Resident Oversight and Municipal Borrowing Decisions†

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Abstract:

We examine how facilitating citizen monitoring through the SEC's 2019 Amendments to Rule 15c2-12—which require municipalities issuing new bonds to disclose private obligations on the centralized EMMA platform—affects municipal borrowing behavior. Using issuance-level data for California municipalities and a stacked difference-in-differences design, we exploit a setting where such information was already public but costly to access. We find that municipalities are significantly less likely to issue private debt following the Amendments. The reduction is strongest in areas where residents display greater information acquisition related to municipal borrowing, higher financial capacity, and stronger historical aversion to debt, consistent with enhanced citizen oversight constraining officials' reliance on opaque private financing. Extending the analysis nationwide, we find even larger reductions in private borrowing among non-California municipalities, where the Amendments newly revealed this information. Overall, the evidence indicates that lowering information-processing costs can strengthen citizen monitoring and meaningfully reshape municipal financing choices.

Keywords: Resident Monitoring, Household Oversight, Information Processing Costs, Municipal Bond, Municipal Disclosure, Public Finance

JEL Classification: G18, H72, H74, D83, M48

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1. Introduction

The municipal borrowing market is sizeable, with approximately $400 billion in new bond issues each year and nearly $4 trillion in outstanding debt (MRSB, 2022). Proceeds from these bonds enable local governments to fund important projects, such as public schools, transportation infrastructure, facilities, and utilities. Despite the vast scale and economic significance of this market, it is surprisingly opaque, even after the passage of regulations aimed at enhancing transparency (e.g., the establishment of the Electronic Municipal Market Access (EMMA); Rule Change SR-MSRB-2010-03) (NFMA 2015; Cuny, 2016; SIEPR 2017). Prior research has largely examined how regulatory efforts that increase transparency affect municipal bond investors (see, e.g., Cuny, 2018; Zhang, 2024). However, much less is known about how greater access to information influences municipal financing decisions, particularly through the monitoring role of residents and taxpayers who ultimately bear the costs of local borrowing.

Our research question is whether and how citizens' improved access to information about municipal private debt influences municipal financing decisions. We address this question by studying the SEC's 2019 Amendments to Continuing Disclosure Rule 15c2-12 (hereafter, "Amendments"), which require municipalities that issue new bonds on or after February 27, 2019 to disclose all subsequent private obligations, including bank loans and private placements, on the centralized EMMA repository.[1] [2] Our analysis focuses on California municipalities because they were already required to make this information publicly available through the State Treasurer's

[1] We use terms "private borrowing", "private debt", and "private obligations" synonymously to include privately arranged borrowing, such as bank loans or debt sold directly to banks, pension funds, insurance companies, and other financial institutions (SIEPR, 2017).

[2] Information on total debt outstanding is available in municipalities' annual reports. However, EMMA provides issuance-level disclosures for private obligations, detailing seniority, maturity, and interest rates. In contrast, annual reports present aggregated figures, and private obligations or other financial instruments are often not reported separately. This opacity was what the 2019 regulation sought to address.

Office website.[3] Thus, while the Amendments did not introduce new disclosure obligations for California municipalities, they reduced the cost of accessing existing information, particularly for non-investor residents who would otherwise have been unlikely to retrieve it from the Treasurer's website. Consequently, this research setting allows us to identify how residents and taxpayers respond when information about municipal debt obligations becomes more accessible and, in turn, more likely to be disseminated through the media or other intermediaries.

Municipal bonds have traditionally been the primary source of credit for local governments (Ivanov and Zimmermann, 2024). Following the 2008 financial crisis, however, municipalities began relying more heavily on private borrowing, issuing debt directly to financial institutions such as banks, pension funds, and insurance companies (Federal Reserve, 2009; SIEPR, 2017). Private debt offers municipalities several advantages vis-à-vis publicly-traded bonds, including lower issuance costs, faster funding, more flexibility in renegotiation, and easier access to capital during fiscal shortfalls (Moldogaziev et al., 2019; Ivanov and Zimmermann, 2024).

Despite these benefits, municipalities' growing dependence on private debt raises significant concerns for other stakeholders. Because private borrowing bypasses voter approval, it is more susceptible to agency conflicts (Jensen and Meckling, 1976). In particular, officials may use private debt to pursue short-term political objectives rather than projects that serve the long-term interests of residents. For example, proceeds from private debt issuances can be directed toward selective tax abatements, expanded discretionary spending ahead of elections, or other initiatives that strengthen officials' political standing at the expense of fiscal responsibility (Alesina and Tabellini, 1990; Alesina and Passalacqua, 2016; Nakhmurina, 2024). A stark illustration is Jefferson County, Alabama, where officials accepted bribes tied to private debt and

[3] In this paper, we use the term "municipalities" broadly to refer to all local government entities that issue debt, including not only cities and towns but also school districts (e.g., Maffett et al. 2025; Ivanov and Zimmerman 2024).

swap deals, ultimately pushing the county into a $4.2 billion bankruptcy in 2011—the largest municipal bankruptcy in U.S. history at the time.[4] The costs of opacity ultimately fell on the community, as sewer rates rose by 7.9% annually for four consecutive years.

Beyond these and other governance concerns, private debt often carries higher borrowing rates and exposes municipalities to greater credit and liquidity risk. Unlike public bonds, it typically has shorter maturities, higher seniority, and more restrictive covenants, and is secured by collateral or specific revenue pledges. During fiscal stress, these features can trigger higher borrowing costs or early repayments. The resulting fiscal pressure is ultimately borne by local residents, either through higher taxes to service debt obligations or through cuts to essential services such as education, policing, and sanitation (e.g., Nguyen, Volla, Wong 2017).

We argue that the SEC's Amendments strengthened voters' ability to monitor their municipalities' debt obligations, thereby reducing both the appeal and reliance on private debt. The EMMA repository made information about private debt more conspicuous and accessible than what was already available on the State Treasurer's Office website (e.g., Christensen et al., 2017). Consequently, local media and watchdogs could more easily obtain and disseminate this information. This improved accessibility likely enhanced citizens' monitoring and deterred officials from engaging in opaque financing practices (e.g., Besley & Smart, 2007; Errico et al., 2025), as local officials are sensitive to reputational, career, and re-election concerns (Cuny, 2016).

However, whether the Amendments reduced municipalities' use of private debt is *a priori* ambiguous. Municipalities generally have a relatively low risk of default because of their taxing authority or dedicated revenue streams (e.g., toll roads, utilities). Consequently, residents may not

[4] Detroit's 2013 bankruptcy, the largest municipal bankruptcy in U.S. history, also underscores the importance of transparency in municipal financial obligations. The city entered into private interest rate swaps and pension-related borrowing that bypassed public disclosure, limiting effective oversight.

perceive private debt as a significant concern or respond strongly to related disclosures. Moreover, enhanced monitoring may not discourage private borrowing, but may instead encourage municipalities to use such funds more efficiently, directing them towards projects that benefit residents (e.g., Besley and Smart, 2007; Gadenne, 2017; Nakhmurina, 2024).

To assess whether the Amendments reduced municipalities' reliance on private debt, we examine the choice between public and private debt at the debt-issuance level, using a stacked difference-in-differences (DiD) design.[5] The design covers three years before and after each cohort's treatment year from 2019 to 2021, yielding an overall sample period of 2016 - 2023. For each cohort, defined by its treatment year, we classify as "treated" the debt issuances of municipalities that issued new bonds during that year—thereby becoming subject to the new disclosure requirements for subsequent private borrowing—and as "control" the issuances of municipalities that had not yet issued new bonds by that year.

By analyzing only municipalities that borrow, our design abstracts from changes in overall issuance incentives and isolates the choice between public and private debt. In addition, we include municipality x cohort and year x cohort fixed effects to absorb municipality-specific characteristics and cohort-specific time trends. Our results suggest that treated municipalities are significantly less likely to issue private debt. These reductions appear only after municipalities became subject to the new disclosure requirements (phased in between 2019 and 2021), not before, alleviating concerns about differential pre-trends in debt choice or a single event driving the results. The consistent divergence across 2019, 2020, and 2021 further supports the interpretation that the effect

[5] We use the linear probability model (LPM) because it allows our main coefficient to be interpreted as the average treatment effect. In nonlinear models, coefficients do not capture average effects, and the DiD effect can vary with covariates (Ai and Norton, 2003; Puhani, 2012; Greene, 2012). The LPM also accommodates rich fixed effects without the incidental parameters problem that arises in nonlinear models, particularly when the number of cross-sectional units (N) is large and the number of time periods (T) is small, as in our setting (Neyman and Scott, 1948; Lancaster, 2000; Arellano and Honoré, 2001). As shown in the robustness section, our results are robust to logit and probit.

stems from the regulatory change itself, rather than from coincidental shocks—such shocks are unlikely to persist across three consecutive years and to affect only municipalities covered by the amendments in each of the years.

Next, we investigate whether the observed reduction in private debt can be attributed to enhanced monitoring by local residents. If the Amendments improve residents' oversight of municipal borrowing, we should observe larger reductions in private debt when residents have greater capacities or incentives to monitor fiscal behavior. We test this mechanism using three proxies for resident monitoring. First, we measure public information acquisition using county-level Google search activity for municipal borrowing–related terms. Second, we capture residents' capacity to interpret and act on financial disclosures through demographic characteristics—financial literacy, income, and age composition. Third, we proxy residents' general aversion to municipal borrowing using voting outcomes from school district bond elections held before the Amendments. A lower percentage of "Yes" votes indicates greater concern about new borrowing and thus stronger incentives to monitor, especially for private debt, which typically entails higher borrowing costs and is more likely to finance politically motivated projects than public bonds.

We find that reductions in private borrowing are significantly larger in municipalities where residents exhibit higher information acquisition, stronger financial capacity, and greater reluctance toward new debt. These results suggest that the Amendments amplified local monitoring, particularly in communities with residents who are better equipped or more motivated to scrutinize fiscal decisions. Taken together, the evidence supports enhanced citizen oversight, enabled by lower information-processing costs, as a key mechanism driving the decline in private borrowing following the Amendments.

We acknowledge that the Amendments may influence municipal borrowing through channels other than residential monitoring, particularly by affecting how bond investors process and act on the newly accessible information.[6] However, in California, institutional investors—who dominate the municipal bond market in terms of trading activity, market influence, and price discovery (e.g., Green, Hollifield, and Schürhoff, 2007; Schwert, 2017)—were likely already aware of and using the information available on the State Treasurer's Office website prior to the Amendments, or indirectly incorporating it through credit ratings, which reflect municipalities' private debt obligations (e.g., Nguyen et al., 2017; Moody's, 2021).[7] Accordingly, information about private debt was likely already incorporated into the pricing of California municipal bonds, making it less likely that the Amendments had a material price effect. Moreover, even if bond pricing did adjust, for instance due to changes in retail investors' trading, it is unclear how such changes would translate into a reduced reliance on private debt or a shift toward public bonds.

To empirically rule out the possibility that bond trading drove the reduction in private borrowing, we conduct two falsification tests. First, if retail investors' trading behavior was responsible for the reduction, municipalities with greater retail ownership of their bonds should exhibit larger reductions in private debt. We find no significant differences between municipalities with high versus low retail ownership. Second, if post-Amendment revisions in investor risk assessments drove the decline in private debt, municipalities with sparser credit rating coverage prior to the Amendments should have exhibited larger reductions, since credit ratings convey overall credit risk, including private obligations. We again find no such differences. Together, this

[6] Investors and residents are not mutually exclusive groups, but they influence municipalities through distinct channels. Investors monitor through bond pricing, whereas residents do so through civic and political engagement, including petitions, advocacy, protests, and voting on local officials and initiatives such as budgets and capital projects. Retail investors who, following the Amendments, gain greater understanding of municipal borrowing may also influence borrowing decisions in their capacity as residents, which would be consistent with our proposed mechanism.

