Sunlight at City Hall: The Effect of Officials' Financial Disclosure on Municipal Bond Markets

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Abstract

We examine how transparency of officials' personal financial interests affects municipal borrowing costs. Exploiting the staggered adoption of online personal financial disclosure (PFD) for public officials across U.S. states, we find municipal bond yields decline by 9–12 basis points following adoption, with results robust across stacked regressions and a border-discontinuity difference-in-differences design. We identify two plausible underlying channels. The governance channel facilitates monitoring, improves operational efficiency, reduces underwriting concentration and rent-seeking, and strengthens citizen approval of governments. The information channel mitigates information frictions, with larger effects in weaker information environments. Our findings suggest that online PFD enhances governance and transparency quality, generating meaningful fiscal savings for local taxpayers without materially increasing aggregate debt burdens.

Keywords: Personal financial disclosure; Municipal bonds; Government efficiency; Rent-seeking; Government approval; Information frictions

JEL Classification: G14, G18, G28, H74, K42

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"Sunlight is said to be the best of disinfectants; electric light the most efficient policeman."

— *Louis D. Brandeis, Other People's Money and How the Bankers Use It (1914)*

1 Introduction

Over the past two decades, the U.S. municipal bond market has remained one of the world's largest fixed-income markets, with outstanding debt measured in trillions of dollars (TPC, 2024; MSRB, 2025). Municipal bonds finance long-lived public investments, including schools, roads, hospitals, and water systems, so that their financing costs directly shape local fiscal capacity (CBPP, 2018; SEC, 2025). Because projects are large and maturities are long, even modest differences in borrowing costs can cumulate into meaningful differences in debt service and, ultimately, the tax burden borne by residents. Yet relative to corporate credit markets, municipal securities are widely viewed as informationally opaque and lightly regulated (Green et al., 2007; Chalmers et al., 2021; Ivanov et al., 2025), which raises a fundamental question: how do investors assess and price municipal risk when governance quality and decision-maker incentives are difficult to observe?

A distinctive feature of municipal finance is that fiscal risk and resource allocation are not determined by balance sheets alone. In many jurisdictions, a small number of elected or appointed officials exercise substantial discretion over choices that matter for debt pricing: the scale of borrowing, project selection, intermediary choice, and the issuance method (GFOA, 2005; Whalley, 2013; Brown, 2017). When officials' personal financial interests and political connections are opaque, discretion can generate large private benefits at public expense. Historical episodes illustrate how nontransparent relationships and misaligned incentives may allow intermediaries and officials to extract rents from taxpayers. Examples include pay-to-play dynamics surrounding negotiated offerings and yield-burning schemes in advance refundings (SEC, 2000; Butler et al., 2009; Garrett, 2024; Brancaccio and Kang, 2025). These episodes underscore a broader point: in municipal markets, perceived credit quality is tightly linked to institutions that shape the incentives and constraints of individual decision makers.

This paper asks whether mandating transparency of officials' personal financial interests affects municipal borrowing costs and market outcomes. We exploit the staggered adoption

across states of online personal financial disclosure (PFD) platforms for public officials. These platforms centralize and publicize officials' disclosure filings, which include asset holdings, income sources, and potential conflicts of interest. They reduce the cost of access for citizens, journalists, counterparties, and investors. By making officials' financial ties more observable, PFD platforms may raise the expected cost of misconduct, improve perceived governance quality, and reduce political and agency risk premia embedded in municipal bond yields.

We assemble bond-level data on primary-market municipal bond issuances from 2004 to 2019 and construct a bond–state–year panel. Our empirical strategy exploits the staggered rollout of online PFD across 28 U.S. states and compares changes in offering yields between treated and control states before and after adoption. Because under staggered adoption conventional two-way fixed effects (TWFE) event-study estimates can be biased in the presence of treatment-effect heterogeneity (Baker et al., 2022), we implement three complementary difference-in-differences (DiD) designs to strengthen identification. First, we estimate a baseline specification with state and year fixed effects and restrict attention to a symmetric event window around adoption (Cornaggia et al., 2022; Scherf, 2024). Second, we implement a stacked regression framework that constructs cohort-specific control groups and absorbs cohort-by-state and cohort-by-year fixed effects (Sun and Abraham, 2021). Third, we employ a border-discontinuity DiD design that exploits the spatial discontinuity in state-level online PFD regimes at state borders (Dube et al., 2010; Cornaggia et al., 2022). Across all specifications, we find that online PFD adoption reduces municipal bond offering yields by approximately 9–12 basis points, representing a 2–3% decline in average borrowing costs.

A key concern is that the timing of platform implementation may not be random. States could launch online PFD in response to fiscal stress or financing needs, factors that may also affect bond yields. To address these endogeneity concerns, we first model adoption timing using a Cox proportional hazards framework. The results indicate that state-level bond-market characteristics, fiscal conditions, and other covariates do not significantly predict adoption timing. We then employ event-study designs to examine pre-treatment trends and trace the dynamic evolution of treatment effects. The event-study estimates reveal no systematic differences in yield trends between treated and control states prior to adoption. Post-adoption, the treatment effect emerges gradually and achieves statistical significance in subsequent periods, consistent with a gradual incorporation of governance information into bond pricing. These patterns support the parallel-trends assumption and mitigate concerns about reverse causality or omitted time-varying confounders.

Beyond addressing endogeneity, we conduct extensive robustness tests to verify the stability of our main findings. First, the results remain robust when we use alternative mea-

sures of financing costs, including credit ratings at issuance and adjusted spreads relative to maturity-matched Treasury yields. Second, the estimated effects are stable, and in some cases larger, when we refine the sample to focus on bonds more directly exposed to the policy, exclude state-government issuances to rule out self-interested adoption motives, or restrict attention to states whose adoption was primarily motivated by modernization efforts rather than governance crises. Third, to address potential correlation in regression residuals along both the state and year dimensions, we report two-way clustered standard errors by issuing state and issuance year, and our inferences remain intact. Finally, placebo tests in both time and space confirm that our estimates are not driven by pre-existing trends, common shocks, or spurious correlations: randomly reassigning adoption years across states or shifting actual adoption years backward in time yields null effects, reinforcing a causal interpretation of the baseline findings.

After establishing the baseline results, we explore plausible underlying mechanisms. Building on two core dimensions of municipal bond pricing, default-related risk and liquidity (Schwert, 2017), we summarize the effect of online PFD on borrowing costs through two complementary yet distinct channels: the governance channel and the information channel.

We first examine the governance channel. Online PFD improves governance quality through three reinforcing mechanisms. First, financial disclosure acts as a complementary monitoring institution that increases corruption referral and prosecution rates (Scherf, 2024). Consistent with this logic, the yield reduction is more pronounced in states with higher pre-reform per-capita direct judiciary spending and stronger public monitoring intensity, while the marginal effect is weaker in states with higher pre-reform corruption conviction rates. Further, from the perspective of debt-management behavior, we find that the adoption of online PFD significantly reduces the likelihood of advance refunding, an inefficient debt operation (Ang et al., 2017), suggesting that governments make more prudent fiscal decisions. We also find that online PFD adoption is associated with improvements in county-year measures of local government efficiency, including increased welfare spending, reduced debt burdens, and leaner government payrolls (La Porta et al., 1999; Gao et al., 2020). Second, by increasing transparency about officials' financial interests and potential ties to market intermediaries involved in the underwriting process, online PFD limits relationship-based rent extraction in municipal bond issuance (Butler et al., 2009; Brancaccio and Kang, 2025). Yield reductions are significantly larger for negotiated offerings than for competitively bid issues and are stronger when issuers do not appoint co-managers. Underwriting concentration declines following online PFD adoption, especially within negotiated offerings, and issuers in high-discretion settings become more likely to select high-reputation underwriters. Third,

greater personal transparency improves citizens' evaluations of state institutions and reduces governance uncertainty (Djankov et al., 2010; Hollyer et al., 2011). Using individual-level survey data from the Cooperative Election Study (CCES), we find that online PFD adoption increases citizens' approval of state legislatures and governments, but does not significantly affect approval of the president, consistent with an institution-specific improvement in perceived accountability rather than a broad shift in overall political attitudes.

We next turn to the information channel. By increasing the accessibility and verifiability of information about public officials' potential conflicts of interest, online PFD may reduce information asymmetries between issuers and investors, lowers information acquisition costs, and compresses the information risk premium (Green et al., 2007; Chalmers et al., 2021). Consistent with this channel, the yield-reducing effect is stronger when disclosure requirements are more comprehensive (e.g., extending to local officials) and in states with weaker pre-reform information environments (lower newspaper circulation or internet access). In contrast, whether a state prepares government financial statements under GAAP does not significantly affect the magnitude of the effect, suggesting that official-level disclosure and entity-level financial reporting operate through largely independent informational dimensions (Baber and Gore, 2008). We further show that online PFD complements rating information at issuance: effects are larger when rating signals are scarcer or rating disagreement is higher. Using MSRB secondary-market trade data, we document that after online PFD implementation, price dispersion and customer markups decline significantly in the post-issuance period, with the reduction more pronounced over the first 60 days than over longer windows. Taken together, these results indicate that online PFD reduces information frictions between issuers and investors and materially lowers secondary-market trading costs in the early period.

We further examine additional economic consequences of online PFD adoption, asking whether the decline in financing costs induces governments to expand borrowing or shift their fiscal stance. In each state's municipal bond market, we find no systematic evidence that lower borrowing costs translate into greater issuance or a deterioration in fiscal burdens over the sample period.

Our paper contributes mainly to three strands of literature. First, we contribute to the literature on municipal bond disclosure and market transparency (Green et al., 2007; Chalmers et al., 2021). Prior work demonstrates that improvements in issuer-level disclosure significantly affect municipal bond pricing and liquidity (Baber et al., 2013; Cuny, 2018; Zhang, 2024). Related work emphasizes that access to government records and disclosure mandates can reduce the cost of government financing (Baber and Gore, 2008). This literature primarily focuses on organizational transparency, that is, what government entities disclose about

their finances, operations, and records. Our paper contributes to this literature by shifting attention to individual-level transparency. We examine how reforms mandating online access to public officials' PFD affect municipal bond market outcomes. Importantly, we emphasize that individual-level and organizational-level information operate through distinct informational channels. While organizational disclosure primarily reveals fiscal capacity and creditworthiness, PFD reveals governance quality, potential conflicts of interest, and the risk of rent extraction. Our empirical findings suggest these two types of information are complements rather than substitutes in the eyes of market participants, as both dimensions are independently priced in municipal bond markets.

Second, our paper contributes to the literature on local media coverage and monitoring capacity. A complementary literature emphasizes the role of local media in shaping the effectiveness of accountability mechanisms and, consequently, the quality of public decision-making (Snyder Jr and Strömberg, 2010; Gentzkow et al., 2011; Gao et al., 2020). Our study contributes to this literature by identifying a novel channel through which information technology enhances monitoring. Online PFD platforms reduce the cost for market participants to access, verify, and interpret information about officials' potential conflicts of interest, and these monitoring benefits are reflected in bond prices. Furthermore, we explore the conditions under which transparency has a significant impact. Specifically, we find that the effects of online PFD are stronger in states with weaker pre-reform information environments and are concentrated among bonds with less rating information available at issuance or greater rating disagreement. This pattern suggests that information modernization efforts can improve the functioning of existing accountability and monitoring institutions by easing informational constraints and enhancing information accessibility and verifiability.

Third, our paper contributes to the literature on corruption and political connections in municipal finance. Research in this area documents that opaque relationships between officials and underwriters can raise municipal borrowing costs through rent extraction, particularly in settings with greater discretion such as negotiated offerings (Butler et al., 2009; Brown, 2017; Brancaccio and Kang, 2025). Rather than measuring corruption within a given institutional environment, we study how an anti-corruption reform alters the institutional environment itself. The staggered adoption of online PFD across states provides a quasi-experimental setting to identify the causal effect of transparency on municipal borrowing costs and market structure. We further connect these findings to the political risk literature by providing evidence that personal financial transparency operates through a citizen-approval mechanism (Djankov et al., 2010; Pástor and Veronesi, 2013; Baker et al., 2016; Kelly et al., 2016; Liu and Shaliastovich, 2022; Ambrose et al., 2025). By reducing perceived

governance uncertainty, online PFD compresses the risk premium investors demand, thereby lowering municipal borrowing costs without relying on direct fiscal policy interventions or inducing a systematic expansion in debt issuance.

2 Institutional Background

2.1 PFD as a Conflict-of-Interest Governance Tool

Within the ethics and anti-corruption governance framework of U.S. states and localities, PFD constitutes a core conflict-of-interest disclosure regime. States typically require a defined set of public officials to file standardized disclosure forms at regular intervals, reporting economic interests and affiliations that may generate conflicts of interest, including major sources of income, assets and liabilities, and outside employment or business ties.[1] Because many state legislatures lack full professionalization, it is common for officials to hold outside jobs (NCSL, 2021), earn non-government income, or maintain ongoing business relationships while in office. PFD therefore serves as critical institutional infrastructure for identifying potential conflicts, tracking changes in officials' interest networks, and strengthening ethics constraints.[2]

Importantly, PFD is not merely an internal compliance record. In many states, PFD filings are treated as public records and are, in principle, accessible to the public. Consequently, PFD can support accountability and enforcement ex post, and it can deter misconduct ex ante by raising reputational and political costs when potential conflicts can be identified and continuously monitored by third parties. For instance, in the 2014 guilty plea of Dale Mallory, then a member of the Ohio House of Representatives, court documents emphasized his "false financial disclosure statement" for failing to report prohibited gratuities, an omission that became central to the ethics prosecution (FBI, 2014). This case illustrates how PFD can serve as both an investigative tool and evidentiary source in corruption prosecutions.

2.2 The Transition from Request-Based to Online Disclosure

The institutional change examined in this paper is not whether PFD filing obligations exist, but rather whether existing filings become practically accessible to the public through a shift in the disclosure channel. Historically, access in many states relied on offline, fragmented,

[1] Appendix Table A1 presents an illustrative example of PFD requirements.
[2] Appendix Figure A1 presents an illustrative disclosure filing.

request-based procedures. Over time, states began making filings publicly available online through official state websites.[3] This distinction matters because online availability does not necessarily create new information; the same filings may already be public in principle. What changes is the dissemination technology and the frictions associated with obtaining and using the information.

Request-based access typically requires submitting formal requests (e.g., via the Freedom of Information Act), waiting for processing, and conducting on-site review or piecemeal acquisition. These steps raise the cost of use and tend to confine access to a small set of highly motivated, well-resourced actors. Online availability, in contrast, lowers acquisition costs, facilitates timely retrieval, enables intertemporal tracking and cross-official comparisons, and supports large-scale compilation and repeated use. For municipal bond market participants, the economic implication is that governance-relevant, non-financial information linked to issuer integrity, corruption risk, and potential conflicts of interest is more likely to enter routine due diligence and risk assessment, rather than relying primarily on sporadic individual requests or third-party curation.

2.3 Staggered Adoption Across U.S. States

The transition to online availability is not a nationally synchronized reform, it unfolds gradually over the sample period (2004-2019).[4] Institutionally, adoption reflects coordination between legislative rule-making and administrative implementation. State legislatures typically define the legal basis and access boundaries through statutes and implementing rules, specifying covered officials, filing requirements, public-access provisions, and privacy exemptions or redaction standards. Administrative agencies charged with overseeing PFD, most commonly state ethics commissions, operationalize these requirements by collecting, reviewing, and archiving filings; processing records for public release; and publishing and maintaining the system to ensure ongoing access.

We define the onset of online disclosure as the first year in which a state makes PFD filings available to the public on an official state website in a manner that allows users to view, download, or search records without submitting case-specific requests. Based on the data compiled by Scherf (2024), by 2019 a total of 35 states had established online PFD systems, among which 17 states disclosed filings for local government officials in addition to

[3] Figure 1 shows the geographic distribution of states that had made PFD filings publicly available online by 2019, which is the end of our sample period.

[4] Figure 2 presents the timeline of the rollout of online public access to PFD filings.

state-level officials.[5] The study further classifies states into "efficiency-motivated adopters" and "corruption-motivated adopters" according to the stated motivations for adoption. The staggered timing of adoption across states provides a quasi-experimental setting to examine whether lowering the frictions associated with acquiring and using conflict-of-interest information is associated with systematic changes in municipal bond yields. To address potential endogeneity arising from contemporaneous anti-corruption policies, we also estimate our specifications on the subsample of "efficiency-motivated adopters" in Section 4.[6]

2.4 Economic Channels: Disclosure Accessibility and Bond Pricing

The theoretical framework linking online PFD adoption to municipal bond yields operates through several interconnected channels. First, online PFD *facilitates monitoring and improves government operational efficiency*. Enhanced public and media access to PFD information strengthens external monitoring, increasing the likelihood that conflicts of interest and potential misconduct are detected and publicized. This heightened scrutiny can deter corrupt behavior *ex ante* and support enforcement activity *ex post* (Ferraz and Finan, 2008; Avis et al., 2018). Empirical evidence from Scherf (2024) demonstrates that online PFD adoption for local officials is associated with increased federal corruption referral rates and higher prosecution rates conditional on referral. Field interviews with 93 federal prosecutors and journalists reveal that online PFD enhances both the investigative value (by providing links to other evidence and third-party organizations) and evidentiary value (by documenting omissions that establish consciousness of guilt) of financial disclosures. Prosecutors emphasized that online systems enable covert investigations by allowing anonymous access to PFD, thereby avoiding premature disclosure or information leakage about ongoing investigations. The resulting improvement in governance quality and reduction in corruption risk may translate into lower perceived default risk and reduced bond yields (Butler et al., 2009; Nakhmurina, 2024).

