Roe v. Rates:
Reproductive Healthcare and Public Financing Costs

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Abstract

After the U.S. Supreme Court overturned Roe v. Wade, states enacting near-total abortion bans experience an increase in municipal bond yields relative to states with preexisting laws protecting abortion. The effects are stronger in counties with higher ex-ante utilization of abortion services, sharper ex-post declines in access to abortion, greater public support for abortion rights, and higher dependence on female labor force participation. Using the stock market's reaction to abortion bans following the overturning of Roe v. Wade and the staggered state-level adoption of laws restricting abortion providers in earlier years, we identify negatively impacted firms and reduced net in-migration as key factors underlying the rise in yields. Together, our results highlight the importance of reproductive healthcare, in particular abortion policies, in driving public financing costs.

Keywords: Reproductive healthcare, TRAP laws, Public finance, Municipal bonds
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1. Introduction

Social policies in the U.S. are increasingly polarizing and often fuel heated debates. From affirmative action in education and employment to gun control and LGBTQ+ rights, social policies are typically framed within legal, political, and moral contexts. However, these policies can have spillover effects on economies, for example via their impact on businesses and labor markets.

The right to access reproductive healthcare, especially abortion, is a particular case in point. Since the landmark decision Roe v. Wade (1973), which legalized abortion nationwide, states have diverged sharply in abortion policies, with some imposing stringent abortion restrictions while others enshrining protections for abortion rights.[1] These policies could have ramifications beyond the legal and political realms, leading to major impacts on local economies. For instance, following the overturning of Roe v. Wade in 2022, some of the biggest businesses in the US announced plans to relocate operations or allow employees to transfer from states imposing abortion bans.[2] Moreover, the availability of abortion services has been shown to play a crucial role in advancing gender equality and enhancing women's economic well-being, influencing their educational investment, labor force participation, and engagement in entrepreneurship (Ananat et al., 2009, Zandberg, 2021, Ravid and Zandberg, 2022).

In this paper, we explore the economic spillovers of restrictive abortion policies, with a focus on their effects in the municipal bond market. By doing so, we offer a market-based assessment of the economic impacts of these policies, providing critical insights into their broader implications on communities and regional economies.

We exploit variations in state laws on abortion around the overturning of Roe v. Wade to show that restricting access to abortion increases public financing costs. We find that states implementing near-total abortion bans during this period see an increase in municipal bond yields relative to states with preexisting laws protecting abortion. The effects are stronger in counties with a greater ex-ante utilization of abortion services and sharper ex-post declines

[1] See Guttmacher Institute (2021a) and Kaiser Family Foundation (2022a).
[2] See CNBC (2022), CBS (2022a), Washington Post (2022), Financial Times (2022).

in access to abortion, greater public support for abortion rights, and higher dependence on female labor force participation. We further explore channels underlying the rise in municipal bond yields. Using the stock market's reaction to abortion bans following the overturning of Roe v. Wade and the staggered state-level adoption of laws restricting abortion providers in earlier years, we identify negatively impacted firms and reduced net in-migration as key factors underlying the rise in yields.

In December 2021, the U.S. Supreme Court heard Dobbs v. Jackson Women's Health Organization, which challenges a 2018 Mississippi law banning abortion at 15 weeks of pregnancy, to determine whether "all pre-viability prohibitions on elective abortions are unconstitutional" (Kaiser Family Foundation, 2022b). The Court was expected to issue a decision by mid-2022. However, on May 2, 2022, a draft Supreme Court opinion overturning Roe v. Wade was leaked and published (Politico, 2022). Roughly two months later, on June 24, 2022, the Court held that the "Constitution does not confer a right to abortion," overruling Roe v. Wade and returning to states the power to regulate abortion. Sixteen states and D.C. had state laws protecting the right to abortion without relying on the Roe decision and should see no change in access to abortion after the ruling. In contrast, in the year following the Dobbs decision, 13 states implemented near-total bans on abortion, prohibiting abortion with limited or no exceptions.[3] We exploit this difference in state laws and the timing of abortion bans to study the effect of restricting access to abortion on municipal bond yields and local economies.

Using a dynamic difference-in-differences (DID) event study design (Sun and Abraham, 2020), we compare municipal bond yields before vs. after states impose near-total abortion bans ("ban states"), using bond yields in states where abortion is protected by preexisting state laws during the same time period as control ("protective states"). We find that secondary-market municipal bond yields in ban states on average increase by 6.3 basis points (bps) in the six months following the bans relative to those in protective states, approximately 2% of the average bond yields over this sample period. Importantly, ban states and protective states show parallel trends in yields before the abortion bans. We also document a relative increase of 19.5 bps in primary market offering yields in ban states, translating

[3]We use "Dobbs decision" and "overturning of Roe v. Wade" interchangeably in the paper.

into a total of $185 million in additional interest payments over the lifetime of bonds per ban state.[4] Our results are robust to controlling for state economic conditions, state tax cuts, and gubernatorial election status in 2022, as well as alternative definitions of treatment states and regression specifications.

While areas within a state are all affected after an abortion ban is implemented, areas' exposure depends on the ex-ante utilization of abortion services, change in access to these services, local attitudes toward abortion, and local reliance on female workforce. To further tighten the link between abortion policies and municipal bond yields, we first use the pre-Dobbs abortion to live birth ratio and the post-Dobbs change in the distance to the nearest abortion clinic to identify counties that are more vs. less affected and study their differential responses. We find that while yields in both more and less affected counties increase following abortion bans, the rise in more affected counties is nearly twice as large. In addition, we gauge local residents' predispositions toward abortion using the share of the county population identifying as religious and the percentage of Gallup survey respondents who identify themselves as pro-choice, view abortions as morally acceptable, or consider abortions to be legal in all or most cases. We find that the relative increase in yields is concentrated in treated counties with a stronger public support for abortion: those with a lower share of religious population and those with residents holding a more favorable view of abortion. Finally, we measure local reliance on female workforce using the female labor force participation rate and the share of employment in female-dominated industries at the county level. The relative increase in yields is indeed stronger in treated counties that depend more heavily on female workers.

To investigate the generalizability of our findings from the abortion bans around the Dobbs decision and to explore the longer-term effects of restrictive abortion policies, we turn to Targeted Regulation of Abortion Providers (TRAP) laws. These laws were adopted at different times by various states during the 2000s and 2010s. They impose stringent regulations on abortion providers and have been shown to reduce abortion (e.g., Arnold, 2022, Jones and Pineda-Torres, 2021). We use a dynamic DID event study comparing municipal

[4]The $185 million is calculated as 19.5 bps $\times$ 10 years (the average bond life) $\times$ $9.5 trillion (the average annual bond issuance amount for all issuers within a state over 2018–2022).

bond yields in states before and after their first enactment of TRAP laws, using yields in states that never enact one during the same period as controls. We document an average of 3.5 bps increase in yields in the three years following a state's enactment of TRAP laws relative to the control states. The consistency in findings between near-total abortion bans and TRAP laws suggest that these yield effects are likely generalizable to other policies that restrict access to abortion.

As a final set of analyses, we investigate potential channels underlying the increase in municipal bond yields. First, we explore how restrictive abortion policies negatively affect local *firms*, thereby contributing to yield increase. We find that the value of firms falls after the states of their headquarters implement near-total abortion bans: cumulative abnormal returns (CARs) of these firms are -2 percentage point (pp) over the 20 days after the ban implementations. Consistent with the decreased firm value, we find that firms per capita and net job creation fall after enactments of TRAP laws using data from the Business Dynamics Statistics. Next, we investigate the negative impact of restrictive abortion policies on local *residents* and its possible contribution to yield increase. Using state-to-state migration based on IRS tax filings and LEHD job-to-job flows by age and sex, we show that states enacting TRAP laws experience a decline in net in-migration, primarily driven by decreased inflow, and a consistent drop in net job inflow among fertile women, who are likely more affected by abortion policies. Together, our results imply that restrictive abortion policies—whether near-total abortion bans or stringent requirements on abortion providers—adversely affect businesses and reduce net in-migration, potentially deteriorating local economy, shrinking local tax bases, and contributing to higher public financing costs.

Our paper relates to several strands of literature. First, we contribute to a growing literature exploring the implications of social issues and related policies in public finance. Prior papers have shown that social issues and policies could affect public financing costs through their effect on cash flows and tax base (Cornaggia et al., 2022a, Cheng et al., 2023, Gao et al., 2022), intermediary and issuance process (Dougal et al., 2019, Garrett and Ivanov, 2022), and clientele effect (Baker et al., 2022, Dougal et al., 2019). We show that abortion policies, one of the most heatedly debated social policies, affect public finances via their impact on local firms and residents.

We also contribute to a nascent literature studying the role of public health factors in public finance, such as Medicaid expansion, marijuana legalization, opioid abuse, telehealth provision, and aging population (Butler and Yi, 2022, Cheng et al., 2023, Cornaggia et al., 2022a,b, Gao et al., 2022). We contribute by quantifying the effect of a novel health factor—access to abortion—on financing costs for state and local governments.[5] Given the important role public financing plays in stimulating local economies (e.g., Adelino et al., 2017, Dagostino, 2018), the effects of restrictive abortion policies on municipal bond yields could have long-lasting impact on regional economic development and reshape the lines of economic competition between states with different policies pertaining to abortion.

Our results also add to a broad literature studying the socioeconomic effects of access to reproductive healthcare. The right to access reproductive services plays a critical role in advancing gender equality and enhancing women's economic well-being. Prior findings have shown that improved access to contraception and legal abortion reduces birthrates, delays fertility and family formation, and increases women's educational investment, labor force participation, and entrepreneurship (Ananat et al., 2007, 2009, Angrist and Evans, 1996, Arnold, 2022, Bailey, 2006, 2010, Bitler and Zavodny, 2001, Bloom et al., 2009, Goldin and Katz, 2002, Guldi, 2008, Jones and Pineda-Torres, 2021, Myers, 2017, Ravid and Zandberg, 2022, Zandberg, 2021). In contrast, restricting or denying access to abortion services has been shown to induce financial distress of the denied women (Miller et al., 2023) and negatively impact the next generation's education and labor market performance (Pop-Eleches, 2006). These studies provide valuable insights into how access to reproductive healthcare affects *individuals'* well-being. Building upon these studies, we present the first evidence of the *aggregate* economic consequences of restricting access to abortion.

More generally, our paper relates to the literature examining the effect of family policies that aim at lowering barriers for women to enter or remain in labor force, such as paid maternity leave and subsidized childcare (e.g., Bennett et al., 2020, Gottlieb et al., 2022, Liu

[5]Our findings also relate to a broad literature examining determinants of public financing costs, including liquidity and default risks (Ang et al., 2014, Schwert, 2017), tax policy (Ang et al., 2010, Babina et al., 2021, Garrett et al., 2017, Longstaff, 2011, Schultz, 2012), corporate subsidies and bankruptcies (Chava et al., 2023, 2022), political connections (Butler et al., 2009), the information environment (Cuny, 2018, Farrell et al., 2023, Gao et al., 2020), climate change (Goldsmith-Pinkham et al., 2021, Painter, 2020), policies and political uncertainty (Gao et al., 2019a,b, 2024), underwriting processes (Garrett, 2021), and COVID-induced migration (Gustafson et al., 2023).

et al., 2023, Simintzi et al., 2023). Reproductive services, similar to these policies, have the potential to foster the advancement of women in labor force and alleviate the underrepresentation of women in corporate leadership (He and Whited, 2023). In addition, we document that both the general population and women of childbearing age adjust their migration decisions in response to abortion policies. By showing that polarizing social policies could lead to geographic sorting, we add to an emerging literature studying the causes of geographic polarization (e.g., Bernstein et al., 2022, McCartney et al., 2021).

Taken together, our results demonstrate the economic impact of restricting access to abortion. Reproductive healthcare policies, particularly abortion policies, not only have the intended effects on reproductive behaviors, but also can affect public financing and geographic sorting, leading to divergence in economic development across regions.

2. Institutional Background and Data

2.1 Reproductive rights in the United States

Before 1973, abortion regulation and enforcement in the U.S. was left up to states. While legal abortion became widely available in Alaska, Hawaii, New York, and Washington in 1970, it was outlawed in other states except to save a woman's life or for limited reasons such as rape or incest. The legal landscape regarding abortion changed in 1973. In its decision for Roe v. Wade, 410 U.S. 113 (1973), the Supreme Court held that abortion access was a right protected by the U.S. Constitution, rendering antiabortion laws unconstitutional and legalizing abortion nationwide.