[7] Consistent with this, untabulated analysis indicates a negative relationship between municipalities' private debt obligations and their credit ratings.

evidence suggests that bondholders' trading behavior is unlikely to be the primary driver of the observed reduction in private borrowing.

We conduct a series of robustness tests to address concerns about our research design and to assess the generalizability of our findings and inferences. One concern is that treatment may depend on municipalities' endogenous decision to issue bonds after the Amendments. To address this, we limit our analysis to municipalities that held bond elections in the four months prior to the compliance date and classify those with approved issuances as "treated" and those with rejected issuances as "controls." Because bond elections are determined by voter approval, which lies outside the control of municipal officials, this design introduces plausibly exogenous variation in treatment status. We continue to find that treated municipalities reduce their private borrowing.

We also examine the sensitivity of our results to the definition of treatment cohorts. We define cohorts at the calendar year–month–date level, rather than at the calendar-year level, which is more granular and, in turn, provides tighter control for municipality-specific trends, though at the cost of increasing overlap among control groups across cohorts and reducing the number of observations per cohort. Our findings remain robust to this alternative research design. Additionally, we confirm the results from probit and logit models are consistent with our findings from the OLS specification, These analyses provide consistent evidence that increased public accessibility to private borrowing information reduces municipalities' reliance on private debt.

To further address endogeneity concerns and assess the external validity of our inferences, we compare California with other states, where the Amendments made *new* information available, which can be considered an extreme reduction of information processing costs. We classify municipalities in states other than California as treated and those in California as controls, with treatment timing defined based on the effective date of the Amendments. Because pre-period

private debt data are unavailable for non-California municipalities, we infer private debt by subtracting public debt (from the Common Core of Data) from total new debt issuance (from SDC Platinum). We find that the ratio of private debt falls significantly more in non-California states. This evidence supports the external validity of our findings beyond California's institutional setting, though it carries the caveat that reductions in non-California states' private debt may also reflect institutional or retail investors responding to improved transparency, since they lacked access to private debt information prior to the Amendments, unlike in California, where it was available through the State Treasurer's Office.

Finally, using school districts across California and non-California municipalities, we examine how changes in municipalities' debt structures affected their capital spending. We compare capital expenditures of California municipalities with those in other states and find a greater reduction in total capital spending in non-California municipalities, which experienced larger reductions in information-processing costs. Moreover, this reduction is concentrated in construction-related expenditures rather than equipment-related expenditures, suggesting that municipalities adjust not only the amount, but also the composition of their capital spending.

There are several caveats to our study. First, our evidence does not speak to the welfare implications of reduced private debt. While greater transparency benefits investors and residents, municipalities may overreact to monitoring. Because private debt often complements public bonds by offering flexibility and lower issuance costs, excessive reductions in private debt could lead municipalities to deviate from optimal debt structures and forgo valuable projects. Future research could assess welfare tradeoffs by examining the Amendments' impact on investment efficiency. Second, although we conduct extensive robustness checks, we cannot fully rule out strategic

behavior by municipal officials—e.g., delaying bond issuance to avoid new disclosure requirements—which could introduce a spurious relationship in our main research design.

Despite these caveats, our findings show that disclosure enables residents to influence municipal borrowing. In doing so, our paper contributes to three strands of literature. First, we extend the growing body of research on different stakeholders of disclosure, including consumers, NGOs, and regulators (e.g., Christensen et al., 2017; Blankespoor et al., 2019; Kielty et al., 2023; Duguay et al., 2023; Zhang, 2024; Even-Tov et al., 2024; Noh et al. 2025; Gipper et al. 2025). We examine how improved information accessibility for local residents affects municipal borrowing. This perspective is important because municipalities represent a sizeable component of the U.S. economy, and their residents serve as both taxpayers and monitors of government activities.

Second, we contribute to research on information provision in the municipal bond market. Prior studies examine the determinants of issuers' voluntary disclosure and the effects of information provision on trading activity, small-trade premiums, and transaction costs (Cuny, 2016; Cuny, 2018; Cuny et al., 2021; Zhang, 2024; Cuny et al., 2025). We extend this literature by showing that enhanced information provision not only affects secondary market outcomes but also shapes municipal borrowing.

Third, our paper contributes to the literature on debt structure. Debt financing is an important source of capital for both corporations and municipalities. A large body of work in accounting and corporate finance highlights tradeoffs between private and public debt and the role of the information environment in shaping corporate choices (Armstrong et al., 2010; Diamond, 1991; Rajan, 1992; Bharath et al., 2008). Our study extends these insights to the public finance setting, showing how information influences municipalities' debt structure and highlighting the role of residential monitoring of municipal financing decisions.

2. Institutional Background

2.1. Municipal Borrowing and Regulatory Background

Municipalities play a vital role in the U.S. economy by providing essential public goods and services, such as infrastructure, public facilities, schools, and hospitals—investments that often require significant upfront funding. To finance these initiatives, municipalities frequently rely on borrowing. They can access funds through various means, including municipal bonds, private loans, lease-purchase agreements, and loans from federal agencies. Among these, municipal bonds have historically been the primary source of financing for local governments in the United States (SIEPR, 2017). As of 2022, approximately one million municipal securities were outstanding, totaling about $4 trillion, with annual new issuances averaging nearly $440 billion (MSRB, 2022).

Following the 2008 financial crisis, however, municipalities began relying more heavily on private borrowing, including direct loans and private placements (Federal Reserve, 2009; Ivanov & Zimmermann, 2024).[8] In California, for example, approximately 44% of new debt issuances in 2023 were private debt, representing 15% of total par value—up sharply from only 15% of issuances and 0.7% of par value in 2007 (SIEPR, 2017; CDIAC data). Unlike public bond offerings, private debt involves the direct sale of obligations to a limited group of institutional investors, such as banks, insurance companies, or pension funds. This financing channel provides several advantages, including lower origination and advisory fees, greater flexibility in renegotiation, and more reliable access to capital during fiscal shortfalls.[9] It also permits the use

[8] The financial crisis strained the municipal bond market by weakening financial guarantors (e.g., credit enhancers and liquidity providers), which raised borrowing costs. These guarantors help municipalities secure lower rates by backing bonds with additional credit support.

[9] For example, in September 2016, the District of Columbia Water and Sewer Authority (DC Water) executed a $25 million private placement of tax-exempt environmental impact debt. A DC Water official noted that the authority saved hundreds of thousands of dollars in issuance costs by avoiding a public offering, which would have required obtaining a public credit rating and paying underwriting fees to investment banks (Bond Buyer, 2017).

of credit lines that adjust to changes in a government's credit quality and financial fundamentals (Moldogaziev et al., 2019; Ivanov & Zimmermann, 2024).

Despite these benefits, private debt also poses risks that have drawn growing regulatory attention. Because it bypasses competitive pricing in public markets, municipalities often face higher interest rates (Ivanov & Zimmermann, 2024). Even more concerning is the historic absence of mandatory disclosure requirements (with the exception of California, discussed below), in contrast to the municipal bond market where reporting rules have been steadily strengthened.[10] This opacity heightens the risk of overborrowing and fiscal instability, exposing bondholders to dilution of claims and potential mispricing (Ivanov et al., 2022).[11] For example, municipalities may pledge assets or revenues previously available to bondholders as collateral for new private debt, or enter into agreements with covenants that accelerate repayment in the event of default. Because private debt typically carries shorter maturities, these features increase refinancing risk and reduce the security of existing bondholders (NFMA, 2015).[12]

Limited disclosure also weakens the ability of residents and investors to monitor municipal officials. Without such oversight, officials may circumvent institutional checks and balances, raising the likelihood of suboptimal financial decisions, misallocation of funds, or opportunistic behavior (Nakhmurina, 2024). These concerns provide important context for the SEC's 2019 Amendments, which extended disclosure requirements to private obligations in an effort to reduce information frictions.

[10] e.g., SEC's Continuing Disclosure Rule 15c2-12 implemented in 1989, the launch of the Electronic Municipal Market Access (EMMA) in 2009, and the 2014 Municipalities Continuing Disclosure Cooperation (MCDC) initiative.
[11] Ivanov and Zimmermann (2024) show that bank lending to state and local governments is typically heavily collateralized and holds high contractual priority.
[12] Although municipal bonds have historically had relatively low default rates, bank assessments suggest that 15–25% of municipal borrowers carry meaningful credit risk (Ivanov and Zimmermann (2024)).

To enhance transparency, the SEC amended Rule 15c2-12 in 2018, with the changes taking effect on February 27, 2019. The amendments require issuers of bonds with a principal amount of at least $1 million to promptly disclose any subsequent incurrence or modification of private financial obligations—including bank loans, private placements, and related derivatives. The incurrence of such debt, as well as any defaults or modifications of terms, must be reported within 10 business days to the Municipal Securities Rulemaking Board's EMMA system. This rule change followed growing concerns that the rapid rise in undisclosed bank loans could obscure municipalities' true financial positions and distort bond pricing (Reuters, 2017). By mandating timely disclosure of these obligations, the SEC aimed to prevent issuers from accumulating hidden debts that could undermine liquidity and mislead investors. As then-SEC Chair Jay Clayton noted, the enhanced requirements were intended to ensure that investors and intermediaries are better equipped to make informed decisions about municipal securities.

2.2. SEC's Amendments in California and Monitoring by Households

While the SEC's 2018 Amendments represented an important step toward improving transparency nationwide, California had already introduced similar requirements in 2014. Assembly Bill No. 2274, approved on July 23, 2014, required California municipalities to report their private obligations to the California Debt and Investment Advisory Commission (CDIAC) within 21 days of issuance. These records have been publicly accessible through the State Treasurer's Office website since 2014. After the Amendments took effect in 2019, however, this information was also disseminated via the Electronic Municipal Market Access (EMMA) platform.

The difference between these two platforms is meaningful and important. The Treasurer's Office website, while functional, is not designed for broad public accessibility. It primarily provides information on initial bond offering statements, with limited visibility, searchability, and

user-friendliness. By contrast, EMMA is the SEC's centralized disclosure platform built specifically for the municipal bond market. It is well publicized, widely trusted, and regularly used by investors and the general public. EMMA offers real-time trade prices, official statements, credit ratings, and continuing disclosures (filed yearly; rather than just initial bond offering disclosure), making it far more comprehensive and accessible. Because EMMA is already the standard resource for anyone seeking information about municipal bonds, private debt disclosures made available there are far more likely to be noticed and accessed.

Thus, although the Amendments did not introduce new information for California municipalities, they reduced the costs of accessing and processing existing disclosures (Blankespoor, deHaan, and Marinovic (2020)). This change is particularly relevant for non-investors—such as residents and journalists—who were unlikely to rely on the State Treasurer's website. California thus provides a unique setting to examine how enhanced disclosure accessibility for households, and the monitoring it facilitates, influence municipal borrowing behavior, allowing us to isolate these effects from those related to improved information accessibility for investors.

3. Theoretical Framework and Hypothesis Development

Municipal officials consider several factors when deciding how to finance local projects, including the overall cost of financing, the flexibility of terms and repayment schedules, and the degree of oversight by lenders or other external stakeholders (Diamond, 1984; Diamond, 1991; Moldogaziev et al., 2019; Ivanov & Zimmermann, 2024). Similar to the corporate context, the separation of ownership and control in municipal finance introduces agency conflicts between municipal officials (agents) and stakeholders such as bond investors, private lenders, and residents (principals) (Jensen & Meckling, 1976). Such conflicts can induce officials to pursue borrowing

strategies that are politically expedient but economically inefficient—for instance over-borrowing to fund visible projects before elections or relying on opaque private debt to avoid public scrutiny.