Second, online PFD may *curb relationship-based underwriting and rent-seeking behavior* in the municipal bond market. When officials' financial interests and business relationships become more transparent and subject to public scrutiny, the scope for extracting private benefits through preferential underwriter selection narrows (Brown, 2017). This can reduce

[5] State-level officials include both elected and appointed officials in state government (e.g., state legislators, governors, and heads of state agencies), whereas local-level officials include both elected and appointed officials at the municipal and county level (e.g., mayors, county commissioners, city council members, and heads of local agencies). Figure A2 illustrates the geographic distribution of disclosure scope across states.

[6] "Efficiency-motivated adopters" implement online PFD primarily to modernize administrative workflows, whereas "corruption-motivated adopters" adopt online PFD as part of broader anti-corruption reform packages. Figure A3 illustrates the geographic distribution of these adopter types.

underwriting concentration and mitigate concerns about conflicts of interest in the bond issuance process, potentially leading to more competitive underwriting fees and improved bond pricing for issuers (Li, 2024).

Third, by enhancing transparency and accountability in government, online PFD may *strengthen citizens' approval in state institutions* (OGE, 2021). Greater public confidence in government integrity can improve the perceived creditworthiness of municipal bond issuers, as investors may view governments with stronger institutional quality and public approval as less likely to default on their obligations (Liu and Shaliastovich, 2022; Ambrose et al., 2025). This channel operates through both direct effects on perceived default risk and indirect effects through improved fiscal discipline and governance practices (Snyder Jr and Strömberg, 2010; Besley and Dray, 2024).

Fourth, online PFD *mitigates information frictions* by eliminating the need for formal information requests and enabling direct, anonymous access to officials' financial disclosures. This reduction in acquisition costs allows investors, rating agencies, and other market participants to incorporate governance risk signals into their bond pricing and investment decisions more efficiently (Gao et al., 2019; Babina et al., 2021). The effect is expected to be particularly pronounced in weaker information environments where alternative sources of governance information are scarce, and for bonds with greater credit-rating disagreement where additional governance signals provide greater marginal value for risk assessment (Schultz, 2012; Chalmers et al., 2021).

Collectively, these channels suggest that online PFD adoption should be associated with lower municipal bond yields, with the magnitude of the effect varying across different bond characteristics, institutional contexts, and information environments. The empirical analysis in this paper tests these predictions and explores the relative importance of each channel in explaining the observed relationship between online PFD adoption and municipal bond pricing.

3 Data and Summary statistics

3.1 Data

We compile state–year policy and covariate data and merge them with bond-level observations by the issuer's state and issuance year to evaluate how state-level online PFD reforms affect municipal issuance costs. We also assemble county-level and individual-level data to conduct mechanism tests and complementary analyses.

Municipal bond characteristics. Our primary issuance data are drawn from the Mergent Municipal Fixed Income Database (Mergent FISD), which provides comprehensive information on municipal bond issuance terms and pricing, including offering yields and related measures of borrowing costs. We begin with the universe of municipal bonds in Mergent and construct our baseline sample over 2004–2019. We start in 2004 because key pricing and contractual fields exhibit substantial missingness in earlier years, which would compromise comparability and the consistency of variable definitions. We end in 2019 for two reasons. First, the state-level policy timing data compiled in Scherf (2024) are recorded through 2019, which facilitates an external validation of our coding of policy adoption dates. Second, the latest online PFD adoption in our sample occurs in 2017, so extending the sample through 2019 ensures at least two years of post-adoption observations for the last-treated states and improves the comparability of dynamic-effect estimates.

We restrict the sample to new-money bonds, exclude bond types with nonstandard structures or pricing measures that are not comparable across issues, and focus on general obligation (GO) bonds issued through conventional municipal financing channels. We concentrate on GO bonds because we do not observe the project-specific characteristics needed to model the credit risk of revenue bonds in a comparable manner (Cornaggia et al., 2022; Schwert, 2017). Geographically, we exclude bonds issued by U.S. territories and further remove bonds issued in states that either made PFD filings publicly available online prior to 2006, which ensures a sufficiently long pre-treatment window, or cannot be cleanly incorporated into the event-study framework for institutional or data-related reasons.[7] The detailed sample selection procedure is reported in Appendix Table A2. In addition, we use municipal bond transaction data from the Municipal Securities Rulemaking Board (MSRB) to construct secondary-market measures.

Underwriter information. To more precisely classify issuers by governmental level and to enrich syndicate information, we incorporate data from SDC Platinum and match it to Mergent at the issuance level. The match is anchored on the nine-digit CUSIP and, when necessary, verified using issuance dates and deal size for consistency. SDC provides finer issuer classifications and government-level identifiers (e.g., state versus local issuers), as well as detailed syndicate structure and underwriter roles (e.g., lead underwriter, co-leads, and syndicate members), which we use to define issuer-level categories and construct underwriter-related variables.

[7]For Maryland, the launch date of online PFD is not precisely verifiable from the Wayback Machine archives. For Georgia, online PFD was later discontinued after it became available, resulting in a nonpersistent policy regime.

Credit ratings. For credit ratings, we use the rating observed at issuance. Our baseline approach relies on ratings recorded in Mergent and cross-checks them against the corresponding rating information in SDC. Beyond rating levels, SDC also reports the number of rating agencies and whether ratings disagree across agencies; we use these fields to construct additional measures in our mechanism analyses. When multiple agencies rate the same bond, we adopt a conservative rule by taking the lowest (worst) rating and map letter grades into a numeric scale for empirical analysis. In rating regressions, we use the underlying rating. In yield-related analyses, we use insured ratings for insured bonds and underlying ratings for uninsured bonds.

County-level fiscal outcomes. To conduct complementary analyses at the county level, we assemble county-level fiscal data from the U.S. Census Bureau's Annual Survey of State and Local Government Finances and county-level employment data from the U.S. Bureau of Labor Statistics (BLS). Using these data, we construct measures of welfare expenditures, debt service, wage expenditures, and employment, and scale them by total county expenditures or population as appropriate (e.g., expenditure shares or per-capita measures). We further include county-level demographic and economic controls, including personal income per capita (in thousands of dollars), population (in logs), and their annual growth rates.

State-level covariates. Additional controls in the baseline regressions are measured at the state–year level and merged to bonds by the issuer's state and issuance year. Following the literature, we control for time-varying state macroeconomic conditions, the information and oversight environment, fiscal and tax conditions, and political and governance institutions (Butler et al., 2009; Gao et al., 2020; Butler and Yi, 2022). Specifically, our economic fundamentals include the state unemployment rate, the natural logarithm of state population, gross state product per capita (in millions of dollars), and personal income per capita (in thousands of dollars), sourced from the Federal Reserve Economic Data (FRED). To capture the information environment and monitoring capacity, we include the internet access rate from the National Telecommunications and Information Administration (NTIA) and judicial expenditures per capita (in thousands of dollars) from U.S. Census Bureau government finance statistics. To capture fiscal and tax conditions, we control for a fiscal health measure defined as state total revenues minus total expenditures scaled by debt interest payments, as well as the top marginal wage tax rate and the long-term capital gains tax rate. These variables are drawn from state budget data and the NBER TAXSIM database. Finally, to proxy for political and governance conditions, we compute public-corruption convictions per million residents using data from the Department of Justice's Public Integrity Section and an

indicator for split government from the National Conference of State Legislatures (NCSL).

3.2 Summary statistics

Table 1 reports summary statistics for the main variables used in the analysis. Our final sample comprises 327,705 bond-issue observations spanning 2004–2019. The average offering yield is 2.86%, with substantial cross-sectional variation (standard deviation of 1.23 percentage points), reflecting differences in credit quality, maturity structure, and market conditions across issuers and time periods. This level and dispersion of offering yields are broadly comparable to those documented in prior empirical studies of the municipal bond market (Painter, 2020), supporting the representativeness and external comparability of our sample. The average coupon rate is 3.50%, slightly higher than the offering yield, consistent with bonds being issued at a premium on average. The median bond has an issuance amount of approximately $480,000 (log scale: 13.08) and a maturity of approximately 10 years (log scale: 2.23). The distribution of issuance amounts is right-skewed, with the 75th percentile ($1.2 million) substantially larger than the median, reflecting the presence of large bond issues by major municipalities.

Approximately 58% of bonds in our sample are callable, allowing issuers to refinance if interest rates decline. About 26% of bonds are insured, and roughly 28% are sold through negotiated offerings rather than competitive bidding. County-level and state-level characteristics exhibit considerable cross-sectional and time-series variation, reflecting heterogeneity in economic conditions, fiscal health, and governance quality across jurisdictions and over time.

4 Online PFD and Municipal Bond Pricing

4.1 Baseline DiD

To identify the causal effect of online PFD adoption on municipal borrowing costs, we exploit the staggered rollout of online PFD across U.S. states and implement a DiD research design. Over the sample period, there are 28 rollout events, providing substantial cross-state and intertemporal variation in policy adoption. For bond b issued in state s in year t, let τ_s denote the adoption year and define event time as $k = t - \tau_s$. Because treatment-effect heterogeneity in staggered-adoption settings can bias conventional two-way fixed effects (TWFE) estimates (Baker et al., 2022), our baseline specification restricts treated observations to a symmetric

event window around adoption.[8]

Following Gao et al. (2020) and Curti et al. (2023), we use the bond offering yield as the dependent variable $Y_{b,s,t}$, which captures the yield compensation demanded by primary-market investors and thereby reflects municipal bond issuance costs. Our key explanatory variable is a state-year indicator $Online\ PFD_{s,t}$ that takes the value of one if state s has implemented the online PFD regime by year t (i.e., $t \geq \tau_s$), and zero otherwise. We estimate the following DiD specification:

$$Y_{b,s,t} = \alpha + \beta\, Online\ PFD_{s,t} + \gamma' BondControls_{b,s,t} + \theta' StateControls_{s,t} + \mu_s + \tau_t + \varepsilon_{b,s,t} \quad (1)$$

$BondControls_{b,s,t}$ is a vector of bond-level controls, including the coupon rate, the natural logarithm of issuance amount, the matched Treasury yield corresponding to the bond's maturity, the natural logarithm of maturity, the natural logarithm of the number of maturities within the same issue, and indicators for callable bonds, insurance enhancement, and negotiated offerings. $StateControls_{s,t}$ is a vector of state-year controls, including the unemployment rate, the natural logarithm of population, gross state product per capita, per capita income, the internet access rate, judicial spending per capita, state fiscal health, the top marginal wage tax rate, the top capital gains tax rate, corruption conviction rate, and an indicator for split government. Bond characteristics are measured at the bond-issue level, whereas state characteristics are measured at the state-year level and merged to bonds by the issuer's state and issuance year. The regression includes state fixed effects μ_s and year fixed effects τ_t to absorb time-invariant state heterogeneity and common national shocks, and standard errors are clustered at the state level. The coefficient β captures the average effect of online PFD adoption on offering yields within the event window.

Table 2 reports baseline estimates. Column (1) uses the symmetric three-year event-time window as the benchmark specification. The estimates indicate that online PFD adoption reduces offering yields by 9.12 basis points, statistically significant at the 1% level. Relative to the sample mean offering yield of approximately 4%, this reduction represents a 2.3% decline in average borrowing costs, translating into substantial fiscal savings for municipal issuers over the life of a bond.

Column (2) redefines the treatment indicator following Scherf (2024), treating the adoption year as part of the control period so that the treatment turns on strictly after adoption. This modification addresses potential contamination from within-year timing and allows for delayed policy effects. Under this definition, the estimated effect is 12.36 basis points, larger

[8] In subsequent robustness checks and channel analyses, we keep this core event window fixed and, in most specifications, focus on a three-year window on each side of adoption.

in magnitude than the baseline and significant at the 1% level. Column (3) expands the event window to five years before and after adoption, excluding the adoption year itself (Cornaggia et al., 2022). The estimated effect is 10.98 basis points, again significant at the 1% level. Across all three specifications, the estimated online PFD effect ranges from 9.1 to 12.4 basis points and remains consistently negative and highly significant.

The control variables behave as expected. Offering yields increase with the matched Treasury yield and maturity, reflecting interest-rate risk and term premia. Callable bonds carry higher yields, compensating investors for reinvestment risk. Larger issues are associated with lower yields, consistent with liquidity premia and economies of scale. Among state-level controls, higher unemployment rates predict higher yields, reflecting fiscal stress, while higher internet access rates and per-capita income are associated with lower yields.

A natural concern is that online PFD adoption may not be random across states. If states with higher financing costs adopt online PFD earlier, the observed yield declines could reflect endogenous policy timing rather than a causal effect. A key identification assumption is therefore that, absent treatment, treated and control states would exhibit parallel trends in borrowing costs. We assess this assumption in two ways.

First, we estimate Cox proportional hazards models for the timing of online PFD adoption. The failure event is defined as the first year in which a state makes PFD filings publicly available online, and the state exits the risk set once the event occurs. As shown in Table 3, none of the state-level municipal bond market characteristics significantly predict adoption timing, nor do other state-level covariates. This evidence suggests that online PFD adoption is unlikely to be systematically driven by pre-existing differences in bond market conditions or financing needs.

Second, we test for parallel pre-trends directly using an event-study specification. We replace the baseline treatment indicator with a set of event-time indicators relative to the adoption year and omit the year immediately prior to adoption as the reference period:

$$\begin{aligned} Y_{b,s,t} = \alpha + \sum_{k \neq -1} \beta_k \cdot 1[t - \tau_s = k] + \gamma' BondControls_{b,s,t} \\ + \theta' StateControls_{s,t} + \mu_s + \tau_t + \varepsilon_{b,s,t} \end{aligned} \tag{2}$$

Figure 3 plots the estimated event-study coefficients along with 95% confidence intervals. The figure reveals three key patterns. First, all pre-treatment coefficients are statistically indistinguishable from zero, indicating no differential trends between treated and control states prior to adoption. Second, the treatment effect emerges in the adoption year but

does not achieve statistical significance until the following year, suggesting that the market's response to enhanced disclosure transparency requires time to fully materialize. Third, the effect persists and strengthens over time, reaching approximately 20 basis points three years post-adoption. The dynamic pattern is consistent with a gradual incorporation of governance information into bond pricing, rather than an immediate market reaction or anticipation effect. Overall, we find no evidence that the parallel-trends assumption is violated, supporting a causal interpretation of the baseline estimates.

4.2 Stacked DiD

To further mitigate potential biases that arise under staggered adoption when treatment effects are heterogeneous, we implement a stacked-regression framework to identify average treatment effects (Sun and Abraham, 2021). Specifically, we construct cohorts based on each state's online PFD rollout year and, for each rollout event, build a "clean" control sample for the corresponding treated cohort. For a state that rolls out online PFD in year τ_s, we define τ_s as the event year and restrict attention to a symmetric three-year event-time window, retaining observations with $k = t - \tau_s \in [-3, 3]$. We then stack the cohort-specific subsamples to form the final estimation sample and estimate the stacked regression model:

$$Y_{b,s,t} = \alpha + \beta\, Online\ PFD_{s,t} + \gamma' BondControls_{b,s,t} + \theta' StateControls_{s,t} + \delta_{s,h} + \lambda_{t,h} + \varepsilon_{b,s,t} \quad (3)$$

To absorb cohort-specific time-varying shocks and differential trends across cohorts, we include state-by-cohort fixed effects $\delta_{s,h}$ and year-by-cohort fixed effects $\lambda_{t,h}$. All other variable definitions and the set of controls follow the baseline specification in equation 1.

Panel A of Table 4 reports estimates from the stacked DiD framework. Column (1) defines cohorts by states' first adoption year, groups states with the same first adoption year into the same cohort, and restricts the control group to never-treated states. The estimated treatment effect is 12.05 basis points, statistically significant at the 1% level and quantitatively similar to the baseline estimate. This consistency suggests that treatment-effect heterogeneity is not a major concern in our setting.

Column (2) expands the control group by additionally including pre-adoption observations from not-yet-treated states, improving sample coverage and precision. The estimated effect is 10.91 basis points, remaining statistically significant at the 1% level. Column (3) further relaxes the cohort definition by treating each state-level online PFD rollout as a distinct cohort event, allowing states with the same adoption year to contribute to different cohorts (Baker et al., 2022). The estimated effect is 12.72 basis points, again significant at

the 1% level. Across all three specifications, the estimated online PFD effect ranges from 10.9 to 12.7 basis points, consistent with the baseline findings.

Beyond estimating average treatment effects, the stacked DiD framework allows us to examine the dynamic evolution of treatment effects and test for parallel pre-trends more rigorously. We replace the treatment indicator in Eq. 3 with a set of event-time indicators relative to the adoption year and omit the year immediately prior to adoption as the reference period:

$$\begin{aligned} Y_{b,s,t} = \alpha + \sum_{k \neq -1} \beta_k \cdot 1[t - \tau_s = k] + \gamma' BondControls_{b,s,t} \\ + \theta' StateControls_{s,t} + \delta_{s,h} + \lambda_{t,h} + \varepsilon_{b,s,t} \end{aligned} \tag{4}$$

Figure 4 plots the estimated coefficients β_k along with 95% confidence intervals. The figure shows no statistically detectable deviations from zero prior to adoption and no evidence of systematic pre-trends. In contrast, post-adoption coefficients are persistently negative, with the effect emerging in the adoption year but achieving statistical significance only in subsequent periods. The effect then persists and remains statistically significant throughout the post-treatment period. The dynamic pattern is consistent with a gradual incorporation of governance information into bond pricing following online PFD adoption, rather than an immediate market reaction or anticipation effect. Overall, we find no evidence that the parallel-trends assumption is violated, supporting the credibility of the baseline estimates.