This landmark decision soon sparked a decades-long legal and political battle over abortion rights across the nation. Antiabortion lawmakers fought hard to limit abortion access. For example, dozens of states have passed Targeted Regulation of Abortion Providers (TRAP) laws that impose stringent, often medically unnecessary, requirements on abortion providers (Guttmacher Institute, 2021b). In more recent years, antiabortion policymakers began enacting policies that directly contradict Roe, e.g., prohibiting abortion before fetus viability or roughly 24 weeks of pregnancy, to provoke a Supreme Court challenge to the precedent. One prominent example is a 2018 Mississippi law that bans abortion at 15 weeks of pregnancy. In May 2021, the U.S. Supreme Court granted a review of Dobbs v. Jackson Women's Health Organization, which challenges the Mississippi abortion law, to examine whether "all pre-viability prohibitions on elective abortions are unconstitutional" (Kaiser Family Foundation, 2022b). The case was heard in December 2021, and the Court was expected to issue a decision by mid-2022. However, on the night of May 2, 2022, Politico published a leaked draft majority opinion written by Justice Samuel Alito, indicating the Court had voted to strike down Roe v. Wade (Politico, 2022). Although the leaked document greatly increased the likelihood of an eventual overturn of Roe, the decision remained uncertain as justices can and sometimes do change their votes as draft opinions circulate and undergo multiple amendments. Finally, on June 24, 2022, the Supreme Court issued an official ruling and held that the "Constitution does not confer a right to abortion," ending a half-century of federal constitutional protection of abortion rights and allowing each state to decide whether to protect, restrict, or ban abortion.

Sixteen states and D.C. already have laws in place that explicitly protect the right to abortion without relying on the Roe decision (Kaiser Family Foundation, 2022a). Most of these laws prohibit states from interfering with pre-viability abortion, and others offer legal protections for abortion providers. In contrast, 13 state legislatures have instituted "trigger" laws designed to take effect automatically or by quick state action to ban abortion with few exceptions once the Roe precedent is struck down. Additionally, nine states have pre-Roe abortion bans on the books that were rendered unenforceable after the Roe decision but could be reinstated if the decision is overruled.[6]

These *preexisting* differences in state laws on abortion create variations in state-level abortion access *after* Roe was overturned: states with laws protecting abortion should see little change in access to abortion while those with trigger or pre-Roe abortion bans should experience a large reduction. Indeed, in the year following the Dobbs decision, 13 out of the 18 states with either trigger or pre-Roe abortion bans ended up implementing near-total bans on abortion, which prohibit abortions with limited or no exceptions.[7] Appendix Figure A1

[6] In states with multiple bans, state officials will determine which ban to enforce when Roe is overruled. See Guttmacher Institute (2022) for more details.

[7] Note that Oklahoma implemented a near-total abortion ban in May 2022 right before the Dobbs decision. The law was able to remain unblocked because it delegated enforcement to private citizens instead of state officials.

plots the cumulative number of states implementing near-total abortion bans over time, and Appendix Table A1 lists these states.

We exploit these differences and the timing of abortion bans to study the economic implications of restricting access to abortion. Control states are defined as those with preexisting state laws protecting abortion independent of the Roe decision (hereafter, "protective states"). Treated states are those that implemented near-total bans on abortion from 2022 through June 2023 (hereafter, "ban states").[8] For robustness, we expand the treatment definition to include states with six-week gestational bans, which prohibit abortions after approximately six weeks of pregnancy or detection of "cardiac activity" in the embryo.

2.2 Potential impact on municipal bonds

Municipal bond investors are primarily concerned with the cash flows of local governments, as these determine governments' ability to meet debt obligations. Given the payoff structure of municipal bond investments, investors would focus on policies that generate downside risk for the communities providing the cash flows for debt payment. Abortion policies may influence municipal bond pricing through various channels.

Policies that restrict abortion access can negatively impact two important determinants of a state's tax base: residents and businesses. First, limiting access to abortion may prompt individuals who value reproductive rights to relocate, thereby reducing the local labor supply. For example, in the wake of the Dobbs decision and ensuing abortion bans, nearly half of working adults said they would consider moving to abortion-friendly states in a survey commissioned by Bloomberg (2022); indeed, Lin et al. (2023) find that female auditors who value reproductive rights move away from states that enacted TRAP laws. Studies have also shown that access to abortion affects female labor force participation and their engagement in professional occupations and entrepreneurship (e.g., Zandberg, 2021, Ravid and Zandberg, 2022). Second, restricting abortion may prompt firms to relocate and increase their financial burden. For example, following the Dobbs decision, many U.S. companies

[8]Our municipal bond data end in December 2023. To ensure at least six months of post-ban period, we only consider bans that went into effect by June 2023; the last treated state in the main sample is North Dakota, which implemented its near-total ban in May 2023. States like Indian, whose ban was implemented in August 2023, are not included.

announced they would consider expanding or allowing employees to move away from states with abortion bans (CNBC, 2022), which could generate costs associated with employee relocation and operational disruptions. In addition, many firms remaining in states that ban abortions said they would cover travel costs for employees who need abortion services (CBS, 2022b), and some faced backlash from existing employees and had to increase posted wages after announcing these travel benefits (Adrjan et al., 2023). This exodus of talent and businesses, combined with increased operating costs, likely shrinks state tax bases, constrains government fiscal capacity, and negatively affects municipal bonds whose sources of repayment are tied to local economic conditions. In Section 5, we explore these channels in detail.

In addition, diminished access to abortion could increase burdens on social welfare programs and strain public resources and spending. Abortion bans can lead to a surge in unplanned pregnancies, resulting in extra medical expenses related to prenatal care, childbirth, and postpartum recovery, a significant proportion of which are covered by Medicaid, which relies on both federal and state funding (Kaiser Family Foundation, 2022c). Limiting access to abortion services can also give rise to public health issues stemming from inadequate care, increased rates of pregnancy complications, and elevated rates of preterm births, as well as infant and maternal mortality (Stevenson et al., 2022).

If investors view restrictive abortion policies as a potential risk of investment, they are likely to demand a higher yield to compensate for the additional risk. This gives rise to the main hypothesis of the paper: municipal bonds in states restricting access to abortion will on average have higher yields.

2.3 Bond Data and Summary Statistics

We obtain municipal bond data from two sources: secondary market transaction data from the Municipal Securities Rulemaking Board (MSRB) and primary market issuance data and bond characteristics from the Mergent Municipal Fixed Income database (Mergent).

MSRB covers the universe of secondary market transactions for municipal bonds in the U.S. and includes transaction-level information such as the transaction date, yield, price, and trade size. Our analysis utilizes the secondary market transaction data spanning the period from 2000 to 2023.[9] Following the literature, we restrict our sample to tax-exempt bonds issued in the 50 states and D.C., transactions beyond two weeks of issuance to exclude primary market issuance transactions (Schultz, 2012), and transactions with at least a one-year time-to-maturity to prevent small price deviations from generating large price swings (Schwert, 2017). We also construct a sample of primary market issuance data using the Mergent database of bonds offered via conventional channels with non-missing offering amounts and coupon rates in the 50 states and D.C. We geolocate each bond based on the issuer's county information using the first six digits of the 9-digit bond CUSIP that uniquely identifies an issuer.

Our main measure of public financing cost is municipal bond yield in secondary markets. We calculate the monthly bond yield using the trade size-weighted average yield across all transactions for each bond in a given month. We include all bonds issued by various types of state and local municipal issuers, such as states, counties, cities, townships, school districts, and special districts. We winsorize yields in the top and bottom 0.5 percentiles to prevent outliers from driving our estimates. As an alternative measure, we use monthly bond spread computed as the size-weighted average difference between bond yield and the corresponding maturity-matched after-tax Treasury yield.[10] We also examine bond credit ratings to gauge whether and how credit rating agencies adjust their assessments of bond default risks.[11]

We present summary statistics of the main sample in Table 1. Panel A reports bond characteristics for all bonds in the secondary-market sample from December 2021 through November 2023.[12] We observe that the average yield is about 315 bps, the average spread between bond yield and the corresponding Treasury yield is 99 bps, average time to maturity

[9]We obtained municipal bond transaction data from 2005 to 2023 via WRDS MSRB and acquired pre-2005 trade data directly from MSRB EMMA.

[10]To calculate the after-tax Treasury yield, we measure marginal tax rates using estimates of top state rates based on the NBER Taxism model. For information about state rates, see http://users.nber.org/~taxsim/state-rates/maxrate.html. For details about the Taxism model, see http://users.nber.org/~taxsim/state-rates/.

[11]We use the credit rating at the time of each transaction for the secondary market analysis. We supplement Mergent rating data with rating histories provided by Ryan Israelsen and Marc Joffe. When rating information is available from multiple rating agencies, we use the lowest one.

[12]We start the main sample in December 2021 to ensure the earliest treated state Oklahoma, which implemented its near-total abortion ban in late May 2022 (coded as June 2022 treatment because the effective date is after 15th of the month), has six months of pre-ban period. We end the sample in November 2023 so that our last treated state North Dakota, which implemented its ban in late April 2023 (coded as May 2023 treatment), has six months of post-ban period.

is 6.5 years, and average rating is 18 (equivalent to AA- or Aa3 rating). In addition, about 66% of transactions pertain to callable bonds, 14% insured bonds, 49% general obligation bonds, and 55% bonds issued through negotiation (instead of competitive offerings).[13]

Panel B compares characteristics of bonds issued in ban states with those issued in protective states from beginning of our sample till the month before the Dobbs decision. While there are more transactions in protective states than in ban states (287,796 vs. 124,228), the average yields and spreads are similar between the two groups (221 and 105 bps in ban states vs. 218 and 102 bps in protective states), and the difference is statistically insignificant. In addition, the average bond ratings are nearly identical, suggesting that issuers in these states share similar creditworthiness and debt obligations before the shock. Bonds in ban states and in protective states also share similar maturity, callability, and insurance status. However, bonds in ban states are less likely to be issued through negotiation. We control for all these bond characteristics in regressions where applicable.

3. The Effects of Abortion Bans

3.1 Dynamic DID event study design

Since states imposed near-total abortion bans at different times, we use a dynamic DID event study (Sun and Abraham, 2020) to examine the effects of abortion bans on bond yields. Specifically, we compare bond yields before vs. after a state implemented its ban, using bond yields in states where abortion is protected by preexisting state laws during the same time period as control. To implement the dynamic event study, we stack our panel data as a series of 2×2 matrices (bonds in ban/protective states × omitted period/event period) and adapt the R package from Novgorodsky and Setzler (2019).

We define states that implement abortion bans in year-month g as cohort g and cohort-specific event time in calendar year-month ym as $e_g = ym - g$. We run the following

[13]For straight bonds, maturity is defined as the time remaining until the bond's maturity date. For callable bonds, maturity is measured as the time remaining until the next call date.

regression:

$$
\begin{aligned}
Y_{i,ym} = \sum_{g \in G} 1\{cohort = g\} \times [\sum_{e \neq -1} \beta_{e_g} Ban_{s(i),g} 1\{e = y - g\} \\
+ \sum_{e \neq -1} \delta'_{e_g} X_{i,ym} 1\{e = y - g\} + \alpha_{e_g,g} + \alpha_{c(i),g}] + \epsilon_{i,ym},
\end{aligned} \tag{1}
$$

where $Y_{i,ym}$ is the outcome of interest, such as yield, spread, and rating, for bond i in year-month ym. G is the set of treatment cohorts based on the effective date of states' abortion bans: the month when a law goes into effect if the effective day is on or before the 15th or the following month if the day is afterwards.[14] To balance the length of post-period and the number of treatment cohorts included while ensuring a balanced sample in event time, we estimate effects for event months -6 through +6. We define the omitted period as -1, i.e., the month right before the implementation of a ban. $Ban_{s(i),g}$ takes a value of 1 if bond i is issued by state s belonging to cohort g, and cohort g is the treatment cohort. Following Gao et al. (2020) and Cornaggia et al. (2022a), we control for time-varying bond characteristics $X_{i,ym}$, including bond rating at the time of transaction, log maturity, log size, and coupon rate, as well as indicators for whether a bond is categorized as general obligation, callable, insured, reoffered, and negotiated.[15] We include cohort-specific event month fixed effects $\alpha_{e_g,g}$ to absorb national time trends in the bond market, and cohort-specific county fixed effects $\alpha_{c(i),g}$ to account for time-invariant county-level bond market conditions. In the most demanding specification, we control for cohort-specific bond fixed effects $\alpha_{i,g}$ to force comparison within the same bond.

Assuming that yields in states with abortion bans and states with preexisting laws protecting abortion would have shared similar trends absent the abortion bans, we can identify β_{e_g}, the treatment effect on bond yields in treated cohort g in event time e_g. Following Sun

[14] For example, Oklahoma's near-total abortion ban went into effect on May 25, 2022, so its treatment timing is coded as June 2022. Results are very similar if we instead code the treatment timing as the month when a law takes effect regardless of the day.

[15] For insured bonds, rating refers to the higher of the underlying and the insured ratings; for uninsured bonds, the rating is the underlying rating.

and Abraham (2020), we define the average treatment effect for event time e as:

$$\beta_e = \sum_{g \in G} \beta_{e_g} \times w_g \tag{2}$$

where w_g (the aggregation weight) is based on the number of observations used to estimate β_{e_g}. We calculate standard errors clustered by state for β_e via the delta method to account for the state-level roll out of abortion bans and time-series correlations in the residual terms within each state.