To help mitigate these conflicts, the 2019 Amendments to municipal disclosure rules sought to enhance transparency and reduce information frictions in municipal finance. In particular, it lowered the costs of accessing and processing information about private debt, thereby improving visibility for less sophisticated stakeholders. Since 2014, data on public municipal bonds has been broadly accessible through the EMMA platform. The 2019 Amendments extended this accessibility to include more detailed and timely disclosures about private debt, potentially strengthening accountability in municipalities' uses of private debt.

This change is expected to influence municipalities' financing choices, especially the trade-off between public and private borrowing. Prior research suggests that residents often oppose opaque private borrowing, even when it funds local projects, because such debt can raise overall leverage, increase future tax burdens, and constrain public service provision during fiscal stress (Gadenne, 2017; Errico et al., 2025). Moreover, private debt contracts tend to impose higher interest costs and include restrictive covenants that can disadvantage municipalities and their taxpayers.

Historically, private borrowing has also been linked to rent-seeking and politically motivated fiscal cycles—for example, increased spending ahead of elections. Greater disclosure and the ensuing public attention are therefore likely to discourage these behaviors. Enhanced transparency through EMMA makes it easier for local residents, journalists, and watchdog organizations to detect and question questionable debt practices. In particular, residents who previously faced high information barriers can now more easily monitor officials' borrowing decisions, as EMMA's expanded coverage likely increased awareness of municipalities' private

debt usage. By reducing the opacity surrounding such borrowing, the Amendments empowered residents to hold municipal officials accountable. This transparency likely constrained officials' self-interested behavior, driven by concerns about reputation, re-election, and tenure, and therefore encouraged a shift toward more transparent forms of financing (Cuny, 2016; Nakhmurina, 2024).[13]

Taken together, these arguments motivate our hypothesis:

H: *Municipalities subject to the 2019 Amendments reduce their use of private debt.*

While this hypothesis is directional, our analysis goes beyond a simple yes-or-no test. We examine heterogeneity in the effects of the Amendments across municipalities, guided by the predictions of our proposed resident-monitoring mechanism. Specifically, we expect the decline in private borrowing to be more pronounced in municipalities where residents have historically shown stronger opposition to rising debt, proxied by voting outcomes in bond elections prior to EMMA. We also expect larger reductions in private borrowing in municipalities where residents are better equipped to monitor government actions, as reflected in local information acquisition and financial sophistication.

Collectively, these predictions form the basis for our empirical tests, which evaluate both the average and cross-sectional effects of enhanced transparency on municipal borrowing decisions.

4. Data and Research Design

4.1. Data and Sample

We collect the debt issuance data from the California Debt and Investment Advisory Commission (CDIAC) website, maintained by the California Treasurer's Office, which provides comprehensive records on municipal borrowings across the state. The CDIAC database provides

[13] In Appendix B, we show that borrowing costs for private debt are higher than those for public bonds, consistent with Ivanov & Zimmermann (2024).

detailed information on each debt issuance, including the type of debt, issuance and maturity dates, principal amount, interest rate, and whether the debt was issued privately. Figure 1 presents the yearly borrowing activities of California municipalities.

To construct our sample, we begin with the universe of all public and private debt issuances by California municipalities from 2016 to 2023. During this period, a total of 20,239 debts were issued. We exclude 2,711 debts issued directly by the State of California, as our analysis focuses on comparing municipalities within the state that differ in the timing of when their private debt disclosures first appeared on EMMA. We also exclude 6,827 debts issued by Joint Powers Authorities (JPAs) and Marks-Roos entities, which are joint legal entities established by multiple municipalities to facilitate collective financing. The resulting sample consists of 9,855 debts issued by 1,932 unique municipalities. The most common types of municipalities issuers include school districts (37% of debts), counties (19%), and cities (20%). Among the 9,855 debts, 6,462 debts (66%) are public municipal bonds, and 3,393 debts (34%) are private debts. Due to data availability, some of our analyses focus on school districts specifically. Among the 3,607 debts issued by school districts, 3,196 debts (89%) are public municipal bonds and 411 (11%) are private debts.

We further augment the CDIAC data by linking it to several complementary sources. Public municipal bonds are matched to the SDC Platinum database to obtain CUSIP identifiers. School district issuers are linked to the Common Core of Data (CCD) to obtain additional capital information, including capital outlay expenditures. Finally, to measure the share of municipal bonds traded by retail investors, we merge transaction-level data from Wharton Research Data Services (WRDS) using the CUSIP identifiers obtained from SDC Platinum.

4.2. Research Design

The Amendments apply only to municipalities that issued new municipal bonds with principal amounts exceeding $1 million on or after February 27, 2019. They require these issuers to disclose all subsequent private obligations, including bank loans and private placements, on the centralized EMMA repository. However, the rule does not apply retroactively to undertakings entered into before that date. This regulatory shock creates an opportunity for a difference-in-differences (DiD) research design, comparing municipalities that have issued new qualifying municipal bonds after the compliance date (treatment group) with those that have not (control group). Because municipalities become treated at different points in time, we employ a stacked difference-in-differences (DiD) approach, which provides more unbiased estimates of average treatment effects (Baker et al., 2022). We group observations of all municipalities that become treated in the same year between 2019 and 2021 into a single cohort. For each treatment cohort, we use observations from never-treated municipalities as well as pre-treatment observations from not-yet-treated municipalities as controls.[14] We then compare debt issuances of treated municipalities, which become subject to the requirement to disclose their private debt on EMMA, with debt issuances of control municipalities that are not yet required to do so, over a three-year window before and after the treatment year for each cohort. We conduct our analysis at the debt issuance level to control for municipalities' decisions to issue debts. Specifically, issuing private (or public) debts is a joint decision of i) issuing debt, and ii) choosing between private or public debts. The analysis conducted at the debt issuance level is conditional on the municipalities'

[14] To avoid creating too many small cohorts, which can lead to noisy and imprecise estimates due to limited observations per cohort, we group treated and control municipalities from the same year into a common cohort, resulting in three cohorts. In robustness tests (Table 8B), we show that our results remain similar when we instead treat each treatment date as a separate shock (yielding 353 cohorts). In untabulated analyses, we also confirm that our findings are robust when restricting the control group to never-treated municipalities only.

decision to issue debts. Thus, we can isolate the effects on municipalities' choice between private and public debts while holding constant municiplities' decision to issue debt.

We stack all treatment and control cohorts and estimate the following ordinary least squares (OLS) regression at the debt-issuance level:

$$Private_i = \beta_1 Treat\_Post_i + \sum \beta_n Controls_n + \text{Municipality} \times \text{Cohort FE} + \text{Year} \times \text{Cohort FE} + \text{Quarter FE} + \varepsilon_i \quad (1)$$

where i indexes each public or private debt issuance. The dependent variable, *Private*, is an indicator equal to one if the debt is privately placed. The key variable of interest, *Treat_Post*, equals one for debts issued by treated municipalities after they become subject to the disclosure requirements, and zero otherwise. Specifically, *Treat_Post* is zero for all debts issued either by control municipalities or by treated municipalities before the disclosure requirements take effect. We exclude the first public bond issuance that triggers treatment (i.e., the issuance that makes a municipality subject to the Amendments), which determines treatment assignment.[15]

The control variables include the number of public bonds, the number of private debts, the total par value of debt, the total amount of refinancing among all debts issued in the past two years, and an indicator for whether any of these debts were rated by a credit rating agency. We include municipality × cohort fixed effects to account for time-invariant differences across municipalities within each cohort, year × cohort fixed effects to capture cohort-specific time trends, and quarter fixed effects to control for seasonal borrowing patterns. Standard errors are clustered at the municipality level. Table 1 presents descriptive statistics for all variables used in the analysis.

[15] A potential concern is that municipalities have discretion over whether to issue bonds exceeding $1 million, which could make their exposure to the Amendments endogenous. Some may try to avoid disclosure requirements by issuing smaller bonds. However, this is unlikely given their heavy reliance on external financing. Consistent with this, Figure 2 shows no abnormal changes in borrowing behavior before the first issuance of bonds over $1 million after the Amendments. Even if municipalities strategically adjust the timing or size of debt issuance, such actions are unlikely to explain the persistent changes in private versus public borrowing up to three years after they become subject to the Amendments.

5. Empirical Results

5.1 The Effect of the Amendments on Municipalities' Borrowing Behavior

To examine the impact of the Amendments on municipalities' borrowing behavior, we estimate Equation (1) and report the results in Table 2. Columns (1) and (2) present OLS estimates without and with control variables, respectively. The coefficients on *Treat_Post* are negative and statistically significant. The magnitude of the coefficient in Column (2), –0.228, indicates that, on average, the probability of issuing private debt decreases by 22.8% for municipalities subject to the Amendments. In Columns (3) and (4), we re-estimate Equation (1) using the subsample of school districts, for which we have more granular data on bond election outcomes and types of capital expenditures, and we use these data in subsequent analyses. The results remain robust. Overall, these findings indicate that municipalities subject to the Amendments, which make information about their private obligations easier for citizens to access, exhibit a reduced reliance on private debt.

We further examine the dynamic pattern of treatment effects and assess the validity of the parallel trends assumption in Table 3. Specifically, we decompose the *Treat_Post* indicator into interactions between the treated municipality indicator and year indicators relative to the treatment year. Table 3 shows no significant differences between treated and control municipalities before the disclosure requirements, while the treated municipalities' probability of issuing private debt (conditional on borrowing) declines significantly once they become subject to the new rules, phased in between 2019 and 2021. Figure 3 visually presents these dynamic effects. The consistent and sequential divergence across 2019, 2020, and 2021 indicates that the decline in private debt issuance is a result of the regulatory change itself, rather than coincidental shocks or broader

economic trends, which would be unlikely to emerge exactly when the amendments took effect and persist across all three implementation years.

5.2. Mechanism: Citizens' Monitoring

As discussed in Section 3, we propose that the reduced information processing costs by the Amendments may affect the municipalities' borrowing behavior through enhanced monitoring by residents. We conduct a series of cross-sectional tests to corroborate this mechanism and to rule out alternative explanations.

5.2.1 Monitoring by Local Residents

Local residents may be particularly concerned about private borrowing because it is less transparent, bypasses voter approval, and can increase fiscal risk (Gadenne, 2017; Errico et al., 2025). Unlike public bond issues, which are subject to disclosure and often require voter authorization, private debt is negotiated bilaterally with limited public visibility, even though residents ultimately bear the fiscal consequences. The Amendments make it easier for residents to access and interpret borrowing information, thereby strengthening their oversight of local officials. Stronger monitoring, in turn, constrains opaque or politically motivated borrowing and reduces municipalities' reliance on private debt. If this monitoring channel drives the observed effects, the reduction should be more pronounced among municipalities where residents are more attentive and have stronger incentives to monitor fiscal behavior.

To test this mechanism, we conduct cross-sectional analyses using three proxies for residents' monitoring: (1) their general tendency to acquire information about municipal borrowing, measured by Google search activity; (2) their capacity to understand and act on such information, inferred from demographic characteristics; and (3) their aversion to municipal borrowing, reflected in prior bond election outcomes.

