such as wildfires and climate change. For example, voter mobilization in response to wildfire risk depends less on proximity to wildfire, and more on partisan identity (Hazlett and Mildenberger, 2020): while both Democratic and Republican voters face comparable risks of losing their homes,

Figure 5



Republicans, in support of a full abortion ban in 2021, prior to *Dobbs v Jackson*. The results indicate that opinions on abortion restrictions differ minimally across gender, with women reporting slightly more liberal views (Figure 5) compared to men. However, the supporting proportions diverge sharply along partisan lines, with about two thirds of Republicans supporting abortion bans, compared to only one fifth of Democrats. These survey averages indicate that partisanship rather than gender is a stronger determinant of attitudes toward abortion, and aligns with evidence showing that pro-choice and pro-life views are sharply divided along partisan lines (Hartig, 2022; Hout, 2022; Pew Research Center, 2025). Together, these survey responses suggest that abortion-related attitudes are split along party lines rather than exposure to the costs and benefits of the policy.

5.2 Attitudes as a Driver of Voter Mobilization

First, I examine whether abortion attitudes, proxied by party, mobilize voters. As abortion remains a highly contested and polarized issue, its politicization means it is often perceived less as a matter the extent to which this risk translates into political mobilization depends on individuals' beliefs regarding the reality of climate change.

of healthcare than as an ideological stance. Polling supports this view: Democratic voters ranked abortion as a top priority issue heading into the 2022 midterms, while Republicans reported virtually no change in prioritization, despite abortion restrictions aligning with high Republican vote shares (Schaeffer and Green, 2022). Since abortion attitudes are strongly correlated with partisanship — where Democrats are more likely to be pro-choice and Republicans are more likely to be pro-life — partisan differences in opinion may help explain why abortion restrictions do not mobilize women as a group. Although women are the primary beneficiaries of abortion access, pro-life women may not interpret restrictions on abortion access as an actual loss. As a result, restrictions may mobilize Democrats specifically, rather than women generally.

To test if abortion attitudes mobilize voters, I leverage the staggered timing of the 2022 primary elections and the *Dobbs* decision, to test a natural experiment.¹⁷ I aggregate voting behavior of all Democrats and Republicans nationwide from 2014 to 2022, focusing exclusively on primary elections. This analysis exploits the timing of state primaries relative to the June *Dobbs v. Jackson* decision in 2022, which is plausibly exogenous to primary dates. States with primaries before June 24th are classified as untreated, while states with primaries after June 24th are treated. This analysis tests whether general abortion attitudes, proxied generally by party affiliation, can drive voter mobilization in primaries. If abortion attitudes drive turnout, Democratic turnout should increase in states holding primaries after the *Dobbs* decision. In this section, I do not consider abortion laws by state: treatment is solely defined by the timing of the primaries relative to the 2022 decision.

I caution that turnout patterns in the 2022 primaries provide only suggestive evidence. Primary voters are not representative of the general electorate, turnout is measured relative to registered voters, and Figure 2 in the Appendix shows divergent pre- and post-*Dobbs* trajectories across states, raising concerns about the parallel trends assumption.

Initial estimates in Table A.10 of the Appendix show a positive and statistically significant post-*Dobbs* increase in Democratic primary turnout, but the effect attenuates and loses significance once pre-trends are accounted for; Republican estimates are smaller and similarly sensitive.¹⁸ These

¹⁷I also run additional analyses in the Appendix, in which I test the border county analysis only for Democrats. For further information, see section A.3.

¹⁸These results are echoed by Figure 3 and Table A.11 in the Appendix, which yield positive results for the primary turnout gap, however the estimate is not statistically significant.

results suggest that apparent primary mobilization likely reflects pre-existing state-level trends rather than a causal effect of *Dobbs*, leaving the primary turnout evidence inconclusive. Lastly, because abortion policy is highly politicized, turnout effects may depend on abortion attitudes in conjunction with threat to access, with Democratic women being the most likely group to mobilize. I test this mechanism in section A.3 using Democratic voters and Democratic women in Indiana border counties, I do not find substantive evidence that abortion attitudes interact with access to increase turnout.

