

Accounting Standardization and Separation in the Municipal Debt Market: Evidence from GASB 34

William Baber

Georgetown University

Amanda Beck

Georgia State University

Allison Koester

Georgetown University

ABSTRACT: Governmental Accounting Standards Board Statement No. 34 (GASB 34, 1999) standardized financial reporting and disclosure requirements for U.S. state and local governments. We interpret debt issuing patterns surrounding GASB 34 implementation as evidence of strategic behavior by governments in anticipation of GASB 34 consequences. Specifically, governments that expected more favorable post-GASB 34 evaluations by municipal bond investors delayed new uninsured debt issues until after, whereas governments that expected less favorable evaluations accelerated debt issues to before, GASB 34 information became publicly available. Governments expecting favorable consequences were more likely than governments expecting adverse consequences to substitute away from insured debt and toward uninsured debt, and to choose new debt financing rather than alternative financing sources following GASB 34. These findings are consistent with the notion that expectations about GASB 34 consequences were realized, and that standardization created through GASB 34 facilitated separation in the municipal debt market.

Data Availability: Data are from publicly available sources identified in the manuscript.

JEL Classifications: G18; H74; M48.

Keywords: governmental accounting standards; municipal debt market; government financing.

I. INTRODUCTION

Adopted in June 1999, Governmental Accounting Standards Board Statement No. 34 (hereafter GASB 34) touches all important facets of external financial reporting and disclosure by U.S. state and local governments (Patton and Hutchison 2013). Most significantly, GASB 34 requires governments to combine governmental and business-type activities into accrual-basis government-wide financial statements, report governmental activities

The authors appreciate comments from Christopher S. Armstrong (editor) and two reviewers, as well as Arthur Allen, Sudipta Basu, Christine Cuny, Daniel Garrett, Jacquelyn Gillette, Angela Gore, Michael Granof, Bart Hildreth, Ryan McDonough, Dermot Murphy, Anya Nakhmurina, Jim Naughton, Linda Parsons, Rachna Prakash, Pavel Teterin, Joe Weber, the 2020 Financial Accounting and Reporting Section and 2020 Government and Nonprofit Midyear Meeting attendees, and workshop participants at the College of William & Mary, George Mason University, Rutgers Business School, Temple University, The University of Alabama, The University of Chicago, The University of Notre Dame Mendoza College of Business Virtual Municipal Finance Workshop, and the University of Rhode Island. We thank the Municipal Securities Laboratory and the Fiscal Research Center at the Andrew Young School of Policy Studies at Georgia State University for access to the Mergent Municipal Bond Securities database. Allison Koester acknowledges generous support from the Saleh Romeih Professorship.

William Baber, Georgetown University, McDonough School of Business, Accounting Area, Washington, DC, USA; Amanda Beck, Georgia State University, Robinson College of Business, School of Accountancy, Atlanta, GA, USA; Allison Koester, Georgetown University, McDonough School of Business, Accounting Area, Washington, DC, USA.

Editor's note: Accepted by Christopher S. Armstrong, under the Senior Editorship of W. Robert Knechel.

Submitted: July 2021
Accepted: January 2024
Early Access: February 2024

using both accrual and modified accrual accounting, and provide management's discussion and analysis (MD&A) following specific guidelines. Both GASB and independent financial intermediaries emphasize that GASB 34 provides financial reporting and disclosure standardization that increases comparability among governments ([Governmental Accounting Standards Board \(GASB\) 1999](#), ¶344(e); [Jacob 2004](#)). Despite the standard's significance, no prior study comprehensively examines whether or how GASB 34 reporting and disclosure requirements influenced governments' financing decisions in anticipation of this mandated accounting change. We posit that municipal debt issuers anticipate debt market consequences of GASB 34 disclosures; we find that these anticipated consequences manifest as changes in debt financing profiles.