Figure 1 plots the β_e estimates, capturing how abortion bans change secondary-market municipal bond yields for ban states relative to protective states over the event window. First of all, there is no discernible difference in trends between ban states and protective states leading up to the implementation of abortion bans, suggesting that the parallel pretrend assumption likely holds in our context. Upon the implementation of abortion bans, we see a jump in yields in ban states relative to protective states. The gap in yields continue to grow in the following months with no sign of reversal. By the end of the event window, yields in ban states have on average increased by 11.7 bps compared to those in protective states. Appendix Table A2 reports the estimated coefficients.

Our sample period coincides with an unprecedented interest rate hike in the US, raising concerns that changes in the interest rate term structure may confound our estimates. To alleviate this concern, in Appendix Figure A2, we use the spread between yield and maturity-matched after-tax Treasury rate as an alternative outcome, or additionally control for log(maturity) $\times$ year-month indicators or maturity bins $\times$ year-month fixed effects. Patterns are very similar to before. Given this, we use bond yields as the main outcome in the remainder of the paper.

3.2 Average post effect

To estimate an average treatment effect over the months following near-total abortion bans while accounting for its dynamic nature, we define the average treatment effect as:

$$\beta = \sum_{g \in G} \sum_{e=1}^{e=6} \beta_{e_g} \times w_{g,post} - \sum_{g \in G} \sum_{e=-6}^{e=-1} \beta_{e_g} \times w_{g,pre} \tag{3}$$

where β_{e_g} is estimated from Eq. 1, and $w_{g,post}$ (or $w_{g,pre}$) refers to the aggregation weight for the post-period (or pre-period) based on the number of observations used to estimate β_{e_g} for e=1 to 6 (or e=-6 to -1). We leave e=0 out from the post-period to ensure that all months included are fully treated. Note that β_{-1_g} is 0 by default since we omit -1 and use it as the reference period. We calculate standard errors clustered by state for β via the delta method.

Table 2 column (1) reports the average treatment effect for our main specification. It shows that in the six months following the implementation of abortion bans, yields in ban states increase by an average of 6.3 bps relative to those in protective states. This magnitude represents approximately 2% of the average bond yield in the sample. In columns (2)-(5), we control for potential confounders one by one to ensure the estimated effect is not driven by these confounding factors. First, in column (2) we include cohort-specific lagged-one-period state monthly unemployment rate and quarterly GDP to ensure our effect is not confounded by changes in economic conditions unrelated to abortion bans. Next, 36 states had gubernatorial elections in 2022, and the Dobbs decision has been shown to mobilize women and Democratic voters (Sommer et al., 2023). To account for the increased political uncertainty due to changes in the abortion legal landscape, we include cohort-specific indicator of state governor election in 2022 × event month fixed effects in column (3). Moreover, several states enacted state income tax cuts in 2022, which might impact future municipal cash flows and confound our main estimates. To mitigate this concern, we control for cohort-specific indicator of state tax cut in 2022 × event month fixed effects in column (4). Finally, in column (5), we replace cohort-specific county fixed effects to cohort-specific bond fixed effects. The results are generally robust across specifications. In short, our effect cannot be solely explained by state economic conditions, political uncertainty due to governor elections, state income tax cuts, or change in bond composition.

3.3 Cross-sectional heterogeneity

Our analyses thus far leverage variations in abortion laws *across states*. In this section, we examine variations in exposure to abortion bans *within states* to further tighten the link between law changes and the increase in public financing costs. Specifically, we use local utilization of abortion services, change in access to abortion, attitudes toward abortion, and reliance on female workforce to identify counties that are more vs. less affected by abortion bans and study their differential yield responses to the same law change.

Near-total abortion bans prohibit abortion with limited to no exceptions. If the effects documented in prior sections are indeed driven by a shift in abortion legal landscape, they should be stronger in areas that utilize this service more. To measure local utilization of abortion services, we obtain county-level abortion to live birth ratio in or before 2021 from annual state health statistics reports compiled by Wm. Robert Johnston. We then split bonds within each ban state by whether they are in counties with an above- vs. below-median abortion rate and examine their differential changes in yields relative to protective states.[16] To estimate the heterogeneous effects, we replace $\beta_{e_g} Ban_{s(i),g} 1\{e = y - g\}$ in Eq. 1 with $\textit{High utilization} \times \beta_{e_g} Ban_{s(i),g} 1\{e = y - g\}$ and $\textit{Low utilization} \times \beta_{e_g} Ban_{s(i),g} 1\{e = y - g\}$. Figure 2 plots the estimated event study coefficients. The first observation is that yields in both high- and low-utilization counties within ban states exhibit similar pre-trends to those in protective states. However, while yields in both high- and low-utilization counties increase following abortion bans, the rise in high-utilization counties is nearly twice as large. Table 3 column (1) reports the heterogeneous average treatment effects. In the six months post-ban, yields in high-utilization counties see a relative increase of 7.6 bps, compared to 4.0 bps in low-utilization counties.

Since abortion providers can no longer operate after near-total abortion bans, women in ban states who seek abortions must travel farther to receive the service. Appendix Figure A4 plots the cumulative change in the distance between a county's population centroid

[16] We exclude bonds issued by state governments when we exploit within-state variations in this section because we are unable to assign county-level characteristics to them.

and the nearest abortion clinic from March 2022 to June 2023 separately for ban states and protective states using data from Myers (2024). While counties in protective states experience little to no change in the distance to the nearest abortion clinic, counties in ban states see the distance increase by over 150 miles by June 2023. The resulting distance far exceeds 100 miles—a level that courts have generally treated as not unduly burdensome for women seeking abortions—likely preventing a substantial fraction of women who want abortion service from accessing providers (Myers, 2021). Our documented effects should be stronger in areas with a higher reduction in access to abortion. Therefore, we split bonds by whether they locate in counties with an above- vs. below-change change in distance to the nearest abortion clinic after abotion bans. Table 3 column (2) shows that the relative yield increase is over 80% larger for bonds in high-change counties than bonds in their low-change counties.

We next explore heterogeneity in local residents' attitudes toward abortion. The adverse effects of restricting abortion access should be less pronounced in areas where residents are more accepting of such restrictions. This is because higher local support for abortion bans implies lower perceived negative effects of such policies among residents, who are major holders of municipal bonds (Bagley et al., 2022). Greater support for bans also suggests a lower likelihood of future policy reversal, which reduces uncertainty for investors.

We measure attitudes toward abortion using the share of the county population identifying as religious in the 2010 Religious Congregations and Membership Study (ARDA, 2000) and responses to questions about abortion in the Gallup Poll Social Series (GPSS) between 2012 and 2021. We split bonds by whether they are in treated counties with an above- vs. below-median share of religious residents or GPSS respondents who identify themselves as pro-choice, view abortions as morally acceptable, or think that abortions should be legal in all or most cases within each state. Consistent with our prior, bonds in treated counties with higher non-religious population or abortion acceptance see yields increase by 7 and 9.3 bps relative to those in protective states, while bonds in other treated counties see a much smaller increase (see Table 3 columns 3-4).

The availability of abortion services has been shown to affect women's participation in the labor force (e.g., Ravid and Zandberg, 2022). Consequently, areas that rely heavily on female workforce could experience more pronounced negative effects due to restrictions on abortion. To gauge the extent of women's involvement in the local workforce, we employ two metrics: the county-level share of females participating in the labor force (DOL, 2021) and the county-level share of employment in female-dominated industries based on EEO-1 data (EEOC, 2021) and Quarterly Census of Employment and Wages data (BLS, 2021). In Table 3 columns (5) and (6), we examine effects for bonds in treated counties with an above- vs. below-median share of females participating in the labor force and with an above- vs. below-median share of employment in female-dominated industries, respectively. Indeed, bond yields in treated counties that are heavily dependent on female workforce increase by nearly 7 bps, while the increase is smaller and insignificant in counties that are less dependent.

Taken together, these analyses exploiting within-state heterogeneities show that increases in municipal bond yields are attributable to change in availability of abortion services rather than other contemporaneous policies or shocks that may have disproportionately affected states with abortion bans.

3.4 Robustness

We next explore which type of investors contribute to the yield increase. Following prior literature (e.g., Harris and Piwowar, 2006), we distinguish trades from institutional vs. retail investors based on trade size: likely retail trades are those $\leq \$100,000$ while likely institutional trades are those $> \$100,000$. In Table 4 columns (1) and (2), we estimate the specification in Table 2 column (1) for yields computed from likely retail vs. likely institutional trades. Interestingly, both yields based on institutional and retail trades increase in ban states relative to control states with the increase from institutional side being slightly larger. In other words, both retail and institutional investors appear to play a role in incorporating the effects of restrictive abortion policies into municipal bond pricing.

Thus far, we have included contemporaneous bond credit rating as a control variable. In other words, the relative increase in yields in ban states reflects investors' expectations of default risk and other bond fundamentals above and beyond what is implied by the bond rating. A natural question is whether bond ratings themselves change in response to the implementation of abortion bans. In Appendix Table A3, we use bond rating as the dependent variable. While the sign of the average treatment effects is negative, the effect sizes are economically small and statistically insignificant across most specifications. This lack of effect on bond rating is consistent with rating agencies' reluctance to publicly address the abortion rights issue in the U.S. (Responsible Investor, 2022). In addition, credit ratings might be too coarse to capture the effects of abortion bans when economic impacts have not yet manifested in the financial statements of exposed municipalities.

In our main analyses, treated states are defined as those that implemented near-total abortion bans. For robustness, we expand the treatment definition to also include states that implemented six-week gestational bans. We reestimate Eq. 1 using the expanded treatment definition and plot the event study coefficients in Appendix Figure A3. The resulting pattern is very similar to our original finding, albeit with a smaller average treatment effect (4.2 bps now vs. 6.3 bps before). The smaller effect size is not surprising, as six-week gestational bans do not entirely prohibit abortions, and Texas's gestational ban, which was implemented before Roe v. Wade was struck down, faced legal challenges during the initial months of its implementation.

Finally, to ensure our effect is not driven by any single ban state, in Appendix Table A4 we exclude one ban state at a time from our regression sample. Results remain robust.

3.5 Results from primary markets

We have so far focused on how investors price municipal bonds in the secondary market, as this allows us to isolate the effect on yields from the effect on governments' issuing behavior. We now shift our attention to the *primary* market to examine how abortion bans affect offering yields and issuance amounts.

The analysis of the primary market complements our earlier findings. First, it offers a more concrete assessment of the impact of abortion bans on municipalities' borrowing costs, expressed in both basis points and dollar terms. This contrasts with secondary market analysis, where dollar estimates of increased interest expenses are hypothetical due to the fixed coupons on outstanding bonds. Additionally, primary market analysis helps address concerns about incomplete price discovery in the secondary market, where trading tends to be relatively thin. However, primary market analysis is not without limitations – municipalities facing higher borrowing costs may choose not to issue bonds in the post-Dobbs period, making their costs unobservable. Therefore, the findings in this section provide an estimate of the cost of bond issuance, conditional on municipalities' decisions to issue new bonds given the prevailing price conditions.

Our data on primary market issuance transactions comes from the Mergent database, which covers the universe of primary market issuance of municipal bonds in the U.S. till 2023 (see Section 2.3). We begin by estimating Eq. 1 and Eq. 3 for bond-level offering yields. Table 5 column (1) shows that offering yields in ban states increase by 19.5 bps following near-total abortion bans relative to yields in protective states during the same time period. This effect is larger in magnitude than the corresponding impact observed in the secondary market, which aligns with our expectation that secondary market illiquidity may result in incomplete price discovery. To provide a dollar-based estimate of the increased interest payments, we calculate the average bond issuance amount for all issuers within a state over the four years preceding the 2022 shock, which totals approximately $9.5 trillion. Given an average municipal bond maturity of 10 years, a 19.5 bps increase in yields translates into an additional $185 million in interest payments over the life of the bonds per state.

Next, we examine state-level issuing behavior using two proxies: monthly likelihood of issuing new bonds (extensive margin) and log offering amount per month (extensive margin). Columns (2) and (3) reveal that states do not significantly adjust their issuing behavior after implementing abortion bans. However, this non-result should be interpreted with caution. First, our post-shock period spans only six months, which may be too short for municipalities to adjust their issuing behavior. Municipal budgets are typically set on an annual basis, and the bond issuance process requires coordination between municipal issuers, underwriters, and financial advisors. Additionally, in many states, bond issuance requires voter approval, with bonds often approved and scheduled for issuance one or two years in advance (Ambrose et al., 2023, Ambrose and Valentin, 2024). These factors limit municipalities' ability to adjust issuance within a short window, potentially masking the longer-term impacts of the policy change.

4. The Effects of TRAP Laws

In this section, we explore the effects of state-level staggered adoption of Targeted Regulation of Abortion Providers (TRAP) laws on public financing costs.

4.1 Background and data

TRAP laws are supply-side regulations that focus on regulating abortion at the provider level, ultimately creating obstacles for women seeking abortions. The earliest TRAP laws were enacted in the 1970s, followed by a recent surge of enactments in the 2000s and 2010s. The nature and severity of these laws vary. For instance, some laws require abortion clinics to comply with state standards for ambulatory surgical centers, leading to expensive renovations. Other laws mandate abortion providers to hold admitting privileges at hospitals within a specific distance, which could be unfeasible if the nearest hospital locates outside of this range or is unwilling to extend these privileges. Although TRAP laws cannot fully ban abortion given the constitutional protections provided in the Roe (later Casey) decision, they have been linked to a decrease in provider availability (Grossman et al., 2014) and shown to reduce abortion (e.g., Arnold, 2022, Jones and Pineda-Torres, 2021).