5.3 Strength of Attitudes as a Driver of Voter Mobilization

As a second test, I examine whether the strength of abortion attitudes predicts political participation. While individuals may identify as pro-choice or pro-life (generally proxied by party), these positions may not be decisive for turnout unless they are held with sufficient importance. This may help explain why I find little evidence of Democratic mobilization. Party identification may be too broad a proxy for abortion attitudes: many Democrats may be pro-choice, but not all hold the issue with sufficient intensity or importance for it to affect turnout for low-turnout voters. By contrast, individuals who care deeply about abortion access may be more likely to mobilize in response to *Dobbs*. Thus, individuals for whom abortion is a highly salient issue (rather than Democrats overall) should be more likely to participate, particularly among voters whose preferences are not already reflected in policy outcomes.

In the Pew Research Center American Trends Panel from October 2022, respondents were asked how important abortion was to their vote choice (on a four-point scale), as well as how motivated they are to vote (on a five-point scale) and whether they plan to vote. I construct a binary turnout intention measure equal to one for respondents who report having already voted or planning to vote, and zero otherwise. The abortion importance variable is treated as ordinal, with higher values indicating greater issue salience. If a respondent considers abortion as a high-importance issue to their vote choice, I consider their abortion attitudes to stronger than a respondent who said that abortion is not a high-importance issue to their vote choice. The analysis is restricted to registered voters, and incorporates survey weights. Because these questions are only available in 2022, I

estimate cross-sectional OLS models with Census region fixed effects.

I first examine self-reported motivation to vote in Table 3. Among respondents who report that abortion is highly important, Democrats exhibit higher levels of voting motivation, while estimates for Republicans are small and statistically insignificant. I then consider reported voting plans, which show a similar pattern: abortion importance is positively associated with turnout intention among Democrats, but not among Republicans. Given that both the independent and dependent variables are ordinal and that survey-based turnout is often overstated, the magnitude of these estimates should be interpreted with caution.

These results suggest that while general abortion attitudes (proxied by party) do not appear to drive turnout, the intensity of those attitudes may play a role in mobilization — particularly among voters whose policy preferences are not already aligned with the status quo, such as Democrats. Democratic voters who hold abortion with sufficient importance to their vote choice are more likely to report higher rates of vote motivation, and are more likely to make a plan to vote, compared to Democrats who - while likely still pro-choice - do not hold the issue with sufficient importance.

How should the weak evidence for increased Democratic turnout in section 5.2 be reconciled with the positive estimates for abortion importance in this section? These results may reflect both measurement and ceiling effects. Party identification captures broad abortion attitudes, whereas abortion importance captures strength of attitudes. Yet the Pew 2022 survey was conducted among validated voters who already had prior voting records. Only about a quarter of 2020 voters in the survey had no record of voting in 2018 (Pew Research Center, 2022a); moreover, respondents reported high baseline levels of political motivation, with an average of 86 percent saying they planned to vote and, after recoding motivation as a binary instead of an ordinal measure, 88 percent saying they were motivated to vote. This suggests a ceiling effect, in which those who rated abortion as highly important were likely already habitual voters. As a result, abortion importance can predict turnout intention among already engaged Democrats without producing aggregate turnout gains among Democrats overall.

Table 3: Abortion Importance by Party

	Vote Motivation		Vote Plan	
	Democrats	Republicans	Democrats	Republicans
Abortion Importance	0.233*** (0.058)	-0.030 (0.028)	0.068*** (0.020)	-0.013 (0.011)
Region FE	Yes	Yes	Yes	Yes
Observations	1,501	1,328	1,534	1,368

Robust standard errors in parentheses. All models use probability weights.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Discussion and Conclusion

The *Dobbs v. Jackson* decision marked a historic shift in abortion rights policy, prompting widespread speculation about its influence on voter turnout, particularly among women. However, using a Difference-in-Differences model with the voter file, I find no evidence that increased barriers to abortion access significantly mobilized the affected voter base – i.e. women – to turn out in the 2022 midterms. These results suggest that access to abortion alone, despite its salience in popular media, may not function as an independent catalyst for political mobilization. I also find little evidence that broad abortion attitudes, proxied by party identification, mobilized voters overall.

This distinction has broader implications for theories of political behavior and policy feedback: material exposure to certain policy changes may be insufficient to generate participation absent attitudinal alignment and partisan cues. As states continue to diverge in abortion policy, understanding when and how access translates into political engagement will therefore require greater attention and further study.

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Online Appendix

Intended for online publication only.

A.1 Treatment Classification of States

Classification of Control and Treated States, based on the policy environment between June and November 2022:

Alabama: It is illegal to perform an abortion unless it is deemed medically necessary by a licensed physician and verified by a second physician within 180 days after the procedure. This legislation went into effect on June 24, 2022.