First, we partition the sample based on county-level Google search volume for municipal bond–related keywords, using search activity as a proxy for the extent of public information acquisition (e.g., Noh, So, Zhu 2025; Gipper, Gu, Kim, Noh 2025). We obtain 2018 Google search indices—the year before the Amendments—for each of California's 58 counties and assign them to municipalities, which are more granular units.[16] [17] We divide municipalities into above- and below-median subsamples. We then estimate Equation (1) separately for each subsample. The results are shown in Table 4 Panel A. The coefficient on $Treat\_Post_i$ is negative in both groups but larger and more significant among municipalities in high-search counties ($p < 0.05$). Because many municipalities share same Google index values (due to the standardized nature of the Google Trend data) differ in the number of debt issuances, and are unevenly distributed across counties, the subsample sizes are unbalanced. To address potential bias from this imbalance, we exclude Los Angeles County, which accounts for 1,153 observations. The results, shown in Table 4, Panel B, remain robust ($p < 0.01$). These findings suggest that the Amendments had stronger effects where residents exhibited greater attention to local borrowing, consistent with enhanced monitoring.

Second, we examine heterogeneity by household demographics—financial literacy, income, and age. Residents with higher education, greater income, and working-age status may have greater capacity to interpret fiscal disclosures and monitor officials effectively. We proxy financial literacy by the percentage of the population with a bachelor's or higher degree in business, income by the percentage of families earning above $45,000 annually, and age by the share of residents aged 25–60 in the county. Using 2018 Census data (the year prior to the Amendments),

[16] We exclude municipalities, such as water and school districts, that span multiple counties.
[17] The keywords related to municipal bonds include the municipality name followed by one of the following terms: "municipal bond," "municipal bonds," "muni bond," "muni bonds," "muni," or "munis" (e.g., Alameda municipal bond).

we divide the sample at the median for each demographic measure and re-estimate Equation (1).[18] The results, shown in Table 5, Panel A, indicate that the coefficient on $Treat\_Post_i$ is significantly negative in both groups but larger in municipalities with higher financial literacy, income, and working-age populations ($p < 0.05$; $p < 0.10$; $p < 0.10$). After excluding Los Angeles County to balance sample sizes, the results remain robust ($p < 0.05$; $p < 0.10$; $p < 0.05$), as shown in Table 5, Panel B. These findings suggest that reductions in private borrowing are more pronounced where residents have greater capacity to process and act on fiscal information.

Third, we assess heterogeneity in residents' reluctance toward municipal borrowing. We expect larger effects where residents have historically shown stronger aversion to municipal debt, proxied by voting outcomes from municipal bond elections held prior to the Amendments. The rationale for using this measure as a proxy for resident monitoring is as follows: residents who oppose increases in municipal borrowing do so because additional debt can raise their tax burden or heighten the risk of financial distress for their municipality. These residents are likely to be particularly concerned about private borrowing, which generally entails higher borrowing costs, shorter maturities that heighten refinancing risk, greater seniority, and more restrictive covenants—characteristics that can further elevate a municipality's financial risk —and is more often used for politically motivated projects.

We obtain municipal bond election data from the California Treasurer's Office. These elections typically involve General Obligation (GO) bonds issued by school districts, which require voter approval. Because municipalities with such elections are primarily school districts, we focus on this subset for this analysis. We partition the sample based on the percentage of "Yes"

[18] Similar to the Google index analysis, we exclude municipalities that span multiple counties. Because municipalities differ in the number of debt issuances and counties vary in the number of municipalities they contain, the subsample sizes are unbalanced.

votes, measured both in absolute terms and relative to the passing threshold, and estimate Equation (1) separately for each subsample.[19] The results, presented in Table 6, show that the coefficient on $Treat\_Post_i$ is negative in both groups but larger and more significant in the subsample with fewer "Yes" votes ($p < 0.05$). These results indicate that the Amendments had stronger effects where residents were more skeptical of municipal borrowing, consistent with enhanced citizen monitoring as the underlying mechanism.

5.2.2 Testing the Retail Investors' Learning Channel

One possible alternative explanation is that the observed changes in municipalities' borrowing behavior are driven by shifts in pricing pressure from bond investors.[20] In particular, retail investors—who have more limited resources—may have been less aware of the State Treasurer's Office website and more reliant on the EMMA repository. If so, the Amendments could have changed their trading behavior, indirectly influencing municipalities' financing decisions. However, the municipal bond market is dominated by institutional investors who likely already incorporated information about municipalities' private debt. Thus, we argue that it is unlikely that changes in retail investors' trading behavior explain the reductions in private borrowing we document.

To test this empirically, we partition the sample based on the percentage of municipal bonds traded by retail investors, following Gillette et al. (2020). We obtain municipal bond transaction data from Wharton Research Data Services (WRDS) and identify retail trades as those with par values below $100,000, consistent with DeHaan et al. (2023) and Zhang (2024). We then estimate Equation (1) separately for each subsample, and the results are presented in Table 7, Panel

[19] We exclude school districts without bond elections within two years before the Amendments. Because municipalities differ in the number of debt issuances, subsample sizes are not the same.

[20] To clarify, we aim to rule out retail investors influencing borrowing decisions through bond pricing. Their influence as residents, for example through civic or voting channels, is consistent with our proposed mechanism.

A. The coefficient on $Treat\_Post_i$ is significantly negative for both municipalities with lower and higher retail trading shares, and the difference between the two subgroups is not statistically significant ($p > 0.10$). These results suggest that retail investors are not the primary channel through which the Amendments affected municipalities' use of private debt.

5.2.3 Testing the Credit Risk-Updating Channel

Another potential explanation is that the Amendments influenced municipalities' borrowing decisions by altering investors' assessment of municipal credit risk. By providing more comprehensive fiscal information—including details about private obligations—the Amendments could have prompted investors (both institutional and retail) to revise their risk evaluations, leading municipalities to rely less on private borrowing. If this mechanism were at play, we would expect larger reductions among municipalities with sparser credit rating coverage before the Amendments, since ratings already convey overall credit risk, including exposure to private debt.

To test this possibility, we partition municipalities into two subsamples based on the percentage of their bonds covered by credit rating agencies. A municipality is classified as low (high) coverage if the share of its rated bonds in the two years preceding the Amendments is below (above) the median. We then estimate Equation (1) for each subsample. As reported in Table 7, Panel B, the coefficient on $Treat\_Post_i$ is significantly negative in both subsamples, and the difference between the two coefficients is statistically insignificant ($p > 0.10$). These results suggest that credit risk updating by investors is not a primary driver of the observed reduction in municipalities' private borrowing.

5.3 Robustness Tests

Our empirical design may be subject to the concern that the issuance of new municipal bonds after the compliance date is an endogenous decision by municipal officials. Some

municipalities might strategically delay issuing new bonds or reduce their principal amounts to avoid triggering the disclosure requirements under the Amendments, which apply when a municipality issues a public bond with a principal amount exceeding $1 million. Such endogenous timing or sizing of bond issuances could raise concerns about bunching around the treatment threshold. Although more than 95% of municipal bonds have principal amounts exceeding $1 million, making it unlikely that municipalities with substantial financing needs would strategically reduce their borrowing amounts, we conduct several additional robustness tests to further address this concern.

5.3.1 Bunching in Principal Amounts and Timing of New Municipal Bonds

We first examine whether municipalities strategically adjusted either the principal amounts or the timing of new bond issuances around the Amendments to avoid triggering the new disclosure requirements. Figure 2, Panel A, plots the percentage of municipal bonds issued with principal amounts of at least $1 million. The figure shows that the vast majority of bonds exceed this threshold and that the share of such bonds remains stable around the time of the Amendments. This pattern suggests little evidence of bunching just below the $1 million cutoff and indicates that municipalities did not strategically reduce bond sizes to avoid compliance, consistent with the fact that most municipalities require funding well above $1 million to finance their projects.

Figure 2, Panel B, plots the monthly number of new municipal bond issuances around the Amendments. While there is a modest decline in total issuances following the Amendments, the pattern appears consistent with normal seasonal fluctuations in municipal borrowing rather than a systematic response to the new regulation. We do not observe any clear clustering or postponement of bond issuances around the compliance date.

It is also important to note that any strategic behavior by municipalities to delay or avoid issuing public bonds—and thereby avoid disclosure of their private obligations—would bias our estimates downward rather than upward. If municipalities that rely heavily on private debt strategically postpone issuing public bonds, they would remain untreated, reducing the estimated magnitude of the treatment effect. In other words, we believe the potential selection of municipalities with relatively lower disclosure costs into treatment would make our findings conservative.

5.3.2 Exogenous Variation Based on the Outcome of Bond Elections

To further alleviate the concerns about municipal officials' endogenous selection, we exploit a variation that affects treatment status but is plausibly exogenous to municipal officials' bond issuance selection – the outcomes of bond elections. Because bond elections are proposed by municipal officials, we can hold municipal officials' incentives relatively constant for municipalities that hold bond elections. As discussed in Section 5.2.1, the outcomes of bond elections are determined by local residents, whose objective function is plausibly exogenous to that of municipal officials. Thus, by classifying treatment status based on the election outcome, we can isolate the exogenous variation in treatment status, conditional on relatively similar municipal officials' incentives.

We restrict the sample to the 94 school districts that held bond elections between November 1st, 2018, and February 27th, 2019 (the compliance date) and that issued bonds after the Amendments if the proposals were approved.[21] Conditional on having proposed a bond election, municipalities with approved issuances are classified as treated, while those with rejected

[21] In California, bond elections are typically held in conjunction with general elections. In our sample, nearly all bond elections occurred on November 6, 2018—the date of the statewide general election—with only one additional election on February 26, 2019. The most recent elections prior to November 6, 2018, were held on June 5, 2018.

issuances serve as controls. Among them, 79 (84%) municipalities' bond elections were passed, creating the treatment group. We estimate Equation (1) with this restricted sample and report the results in Table 8 Panel A. The coefficient on *Treat_Post* $_i$ is significantly negative and its magnitude is comparable to the baseline estimates. These results support the internal validity of our findings by showing that the estimated effects hold in a setting where treatment status is determined by residents' voting outcomes rather than municipal officials' strategic selection.

5.3.3 Define Cohorts by Treatment Date

In our main analysis, we define cohorts by treatment year, grouping all municipalities that become treated within the same calendar year into a single cohort. This approach results in three unique cohorts. Defining cohorts annually provides a sufficient number of observations per cohort, which helps reduce sampling noise and ensures more stable estimation. It also limits unnecessary overlap in control observations across cohorts, The tradeoff, however, is that it aggregates municipalities with different treatment dates within a year, potentially blurring short-term dynamics between treated and control observations within each cohort.

To address this concern, we conduct a robustness test in which we instead define each municipality's exact treatment date as a separate cohort, yielding 353 unique cohorts. As shown in Table 8, Panel B, our results remain robust under this more granular cohort definition, suggesting that our findings are not sensitive to how treatment cohorts are defined.

5.3.4 Nonlinear Models: Probit and Logit Models

In our analysis, we opt for the linear probability model (LPM) because it facilitates the interpretation of coefficients. The LPM allows our main coefficient to be interpreted as the average treatment effect—the average difference in outcomes between treated and control groups before and after EMMA. In contrast, in nonlinear models such as probit or logit, coefficients do not

capture average effects, and the DiD effect can vary with covariates in both sign and magnitude (Ai and Norton, 2003; Puhani, 2012; Greene, 2012). Moreover, the LPM accommodates rich fixed effects without the computational burden or "incidental parameters problem" associated with nonlinear models. The incidental parameters problem arises in nonlinear models with many fixed effects because, unlike linear models where individual effects can be partialled out, nonlinear estimators must jointly estimate all parameters (Angrist and Pischke, 2009; Greene, 2012). The problem is particularly severe when the number of cross-sectional units (N) is large but the number of time periods (T) is small, as in our setting (Neyman and Scott, 1948; Lancaster, 2000; Arellano and Honoré, 2001; Wooldridge, 2010).