4.3 Border-Discontinuity DiD

Although online PFD rollouts primarily reflect improvements in disclosure infrastructure and oversight capacity and are unlikely to be directly driven by short-run financing needs or deal-specific issuance conditions, we cannot fully rule out residual unobserved differences between treated and control states or contemporaneous institutional changes that coincide with the rollout. To further mitigate such endogeneity concerns, we draw on the intuition of border-discontinuity designs and re-estimate the baseline relationship in a subsample of counties near state borders (Dube et al., 2010; Cornaggia et al., 2022). This approach exploits the continuity of socioeconomic conditions and trends across geographically adjacent border counties, while leveraging the discontinuity in state-level policy regimes, thereby focusing comparisons on counties that are plausibly similar along both observable and unobservable dimensions but are subject to different state policy environments.

Specifically, we retain only bond observations that can be uniquely matched to an issuing county and then construct cross-state adjacent county pairs on either side of state borders.

If a county borders multiple out-of-state adjacent counties, the bond observations from that county enter the data multiple times, once for each eligible county-pair match.[9] Using this paired-county sample, we estimate a border-discontinuity DiD specification:

$$\begin{aligned} Y_{b,c,s,t} = \alpha + \beta\, Online\ PFD_{s,t} + \gamma' BondControls_{b,c,s,t} + \theta' StateControls_{s,t} \\ + \psi' CountyControls_{c,t} + \mu_c + \xi_p + \tau_t + \varepsilon_{b,c,s,t} \end{aligned} \tag{5}$$

We include a set of county-level covariates ($CountyControls_{c,t}$) to account for residual differences across border counties, including county-level per-capita income and the natural logarithm of county population, and their annual growth rates. We also absorb issuing-county fixed effects μ_c, so identification comes from within-county changes in offering yields before versus after online PFD rollout. More importantly, to directly address potential selection into online PFD adoption at the state level, we additionally absorb border county-pair fixed effects ξ_p, which restrict identification to comparisons between adjacent counties on opposite sides of the state border. These counties share highly similar geography and local socioeconomic environments and face comparable long-run trends and demographic composition, yet operate under different state policy regimes, providing a more credible counterfactual.

Panel B of Table 4 reports estimates from the border-discontinuity DiD design. Column (1) presents the border specification using a symmetric three-year event window, yielding an estimated treatment effect of -9.00 basis points that is statistically significant at the 1% level. The magnitude of this estimate is broadly consistent with those from the baseline regressions and the stacked DiD specifications, indicating that the yield-compressing effect of online PFD remains robust and economically meaningful even under a more stringent identification strategy that absorbs county-pair fixed effects and benchmarks treated counties against their cross-border neighbors, thereby sharply limiting the scope for unobserved state-level confounders.

Columns (2) and (3) probe the sensitivity of our conclusions to alternative choices of treatment timing and event-window definitions. Column (2) redefines the treatment indicator by classifying the adoption year as part of the control period, yielding an estimated effect of -12.46 basis points. Column (3) expands the event window to five years on each side of adoption and excludes the adoption year, yielding an estimate of -11.70 basis points. Both estimates are statistically significant at the 1% level and are broadly consistent in magnitude with the baseline and stacked DiD results, further corroborating that online PFD materially reduces municipal borrowing costs at issuance.

[9] The geographic distribution of the sample counties is illustrated in Appendix Figure A4.

4.4 Robustness Tests

Table 5 evaluates the robustness of the online PFD effect along four dimensions: alternative outcome measures, alternative sample definitions, two-way clustered standard errors by state and year, and placebo tests. Panels A and B use alternative proxies for financing costs by examining the effect of online PFD on (i) the bond's underlying (uninsured) rating at issuance, when available, and (ii) the adjusted spread, constructed following Schwert (2017). The results show that online PFD significantly improves bond ratings and reduces adjusted spreads, consistent with the baseline evidence.

We further mitigate identification concerns by imposing additional sample restrictions. Panel C refines the treatment definition by accounting for policy coverage: in some states, online PFD applies only to state officials, so bonds issued by county and sub-county governments may be affected mainly through indirect spillovers from state-level disclosure, implying a weaker direct effect. After excluding such bonds and focusing on observations more directly exposed to the policy, the estimated online PFD effect remains significantly negative and becomes economically larger than in the baseline specification, as expected.[10]

Panel D exploits the fact that online PFD is implemented at the state-website level and excludes all bonds issued by state governments. Because county and sub-county governments are less able to influence state decisions to launch online disclosure, this restriction helps rule out the alternative explanation that states adopted online PFD primarily for their own financing purposes. The online PFD coefficient remains negative and statistically significant at the 1% level.

Panel E directly addresses concerns about contemporaneous governance shocks. If online PFD adoption coincides with other anti-corruption or governance reforms, the estimated effect may conflate multiple policy changes. We restrict the sample to states whose online PFD adoption is primarily motivated by modernization efforts. The estimated online PFD effect remains significantly negative.

Panel F addresses potential correlation in the regression residuals along both the state and year dimensions. Following Petersen (2008), we report standard errors clustered two ways by state and by year, which allows for arbitrary dependence within either dimension and strengthens the robustness of statistical inference. Under this specification, our main findings remain statistically significant.

Finally, to further alleviate concerns about unobserved confounders, we conduct two

[10] Guided by Table A4, we exclude local-government bond issues from state-years in which local officials' PFD filings were not yet publicly accessible online (in most states that cover local officials, the online availability of local filings coincides with the rollout for state officials).

placebo tests. In the in-space placebo in Panel G, we keep the empirical distribution of adoption years fixed and randomly (without replacement) reassign states to "pseudo" adoption years, thereby breaking the true mapping between states and their actual adoption timing, and re-estimate the model. We repeat this procedure 2,000 times and summarize the resulting placebo estimates in A5. If the baseline findings were driven by unobserved shocks occurring near the adoption years, such shocks would still be present in this framework and could generate spurious effects. Instead, across specifications, the placebo coefficients are close to zero and statistically insignificant.

In the time placebo in Panel H, we shift each state's actual adoption year three years earlier to construct a pseudo event time and re-estimate the model. If the baseline results merely reflect common trending co-movement between adoption timing and yields, significant effects could also appear under the shifted pseudo event. We find no such evidence: the coefficient on the pseudo online PFD treatment is statistically insignificant.

5 Plausible Economic Channels

The preceding analysis shows that the adoption of online PFD significantly lowers municipal borrowing costs. We now articulate the economic mechanisms underlying this effect. Building on Schwert (2017), which decomposes municipal bond spreads into default-related risk and liquidity components, we organize the role of online PFD around two complementary yet distinct channels.

First, the governance channel operates primarily through default-related risk. By facilitating monitoring and raising the costs of misconduct, online PFD improves governmental operational efficiency, curbs rent-seeking in the underwriting process, and strengthens democratic accountability. These improvements jointly reduce credit risk and intermediation frictions, compressing default-related risk premia embedded in yields.

Second, the information channel operates primarily through liquidity. Online PFD lowers information acquisition and processing costs, mitigates information asymmetries and adverse selection between issuers and investors, improves secondary-market trading conditions, and reduces liquidity discounts driven by informational frictions, thereby compressing liquidity risk premia.

In the next section, we examine whether the yield effects of online PFD are stronger for bonds with greater exposure to default-related risk, providing evidence consistent with improved creditworthiness and laying the foundation for the subsequent governance-channel

analyses. We then study the governance channel in Sections 5.2 to 5.4 through the lenses of government efficiency, underwriting integrity, and public approval. Finally, in Section 5.5, we examine whether online PFD affects municipal bond yield spreads by improving the information environment.

5.1 Online PFD and Default Risk

If online PFD improves issuers' creditworthiness by strengthening governance quality and curbing rent extraction, its effects on offering yields should be more pronounced for bonds that are more sensitive to default risk and whose credit pricing depends more heavily on governance quality. To test this prediction, we split the sample along two bond characteristics closely related to exposure to default-related uncertainty (Cheng, 2021): maturity length and enhanced credit quality.

Panel A of Table 6 reports results from estimating our baseline specification separately for long-maturity versus short-maturity bonds and for bonds with high versus low enhanced ratings. We define long maturity as bonds with maturities exceeding 10 years, and high enhanced rating as bonds with enhanced ratings of AA or above. The intuition is straightforward: longer-maturity bonds expose investors to governance risk over extended horizons, while bonds with weaker credit enhancement rely more heavily on the underlying issuer's governance quality.

The results strongly support a risk-reduction interpretation. Column (1) shows that online PFD reduces offering yields by 7.13 basis points for long-maturity bonds, compared to 5.40 basis points for short-maturity bonds in column (2); the difference of 1.73 basis points is statistically significant at the 5% level. This pattern is consistent with online PFD having larger effects when investors face greater uncertainty about future governance quality and official behavior.

The contrast is even more striking when we split by enhanced rating. Column (3) shows that for bonds with high enhanced ratings (AA or above), the online PFD effect is economically negligible and statistically insignificant (0.44 basis points). In contrast, column (4) documents a large and highly significant effect of 13.49 basis points for bonds with low enhanced ratings. The difference of 13.93 basis points is significant at the 1% level. This finding indicates that when bonds benefit from strong external credit support, such as insurance or state guarantees, that effectively insulates investors from issuer-level governance risk, online PFD has little marginal impact on pricing. Conversely, when credit quality depends more directly on the issuer's own fiscal management and decision-making, transparency about

officials' personal financial interests becomes a first-order determinant of borrowing costs.

Taken together, these results suggest that online PFD does not compress risk premia uniformly across bonds. Instead, the yield reduction is concentrated in settings where default risk is priced more heavily through governance quality and official integrity. This pattern is consistent with the institutional role of online PFD as a governance monitoring device, thereby supporting a governance-channel interpretation: by strengthening accountability, mitigating agency problems, and reducing political risk, online PFD improves perceived issuer creditworthiness, rather than mechanically repricing unchanged fundamentals.

5.2 Governance Channel: Government Efficiency

Municipal borrowing costs are closely tied to the quality of local governance (Butler et al., 2009). When public officials exercise discretion over fiscal decisions, including project selection, budget allocation, and debt issuance, without adequate oversight, agency problems can lead to inefficient resource allocation, excessive spending, and elevated default risk (Brown, 2017). Online PFD operates as a monitoring institution that may mitigate these governance frictions through two pathways. First, by making potential conflicts of interest transparent and verifiable, online PFD raises the expected cost of misconduct ex ante (Ferraz and Finan, 2008), deterring corruption and self-dealing before they occur. Second, by supplying actionable information to oversight bodies and the public, online PFD facilitates the detection, referral, and prosecution of malfeasance ex post, thereby strengthening accountability. If these disciplinary mechanisms are effective, local governments should exhibit more prudent fiscal behavior, more standardized decision-making processes, and ultimately lower governance-related risk premia in bond pricing.

We test this mechanism through three sets of empirical implications. First, we examine how the strength of online PFD varies with pre-existing monitoring and enforcement capacity. If online PFD operates primarily by improving the effectiveness of oversight and prosecution, its marginal effect should be more pronounced in jurisdictions with stronger institutional capacity to convert disclosed information into meaningful accountability outcomes. Second, we investigate whether online PFD curbs local governments' use of advance refunding, providing evidence on its implications for debt-management efficiency. Third, we evaluate whether the adoption of online PFD is followed by measurable improvements in government efficiency, as reflected in county-level fiscal outcomes.

Table 7 reports the results. Panel A implements cross-sectional tests based on pre-reform oversight and enforcement conditions, using the same controls and fixed effects as in

the baseline specification. Schultz (2012) documents that the U.S. municipal bond market is segmented into 50 largely separate submarkets that broadly align with state boundaries. Motivated by this institutional feature, we conduct our tests at the state level. We interact the online PFD indicator with three proxies for institutional capacity: per-capita judiciary spending, public monitoring intensity (following Campante and Do 2014), and corruption conviction rate. In each case, we split states at the median of the pre-reform measure and label them as "High" or "Low."

The results reveal a consistent pattern: online PFD is more effective at reducing borrowing costs when monitoring and enforcement institutions are stronger. Column (1) shows that the yield-reducing effect of online PFD is 12.0 basis points in states with above-median judiciary spending, but this effect is significantly attenuated (by 7.9 basis points) in states with below-median judiciary spending, where the interaction term is positive and statistically significant at the 10% level. This pattern suggests that limited judicial capacity weakens the institutional ability to process disclosed information and act on it, thereby attenuating the extent to which online PFD converts transparency into accountability. Column (2) shows a similar pattern using public monitoring intensity: the baseline effect is 12.2 basis points in high-monitoring states, but the interaction with low monitoring is positive and significant at the 5% level, indicating an 11.0 basis point attenuation. These findings are consistent with online PFD serving as an input into a broader accountability system: disclosure alone is insufficient without the institutional capacity to investigate and sanction misconduct (Avis et al., 2018).

Column (3) conducts the test using the pre-reform corruption conviction rate, which captures both enforcement capacity and the severity of pre-existing corruption problems. The interaction term is positive and marginally significant at the 10% level, suggesting that the yield-reducing effect is somewhat weaker in high-conviction states. We interpret this pattern as reflecting diminishing returns: in jurisdictions where enforcement is already active and corruption cases are regularly prosecuted, the marginal informational contribution of online PFD may be smaller, as oversight bodies already possess effective tools for detecting and advancing cases. In contrast, when convictions are rare, either because corruption is less prevalent or because enforcement is weaker, online PFD may provide a more substantial boost to accountability by making previously hidden conflicts of interest visible and actionable. Taken together, the evidence in Panel A indicates that online PFD does not operate in isolation. Its effectiveness depends critically on the presence of institutions that can convert transparency into accountability. When monitoring capacity is weak or enforcement infrastructure is underdeveloped, disclosure has limited bite; when these institutions are stronger,

online PFD serves as a force multiplier, enhancing the efficiency of oversight and prosecution.

Panel B examines whether online PFD curbs local governments' use of advance refunding. Advance refunding refers to the practice of refinancing outstanding debt by issuing new bonds to retire a callable bond prior to its first call date, effectively achieving an early redemption before the contractual call provision becomes exercisable. Prior research shows that such transactions often entail sizable efficiency losses and thus provide an observable proxy for inefficient debt management (Ang et al., 2017; Gao et al., 2020). For example, roughly 85% of advance refundings occur when the net present value is negative, implying aggregate losses of about $15 billion over 1995–2013; fiscal constraints and short-term cash-flow pressures may induce governments to engage in advance refunding even under unfavorable terms. If disclosure improves government performance by strengthening monitoring and accountability, we expect the likelihood of advance refunding to decline following the adoption of Online PFD.

Following Ang et al. (2017) and Gao et al. (2020), we construct a bond–year panel from the sample of callable bonds in the database. For each bond, we generate annual observations beginning in the issuance year and terminate the series under one of two conditions: if the bond is advance refunded, the panel ends in the year of the transaction; otherwise, it ends in the year of the first call date. Based on this risk-set construction, we define an indicator $\mathbf{1}^{AR}_{b,s,t}$ that equals one if and only if the bond is advance refunded in a given bond–year and zero otherwise; for bonds that are never advance refunded, $\mathbf{1}^{AR}_{b,s,t}$ equals zero in all years of the series. We then include the standard set of controls used in the prior literature and estimate binary-response models under alternative distributional assumptions for the link function $F(\cdot)$. Specifically, we define advance refunding using two alternative thresholds relative to the first call date: at least 90 days ($>$ 90 Days) or at least one year ($>$ 1 Year) prior, and we estimate logit and probit specifications under each definition. The model is summarized in Eq. 6.

$$\begin{aligned}\Pr\left(\mathbf{1}^{AR}_{b,s,t} \mid \cdot\right) = F\Big(&\alpha + \beta\, Online\ PFD_{s,t} + \theta' StateControls_{s,t} \\ &+ \eta' Time\ to\ First\ Call_{b,s,t} + \zeta' Time\ to\ Maturity_{b,s,t} + \tau_t\Big)\end{aligned} \tag{6}$$

The results show that online PFD adoption significantly reduces the likelihood of advance refunding. The coefficient on Online PFD is negative and statistically significant at the 1% level under both event thresholds and both logit and probit specifications, with highly comparable magnitudes. Economically, the estimates imply that, after PFD filings become available online, local governments are less likely to engage in advance refunding, a financing

practice that can be costly and may generate negative net present value. For example, in Column (1), the model-implied predicted probability of advance refunding declines from 0.1427% to 0.0905%, a 36.60% reduction relative to the baseline. These results are consistent with a monitoring and accountability channel in which greater transparency makes officials' potential conflicts of interest and fiscal decisions more verifiable to oversight bodies and the public, thereby strengthening discipline and reducing opportunistic or inefficient debt-management behavior.

Panel C turns to local fiscal outcomes and examines whether government efficiency improves following the adoption of Online PFD. Following established approaches in the previous literature (La Porta et al., 1999; Gao et al., 2020), we construct four county-year measures of government efficiency and estimate the effect of online PFD on these outcomes, augmenting the baseline state-level controls with county-level characteristics including per-capita personal income, logarithm of population, and annual growth rates of income and population. The four measures capture different dimensions of fiscal restraint and allocative efficiency. Welfare is the share of welfare-related spending in total government expenditure, reflecting the government's prioritization of social safety net programs. Debt is government debt outstanding scaled by county GDP, measuring the debt burden and fiscal sustainability. Wage is the share of county government payroll in total county payroll, capturing the relative size of the public-sector wage bill. Employment is county government employment scaled by county population, measuring the size of the public workforce.

The estimates indicate that online PFD adoption is associated with significant improvements across all four dimensions. Welfare spending increases by 0.44 percentage points (significant at the 1% level), suggesting that governments allocate a larger share of resources toward social programs, consistent with reduced capture by narrow interest groups and improved responsiveness to citizen needs. Simultaneously, Debt declines by 0.17 percentage points (significant at the 1% level), Wage falls by 0.46 percentage points (significant at the 5% level), and Employment decreases by 0.16 percentage points (significant at the 5% level). These patterns indicate that local governments become more restrained in debt accumulation, personnel spending, and organizational expansion following online PFD adoption. The simultaneous increase in welfare spending and decline in debt and wage costs suggests a reallocation of resources away from potentially inefficient or patronage-driven expenditures toward more productive uses, consistent with improved governance quality and reduced rent extraction.