We study TRAP laws for several reasons. First, TRAP laws allow us to test the generalizability of the findings around the overturning of Roe v. Wade. If restrictive abortion policies raise public financing costs through reduced availability of abortion services, TRAP laws should similarly affect municipal finances, albeit at a smaller magnitude as they do not completely ban abortion. Moreover, TRAP laws constitute multiple events over decades, which mitigates potential noise associated with relying on policies that roll out close to each other. Finally, TRAP laws were passed much earlier than the abortion bans implemented in 2022 and 2023. The early passage provides us with sufficient post periods during which important economic and migration data are available, allowing us to study longer-run effects and underlying mechanisms of restrictive abortion policies.

To identify the timing of treatment, we use data on TRAP laws collected by Austin and Harper (2019). The data cover the year of enactment and other details for TRAP laws based on various sources, such as state websites, Lexis Nexis Quicklaw, and WestlawNext, updated through mid-2016. We focus on the 16 states that enacted their first set of TRAP laws from 2000 through 2016 (see Appendix Table A1). We choose this time period because (i) our municipal bond data start in 2000, and (ii) Austin and Harper recommend that researchers focus on post-1991 laws due to differences in policy intentions between early and more recent TRAP laws.

4.2 Event study around TRAP laws

Similar to Section 3.1, we use a dynamic DID event study design to examine the effects of TRAP laws on bond yields. Specifically, we compare the yields in states that enacted their first set of TRAP laws between 2000 and 2016 ("treatment states") against those in states that never enacted TRAP laws by end of 2016 ("control states"). We focus on a state's *first* enactment of TRAP laws to prevent our estimates from being contaminated by the effects of prior enactments following Zandberg (2021).[17] To implement the design, we stack our panel data as a series of 2×2 matrices (bonds in treatment/control states × omitted period/event period).

We define states that enact their TRAP laws in year g as cohort g and cohort-specific event time in calendar year y as $e_g = y - g$. We run the following regression for secondary-market municipal bond yields of bond i in year-month ym:

$$\begin{aligned} Y_{i,ym} = \sum_{g \in G} 1\{cohort = g\} \times [\sum_{e \neq -1} \gamma_{e_g} TRAP_{s(i),g} 1\{e = y - g\} \\ + \sum_{e \neq -1} \delta'_{e_g} X_{i,ym} 1\{e = y - g\} + \alpha_{e_g,ym,g} + \alpha_{c(i),g}] + \epsilon_{i,ym}. \end{aligned} \tag{4}$$

While the data are monthly, for precision and ease of presentation we estimate effects by event year. G is the set of years from 2000 through 2016 when states enact their first TRAP laws. To avoid extending the sample period into the pandemic year of 2020 while having a balanced sample in event time, we only estimate effects between -3 and +3 event years. We omit year -1 as the reference period. $TRAP_{s(i),g}$ takes a value of 1 if bond i is issued by

[17] Some states blocked previously enacted TRAP laws or never enforced them. We define these states as treated because TRAP law enactments, regardless of enforcement, signal decreasing access to abortion. To the extent that the effect is weaker in these states, our estimate serves as a lower bound.

state s belonging to cohort g, and cohort g is the treatment cohort (i.e., $1\{cohort = g\} = 1$). $X_{i,ym}$ denotes time-varying bond characteristics defined as in Eq. 1, $\alpha_{ym,g}$ denotes cohort- and-event-specific year-month fixed effects, and $\alpha_{c(i),g}$ denotes cohort-specific county fixed effects. γ_{e_g} is then aggregated to γ_e using Eq. 2. We calculate standard errors clustered by state for γ_e via the delta method.

Figure 3 plots the γ_e coefficients. Consistent with findings in Section 3, municipal bond yields increase following TRAP laws. In the three years leading up to TRAP law enactments, yields in TRAP-law states are not statistically different from yields in control states. However, once TRAP laws are enacted, yields in treatment states start to rise relative to control states, with the difference peaking at nearly 5 bps in year 1 before gradually attenuating in years 2 and 3.[18] The average post-enactment effect from years 1 through 3 is roughly 3.5 bps. Aggregating across years 1 through 3, the effect implies a $28 million increase in cost of financing for a treatment state over the life of the bonds.[19] Regression coefficients are reported in Table A5.

One concern regarding TRAP laws is their potential correlation with state tax cuts. States with more conservative voters are more likely to pass both tax cuts and restrictive abortion policies. However, when we control for the annual top marginal state income tax, results remain similar (see Appendix Table A6 column 1). This indicates that our results are not driven by contemporaneous state tax changes.

5. Exploring Channels

Results in the prior sections demonstrate that restrictive abortion policies, either near-total abortion bans or restrictions on abortion providers, lead to increase in municipal bond yields. We now explore the mechanisms behind this increase by studying how these policies affect two important components of local economy: firms and residents. We first assess the impact on firms by analyzing the stock market's reaction to near-total abortion bans in 2022 and 2023 and changes in business dynamics and job creation following TRAP laws

[18]Six states in the treatment group blocked TRAP laws within two years of enactment, which may have contributed to the decline in the treatment effect in year 3.

[19]The $28 million is calculated as 3.5 bps $\times$ 10 years (the average life of a municipal bond) $\times$ $8 billion (the annual total issuance amount per state over the sample period).

(Section 5.1). We next investigate the impact on residents by studying changes in cross-state migration and job flow following TRAP laws (Section 5.2).

5.1 Impact on firms

Firms constitute an important component of local economy, which determines government's debt repayment ability. We expect firms to be negatively affected by restrictive abortion policies for several reasons. First, firms may need to help employees cover the costs of traveling out-of-state to obtain abortion services. Second, workers, especially high-skilled and mobile workers, may decide to leave states that restrict abortion, leading to costs associated with hiring and training new personnel. Finally, firms may move out of states with abortion bans in response to employees' demands or labor supply shortages, resulting in operational disruptions and relocation costs. All of these are likely to increase firms' operating costs, decrease future cash flows, and weaken firms' bottom lines, thereby lowering tax revenue for municipalities and municipal bond yields.

5.1.1 Changes in firm value around near-total bans in 2022 and 2023

We begin by studying how near-total abortion bans implemented around the overturning of Roe v. Wade affect firms. Ideally, we would analyze firms' financial performance using measures such as EBITDA, cost of goods sold (COGS), and selling, general, and administrative (SG&A) expenses. However, since these financial metrics take time to reflect the impact, we instead focus on changes in the market value of firms following the abortion bans. This approach provides a summary statistic that captures market expectations of the economic impact on firms.

To estimate the effects on firm value, we obtain daily trading data for public companies from the CRSP/Compustat Merged Database. We exclude penny stocks and restrict our sample to common share stocks listed on NYSE, Nasdaq, and AMEX, and firms headquartered in ban states. Our final sample consists of 513 firms. We compute daily cumulative abnormal returns (CARs) adjusted for the Fama-French four-factor model. We estimate each firm's model using the 100 days before the effective date of the abortion ban in their headquarter state.

Table 6 presents the results. Upon states implementing near-total abortion bans, firms headquartered in those states experience negative abnormal returns. Specifically, column (3) shows that the abnormal returns of firms headquartered in ban states are approximately -210 bps over the (0, +20) window following abortion bans. The impact is economically smaller but still statistically significant for the (0, +5) window (column 2).

In columns (4) to (6), we explore cross-sectional heterogeneity using the pre-Dobbs abortion to live birth ratio in the headquarter counties of firms. We expect the effect to be larger for firms whose headquarter counties have an above-median pre-Dobbs utilization of abortion service under a similar rationale provided in Section 3.5. This is indeed the case. Over the (0, +20) window following abortion bans, abnormal returns are 260 bps lower for firms headquartered in high-utilization counties than firms in low-utilization counties. When we shrink the window to (0, +5), firms in high-utilization counties still see 133 bps lower abnormal returns.

We next explore the differential responses by the post-Dobbs change in access to abortion service of firms' headquarter counties. Following Section 3.5, we compare firms headquartered in ban counties with an above- vs. below-median change in distance to the nearest abortion clinic from March 2022 through June 2023. Consistent with our prior findings, firms headquartered in high-change counties experience more negative abnormal returns than those in low-change counties following abortion bans: -142 bps over the (0, +5) window and -155 bps over the (0, +20) window.

To account for firm-level characteristics that may influence abnormal returns, we include controls for book-to-market ratio (B/M), return on assets (ROA), and net profit margin, calculated using the most recent quarterly financial disclosure prior to the shock. Table A7 presents the robustness. Results are similar across all specifications.

Collectively, our findings suggest that investors perceive abortion bans as negative news for firms in affected states, suggesting that negatively impacted firms could contribute to our documented yield effect.

5.1.2 Changes in businesses around TRAP law enactments

To further understand how restrictive abortion policies affect local firms, we turn to TRAP laws. Specifically, we study how the enactment of TRAP laws impact annual state-level firm entry/exit and job creation/destruction using data from Census Bureau's Business Dynamics Statistics (BDS). We run the following state-annual level regression:

$$\mathrm{Y}_{s,y} = \sum_{g \in G} 1\{cohort = g\} \times \{\sum_{e \neq -1} \phi_{e_g} Treat_{s,g} 1\{e = y - g\} + \alpha_{y,g} + \alpha_{s,g}\} + \epsilon_{s,y}, \qquad (5)$$

where $Y_{s,y}$ is our outcome of interest—firms per 100,000 residents or net job creation rate—for state s in year y. $\alpha_{y,g}$ denotes cohort-specific year fixed effects and $\alpha_{s,g}$ denotes cohort-specific state fixed effects. We then aggregate ϕ_{e_g} to ϕ_e using Eq. 2 and calculate standard errors clustered by state via the delta method.[20]

Estimates of ϕ_e are presented in Figure 4 and in Table A5. Consistent with our prior, states' business dynamism weakens following TRAP laws relative to control states. Specifically, panel (a) shows that in the year when states enact TRAP laws, their annual number firms per capita declines relative to control states, with the gap continuing expanding over the next three years. Summing across the three years post-enactment, states that enact TRAP laws see a total reduction of 45 firms per capita compared to control states. This finding is consistent with Zandberg (2021), who shows a decrease in female entrepreneurship after TRAP laws. Moving to panel (b), we see that the net job creation rate in TRAP states decreases relative to control states starting in the year of enactment and continues to drop over the next three years. In the three years after states enact TRAP laws, they experience a relative decrease of 0.44% in net job creation rate.

These findings indicate that following restrictive abortion policies, firms are less likely to be founded or more likely to close down, which hinders local employment and could negatively affect the tax base and fiscal capacity of local governments. These results also correspond well to the findings in Section 5.1.1 that restrictive abortion policies decrease firm value.

[20] we do not have enough number of years in the sample to double cluster by state and year.

5.2 Impact on residents

Population is another important component of local economy, with residents contributing as consumers and workers. Local residents are also major holders of municipal bonds (Bagley et al., 2022). If individuals who value reproductive rights are less attracted to states with restrictive abortion policies, they may move out of those states or be less inclined to move to them. We thus expect states that restrict access to abortion to see a decline in net in-migration (i.e., inflow - outflow). A shrinking state population is likely to deteriorate local economy, reduce the tax base, erode future cash flows to municipalities, and dampen local demand for municipal bonds, all of which could contribute to a rise in municipal bond yields.

To examine this channel, we study how state-to-state migration changes after enactments of TRAP laws using data from the Statistics of Income (SOI) (IRS, 2023). The data are based on year-to-year address changes reported on individual income tax returns filed with the IRS and are available for all 2,550 ordered state pairs in the U.S. We use the number of personal exemptions claimed to approximate the number of individuals who migrate.[21] We estimate Eq. 5, plot ϕ_e estimates in Figure 5, and report the estimates in Table A5.

Figure 5 panel (a) shows that the net in-migration drops in the year when states enact TRAP laws relative to control states, with the gap continuing to grow in the following years. The average treatment effect in the three years after enactment is -1.5, representing a decline of 150 per 100,000 state residents in net in-migration or a \$4.7 million loss in adjusted gross income per year.[22] Results are similar when we restrict the flows to be from or to control states (see Appendix Figure A5). Panels (b) and (c) plot the effects on migration inflows and outflows, respectively. While the effects of TRAP laws on outflows are positive and short-lived, the effects on inflows are negative, larger in magnitude, and longer-lasting. This implies that although both in- and out-of-state residents adjust their moving patterns when a state enacts TRAP laws, out-of-state residents' lower tendency to move to a TRAP-law state accounts for most of the negative effect on net in-migration.

[21] We do not use the number of returns filed because households tend to file taxes jointly and doing so would undercount the number of people who migrate.

[22] This number is obtained by multiplying 150 by 0.5 (to translate the number of residents to the number of returns) and multiplying that number by \$63,000, i.e., the average adjusted gross income per return (2000-2017).