We conjecture that municipal managers, as informed insiders, anticipate outside investor responses to GASB 34 information and then incorporate these expected responses into decisions about whether and when to issue uninsured debt. We find that profiles of government debt issues during the period that encompasses GASB 34 implementation support this characterization. First, the variance of distributions of new uninsured debt issues increases following GASB 34 implementation, which is consistent with the notion that municipal managers differentially anticipate and respond to GASB 34 implementation. Second, changes in the time series of unexpected new uninsured debt issues by governments during the period surrounding initial GASB 34-compliant financial disclosures are atypical. We exploit these findings to infer municipal manager expectations about GASB 34 consequences for their governments. We reason that managers who expect incrementally favorable assessments by prospective investors strategically defer new debt issues until GASB 34-compliant financial statements become publicly available, whereas managers who expect incrementally unfavorable assessments by prospective investors accelerate debt issues to before GASB 34-compliant disclosures are revealed. We document predictable differences in the post-GASB 34 use of debt insurance and the choice between debt and alternative financing sources, conditional on inferred expectations. These results suggest that municipal managers anticipate and respond strategically to incremental separation in municipal debt markets created through accounting standardization.

A distinguishing feature of the study is that we consider state governments and local governments (including special district governments) within a state to be a nexus of financially and politically interrelated jurisdictions that collaboratively provide and finance public services to overlapping constituencies. This perspective, which is common in the municipal finance literature,¹ recognizes and accommodates three characteristics of U.S. state and local governments. First, constituents are indifferent as to which governmental entity—state, local, or special district—provides public services, issues debt, or collects taxes. Second, jurisdictions within a state can shift public services and associated debt burdens among existing and/or newly created governmental entities. Such behavior creates the potential for “off-balance sheet” financing that studies focusing on only state governments or only substate entities (i.e., local or special district entities) can inadvertently overlook. Finally, the extent that state governments impose, monitor, and enforce restrictions on how and whether subjurisdictions issue debt varies between states, potentially compromising between-state comparisons of state or substate governmental entities.²

These points imply that state boundaries delineate the lowest level where sample observations are independent. Thus, we treat debt capacity as a “fiscal common resource” shared by overlapping jurisdictions within a state ([Greer 2015](#)). We aggregate debt issued by all state, local, and special district governments within each of the 50 states to obtain observations that are unaffected by differences in public services and financing policies among financially interrelated state and substate jurisdictions. These aggregated amounts—designated *governments* in the exposition that follows—indicate changes in the nonfederal government debt burden on citizens within each state during each period.³

The research design also considers that GASB 34 implementation unfolds gradually. Governments with 1999 total revenues greater than \$100 million—those with the most frequent and economically substantive debt issues—were first required to provide GASB 34-compliant financial reports for the fiscal year ending on or after June 15, 2002 (FY2002). After confirming GASB 34 adoption by states and large local entities, we identify FY2003 as the period when municipal

¹ Studies that aggregate municipal debt issues to the state level include [Bahl and Duncombe \(1993\)](#); [Denison, Hackbart, and Moody \(2009\)](#); [Fisher and Wassmer \(2014\)](#); [Greer and Denison \(2016\)](#); [Hackbart and Leigland \(1990\)](#); [Johnson \(1999\)](#); [Regens and Lauth \(1992\)](#); and [Shi and Hendrick \(2020\)](#).

² The U.S. Constitution delegates the creation, oversight, and control of governments within (but not across) state boundaries to state government officials. Consistent with this view, the Pew Charitable Trust asserts, “[state primary governments] can retain most of the power to borrow or share this responsibility with other entities” ([Pew Charitable Trusts \(Pew\) 2017](#), 11). State constitutions and/or statutes often limit the amount of debt that local governments may issue ([T. Patton, S. Patton, and Waymire 2022](#)), such that “a state may choose to rely heavily on component unit debt [or] expect local governments and other substate entities such as school districts to borrow for capital projects within their jurisdictions” ([Pew 2017](#), 11). Owing to this potential shifting, constitutional debt limits have “limited impact on a state’s total debt” ([Pew 2017](#), 26).

