Roe v. Rates:
Reproductive Healthcare and Public Financing Costs

Runjing Lu Zihan Ye*

August 31, 2023

Abstract

After the U.S. Supreme Court overturned Roe v. Wade, states with abortion "trigger" bans tied to the decision see an increase in municipal bond yields relative to states with preexisting laws protecting abortion. The effects are stronger in counties where access to abortion services decrease more after the court ruling, where residents are more accepting of reproductive healthcare, and which rely more on female workforce. Using the stock market's reaction following the Court's decision and the staggered state-level adoption of laws targeting abortion providers, we identify deteriorated firm value, worsening business dynamism, and net out-migration of residents as key factors underlying the rise in municipal bond yields. Together, our results highlight the importance of reproductive healthcare policies in driving local economies and public financing costs.

Keywords: Reproductive healthcare, TRAP laws, Public finance, Municipal bonds
JEL Classification Numbers: H74, G12, I18

*Runjing Lu: University of Alberta (runjing1@ualberta.ca); Zihan Ye: University of Tennessee (zihan.ye@utk.edu). We thank John Barrios, Jess Cornaggia, Kimberly Cornaggia, Gordon Dahl, Joey Engelberg, Mark Huson, Xuelin Li, William Mullins, Lukas Roth, Matthew Serfling, and Sheng-Jun Xu for valuable comments. We thank Banks Osborne, Zhi Sheng and Zihao Ye for excellent research assistance.

1. Introduction

The right to access reproductive healthcare plays a critical role in advancing gender equality and enhancing women's economic well-being. Prior studies have shown that access to reproductive healthcare can affect women's educational investment, labor force participation, and engagement in professional occupations and entrepreneurship (Ananat et al., 2009, Bailey, 2006, Goldin and Katz, 2002, Zandberg, 2021, Ravid and Zandberg, 2022). Yet, this access is now threatened. On June 24, 2022, the U.S. Supreme Court overturned Roe v. Wade, ending a half-century of federal constitutional protection of abortion rights, and prompting sweeping changes in reproductive healthcare policies across states. These changes could have ramifications beyond the legal and political realms, leading to major impacts on local economies. For example, following the court ruling, some of the biggest businesses in the U.S. announced they would expand or allow employees to relocate away from states that enact abortion bans.[1] In a survey commissioned by Bloomberg (2022), nearly half of working adults said they would consider moving to abortion-friendly states. Such geographic shifts in business activities and changes in the labor supply could affect local business dynamism and reshape the relative competitiveness of states with different reproductive healthcare policies. This paper estimates the economic implications of these policies, providing important evidence that could inform lawmakers in policy making.

In this paper, we explore how reproductive healthcare policies are priced in the municipal bond market and quantify the economic impacts therein. Municipal bonds offer a useful setting for measuring investors' expectations of policy-induced economic effects because bond repayments depend on local government cash flows, and ultimately, local economic conditions. As such, we can translate effects on asset prices into more general economic effects on impacted communities and obtain a market-based assessment of policy effects as they happen.

We exploit variations in preexisting state laws on abortion and the overturning of Roe v. Wade to show that restricting access to reproductive healthcare increases public financing costs. After the Court's decision, states with abortion bans that are designed to be triggered

[1] See CNBC (2022), CBS (2022a), Washington Post (2022), Financial Times (2022).

by Roe's overturn see an increase in municipal bond yields relative to states with preexisting laws protecting abortion. The effects are stronger in counties with sharper declines in access to abortion services after the court ruling, where residents are more accepting of reproductive healthcare, and which rely more on female labor force. By analyzing the stock market's reaction to the court decision and the staggered state-level adoption of laws targeting abortion providers, we identify deteriorated firm value, worsening business dynamism, and net out-migration as important factors underlying the rise in municipal bond 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. Meanwhile, 13 states have abortion "trigger" bans in place, which are designed to quickly ban abortion if Roe were overruled. In contrast, 16 states and D.C. have enshrined protection of abortion in state laws without relying on the Roe decision. We exploit this difference in preexisting state laws and the timing of the leaked draft and the court ruling to study the effect of restricting access to reproductive healthcare on municipal bond yields and local economies.

Using a difference-in-differences (DID) event study framework, we compare municipal bond yields in states with trigger laws (treatment states) vs. those in states with laws protecting abortion (control states), before vs. after the draft leakage and the ensuing court decision. We find that secondary market bond yields in treatment states increase by 7-11 basis points (bps) relative to those in control states after the Dobbs decision, equivalent to roughly 3% of the sample mean. The findings also confirm that treatment and control states experience parallel pre-trends in yields. Moreover, we find a relative increase of 20-23 bps in primary market offering yields in treatment states. Our results are robust to alternative definitions of treatment states, sampling criteria, and regression specifications. Falsification tests using data from the months prior to the decision as placebo treatments yield small and insignificant estimates.

While the leaked draft and the Dobbs decision constitute an exogenous shock to abortion access in treatment states, areas' exposure to it depends on whether and how access has changed, local attitudes, and local reliance on female workforce. To further tighten the link between abortion access and municipal bond yields, we use the change in the distance to the nearest abortion clinic before vs. after the Dobbs decision to identify counties in each state where access has decreased the most. We find that the relative increase in yields is twice the size in treated counties that experience an above-median increase in the distance, compared to other treated counties. 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 view abortion as morally acceptable. We find that the relative increase in yields is concentrated in treated counties that are more open to abortion: those with a lower share of religious population and those with a higher share of residents holding a 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.

We also explore effect heterogeneity along two important determinants of bonds' fundamental risk: maturity and credit rating. We find that the increase in yields predominantly comes from bonds with a longer time to maturity and a lower credit rating. These results imply that investors perceive the change in reproductive healthcare access as a long-run fundamental risk that would impact future cash flows of local governments, especially those with a higher ex-ante default risk.

We use a model developed by Goldsmith-Pinkham et al. (2022) to interpret the economic significance of our findings. This model, building on Merton's (1974) model of credit risk, allows us to convert the estimated effects on bond yields into changes in the distribution of local government cash flows along two dimensions—changes in the cash flow level and its volatility. We find that, depending on the leverage ratio of the municipal issuer (ranging from 0.1 to 0.7), the 7.0 bp yield increase estimated in our more stringent specification translates into a decrease of 2.3% to 5.3% in the present value of the underlying cash flows, an increase of 1.4% to 2.4% in the volatility of cash flows, or a combination of the two. This model-implied effect on local government cash flows suggests that investors expect a substantial negative impact of restrictive reproductive care policies on local economies.

Finally, we investigate potential channels underlying the increase in municipal bond yields. To do so, we study how restrictive reproductive healthcare policies affect two vital determinants of local economies and tax bases: firms and residents. We first show that the value of impacted firms fall after the overturn of Roe: cumulative abnormal returns (CARs) over the 10 days after the court ruling are roughly 1% lower for firms headquartered in trigger law states than in control states. To overcome the lack of recent data following Dobbs, we turn to the staggered state-level adoption of Targeted Regulation of Abortion Providers (TRAP) laws. TRAP laws enforce stringent regulations on abortion providers and have been shown to reduce abortion (Arnold, 2022, Jones and Pineda-Torres, 2021, Zandberg, 2021). Following Sun and Abraham (2020), we use a dynamic DID event study to compare states that enacted TRAP laws with states that never enacted one during the sample period. The results show a 5 bp relative increase in municipal bond yields in the three years after a state's first enactment of TRAP laws. This finding indicates that our documented effect of the Dobbs decision is likely generalizable to other policies restricting access to reproductive healthcare. Moreover, using the Business Dynamics Statistics data from the U.S. Census Bureau and state-to-state migration based on IRS tax filings, we show that states that enact TRAP laws experience declines in firms per capita and net job creation, as well as increased out-migration and decreased in-migration. These results suggest that deteriorated firm value, declining business dynamism, and net out-migration likely explain the increase in public financing costs following the implementation of policies that restrict access to reproductive healthcare.

Our paper relates to two strands of literature. First, we contribute to a growing literature studying the effects of public health on government financing costs, such as Medicaid expansion, aging population, opioid abuse, marijuana legalization, and telehealth provision (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 reproductive healthcare—on financing costs for state and local governments.[2]

Second, we contribute to a broad literature studying the effects of access to reproductive healthcare.[3] 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 engagement in professional occupations 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 (Miller et al., 2023), to negatively impact the next generation's education and labor market performance (Pop-Eleches, 2006), and to prompt female auditors to relocate (Lin et al., 2023). These studies provide valuable insights into how access to reproductive healthcare affects *individuals'* health and socioeconomic outcomes, leading to potential aggregate effects. Building upon these studies, we present the first evidence of the *aggregate* economic consequences of restricting access to reproductive healthcare on state and local governments, and identify firm value, business dynamism, and residential migration as important driving forces.

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

The paper proceeds as follows. Section 2 describes the institutional background, data, and sample. Section 3 presents the empirical strategy and results regarding effects of the overturning of Roe v. Wade. Section 4 explores underlying channels using equity market's

[2]More broadly, our findings add to the 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), 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), underwriting processes (Garrett, 2021, Garrett and Ivanov, 2022), and COVID-induced migration (Gustafson et al., 2023).

[3]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 et al., 2023, Simintzi et al., 2023).

response to the Dobbs decision and the effects of TRAP laws. Section 5 concludes.

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, California, 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 laws that impose stringent (often medically unnecessary) requirements on abortion providers (Guttmacher Institute, 2021). 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).[4] Most of these policies 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. In addition, nine states have pre-Roe abortion bans on the books, and 11 states have early gestational age bans that are blocked by court orders.[5] These *preexisting* differences in state laws on abortion creates variations in state-level abortion access *after* Roe was overturned. We exploit these differences in laws as well as the timing of the leaked draft opinion and the Dobbs decision to study the economic implications of restricting access to reproductive healthcare. In the main analysis, we define treatment states as those with trigger bans as of May 2022 because these laws signal the strongest intention to ban abortion compared to other bans. In the Appendix, we expand the treatment definition to include states with pre-Roe bans or early gestational age bans and find similar results.

2.2 Potential impact on municipal bonds

Municipal bond investors care about the cash flows of local governments. The payoff structure of municipal bond investments suggests that investors are primarily concerned with how policies generate downside risk for communities that back the cash flows for debt payment. Reproductive healthcare policies may impact municipal bond pricing through multiple channels.

[4]These states are California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Maine, Maryland, Massachusetts, Nevada, New Jersey, New York, Oregon, Rhode Island, Vermont, Washington, and D.C.

[5]States with trigger bans are Arkansas, Idaho, Kentucky, Louisiana, Mississippi, Missouri, North Dakota, Oklahoma, South Dakota, Tennessee, Texas, Utah, and Wyoming. States with abortion laws predating Roe v. Wade are Alabama, Arizona, Arkansas, Michigan, Mississippi, Oklahoma, Texas, West Virginia, and Wisconsin. States with blocked early gestational age bans are Georgia, Idaho, Iowa, Kentucky, Mississippi, North Dakota, Ohio, Oklahoma, South Carolina, Tennessee, and Texas. In states with multiple bans, state officials will determine which ban to enforce when Roe is overruled. See Guttmacher Institute (2022) for more details.

Policies that restrict reproductive healthcare access can negatively impact two important determinants of a state's tax base: residents and businesses. First, limiting reproductive care 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 reproductive healthcare affects female labor force participation and their engagement in professional occupations and entrepreneurship (e.g., Bailey, 2006, Zandberg, 2021, Ravid and Zandberg, 2022). Second, restricting reproductive care may prompt firms to relocate and increase their financial burden. For example, following the Dobbs decision, many U.S. companies 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). 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 4, we explore these channels in detail.