Classification: Treated

Alaska: Alaska's constitution protects the right to an abortion.

Classification: Untreated

Arizona: In March 2022, Arizona enacted a law banning abortions after 15 weeks of pregnancy. Following *Dobbs* on June 24, the state's attorney general sought to reinstate an 1864 statute that would have banned nearly all abortions. As a result, between June and the November elections, Arizona's abortion policy entered a period of legal uncertainty, marked by emergency motions, court challenges, and temporary blocks on enforcement.

Classification: Treated

Arkansas: All abortions are banned except to save the life of the mother. This legislation went into effect on June 24, 2022.

Classification: Treated

California: Abortion is permitted up to fetal viability.

Classification: Untreated

Colorado: Legislation passed in April 2022 guarantees the right to an abortion.

Classification: Untreated

Connecticut: Abortion is legal in the state of Connecticut.

Classification: Untreated

Delaware: Abortion is permitted up to fetal viability.

Classification: Untreated

District of Columbia: The right to an abortion is protected.

Classification: Untreated

Florida: In July 2022, legislation was enacted limiting abortions to 15 weeks, with exceptions for medical emergencies.

Classification: Treated

Georgia: After *Dobbs*, Georgia reverted to its 2019 heartbeat law, which bans abortions after six weeks. The law was enjoined until November 2022 amid ongoing court challenges.

Classification: Treated

Hawaii: Abortion is permitted up to fetal viability.

Classification: Untreated

Idaho: In August 2022, legislation banning most abortions, with limited exceptions, went into effect.

Classification: Treated

Illinois: Abortion is permitted up to fetal viability.

Classification: Untreated

Indiana: An abortion ban was enjoined and contested in court between June and November 2022; it ultimately went into effect in 2023.

Classification: Treated

Iowa: A 24-hour waiting period was enacted after the *Dobbs* decision, and constitutional protections around the right to an abortion were lowered in 2022. (A six-week ban went into effect in 2023.)

Classification: Untreated

Kansas: The state constitution protects the right to an abortion, and a ballot initiative to amend this protection failed in August 2022.

Classification: Untreated

Kentucky: A near-total abortion ban took effect on June 24, 2022, with limited exceptions.

Classification: Treated

Louisiana: A near-total abortion ban took effect on June 24, 2022, with limited exceptions.

Classification: Treated

Maine: Abortion is permitted up to fetal viability.

Classification: Untreated

Maryland: Abortion is permitted up to fetal viability.

Classification: Untreated

Michigan: Following *Dobbs*, Michigan's 1931 abortion ban was temporarily blocked by the courts and remained unenforced between June and November 2022.

Classification: Treated

Minnesota: The right to an abortion is recognized under the state constitution.

Classification: Untreated

Mississippi: Following *Dobbs*, abortion was banned with limited exceptions.

Classification: Treated

Missouri: An abortion ban went into effect following *Dobbs*, with an exception to save the life of the mother.

Classification: Treated

Montana: Abortion rights are protected up to fetal viability.

Classification: Untreated

Nebraska: Between June and November 2022, Nebraska law prohibited abortions after 20 weeks, imposed physician reporting requirements, and restricted telemedicine abortions (American Society for Reproductive Medicine, 2024).

Classification: Treated

Nevada: Abortion is permitted prior to 24 weeks of pregnancy.

Classification: Untreated

New Hampshire: Abortion is permitted up to 24 weeks of pregnancy.

Classification: Untreated

New Jersey: Pre-*Dobbs* statutory protections for abortion remained in place, and abortion remained legal throughout the period.

Classification: Untreated

New Mexico: Abortion remained legal.

Classification: Untreated

New York: The right to an abortion is guaranteed under state law.

Classification: Untreated

North Carolina: Following *Dobbs*, abortion restrictions tightened, with a ban after 20 weeks in place during this period.

Classification: Treated

North Dakota: A trigger ban prohibiting abortion took effect in June 2022 but was quickly contested in court; a new ban was enacted in 2023.

Classification: Treated

Ohio: Following *Dobbs*, a six-week abortion ban took effect but was halted during this period due to legal challenges.

Classification: Treated

Oklahoma: A near-total abortion ban went into effect on June 24, 2022.

Classification: Treated

Oregon: Abortion is protected as a right under state law.

Classification: Untreated

Pennsylvania: Abortion remained legal.