Although SOI data cover all tax filers in the US and offer a broad view of migration response to TRAP laws, they lack detailed demographic information. To zoom into fertile women, who are potentially more affected by TRAP laws, we turn to LEHD J2J data from U.S. Census Bureau (2023), detailing job-to-job flows across states by age and sex. Figure 6 plots the effects on job flows among women ages 19-44. Panel (a) shows a decline in net job flow starting in the first year of TRAP laws, and the gap grows over the next three years. The average treatment effect in the three years after TRAP laws is -4.3, representing a decrease of 4.3 net job inflows per 1,000 fertile women. Panels (b) and (c) separately plot job inflow and outflow. While effects on outflow are positive yet small and marginally insignificant, effects on inflow are much larger and the magnitude rises over time. This difference in responses between inflow and outflow aligns well with the overall migration patterns from SOI data.

Taken together, our results suggest that restrictive abortion policies—whether near-total abortion bans or excessive requirements imposed on abortion providers—negatively impact local businesses and reduce net in-migration, potentially deteriorating local economy, shrinking state tax bases, and contributing to higher public financing costs.

6. Conclusions

The right to abortion is a highly contentious issue in the U.S. While debates often focus on the legality and morality of abortion, its economic implications are frequently overlooked. To fill this gap, we examine the public financing costs of restricting abortion on state and local governments. Our analysis reveals that municipal bond yields increase in states that restrict or ban abortion compared to states that protect access to abortion. Furthermore, we identify two key channels driving these effects: negatively impacted firms and reduced net in-migration.

Given the important role of public financing in supporting government operations and public projects, abortion policies could have long-lasting impacts on public services, infrastructure, and economic growth, and may reshape the lines of economic competition between states with different policies. Lawmakers should consider the economic ramifications of these policies in addition to their legal and moral implications.

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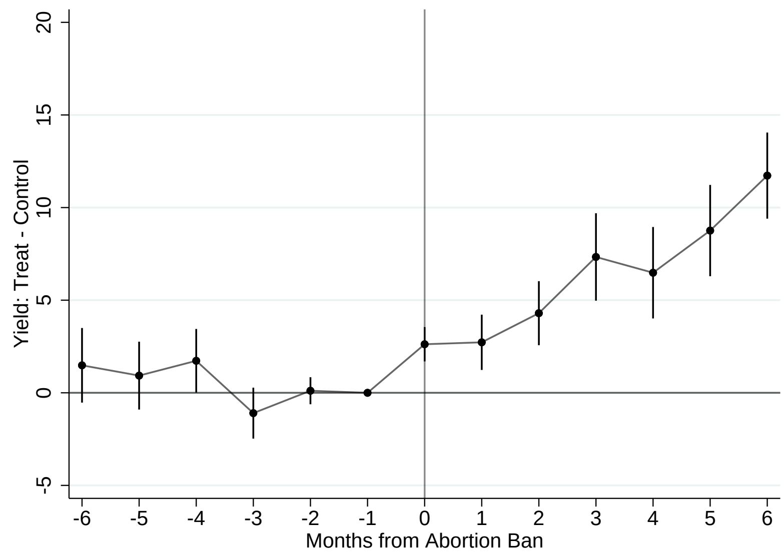


Figure 1: Effect of Abortion Bans on Municipal Bond Yields

Note: This figure plots dynamic treatment effects (and 90% confidence intervals) of near-total abortion bans on secondary market municipal bond yields in states that implemented these bans around the overturning of Roe v. Wade relative to states with preexisting laws protecting abortion. The outcome is the size-weighted average yield at the bond-month level; units are bps. The vertical line (and event time 0) denotes the month when the abortion ban went into effect in a state; same month of the effective date if the effective day is before 15th of the month and the following month if after. The omitted month is -1, i.e., the month before the implementation. All regressions control for cohort-specific county fixed effects, year-month fixed effects, bond characteristics (bond rating at the time of transaction, log maturity, log size, coupon rate, indicators for whether a bond is general obligation, callable, insured, reoffered, and negotiated). Standard errors are clustered by state. Coefficients are reported in Appendix Table A2.

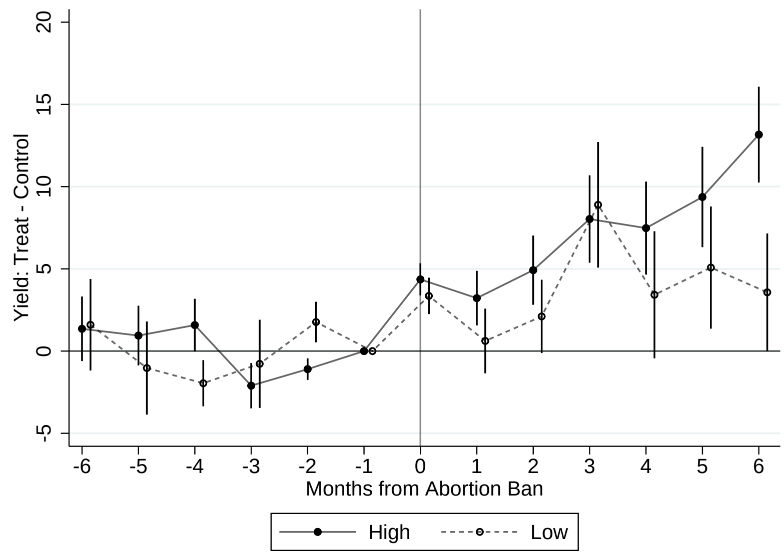


Figure 2: Effect of Abortion Bans on Municipal Bond Yields by *Local Utilization of Abortion Services*

Note: This figure plots dynamic treatment effects (and 90% confidence intervals) of near-total abortion bans on secondary market municipal bond yields by ex-ante local utilization of abortion services. The outcome is the size-weighted average yield at the bond-month level; units are bps. The sample excludes bonds issued by state governments to ensure we can assign county-level utilization of abortion services (measured by pre-Dobbs abortion to live birth ratio) to bonds in the sample. We split bonds within each ban state by whether they are in counties with an above- vs. below-median abortion rate and examine their differential changes in yields relative to protective states. Specifically, we replace $\beta_{e_g} * Ban_{s(i),g} * 1\{e = y - g\}$ in Equation 1 with $\textit{High utilization} \times \beta_{e_g} * Ban_{s(i),g} * 1\{e = y - g\}$ and $\textit{Low utilization} \times \beta_{e_g} * Ban_{s(i),g} * 1\{e = y - g\}$. Everything else follows Figure 1. Standard errors are clustered by state. The average treatment effects are reported in Table 3.

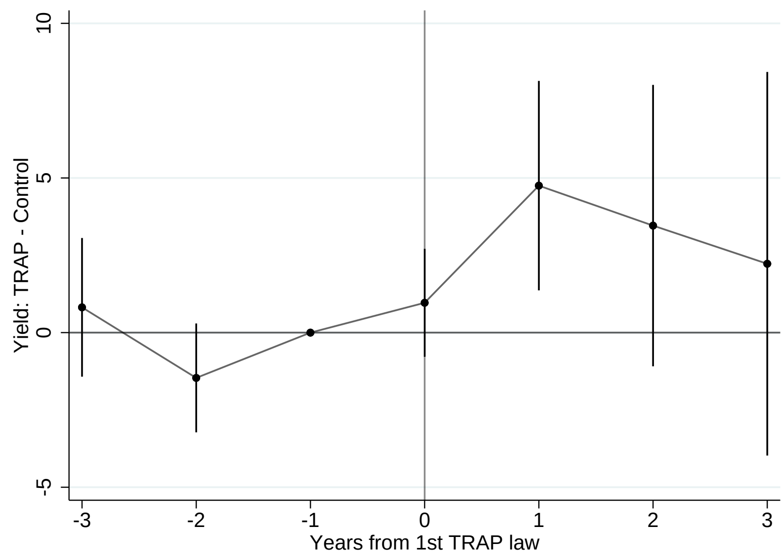
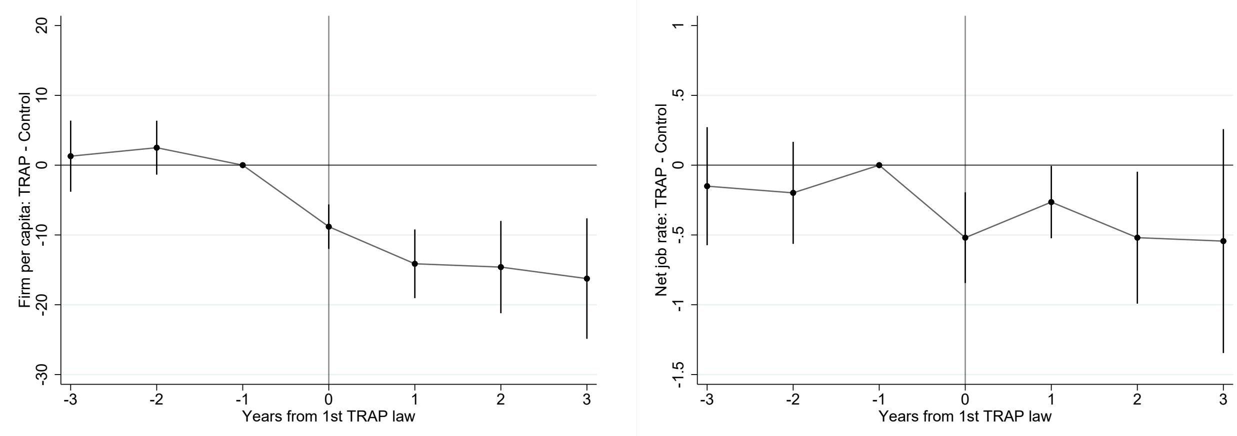


Figure 3: Effect of TRAP Laws on Municipal Bond Yields

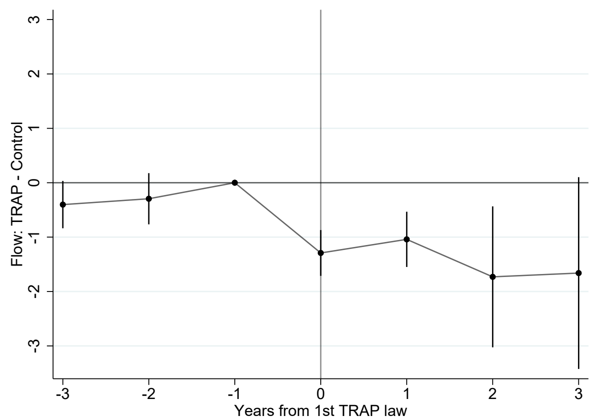
Note: This figure plots dynamic treatment effects (and 90% confidence intervals) of TRAP laws on secondary market municipal bond yields in states that ever enacted TRAP laws relative to states that never. The outcome is the size-weighted average yield at the bond-month level; units are bps. Only states that enacted their first TRAP laws between 2003 and 2016 are considered because (i) our municipal bond data start in 2000, (ii) we require 3 years of pre-period, and (iii) data on TRAP law is updated through mid-2016. The vertical line (and event time 0) denotes the year when a state first enacted TRAP laws. The omitted period is -1, i.e., the year before enactment. Standard errors are clustered by state. Coefficients are reported in Table A5.



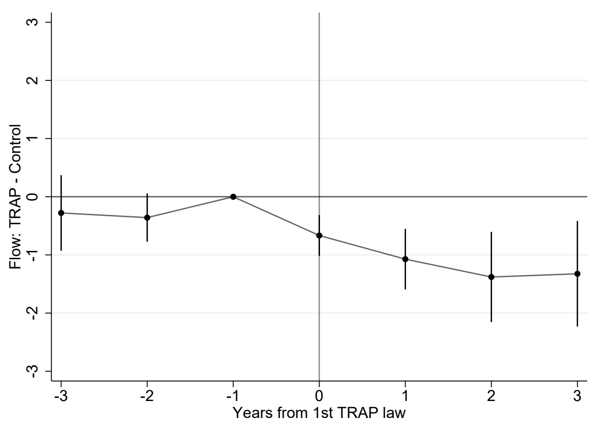
(a) Firms per 100,000 residents **(b)** Net job creation rate

Figure 4: Effect of TRAP Laws on *State Business Dynamics*

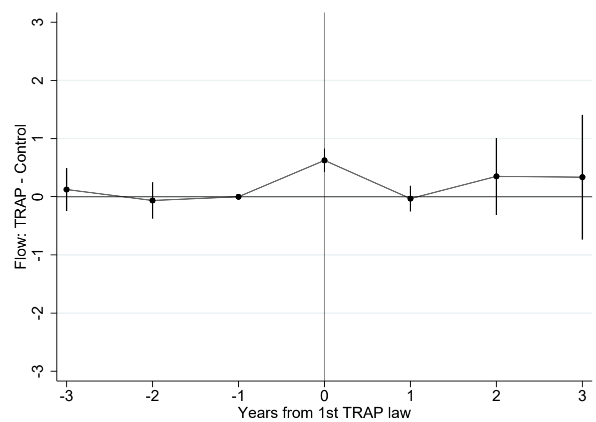
Note: This figure plots dynamic treatment effects (and 90% confidence intervals) of TRAP laws on business dynamics in states that ever enacted TRAP laws relative to states that never. Panel (a) plots the effect on firms per 100,000 state residents, i.e., the number of firms divided by state population and multiplied by 100,000. Panel (b) plots the effect on net job creation rate, i.e., total jobs created minus total jobs destructed and divided by the average of employment for years t and t-1 (Davis-Haltiwanger-Schuh denominator). The vertical line (and event time 0) denotes the year when a state first enacted TRAP laws; only states that first enacted TRAP laws between 2000 and 2016 are considered. The omitted period is -1, i.e., the year before enactment. Standard errors are clustered by state. Coefficients are reported in Table A5.