In addition, diminished access to reproductive healthcare 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 reproductive healthcare 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 with restrictive reproductive healthcare policies will on average have higher yields.

2.3 Data

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 of municipal bonds in the U.S. and includes information on transaction-level data, such as transaction date, yield, price, and trade size. Following the literature, we restrict our sample to bonds issued via conventional channels (e.g., limited offerings, private placements, and remarketing) 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 focus on bonds issued directly by state, county, and city governments and geolocate each bond based on the issuer's county information from Bloomberg.[6] Finally, we focus on bonds issued prior to the leak of the Supreme Court draft opinion to avoid effects being contaminated by changes in bond issuance.

Our main measure of public financing cost is municipal bond yield in secondary markets. We calculate monthly bond yield using the trade size-weighted average yield across all transactions for each bond in a given month.[7] 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.[8] We also examine bond credit ratings to gauge whether and how credit rating agencies adjust their assessments of

[6]Bloomberg classifies issuers as "state," "county," "city," or "unidentified." We complement this information by categorizing "unidentified" issuers using issuer names from Mergent. Specifically, we categorize issuers as states if their names include "st", "state", "commonwealth", or state names (full or postal abbreviation); as counties if names include "county", or "cnty"; and as cities if names include "city".

[7]If yields are missing, we calculate them using prices.

[8]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/.

bond default risks. We use the credit rating at the time of each transaction for the secondary market analysis.[9]

We also construct a sample of primary market issuance transactions using the Mergent database, which covers the universe of primary market issuance of municipal bonds in the U.S. Following the literature, we focus on bonds offered via conventional channels in the 50 states and D.C. In addition, we restrict our sample to bonds issued by state, county, or city governments with non-missing offering amounts and coupon rates that represent new borrowing.

2.4 Summary Statistics

We present summary statistics of our samples in Table 1. Panel A reports bond characteristics for all bonds in our secondary -market sample from October 2021 through December 2022. We observe that the average yield is about 317 bps, the average spread between bond yield and the corresponding Treasury yield is 149 bps, average time to maturity is 8 years, and average rating is 18 (equivalent to AA- or Aa3 rating). In addition, about 71% of transactions pertain to callable bonds, 19% pertain to insured bonds, 46% pertain to general obligation bonds, and 45% pertain to bonds issued through negotiation (instead of competitive offerings).

Panel B compares bond characteristics between states with trigger laws (treatment states) and states with laws protecting abortion (control states) in the pre-shock period (October 2021 – April 2022). While there are more transactions in control states than in treatment states (108,733 vs. 44,136), the average yields and spreads are similar between the two groups (258 and 149 bps in the treatment group vs. 262 and 155 bps in the control group), and the difference is statistically insignificant. In addition, the average bond ratings between the treatment and control states are very close, suggesting that issuers in these states share similar creditworthiness and debt obligations before the shock. Finally, bonds in treatment states have a longer maturity, are more likely to be insured, and are smaller. We control for these bond characteristics in all regressions where applicable.

[9]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.

3. The Effects of Overturning Roe v. Wade

3.1 Event study around the Dobbs decision

Our first approach is a DID event study design contrasting bonds in states with trigger bans vs. bonds in states where abortion is protected when Roe v. Wade is overturned. We estimate the following regression:

$$\mathrm{Y}_{i,t} = \sum_{t=-7;t\neq-3}^{7} \beta_t \times Trigger_{s(i)} + \delta' X_{i,t} + \alpha_t + \alpha_i + \epsilon_{i,t} \tag{1}$$

where $Y_{i,t}$ is the outcome of interest, such as yield, spread, and rating, for bond i in month t. $Trigger_{s(i)}$ is 1 if bond i is issued in state s with a trigger law, and 0 in a state with a preexisting law that protects access to abortion.[10] Event time t indexes the number of months relative to the leaked Supreme Court draft majority opinion striking down Roe v. Wade in May 2022. We define $t = 0$ as the month of the draft leakage and omit $t = -3$ to form the reference period. We include event months up to +7, as the bond data are only available through December 2022. To ensure symmetry, we set the beginning of the sample period to month −7 (October 2021). Following Gao et al. (2020) and Cornaggia et al. (2022a), we control for a vector of bond characteristics $X_{i,t}$ consisting of 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.[11] We include year-month fixed effects α_t to absorb any national time trends in the bond market, and bond fixed effects α_i to force comparison within the same bond. We double-cluster standard errors by state and year-month to account for the cross-sectional and time-series correlations in the residual terms.

A key assumption of the DID event study methodology is that trends of bond outcomes between treatment and control states would have been parallel in the absence of the Dobbs decision. In this case, the β_t vector in Equation 1 identifies the causal impact of restricting

[10] As shown in the Appendix, we expand the treatment definition to include pre-Roe abortion bans and early gestational age bans; results are somewhat smaller in magnitude, but still significant.

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

reproductive healthcare on public financing cost. As shown below, this assumption appears to hold.

Figure 1 panel (a) plots the β_t coefficients from Equation 1, capturing how the draft leakage and the overturning of Roe v. Wade change bond yields for trigger law states relative to control states over the event window. The plot shows no pre-trends leading up to the date the draft was leaked, a small yet insignificant uptick in relative yields in the month the draft was leaked, and a significant increase after the Court's ruling. The estimated effect grows from 2 bps in month 2 to 6 bps in months 5 through 7.

Next, we shift our focus to general obligation (GO) bonds. GO bonds are backed by a local government's tax base and taxing authority. If reproductive healthcare policies indeed affect local economies and the tax bases of local governments, the effect on bond yields should be concentrated in GO bonds. Figure 1 panel (b) zooms in on GO bonds and plots the β_t coefficients from Equation 1. Compared to control states, GO bonds in trigger law states see a significant 3 bp relative jump in yields in month 2, right after the Court struck down Roe v. Wade; the gap between trigger law and control states increases to 9 bps in months 5 through 7, roughly 33 % higher than the effect among all bonds shown in panel (a). In the remainder of this paper, we focus on GO bonds.

3.2 Difference-in-differences design

To summarize the coefficients into an average treatment effect over the months following the Dobbs decision, we estimate the following equation:

$$Y_{it} = \beta \times Trigger_{s(i)} \times Post_t + \delta' X_{i,t} + \alpha_t + \alpha_{s(i)} + \epsilon_{it} \qquad (2)$$

where $Y_{i,t}$ demotes the outcomes of interest, such as yield, spread, and rating, for bond i in month t. Post_t is an indicator equal to 1 for the months after the Supreme Court overturned Roe v. Wade on June 24, 2022; we exclude May and June 2022 to avoid potential anticipation effects after the draft opinion was leaked. We include bond characteristics $X_{i,t}$, state fixed effects $\alpha_{s(i)}$, and year-month fixed effects α_t. In more demanding specifications, we add bond fixed effects α_i, Republican governor $\times$ year-month fixed effects, and state-level quarterly GDP and monthly unemployment rate to control for time-invariant bond characteristics, contemporaneous shocks to states with Republican governors, and state-level economic conditions. We double-cluster standard errors by state and year-month.

Table 2 reports the estimates from Equation 2. We include increasingly stringent fixed effects and controls moving from column (1) to (4). Consistent with the patterns revealed by the DID event study, coefficients on $Trigger \times Post$ are positive and statistically significant. Column (2), which includes CUSIP fixed effects and year-month fixed effects, shows a point estimate of 7.0. In other words, bonds in states with trigger laws experience a 7 bp increase in yields, on average, relative to bonds in states with laws protecting access to abortion after Roe v. Wade was overturned. It is worth noting that all states with trigger laws were led by Republican governors in 2022, except for Kentucky and Louisiana. To check whether the increase in bond yields around the court decision is due to common shocks to states with Republican governors, we include Republican governor × year-month fixed effects in column (3). The point estimate increases to 8 bps, suggesting that the documented effect cannot be explained by contemporaneous shocks to Republican states. Another concern is that the effect is driven by changes in economic conditions unrelated to reproductive healthcare policies in trigger law states. To alleviate this concern, we further include state-level monthly unemployment rate and quarterly GDP in column (4), which shows an even stronger effect of over 9 bps. Across the columns, our estimated effects are non-trivial, equivalent to 2.4% to 3.6% of the sample mean. In Appendix Table A2, we examine bond spread as an alternative outcome and find similar results.

Thus far, we have controlled for contemporaneous bond credit rating. In other words, the relative increase in yields in trigger law 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 also change in response to the Dobbs decision. In Appendix Table A3, we re-estimate Table 2 using bond rating as the dependent variable. 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. Although the signs of the coefficients for $Trigger \times Post$ are negative, they are economically small (between -0.12

and -0.010) and almost always statistically insignificant. 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 reproductive healthcare policies when economic impacts have not yet manifested in the financial statements of exposed municipalities.

In our main analysis, we define treatment states as those with trigger bans in place before the Dobbs decision. For robustness, in Appendix Table A4, we expand the treatment definition to include states with pre-Roe abortion bans and early gestational age bans. The estimated effects are consistent with Table 2, albeit smaller in magnitude. The smaller effect size is not surprising, as the two other bans are either legacy laws passed before 1973 or do not completely ban abortion, in contrast to trigger bans specifically designed to take effect once Roe v. Wade is struck down.

Our identification strategy relies on a single shock, giving rise to the concern that the effect is merely an artifact of seasonality or random noise in municipal bond yields. To alleviate this concern, we conduct falsification tests. In Table A5, we replicate Table 2 using 2019 or 2021 as the placebo treatment year.[12] All of the coefficient estimates are insignificant and small, suggesting that our main effect cannot be explained by seasonality or random noise.

Finally, our main analysis focuses on how investors price municipal bonds in *secondary* markets to ensure that the effect on yields is not contaminated by governments' strategic issuance behavior. In Table 5, we examine offering yields in *primary* markets. Column (1) reveals that the offering yields in trigger law states are 23 bps higher than those in control states after the Court struck down Roe v. Wade. Because each municipal bond is issued only once, we can not include bond fixed effects. As an alternative, we include issuer fixed effects in column (2) and obtain similar results. In column (3), we include Republican governor × year-month fixed effects; results remain similar.

[12]We do not use 2020 because it coincides with the height of the COVID-19 pandemic.

3.3 Cross-sectional heterogeneity

The analyses in prior sections exploit variations in preexisting state laws regarding abortion rights. In this section, we explore *within-state* variations in exposure to the overturning of Roe v. Wade. Specifically, we utilize data on distance to the nearest abortion clinic, local attitudes toward abortion, and local reliance on female workforce to identify counties that are more affected by the Court's ruling. This set of results further tightens the link between changes in reproductive healthcare policies and increased public financing costs.

The overturning of Roe v. Wade abruptly eliminates legal abortion access in states with trigger laws. As such, women in these states who seek abortions must travel farther to reach providers. Appendix Figure A1 plots the cumulative change in the distance between a county's population centroid and the nearest abortion clinic between October 2021 and December 2022 separately for states with trigger laws and states with laws protecting access to abortion, using data from Myers (2023). While counties in control states experience no change in the distance to the nearest abortion clinic, for counties in trigger law states, the distance increase by 60 miles in July 2022, continuously increasing to 170 miles by September 2022. 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 abortions from accessing providers (Myers, 2021).