However, in this section, we confirm that our results are consistent across alternative functional forms for the binary outcome, specifically probit and logit specifications. The results are shown in in Table 8 Panel C.[22] The coefficient on $Treat\_Post_i$ remains negative and statistically significant.

5.3.5 Comparing California with Non-California Municipalities

For non-California municipalities, the Amendments made private obligations publicly accessible for the first time. This new transparency represents an extreme reduction in information processing costs, likely producing stronger treatment effects. We therefore conduct a difference-in-differences (DiD) analysis that treats non-California municipalities as a high-treatment group and California municipalities as a low-treatment group. Because the Amendments were exogenously imposed, this design helps mitigate concerns about endogenous selection by municipal officials. In addition, comparing California to other states allows us to assess the

[22] The number of observations is smaller in the probit regression, because some fixed effects groups perfectly predict the outcome variable.

external validity of our findings beyond the specific institutional setting of California. We expect a larger reduction in private borrowing among non-California municipalities.

A key challenge of this design is that private debt issuances were not disclosed for non-California municipalities before the Amendments, making direct measurement of pre-period private borrowing impossible. To address this issue, we infer private debt amounts as the difference between total new debt issuances and public bond issuances for each fiscal year. We focus on school districts, for which annual totals of new debt issuances are available from the Common Core of Data (CCD). Data on public bond issuances are obtained from SDC Platinum. We measure borrowing choices as the ratio of private to total new debt issuances in each municipality and fiscal year: Amounts of private debts / Amounts of total debts. We estimate the following model:

$$Ratio\ of\ Private\ Debt_{i,t} = \beta_1\ Treat_{i,t} * Post_{i,t} + \sum \beta_n\ Controls_n + \text{Municipality FE} + \text{Year FE} + \varepsilon_{i,t} \quad (2)$$

where i indexes municipalities and t indexes years. The treatment group consists of non-California municipalities that experience a larger reduction in information processing costs, while the control group consists of California municipalities. *Post* equals one for fiscal years 2019 onward. The sample covers fiscal years 2017–2021, excluding 2019.

Column (1) of Table 9, Panel A reports the results. The coefficient on $Treat_{i,t} * Post_{i,t}$ is significantly negative, suggesting that a greater reduction in information processing costs leads to a greater decline in the use of private debt. However, this finding should be interpreted with caution, as the observed decrease in private debt among non-California states could partly reflect bond investors' reactions to improved transparency. Prior to the Amendments, these investors lacked access to private debt information—unlike in California, where such data were already available through the State Treasurer's Office.

We also validate our inferred measure of private debt using the California sample, where both private and public borrowing are directly observable. Specifically, we define the treatment group as municipalities that issued new municipal bonds after 2019, exclude each municipality's treatment year, and re-estimate Equation (2). Column (2) of Table 9, Panel A shows that the coefficient on $\text{Treat}_{i,t} \times \text{Post}_{i,t}$ remains significantly negative and comparable in magnitude to the baseline results in Table 2, supporting the validity of the inferred private debt ratio measure.

5.3.6 Real Effects on Municipalities' Fiscal Spending Adjustments

Descriptive statistics show that proceeds from private debt are more frequently used for capital projects (e.g., school facility improvements), whereas proceeds from public bonds are more often used for refinancing. This pattern suggests that changes in debt structure following reduced information processing costs may also alter municipalities' real fiscal activities.

We test this prediction using a DiD framework that compares capital expenditures of California municipalities with those of other states, focusing on the school district sample for which detailed expenditure data are available. As reported in Table 9, Panel B, we find a larger decline in total capital expenditures among non-California municipalities. This decline is driven primarily by reductions in construction-related expenditures, rather than instructional equipment expenditures, indicating that municipalities adjust both the level and composition of capital spending in response to the Amendments.

6. Conclusion

This paper examines how the SEC's Amendments to Rule 15c2-12 reshape municipal borrowing. The Amendments, which require disclosure of municipalities' private obligations on the EMMA platform, substantially increase transparency around these obligations. We find that municipalities subject to the Amendments significantly reduce their reliance on private debt

relative to unaffected municipalities. The effect is especially pronounced where local residents have stronger incentives to monitor, underscoring the central role of household oversight in municipal finance. Together, these findings show that lowering information-processing costs can meaningfully alter public-sector financing decisions, highlighting the power of transparency to influence how local governments structure their debt.

To our knowledge, this study is the first to demonstrate that residents' access to fiscal information affects municipalities' financing choices. While prior research focuses primarily on how information provision affects municipal bond investors, we extend this literature by showing that enhanced accessibility of information for residents also shapes local governments' capital-structure decisions between public and private debt. By documenting how improved transparency empowers residents to influence borrowing practices and subsequent capital spending, we uncover a previously underexplored channel through which disclosure regulation affects real outcomes in the public sector. Our findings offer relevant insights for regulators such as the Municipal Securities Rulemaking Board (MSRB), suggesting that transparency policies can have broader and lasting effects on fiscal behavior and accountability.

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Appendix A. Variable Definitions

Variable	Definition
Capital outlay - construction $_{j,t}$	Direct expenditure for construction of buildings, roads, and other improvements in municipality *j* and fiscal year *t*.
Capital outlay - instructional equipment $_{j,t}$	Direct expenditure for purchases of instructional equipment in municipality *j* and fiscal year *t*.
Credit Rating $_i$	Credit rating of the municipalities' debt *i*.
Federal Taxable $_i$	A dummy variable indicating whether the municipalities' debt *i* is federally tax-exempt.
Have Rating$_i$	A dummy variable indicating whether the municipalities' debt *i* has credit rating in the past two years.
Have Credit Rating $_i$	A dummy variable indicating whether the municipalities' debt *i* is has credit rating.
NIC $_i$	True interest costs of the debt *i*, calculated as the interest cost to be paid by the issuer over the life of all the bonds by accounting for the net original issue discount/premium and the interest to be paid over the life of the issue but not accounting for the time value of money.
Num_Private $_i$	The number of private debt issued by the municipality in the past two years before the debt *i*. We take log transformation.
Num_Public $_i$	The number of public municipal bonds issued by the municipality in the past two years before the debt *i*. We take log transformation.
Par$_i$	Amount of Par values of the debt issued by the municipality in the past two years before the debt *i*. We take log transformation.
Par Value $_i$	Amount of Par values of the debt *i*. We take log transformation.
Private $_i$	= 1 if the debt *i* is private debt and 0 if it is public municipal bond.
Ratio of Private Debt $_{j,t}$	The ratio of new private debt issuance to total new debt issuance in municipality *j* and fiscal year *t*. The new private debt issuance is inferred as the difference between total new debt issuance and total public bond issuance.
Refunding $_i$	Amount of refunding of the debt issued by the municipality in the past two years before the debt *i*. We take log transformation.
Treat_Post $_i$	= 1 if the municipality is subject to the new disclosure requirements when issuing the debt *i*, and 0 if it is not.
TIC $_i$	True interest costs of the debt *i*, calculated as the interest cost to be paid by the issuer over the life of all the bonds by accounting for the time value of money.
Time to Maturity $_i$	Time to maturity of the debt *i*, calculated as the number of month between maturity date and sale date.
Total Capital outlay $_{j,t}$	Direct expenditure for construction of buildings, roads, and other improvements, and for purchases of equipment, land, and existing structures in municipality *j* and fiscal year *t*.

Appendix B
Do Private Debt Have Higher Interest Rates?

Panel A: Descriptive Statistics

	N	Mean	STD	25th	Median	75th
TIC_i	5,423	3.339	1.930	2.243	3.049	3.878
NIC_i	5,026	3.457	1.964	2.319	3.190	3.990

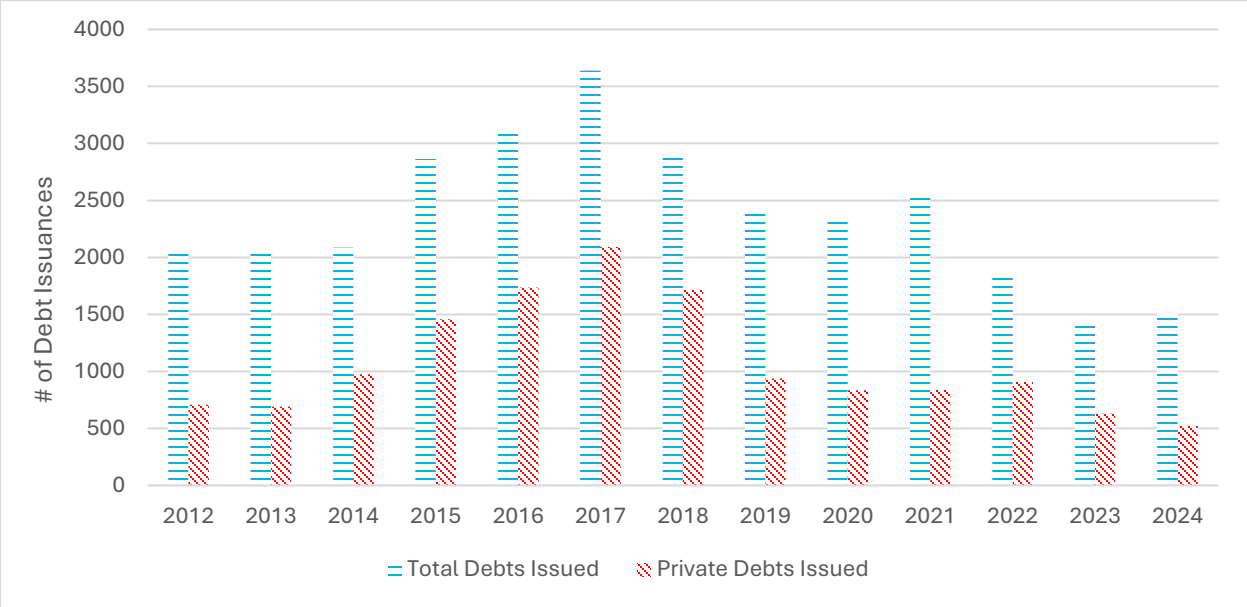
Panel B: Interest Rates of Private Debt

DV =	TIC_i	NIC_i	TIC_i	NIC_i
	(1)	(2)	(3)	(4)
Private $_i$	0.334***	0.200***	0.408***	0.303***
	(5.92)	(2.90)	(9.08)	(6.78)
Time to Maturity $_i$	0.006***	0.006***	0.006***	0.007***
	(27.20)	(27.96)	(51.63)	(54.47)
Par Value $_i$	-0.181***	-0.179***	-0.172***	-0.155***
	(-2.92)	(-2.74)	(-7.89)	(-7.11)
Federal Taxable $_i$	0.651***	0.573***	0.721***	0.640***
	(11.69)	(10.85)	(15.24)	(13.57)
Have Credit Rating $_i$	0.596	1.028	1.498***	1.965***
	(1.07)	(1.42)	(3.39)	(3.88)
Credit Rating $_i$	-0.091**	-0.123**	-0.149***	-0.185***
	(-2.53)	(-2.18)	(-3.59)	(-3.97)
Num_Private $_i$	-0.084**	-0.070*	-0.014	0.004
	(-2.13)	(-1.81)	(-0.44)	(0.12)
Num_Public $_i$	0.077	0.124**	0.354***	0.377***
	(1.44)	(2.36)	(32.17)	(37.60)
Sample	All Issuers			
Municipality FEs	Y	Y	N	N
Year-Quarter FEs	Y	Y	Y	Y
Observations	5,423	5,026	5,995	5,601
Adj. R^2	0.80	0.80	0.78	0.80