How do these efficiency gains translate into lower borrowing costs? We interpret the results through the lens of fiscal risk and governance uncertainty. When governments exhibit

more prudent debt management, leaner bureaucracies, and stronger allocative performance, they signal lower default risk and reduced scope for fiscal mismanagement. Bond investors, observing these improvements either directly through fiscal data or indirectly through improved credit assessments, should demand lower risk compensation (Schwert, 2017; Nakhmurina, 2024). The evidence in Panel B and Panel C thus provides a direct link between online PFD-induced governance improvements and the yield reductions documented in the baseline results. In sum, Table 7 supports the government efficiency mechanism through three sets of findings. First, online PFD is more effective at reducing borrowing costs when it operates within a stronger institutional environment for monitoring and enforcement, consistent with disclosure serving as a key input into a broader accountability system. Second, following online PFD adoption, the likelihood of advance refunding in the municipal bond market, a practice indicative of inefficient debt management, declines significantly, suggesting that the reform promotes more disciplined and prudent fiscal decision-making. Third, online PFD adoption is followed by measurable improvements in county-level fiscal outcomes, including increased welfare spending, reduced debt burdens, and leaner government payrolls. Together, these patterns indicate that online PFD lowers municipal borrowing costs by improving government efficiency, reducing governance uncertainty, and compressing the fiscal risk premium embedded in bond yields.

5.3 Governance Channel: Underwriting Integrity

Municipal borrowing costs depend critically on whether the underwriting process is disciplined and transparent (Brown, 2017; Ivanov et al., 2025). In practice, the selection of municipal underwriters often involves substantial discretion, creating opportunities for conflicts of interest and rent extraction. Governance risks in underwriting have well-known institutional manifestations. Under pay-to-play arrangements, financial intermediaries may exchange political contributions, side payments, or relationship maintenance for underwriting appointments and more favorable contractual terms (Butler et al., 2009). Similarly, yield burning, a practice in which underwriters overcharge issuers for securities used in advance refunding transactions, can distort pricing and shift rents through seemingly technical steps in the issuance process, leaving issuers bearing higher borrowing costs within an ostensibly compliant transaction structure (SEC, 2017). A common feature of these practices is their reliance on opaque interactions between public officials and financial intermediaries and limited ex post accountability. By making officials' financial ties and potential conflicts of interest more transparent and verifiable, online PFD increases the likelihood that relationship-driven allocations are detected and continuously scrutinized by external stakeholders, thereby rais-

ing reputational and accountability costs and compressing the scope for rent extraction in underwriting decisions.

We test this mechanism from three angles. First, we examine whether the yield-reducing effect of online PFD is concentrated in settings where issuers exercise greater discretion over underwriter selection, as these environments provide more scope for relationship-based allocations. Second, we evaluate whether online PFD adoption is followed by changes in underwriting market structure, specifically whether concentration declines as issuers' reliance on a narrow set of intermediaries weakens. Third, we investigate whether online PFD shifts underwriter matching away from relationship-driven selection toward reputation- and capability-based selection, as reflected in the propensity to appoint high-reputation lead underwriters. Table 8 reports the results.

Panel A exploits variation in discretion in the issuance process and tests whether online PFD has stronger effects precisely where rent seeking is more likely. We estimate the baseline specification separately by issuance method and by syndicate structure. Negotiated offerings involve direct bilateral negotiations between issuers and underwriters, allowing more flexible pricing and greater scope for relationship considerations, whereas competitive offerings require underwriters to submit sealed bids, limiting discretion (Brancaccio and Kang, 2025). Similarly, the presence of a co-manager introduces additional oversight and external checks within the underwriting syndicate, potentially constraining the lead underwriter's ability to extract rents (MSRB, 2024). The results show that the yield-reducing effect of online PFD is concentrated in negotiated offerings and in issues without a co-manager, with cross-group differences significant at the 1% level. Specifically, online PFD reduces offering yields by 14.5 basis points in negotiated offerings but only 2.4 basis points in competitive offerings; the difference of 12.1 basis points is economically large and statistically significant. Similarly, the effect is 10.7 basis points for issues without a co-manager but only 4.2 basis points for issues with a co-manager. These patterns indicate that in high-discretion environments, where relationship-based allocations and rent extraction are more feasible, online PFD is more likely to discipline underwriting decisions by increasing the visibility and accountability of officials' relevant ties, thereby lowering borrowing costs.

Panel B examines whether online PFD reshapes underwriting competition through changes in market structure. If online PFD raises the institutional cost of relationship-based underwriting and reduces issuers' reliance on a small set of intermediaries, underwriting concentration should decline at the state-year level. Lower concentration in turn implies stronger competitive pressure and lower financing costs, as a more fragmented market reduces individual underwriters' pricing power (Li, 2024). Accordingly, we aggregate bond-level under-

writing shares to the state-year level and construct the underwriter Herfindahl-Hirschman Index (HHI) as the dependent variable. We estimate the effect of online PFD in both the full sample and the negotiated subsample, controlling for state characteristics as well as state-year underwriting market size (measured by total issuance volume) and the number of active underwriters, so as to separate concentration changes from market expansion or entry. The estimates indicate a significant decline in underwriting concentration following online PFD adoption. In the full sample, the HHI falls by 2.27 percentage points (significant at the 5% level), representing approximately 15% of a standard deviation. The effect is larger in the negotiated subsample, where the HHI declines by 2.85 percentage points (significant at the 10% level), consistent with negotiated offerings being more susceptible to relationship-driven concentration. These results suggest that online PFD adoption is followed by a structural shift toward more competitive underwriting markets, as issuers diversify their underwriter relationships and reduce reliance on a narrow set of intermediaries.

Panel C investigates the underwriter-quality and matching channel. While relationship-driven allocations are constrained and concentration falls, we ask whether issuers become more likely to select high-reputation lead underwriters. Following Butler et al. (2009), we measure underwriter reputation by the underwriter's national market share in the issuance year, which captures both scale and track record. We rank lead underwriters nationwide within each issuance year and define a binary indicator $\mathbf{1}^{HR}_{b,s,t}$ that equals one if the lead underwriter is among the top ten underwriters nationally in that year and zero otherwise. This cutoff is chosen to identify underwriters with substantial national presence and established reputations, consistent with prior research (GAO, 2012; Brancaccio and Kang, 2025). Using this indicator as the dependent variable and conditioning on bond-level and state-level controls, we estimate the following binary-response model separately by issuance method. Under alternative distributional assumptions for the link function $F(\cdot)$, we implement Logit and Probit specifications, respectively, to estimate Eq. 7 and capture differences in issuer discretion in underwriter selection. In competitive offerings, underwriter selection is largely determined by bidding rules and price competition rather than issuer discretion, providing a natural low-discretion benchmark.

$$\begin{aligned}\Pr\left(\mathbf{1}^{HR}_{b,s,t} \mid \cdot\right) = F\Big(&\alpha + \beta\, Online\ PFD_{s,t} + \gamma' BondControls_{b,s,t} \\ &+ \theta' StateControls_{s,t} + \mu_s + \tau_t\Big)\end{aligned} \tag{7}$$

The results show that online PFD significantly increases the likelihood of selecting a high-reputation lead underwriter in negotiated offerings, whereas the effect is not statistically significant in competitively bid issues. Specifically, in the negotiated subsample, the

Logit coefficient is 0.51 and is significant at the 5% level. Based on model-implied predicted probabilities (conditioning on bond- and state-level controls and absorbing state and year fixed effects), making PFD filings available online raises the predicted probability of appointing a high-reputation lead underwriter from 45.9% to 55.7%, an increase of more than 20% relative to the baseline. In contrast, the corresponding coefficient in the competitive subsample is -0.08 and is statistically indistinguishable from zero. The cross-subsample difference in coefficients is significant at the 1% level, indicating that the effect of online PFD is concentrated in negotiated offerings where issuers have greater discretion in underwriter selection, consistent with our hypothesis. Probit estimates yield qualitatively similar conclusions in terms of sign and statistical significance.

Taken together, Table 8 documents three mutually reinforcing empirical patterns consistent with the underwriting-integrity mechanism. The issuance-cost effect is strongest in settings with greater discretion and greater scope for relationship-based deals, specifically in negotiated offerings and in issues without co-managers. This yield reduction is accompanied by declining state-year underwriting concentration, indicating a structural shift toward more competitive markets, and by an increased propensity to select high-reputation lead underwriters in discretionary settings, indicating improved matching quality. These changes point to a reduction in hidden issuance frictions and rent extraction, which compresses the risk premia embedded in bond pricing and ultimately lowers municipal borrowing costs.

5.4 Governance Channel: Public Approval

Municipal bond investors face not only credit risk but also political and governance risk (Babina et al., 2021; Nakhmurina, 2024). When public officials have discretion over fiscal decisions, investors must assess whether resources will be allocated prudently or diverted through opaque channels. A key determinant of this governance risk is the strength of democratic accountability: when citizens are better informed about officials' potential conflicts of interest and can more effectively monitor and sanction misconduct, officials face stronger incentives to act in the public interest, and the scope for opportunistic behavior narrows (Snyder Jr and Strömberg, 2010; Besley and Dray, 2024). In this setting, public approval in state institutions serves as a summary measure of perceived governance quality and the credibility of accountability mechanisms. If online PFD strengthens public approval by making conflicts of interest more transparent and verifiable, investors should interpret this as a reduction in political and governance uncertainty, leading them to demand lower risk compensation.

We test this mechanism using individual-level survey data from the Cooperative Congressional Election Study (CCES), which provides annual measures of citizens' approval of state-level political institutions (Kuriwaki, 2025). Our primary outcomes are respondents' approval of the state legislature and the governor, and we additionally examine approval of the president as a placebo outcome. All approval measures are constructed from a five-point scale and coded to range from -2 (strongly disapprove) to $+2$ (strongly approve). We focus on the state legislature because it is most directly affected by online PFD: legislators are typically subject to disclosure requirements, and their central roles in budget authorization and oversight of the executive branch make conflicts of interest particularly salient. We also analyze governor approval, which is widely used as a proxy for citizens' assessments of state government performance and legitimacy (Cohen and King, 2004; Brown, 2010). Finally, presidential approval serves to test whether online PFD induces a broad-based shift in political attitudes: because the online PFD disclosures do not pertain to the president, a null effect on this outcome would be more consistent with the interpretation that the policy's impact is concentrated in evaluations of state institutions and their officials.

Table 9 reports the results. The regressions control for state-level characteristics and individual-level covariates, including gender, race, education, age, political interest, and partisan identity, and absorb state and year fixed effects. Standard errors are clustered at the state level. In Panels A and B, column (1) reports OLS estimates indicating that online PFD increases approval of the state legislature and of the state government by 0.19 and 0.17 points, respectively, on the five-point scale.

We further dichotomize the ordered outcome and construct a binary dependent variable $\mathbf{1}^{HA}_{i,s,t}$ that equals one if approval exceeds zero and zero otherwise. We then control for respondents' individual characteristics, including age, sex, race, educational attainment, political interest, and partisan identity, and estimate Logit and Probit models under alternative distributional assumptions for the link function $F(\cdot)$, as specified in Eq. 8.

$$\begin{aligned}\Pr\left(\mathbf{1}^{HA}_{i,s,t} \mid \cdot\right) = F\Big(&\alpha + \beta\, Online\ PFD_{s,t} + \kappa' IndividualControls_{i,s,t} \\ &+ \theta' StateControls_{s,t} + \mu_s + \tau_t\Big)\end{aligned} \tag{8}$$

Columns (2) and (3) in each panel report Logit and Probit estimates, respectively, and both indicate that online PFD significantly increases the probability that respondents express favorable evaluations of the state legislature and the state government. Specifically, the Logit coefficients are 0.28 and 0.24. The corresponding average predicted probabilities (margins) show that, conditional on state- and individual-level controls and absorbing state and year

fixed effects, online PFD raises approval of the state legislature from 35.62% to 41.93% (a 17.72% increase relative to the baseline) and raises approval of the state government from 45.04% to 50.82% (a 12.83% increase relative to the baseline). Probit estimates yield qualitatively similar conclusions in terms of sign and statistical significance.

Importantly, the placebo test indicates that the estimated effect reflects enhanced approval in state institutions rather than a broad-based shift in political mood. Panel C uses respondents' approval of the president as the dependent variable; under the same specification, the PFD coefficient is economically small and statistically insignificant. This contrast suggests that the attitudinal improvement is not a uniform increase across all political objects, but is instead concentrated in evaluations of the state legislature and state government as key state-level institutional actors whose officials are directly subject to the disclosure requirements.

How does an increase in public approval translate into lower borrowing costs? We interpret these findings through the lens of political and governance risk. Owing to cross-state differences in tax treatment, the municipal bond market is highly segmented, and bondholders are predominantly in-state residents (Schultz, 2012). When citizens express higher approval of state institutions, they are more likely to perceive governance as effective and accountable, thereby reducing the perceived likelihood of fiscal mismanagement or opportunistic resource diversion (Ambrose et al., 2025). If bond investors incorporate public sentiment as a signal of governance quality, either directly through survey measures and media coverage or indirectly through the disciplinary effect of voter monitoring on officials' behavior, then stronger public approval should compress the political risk premium embedded in municipal bond yields (Besley and Prat, 2006; Ferraz and Finan, 2008; Pástor and Veronesi, 2013; Liu and Shaliastovich, 2022). Consistent with this interpretation, the evidence in Table 9 suggests that online PFD lowers municipal borrowing costs in part by strengthening public approval of state institutions, reducing political and governance uncertainty, and thereby compressing investors' required compensation for political risk.

5.5 Information Channel

The U.S. municipal bond market has long been viewed as informationally opaque, making it costly for investors to assess local governments' credit risk and governance quality; as a result, primary-market pricing often embeds a sizable information risk premium (Schultz, 2012; Schwert, 2017). This concern is particularly salient when fiscal decisions hinge on the discretion of a small set of key officials and when potential conflicts of interest can shape

resource allocation and risk taking. In such settings, investors' central uncertainty is not only whether a government can repay, but also whether it may allocate resources in nontransparent ways, accumulate off-balance-sheet or otherwise hidden risks, or engage in connected transactions with specific interest groups. When verifiable governance information is limited, adverse selection becomes more likely: issuers that rely more heavily on relationships and opaque practices are more prone to access the market, while investors struggle to distinguish their true risk, thereby demanding higher yield compensation and raising overall borrowing costs (Green et al., 2007). Against this backdrop, we expect online PFD to reduce issuance costs by supplying more direct, granular, and cross-verifiable governance-related information, lowering investors' information acquisition costs and adverse-selection risk, and thus compressing the information premium. Table 10 provides mechanism evidence from three complementary angles: improvements in the within-state information environment, the marginal informational content of credit ratings at issuance, and reduced information asymmetry in secondary-market trading.

In Panel A, we conduct cross-sectional tests based on the pre-adoption information environment, using the same set of controls and fixed effects as in the baseline specification. The results show that when online PFD applies only to state-level officials, implying a narrower information coverage for local issuers, the estimated effect is significantly attenuated. Moreover, using pre-reform newspaper circulation per capita and internet access as proxies for the information environment (Gao et al., 2020), we find that the online PFD effect is weaker in states with better pre-existing information supply, consistent with an information-substitution channel.[11] In contrast, when we proxy the standardization and readability of governmental financial reporting by whether the municipal sector is required to follow GAAP (Baber and Gore, 2008), we find no evidence of a significant substitutive or complementary relationship between online PFD and GAAP, suggesting that PFD and governmental financial reporting operate through distinct informational dimensions.[12]

Panel B evaluates whether online PFD improves investors' assessment of issuers' credit quality and the accuracy of primary-market pricing. Using SDC, we identify how many of the "Big Three" rating agencies (S&P, Moody's, and Fitch) rate the bond at issuance, and measure rating disagreement when multiple ratings are available. We find that the yield-reducing effect of online PFD is weaker when bonds receive multiple ratings at issuance, where credit information is more comprehensive. Within the multiple-rating subsample, the

[11] Newspaper data are obtained from the Editor & Publisher Yearbook and the US News Deserts Database Archive maintained by UNC's Center for Innovation and Sustainability in Local Media. We aggregate data to construct a state–year panel.

[12] State GAAP adoption information is collected from the National Association of Counties.

online PFD effect is also weaker when ratings are more concordant (i.e., lower informational noise), and the between-group differences are significant at the 1% level. Overall, these patterns indicate that online PFD delivers larger marginal informational benefits precisely when rating information is relatively sparse or exhibits greater disagreement, thereby more effectively mitigating information frictions in primary-market pricing.

Panel C examines whether online PFD alleviates information asymmetry in the secondary market. Market microstructure theories predict that dealers demand greater compensation when trading against potentially more informed counterparties, due to heightened adverse-selection risk (Kyle, 1985; Glosten and Milgrom, 1985). Accordingly, we use MSRB transaction data to construct two measures of trading frictions and evaluate them within 60-day and 90-day windows after issuance. First, we measure the customer markup as the deviation of the average customer buy price from the average dealer price within the window, standardized by the average interdealer price. Second, we measure price dispersion as the weighted standard deviation of price changes across consecutive customer–dealer trades within the window, and we normalize this measure by the average interdealer price (Jankowitsch et al., 2011). The results show that after online PFD adoption, price dispersion and customer markups decline by 2.8 bps and 4.5 bps, respectively, with stronger effects in the 60-day window where information asymmetry and trading frictions are typically more pronounced.

Taken together, the evidence in Table 10 consistently supports the information-frictions channel: online PFD reduces information acquisition costs and adverse-selection risk, improves investors' assessment of local-government credit risk and the trading-price formation process, and thereby contributes to lower municipal bond financing costs.