If effects documented in previous sections are indeed driven by decreased access to reproductive healthcare, bond yields should have increased more in treatment counties with a greater increase in the distance to the nearest abortion provider. We thus split treatment counties within each state by whether they have an above- vs. below-median change in the distance to the nearest abortion provider and separately examine their bond yields relative to those in the control states.[13] We replace $\beta_t \times Trigger_{s(i)}$ in Equation 1 with $\beta_t \times Trigger_{s(i)} \times High\ change$ and $\beta_t \times Trigger_{s(i)} \times Low\ change$ and plot the coefficients in Figure 2 panel (a). While bonds in both high and low-change treatment counties share parallel pre-trends with those in control counties, the trends diverge after the overturning of Roe v. Wade. Bonds in high-change treatment counties experience a 6 bp relative increase

[13]We exclude bonds issued by state governments in this and the following analyses using county-level characteristics because county identifiers are required to assign these characteristics to bonds.

in yields in month 2 whereas those in low-change counties increase by only 1.5 bps. Although yields in both types of treatment counties continue to rise through the end of the sample period, the gap persists. Table 3 column (1) summarizes the monthly treatment effects into an average treatment effect: bond yields in high-change treatment counties increase by 7 bps compared to control counties after the overturning of Roe v. Wade, while yields in low-change treatment counties rise by less than half that amount.

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 restrictions implies lower perceived negative effects of such policies among residents, who are major holders of municipal bonds (Bagley et al., 2022). Greater support 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 based on the 2010 Religious Congregations and Membership Study (ARDA, 2000) and responses to questions about abortion in the Gallup Poll Social Series (GPSS) between 2013 and 2021. We split treatment counties by whether they have an above- vs. below-median share of religious residents or GPSS respondents who view abortion as morally acceptable within each state. We plot coefficients of the interactions between indicators for high and low local religiosity (acceptance of abortion) and $\beta_t \times Trigger_{s(i)}$ in Figure 2 panel (b) (panel c). Panel (b) reveals that yields in treatment counties with high and low religiosity are similar to those in control counties before the draft leakage. However, relative yields in treatment counties with low religiosity, where residents are more likely to be pro-choice, jump after the draft leakage and ramp up to 12 bps by month 7, while those in treatment counties with high religiosity, where residents are more likely to be pro-life, remain flat. Panel (c) demonstrates a similar divergence after the draft opinion was leaked. Relative bond yields in treatment counties where abortion is viewed as more morally acceptable rise while those in counties where it is viewed as less acceptable drop, with the gap widening to 12 bps (+11 vs. -1 bp) by month 7. In Table 3, columns (2) and (3) report the corresponding average treatment effects. Bond yields in treatment counties with low religiosity and high abortion acceptance increase by 9 bps relative to the control counties, whereas yields in other treatment counties are not statistically distinguishable from those in control counties.

The availability of reproductive healthcare has been shown to have a significant impact on women's participation in the labor force and occupational decisions (Zandberg, 2021). Consequently, areas that rely heavily on female workforce could experience more pronounced negative effects due to restrictions on reproductive healthcare. 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).[14] In Table 3 columns (4) and (5), we examine effects for treatment 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. Consistent with our conjecture, bond yields in treatment counties that were heavily dependent on female workforce increased by 6.5 bps, but the increase is negligibly in counties that are less dependent on female workforce

Taken together, the analyses exploiting within-state heterogeneities demonstrate that increases in municipal bond yields are indeed attributable to reduced access to reproductive healthcare following the Dobbs decision rather than other contemporaneous policies or shocks that may have disproportionately affected states with trigger bans.

Finally, we explore effect heterogeneities along two important determinants of bonds' fundamental risk: maturity and credit rating. In Table 4 columns (1) and (2), we replicate Table 2 column (2) separately for long-term bonds and short-term bonds. We define long-term bonds as those with a time-to-maturity of at least 10 years and short-term bonds as those with a time-to-maturity of less than 10 years. The results show that the increase in bond yields is concentrated in long-term bonds, whereas short-term bonds are affected very little. These results suggest that investors perceive restrictions on reproductive healthcare as a long-run fundamental risk that impacts cash flows of local governments in the long term rather than in the immediate term.

To the extent that the increase in bond yields reflects bonds' deteriorating fundamentals

[14]We define female-dominated industries as NAICS 3-digit industries with female employee ratios above 50%.

in trigger law states, the increase should be stronger among bonds with poorer credit ratings. This is because lower rated jurisdictions generally have less diversified economies, lower incomes, and smaller fiscal capacity, making them more vulnerable to business and residential relocation and shrinking tax bases. In columns (3) and (4), we re-estimate Table 2 column (2) separately for high rated bonds and low rated bonds; we define high rated bonds as those rated AA– or above in the pre-period and low rated bonds as those with ratings below AA–. As expected, the effect is concentrated in bonds rated below AA–.

3.4 Interpreting the economic magnitude

To interpret the economic significance of our documented effects, we use the structural model in Goldsmith-Pinkham et al. (2021) to translate the increase in municipal bond yields to changes in the distribution of government cash flows. Goldsmith-Pinkham et al. (2021) adapt Merton (1974) distance-to-default-style model to the municipal bond setting, where the municipal credit risk depends on the present value of future cash flows (e.g., tax revenues) and the uncertainty of future cash flows (i.e., asset volatility). We calibrate the model to match the secondary market yield-to-maturity (3.17%) in our sample and use a tax-exempt risk-free rate of 1.95%. Following Goldsmith-Pinkham et al. (2021), we use an average maturity of 10 years and a duration of 7.5 years. Because we do not directly observe the leverage ratio (K/V) for bond issuers, we estimate the model using a set of leverage ratios ranging from 0.1 to 0.7.

Figure A2 presents the corresponding changes in the level and volatility of municipal cash flows implied by the 7.0 bp increase in municipal bond yields from column (2) of Table 2. The results show: a decrease of 2.3% to 5.3% in the present value of cash flows assuming a zero change in volatility, as given by the intercepts between a horizontal line at volatility=0 with the purple solid line (K/V=0.1) and the orange dotted line (K/V=0.7); an increase of 1.4% to 2.4% in the volatility of cash flows assuming a zero change in cash flow levels, as given by the intercepts between a vertical line at present value=0 with the purple and the orange dotted lines; or some mixture of the two. This model-implied effect on local government cash flows suggests that investors anticipate restrictive reproductive care policies to have a material impact on local economies.

4. Exploring Channels

To understand why municipal bond yields increase, leading to the implied negative economic effects of restrictions on reproductive healthcare, we examine the impacts of these restrictions on two vital determinants of local economies and tax bases: firms and residents. In Section 4.1, we examine the impact of the Dobbs decision on firms using firm value as a summary statistic. In Section 4.2, we exploit staggered adoption of state laws that restrict abortion providers and study the impact of limiting reproductive healthcare on firms and job creation (Section 4.2.2) and residential migration (Section 4.2.3).

4.1 Changes in firm value around the overturning of Roe v. Wade

We expect firms in trigger law states to be negatively affected by the Dobbs decision for several reasons. First, firms may need to help employees cover the costs of traveling out-of-state to obtain reproductive healthcare services. Second, workers, especially high-skilled and mobile workers, may decide to leave states that restrict reproductive healthcare, leading to costs associated with hiring and training new personnel. Finally, firms may move out of states with restrictive reproductive healthcare policies in response to employees' demands or labor supply shortages, resulting in operational disruptions and relocation costs. All of these factors are likely to increase firms' operating costs and decrease future cash flows, leading to weaker bottom lines.

Ideally, we would examine firms' earnings and cost measures such as EBITDA, cost of goods sold (COGS), and selling, general, and administrative (SG&A) expenses. Because comprehensive firm data for 2023 are not yet available, we study changes in firm value after Roe v. Wade was overturned to measure economic impacts on firms. We obtain daily trading data for all 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, stocks actively traded as of May 2, 2022 (i.e., the day before the Supreme Court's draft decision was leaked), and firms headquartered in either trigger law states or states that protect access to abortion. Our final sample consists of 2,383 firms.

To estimate the effects on firm value, we compute daily cumulative abnormal returns

(CARs) adjusted for the Fama-French four-factor model following the Dobbs decision. We estimate each model for the 100 days prior to the day before the decision. Because firms could operate in both treatment and control states, we define an indicator, *HQ Trigger*, for firms headquartered in trigger law states to capture firms with relatively higher exposure to the change in the legal landscape of reproductive rights.

Table 6 reports the results, which provide evidence of lower abnormal returns for firms with greater exposure to the Dobbs decision. Specifically, as shown in the first three columns, firms headquartered in trigger law states experience negative abnormal returns of about 102 bps for the (0, 10) window relative to firms headquartered in control states, with economically smaller and statistically insignificant effects for the (0,+2) and (0,+5) windows. In the next three columns, we utilize location data for abortion clinics and compare the effects for treated firms headquartered in counties with an above-median (*High change*) vs. below-median (*Low change*) change in distance to the closest abortion clinic after the Dobbs decision. The negative effects are much more pronounced for firms headquartered in counties where the distance to the closest abortion clinic has increased more: -133 bp and -170 bp for the (0,+5) and (0,+10) windows. In contrast, effects are negligible for low change firms. This heterogeneity aligns well with our finding in Section 3.3, suggesting that the reduction in firm value is indeed driven by diminished access to reproductive healthcare. Lastly, in the final three columns, we compare effects for treated firms operating in a single state (*Single-state*) vs. those in multiple states (*Multi-state*). Since single-state firms lack geographic diversification and cannot easily shift production across states, they should be more susceptible to the increased costs associated with restrictive reproductive healthcare policies. Our findings show this is indeed the case: single-state firms headquartered in trigger law states see abnormal returns ranging from -125 to -240 bps for the (0,+2) and (0,+10) windows, while multi-state firms headquartered in trigger law states see no significant changes.

4.2 Evidence from TRAP laws

To further investigate mechanisms underlying the increase in municipal bond yields following restrictions on reproductive healthcare, we turn to the staggered adoption of Targeted Regulation of Abortion Providers (TRAP) laws across states.

TRAP laws refer to laws that limit abortion access by imposing excessive requirements on abortion providers. Although TRAP laws cannot fully ban abortion given the constitutional protections provided in the Roe (later Casey) decision, they have been shown to reduce abortion (Arnold, 2022, Jones and Pineda-Torres, 2021, Zandberg, 2021). To the extent that TRAP laws limit women's ability to access reproductive healthcare, studying these laws can shed light on the channels whereby restrictions on reproductive healthcare affect public financing and local economies. Moreover, TRAP laws constitute multiple events across different states over various points in time, which mitigates potential biases and noise associated with relying on a single policy shock.

We use data on TRAP laws collected by Austin and Harper (2019). The data cover the year of first enactment and other details for various TRAP laws based on sources updated through mid-2016. We focus on the 16 states that enacted their first set of TRAP laws between 2003 and 2016. 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.

4.2.1 Effects on municipal bond yields

Similar to the overturning of Roe v. Wade, TRAP laws are expected to have negative impacts on local economies and increase municipal bond yields. However, the magnitude of these impacts is likely to be smaller, as TRAP laws do not completely ban abortion.

Following Sun and Abraham (2020) we use a DID dynamic event study to examine the effects of TRAP laws on bond yields. Specifically, we compare the yields in treatment states against those in control states that never enacted TRAP laws before mid-2016. We focus on the *first* enactment of TRAP laws in a state to prevent our estimates from being contaminated by the effects of prior enactments following Zandberg (2021).[15] To implement the dynamic event study, we stack our panel data as a series of 2$\times$2 matrices (bonds in treatment/control states $\times$ omitted period/event period) and adapt the R package from Novgorodsky and Setzler (2019).

[15]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 reproductive care. To the extent that the effect is weaker in these states, our estimate serves as a lower bound.