³ This approach implies that, from a lender-investor perspective, state government financial condition is relevant for assessing debt issues by local governments within the state (and *vice versa*).

debt investors were first able to access and compare GASB 34-compliant financial reports for economically important municipal debt issuers.⁴

The empirical analyses address characteristics of municipal debt issues during the nine years centered on FY2003. We present the results in two parts. First, we compare distributions of uninsured debt issues encompassing the first year that they publicly disclose GASB 34-compliant financial statements. We find statistically reliable changes in distributions of both the amount and the timing of new uninsured debt issues surrounding this period. We exploit both pre- and post-GASB 34 debt-issuing behavior to infer government expectations about the consequences of GASB 34 disclosures. We classify governments with abnormally high amounts of uninsured new debt before, and abnormally low amounts of uninsured new debt after, GASB 34 implementation as governments expecting adverse debt market consequences. In contrast, we classify governments with abnormally low uninsured new debt before, and abnormally high uninsured new debt after, GASB 34 implementation as governments expecting favorable debt market consequences.

Next, we consider whether two debt financing characteristics—the use of third-party debt insurance (Gore, Sachs, and Trzcinka 2004; Cuny 2016) and the substitution of debt for alternative financing sources (taxes, user fees, and inter-governmental transfers) (R. Musgrave and P. Musgrave 1989)—differ predictably post-GASB 34 between governments expecting favorable versus expecting adverse GASB 34 consequences. We find that, compared to governments anticipating favorable consequences, governments anticipating adverse consequences increase their use of third-party debt insurance and decrease their use of new debt relative to alternative financing sources following GASB 34. These results are consistent with the notion that atypical debt-issuing behavior surrounding GASB 34 implementation reveals municipal manager expectations, which are realized following GASB 34 implementation.

Additional, procedures corroborate interpretations of the core results. First, we apply falsification procedures to demonstrate that associations are unique to the GASB 34 implementation period. More specifically, results are absent when (1) randomly assigning treatment and control time periods; (2) randomly assigning governments' classifications as expecting adverse, favorable, or neutral GASB 34 consequences; and (3) applying the empirical design using data from nine-year periods immediately before and after the sample period when GASB 34 is implemented. Second, we verify that economy-wide time trends do not explain post-GASB 34 changes in the substitution between uninsured and insured debt or between debt and other financing sources. Third, we demonstrate that results are robust when using only bond issues by state level entities, alleviating concerns that the core results are a consequence of aggregating state and local data within a state boundary. Finally, we show that changes in the yield premiums for new debt issues by state governments are consistent with interpretations of the primary results.

An important empirical design choice is that we use new public debt issues, rather than bond ratings or bond yields, to infer and evaluate GASB 34 consequences. Governments exist to provide services to constituents, rather than to create wealth for shareholders. Therefore, changes in debt service costs created by GASB 34 disclosures can manifest in ways other than as changes in residual claims. For example, governments that experience reductions in prevailing debt costs attributable to GASB 34 can “redistribute” these savings by issuing debt to finance new programs, causing interest rates on new debt to revert to politically and financially acceptable levels that existed before GASB 34. This redistribution would result in no change to bond yields on new debt issues post-GASB 34. Moreover, favorable (adverse) GASB 34 effects can cause governmental entities to enter (withdraw from) public debt markets. If so, then comparisons of bond ratings and bond yields are compromised, as we can observe neither bond ratings nor bond yields on debt not issued. Thus, we consider the source of such endogeneity directly—more specifically, we use new public debt issues (rather than debt ratings or yield premia) to assess GASB 34 effects.

This study advances the governmental accounting literature both conceptually and methodologically. In contrast with prior studies that investigate specific GASB 34 provisions for specific classes of governmental entities (Plummer, Hutchison, and Patton 2007; Vermeer, Patton, and Styles 2011; Callahan and Waymire 2015; Bloch 2016; McDonough and Yan 2021), we characterize GASB 34 broadly as a comprehensive set of standards that substantially and systemically changed the state and local government financial reporting landscape. GASB 34 standardized accounting practices, which modified investors' assessments and—by extension—changed the way governments engaged municipal debt markets. Although prior studies also consider strategic behavior in response to governmental accounting standards (Baber and Gore 2008; Kido, Petacchi, and Weber 2012; Costello, Petacchi, and Weber 2017; Beck 2018), we are the first to provide evidence of strategic behavior by governments both in anticipation of, and in response to, consequences that result from financial reporting standardization and disclosure. This contribution transcends governmental accounting to

⁴ We find that 47 of the 50 state governments and 30 of 37 largest city governments provide GASB 34-compliant annual comprehensive financial reports (ACFRs) during FY2003 (between July 1, 2002 and June 30, 2003). State governments and large cities are more likely to issue debt than smaller local subunits (not tabulated), which further justifies designating FY2003 as the period when GASB 34-compliant financial statements are first publicly disclosed.

the extent that our evidence informs thinking about how financial reporting entities, in general, respond to mandated accounting standards.