6 Additional Economic Consequences

Having established that online personal financial disclosure (PFD) adoption lowers municipal bond offering yields through multiple channels, a natural follow-up question is whether the decline in financing costs induces governments to borrow more or alters their fiscal stance. On the one hand, lower yields reduce the marginal cost of debt and may encourage greater issuance. On the other hand, if online PFD also strengthens fiscal discipline and external oversight, governments may exercise greater restraint even as borrowing becomes cheaper.

To investigate this question, we aggregate our bond-level data to the state–year level and examine three outcomes: the log of total issuance amount, the log of the number of bond issues, and the debt burden (defined as outstanding debt divided by tax revenue). Table 11 reports results from regressing each outcome on the online PFD indicator, including the

same state-level controls as in our baseline specifications and absorbing state and year fixed effects.

Across all three measures, we find no statistically significant relationship between online PFD adoption and state-level borrowing activity or fiscal burdens. The point estimate for log issuance amount is 0.0237, implying an economically negligible and statistically insignificant 2.4% increase. Similarly, the number of bond issues and the debt burden show small and insignificant coefficients. These null results are precisely estimated, with adjusted R-squared values exceeding 0.88 in all specifications, indicating that our models explain the vast majority of variation in these fiscal outcomes.

The absence of a significant borrowing response suggests that, over our sample period, the primary beneficiaries of lower financing costs are taxpayers and existing bondholders, rather than expanded government programs financed by additional debt. This finding is consistent with online PFD operating primarily through a governance and risk repricing channel that reduces the corruption premium and political risk embedded in yields, rather than fundamentally altering governments' demand for borrowed funds. It also alleviates concerns that our main results could be confounded by endogenous changes in issuance behavior following online PFD adoption.

Of course, these aggregate fiscal outcomes represent only one dimension of government behavior, and our sample period may be too short to capture longer-run adjustments in capital budgeting and project selection. Nonetheless, the evidence in Panel B reinforces the interpretation that the primary economic effect of online PFD operates through the cost of capital rather than the quantity of borrowing, returning part of the corruption premium to taxpayers without triggering a debt expansion that could offset these gains.

7 Conclusions

This paper examines whether the adoption of online PFD for public officials, as a form of individual-level transparency, affects municipal borrowing costs and market outcomes. Exploiting the staggered implementation of PFD platforms across U.S. states from 2004 to 2019, we find that these reforms reduce municipal bond offering yields by approximately 9–12 basis points. Event-study estimates support parallel trends prior to adoption and show that yield reductions emerge only after platform implementation. Moreover, the timing of online PFD adoption is not systematically related to state economic conditions or municipal financing needs, and our findings are robust to a range of robustness checks, including alternative outcome measures, sample constructions, clustering of standard errors, and placebo

tests in both time and space. We identify four channels through which online PFD lowers borrowing costs: enhancing government effectiveness, strengthening underwriting integrity, increasing public approval, and reducing information frictions. The yield effects are stronger for bonds with greater default exposure and in settings where information is scarcer, consistent with lower default-risk premia and improved liquidity. Following platform adoption, the likelihood of costly advance refunding declines significantly and government-efficiency measures improve. Underwriting concentration falls, and issuers become more likely to appoint high-reputation lead underwriters. In the secondary market, price dispersion and customer markups decrease significantly, further indicating an improved information environment and lower trading costs.

Our findings carry important implications for public finance and governance. First, they underscore that transparency reforms targeting individual decision-makers, not only organizational entities, can have first-order effects on municipal borrowing costs and market functioning. While prior research emphasizes the role of issuer-level financial reporting and market-wide disclosure regimes, our results demonstrate that online PFD serves as a distinct and powerful institution that shapes the incentives and constraints of officials who exercise discretion over debt issuance and intermediary selection. Second, by documenting that online PFD adoption reduces rent extraction in opaque underwriting relationships and returns part of the corruption premium to taxpayers, our study highlights the economic value of anti-corruption institutions in settings where agency problems and political connections are difficult to observe directly. Finally, evidence that online PFD increases citizen approval of state legislatures and state governments, while improving local government efficiency, suggests that personal transparency reforms can strengthen democratic accountability and governance quality more broadly, with benefits extending beyond financial markets to the political economy of public decision-making.

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Figures

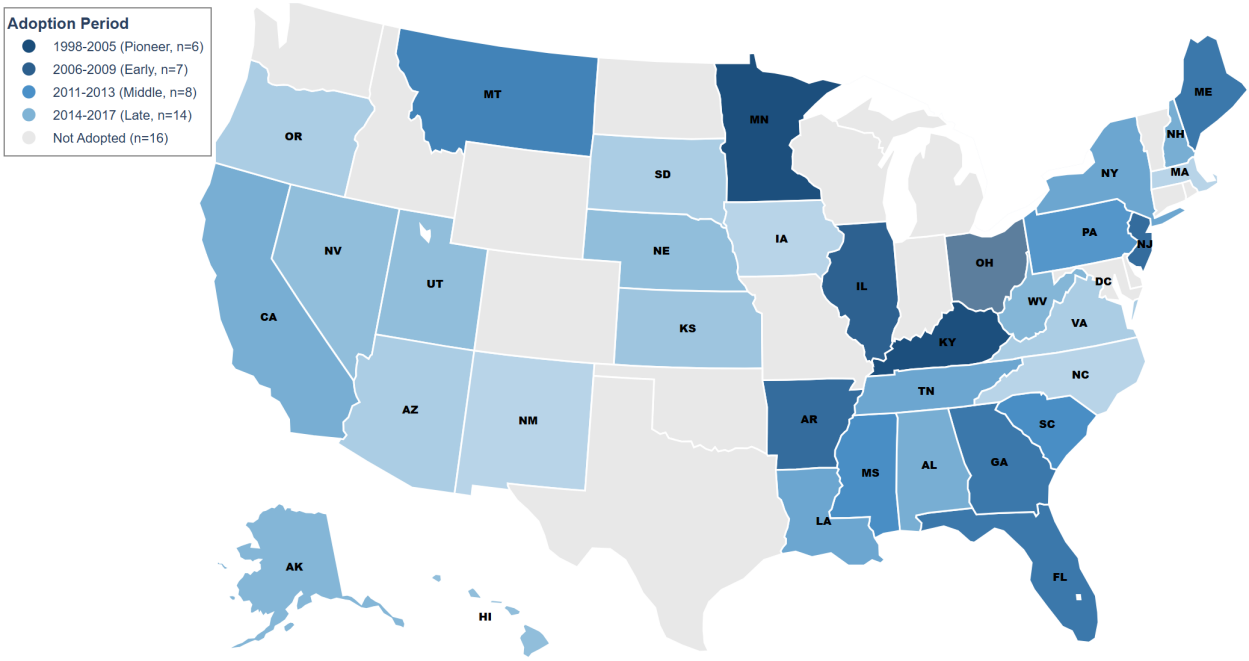


Figure 1: Spatial and Temporal Variation in Online PFD Adoption

This figure displays the staggered adoption of online PFD systems across U.S. states from 1998 to 2017. The map uses color intensity to represent adoption timing: darker blue indicates earlier adoption (1998–2005), while lighter blue indicates later adoption (2014–2017). Gray shading denotes states that did not implement online PFD systems for state-level officials during the sample period. The adoption year represents the first year in which a state government provided public access to officials' PFD filings on an official state website, allowing users to view, download, or search records without submitting case-specific requests. Table A3 reports the adoption year of online PFD systems for each state.

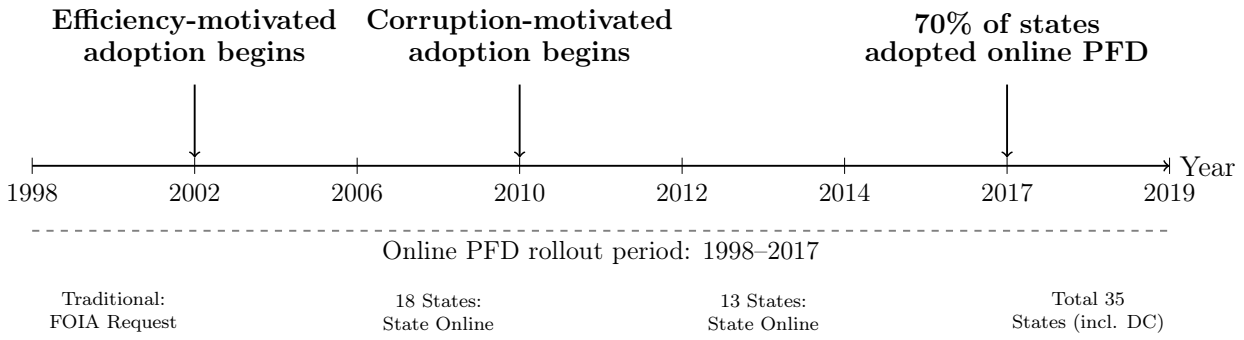


Figure 2: Timeline of Online PFD Adoption

This figure illustrates the temporal pattern of online PFD adoption before 2019. Prior to 1998, access to officials' financial disclosures in all states required submitting a formal request. The first wave of efficiency-motivated adoptions began around 2000, with 18 states (AK, AZ, CA, GA, HI, KS, KY, ME, MN, MT, NH, NJ, NC, OH, PA, SC, SD, MA) implementing online systems primarily for administrative modernization and cost reduction. The second wave of corruption-motivated adoptions began around 2006, with 13 states (AL, DC, FL, LA, MS, NE, NM, NV, NY, OR, TN, VA, WV) adopting in response to corruption scandals or as part of broader ethics reform packages. By 2017, a total of 35 states (including the District of Columbia) had implemented online PFD systems.

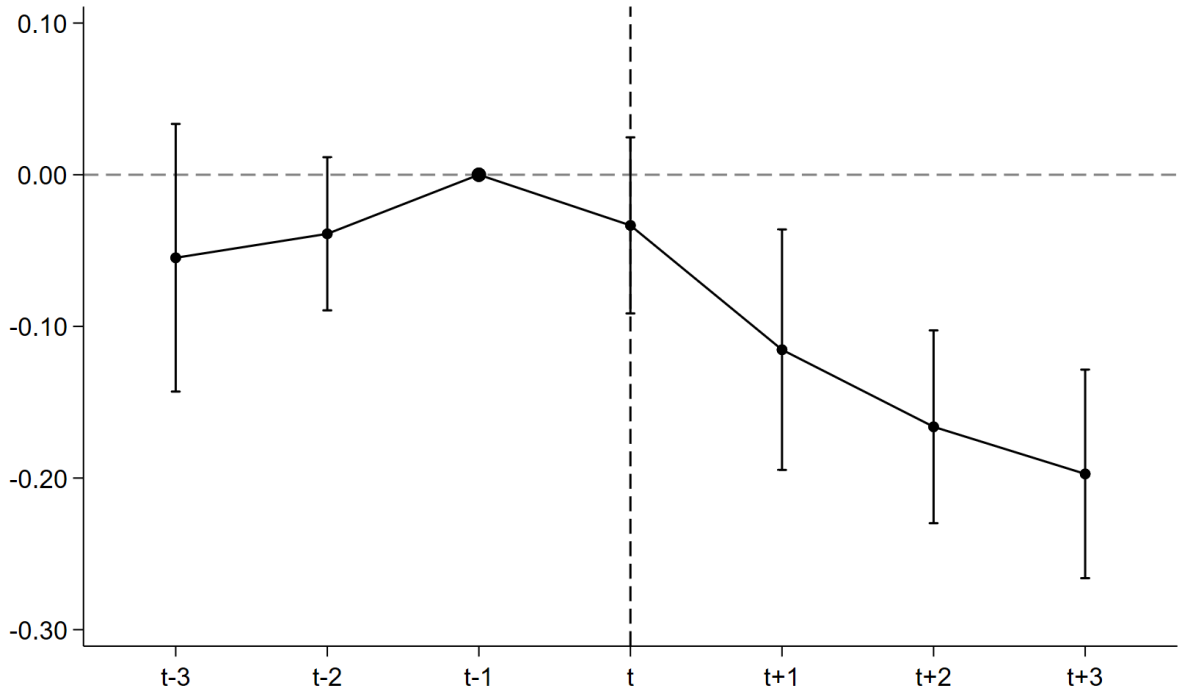


Figure 3: Event-Study Estimates: Baseline DID

This figure plots event-study estimates of the effect of online PFD adoption on municipal bond offering yields using a baseline specification. The horizontal axis measures event time relative to the adoption year, with $t = 0$ denoting the year of adoption. The vertical axis measures the estimated treatment effect in percentage points. Each point represents the coefficient on an event-time indicator, with $t - 1$ (the year immediately prior to adoption) omitted as the reference period. The vertical bars represent 95% confidence intervals constructed from standard errors clustered at the state level. The vertical dashed line marks the adoption year. The specification includes state and year fixed effects, along with the full set of bond-level and state-level controls described in Section 4.1.

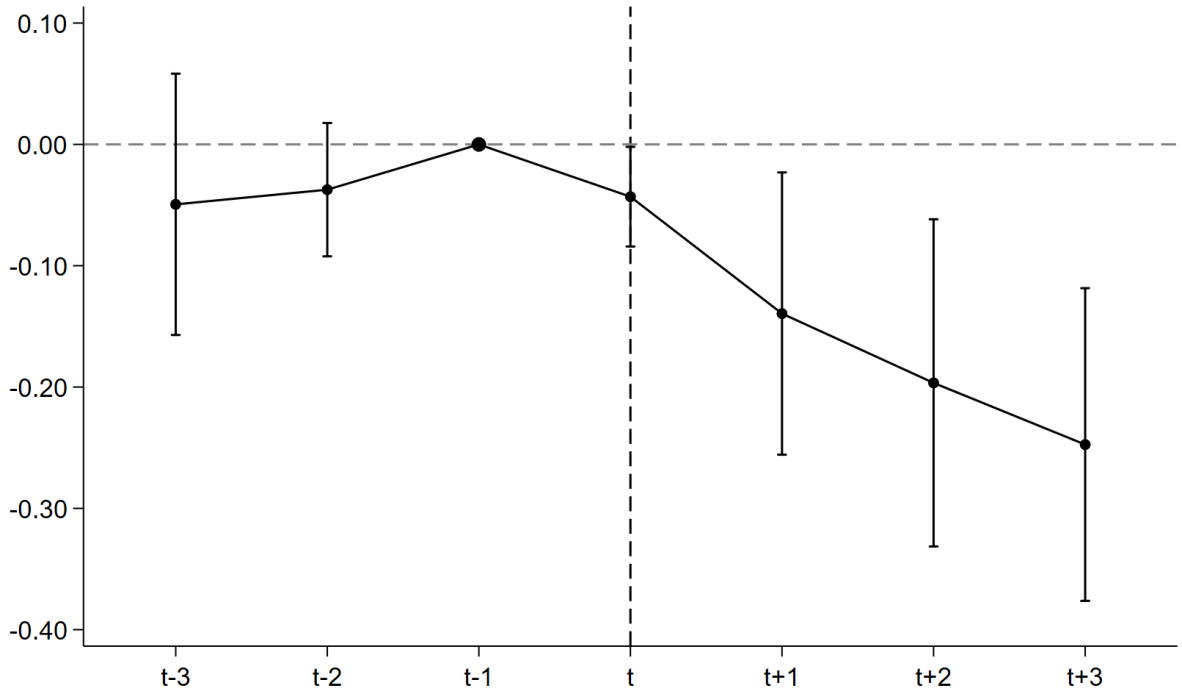


Figure 4: Event-Study Estimates: Stacked DID

This figure plots event-study estimates of the effect of online PFD adoption on municipal bond offering yields using a stacked DiD specification. The horizontal axis measures event time relative to the adoption year, with $t = 0$ denoting the year of adoption. The vertical axis measures the estimated treatment effect in percentage points. Each point represents the coefficient on an event-time indicator, with $t - 1$ (the year immediately prior to adoption) omitted as the reference period. The vertical bars represent 95% confidence intervals constructed from standard errors clustered at the state level. The specification follows equation 4 and includes state-by-cohort and year-by-cohort fixed effects, along with the full set of bond-level and state-level controls. The control group consists of never-treated states. The estimates correspond to the event-study version of Column (1) in Panel A of Table 4.

Tables

Table 1: Summary Statistics

This table reports summary statistics for the main variables used in the analysis. Bond characteristics are measured at the bond-issue level. County characteristics are measured at the county-year level and merged to the bond-issue data using the issuer's county and issuance year. State characteristics are measured at the state-year level and merged using the issuer's state and issuance year. The sample construction procedure is described in Appendix Table A2.