This table presents debt-issuance-level regression results examining the association between interest rates and private debt. Panel A reports descriptive statistics for the debt-level sample, and Panel B presents the OLS regression results. Variable definitions are provided in Appendix A. Standard errors are clustered by municipality, and t-statistics are reported in parentheses below the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Figure 1. Yearly Borrowing Activities in California Municipalities

Panel A: Number of total debts versus private debts issued per year – All Issuers



Panel B: Number of total debts versus private debts issued per year – School Districts

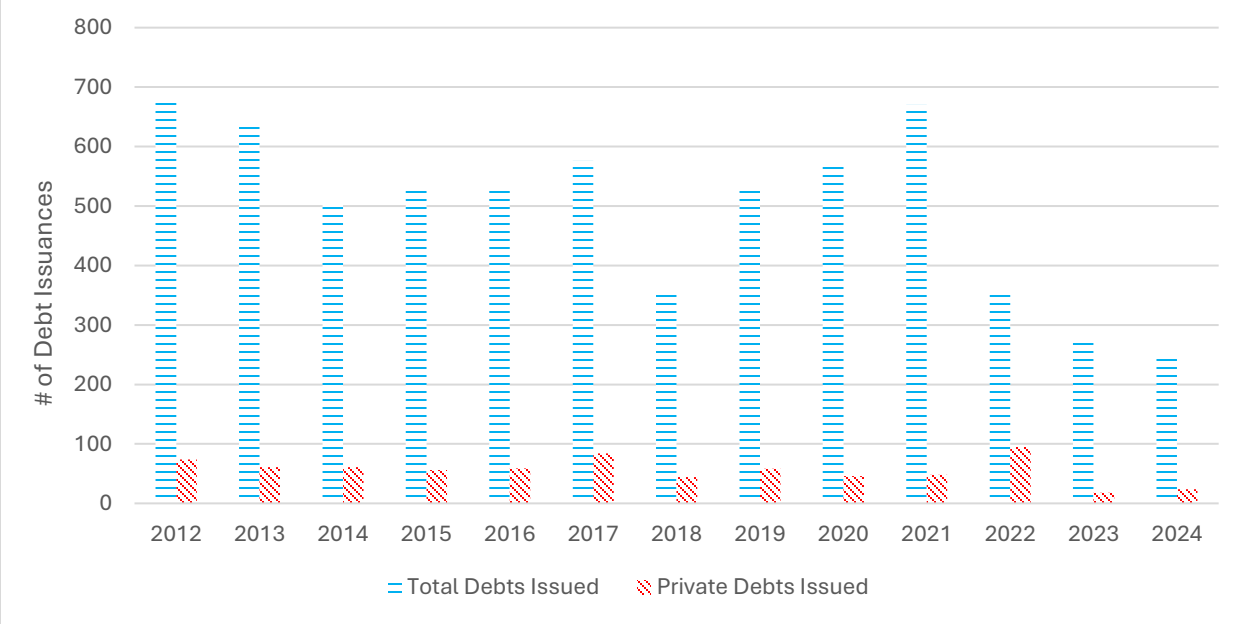
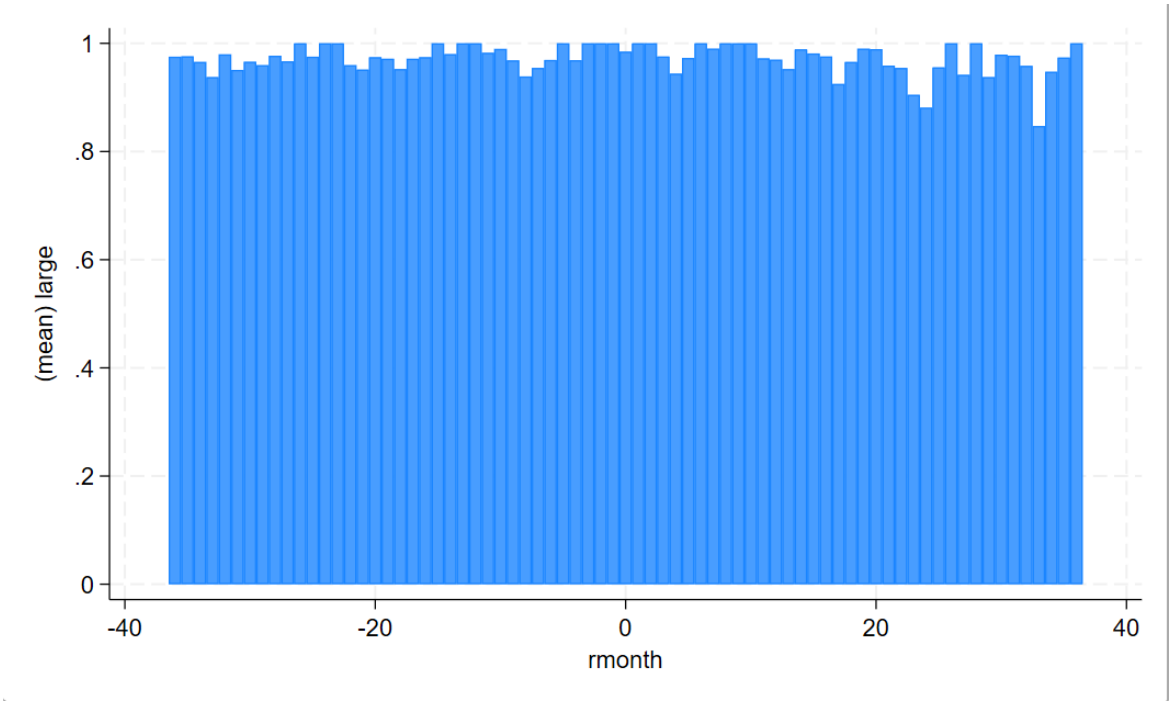


Figure 2. Distribution of Municipal Bonds issued around the Amendments

Panel A: Percentage of Municipal Bonds with Principle Amounts at least $1 Million



Panel B: Number of Monthly New Municipal Bond Issuance

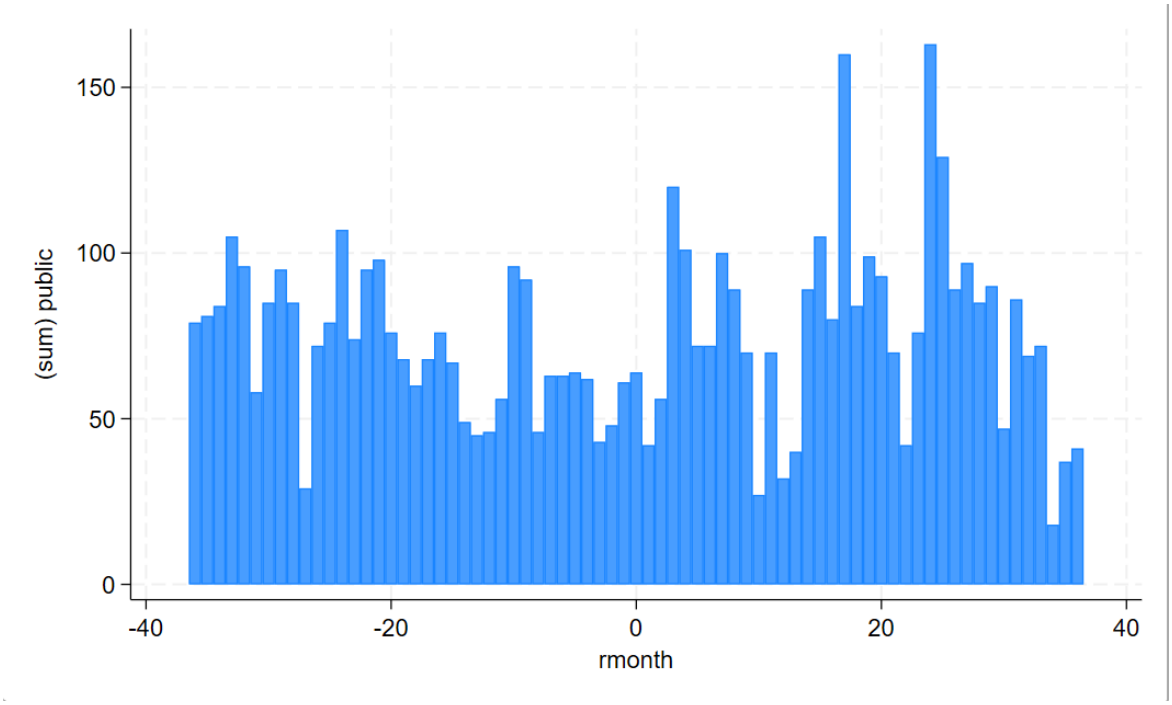
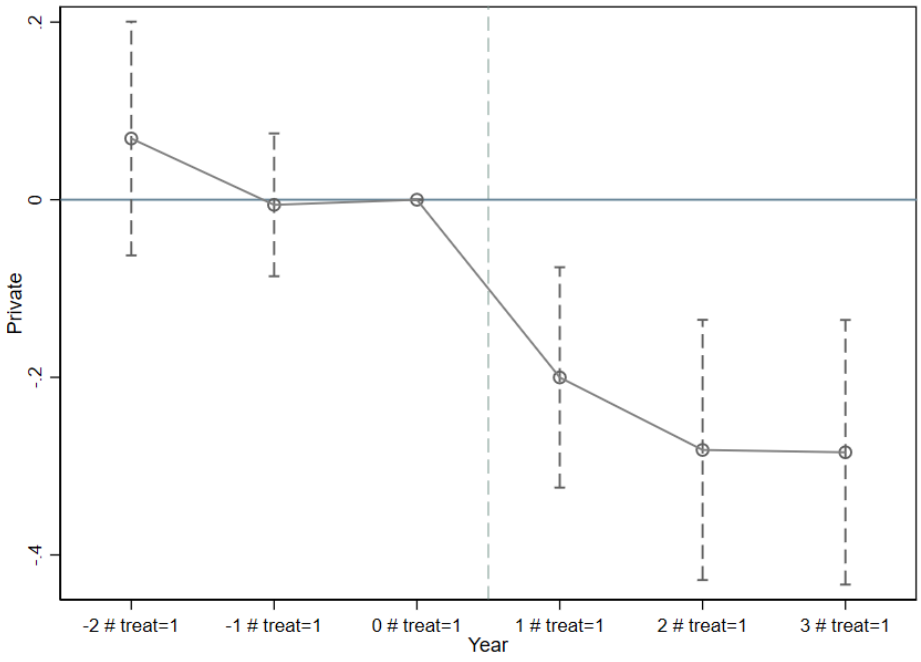