We also introduce two novel empirical approaches into the governmental accounting literature. First, our research design considers the possibility that state, local, and special district governments respond collaboratively to opportunities and challenges created through GASB 34 implementation. Failure to accommodate such collaborative behavior potentially undermines researchers' and policy makers' abilities to detect consequences resulting from governmental financial reporting standardization and disclosure practices. Second, we direct attention to endogeneity concerns that potentially result from the use of debt costs and bond ratings to investigate accounting method consequences. To address these concerns, we advance measures based on debt *issuing* behaviors. We reason that these measures more directly indicate governments' expected GASB 34 outcomes than debt costs or bond ratings.

II. FINANCIAL REPORTING CONSEQUENCES OF GASB 34

ACFRs⁵ inform outsiders, including municipal debt investors, about both *governmental activities* (e.g., raising tax revenues to finance public services such as education and safety) and *enterprise activities* (e.g., charging user fees to finance services such as utilities). Prior to GASB 34, most governments used only fund accounting to report governmental activities. Fund accounting focuses on increases and decreases in financial resources and facilitates short-term stewardship and accountability by comparing annual budgets (organized by “funds” with statutorily designated purposes) to actual results (GASB 1999). Short-term focus does not provide outsiders—including potential investors in publicly traded municipal debt—with a long-term financial picture, however. Voluntary accounting and disclosure practices, coupled with independent audits, are potential solutions to financial accounting and disclosure problems (Healy and Palepu 2001). The Board argued, however, that substantial pre-GASB 34 differences in the presentation, usefulness, and clarity of accounting and reporting created a barrier to achieving meaningful standardization without standard-setter involvement (Governmental Accounting Standards Board (GASB) 2023).⁶

Thus, GASB 34 was adopted in June 1999, with implementation by large governments required for the first fiscal year ending on or after June 15, 2002 (GASB 1999). The standard retains fund accounting for general governmental activities and adds the requirement of accrual-based government-wide financial statements. Application of accrual accounting caused most governments to recognize long-term assets (e.g., infrastructure) and long-term liabilities (e.g., pensions and compensated absences) in their financial statements for the first time. The standard also requires reporting entities to disclose events that create long-term assets and liabilities during the reporting period, reconcile modified accrual and full accrual accounting reports, and prepare a MD&A with standard components using a specified format. Finally, the standard retains the requirement to compare final general fund budgets with actual revenues and expenditures and adds a new requirement to present the original budget alongside final budgeted amounts (GASB 1999).

At the conceptual level, standardization brings information “into conformity...to assure consistency and regularity.”⁷ In the financial reporting context, standardization facilitates credible communication of the financial implications of governmental decisions and activities to outsiders (GASB 1999; Akerlof 1970; Spence 1973; Diamond and Verrecchia 1991). Borrowing from Barth and Schipper (2008) and Armstrong, Barth, Jagolinzer, and Riedl (2010), which consider the potential consequences of accounting standardization in the context of International Financial Reporting Standards, we advance competing perspectives regarding the consequences of regulated accounting standards. One perspective—the perspective advanced by GASB—is that accounting standards reduce information asymmetry and facilitate comparisons among governments that better inform investors in municipal debt markets.⁸ That is, GASB 34 resolves a pre-existing

⁵ The term “comprehensive annual financial report” is replaced by “annual comprehensive financial report” (ACFR) for fiscal years ending after December 15, 2021 (Governmental Accounting Standards Board (GASB) 2021).

⁶ GASB notes that pre-GASB 34 financial reporting differences present to financial statement users: “The fragmented nature of [pre-GASB 34] governmental fund reporting makes it difficult for most users of the financial statements to obtain a clear picture of total governmental activities...[and] reduce[s] users' ability to compare information reported for the same funds or fund types over time, as well as...across similar governments” (GASB 1999, ¶248; emphasis added).