(a) Net inflow per 1,000 residents



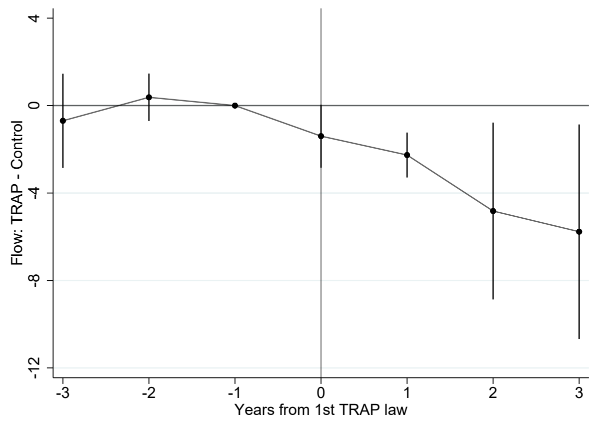
(b) Inflow per 1,000 residents



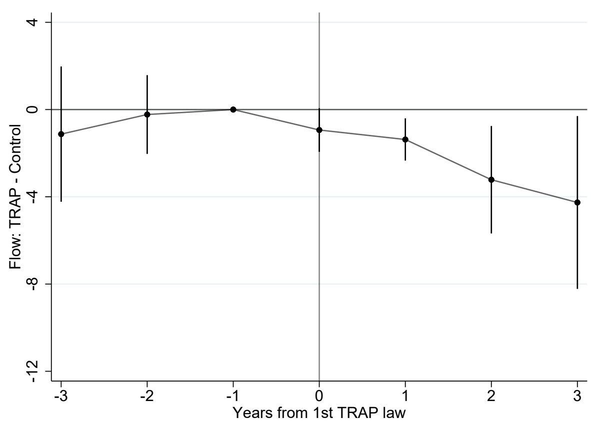
(c) Outflow per 1,000 residents

Figure 5: Effect of TRAP Laws on *Cross-State Migration*

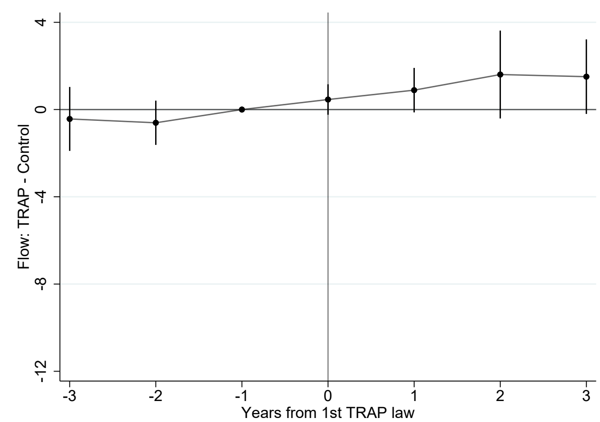
Note: This figure plots dynamic treatment effects (and 90% confidence intervals) of TRAP laws on cross-state migration in states that ever enacted TRAP laws relative to states that never. Panels (a) through (c) plot effects on net flow to a state, total inflow to a state, and total outflow from a state, respectively; all flow measures are divided by state population and multiplied by 1,000. Everything else follows Figure 4. Standard errors are clustered by state. Coefficients are reported in Table A5.



(a) Net job inflow per 1,000 fertile women



(b) Job inflow per 1,000 fertile women



(c) Job outflow per 1,000 fertile women

Figure 6: Effect of TRAP Laws on *Cross-State Job Flows among Fertile Women*

Note: This figure plots dynamic treatment effects (and 90% confidence interval) for TRAP laws on cross-state job flows among women ages 19-44 in states that ever enacted such laws relative to states that never. Panels (a) through (c) plot effects on net job flow to a state, total job inflow to a state, and total job outflow from a state, respectively; all flow measures are for women ages 19-44 and are divided by the number of women of the same age group and multiplied by 1,000. Everything else follows Figure 4. Standard errors are clustered by state. Coefficients are reported in Table A5.

Table 1: Summary Statistics

Panel A: Full Sample

Variable	Mean	Std	P25	Median	P75
Yield (bp)	314.67	100.65	256.73	317.09	380.01
Spread (bp)	99.02	72.09	45.35	87.68	145.29
Rating	18.20	1.92	17.00	19.00	19.00
Bond size (log)	15.14	1.50	14.07	15.07	16.16
Maturity (year)	6.54	5.47	2.92	4.92	7.83
Callable	0.66	0.47	0.00	1.00	1.00
Insured	0.14	0.35	0	0	0
General Obligation	0.49	0.50	0	0	1
Negotiated	0.55	0.50	0	1	1
N obs	963,362				

Panel B: Ban vs. Protective States before Dobbs

	Ban states	Protective states
Yield (bp)	221.00	218.37
Spread (bp)	105.19	102.16
Rating	18.32	18.13
Bond Size (log)	14.94	15.27
Maturity (year)	5.80	5.79
Callable	0.62	0.63
Insured	0.13	0.13
General Obligation	0.49	0.49
Negotiated	0.47	0.58
N obs	124,228	287,796

Note: This table reports summary statistics for bond characteristics in the secondary market. *Yield* is the size-weighted average yield at the bond-month level; units are bps. *Spread* is the size-weighted average difference between the bond yield and the maturity-matched after-tax Treasury yield for a bond in a month; units are bps. *Rating* is the numeric value of a bond's credit rating in a month: the highest rated bonds (AAA) are assigned a value of 21, the second highest ($AA+$) 20, and so forth to the lowest rated bonds, (C), which are assigned a value of 1. *Bond size* is the natural log of a bond's offering amount. *Maturity* is defined as the remaining time to maturity for straight bonds and the remaining time to next call for callable bonds. *Callable* is an indicator for a bond being callable. *Insured* is an indicator for a bond being insured. *General Obligation* is an indicator for a general obligation bond backed by the taxing authority of the issuer. *Negotiated* is an indicator for a bond being offered through negotiation (as opposed to competitive offering). In Panel A, we report summary statistics for bonds in our main sample from December 2021 through November 2023. In Panel B, we compare characteristics between bonds in states that implemented near-total abortion bans ("ban states") and states with preexisting laws protecting abortion ("protective states") from December 2021 through May 2022 before the Dobbs v. Jackson decision.

Table 2: Effect of Abortion Bans on Municipal Bond Yields

	Dependent variable: Yield (bp)				
	(1)	(2)	(3)	(4)	(5)
Avg Treatment Effect	6.290***	6.664***	4.413***	5.809***	7.623***
	(1.791)	(1.702)	(1.191)	(1.156)	(1.966)
R^2	0.754	0.754	0.754	0.754	0.929
Outcome mean	302.008	302.008	302.008	302.008	302.008
Bond characteristics	Y	Y	Y	Y	Y
Year-month FE	Y	Y	N	N	N
County FE	Y	Y	Y	Y	N
Economic controls	N	Y	N	N	N
Governor election×Year-month FE	N	N	Y	N	N
Tax cut×Year-month FE	N	N	N	Y	N
CUSIP FE	N	N	N	N	Y

Note: This table presents the average treatment effect of near-total abortion bans on secondary-market municipal bond yields in states that implemented these bans around the overturning of Roe v. Wade relative to states with preexisting laws protecting abortion. The outcome is the size-weighted average yield at the bond-month level; units are bps. Column (1) controls for cohort-specific county fixed effects and year-month fixed effects; column (2) additionally includes lagged-one-period state monthly unemployment rate and quarterly GDP; columns (3) and (4) replace cohort-specific year-month fixed effects with cohort-specific state governor election in 2022 × year-month fixed effects, and cohort-specific state tax cut in 2022 × year-month fixed effects, respectively; column (5) replaces cohort-specific county fixed effects with cohort-specific CUSIP fixed effects. All regressions control for bond characteristics, including bond rating at the time of transaction, log maturity, log size, coupon rate, and indicators for a bond being general obligation, callable, insured, reoffered, and negotiated. Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table 3: Effect of Abortion Bans on Municipal Bond Yields
by *County-Level Exposure to Ban*

	Dependent variable: Yield (bp)					
	By degree of shock		By local attitude		By reliance on female workers	
	Preexisting utilization (1)	Δ Distance to nearest clinic (2)	Non-religiosity (3)	Abortion acceptance (4)	Female LFP (5)	Female industry (6)
High Avg Treatment Effect	7.562***	8.175***	7.011	9.307***	7.054***	6.560***
	(2.030)	(2.163)	(2.050)	(2.147)	(2.125)	(2.061)
Low Avg Treatment Effect	3.995*	4.486**	2.276***	0.142	0.462	2.440
	(2.097)	(1.986)	(2.155)	(2.009)	(2.248)	(2.219)
R^2	0.746	0.746	0.746	0.746	0.746	0.746
Outcome mean	303.862	303.862	303.862	303.862	303.862	303.862
Bond characteristics	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y

Note: This table presents heterogeneous effects of near-total abortion bans on secondary-market municipal bond yields by treated county's exposure to these bans. The outcome is the size-weighted average yield at the bond-month level; units are bps. The sample excludes bonds issued by state governments to ensure bonds can be assigned county-level characteristics to. Columns (1) reports the average treatment effects (event month +1 through +6) for bonds in treated counties with an above-median ("High") vs. below-median pre-Dobbs abortion rate ("Low") within each state using data from Wm. Robert Johnston. Column (2) reports effects for bonds in treated counties with an above-median vs. below-meidan change in distance to the nearest abortion clinic from March 2022 to June 2023 using data from Myers (2024). Columns (3)-(4) report the effects for bonds in treated counties with an above- vs. below-median share of religious population using the 2010 Religious Congregations and Membership Study (ARDA, 2000) and share of respondents who identify themselves as pro-choice, view abortions as morally acceptable, or consider abortions to be legal in all or most cases in the Gallup GPSS survey between 2012 and 2021, respectively. Columns (5)-(6) report the effects for bonds in treated counties with an above- vs. below-median female labor force participation rate from DOL (2021) and employment share in female-dominated industries from BLS (2021), respectively. Female-dominated industries are defined as NAICS 3-digit industries with a share of female employees above 50% according to EEO-1 data (EEOC, 2021). Everything else follows Table 2 column (1). Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table 4: Effect of Abortion Bans on Municipal Bond Yields by *Likely Retail vs. Institutional Trades*

	Dependent variable: Yield (bp)	
	Likely retail	Likely institutional
	(1)	(2)
Avg Treatment Effect	6.542***	7.839***
	(1.735)	(2.068)
R^2	0.776	0.767
Outcome mean	302.111	298.118
Bond characteristics	Y	Y
Year-month FE	Y	Y
County FE	Y	Y

Note: This table reports heterogeneous effects of near-total abortion bans on secondary-market municipal bond yields separately for likely retail vs. institutional trades. Retail trades are defined as those $\leq \$100,000$ and institutional trades those $> \$100,000$ following Harris and Piwowar (2006). The outcome in column (1) and column (2) is the size-weighted average yield at the bond-month level from approximate retail trades and institutional trades, respectively; units are bps. Everything else follows Table 2 column (1). Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table 5: Effect of Abortion Bans on *Primary Markets*

	Offering yield (bp) (1)	Log(issuance) (2)	Any issuance (3)
Avg Treatment Effect	19.469***	0.151	0.025
	(5.910)	(0.233)	(0.016)
R^2	0.928	0.739	0.134
Outcome mean	304.011	19.626	0.995
Year-month FE	Y	Y	Y
County FE	Y	N	N
State FE	N	Y	Y

Note: This table presents the average treatment effect of near-total abortion bans on primary-market offering yields and issuance behavior in states that implemented these bans around the overturning of Roe v. Wade relative to states with preexisting laws protecting abortion. The outcome is the offering yield at the time of issuance (column 1), the natural logarithm of state-level monthly issuance amount (column 2), and an indicator for states issuing any bonds in a month (column 3). Column (1) controls for cohort-specific year-month fixed effects and county fixed effects while columns (2) and (3) control for cohort-specific year-month fixed effects and state fixed effects. Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table 6: Effect of Overturning Roe v. Wade on *Firm Values*
Cumulative Abnormal Returns (CARs)