Figure 3. Dynamic Test

Panel A: Full Sample



Panel B: School District Sample

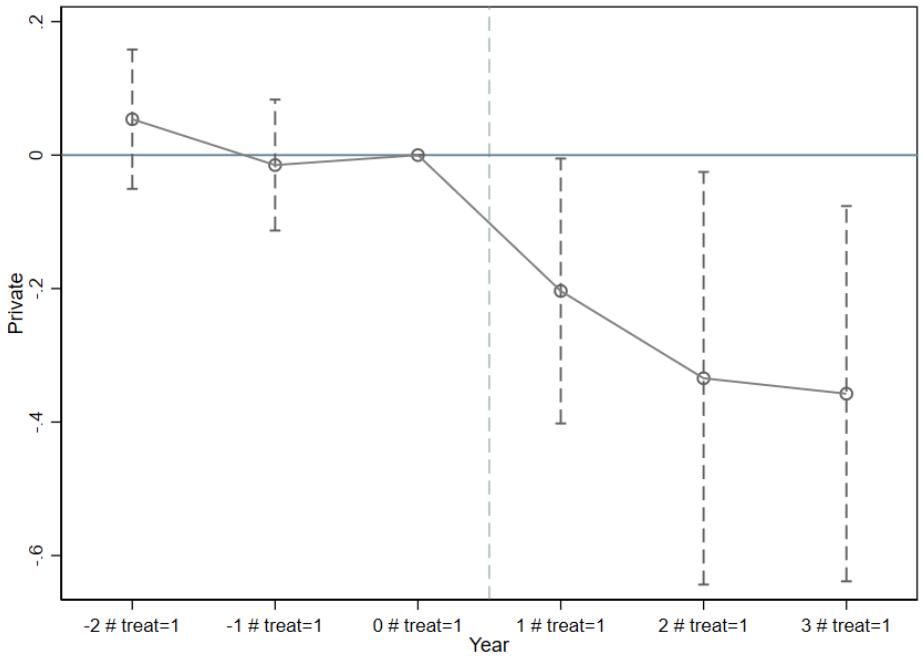


Table 1
Descriptive Statistics

	N	Mean	STD	25th	Median	75th
Dependent variables						
Private $_i$	8,290	0.422	0.494	0.000	0.000	1.000
Independent variables						
Treat_Post $_i$	8,290	0.263	0.441	0.000	0.000	1.000
Num_Private $_i$	8,290	1.322	2.268	0.000	0.000	1.099
Num_Public $_i$	8,290	1.009	0.885	0.000	1.099	1.386
Par_i	8,290	3.804	2.772	1.470	3.714	6.097
$Refunding_i$	8,290	0.403	0.490	0.000	0.000	1.000
$Have\ Rating_i$	8,290	0.225	0.417	0.000	0.000	0.000

Descriptive statistics for the debt-level sample used in the borrowing decision analyses. See Appendix A for variable definitions.

Table 2
Municipalities' Borrowing Decisions After the Amendments

DV = *Private* $_{i,}$	(1)	(2)	(3)	(4)
Treat_Post $_i$	**-0.207*** **	**-0.228*** **	**-0.250*** **	**-0.277*** **
	(-5.25)	**(-5.65)**	**(-2.74)**	**(-2.87)**
Num_Private $_i$		-0.088***		-0.204***
		(-2.66)		(-4.35)
Num_Public $_i$		0.015		0.067*
		(0.50)		(1.95)
Par_i		0.009		0.008
		(1.01)		(0.68)
$Refunding_i$		0.003		-0.022
		(0.14)		(-0.94)
Have Rating$_i$		0.027		-0.003
		(0.60)		(-0.05)
Sample	All Issuers		School Districts	
Municipality × Cohort FE	Y	Y	Y	Y
Year × Cohort FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Observations	8,290	8,290	2,819	2,819
Adj. R^2	0.58	0.58	0.05	0.09

This table presents debt-issuance-level regression results estimating the effects of the Amendments on municipalities' subsequent borrowing decisions. The dependent variable, $Private_i$, equals one if debt *i* is privately placed and zero if it is a public municipal bond. Columns (1) and (2) report results for the full sample of all municipal issuers, while Columns (3) and (4) report results for the subsample of school districts. Variable definitions are provided in Appendix A. Standard errors are clustered by municipality, and *t*-statistics are reported in parentheses below coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 3
Municipalities' Borrowing Decisions After the Amendments – Dynamic Test

Types of Municipalities:	All Issuers	School Districts
DV = *Private* $_{i,}$	(1)	(2)
*-2 Year * Treat*	0.069	0.054
	(1.03)	(1.01)
*-1 Year * Treat*	-0.006	-0.015
	(-0.14)	(-0.30)
+1 Year * Treat	**-0.200*** **	**-0.204** **
	(-3.16)	**(-2.02)**
+2 Year * Treat	**-0.282*** **	**-0.334** **
	(-3.77)	**(-2.12)**
+3 Year * Treat	**-0.284*** **	**-0.357** **
	(-3.75)	**(-2.50)**
Num_Private $_i$	-0.085**	-0.201***
	(-2.51)	(-4.32)
Num_Public $_i$	0.017	0.075**
	(0.61)	(2.17)
Par_i	0.009	0.008
	(0.98)	(0.68)
$Refunding_i$	0.004	-0.023
	(0.17)	(-0.97)
$Have\ Rating_i$	0.016	-0.022
	(0.34)	(-0.52)
Sample	All Issuers	School Districts
Municipality × Cohort FE	Y	Y
Year × Cohort FE	Y	Y
Quarter FE	Y	Y
Observations	8,290	2,819
Adj. R^2	0.58	0.09

This table presents debt-issuance-level regression results that decompose the *Treat_Post* indicator into interactions between treated municipalities and year indicators relative to the treatment year. The dependent variable, $Private_i$, equals one if debt *i* is privately placed and zero if it is a public municipal bond. Column (1) reports results for the full sample of all municipal issuers, and Column (2) reports results for the subsample of school districts. Variable definitions are provided in Appendix A. Standard errors are clustered by municipality, and *t*-statistics are reported in parentheses below coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4
Municipal Borrowing by Households' Online Information Acquisition

Panel A: Above vs. Below Median

Pre-Period Google Search Volume:	Low	High
DV = *Private* $_{i,}$	(1)	(2)
Treat_Post $_{i}$	**-0.078**	**-0.302*****
	(-1.14)	**(-5.40)**
Num_Private $_{i}$	-0.124**	-0.064
	(-2.47)	(-1.45)
Num_Public $_{i}$	-0.025	0.055
	(-0.57)	(1.56)
Par_i	-0.001	0.014
	(-0.10)	(1.05)
$Refunding_i$	0.063*	-0.040
	(1.92)	(-1.54)
$Have\ Rating_i$	0.205***	0.001
	(4.01)	(0.02)
Test of coefficient difference	$\chi 2$ = 6.52** (p-value<0.05)	
Sample	All Issuers	
Muni × Cohort FE	Y	Y
Year × Cohort FE	Y	Y
Quarter FE	Y	Y
Observations	2,935	4,948
Adj. R^2	0.46	0.65

Panel B: Above vs. Below Median – Excluding Los Angeles County

Google Search Volume:	Low	High
DV = *Private* $_{i,}$	(1)	(2)
Treat_Post $_{i}$	**-0.078**	**-0.336*****
	(-1.14)	**(-6.05)**
Num_Private $_{i}$	-0.124**	-0.113***
	(-2.47)	(-3.19)
Num_Public $_{i}$	-0.025	0.046
	(-0.57)	(1.30)
Par_i	-0.001	0.020
	(-0.10)	(1.57)
$Refunding_i$	0.063*	-0.037
	(1.92)	(-1.42)
$Have\ Rating_i$	0.205***	0.003
	(4.01)	(0.05)
Test of coefficient difference	$\chi 2$ = 8.71 ***	

(p-value<0.01)

Sample	All Issuers	
Muni × Cohort FE	Y	Y
Year × Cohort FE	Y	Y
Quarter FE	Y	Y
Observations	2,935	3,795
Adj. R^2	0.46	0.41

This table partitions the sample based on county-level Google search volume for keywords related to municipal bonds. A municipality is classified as having low (high) search volume if its pre-Amendment Google search volume for municipal borrowing–related keywords is below (above) the median. Panel A uses the full regression sample, while Panel B excludes Los Angeles County, which has an exceptionally large number of observations. The dependent variable, $Private_i$, equals one if debt *i* is privately placed and zero if it is publicly issued. Variable definitions are provided in Appendix A. Standard errors are clustered by municipality, and *t*-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5
Municipal Borrowing by Household Financial Literacy, Income, and Age Composition

Panel A: Above vs. Below Median

	Financial Literacy		**Income**		**Age**	
	Low	High	Low	High	Low	High
DV = *Private* $_{i,}$	(1)	(2)	(3)	(4)	(5)	(6)
Treat_Post $_i$	**-0.151*** **	**-0.365*** **	**-0.144*** **	**-0.299*** **	**-0.132** **	**-0.291*** **
	(-3.08)	**(-5.38)**	**(-2.61)**	**(-5.22)**	**(-2.10)**	**(-4.54)**
Num_Private $_i$	-0.071*	-0.149***	-0.074	-0.106**	-0.119**	-0.057
	(-1.81)	(-3.32)	(-1.59)	(-2.25)	(-2.57)	(-1.20)
Num_Public $_i$	-0.015	0.162***	-0.033	0.072	-0.023	0.073*
	(-0.40)	(4.37)	(-0.61)	(1.36)	(-0.58)	(1.95)
Par_i	0.013	-0.010	0.023	-0.004	0.003	0.008
	(1.06)	(-0.64)	(1.58)	(-0.34)	(0.18)	(0.61)
$Refunding_i$	-0.009	0.016	-0.010	0.024	0.029	-0.018
	(-0.34)	(0.46)	(-0.36)	(0.69)	(0.98)	(-0.61)
$Have\ Rating_i$	0.055	-0.011	0.058	0.040	0.215***	-0.006
	(1.13)	(-0.20)	(1.22)	(0.67)	(4.20)	(-0.11)
Test of coefficient difference	**$\chi 2$ = 6.62**** (p-value<0.05)		**$\chi 2$ = 3.83*** (p-value<0.10)		**$\chi 2$ = 3.20*** (p-value<0.10)	
Sample	All Issuers					
Muni × Cohort FE	Y	Y	Y	Y	Y	Y
Year × Cohort FE	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y
Observations	5,476	2,408	4,437	3,447	3,311	4,572
Adj. R^2	0.63	0.46	0.66	0.45	0.44	0.67

Panel B: Above vs. Below Median – Excluding Los Angeles County

	Financial Literacy		**Income**		**Age**	
	Low	High	Low	High	Low	High
DV = *Private* $_{i,}$	(1)	(2)	(3)	(4)	(5)	(6)
Treat_Post $_i$	**-0.151*** **	**-0.365*** **	**-0.154** **	**-0.299*** **	**-0.132** **	**-0.339*** **
	(-2.74)	**(-5.38)**	**(-2.42)**	**(-5.22)**	**(-2.10)**	**(-5.34)**
Num_Private $_i$	-0.102***	-0.149***	-0.118***	-0.106**	-0.119**	-0.111***
	(-2.74)	(-3.32)	(-3.20)	(-2.25)	(-2.57)	(-2.86)
Num_Public $_i$	-0.037	0.162***	-0.064	0.072	-0.023	0.066*
	(-1.10)	(4.37)	(-1.28)	(1.36)	(-0.58)	(1.73)
Par_i	0.018	-0.010	0.031**	-0.004	0.003	0.014
	(1.40)	(-0.64)	(2.29)	(-0.34)	(0.18)	(1.14)
$Refunding_i$	-0.000	0.016	0.003	0.024	0.029	-0.018
	(-0.00)	(0.46)	(0.10)	(0.69)	(0.98)	(-0.63)

Have Rating$_i$	0.072	-0.011	0.079	0.040	0.215***	-0.006
	(1.39)	(-0.20)	(1.61)	(0.67)	(4.20)	(-0.10)
Test of coefficient difference	$\chi 2 = 6.08$** (p-value<0.05)		$\chi 2 = 2.88$* (p-value<0.10)		$\chi 2 = 5.46$** (p-value<0.05)	
Sample	All Issuers					
Muni × Cohort FE	Y	Y	Y	Y	Y	Y
Year × Cohort FE	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y
Observations	4,323	2,408	3,284	3,447	3,311	3,419
Adj. R^2	0.43	0.46	0.42	0.45	0.44	0.42