Classification: Untreated

Rhode Island: Abortion is recognized as a protected right.

Classification: Untreated

South Carolina: A six-week abortion ban took effect following *Dobbs*.

Classification: Treated

South Dakota: Abortion became a felony offense following *Dobbs*.

Classification: Treated

Tennessee: A near-total abortion ban took effect during this period.

Classification: Treated

Texas: A near-total abortion ban took effect during this period.

Classification: Treated

Utah: An 18-week abortion ban took effect in June 2022, while a full abortion ban was enjoined.

Classification: Treated

Vermont: Abortion remained legal.

Classification: Untreated

Virginia: Abortion is permitted up to fetal viability.

Classification: Untreated

Washington: Abortion is permitted up to fetal viability.

Classification: Untreated

West Virginia: A near-total abortion ban took effect in September 2022, with limited exceptions.

Classification: Treated

Wisconsin: An 1849 abortion ban went into effect, forcing abortion clinics to close during this period.

Classification: Treated

Wyoming: A trigger ban was in place but halted during this period; a temporary restraining order was issued in 2023.

Classification: Treated

A.2 Additional Tables

Table A.1: Difference-in-Differences Results: Excluding Contested States

	(1) Women's Turnout	(2) Turnout Gap	(3) Men's Turnout
Ban*2022	-0.022 (0.012)	0.007* (0.003)	-0.015 (0.010)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	200	200	200
Number of States	40	40	40

Standard errors are clustered at the state level. Ban is defined as a clear restriction on abortion access, such as a trigger ban or 6-week rule. States like Michigan or Indiana, which had abortion restrictions contested in courts in the period between June and November 2022, are excluded from this analysis.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2: Difference-in-Differences Results: Non-binary Treatment Design

	(1) Women's Turnout	(2) Turnout Gap	(3) Men's Turnout
Restriction Intensity*2022	-0.011 (0.006)	0.004* (0.001)	-0.007 (0.005)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	255	255	255
Number of States	51	51	51

Standard errors are clustered at the state level. Restriction Intensity can take a value of 0 for abortion protected states, 1 for contested states (medium intensity), and 2 for ban states (high intensity).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3: Difference-in-Differences Results: Sub-setting to Reproductive Age

	(1) Women Turnout	(2) Turnout Gap
Post*Treated	-0.021 (0.016)	0.004 (0.005)
State FE	Yes	Yes
Year FE	Yes	Yes
Observations	250	250
Number of States	50	50

Reproductive age is defined in this analysis as 18 to 45. Standard errors (in parentheses) are clustered at the state level. California is excluded due to data size constraints. The denominator for this analysis is registered voters instead of the Voting Age Population, as granular data on the population between 18 and 45 is not available year to year.

Table A.4: Difference-in-Differences Results: Indiana's Border Counties (Men's Turnout)

	Men's Turnout (1)	Men's Turnout (Midterms Only) (2)
NonBorder*2022	-0.012* (0.005)	-0.011* (0.005)
County FE	Yes	Yes
Year FE	Yes	Yes
Observations	644	368
Number of Counties	92	92

Standard errors in parentheses, and clustered at the county level.

Table A.5: Synthetic Difference-in-Differences Results: State-level Analysis

	Women's Turnout	Turnout Gap
Treated*Post	-0.012 (0.010)	0.006* (0.003)
Observations	255	255

Standard errors (Bootstrap) in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: Placebo Test Results: State-level Analysis

	(1) Women's Turnout	(2) Turnout Gap
Treated*Post	-0.016 (0.012)	0.006* (0.003)
treated_2018	-0.004 (0.008)	0.005* (0.002)
treated_2020	-0.006 (0.009)	-0.001 (0.002)
R-squared	0.946	0.893
Observations	255	255

Standard errors in parentheses. Includes FE, and clustered SE at the state level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.7: Placebo Test Results: County-level Analysis

	(1) Border Counties: Women's Turnout	(2) Border Counties: Turnout Gap
NonBorder*2022	-0.019** (0.006)	0.005 (0.003)
treated_2018	-0.001 (0.004)	-0.001 (0.004)
treated_2020	-0.009 (0.007)	0.001 (0.005)
Clinic*2022		
R-squared	0.959	0.901
Observations	644	644

Standard errors in parentheses. Includes FE, and clustered SE at the county-level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8: Synthetic Difference-in-Differences Results: Indiana's Border Counties

	Women's Turnout	Turnout Gap
NonBorder*2022	-0.014* (0.006)	0.004* (0.002)
Observations	644	644