We define states that enact their first 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 monthly bond yields for bond i in year y and month m:

$$\begin{aligned} Y_{iym} = & \sum_{g \in G} 1\{cohort = g\} * \{\sum_{e \neq -1} \beta_{e_g} * Treat_{s(i),g} * 1\{e = y - g\} \\ & + \sum_{e \neq -1} \gamma'_{e_g} X_{ym} * 1\{e = y - g\} + \alpha_{e_g,ym} + \alpha_{g,i}\} + \epsilon_{iym}, \end{aligned} \tag{3}$$

While the data are monthly, for precision and ease of presentation we estimate effects by event year. G is the set of years between 2003 and 2016 when states enact their first TRAP laws. To avoid extending the sample period into the pandemic year of 2020 and to estimate effects across all treated states, we only estimate event year effects between -3 and +3 years. We define the omitted period as year -1. $Treat_{s(i),g}$ takes a value of 1 if bond i is issued by state s belonging to cohort g, and cohort g is the treatment cohort (i.e., $1\{cohort = g\} = 1$). X_{ym} denotes time-varying bond characteristics, $\alpha_{e_g,ym}$ denotes cohort event year-specific month fixed effects, and $\alpha_{g,i}$ denotes cohort-specific bond fixed effects.

Assuming that yields in states with TRAP laws and states that have never enacted them would have shared similar trends absent the laws, we can identify the treatment effect on bond yields in treated cohort g in event year e_g, labeled as β_{e_g}. Following Sun and Abraham (2020), we define the average treatment effect for event year e as:

$$\beta_e = \sum_{g \in G} \beta_{e_g} \times w_g \tag{4}$$

where w_g (the aggregation weight) is the number of observations used to estimate β_{e_g}. We calculate standard errors double clustered by state and year-month for β_e via the delta method.

Figure 3 plots the β_e coefficients. Consistent with findings in Section 3, municipal bond yields increase in response to TRAP law enactments. We observe no discernible difference in yields between treatment and control states leading up to the TRAP law enactments, indicating that the parallel pre-trends assumption likely holds in our case. However, yields in treatment states increases by 3 bps relative to control states in the year of enactment (event 0), and this difference grows to 7 bps by year two before slightly attenuating in year three.[16] Aggregating across years 1 through 3, the effect implies a $22.5 million increase in cost of financing per treatment state. Regression coefficients are reported in Table 7.

4.2.2 Effects on local firms

Having documented effects of TRAP laws on bond yields that are consistent with the findings in Section 3, we next study their impacts on local businesses, an important determinant of state tax bases. Similar to the Dobbs decision, TRAP laws could hurt firms' bottom lines by increasing financial burdens related to employee reproductive healthcare, employee turnover, and firm relocation. All of these could diminish the dynamism of local economies, thereby decreasing government cash flows, and eventually contributing to an increase in municipal bond yields.

To study this channel, we use the Census Bureau's Business Dynamics Statistics (BDS), which enables us to examine state-level firm entry/exit and job creation/destruction each year. Using a similar DID dynamic event study as in the last section, we run the following model:

$$Y_{sy} = \sum_{g \in G} 1\{cohort = g\} * \{\sum_{e \neq -1} \beta_{e_g} * Treat_{s,g} * 1\{e = y - g\} + \alpha_{e_g} + \alpha_{g,s}\} + \epsilon_{sy}, \tag{5}$$

where Y_{sy} is our outcome of interest—firms per 100,000 residents or net job creation rate—for state s in year y. α_{e_g} denotes cohort event year fixed effects and $\alpha_{g,s}$ denotes cohort-specific state fixed effects. Standard errors are clustered by state. Everything else follows Equation 3, and we aggregate effects to the event year level following Equation 4.

Figure 4 plots β_e. The results show that states experience weaker business dynamism than control states after the enactment of TRAP laws. Specifically, panel (a) reveals a decline in the number firms per capita in the year when a state enacts TRAP laws, relative to control states. This gap continues to grow over the following three years. Summing across the three years after enactment, states that enact TRAP laws see a total reduction of 45

[16] 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.

firms per capita compared to control states. This result is consistent with Zandberg (2021), which shows a decrease in female entrepreneurship after the enactment of TRAP laws. The net job creation rate in panel (b) shows a consistent pattern. The net job creation rate in TRAP states decreases relative to control states starting in the year of enactment and continues to decrease over the next three years. On average, states experience a relative decrease of 0.44% in net job creation rate in the three years after they enact TRAP laws.

These findings indicate that firms are less likely to be founded or more likely to close down in states with TRAP laws, which decreases local employment and potentially affects the tax base and fiscal capacity of local governments. These results also correspond well to the findings in Section 4.1 that restrictions on reproductive healthcare decrease firm value.

4.2.3 Effects on migration

Another important determinant of municipal financing is population. Local residents not only contribute to government tax income, but also are major holders of municipal bonds (Bagley et al., 2022). If individuals who value reproductive rights are less attracted to states that enact TRAP laws, they may move out of those states or be less inclined to move to them. We thus expect states that enact TRAP laws to see a decline in net in-migration (i.e., inflow - outflow). A shrinking state population likely decreases the tax base, erodes future cash flows to municipalities, and dampens local demand for municipal bonds, all of which could lead to a rise in municipal bond yields.

To examine this channel, we use state-to-state migration statistics from the IRS's Statistics of Income (SOI) IRS (2023). The migration 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.[17] We estimate Equation 5 and plots the β_e coefficients in Figure 5.

Panel (a) shows that the net in-migration in treatment states begins to drop in the year of enactment relative to control states and continues to fall in the following three years. The

[17]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.

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.[18] Results when restricting the flows to be from or to control states show similar patterns (see Appendix Figure A3). We also separately plot the effects on migration inflows and outflows in panels (b) and (c), 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 state accounts for most of the negative effect on net in-migration.

Taken together, our results suggest that restrictions on reproductive healthcare—whether complete bans on abortion or excessive requirements imposed on abortion providers—negatively impact local businesses and reduce net in-migration, potentially shrinking state tax bases and contributing to higher public financing costs.

5. Conclusions

Rights to reproductive healthcare have become an increasingly contentious issue in the U.S. Debates about this issue often center around legality and morality, but focus much less on economic implications. To fill this gap, we examine the public financing costs of restricting access to reproductive healthcare on state and local governments and quantify the real economic impacts implied by the change in costs. We find that municipal bond yields increase in states that restrict or ban abortion relative to states that protect access to abortion. Moreover, we uncover deteriorated firm value, weakened business dynamism, and declined net in-migration as potential channels underlying the effects.

Given the important role of public financing in supporting government operations and public projects, reproductive healthcare 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

[18] 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).

ramifications of reproductive healthcare policies in addition to the legal and moral implications.

References

Ananat, E. O., Gruber, J., and Levine, P. (2007). Abortion legalization and life-cycle fertility. *Journal of Human Resources*, 42(2):375–397.

Ananat, E. O., Gruber, J., Levine, P. B., and Staiger, D. (2009). Abortion and selection. *The Review of Economics and Statistics*, 91(1):124–136.

Ang, A., Bhansali, V., and Xing, Y. (2010). Taxes on tax-exempt bonds. *The Journal of Finance*, 65(2):565–601.

Ang, A., Bhansali, V., and Xing, Y. (2014). The muni bond spread: Credit, liquidity, and tax. *Columbia Business School Research Paper*, (14-37).

Angrist, J. and Evans, W. N. (1996). Schooling and labor market consequences of the 1970 state abortion reforms. *NBER Working Paper*.

ARDA (2000). Religious congregations and membership study, 2010 (county file). The Association of Religion Data Archives. https://thearda.com/data-archive?fid=RCMSCY10 (accessed April 28, 2021).

Arnold, G. (2022). The impact of targeted regulation of abortion providers laws on abortions and births. *Journal of Population Economics*, 35(4):1443–1472.

Austin, N. and Harper, S. (2019). Constructing a longitudinal database of targeted regulation of abortion providers laws. *Health Services Research*, 54(5):1084–1089.

Babina, T., Jotikasthira, C., Lundblad, C., and Ramadorai, T. (2021). Heterogeneous taxes and limited risk sharing: Evidence from municipal bonds. *The Review of Financial Studies*, 34(1):509–568.

Bagley, J., Vieria, M., and Hamlin, T. (2022). Trends in municipal securities ownership. Technical report, Municipal Securities Rulemaking Board.

Bailey, M. J. (2006). More power to the pill: The impact of contraceptive freedom on women's life cycle labor supply. *The Quarterly Journal of Economics*, 121(1):289–320.

Bailey, M. J. (2010). "Momma's got the pill": How anthony comstock and griswold v. connecticut shaped US childbearing. *American Economic Review*, 100(1):98–129.

Bennett, B., Erel, I., Stern, L. H., and Wang, Z. (2020). Paid leave pays off: The effects of paid family leave on firm performance. Technical report, National Bureau of Economic Research.

Bitler, M. and Zavodny, M. (2001). The effect of abortion restrictions on the timing of abortions. *Journal of Health Economics*, 20(6):1011–1032.

Bloom, D. E., Canning, D., Fink, G., and Finlay, J. E. (2009). Fertility, female labor force participation, and the demographic dividend. *Journal of Economic growth*, 14:79–101.

Bloomberg (2022). Nearly half of working adults say they're open to relocating to abortion-friendly states. https://www.bloomberg.com/news/articles/2022-07-19/reality-bites-for-workers-looking-to-abortion-friendly-states#xj4y7vzkg (accessed May 1, 2023).

BLS (2021). Quarterly census of employment and wages. QCEW Data Files. https://www.eeoc.gov/data/job-patterns-minorities-and-women-private-industry-eeo-1-0#Explore (accessed August 18, 2023).

Butler, A. W., Fauver, L., and Mortal, S. (2009). Corruption, political connections, and municipal finance. *The Review of Financial Studies*, 22(7):2873–2905.

Butler, A. W. and Yi, H. (2022). Aging and public financing costs: Evidence from US municipal bond markets. *Journal of Public Economics*, 211:104665.

CBS (2022a). Google tells U.S. workers they can move to states where abortion is legal: report. https://www.cbsnews.com/news/abortion-google-workers-can-move-to-states-where-abortion-is-legal-roe-v-wade/ (accessed May 1, 2023).

CBS (2022b). These companies are paying for abortion travel. https://www.cbsnews.com/news/abortion-travel-companies-paying-benefits-amazon-starbucks-target/ (accessed July 25, 2023).

Cheng, S. F., De Franco, G., and Lin, P. (2023). Marijuana liberalization and public finance: A capital market perspective on the passage of medical use laws. *Journal of Accounting and Economics*, 75(1):101516.

CNBC (2022). Large Indiana employers Eli Lilly and Cummins speak out about the state's new restrictive abortion law. https://www.cnbc.com/2022/08/06/eli-lilly-says-indianas-abortion-law-will-lead-the-drugmaker-to-grow-in-other-states.html (accessed May 1, 2023).

Cornaggia, K., Hund, J., Nguyen, G., and Ye, Z. (2022a). Opioid crisis effects on municipal finance. *The Review of Financial Studies*, 35(4):2019–2066.

Cornaggia, K. R., Li, X., and Ye, Z. (2022b). Virtual competition and cost of capital: Evidence from telehealth. *SSRN Working Paper*.

Cuny, C. (2018). When knowledge is power: Evidence from the municipal bond market. *Journal of Accounting and Economics*, 65(1):109–128.

DOL (2021). Labor force participation rate by sex, state and county. Data and Statistics. https://www.dol.gov/agencies/wb/data/lfp-rate-sex-state-county (accessed August 18, 2023).