Variable	N	Mean	Std	P25	Median	P75
Bond characteristics						
Offering yield	327,705	2.86	1.23	1.91	2.83	3.81
Coupon rate	327,705	3.50	1.25	3.00	3.75	4.38
Ln(issuance amount)	327,705	13.08	1.51	12.13	12.99	14.00
Matched treasury yield	327,705	2.68	1.18	1.88	2.54	3.30
Ln(maturity years)	327,705	2.23	0.73	1.86	2.39	2.77
Ln(number of maturities)	327,705	2.79	0.48	2.64	2.89	3.00
Callable	327,705	0.58	0.49	0.00	1.00	1.00
Insured	327,705	0.26	0.44	0.00	0.00	1.00
Negotiated	327,705	0.28	0.45	0.00	0.00	1.00
County characteristics						
Welfare	29,825	0.06	0.11	0.00	0.01	0.08
Debt	29,856	0.02	0.09	0.00	0.01	0.02
Wage	44,927	0.16	0.10	0.10	0.14	0.20
Employment	44,929	0.05	0.03	0.04	0.05	0.06
County personal income	45,335	36.80	11.27	29.61	34.92	41.54
Ln(county population)	45,335	10.25	1.49	9.28	10.16	11.11
County population growth	45,330	0.00	0.02	-0.01	0.00	0.01
County income growth	45,330	0.04	0.06	0.01	0.04	0.06
State characteristics						
Underwriter HHI (all issues)	686	0.23	0.15	0.12	0.18	0.27
Underwriter HHI (negotiated issues)	679	0.35	0.21	0.19	0.28	0.45
Online PFD	686	0.30	0.46	0.00	0.00	1.00
Unemployment rate	686	5.60	2.14	4.02	5.11	6.77
Ln(population)	686	15.01	1.06	14.10	15.09	15.69
GSP per capita	686	0.06	0.02	0.05	0.05	0.06
Personal income	686	43.16	9.28	36.24	41.64	48.20
Internet access rate	686	0.72	0.11	0.64	0.73	0.80
Judiciary per capita	686	0.08	0.06	0.04	0.07	0.11
Fiscal health	686	1.77	4.96	-0.45	1.77	3.83
Top wage tax rate	686	4.79	3.22	2.90	5.00	6.97
Top capital gains tax rate	686	4.69	3.09	3.07	5.00	6.85
Convictions per capita	686	4.00	7.82	1.20	2.37	4.11
Split government	686	0.39	0.49	0.00	0.00	1.00

Table 2: Baseline Results: Online PFD and Offering Yield

This table reports estimates of the effect of online PFD adoption on municipal bond offering yields under alternative regression specifications. Column (1) restricts the sample to three years before and three years after the reform and defines Online PFD as an indicator equal to one if the issuing state is treated and the issuance year is weakly after the state's adoption year, and zero otherwise. Column (2) addresses concerns about delayed policy effectiveness by redefining the treatment indicator to turn on strictly after adoption (Scherf, 2024). Column (3) expands the event window to five pre- and five post-treatment years and excludes observations in the adoption year (Cornaggia et al., 2022). All regressions include bond-level and state-level controls. Standard errors are clustered at the state level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Dep. Var:	Offering yield		
	(1)	(2)	(3)
Online PFD	-0.0912***	-0.1236***	-0.1098***
	(0.0200)	(0.0244)	(0.0261)
Coupon	-0.0197	-0.0187	-0.0163
	(0.0834)	(0.0829)	(0.0754)
Ln(issuance amount)	-0.0566**	-0.0570**	-0.0520**
	(0.0266)	(0.0265)	(0.0254)
Matched Treasury yield	0.6999***	0.6995***	0.6892***
	(0.0882)	(0.0875)	(0.0713)
Ln(maturity years)	0.4676***	0.4667***	0.4752***
	(0.0475)	(0.0472)	(0.0406)
Ln(number of maturities)	-0.0535	-0.0550	-0.0527
	(0.0379)	(0.0376)	(0.0338)
Callable	0.1701*	0.1724*	0.1636**
	(0.0889)	(0.0876)	(0.0714)
Insured	0.0681***	0.0679***	0.0551**
	(0.0246)	(0.0245)	(0.0229)
Negotiated offer	0.0623	0.0628	0.0821
	(0.0705)	(0.0708)	(0.0672)
Unemployment rate	0.0667***	0.0692***	0.0866***
	(0.0157)	(0.0154)	(0.0294)
Ln(population)	-0.8851**	-0.9640**	-0.8099**
	(0.3942)	(0.3631)	(0.3456)
GSP per capita	-4.6776	-3.8695	-3.1488
	(7.0708)	(6.6488)	(7.2632)
Personal income	-0.0165**	-0.0150**	-0.0147*
	(0.0065)	(0.0065)	(0.0084)
Internet access rate	-2.0723***	-2.1139***	-1.9597**
	(0.6429)	(0.6613)	(0.7964)
Judiciary per capita	-0.2382	-0.5181	-1.1518
	(0.9309)	(0.8787)	(0.9377)
Fiscal health	-0.0002	-0.0004	-0.0018
	(0.0032)	(0.0034)	(0.0033)

Top wage tax rate	0.0104	0.0070	-0.0145
	(0.0136)	(0.0153)	(0.0209)
Top capital gains tax rate	-0.1051***	-0.1103***	-0.0680**
	(0.0183)	(0.0193)	(0.0285)
Convictions per capita	0.0103	0.0120	0.0141
	(0.0081)	(0.0080)	(0.0089)
Split government	-0.0061	-0.0177	-0.0472*
	(0.0215)	(0.0208)	(0.0274)
Constant	17.7287***	18.9638***	16.1554***
	(5.7870)	(5.2640)	(5.2121)
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	231,483	231,483	262,188
Adjusted R^2	0.8686	0.8689	0.8747

Table 3: The Determinants of Online PFD

This table reports estimates from Cox proportional hazards models, with coefficients presented as hazard ratios. The failure event is defined as a state first making PFD filings publicly available on the state website; once the event occurs, the state exits the risk set and is removed from the sample thereafter. The dependent variable is the number of years from 2004 until a state adopts online disclosure of PFD. Column (1) includes only core state-level aggregated municipal bond market variables: the one-year lag of log issuance amount; the growth in log issuance amount relative to the previous year and to two years prior; the issuance-amount-weighted one-year lag of yield to maturity; and the growth in yield to maturity relative to the previous year. Column (2) further adds additional one-year lagged state-level aggregated bond market characteristics. Columns (3)–(5) sequentially add one-year lagged state-level control variables. All covariates are standardized to have mean zero and unit standard deviation. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)
Ln(issuance amount)	0.0486	-0.1173	-0.6049	-0.3183	-0.7368
	(0.3344)	(0.4308)	(0.8572)	(0.9931)	(1.0501)
Change in ln(issuance amount)	-0.1438	-0.0180	-0.1131	0.0935	0.0189
	(0.3677)	(0.3969)	(0.4115)	(0.4559)	(0.4841)
Pre-trend in ln(issuance amount)	-0.6704	-0.6169	-0.4280	-0.5819	-0.4557
	(0.7211)	(0.7604)	(0.7274)	(0.7788)	(0.7719)
Yield to maturity	-0.0059	-0.1813	-0.2119	-0.3282	-0.4164
	(0.3818)	(0.6183)	(0.6014)	(0.7955)	(0.7982)
Change in yield to maturity	-0.1693	-0.2056	-0.2560	-0.2986	-0.3279
	(0.2663)	(0.2906)	(0.2755)	(0.2681)	(0.2911)
Weighted avg. maturity (years)		-0.2222	-0.3221	-0.3133	-0.1658
		(0.3085)	(0.3497)	(0.3594)	(0.4052)
Weighted avg. coupon		0.3770	0.3983	0.4317	0.3908
		(0.3253)	(0.3195)	(0.4334)	(0.4143)
Callable share		0.2524	0.2948	0.3701	0.4067
		(0.1821)	(0.1990)	(0.2515)	(0.2516)
Insured share		0.2338	0.1321	0.0833	0.1993
		(0.4265)	(0.4418)	(0.4460)	(0.4497)
Negotiated share		-0.2581	-0.2928	-0.1730	-0.1987
		(0.3128)	(0.3391)	(0.3985)	(0.3886)
Unemployment rate			-0.0680	-0.0454	-0.1482
			(0.2913)	(0.3893)	(0.4529)
Ln(population)			0.4704	0.3443	0.6229
			(0.5246)	(0.5745)	(0.6681)
GSP per capita			0.5084**	0.2962	0.4516
			(0.2372)	(0.2805)	(0.4772)
Personal income per capita			-0.5842	-0.2148	-0.2223
			(0.4185)	(0.5726)	(0.5852)
Internet access rate				-1.0873*	-1.0248
				(0.6332)	(0.7357)
Judiciary per capita				0.0780	0.0407
				(0.4896)	(0.5266)
Fiscal health				0.1327	0.1539
				(0.2302)	(0.2280)

Top wage tax rate					0.8183
					(0.5500)
Top capital gains tax rate					-0.5938
					(0.5122)
Convictions per capita					-0.0622
					(0.2763)
Split government					0.4854
					(0.4713)
Clustering	State	State	State	State	State
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	397	396	396	396	396

Table 4: Addressing Endogeneity: Alternative Designs

This table addresses endogeneity concerns underlying the baseline estimates by adopting alternative research designs. Panel A implements stacked DiD designs to estimate average treatment effects (Sun and Abraham, 2021). Column (1) defines cohorts by states' first adoption year, grouping states with the same first adoption year into the same cohort, and restricts the control group to never-treated states. Column (2) extends Column (1) by additionally including bonds issued by not-yet-treated states during their pre-adoption periods in the control group. Column (3) further expands the cohort definition by treating each state-level online PFD rollout as a separate cohort event, allowing states with the same adoption year to contribute to different cohorts (Baker et al., 2022). Panel B implements a border-discontinuity DiD design using a sample of cross-state adjacent county pairs, examining the effect of online PFD adoption on municipal bond offering yields. Following Dube et al. (2010) and Cornaggia et al. (2022), we retain only bond observations that can be uniquely matched to an issuing county and then construct adjacent county pairs across state borders. If a county borders multiple out-of-state adjacent counties, the bond observations from that county enter the data multiple times, once for each eligible county-pair match. Columns (1)–(3) of Panel B use the same event-window definitions as Columns (1)–(3) of Table 2, respectively. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Stacked DID Results			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	-0.1205***	-0.1091***	-0.1272***
	(0.0239)	(0.0223)	(0.0319)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year-Stack FE	Yes	Yes	Yes
State-Stack FE	Yes	Yes	Yes
Observations	831,122	1,202,581	2,199,913
Adjusted R^2	0.8755	0.8749	0.8766
Panel B: Border-Discontinuity DID Results			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	-0.0899***	-0.1246***	-0.1170***
	(0.0302)	(0.0455)	(0.0363)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Border-Pair FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Observations	102,670	102,670	122,892
Adjusted R^2	0.9095	0.9097	0.9119

Table 5: Robustness Checks for the Baseline Results

This table reports robustness checks for the baseline results. Panels A and B replace the dependent variable with underlying rating at issuance and the adjusted spread, respectively, with the latter constructed following Schwert (2017). Panel C excludes local-government bonds issued by states where online PFD applies only to state-level officials; these bonds are primarily affected through spillovers from state-level disclosure and thus the policy effect is expected to be weaker. Panel D excludes state-government bond issues, exploiting a setting in which the policy is determined at the state level while outcomes are measured at the local level. Panel E restricts the sample to states adopting online PFD primarily for modernization-related considerations to mitigate identification concerns from other contemporaneous governance shocks. Panel F reports inference based on two-way clustered standard errors by issuing state and issuance year to address potential correlation along both dimensions. Panel G implements an in-space placebo test by randomly assigning states to adoption years drawn without replacement from the empirical distribution of online PFD rollout years. Panel H implements a time-placebo test by shifting each state's online PFD adoption year three years earlier to construct a false treatment timing. Across all panels, the controls, fixed effects, and regression specifications follow Columns (1)–(3) in Table 2. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Underlying Rating as Outcome

Dep. Var:	Underlying rating		
	(1)	(2)	(3)
Online PFD	0.2080**	0.2152***	0.2889***
	(0.0952)	(0.0782)	(0.0879)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	90,685	90,685	104,538
Adjusted R^2	0.5715	0.5715	0.5614

Panel B: Adjusted Spread as Outcome

Dep. Var:	Adjusted spread		
	(1)	(2)	(3)
Online PFD	-0.1372***	-0.1784***	-0.1661***
	(0.0371)	(0.0462)	(0.0487)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	231,483	231,483	262,188
Adjusted R^2	0.6263	0.6269	0.6198

Panel C: Excluding State-Only Online PFD Bonds			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	-0.1182***	-0.1550***	-0.1469***
	(0.0379)	(0.0381)	(0.0465)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	197,613	197,613	215,748
Adjusted R^2	0.8672	0.8675	0.8714
Panel D: Excluding State-Government Bonds			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	-0.1033***	-0.1490***	-0.1388***
	(0.0368)	(0.0377)	(0.0470)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	180,928	180,928	196,970
Adjusted R^2	0.8716	0.8719	0.8755
Panel E: Efficiency-Motivated States Only			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	-0.1315**	-0.1817***	-0.1598***
	(0.0482)	(0.0436)	(0.0524)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	193,443	193,443	211,893
Adjusted R^2	0.8673	0.8677	0.8705

Panel F: Two-Way Clustered Standard Errors			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	-0.0912***	-0.1236***	-0.1098***
	(0.0202)	(0.0311)	(0.0331)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State & Year	State & Year	State & Year
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	231,483	231,483	262,188
Adjusted R^2	0.8686	0.8689	0.8747
Panel G: Placebo Tests: Randomization of Online PFD			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	-0.0130	-0.0189	0.0081
	(0.0303)	(0.0254)	(0.0408)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	237,586	237,586	262,300
Adjusted R^2	0.8726	0.8726	0.8719
Panel H: Placebo Tests: Pseudo-Event Year			
Dep. Var:		Offering yield	
	(1)	(2)	(3)
Online PFD	0.0906	0.0437	0.0448
	(0.0681)	(0.0476)	(0.0663)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	218,467	218,467	247,316
Adjusted R^2	0.8733	0.8731	0.8742

Table 6: Online PFD and Default Risk

This table provides evidence consistent with a reduction in default risk. Online PFD may reduce municipal borrowing costs by compressing default-risk premia related to fiscal discipline and accountability, thereby laying the groundwork for the subsequent analysis of the governance channel. We split the sample along two bond characteristics closely related to default risk exposure (Cheng, 2021): whether maturity exceeds 10 years and whether the enhanced rating exceeds 19 (corresponding to an AA rating), and estimate the effect of online PFD on offering yields within each subsample. All specifications use the event-window definition used in Column (1) of Table 2. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Dep. Var:	Offering yield			
	Maturity years		Enhanced rating	
	Long	Short	High	Low
Online PFD	-0.0713***	-0.0540***	0.0044	-0.1349***
	(0.0238)	(0.0196)	(0.0205)	(0.0461)
Bond Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Diff	-0.017**		0.139***	
P-value	0.018		< 0.001	
Observations	121,678	109,805	52,471	91,124
Adjusted R^2	0.7770	0.8504	0.9343	0.8844

Table 7: Governance Channel: Government Efficiency

This table examines the effect of online PFD on government efficiency. Scherf (2024) argues that online PFD, as a complementary monitoring institution, increases the referral and prosecution of corruption cases, with stronger effects in jurisdictions subject to stricter external oversight; accordingly, we posit that online PFD may promote more standardized government operations through improved case progression and monitoring. Panel A uses offering yield as the dependent variable and examines cross-sectional differences by pre-reform monitoring and enforcement conditions. Columns (1) and (2) compare the effects across states with different levels of pre-reform per-capita direct judiciary spending and public monitoring intensity, respectively, where the public monitoring measure follows Campante and Do (2014). Column (3) splits states by pre-reform per-capita convictions to proxy for enforcement capacity. In each column, "High" and "Low" indicate whether the corresponding state-level measure is above or below its median. Panel B examines whether online PFD reduces the likelihood that local governments engage in advance refunding, a potentially inefficient financing practice (Ang et al., 2017). Following Gao et al. (2020), we construct an advance-refunding indicator as the dependent variable using the sample of callable bonds and consider two alternative definitions: the redemption occurs at least 90 days prior to the first call date ($>$ 90 Days) or at least one year prior to the first call date ($>$ 1 Year). We estimate specifications under both definitions using logit and probit models. Panel C examines downstream implications for government efficiency. Following La Porta et al. (1999), we construct county–year measures of government efficiency and estimate the effect of online PFD on these outcomes, additionally controlling for county-level characteristics. All specifications use the event-window definition used in Column (1) of Table 2. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Facilitate Monitoring

Dep. Var:	Offering yield		
	(1)	(2)	(3)
Online PFD	-0.1200***	-0.1217***	-0.1357***
	(0.0278)	(0.0277)	(0.0334)
Online PFD × Low judiciary	0.0792*		
	(0.0441)		
Online PFD × Low monitoring		0.1099**	
		(0.0463)	
Online PFD × High convictions			0.0974*
			(0.0490)
Bond Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	231,483	231,483	230,109
Adjusted R^2	0.8686	0.8687	0.8685

Panel B: Curtail Advance Refunding

Dep. Var:	Advance refunding			
	(> 90 Days)		(> 1 Year)	
	Logit	Probit	Logit	Probit
Online PFD	-0.4574***	-0.1387***	-0.4637***	-0.1345***
	(0.1477)	(0.0446)	(0.1556)	(0.0474)
Unemployment rate	0.0219	0.0043	0.0362	0.0082
	(0.0372)	(0.0128)	(0.0428)	(0.0144)
Ln(population)	-0.3744***	-0.1152***	-0.4004***	-0.1206***
	(0.0529)	(0.0168)	(0.0529)	(0.0167)
GSP per capita	-33.0556***	-10.8474***	-33.8345***	-11.1035***
	(10.4353)	(3.2045)	(10.9308)	(3.2994)
Personal income	0.0142	0.0045	0.0110	0.0036
	(0.0160)	(0.0051)	(0.0162)	(0.0052)
Internet access rate	5.8072***	1.9048***	6.7807***	2.1721***
	(1.3170)	(0.4411)	(1.2343)	(0.4039)
Judiciary per capita	3.3140***	1.1435***	3.5342**	1.2073**
	(1.2671)	(0.4123)	(1.5681)	(0.4910)
Fiscal health	0.0408**	0.0133**	0.0533***	0.0171***
	(0.0174)	(0.0063)	(0.0166)	(0.0062)
Top wage tax rate	0.0762	0.0275	0.0728	0.0256
	(0.0574)	(0.0192)	(0.0641)	(0.0208)
Top capital gains tax rate	-0.0792	-0.0282	-0.0691	-0.0244
	(0.0576)	(0.0191)	(0.0626)	(0.0203)
Convictions per capita	0.0031	0.0014	-0.0078	-0.0018
	(0.0281)	(0.0088)	(0.0288)	(0.0089)
Split government	-0.2356**	-0.0695**	-0.2266*	-0.0655*
	(0.1130)	(0.0355)	(0.1195)	(0.0361)
Time to first call	-0.2530***	-0.0804***	-0.1880***	-0.0592***
	(0.0178)	(0.0063)	(0.0208)	(0.0072)
Time to maturity	0.0763***	0.0242***	0.0815***	0.0253***
	(0.0046)	(0.0018)	(0.0046)	(0.0018)
Constant	-2.0835*	-1.6202***	-2.6349**	-1.8149***
	(1.0878)	(0.3376)	(1.1497)	(0.3564)
Clustering	Year	Year	Year	Year
Year FE	Yes	Yes	Yes	Yes
Observations	5,201,345	5,201,345	5,201,942	5,201,942
Pseudo R^2	0.0634	0.0629	0.0620	0.0611