	HQ in ban states			By HQ ex-ante abortion utilization			By HQ Δ distance to clinic		
	(0,+2)	(0,+5)	(0,+20)	(0,+2)	(0,+5)	(0,+20)	(0,+2)	(0,+5)	(0,+20)
Avg. CAR	30.004	-65.542**	-209.885***						
	(26.273)	(32.010)	(62.693)						
High utilization				-69.499	-132.649*	-259.399**			
				(62.190)	(72.531)	(128.836)			
High change							-79.440	-142.177**	-154.773
							(59.178)	(69.378)	(128.644)
Intercept				71.460	13.582	-55.155	75.841	16.493	-120.581
				(59.654)	(66.500)	(101.786)	(55.676)	(61.659)	(101.388)
Observations	513	513	513	513	513	513	513	513	513
R^2	-0.00	-0.00	-0.00	0.00	0.01	0.01	0.00	0.00	0.00

Note: This table reports the effects on CARs following the overturn of Roe v. Wade. CARs are calculated using the Fama-French four-factor model based on data in the -101 through -2 days from the abortion ban date; units are bps. Columns (1)-(3) report the average abnormal returns for firms headquartered in ban states. Columns (4)-(6) explore heterogeneous effects for firms headquartered in treated counties with an above-median ("High utilization") pre-Dobbs abortion rate. Columns (7)-(9) explore heterogeneous effects for firms whose headquarter county see an above- (*High change*) in distance within each state to the nearest abortion clinic after the overturn. Robust standard errors are reported in parentheses.
*** 1%, ** 5%, * 10% significance level

Online Appendix

Roe v. Rates: Reproductive Healthcare and Public Financing Costs

Runjing Lu and Zihan Ye

Appendix 1. Additional Figures and Tables

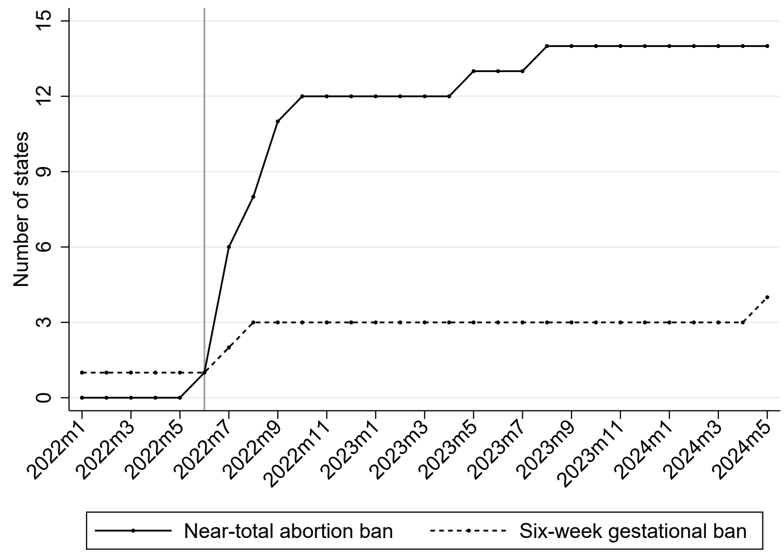
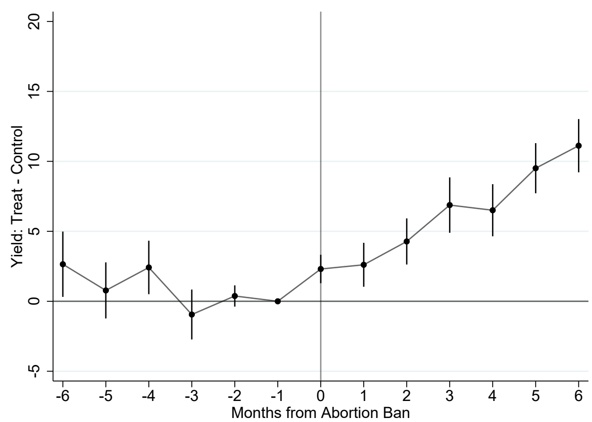
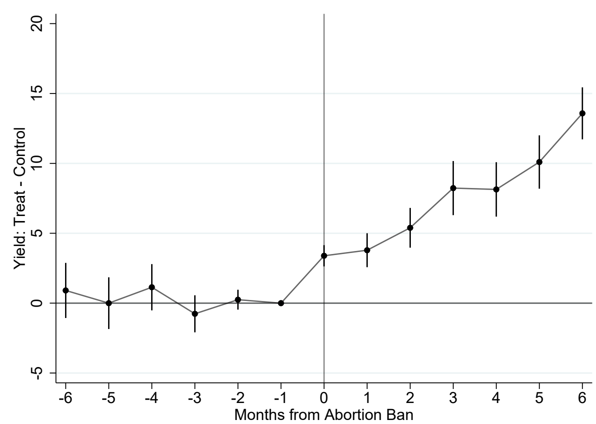


Figure A1: Number of States Implementing Near-Total Abortion Bans Across Time

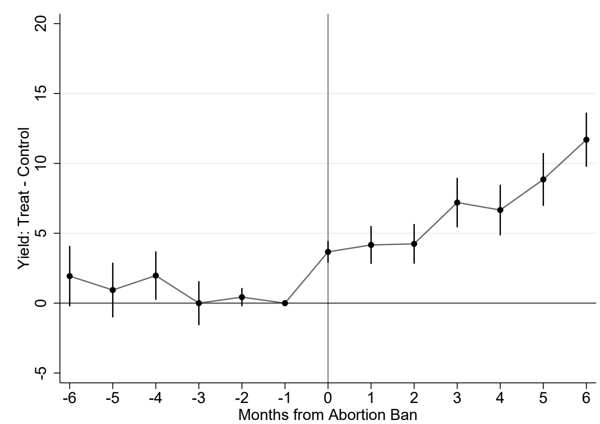
Note: This figure plots the cumulative number of states implementing near-total abortion bans from June 2022 through May 2024; the vertical line denotes the month when the U.S. Supreme Court issued the Dobbs v. Jackson decision that ended the constitutional protection for access to abortion, i.e., June 2022. Data on the effective timing of state abortion laws are based on the beginning-of-the-month snapshots of Guttmacher Institute (2024) on Wayback Machine between June 2022 and May 2024 and google searches conducted in October 2024. If a law takes effect on or before 15th of a month, the law is coded as being implemented in the month; if a law takes effect on or after the 16th of a month, the law is coded as being implemented in the following month. Two states have bans effective before the Dobbs decision: (i) Texas's six-week gestational ban went into effect in September 2021, and (ii) Oklahoma's near-total abortion ban went into effect in May 2022. These laws were allowed to take effect under the precedent established by Roe because they delegated enforcement to private citizens instead of state officials. In addition, given that our municipal bond data ends in December 2023, we exclude two states implementing bans from our analysis to ensure treated states have at least six months post-ban period, i.e., Indiana whose near-total ban took effect in August 2023 and Florida whose six-week became effective in May 2024.



(a) Spread as outcome



(b) Add log(maturity) by year-month FEs



(c) Add maturity bin by year-month FEs

Figure A2: Effect of Abortion Bans on Municipal Bond Yields
Alternative Outcome and Controls

Note: This figure provides robustness tests for Figure 1 by using an alternative outcome and adding additional controls. Panel (a) use spread as outcome, which is the bond-month-level size-weighted average difference between bond yield and maturity-matched after-tax Treasury yield; units are bps. Panel (b) uses the original yield outcome while adding cohort-specific log(maturity) by year-month fixed effects. Panel (c) uses the original yield outcome while replacing cohort-specific year-month fixed effects with cohort-specific year-month $\times$ maturity bins fixed effects (bins are: years 0-2.5, 2.5-5, 5-7.5, 7.5-10, 10 years and above). Everything else follows Figure 1.

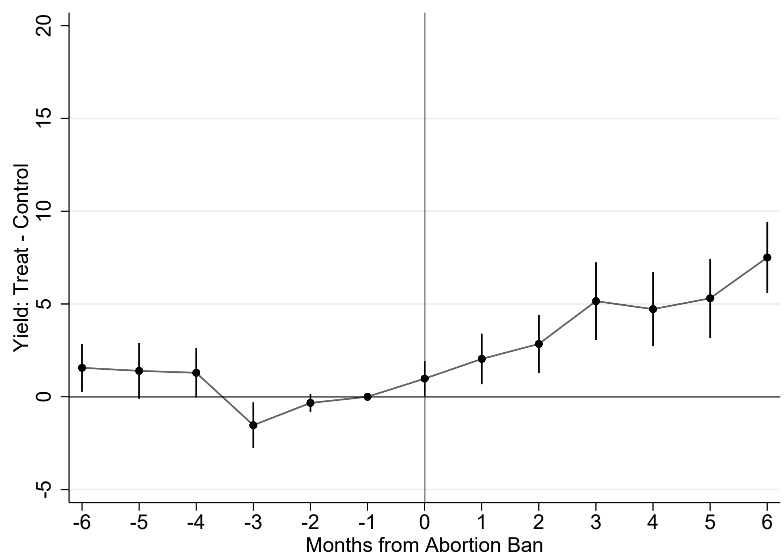


Figure A3: Effect of Abortion Bans on Municipal Bond Yields
Either Near-Total or Six-Week Gestational Bans

Note: This figure provides robustness tests for Figure 1 by expanding the treatment states to also include six-week gestational bans. Everything else follows Figure 1.

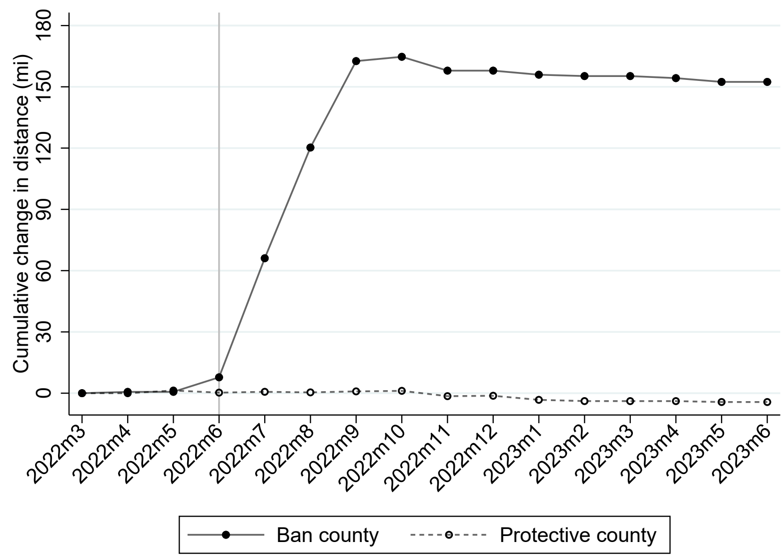
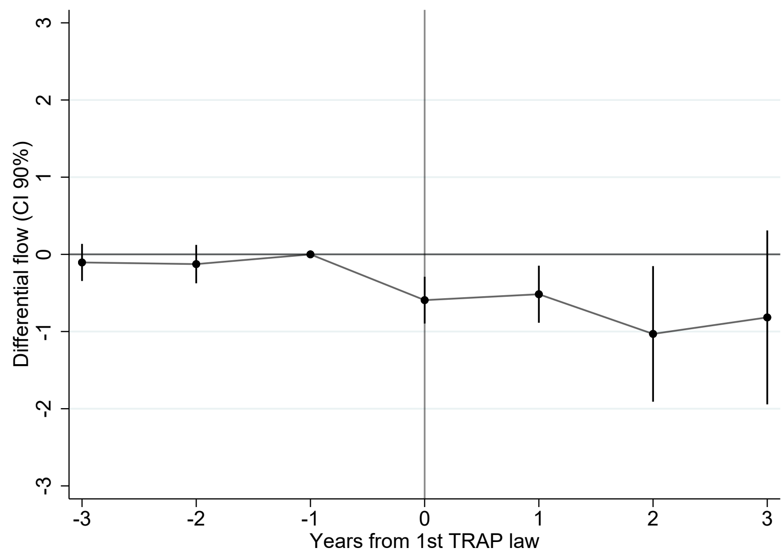


Figure A4: Change in Distance to the Closest Abortion Clinic

Note: This figure plots the average cumulative change in the distance between county population centroids and the nearest abortion clinics from March 2022 to June 2023 for counties in states implementing near-total abortion bans ("ban county") vs. those in states with preexisting laws protecting abortion ("protective county"), using data from Myers Abortion Facility Database (Myers, 2024). We stop in June 2023 as it is the month after our last treated state, North Dakota, implemented its near-total abortion ban.



(a) Net inflow per 1,000 residents

Figure A5: Effect of TRAP Laws on Cross-State Migration
Flow between Treatment and Control states

Note: This figure repeats Figure 5 Panel (a) while restricting the flow to be between TRAP states and control states or between control states themselves. Everything else follows Figure 5 Panel (a).