EEOC (2021). Job patterns for minorities and women in private industry (eeo-1). Data and Analytics. https://www.eeoc.gov/data/job-patterns-minorities-and-women-private-industry-eeo-1-0#Explore (accessed August 18, 2023).

Farrell, M., Murphy, D., Painter, M., and Zhang, G. (2023). The complexity yield puzzle: A textual analysis of municipal bond disclosures. *SSRN Working Paper*.

Financial Times (2022). Eli Lilly says some staff want to leave indiana because of abortion ban. https://www.ft.com/content/07f36d02-b75c-4e20-aeb6-6a2109e8fc6e (accessed May 1, 2023).

Gao, P., Lee, C., and Murphy, D. (2019a). Municipal borrowing costs and state policies for distressed municipalities. *Journal of Financial Economics*, 132(2):404–426.

Gao, P., Lee, C., and Murphy, D. (2020). Financing dies in darkness? The impact of newspaper closures on public finance. *Journal of Financial Economics*, 135(2):445–467.

Gao, P., Lee, C., and Murphy, D. (2022). Good for your fiscal health? The effect of the affordable care act on healthcare borrowing costs. *Journal of Financial Economics*, 145(2):464–488.

Gao, P., Murphy, D., and Qi, Y. (2019b). Political uncertainty and public financing costs: Evidence from us gubernatorial elections and municipal bond markets. *SSRN Working Paper*.

Garrett, D. (2021). Conflicts of interest in municipal bond advising and underwriting. *SSRN Working Paper*.

Garrett, D. and Ivanov, I. (2022). Gas, guns, and governments: Financial costs of anti-ESG policies. *SSRN Working Paper*.

Garrett, D., Ordin, A., Roberts, J. W., and Serrato, J. C. S. (2017). Tax advantages and imperfect competition in auctions for municipal bonds. Technical report, National Bureau of Economic Research.

Goldin, C. and Katz, L. F. (2002). The power of the pill: Oral contraceptives and women's career and marriage decisions. *Journal of Political Economy*, 110(4):730–770.

Goldsmith-Pinkham, P., Gustafson, M. T., Lewis, R. C., and Schwert, M. (2022). Sea level rise exposure and municipal bond yields. Technical report, National Bureau of Economic Research.

Goldsmith-Pinkham, P. S., Gustafson, M., Lewis, R., and Schwert, M. (2021). Sea level rise exposure and municipal bond yields. *Review of Financial Studies, forthcoming*.

Gottlieb, J. D., Townsend, R. R., and Xu, T. (2022). Does career risk deter potential entrepreneurs? *The Review of Financial Studies*, 35(9):3973–4015.

Guldi, M. (2008). Fertility effects of abortion and birth control pill access for minors. *Demography*, 45:817–827.

Gustafson, M., Haslag, P. H., Weagley, D., and Ye, Z. (2023). A flash in the pan (demic)? migration risks and municipal bonds. *SSRN Working Paper*.

Guttmacher Institute (2021). Targeted regulation of abortion providers. https://www.guttmacher.org/state-policy/explore/targeted-regulation-abortion-providers (accessed May 27, 2023).

Guttmacher Institute (2022). 13 states have abortion trigger bans—here's what happens when Roe is overturned. https://www.guttmacher.org/article/2022/06/13-states-have-abortion-trigger-bans-heres-what-happens-when-roe-overturned (accessed May 9, 2023).

IRS (2023). U.S. population migration data. https://www.irs.gov/statistics/soi-tax-stats-migration-data (accessed April 14, 2023).

Jones, K. and Pineda-Torres, M. (2021). TRAP'd teens: Impacts of abortion provider regulations on fertility & education. *IZA Discussion Paper*.

Kaiser Family Foundation (2022a). 16 states and DC have state laws protecting the right to abortion if Roe v. Wade is overturned. https://www.kff.org/womens-health-policy/slide/16-states-and-dc-have-state-laws-protecting-the-right-to-abortion-if-roe-v-wade-is-overturned/ (accessed May 15, 2023).

Kaiser Family Foundation (2022b). Abortion at scotus: Dobbs v. jackson women's health. https://www.kff.org/womens-health-policy/issue-brief/abortion-at-scotus-dobbs-v-jackson-womens-health/ (accessed May 27, 2023).

Kaiser Family Foundation (2022c). Medicaid coverage of pregnancy-related services: Findings from a 2021 state survey. https://www.kff.org/report-section/medicaid-coverage-of-pregnancy-related-services-findings-from-a-2021-state-survey-report/ (accessed July 26, 2023).

Lin, Y., Shen, M., Shi, R., and Zeng, J. (2023). The falling Roe and relocation of skilled women: Evidence from a large sample of auditors. *SSRN Working Paper*.

Liu, T., Makridis, C. A., Ouimet, P., and Simintzi, E. (2023). The distribution of non-wage benefits: maternity benefits and gender diversity. *The Review of Financial Studies*, 36(1):194–234.

Longstaff, F. A. (2011). Municipal debt and marginal tax rates: Is there a tax premium in asset prices? *The Journal of Finance*, 66(3):721–751.

Merton, R. C. (1974). On the pricing of corporate debt: The risk structure of interest rates. *The Journal of finance*, 29(2):449–470.

Miller, S., Wherry, L. R., and Foster, D. G. (2023). The economic consequences of being denied an abortion. *American Economic Journal: Economic Policy*, 15(1):394–437.

Myers, C. (2023). Myers abortion facility database. https://osf.io/8dg7r/ (accessed March 10, 2023).

Myers, C. K. (2017). The power of abortion policy: Reexamining the effects of young women's access to reproductive control. *Journal of Political Economy*, 125(6):2178–2224.

Myers, C. K. (2021). Measuring the burden: The effect of travel distance on abortions and births. *IZA Discussion Paper*.

Novgorodsky, D. and Setzler, B. (2019). Practical guide to event studies. *Github*.

Painter, M. (2020). An inconvenient cost: The effects of climate change on municipal bonds. *Journal of Financial Economics*, 135(2):468–482.

Politico (2022). Supreme Court has voted to overturn abortion rights, draft opinion shows. https://www.politico.com/news/2022/05/02/supreme-court-abortion-draft-opinion-00029473 (accessed May 9, 2023).

Pop-Eleches, C. (2006). The impact of an abortion ban on socioeconomic outcomes of children: Evidence from romania. *Journal of Political Economy*, 114(4):744–773.

Ravid, I. and Zandberg, J. (2022). The future of Roe and the gender pay gap: An empirical assessment. *SSRN Working Paper*.

Responsible Investor (2022). ESG rating agencies dodge abortion rights issue following US Supreme Court leak. https://www.responsible-investor.com/esg-rating-agencies-dodge-abortion-rights-issue-following-us-supreme-court-leak/ (accessed May 30, 2023).

Schultz, P. (2012). The market for new issues of municipal bonds: The roles of transparency and limited access to retail investors. *Journal of Financial Economics*, 106(3):492–512.

Schwert, M. (2017). Municipal bond liquidity and default risk. *The Journal of Finance*, 72(4):1683–1722.

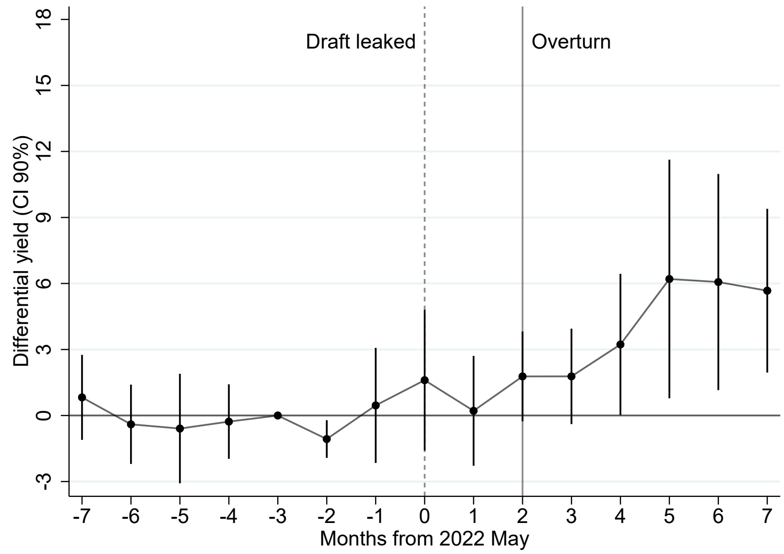
Simintzi, E., Xu, S.-J., and Xu, T. (2023). The effect of childcare access on women's careers and firm performance. *Kenan Institute of Private Enterprise Research Paper*, (4162092).

Stevenson, A. J., Root, L., and Menken, J. (2022). The maternal mortality consequences of losing abortion access. *SocArXiv Working Paper*.

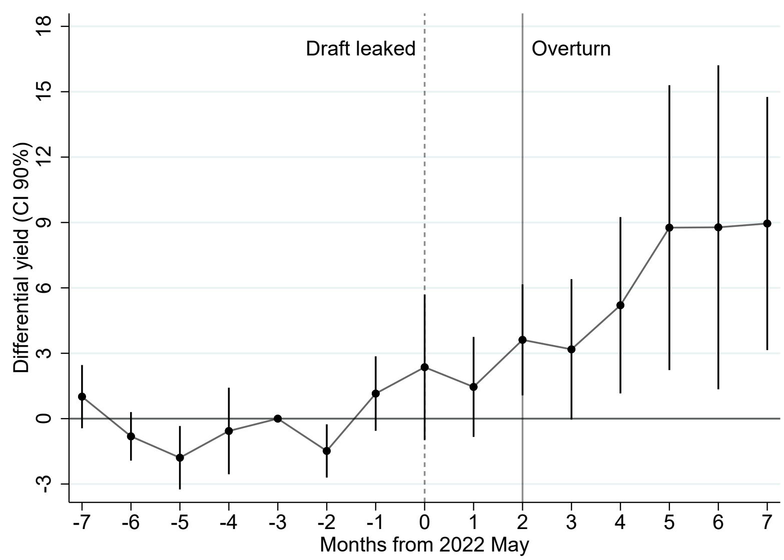
Sun, L. and Abraham, S. (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*.

Washington Post (2022). After state abortion fights, corporate america braces for end of Roe. https://www.washingtonpost.com/business/2022/05/04/companies-abortion-decision/ (accessed May 1, 2023).

Zandberg, J. (2021). Family comes first: Reproductive health and the gender gap in entrepreneurship. *Journal of Financial Economics*, 140(3):838–864.