Panel C: Improve Government Efficiency

Dep. Var:	Welfare	Debt	Wage	Employment
Online PFD	0.0044***	-0.0017***	-0.0046**	-0.0016**
	(0.0014)	(0.0005)	(0.0019)	(0.0006)
Unemployment rate	-0.0013	0.0011***	0.0036***	0.0001
	(0.0009)	(0.0002)	(0.0009)	(0.0003)
Ln(population)	0.1377***	0.0051	-0.0240	0.0104
	(0.0209)	(0.0087)	(0.0377)	(0.0101)
GSP per capita	-1.6880***	0.0139	-1.3864***	-0.4356***
	(0.2696)	(0.1363)	(0.3630)	(0.1393)
Personal income	0.0013**	-0.0002	0.0022**	0.0003
	(0.0006)	(0.0003)	(0.0008)	(0.0002)
Internet access rate	0.0690**	-0.0037	0.0105	0.0145
	(0.0264)	(0.0111)	(0.0324)	(0.0100)
Judiciary per capita	0.0656	-0.0948***	-0.0647	-0.0553***
	(0.0680)	(0.0280)	(0.0599)	(0.0170)
Fiscal health	0.0002	0.0000	-0.0001	-0.0000
	(0.0002)	(0.0001)	(0.0001)	(0.0000)
Top wage tax rate	-0.0015	0.0006	-0.0033**	-0.0005
	(0.0013)	(0.0005)	(0.0014)	(0.0005)
Top capital gains tax rate	0.0011	-0.0001	0.0065***	0.0014***
	(0.0010)	(0.0005)	(0.0011)	(0.0004)
Convictions per capita	0.0002	0.0001	-0.0004	-0.0000
	(0.0003)	(0.0001)	(0.0003)	(0.0001)
Split government	0.0040*	0.0014**	0.0031**	0.0007
	(0.0021)	(0.0005)	(0.0014)	(0.0004)
County personal income	-0.0007***	-0.0000	-0.0020***	0.0000
	(0.0001)	(0.0000)	(0.0001)	(0.0000)
Ln(county population)	0.0060***	-0.0004	-0.0166***	-0.0035***
	(0.0004)	(0.0005)	(0.0006)	(0.0003)
County population growth	-0.1296***	0.1002*	-0.1719**	-0.1064***
	(0.0220)	(0.0482)	(0.0725)	(0.0244)
County income growth	0.0040	-0.0032	0.0043	-0.0132**
	(0.0111)	(0.0058)	(0.0208)	(0.0055)
Constant	-2.1062***	-0.0506	0.7221	-0.0756
	(0.3099)	(0.1335)	(0.5651)	(0.1513)
Clustering	Year	Year	Year	Year
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,056	16,078	25,065	25,069
Adjusted R^2	0.6784	0.1392	0.2308	0.1939

Table 8: Governance Channel: Underwriting Integrity

This table examines whether online PFD curbs relationship-based underwriting and underwriting rent-seeking. Panel A focuses on discretion in the issuance process and estimates the specification separately by negotiated issuance and by whether a co-manager is appointed. Panel B examines underwriting concentration. Motivated by Li (2024), who argues that greater underwriting concentration raises governments' financing costs, we use the state–year underwriter HHI as the dependent variable, constructed separately for the full sample and for the negotiated subsample, and additionally control for state–year underwriting market size and the number of underwriters. Panel C examines the reputation channel. Following Butler et al. (2009), we measure underwriter reputation by the underwriter's national market share in the issuance year, and construct the dependent variable accordingly: it equals one if the lead underwriter is among the top ten underwriters nationwide in that year and zero otherwise. We split the sample by the extent of issuers' discretion in underwriter choice and report Logit and Probit estimates. All specifications use the same event-window definition as Column (1) of Table 2. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Mitigate Underwriting Rent-Seeking

Dep. Var:	Offering yield			
	Negotiated		Co-manager	
	Yes	No	Yes	No
Online PFD	-0.1445***	-0.0236	-0.0419	-0.1070***
	(0.0429)	(0.0221)	(0.0254)	(0.0373)
Bond Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Diff	-0.121***		0.065***	
P-value	< 0.001		< 0.001	
Observations	58,579	172,904	92,113	129,486
Adjusted R^2	0.8849	0.8765	0.9023	0.8656

Panel B: Reduce Underwriting Concentration

Dep. Var:	State-year underwriting HHI	
	All issues	Negotiated issues
Online PFD	-0.0227**	-0.0285*
	(0.0091)	(0.0155)
Unemployment rate	0.0103**	0.0064
	(0.0041)	(0.0078)
Ln(population)	0.1835*	0.2911
	(0.0867)	(0.1801)
GSP per capita	3.0603	2.7078
	(2.2242)	(2.7716)
Personal income per capita	-0.0086*	0.0006
	(0.0043)	(0.0054)
Internet access rate	-0.4142	-0.0667
	(0.2419)	(0.3561)
Judiciary per capita	-0.7781	-1.3059
	(0.4832)	(0.7902)
Fiscal health	-0.0016	-0.0009
	(0.0014)	(0.0019)
Top wage tax rate	0.0175*	0.0087
	(0.0097)	(0.0102)
Top capital gains tax rate	-0.0045	0.0114
	(0.0094)	(0.0133)
Convictions per capita	-0.0005	0.0010
	(0.0013)	(0.0014)
Split government	-0.0087	0.0069
	(0.0077)	(0.0144)
Underwriting Market Size	0.0201*	0.0166
	(0.0114)	(0.0164)
Number of Underwriters	-0.2760***	-0.3522***
	(0.0226)	(0.0233)
Constant	-1.7647	-3.8238
	(1.4076)	(2.8081)
Clustering	Year	Year
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	428	421
Adjusted R^2	0.7933	0.7876

Panel C: Attract High-Reputation Underwriters

Dep. Var:	High reputation underwriter			
	Logit model		Probit model	
	Negotiated	Competitive	Negotiated	Competitive
Online PFD	0.5124**	-0.0779	0.2809**	-0.0510
	(0.2266)	(0.1269)	(0.1307)	(0.0749)
Bond Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Clustering	Year-Month	Year-Month	Year-Month	Year-Month
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Diff	0.590***		0.332***	
P-value	< 0.001		< 0.001	
Observations	53,106	150,254	53,106	150,254
Pseudo R^2	0.2027	0.1461	0.2021	0.1468

Table 9: Governance Channel: Public Approval

This table examines whether online PFD lowers municipal bond issuance costs by strengthening public approval, thereby reducing governance risk and investors' required yields. Using individual-level CCES survey data, we estimate the effect of online PFD on public approval. Panel A uses respondents' approval of the state legislature as the dependent variable. Panel B uses respondents' approval of the governor as the dependent variable, serving as a proxy for citizens' approval in state government (Cohen and King, 2004; Brown, 2010). Panel C conducts a placebo test using respondents' approval of the president as the dependent variable. In each panel, Column (1) codes the five-point survey response into an ordered measure ranging from -2 to 2. Columns (2) and (3) use a binary indicator that equals one if approval is above zero and zero otherwise; Column (2) reports Logit estimates and Column (3) reports Probit estimates. All specifications include the individual-level and state-level controls and the event-window definition used in Column (1) of Table 2. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Strengthens Approval in State Legislature

Dep. Var:	Leg. approval	High leg. approval	
		Logit	Probit
Online PFD	0.1923***	0.2823***	0.1703***
	(0.0607)	(0.0866)	(0.0513)
Individual Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	176,944	176,944	176,944
Adjusted R^2 / Pseudo R^2	0.0747	0.0415	0.0414

Panel B: Strengthens Approval in State Government			
Dep. Var:	Gov. approval	High gov. approval	
		Logit	Probit
Online PFD	0.1662**	0.2429**	0.1493**
	(0.0791)	(0.1050)	(0.0650)
Individual Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	200,683	200,683	200,683
Adjusted R^2 / Pseudo R^2	0.0481	0.0351	0.0351
Panel C: Placebo Test of Presidential Approval			
Dep. Var:	Pres. approval	High pres. approval	
		Logit	Probit
Online PFD	-0.0351	-0.0503	-0.0271
	(0.0560)	(0.0738)	(0.0460)
Individual Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Clustering	State	State	State
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	201,713	201,713	201,713
Adjusted R^2 / Pseudo R^2	0.1499	0.1046	0.1027

Table 10: Information Channel

This table tests the information-frictions mechanism. Panel A studies whether online PFD improves the in-state information environment by estimating the specification separately across subsamples defined by pre-reform state characteristics. Column (1) splits states by whether online PFD applies only to state-level officials. Column (2) splits states by pre-reform newspaper circulation. Column (3) splits states by pre-reform internet access rate. Column (4) splits states by whether the state prepares government financial statements under GAAP. In each column, "High" and "Low" are defined by whether the corresponding state-level measure is above or below its median. Panel B examines whether online PFD complements rating information at issuance. Columns (1) and (2) split bonds by whether they are rated by more than one agency at issuance. Columns (3) and (4) further split the multi-rated subsample by whether ratings exhibit substantial disagreement, based on SDC. Panel C examines whether online PFD mitigates information asymmetry. The dependent variables are Price dispersion and Customer markups measured over the 90-day and 60-day windows after issuance, constructed following Gao et al. (2020) and MSRB (2018) methodologies. All specifications use the same event-window definition as Column (1) of Table 2. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Panel A: Enhances the State Information Environment

Dep. Var:	Offering yield			
	(1)	(2)	(3)	(4)
Online PFD	-0.1485***	-0.1389***	-0.1591***	-0.0607
	(0.0400)	(0.0303)	(0.0373)	(0.0382)
Online PFD × Only State	0.1084**			
	(0.0474)			
Online PFD × High News		0.1015**		
		(0.0459)		
Online PFD × High Inter			0.1294***	
			(0.0446)	
Online PFD × GAAP				-0.0421
				(0.0499)
Bond Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	231,483	231,483	231,483	231,483
Adjusted R^2	0.8687	0.8687	0.8687	0.8686

Panel B: Supplementary Bond Rating Information

Dep. Var:	Offering yield			
	Rating information		Rating consistency	
	More	Less	Yes	No
Online PFD	-0.0613*	-0.0951***	-0.0587*	-0.2030**
	(0.0311)	(0.0277)	(0.0334)	(0.0901)
Bond Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Diff	0.034***		0.144***	
P-value	< 0.001		< 0.001	
Observations	53,056	168,543	46,805	6,251
Adjusted R^2	0.9073	0.8722	0.9091	0.9126

Panel C: Mitigate Information Asymmetry in the Secondary Market

Dep. Var:	Price dispersion		Customer markups	
	Window-90days	Window-60days	Window-90days	Window-60days
Online PFD	-2.7594*	-2.8139**	-4.5117*	-4.5156**
	(1.5323)	(1.3803)	(2.3319)	(2.0437)
Bond Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	82,789	79,596	127,531	125,309
Adjusted R^2	0.1702	0.1786	0.1454	0.1643

Table 11: Additional Economic Consequences

This table provides further evidence on the economic consequences of online PFD, focusing on whether lower borrowing costs are followed by greater borrowing activity and higher fiscal burdens. The dependent variables are measured at the state–year level and capture municipal issuance and debt burdens. Ln(issuance amount) is the log of total municipal bond issuance in a state–year, Ln(number of bonds) is the log of the number of bond issues in a state–year, and Debt burden is defined as outstanding debt divided by tax revenue in a state–year. All specifications use the same event-window definition as Column (1) of Table 2. Standard errors are reported in parentheses. * indicates $p < 0.10$, ** indicates $p < 0.05$, and *** indicates $p < 0.01$.

Dep. Var:	Ln(issuance amount)	Ln(number of bonds)	Debt burden
Online PFD	0.0237	0.0619	0.0013
	(0.0542)	(0.0390)	(0.0189)
State Controls	Yes	Yes	Yes
Clustering	Year	Year	Year
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	428	428	428
Adjusted R^2	0.8989	0.9242	0.8884

Appendix

SCHEDULE C
Income, Loans, & Business Positions
(Other than Gifts and Travel Payments)

CALIFORNIA FORM 700
FAIR POLITICAL PRACTICES COMMISSION

Name: Catherine Barna

► 1. INCOME RECEIVED

NAME OF SOURCE OF INCOME
Stifel

ADDRESS *(Business Address Acceptable)*
2121 Avenue of the Stars, Suite 2100, Los Angeles, CA 90067

BUSINESS ACTIVITY, IF ANY, OF SOURCE
Investment Banking

YOUR BUSINESS POSITION
Spouse is Managing Director

GROSS INCOME RECEIVED
☐ No Income - Business Position Only
☐ $500 - $1,000
☐ $1,001 - $10,000
☐ $10,001 - $100,000
☒ OVER $100,000

CONSIDERATION FOR WHICH INCOME WAS RECEIVED
☐ Salary
☒ Spouse's or registered domestic partner's income (For self-employed use Schedule A-2.)
☐ Partnership (Less than 10% ownership. For 10% or greater use Schedule A-2.)
☐ Sale of _____ *(Real property, car, boat, etc.)*
☐ Loan repayment
☐ Commission or ☐ Rental Income, *list each source of $10,000 or more*
_____ *(Describe)*
☐ Other _____ *(Describe)*

► 1. INCOME RECEIVED

NAME OF SOURCE OF INCOME
Stifel Nicolaus &Co. Inc.

ADDRESS *(Business Address Acceptable)*
2121 Avenue of the Stars, Suite 2150

BUSINESS ACTIVITY, IF ANY, OF SOURCE
Municipal Bond Underwriting

YOUR BUSINESS POSITION
Spouse of Employee

GROSS INCOME RECEIVED
☐ No Income - Business Position Only
☐ $500 - $1,000
☐ $1,001 - $10,000
☐ $10,001 - $100,000
☒ OVER $100,000

CONSIDERATION FOR WHICH INCOME WAS RECEIVED
☐ Salary
☒ Spouse's or registered domestic partner's income (For self-employed use Schedule A-2.)
☐ Partnership (Less than 10% ownership. For 10% or greater use Schedule A-2.)
☐ Sale of _____ *(Real property, car, boat, etc.)*
☐ Loan repayment
☐ Commission or ☐ Rental Income, *list each source of $10,000 or more*
_____ *(Describe)*
☐ Other _____ *(Describe)*

Figure A1: Example PFD Filing: California Form 700 Schedule C

This figure displays an example of California Form 700 Schedule C (Income, Loans, & Business Positions), which public officials are required to file to disclose potential conflicts of interest. The form requires officials to report income sources, business activities, and positions held by themselves or their spouses. In this example, the filer's spouse holds positions in investment banking and municipal bond underwriting at Stifel, with income exceeding $100,000. Online PFD systems make such disclosures publicly accessible via the internet, eliminating the need for formal information requests and enabling investors, journalists, and oversight bodies to identify potential conflicts of interest in municipal bond issuance and underwriting processes. The transparency created by online disclosure allows market participants to assess governance risks more efficiently, particularly when officials involved in municipal finance decisions have financial ties to underwriting firms or other entities that may benefit from discretionary resource allocation.

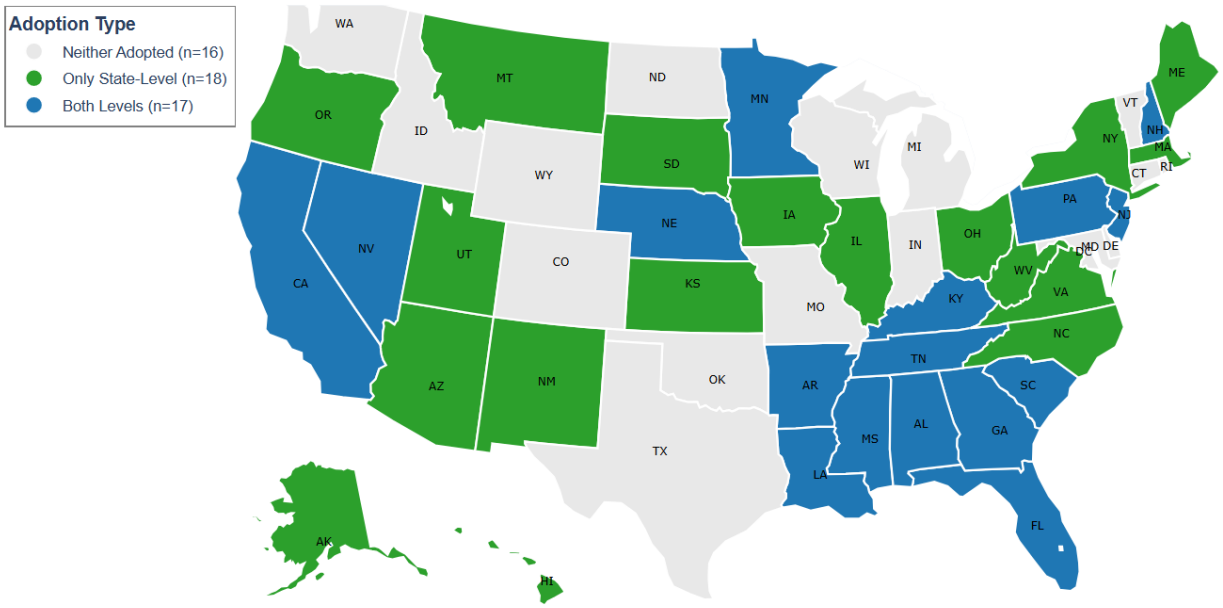


Figure A2: Online PFD Systems by Disclosure Coverage

This figure maps the geographic distribution of online PFD systems across U.S. states from 1998 to 2017, classified by the official coverage of PFD disclosure. States are color-coded based on disclosure coverage: blue indicates states that adopted online PFD systems for both state-level and local-level officials (n=17), green indicates states that adopted only for state-level officials (n=18), and gray indicates states that did not adopt online PFD systems for either state-level or local-level officials (n=16). The classification is based on state ethics commission websites and legislative records. States that adopted online PFD systems for local officials also adopted them for state officials, reflecting no cases of "only local" adoption.