Table A1: Abortion Laws Across States

State	Protective law	Trigger ban	Pre-Roe ban	Near-total ban	Six-week gestational ban	TRAP law
AK	N	N	N	N	N	N
AL	N	N	Y	Y	N	Y
AR	N	Y	Y	Y	N	Y
AZ	N	N	Y	N	N	N
CA	Y	N	N	N	N	N
CO	Y	N	N	N	N	N
CT	Y	N	N	N	N	N
DC	Y	N	N	N	N	N
DE	Y	N	N	N	N	N
FL	N	N	N	N	Y*	Y
GA	N	N	N	N	Y	N
HI	Y	N	N	N	N	N
IA	N	N	N	N	N	N
ID	N	Y	N	Y	N	N
IL	Y	N	N	N	N	N
IN	N	N	N	Y*	N	Y
KS	N	N	N	N	N	Y
KY	N	Y	N	Y	N	N
LA	N	Y	N	Y	N	Y
MA	Y	N	N	N	N	N
MD	Y	N	N	N	N	Y
ME	Y	N	N	N	N	N
MI	N	N	Y	N	N	N
MN	N	N	N	N	N	N
MO	N	Y	N	Y	N	Y
MS	N	Y	Y	Y	N	Y
MT	N	N	N	N	N	N
NC	N	N	N	N	N	N
ND	N	Y	N	Y	N	Y
NE	N	N	N	N	N	N
NH	N	N	N	N	N	N
NJ	Y	N	N	N	N	N
NM	N	N	N	N	N	N
NV	Y	N	N	N	N	N
NY	Y	N	N	N	N	N
OH	N	N	N	N	N	N
OK	N	Y	Y	Y	N	Y
OR	Y	N	N	N	N	N
PA	N	N	N	N	N	Y
RI	Y	N	N	N	N	N
SC	N	N	N	N	N	N
SD	N	Y	N	Y	N	N
TN	N	Y	N	Y	Y	Y
TX	N	Y	Y	Y	Y	Y
UT	N	Y	N	N	N	N
VA	N	N	N	N	N	Y
VT	Y	N	N	N	N	N
WA	Y	N	N	N	N	N
WI	N	N	Y	N	N	Y
WV	N	N	Y	Y	N	N
WY	N	Y	N	N	N	N

Note: This table lists state laws regarding abortion used in this paper. Data on state laws protecting abortion (enacted before the Dobbs decision) are from Kaiser Family Foundation (2022a). Data on states banning abortion (either a near-total ban or a six-week gestational ban) are based on the beginning-of-the-month snapshots of Guttmacher Institute (2024) on Wayback Machine between 2022 June and 2024 May and Google searches conducted in October 2024. Data on TRAP laws are based on Austin and Harper (2019), which is updated through mid-2016. We exclude Indiana from our analysis because its near-total ban took effect in August 2023, leaving us fewer than six months of post-ban period since our yield data ends in December 2023. Similarly, we exclude Florida because its six-week gestational ban went into effect in May 2024. See Appendix Figure A1 for a plot of effective timings of various state laws.

Table A2: Effect of Abortion Bans on Municipal Bond Yields

	Dependent variable: Yield (bp)
Treat×-6M	1.540
	(1.211)
Treat×-5M	0.986
	(1.108)
Treat×-4M	1.811*
	(1.038)
Treat×-3M	-1.053
	(0.830)
Treat×-2M	0.170
	(0.440)
Treat×0M	2.647***
	(0.568)
Treat×1M	2.730***
	(0.911)
Treat×2M	4.305***
	(1.059)
Treat×3M	7.382***
	(1.446)
Treat×4M	6.500***
	(1.507)
Treat×5M	8.758***
	(1.501)
Treat×6M	11.747***
	(1.413)
R^2	0.754
Outcome mean	302.008
Bond characteristics	Y
Year-month FE	Y
County FE	Y

Note: This table reports the coefficients plotted in Figure 1. Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table A3: Effect of Abortion Bans on Municipal Bond *Rating*

	Dependent variable: Rating				
	(1)	(2)	(3)	(4)	(5)
Avg Treatment Effect	-0.005*	-0.008	-0.005	-0.001	0.000
	(0.003)	(0.006)	(0.004)	(0.002)	(0.002)
R^2	0.921	0.921	0.921	0.921	0.999
Outcome mean	17.934	17.934	17.934	17.934	17.934
Bond characteristics	Y	Y	Y	Y	Y
Year-month FE	Y	Y	N	N	N
County FE	Y	Y	Y	Y	N
Economic controls	N	Y	N	N	N
Governor election×Year-month FE	N	N	Y	N	N
Tax cut×Year-month FE	N	N	N	Y	N
CUSIP FE	N	N	N	N	Y

Note: This table repeats Table 2 using bond rating as the outcome. Bond rating is the numeric value of a bond's long-run underlying credit rating in a month. The highest rated bonds (AAA) are assigned a value of 21, the second highest (AA+) 20, and so forth to the lowest rated bonds (C), which are assigned a value of 1. Everything else follows Table 2 except that we exclude bond rating from the controls. Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table A4: Effect of Abortion Bans on Municipal Bond Yield
Exclude One Ban State A Time

	Excluded state													
	AL	AR	ID	IN	KY	LA	MO	MS	ND	OK	SD	TN	TX	WV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Avg Treatment Effect	6.492	6.314	6.312	6.290	6.246	6.362	5.971	6.334	6.498	6.560	6.314	6.254	6.246	5.354
	(1.791)***	(1.800)***	(1.792)***	(1.791)***	(1.800)***	(1.780)***	(1.770)***	(1.792)***	(1.891)***	(1.748)***	(1.792)***	(1.791)***	(1.827)***	(1.923)***
R^2	0.753	0.753	0.754	0.754	0.753	0.754	0.753	0.754	0.766	0.731	0.753	0.753	0.755	0.760
Outcome mean	302.016	301.989	302.008	302.008	301.988	302.046	302.014	302.045	297.045	304.527	302.011	302.049	302.402	296.965
Bond characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: This table reports robustness tests for Table 2 column (1) by excluding one ban state at a time. The column header lists the ban state being excluded. Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table A5: Effect of TRAP Laws on Bond Yields, State Business Dynamics, and Cross-State Migration

	Municipal bond	State business dynamics		Cross-state migration			Cross-state job flow (fertile women)		
	(1) Yield (bp)	(2) Firms per 100k	(3) Net job creation rate (%)	(4) Net flow (in–out)	(5) Inflow	(6) Outflow	(7) Net flow (in–out)	(8) Inflow	(9) Outflow
TRAP × -3Y	0.818	1.287	-0.151	-0.402	-0.278	0.124	-0.696	-1.126	-0.431
	(1.363)	(3.095)	(0.257)	(0.264)	(0.395)	(0.224)	(1.310)	(1.886)	(0.891)
TRAP × -2Y	-1.464	2.502	-0.198	-0.295	-0.359	-0.064	0.378	-0.227	-0.606
	(1.070)	(2.348)	(0.222)	(0.286)	(0.252)	(0.190)	(0.661)	(1.098)	(0.617)
TRAP × 0Y	0.965	-8.814	-0.519	-1.291	-0.666	0.625	-1.397	-0.938	0.459
	(1.061)	(1.938)***	(0.197)***	(0.256)***	(0.213)***	(0.123)***	(0.876)	(0.609)	(0.424)
TRAP × 1Y	4.752	-14.128	-0.265	-1.041	-1.073	-0.032	-2.261	-1.372	0.889
	(2.059)**	(2.991)***	(0.158)*	(0.309)***	(0.316)***	(0.135)	(0.625)***	(0.589)**	(0.620)
TRAP × 2Y	3.460	-14.604	-0.519	-1.730	-1.380	0.350	-4.823	-3.218	1.605
	(2.766)	(4.019)***	(0.287)*	(0.788)**	(0.471)***	(0.402)	(2.460)**	(1.498)**	(1.225)
TRAP × 3Y	2.227	-16.250	-0.544	-1.661	-1.325	0.336	-5.768	-4.260	1.508
	(3.771)	(5.245)***	(0.487)	(1.072)	(0.552)**	(0.652)	(2.982)*	(2.409)*	(1.039)
R^2	0.760	0.998	0.774	0.737	0.953	0.980	0.787	0.976	0.988
Outcome mean	298.664	2,079.461	0.991	0.443	24.875	24.433	0.628	54.706	54.078

Note: This table presents dynamic treatment effects of TRAP laws on states that ever enacted TRAP laws relative to states that never. Outcomes are monthly secondary-market yield (in bp) in column (1) and number of firms per 100,000 population, net job creation rate, annual net flow per 1,000 population to a state, inflow per 1,000 population to a state, outflow per 1,000 population from a state, net job flow per 1,000 fertile women to a state, job inflow per 1,000 fertile women to a state, and job outflow per 1,000 fertile women from a state in columns (2)-(9), respectively. Net job creation rate is calculated as the number of job creation less the number of job destruction, divided by the Davis-Haltiwanger-Schuh denominator (i.e., the average of employment in years t-1 and t) and multiplied by 100. Event time 0 denotes the year when a state first enacted TRAP laws; only states enacted TRAP laws between 2000 and 2016 are considered (see Section 4.1 for details). The omitted period is -1, i.e., the year before enactment. See Equation 4 in Section 4.2 for details about the specification. Coefficients are plotted in Figure 3, Figure 4, Figure 5, and Figure 6, respectively. Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table A6: Effect of TRAP Laws on Bond Yields, State Business Dynamics, and Cross-State Migration Controlling for *State Income Tax*

	Municipal bond	State business dynamics		Cross-state migration			Cross-state job flow (fertile women)		
	(1) Yield (bp)	(2) Firms per 100k	(3) Net job creation rate (%)	(4) Net flow (in–out)	(5) Inflow	(6) Outflow	(7) Net flow (in–out)	(8) Inflow	(9) Outflow
TRAP×-3Y	1.441	1.320	-0.151	-0.392	-0.267	0.124	-0.771	-1.098	-0.327
	(1.331)	(3.290)	(0.259)	(0.275)	(0.394)	(0.215)	(1.347)	(1.887)	(0.861)
TRAP×-2Y	-1.377	2.476	-0.197	-0.284	-0.353	-0.069	0.392	-0.221	-0.612
	(1.075)	(2.374)	(0.222)	(0.286)	(0.252)	(0.189)	(0.662)	(1.099)	(0.613)
TRAP×0Y	0.635	-8.986	-0.525	-1.303	-0.685	0.618	-1.292	-0.955	0.337
	(0.831)	(2.024)***	(0.197)***	(0.250)***	(0.213)***	(0.123)***	(0.850)	(0.615)	(0.405)
TRAP×1Y	4.433	-14.333	-0.273	-1.068	-1.099	-0.031	-2.149	-1.392	0.757
	(1.779)**	(3.071)***	(0.157)*	(0.302)***	(0.316)***	(0.142)	(0.610)***	(0.596)**	(0.597)
TRAP×2Y	2.459	-14.748	-0.527	-1.714	-1.378	0.336	-4.615	-3.228	1.387
	(2.206)	(4.051)***	(0.290)*	(0.780)**	(0.476)***	(0.393)	(2.434)*	(1.521)**	(1.176)
TRAP×3Y	1.284	-16.339	-0.545	-1.591	-1.296	0.295	-5.562	-4.249	1.314
	(3.085)	(5.258)***	(0.487)	(1.073)	(0.566)**	(0.647)	(2.956)*	(2.423)*	(0.991)
R^2	0.760	0.998	0.775	0.743	0.954	0.980	0.788	0.976	0.988
Outcome mean	298.677	2,079.461	0.991	0.443	24.875	24.433	0.628	54.706	54.078

Note: This table presents robustness checks for Table A5 by additionally controlling for annual top state wage tax rate from NBER TaxSim. Everything else follows the corresponding table and column. Standard errors are clustered by state.
*** 1%, ** 5%, * 10% significance level

Table A7: Effect of Overturning Roe v. Wade on *Firm Values*: with controls
Cumulative Abnormal Returns (CARs)

	HQ in ban states			By HQ ex-ante abortion utilization			By HQ Δ distance to clinic		
	(0,+2)	(0,+5)	(0,+20)	(0,+2)	(0,+5)	(0,+20)	(0,+2)	(0,+5)	(0,+20)
Avg. CAR	32.196	-2.006	-116.327						
	(39.368)	(55.442)	(121.580)						
High utilization				-89.198	-164.810**	-309.613**			
				(70.283)	(81.860)	(136.394)			
High change							-101.618	-164.401**	-188.018
							(68.897)	(80.700)	(137.157)
Intercept				82.515	90.966	58.331	93.050	96.445	-3.733
				(62.748)	(77.300)	(141.503)	(62.068)	(79.188)	(151.988)
Firm-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	513	513	513	513	513	513	513	513	513
R^2	0.00	0.01	0.08	0.01	0.02	0.09	0.01	0.02	0.08

Note: This table reports the effects on CARs following the overturn of Roe v. Wade. CARs are calculated using the Fama-French four-factor model based on data in the -101 through -2 days from the abortion ban date; units are bps. Columns (1)-(3) report the average abnormal returns for firms headquartered in ban states. Columns (4)-(6) explore heterogeneous effects for firms headquartered in treated counties with an above-median ("High utilization") pre-Dobbs abortion rate. Columns (7)-(9) explore heterogeneous effects for firms whose headquarter county see an above- (*High change*) in distance within each state to the nearest abortion clinic after the overturn. Firm-levels controls includes B/M, ROA, and net profit margin, calculated from the last quarterly disclosure. Robust standard errors are reported in parentheses.
*** 1%, ** 5%, * 10% significance level