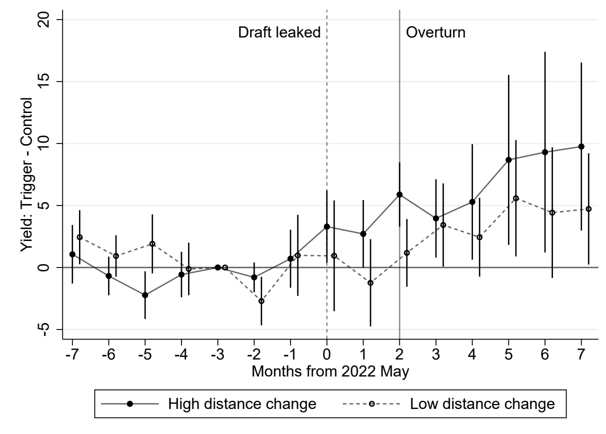
(a) All bonds



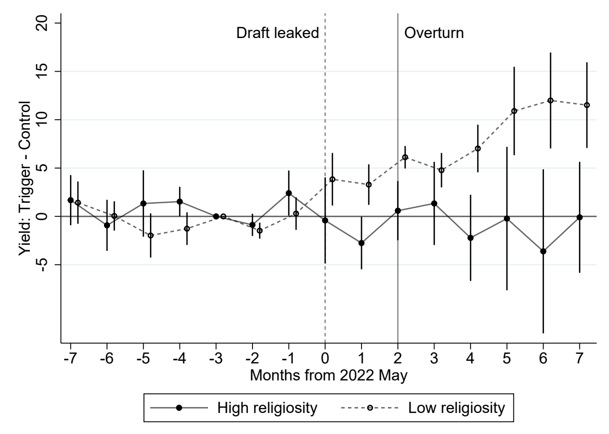
(b) GO bonds

Figure 1: Effect of Overturning Roe v. Wade on Municipal Bond Yields

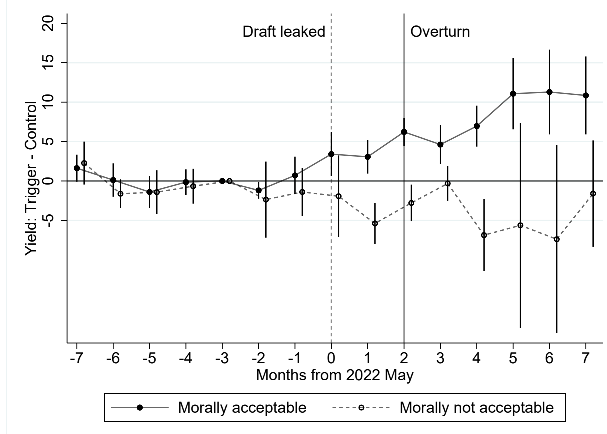
Note: This figure plots effects (and 90% confidence intervals) for secondary market municipal bond yields in states with trigger laws relative to states with laws protecting access to abortion around the decision to overturn Roe v. Wade. The outcome is the size-weighted average yield at the bond-month level; units are bps. The dashed vertical line (and event time 0) denotes May 2022, the month when Justice Samuel Alito's draft majority opinion in favor of overturning Roe v. Wade was leaked. The solid vertical line denotes July 2022, the month immediately after Roe v. Wade was overturned on June 24, 2022. Panels (a) and (b) plot the interactions between month dummies and the trigger law state indicator from Equation 1, estimated using all bonds and general obligation (GO) bonds issued by state, county, and city governments, respectively. The omitted month is -3 (February 2022). All regressions control for CUSIP fixed effects, year-month fixed effects, and time-varying bond characteristics (i.e., bond rating at the time of transaction and log time-to-maturity). Standard errors are double clustered by state and year-month. Coefficients are reported in Appendix Table A1.



(a) By change in distance to closest abortion clinic



(b) By local religiosity



(c) By attitude towards abortion

Figure 2: Effect of Overturning Roe v. Wade on Municipal Bond Yields by *Treatment Intensity*

Note: This figure plots effects (and 90% confidence intervals) of overturning Roe v. Wade on secondary market municipal bond yields by treatment intensity. The outcome is the size-weighted average yield at the bond-month level; units are bps. The sample consists of general obligation bonds issued directly by county and city governments. We exclude bonds issued by state governments in order to assign county-level characteristics. The dashed vertical line (and event time 0) denotes May 2022, the month when Justice Samuel Alito's draft majority opinion in favor of overturning Roe v. Wade was leaked. The solid vertical line denotes July 2022, the month immediately after Roe v. Wade was overturned on June 24, 2022. We multiply the interactions between month dummies and the trigger law state indicator from Equation 1with county-level indicators for: above- vs. below-median change in distance to the nearest abortion clinic within each state after the Dobbs decision (panel a); above- vs. below-median share of religious residents in 2010 (panel b); and above- vs. below-median share of respondents who view abortions as morally acceptable in the Gallup Poll Social Series (GPSS) survey between 2013 and 2021 (panel c). The triple interactions are plotted in the corresponding panel. The omitted month is -3 (February 2022). All regressions control for CUSIP fixed effects, year-month fixed effects, and time-varying bond characteristics (i.e., bond rating at the time of transaction and log time-to-maturity). Standard errors are double clustered by state and year-month.

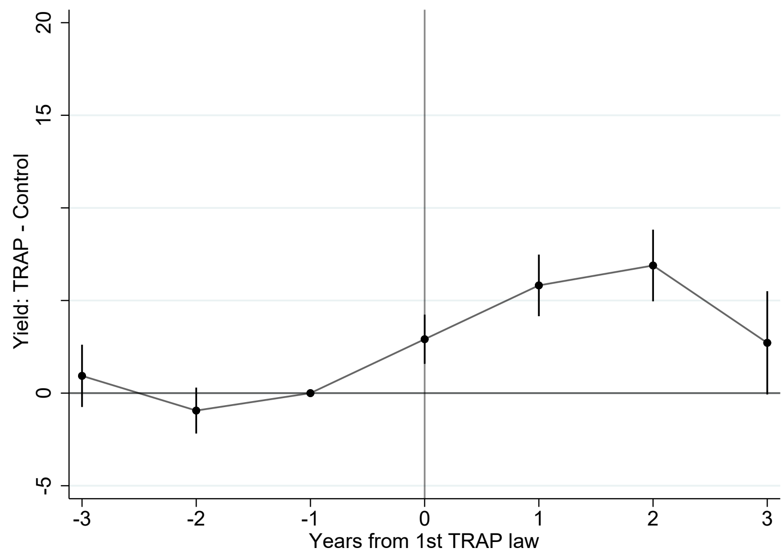
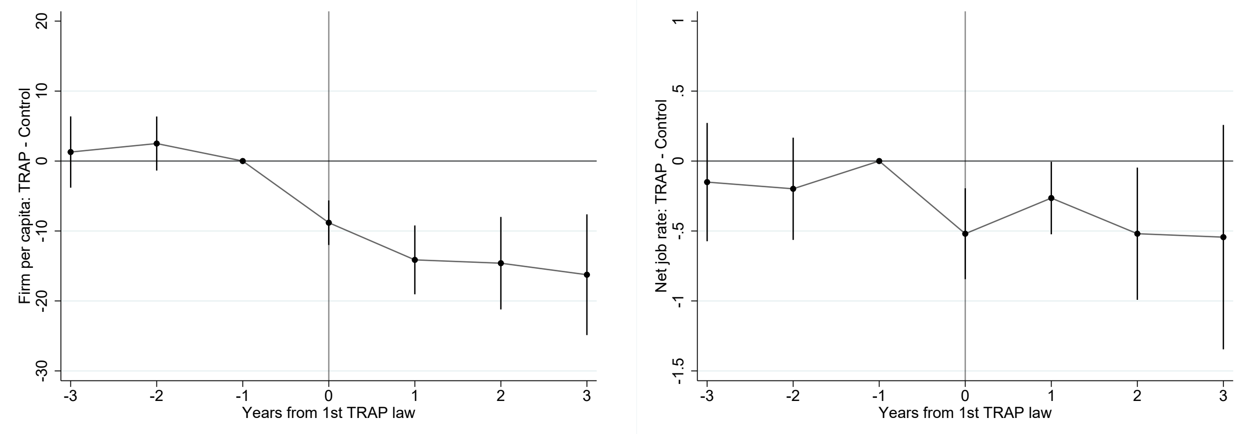


Figure 3: Dynamic DID Effect of TRAP Laws on Municipal Bond Yields

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. The sample consists of general obligation bonds issued directly by state, county, and city governments. 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 2003 and 2016 are considered. The omitted period is -1, i.e., the year before enactment. Standard errors are double clustered by state and year-month. Coefficients are reported in Appendix Table 7.

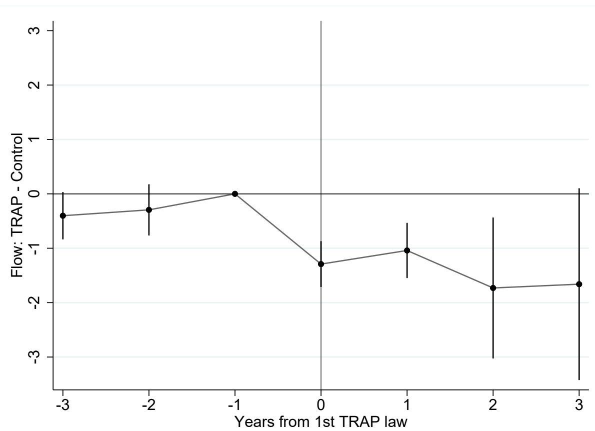


(a) Firms per 100,000 residents

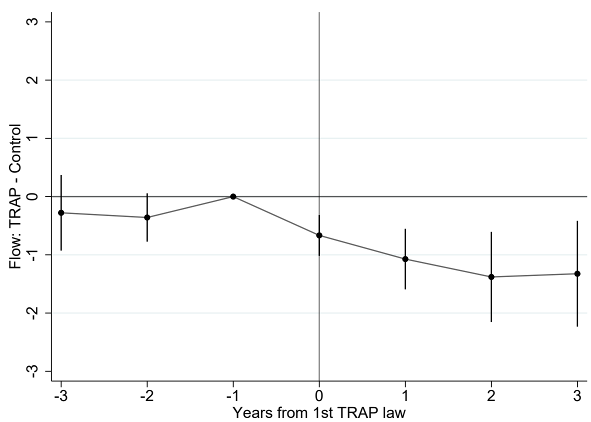
(b) Net job creation rate

Figure 4: Dynamic DID Effect of TRAP Laws on State-level Business Dynamics

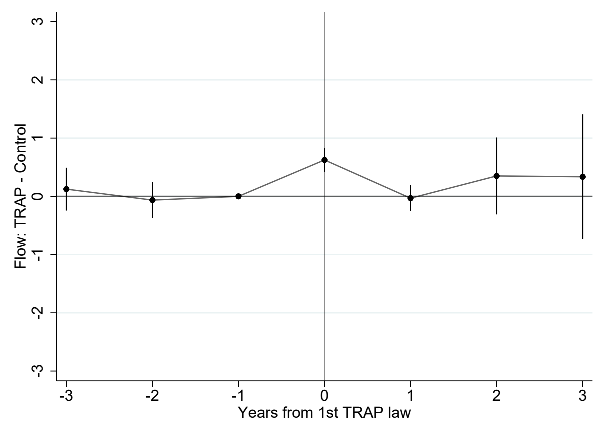
Note: This figure plots dynamic treatment effects (and 90% confidence intervals) of TRAP laws on business dynamics in states that ever enacted TRAP laws relative to states that never. Panel (a) plots the effect on firms per 100,000 state residents, i.e., the number of firms divided by state population 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 2003 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 Appendix Table 7.



(a) Net inflow per 1,000 residents



(b) Inflow per 1,000 residents



(c) Outflow per 1,000 residents

Figure 5: Dynamic DID 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 flows to a state, total inflows to a state, and total outflows from a state, respectively; all flow measures are divided by state population and multiplied by 1000. 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 2003 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 7.