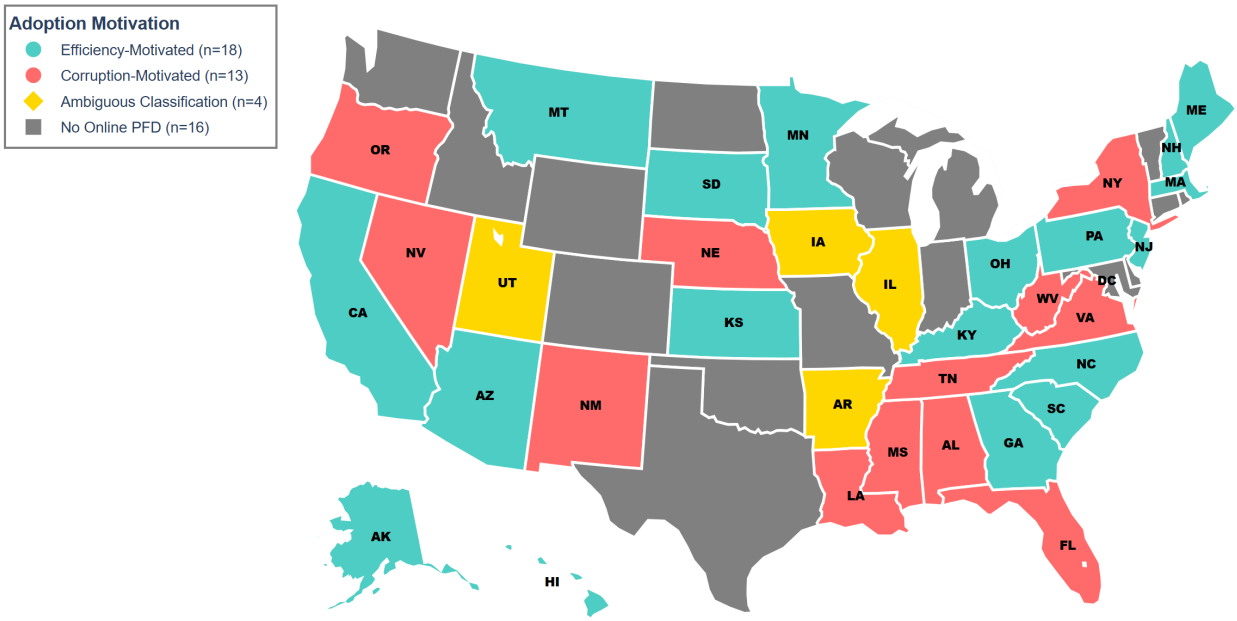


Figure A3: Online PFD Adoption by Motivation Type

This figure maps the geographic distribution of online PFD adoption across U.S. states from 1998 to 2017, classified by adoption motivation. States are color-coded according to motivation, which is identified based on 33 semi-structured interviews with state ethics officials and archival research of legislative records. Teal indicates efficiency-motivated adoptions (AK, AZ, CA, GA, HI, KS, KY, ME, MN, MT, NH, NJ, NC, OH, PA, SC, SD, MA; n=18), where states adopted online PFD primarily for administrative modernization, cost savings, and workflow efficiency. Coral indicates corruption-motivated adoptions (AL, DC, FL, LA, MS, NE, NM, NV, NY, OR, TN, VA, WV; n=13), where adoption occurred in response to corruption scandals or as part of comprehensive ethics reform packages. Gold indicates ambiguous classification (AR, IA, IL, UT; n=4), where interview evidence was inconclusive or revealed mixed motivations. Gray states did not adopt online PFD systems for state-level officials during this period (CO, CT, DE, ID, IN, MD, MI, MO, ND, OK, RI, TX, VT, WA, WI, WY; n=16). The classification represents 94 percent of states that made PFD available online as of 2017.

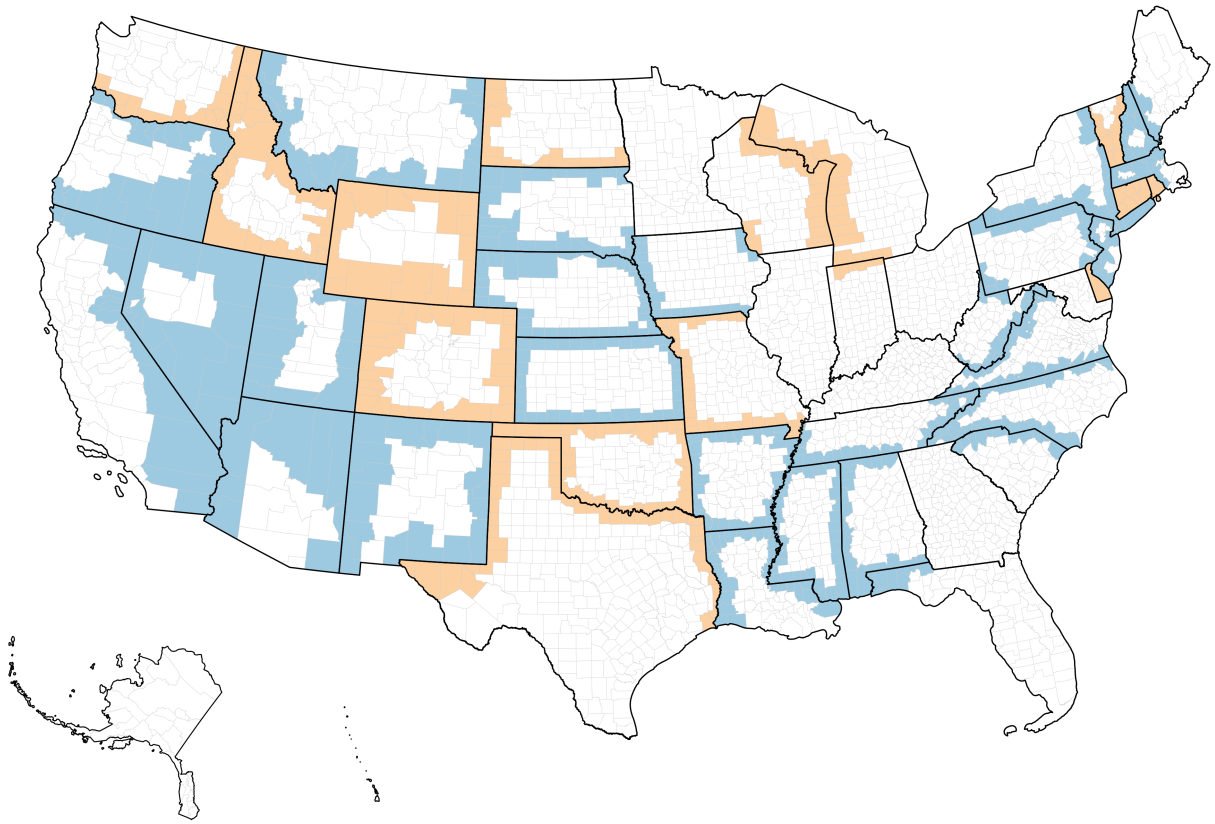


Figure A4: Border-Discontinuity DiD County Sample

This figure maps the geographic distribution of counties used in the border-discontinuity DiD design. Colored areas comprise the analysis sample of 815 counties that can be uniquely matched to issuers and used to construct cross-state adjacent pairs along state borders. If a county borders multiple out-of-state adjacent counties, its bond observations are duplicated for each eligible adjacency match. Colors indicate treatment status: blue denotes counties in states that have adopted online PFD during the sample period, whereas orange denotes counties in non-adopting states.

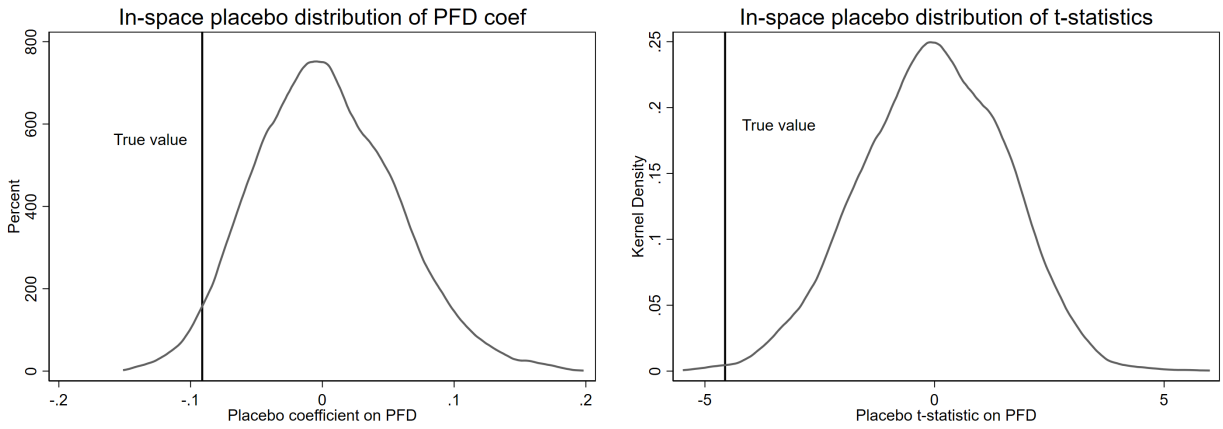


Figure A5: In-Space Placebo Test

This figure reports results from an in-space placebo test. We hold fixed the empirical distribution of online PFD adoption years and randomly (without replacement) reassign states to "pseudo" adoption years, thereby intentionally breaking the mapping between each state and its true adoption timing. After each random reassignment, we re-estimate the baseline model using the same sample and design as in Equation 1. We repeat this procedure 2,000 times and record, for each replication, the estimated coefficient of Online PFD indicator (left panel) and the associated t-statistic (right panel). The vertical solid line in each panel marks the true estimate based on the actual policy adoption. If the baseline findings were driven by unobserved shocks occurring near the adoption years, such shocks would remain present under this design and could generate systematic spurious effects. Instead, the placebo estimates are tightly centered around zero and the true estimate lies in the tail of the placebo distribution, indicating that the main results are unlikely to be driven by random timing assignment or unobserved shocks and are therefore robust.

Table A1: California Form 700 Disclosure Requirements

This table summarizes disclosure requirements for California Form 700 (Statement of Economic Interests) for the 2018–2019 filing period. Filers must report interests held by themselves and their spouses or registered domestic partners. Non-reportable interests may still create conflicts of interest. Detailed instructions available at https://www.fppc.ca.gov/Form700.html. Source: California Fair Political Practices Commission.

Schedule	Reportable	Non-Reportable	Threshold
A-1: Investments	Stocks, including those held in an IRA or 401K. Each stock must be listed	Insurance policies, government bonds, diversified mutual funds, funds similar to diversified mutual funds	Investment value $\geq$ \$2,000
A-2: Business Entities	Business entities, sole proprietorships, partnerships, LLCs, corporations and trusts	Savings and checking accounts, and annuities	Pro rata share $\geq$ \$2,000 or gross income $\geq$ \$10,000
B: Real Property	Rental property in filer's jurisdiction, or within two miles of the boundaries of the jurisdiction	A residence used exclusively as a personal residence (such as a home or vacation property)	Fair market value $\geq$ \$2,000
C: Income	Non-governmental salaries. Note that filers are required to report only half of their spouse's or partner's salary	Governmental salary (from school district, for example)	Aggregated income $\geq$ \$500
D: Gifts	Gifts from businesses, vendors, or other contractors (meals, tickets, etc.)	Gifts from family members	Single gift $\geq$ \$50 or aggregated gifts $\geq$ \$50
E: Travel Payments	Travel payments from third parties (not your employer)	Travel paid by your government agency	Single source $\geq$ \$50 gift or per source $\geq$ \$500 income

Table A2: Sample Construction

This table describes the construction of the empirical sample used in the analysis. Following common practice in the related literature, the analysis is restricted to newly issued general obligation (GO) bonds financed through traditional issuance channels. The table summarizes the sample selection procedure step by step, starting from the full Mergent FISD database and applying sequential filters to arrive at the baseline estimation samples used in different specifications reported in Table 2.

Step	*Data Filter*	*N obs.*
1	All bonds in the Mergent FISD database.	4,664,508
2	Bonds issued during 2004–2019.	2,336,297
3	New-money borrowing bonds, excluding refunding issues.	1,259,517
4	Restrict to general obligation (GO) bonds issued through conventional municipal financing structures, excluding federally issued bonds, tobacco- and tuition-settlement bonds, Build America Bonds, notes, certificates, and taxable bonds, and excluding floating-rate issues.	402,144
5	Exclude bonds issued by U.S. territories, bonds issued in states that made PFD filings publicly available online prior to 2006, and bonds issued in Maryland and Georgia.	335,903
6	Exclude bonds with missing issue state, issue year, or coupon, with missing or implausible offering yields (below 0 or above 50), or with missing or non-positive offering amount or bond maturity.	327,705
7	Baseline sample (Column (1) and (2) of Table 2): Bonds within a three-year event-time window around online PFD adoption.	231,483
8	Baseline sample (Column (3) of Table 2): Bonds within a five-year event-time window around online PFD adoption, excluding the adoption year.	262,188

Table A3: Online PFD Adoption Years by State

This table reports the adoption years of online PFD systems across U.S. states from 1998 to 2017. Panel A lists the 35 states (including the District of Columbia) that implemented online PFD during this period, organized by adoption year; Panel B lists the 16 states that did not implement online PFD during this period. We define the adoption year as the first year in which a state made PFD filings publicly accessible on an official state website, allowing users to view, download, or search records without submitting case-specific requests.

State	**Year**	**State**	**Year**	**State**	**Year**	**State**	**Year**
Panel A: States with Online PFD for State-Level Officials (n=35)							
Ohio	1998	Montana	2007	Louisiana	2011	Kansas	2015
Kentucky	2000	Mississippi	2008	New York	2011	DC	2015
Minnesota	2000	South Carolina	2008	Tennessee	2011	Arizona	2016
Illinois	2004	Pennsylvania	2009	Alabama	2012	Oregon	2016
Arkansas	2005	Florida	2006	California	2012	South Dakota	2016
New Jersey	2005	Georgia	2006	New Hampshire	2012	Virginia	2016
Maine	2006			Alaska	2013	Iowa	2017
				West Virginia	2013	Massachusetts	2017
				Hawaii	2014	New Mexico	2017
				Nebraska	2014	North Carolina	2017
				Nevada	2014		
				Utah	2014		
Panel B: States without Online PFD for State-Level Officials (n=16)							
Colorado		Indiana		North Dakota		Vermont	
Connecticut		Maryland		Oklahoma		Washington	
Delaware		Michigan		Rhode Island		Wisconsin	
Idaho		Missouri		Texas		Wyoming	

Table A4: Online PFD Adoption Years for Local Officials

Among states that implemented online PFD systems between 1998 and 2017, some systems also cover disclosures for local-level officials; however, in a few states, the year in which local-level PFD filings first became publicly available does not coincide exactly with the system adoption year. Local-level officials refer to elected or appointed officials at the municipal and county level, such as mayors, county commissioners, city council members, and heads of local agencies. Panel A lists the 17 states (including the District of Columbia) whose online PFD systems cover local-level officials, ordered by the first year in which local-level PFD filings were made public; Panel B lists 34 states that do not provide online public access to local-level officials' PFD filings, including 16 states without an online PFD system. We define the adoption year as the first year in which a state made local-level officials' PFD filings publicly accessible on an official state website, allowing users to view, download, or search records without submitting case-specific requests. Notably, some states adopted online PFD for state-level officials but did not extend coverage to local-level officials (e.g., Ohio, Illinois, and New York).

State	**Year**	**State**	**Year**	**State**	**Year**	**State**	**Year**
Panel A: States with Online PFD for Local-Level Officials (n=17)							
Kentucky	2000	South Carolina	2008	Alabama	2012	DC	2015
Minnesota	2000	Mississippi	2008	California	2012	Kansas	2015
Arkansas	2005	Pennsylvania	2009	New Hampshire	2012	Arizona	2016
New Jersey	2005	Louisiana	2011	Nebraska	2014	Oregon	2016
Florida	2006	Tennessee	2011	Nevada	2014	Massachusetts	2017
Georgia	2006						
Panel B: States without Online PFD for Local-Level Officials (n=34)							
Alaska		Iowa		New York		Utah	
Colorado		Idaho		North Carolina		Vermont	
Connecticut		Illinois		North Dakota		Virginia	
Delaware		Indiana		Ohio		Washington	
Hawaii		Maine		Oklahoma		West Virginia	
Maryland		Michigan		Rhode Island		Wisconsin	
Missouri		Montana		South Dakota		Wyoming	
New Mexico		Texas					