This table partitions the sample based on financial literacy, income, and age composition, using county-level Census data measured prior to the Amendments. Municipalities are classified as low (high) financial literacy if the county's share of residents with a bachelor's degree or higher in business is below (above) the median. Low (high) income municipalities are those located in counties where the share of families with annual income above $45,000 is below (above) the median. Low (high) working-age composition refers to counties with a below- (above-) median share of residents aged 25 to 60. Panel A uses the full regression sample, and Panel B excludes Los Angeles County, which has an exceptionally large number of observations, to provide more balanced samples. The dependent variable, Private$_i$, equals one if debt i is privately placed and zero if it is a public municipal bond. Variable definitions are provided in Appendix A. Standard errors are clustered by municipality, and t-statistics are reported in parentheses below coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6
Municipal Borrowing by Households' Reluctance Toward Debt

% of "Yes" Votes in Bond Elections:	Low	High	Low	High
DV = *Private* $_{i,}$	(1)	(2)	(1)	(2)
Treat_Post $_i$	**-0.532*** **	**-0.103**	**-0.623*** **	**-0.146**
	(-4.25)	**(-0.59)**	**(-4.41)**	**(-0.89)**
Num_Private $_i$	-0.193**	-0.120	-0.219***	-0.104
	(-2.57)	(-1.63)	(-3.33)	(-1.38)
Num_Public $_i$	0.082	0.071	0.064	0.101
	(1.38)	(1.19)	(1.07)	(1.58)
Par$_i$	0.004	0.017	0.007	0.010
	(0.18)	(0.73)	(0.31)	(0.42)
Refunding$_i$	0.044	-0.045	0.064	-0.059
	(0.98)	(-1.14)	(1.43)	(-1.35)
Have Rating$_i$	0.000	0.015	-0.144**	0.027
	(.)	(0.29)	(-2.42)	(0.43)
Test of coefficient difference	$\chi 2$ = 4.18** (p-value<0.05)		$\chi 2$ = 5.05*** (p-value<0.05)	
Split by	% Yes Vote		% Yes Vote in Excess to Passing Thresholds	
Sample	School Districts			
Muni × Cohort FE	Y	Y	Y	Y
Year × Cohort FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Observations	728	830	685	874
Adj. R^2	0.18	-0.07	0.11	0.07

This table partitions the sample based on the percentage of "Yes" votes in municipal bond elections, measured both in absolute terms and relative to the passing threshold. A municipality is classified as having a low (high) "Yes" vote percentage if its average share of "Yes" votes in bond elections during the two years preceding the SEC Amendments is below (above) the median. The dependent variable, Private$_i$ equals one if debt i is privately placed and zero if it is a public municipal bond. Variable definitions are provided in Appendix A. Standard errors are clustered by municipality, and t-statistics are reported in parentheses below coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7
Heterogeneity by Retail Investors and Credit Ratings

Panel A: Do Retail Investors Play a Role?

% of Trading by Retail Investors:	Low	High	Low	High
DV = *Private* $_{i,}$	(1)	(2)	(3)	(4)
Treat_Post $_i$	**-0.286*** **	**-0.393*** **	**-0.310** **	**-0.355*** **
	(-3.49)	**(-5.21)**	**(-2.33)**	**(-2.76)**
Num_Private $_i$	-0.077*	-0.050	-0.237***	-0.242***
	(-1.77)	(-0.84)	(-4.24)	(-3.04)
Num_Public $_i$	-0.030	0.122**	0.029	0.129**
	(-0.60)	(2.40)	(0.86)	(2.07)
Par$_i$	0.022*	-0.014	0.020	-0.015
	(1.87)	(-0.65)	(1.56)	(-0.57)
Refunding$_i$	-0.014	0.017	-0.002	-0.022
	(-0.51)	(0.47)	(-0.05)	(-0.52)
Have Rating$_i$	0.046	0.000	-0.069*	0.000
	(1.17)	(.)	(-1.71)	(.)
Test of coefficient difference	$\chi 2$ = 0.93 (p-value=0.33)		$\chi 2$ = 0.06 (p-value=0.81)	
Sample	All Issuers		School Districts	
Muni × Cohort FE	Y	Y	Y	Y
Year × Cohort FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Observations	4,075	1,700	1,153	894
Adj. R^2	0.68	0.15	0.03	-0.03

Panel B: Do Changes in Risk Assessment Play a Role?

% of Bonds with Credit Ratings:	Low	High	Low	High
DV = *Private* $_{i,}$	(1)	(2)	(3)	(4)
Treat_Post $_i$	**-0.203*** **	**-0.319*** **	**-0.275***	**-0.327*** **
	(-2.73)	**(-5.44)**	**(-1.70)**	**(-3.42)**
Num_Private $_i$	-0.117*	-0.067	-0.114	-0.239***
	(-1.96)	(-1.63)	(-1.10)	(-5.07)
Num_Public $_i$	0.008	0.017	0.107	0.065**
	(0.20)	(0.42)	(1.10)	(2.06)
Par$_i$	0.010	0.012	0.016	0.009
	(0.60)	(0.99)	(0.49)	(0.74)
Refunding$_i$	-0.002	0.006	-0.046	-0.018
	(-0.05)	(0.24)	(-0.88)	(-0.68)
Have Rating$_i$	-0.108	0.035	0.000	-0.013
	(-0.62)	(0.88)	(.)	(-0.36)
Test of coefficient difference	$\chi 2$ = 1.53 (p-value=0.22)		$\chi 2$ = 0.08 (p-value=0.78)	
Sample	All Issuers		School Districts	
Muni × Cohort FE	Y	Y	Y	Y
Year × Cohort FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
Observations	2,543	5,747	738	2,081
Adj. R^2	0.47	0.61	0.14	0.01

In Panel A, we partition the sample based on the share of municipal bond trading by retail investors. A municipality is classified as having a low (high) retail trading share if the percentage of its municipal bond trading in the two years preceding the SEC Amendments is below (above) the median. In Panel B, we partition the sample based on the share of bonds covered by credit rating agencies. A municipality is classified as having low (high) coverage if the percentage of its municipal bonds rated by credit agencies in the two years before the SEC Amendments is below (above) the median. Columns (1) and (2) in each panel report results for the full sample of municipal issuers, while Columns (3) and (4) present results for the subsample of school districts. Variable definitions are provided in Appendix A. Standard errors are clustered at the municipality level, and *t*-statistics are reported in parentheses below coefficient estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8
Robustness Tests

Panel A: Exploit the Exogenous Variations from Bond Election

Dependent variable = *Private* $_{i,}$	(1)	(2)
Treat_Post $_i$	**-0.277***	**-0.316****
	(-1.89)	**(-2.10)**
Num_Private $_i$		-0.196**
		(-2.26)
Num_Public $_i$		0.085
		(1.04)
Par_i		0.008
		(0.23)
$Refunding_i$		0.026
		(0.46)
$Have\ Rating_i$		-0.054
		(-0.65)
Sample	School Districts	
Muni × Cohort FE	Y	Y
Year × Cohort FE	Y	Y
Quarter FE	Y	Y
Observations	461	461
Adj. R2	0.23	0.25

Panel B: Treatment Cohorts defined at Date Level

Dependent variable = *Private* $_{i,}$	(1)	(2)
Treat_Post $_i$	**-0.312*****	**-0.345****
	(-6.39)	**(-2.45)**
Num_Private $_i$	-0.165***	-0.322***
	(-3.83)	(-3.56)
Num_Public $_i$	0.063	0.168**
	(1.19)	(2.02)
Par_i	-0.003	-0.024
	(-0.21)	(-0.93)
$Refunding_i$	0.058	0.026
	(1.59)	(0.64)
$Have\ Rating_i$	0.071	0.066
	(1.07)	(0.72)
Sample	All Issuers	School Districts
Muni × Cohort FE	Y	Y
Year × Cohort FE	Y	Y
Quarter FE	Y	Y
Observations	476,640	150,815
Adj. R^2	0.69	0.48

Panel C: Probit and Logit Model

Dependent variable = *Private* $_{i,}$	(1)	(2)
Treat_Post $_i$	**-2.715*** **	**-4.551*** **
	(-5.33)	**(-4.40)**
Num_Private $_i$	-0.411**	-0.876***
	(-2.15)	(-3.06)
Num_Public $_i$	0.005	-0.036
	(0.03)	(-0.11)
Par_i	0.158**	0.294**
	(2.15)	(2.28)
$Refunding_i$	-0.011	0.029
	(-0.07)	(0.11)
Have Rating$_i$	-0.084	-0.127
	(-0.28)	(-0.24)
Model	Probit	Logit
Sample	All Issuers	
Muni × Cohort FE	Y	Y
Year × Cohort FE	Y	Y
Quarter FE	Y	Y
Observations	4,528	4,528
Pseudo R^2	0.50	0.51

In Panel A, we exploit exogenous variation in treatment status based on the outcomes of bond elections. In Panel B, we define municipalities treated at different times as separate cohorts. In Panel C, we estimate Probit and Logit regression models as alternative specifications. Variable definitions are provided in Appendix A. Standard errors are clustered at the municipality level, and t-statistics are reported in parentheses below coefficient estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9
External Validity: Comparing California with Other States

Panel A: Inferred Ratio of Private Debt

Dependent variable = *Ratio of Private Debt* $_{j,t,}$	(1)	(2)
Treat $_{j,t}$ ****Post*** $_{j,t}$	**-0.058****	**-0.487*****
	(-2.20)	**(-10.27)**
Sample	School Districts	
Compare	CA vs. Other States	Treated CA vs. Control CA
Municipality FE	Y	Y
Year FE	Y	Y
Observations	11,613	11,613
Adj. R^2	0.29	0.29

Panel B: Subsequent Capital Expenditures

Dependent variable = *Capital expenditures on*	(1) Total	(2) Construction	(3) Instructional equipment
Treat $_{j,t}$ ****Post*** $_{j,t}$	**-0.393*****	**-0.358*****	**0.018*****
	(-4.52)	**(-4.49)**	**(9.40)**
Sample	School Districts		
Municipality FE	Y		
Year FE	Y		
Observations	65,759	65,759	65,759
Adj. R^2	0.24	0.24	0.40

Panel C: Descriptive Statistics

	N	Mean	STD	25th	Median	75th
Inferred Ration of Private Debt						
Ratio of Private Debt $_{j,t}$	11,613	0.619	0.462	0.000	1.000	1.000
Subsequent Real Capital Expenditures						
Total Capital outlay $_{j,t}$	65,759	1.301	2.209	0.178	0.540	1.363
Capital outlay - construction $_{j,t}$	65,759	0.884	1.925	0.000	0.138	0.780
Capital outlay - instructional equipment $_{j,t}$	65,759	0.064	0.117	0.000	0.013	0.074

In Panel A, we conduct a difference-in-differences (DiD) analysis using non-California municipalities as the high-treatment group and California municipalities as the low-treatment group. Column (1) reports the DiD estimates. The dependent variable, *Ratio of Private Deb*$_{j,t}$, is defined as the ratio of new private debt issuance to total new debt issuance for municipality *j* in fiscal year *t*. New private debt issuance is inferred as the difference between total new debt issuance and total public bond issuance. To validate the construction of *Ratio of Private Debt*$_{j,t}$ Column (2) compares the inferred private debt ratio between treated and control municipalities within California. Panel B presents the results of a DiD analysis comparing the capital expenditures of California municipalities with those of municipalities in other states. The unit of observation is a municipality–fiscal year. Panel C reports descriptive statistics for the municipality–fiscal year sample used in Panel B. Variable definitions are provided in Appendix A. Standard errors are clustered at the municipality level, and *t*-statistics are reported in parentheses below coefficient estimates. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.