Table 1: Summary Statistics

Panel A: Full Sample

Variable	Mean	Std	P25	Median	P75
Yield (bp)	317.20	113.65	248.28	330.17	401.11
Spread (bp)	149.90	88.21	79.45	153.25	210.54
Rating	18.30	1.94	17	19	20
Bond size (log)	15.40	1.58	14.27	15.38	16.58
Maturity (year)	8.38	5.95	3.75	6.92	11.58
Callable	0.71	0.45	0	1	1
Insured	0.19	0.39	0	0	0
General Obligation	0.46	0.50	0	0	1
Negotiated	0.45	0.50	0	0	1
N obs	323,838				

Panel B: Pre-shock period: Trigger-law vs. control states

	Trigger-law states	Control states	Difference
Yield (bp)	258.53	262.90	-4.37
Spread (bp)	149.67	155.60	-5.93
Rating	18.34	18.27	0.07
Bond Size (log)	14.93	15.64	-0.71***
Maturity (year)	8.65	8.19	0.46*
Callable	0.71	0.69	0.012
Insured	0.25	0.13	0.12*
General Obligation	0.44	0.46	-0.02
Negotiated	0.38	0.47	-0.09
N obs	44,136	108,733	

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 the remaining time-to-maturity at the time of a trade. *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 all bonds issued directly by state, county, and city governments, and months from October 2021 through December 2022. In panel B, we compare characteristics between bonds in states with trigger laws and those in states with laws protecting abortion, and months from October 2021 through April 2022, i.e., before the Supreme Court draft opinion was leaked and Roe v. Wade was overturned.

Table 2: Effect of Overturning Roe v. Wade on Municipal Bond Yields

	Dependent variable: Yield (bp)			
	(1)	(2)	(3)	(4)
Trigger × Post	10.61**	6.97*	7.86**	9.25**
	(4.04)	(3.23)	(2.97)	(3.29)
Effect as % mean	3.59	2.36	2.66	3.13
Outcome mean	295.29	295.29	295.29	295.29
Observations	128,803	128,803	128,803	128,803
R^2	0.79	0.96	0.96	0.96
Year-month FE	Y	Y	N	N
State FE	Y	N	N	N
CUSIP FE	N	Y	Y	Y
Rep. governor × Year-month FE	N	N	Y	Y
Economic controls	N	N	N	Y
Bond characteristics	Y	Y	Y	Y

Note: This table reports effects on secondary market municipal bond yields in states with trigger laws relative to states with laws protecting abortion. The outcome is the size-weighted average yield at the bond-month level; units are bps. Results using bond spread as the outcome are reported in Appendix Table A2. The sample consists of general obligation bonds issued directly by state, county, and city governments and transactions from October 2021 through December 2022 (excluding May and June 2022). *Post* is an indicator for months after Roe v. Wade was overturned. *Trigger* is an indicator for states that had trigger laws in place before Roe was overturned; the omitted category consists of states with laws protecting abortion. Column (1) controls for state fixed effects and year-month fixed effects; column (2) replaces state fixed effects with CUSIP fixed effects; column (3) replaces year-month fixed effects with Republican governor × year-month fixed effects; and column (4) additionally controls for state monthly unemployment rate and state quarterly GDP. 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 double clustered by state and year-month.
*** 1%, ** 5%, * 10% significance level

Table 3: Effect of Overturning Roe v. Wade on Municipal Bond Yields by *Treatment Intensity*

	Dependent variable: Yield (bp)				
	By degree of shock	By local attitude		By female presence	
	Change in distance (1)	Non-religiosity (2)	Morally acceptable (3)	Female LFP (4)	Female industry (5)
Trigger × Post × High	7.45**	9.30***	8.63***	6.52*	6.55**
	(3.36)	(2.02)	(2.07)	(3.03)	(2.85)
Trigger × Post × Low	3.55*	-1.50	-3.59	3.46	0.15
	(1.97)	(2.85)	(4.31)	(4.04)	(5.07)
Observations	98,050	98,050	93,106	98,050	98,050
R-squared	0.96	0.96	0.96	0.96	0.96
Outcome mean	292.77	292.77	293.2	292.77	292.77
Year-month FE	Y	Y	Y	Y	Y
CUSIP FE	Y	Y	Y	Y	Y
Bond characteristics	Y	Y	Y	Y	Y

Note: This table reports effects on secondary-market municipal bond yields by treatment intensity of the overturn of Roe v. Wade. The outcome is the size-weighted average yield at the bond-month level; units are in bp. Sample consists of general obligation bonds issued directly by county and city governments and transactions from October 2021 through December 2022 (excluding May and June 2022). We exclude bonds issued by state governments in order to assign county-level characteristics. Column (1) reports the effects for treated counties with an above- vs. below-median change in distance to the nearest abortion clinic after the overturn within each state. Columns (2)-(3) report the effects for treated counties with an above- vs. below-median share of religious population in 2010 and share of respondents that view abortions as morally acceptable in the GPSS survey between 2013 and 2021, respectively. Columns (4)-(5) report the effects for treated counties with an above- vs. below-median female labor force participation rate and employment share in female-dominated industries in 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 those in Table 2 column (2). Standard errors are double clustered by state and year-month.
*** 1%, ** 5%, * 10% significance level

Table 4: Effect of Overturning Roe v. Wade on Municipal Bond Yields
by *Maturity and Credit Rating*

	Dependent variable: Yield (bp)			
	Short-term	Long-term	<AA–	≥AA–
	(1)	(2)	(3)	(4)
Trigger × Post	1.93	17.14**	18.83***	4.78
	(1.95)	(6.39)	(4.38)	(2.75)
Observations	93,029	35,774	25,331	88,599
R^2	0.96	0.91	0.94	0.96
Outcome mean	266.46	370.26	311.87	278.34
Year-month FE	Y	Y	Y	Y
CUSIP FE	Y	Y	Y	Y
Bond characteristics	Y	Y	Y	Y

Note: This table reports heterogeneous effects on secondary-market municipal bond yields by maturity and credit rating. The outcome is the size-weighted average yield at the bond-month level; units are in bp. Sample consists of general obligation bonds issued directly by state, county, and city governments and transactions from October 2021 through December 2022 (excluding May and June 2022). Columns (1)-(2) examine the effects for short-term bonds (time-to-maturity<10 years) and long-term bonds (time-to-maturity≥10 years), respectively; columns (3)-(4) examine the effects for low rated bonds (lowest pre-shock rating<AA–) and high rated bonds (lowest pre-shock rating≥AA–), respectively. Everything else follows those in Table 2 column (2). Standard errors are double clustered by state and year-month.
*** 1%, ** 5%, * 10% significance level

Table 5: Effect of Overturning Roe v. Wade on Municipal Bond *Primary Offering Yields*

	Dep. var.: Offering yield (bp)		
	(1)	(2)	(3)
Trigger × Post	23.307**	20.136**	20.465***
	(9.523)	(7.722)	(5.692)
Effect as % mean	9.22	7.97	8.101
Outcome mean	252.62	252.62	252.62
Observations	11,865	11,863	11,863
R^2	0.93	0.96	0.96
Year-month FE	Y	Y	N
State FE	Y	N	N
Issuer FE	N	Y	Y
Rep. governor× Year-month FE	N	N	Y
Bond characteristics	Y	Y	Y

Note: This table reports effects on primary-market offering yields in states with trigger laws relative to states with laws protecting abortion. The outcome is the offering yield at the time of issuance; units are in bp. Sample consists of general obligation bonds issued directly by state, county, and city governments between October 2021 and December 2022 (excluding May and June 2022). Column (1) controls for state fixed effects and year-month fixed effects; column (2) replaces state fixed effects with issuer (six-digit CUSIP) fixed effects; column (3) replaces year-month fixed effects with Republican governor × year-month fixed effects. *Post* and *Trigger* follow the definitions in Table 2. 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 double clustered by state and year-month.

*** 1%, ** 5%, * 10% significance level

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

	By HQ location			By HQ distance to clinic			By single vs. multi-state firms		
	(0,+2)	(0,+5)	(0,+10)	(0,+2)	(0,+5)	(0,+10)	(0,+2)	(0,+5)	(0,+10)
HQ Trigger	-9.27	-42.02	-102.15*						
	(32.32)	(42.04)	(52.72)						
HQ Trigger × High change				-38.56	-133.26**	-170.33**			
				(35.11)	(59.66)	(78.14)			
HQ Trigger × Low change				-3.77	0.55	-59.46			
				(44.35)	(53.52)	(65.21)			
HQ Trigger × Single-state							-125.44**	-192.09**	-239.52**
							(51.68)	(75.23)	(98.95)
HQ Trigger × Multi-state							21.40	6.61	-47.78
							(39.49)	(49.03)	(59.79)
Intercept	14.10	-33.82*	-28.39	17.36	-35.18*	-32.97	17.38	-34.91*	-32.69
	(12.95)	(19.13)	(26.74)	(13.19)	(19.84)	(26.00)	(13.20)	(19.86)	(26.03)
Observations	2,383	2,383	2,383	2,205	2,205	2,205	2,205	2,205	2,205
R^2	0.00	0.00	0.00	0.00	0.00	0.00	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 June 24, 2022; units are in bp. *HQ Trigger* is an indicator for firms headquartered in states that have trigger laws in place before the overturn; the omitted category consists firms in states with laws protecting abortion. Columns (4)-(6) explore heterogeneous effects for firms whose headquarter county see an above- (*High change*) vs. below-median change (*Low change*) in distance within each state to the nearest abortion clinic after the overturn. Columns (7)-(9) examine heterogeneous effects for firms operate in only one states (*Single-state*) vs. in multiple states (*Multiple-state*). Robust standard errors are reported in parentheses.
*** 1%, ** 5%, * 10% significance level

Table 7: Dynamic DID Effect of TRAP Laws on Municipal Bond, Cross-State Migration, and State Business Dynamics

	Municipal bond	Cross-state migration			State business dynamics	
	(1) Yield (bp)	(2) Net flow (in–out)	(3) Inflow	(4) Outflow	(5) Firms per 100k	(6) Net job creation rate (%)
TRAP × -3Y	0.932	-0.402	-0.278	0.124	1.287	-0.151
	(1.022)	(0.264)	(0.395)	(0.224)	(3.095)	(0.257)
TRAP × -2Y	-0.942	-0.295	-0.359	-0.064	2.502	-0.198
	(0.754)	(0.286)	(0.252)	(0.190)	(2.348)	(0.222)
TRAP × 0Y	2.910	-1.291	-0.666	0.625	-8.814	-0.519
	(0.807)***	(0.256)***	(0.213)***	(0.123)***	(1.938)***	(0.197)***
TRAP × 1Y	5.815	-1.041	-1.073	-0.032	-14.128	-0.265
	(1.011)***	(0.309)***	(0.316)***	(0.135)	(2.991)***	(0.158)*
TRAP × 2Y	6.890	-1.730	-1.380	0.350	-14.604	-0.519
	(1.176)***	(0.788)**	(0.471)***	(0.402)	(4.019)***	(0.287)*
TRAP × 3Y	2.715	-1.661	-1.325	0.336	-16.250	-0.544
	(1.695)	(1.072)	(0.552)**	(0.652)	(5.245)***	(0.487)
Avg. treat (1 to 3)	5.140	-1.477	-1.259	0.218	-14.994	-0.443
p value	0.000	0.033	0.004	0.522	0.000	0.145
R^2	0.927	0.737	0.953	0.980	0.998	0.774
Outcome mean	316.562	0.443	24.875	24.433	2,079.461	0.991

Note: This table presents dynamic treatment effects of TRAP laws on states that ever enacted TRAP laws relative to states that never. Outcomes are secondary-market yield (in bp), 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, state-level number of firms per 100,000 population, and state-level net job creation rate, in columns (1)-(6) 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 2003 and 2016 are considered (Austin and Harper, 2019). The omitted period is -1. Standard errors are clustered by state and year-month in column (1) and by state in columns (2)-(6). See Equation 3 in Section 4.2.1 for details about the specification. Coefficients are plotted in Figure 3, Figure 4, and Figure 5. *** 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

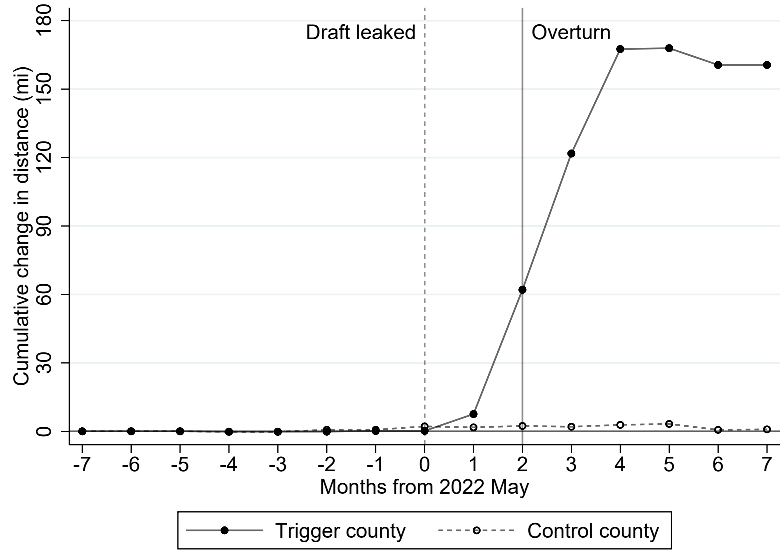


Figure A1: Change in Distance to the Closest Abortion Clinic

Note: This figure plots the cumulative change in the distance between county population centroids and the nearest abortion clinics from October 2021 to December 2022 for counties in states with trigger laws (*Trigger county*) vs. states with laws protecting abortion (*Control county*). The distance data are from Myers Abortion Facility Database (Myers, 2023).