Standard errors (Bootstrap) in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10: Difference-in-Differences Primaries Results by Party

	(1)	(2)	(3)	(4)
	Democrat	Democrat	Republican	Republican
	Turnout	Turnout	Turnout	Turnout
Primary*2022	0.066**	0.037	0.047*	0.020
	(0.023)	(0.034)	(0.021)	(0.035)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pretrend Adj.	No	Yes	No	Yes
Observations	245	245	245	245
States	49	49	49	49

LA and CA are excluded because of data availability purposes. DC is included. Standard errors in parentheses, and clustered by state. State and year fixed effects included. Primary is the treatment variable, which takes a value of 1 if the 2022 state primary is after Dobbs, 0 otherwise. 2022 is the single Post year.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.9: Continuous DiD Results: Distance to Illinois Border

	Women's Turnout	Turnout Gap
Distance*2022	-0.001	0.001
	(0.004)	(0.001)
Observations	644	644

Standard errors in parentheses. Distance measured in 100 km.

Table A.11: Primary Difference-in-Differences Results: Turnout Gap

	(1)
	Primary Turnout
Primary*2022	0.019
	(0.019)
State FE	Yes
Year FE	Yes
States	49
Observations	245

Standard errors in parentheses, clustered at the state level. Fixed effects for state and year included. Primary is a binary variable indicating whether the primary took place after Dobbs. 2022 is the sole treated year.

A.3 Additional Analyses

Attitudes and Access in Indiana:

Given that abortion policy is both highly contentious and closely aligned with partisan divisions, null estimates for restrictive states may indicate that only pro-choice individuals, who are primarily Democrats, are mobilized by restrictions on abortion access. In other words, voter turnout may depend on individuals' attitudes about abortion *in conjunction* with perceived threat to access. Therefore, increases in mobilization should occur among Democratic women in areas where abortion access is threatened (as opposed to Democrats everywhere). Republican voters are unlikely to perceive restrictions as a threat to reproductive healthcare, whereas Democratic voters are likely to interpret them as an attack on abortion rights.¹⁹ This approach captures both potential dimensions of mobilization: the ideological salience of abortion and the tangible loss of local access.

To test this mechanism, I re-estimate the border county regressions within Indiana, restricting the sample to voters registered with the Democratic party.²⁰ Table 3 reports results from the border county analysis, and yields null results for both Democrats and Democratic women, with an effect size very close to zero. One likely explanation is that these counties are not particularly liberal (since the border counties were red in the last election (Indiana Election Division, 2020)), and thus may contain more moderate Democrats for whom abortion is a less salient political issue. However, the fact that a mobilization effect is not observed in Indiana suggests that attitudes do not operate in conjunction with access to mobilize voters.

A.4 Additional Figures

¹⁹Despite Republicans' reported opposition to abortion across surveys (CES; Pew), abortion rates remain relatively similar across treated and untreated counties, suggesting that self-identified pro-life individuals may still access abortion-related care when needed.

²⁰In this analysis, turnout is measured as the share of registered voters that are associated with a party, since data on the voting-age population by party at the county level are unavailable. This measure includes individuals who switched party registration or newly registered between the *Dobbs* decision and the 2022 midterm elections. Although descriptive evidence documents a post-*Dobbs* spike in voter registration (Catalist, 2022), this surge did not translate into higher turnout. Consequently, using registered voters as the denominator may underestimate overall turnout levels, biasing against finding a mobilizing effect.

Table A.12: County-level Difference-in-Differences Results: Mechanism Test

	Democratic Turnout (Border)	Democratic Women Turnout (Border)
Non-Border*2022	-0.000 (0.013)	-0.004 (0.012)
Number of Counties	92	92
Observations	644	644
County FE	Yes	Yes
Year FE	Yes	Yes

Standard errors in parentheses and clustered at the county level. Non-border counties are treated, 0 otherwise. Post = 1 in 2022, and 0 otherwise.

Figure 1

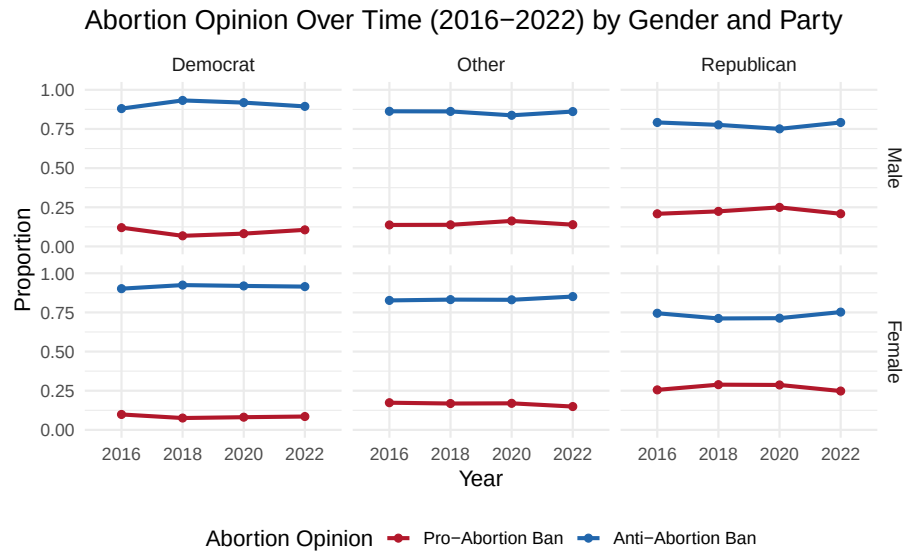
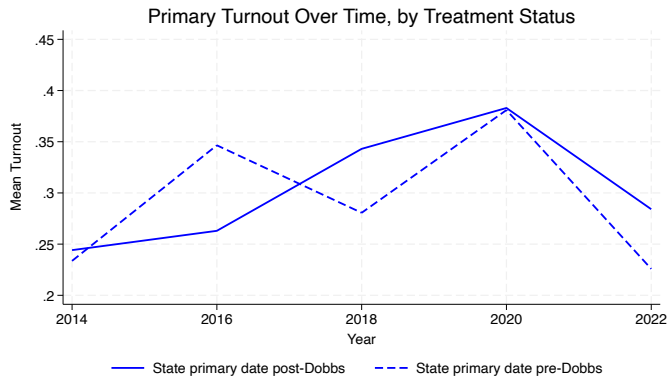
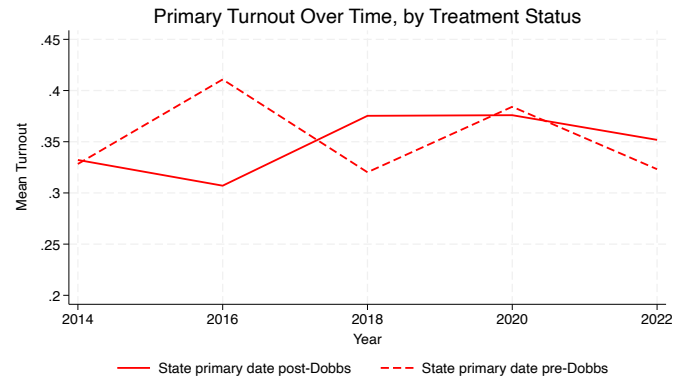


Figure 2: Democratic (left) and Republican (right) Primary Turnout Over Time by Treatment Status



(a) Democratic Primary Turnout



(b) Republican Primary Turnout

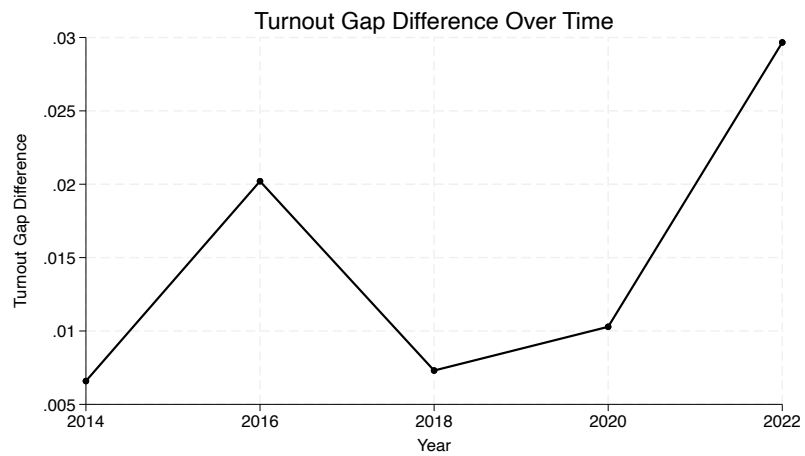


Figure 3: Turnout Gap Differences Over Time