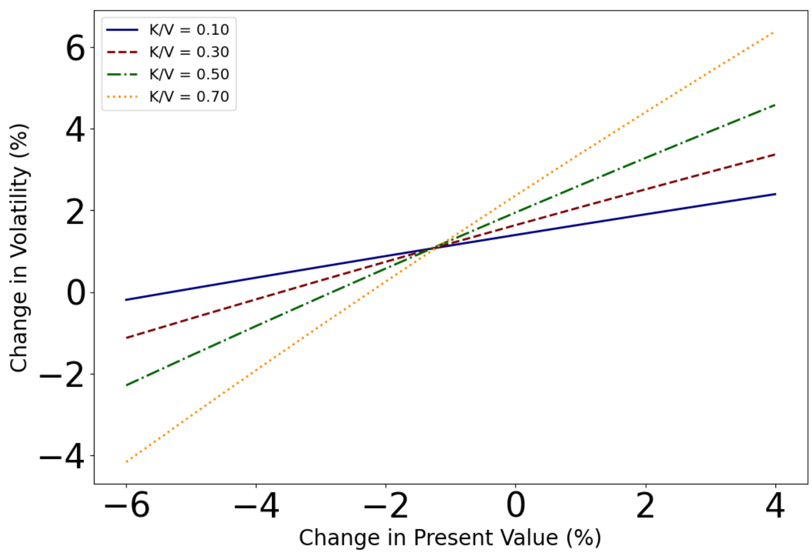
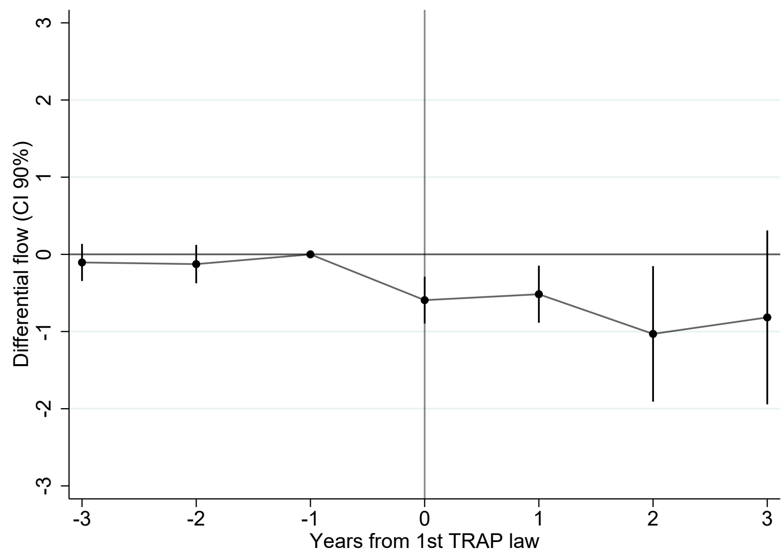


Figure A2: Changes in Present Value and Volatility of Cash Flows

Note: This figure plots the results from a structural estimation examining the changes in the present value and volatility of future cash flows implied by the estimated yield increase from column (2) of Table 2. Each line represents a scenario using a different leverage ratio (K/V).



(a) Net inflow per 1,000 residents

Figure A3: Dynamic DID 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 flows to be between TRAP states and control states or between control states themselves. Everything else follows Figure 5 panel (a).

Table A1: Effect of Overturning Roe v. Wade on Municipal Bond Yields
DID Event Study

	All bonds	GO bonds
Trigger × -7M	1.06	1.22
	(1.02)	(0.79)
Trigger × -6M	-0.02	-0.26
	(0.95)	(0.60)
Trigger × -5M	-0.55	-1.42
	(1.38)	(0.88)
Trigger × -4M	-0.30	-0.17
	(1.01)	(1.22)
Trigger × -2M	-0.81	-0.95
	(0.56)	(0.65)
Trigger × -1M	0.92	1.70
	(1.59)	(1.14)
Trigger × 0M	2.37	2.96
	(2.02)	(2.15)
Trigger × 1M	0.96	1.76
	(1.68)	(1.49)
Trigger × 2M	2.09	3.71**
	(1.31)	(1.54)
Trigger × 3M	2.25	3.40*
	(1.43)	(1.91)
Trigger × 4M	4.23*	5.80**
	(2.10)	(2.59)
Trigger × 5M	7.95**	9.71**
	(3.47)	(4.19)
Trigger × 6M	7.73**	9.56*
	(3.15)	(4.62)
Trigger × 7M	6.96***	9.42**
	(2.33)	(3.58)
Outcome mean	317.19	300.26
Observations	323,838	148,458
R^2	0.96	0.95
Year-month FE	Y	Y
CUSIP FE	Y	Y
Bond characteristics	Y	Y

Note: This table reports coefficients plotted in Figure 1. See note to Figure 1 for details. Standard errors are double clustered by state and year-month.
*** 1%, ** 5%, * 10% significance level

Table A2: Effect of Overturning Roe v. Wade on Municipal Bond *Spreads*

	Dependent variable: Spread (bp)			
	(1)	(2)	(3)	(4)
Trigger × Post	12.49**	6.66*	7.87**	9.32**
	(4.56)	(3.17)	(2.74)	(3.09)
Effect as % mean	9.5	5.06	5.99	7.09
Outcome mean	131.37	131.37	131.37	131.37
Observations	128,803	128,803	128,803	128,803
R^2	0.61	0.92	0.92	0.92
Year-month FE	Y	Y	N	N
State FE	Y	N	N	N
CUSIP FE	N	Y	Y	Y
Rep. governor× Year-month FE	N	N	Y	Y
Economic controls	N	N	N	Y
Bond characteristics	Y	Y	Y	Y

Note: This table repeats Table 2 using municipal bond spread as the outcome. Spread is the bond-month-level size-weighted average difference between bond yield and maturity-matched after-tax Treasury yield; units are bps. Everything else follows Table 2. Standard errors are double clustered by state and year-month.

*** 1%, ** 5%, * 10% significance level

Table A3: Effect of Overturning Roe v. Wade on Secondary Municipal Bond *Rating*

	Dependent variable: Rating			
	(1)	(2)	(3)	(4)
Trigger × Post	-0.121***	-0.014	-0.011	-0.010
	(0.036)	(0.010)	(0.012)	(0.010)
Outcome mean	18.34	18.34	18.34	18.34
Observations	122,954	122,954	122,954	122,954
R^2	0.50	1.00	1.00	1.00
Year-month FE	Y	Y	N	N
State FE	Y	N	N	N
CUSIP FE	N	Y	Y	Y
Rep. governor× Year-month FE	N	N	Y	Y
Economic controls	N	N	N	Y
Bond characteristics	Y	Y	Y	Y

Note: This table repeats Table 2 using bond rating as the outcome. 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. Everything else follows Table 2 except that we exclude bond rating from the controls. Standard errors are double clustered by state and year-month.
*** 1%, ** 5%, * 10% significance level

Table A4: Effect of Overturning Roe v. Wade on Municipal Bond Yields
Alternative Treatment State Definitions

	Dependent variable: Yield (bp)			
	(1)	(2)	(3)	(4)
Panel A: Trigger or pre-Roe ban				
Treat × Post	9.32**	5.96*	6.04**	7.47**
	(4.23)	(3.21)	(2.56)	(2.57)
Effect as % mean	3.16	2.02	2.05	2.53
Outcome mean	294.1	294.1	294.1	294.1
Observations	138,713	138,713	138,713	138,713
R^2	0.79	0.96	0.96	0.96
Panel B: Trigger, pre-Roe, or early gestational age ban				
Treat × Post	7.20	5.25	5.54**	6.85**
	(4.81)	(3.21)	(2.49)	(2.53)
Effect as % mean	2.45	1.79	1.88	2.33
Outcome mean	293.22	293.22	293.22	293.22
Observations	147,229	147,229	147,229	147,229
R^2	0.79	0.96	0.96	0.96
Year-month FE	Y	Y	N	N
State FE	Y	N	N	N
CUSIP FE	N	Y	Y	Y
Rep. governor× Year-month FE	N	N	Y	Y
Economic controls	N	N	N	Y
Bond characteristics	Y	Y	Y	Y

Note: This table repeats Table 2 while redefining treatment states as those with trigger or pre-Roe abortion bans (panel A), or states with trigger, pre-Roe, or early gestational age abortion bans (panel B). The omitted category consists of states with preexisting state laws protecting abortion. Everything else follows Table 2. Standard errors are double clustered by state and year-month.
*** 1%, ** 5%, * 10% significance level

Table A5: Effect of Overturning Roe v. Wade on Municipal Bond Yields
Placeo Tests

	Dependent variable: Yield (bp)					
	Placebo 2019			Placebo 2021		
	(1)	(2)	(3)	(4)	(5)	(6)
Trigger × Post	-0.51	-1.90	-2.09	3.67	-1.06	-0.28
	(1.93)	(1.08)	(1.33)	(2.71)	(1.53)	(1.73)
Effect as % mean	-.19	-.72	-.79	1.92	-.56	-.15
Outcome mean	267.51	267.51	267.51	190.58	190.58	190.58
Observations	155,532	155,532	155,532	124,165	124,165	124,165
R^2	0.97	0.97	0.97	0.99	0.99	0.99
Year-month FE	Y	N	N	Y	N	N
CUSIP FE	Y	Y	Y	Y	Y	Y
Rep. governor × Year-month FE	N	Y	Y	N	Y	Y
Economic controls	N	N	Y	N	N	Y
Bond characteristics	Y	Y	Y	Y	Y	Y

Note: This table conducts placebo tests for Table 2 by replacing 2022 with placebo treatment year 2019 (columns 1-3) or 2020 (columns 4-6). We do not use 2020 because it coincides with the height of the COVID-19 pandemic. Everything else follows Table 2. Standard errors are double clustered by state and year-month. *** 1%, ** 5%, * 10% significance level