⁷ <https://www.merriam-webster.com/dictionary/standardization>

⁸ GASB suggests that “comparability among governments should be enhanced, because all governments will be required to report their operating results using a single measurement focus...Comparisons of governments offering similar programs will no longer be affected by differences in measurement focuses and bases of accounting based on which funds each government uses...[GASB 34] also improves comparability by requiring aggregated reporting of information that is otherwise disaggregated” (GASB 1999, ¶344(e); emphasis added). Supporting this characterization, Jacob (2004, 26–27) notes that “GASB 34 allows for much more meaningful comparisons between governments because a government's financial results in their entirety are being captured in the new statements.” In addition, exposure draft comment letters note that GASB 34 “brings a level of standardization to governmental financial reporting that will provide sorely-needed comparability to the credit evaluation process” (letter #343 by Robert Morris Associates; emphasis added) and that “traditionally the market has subsidized weaker borrowers through poor disclosure and penalized better borrowers...basically because there is not a see through type of situation available now through the financial statements” (letter #104 by State Farm Insurance) (both available on request from GASB and the authors).

collective action problem by providing an auditable set of standard accounting and reporting practices that greatly elevated financial reporting by governments. Another perspective, however, is that standardization undermines financing reporting quality by ignoring local distinctions among governments that engendered differences in pre-GASB 34 accounting and reporting practices (Healy and Wahlen 1999).⁹ Thus, we investigate whether and how state and substate debt issuing entities respond to financial reporting standardization and comparability anticipated from GASB 34.¹⁰

III. HYPOTHESES DEVELOPMENT

We address a series of hypotheses to evaluate the notion that governments anticipate and realize separation in municipal debt markets following disclosure of GASB 34-compliant financial reports. The first three hypotheses consider governments' uses of uninsured debt. When deciding whether to purchase third-party debt insurance, governments balance the cost of obtaining insurance, which is set by insurers, against the interest cost of issuing uninsured debt, which is determined by investors. GASB 34 disclosures are not likely to substantially affect the cost of issuing insured debt for at least two reasons. First, regarding insurance costs, the extent that standardized financial reporting improves insurers' risk assessments is of little consequence because insurers diversify customer portfolios to minimize portfolio risk. Second, regarding insured debt interest costs, investors lack incentives to monitor financial disclosures because the insurer provides ultimate recourse to investors. These points suggest that debt market consequences of government financial reporting standardization primarily materialize in relation to uninsured debt.

We use the term *new uninsured debt effects* to reference pre- versus post-GASB 34 changes in the amount and the timing of new uninsured debt per capita. We posit that governments that anticipate favorable consequences increase, whereas governments that anticipate adverse consequences decrease, new uninsured debt issues per capita following GASB 34. H1 and H2 consider the amount and timing of new uninsured debt, respectively.

First, if GASB 34 fosters separation in municipal debt markets, we expect dispersion of the distributions of new uninsured debt issues to increase following GASB 34 implementation.

H1 (Amount of New Uninsured Debt Effect): Dispersion of new uninsured debt issues increases following GASB 34.

Next, we consider the timing of changes in unexpected debt issues surrounding GASB 34. We presume that financial managers of governments (as insiders) anticipate GASB 34 consequences better than outsiders (e.g., investors and analysts).¹¹ The standard was adopted in June 1999 with the earliest implementation in fiscal years ending on or after June 15, 2002. Thus, governments had ample time to assess the potential consequences of GASB 34-compliant financial reports and incorporate these assessments into decisions that affect the timing of new uninsured debt issues. Governments expecting favorable GASB 34 consequences have incentives to delay uninsured debt issues to post-GASB 34 periods, whereas governments expecting adverse consequences have incentives to accelerate uninsured debt issues to pre-GASB 34 periods. If so, then strategic behavior in response to such incentives manifests empirically as unexpected changes in the timing of new uninsured debt issues surrounding the period when governments first publicly disclose GASB 34-compliant ACFRs.

H2 (Timing of New Uninsured Debt Effect): The distribution of unexpected new uninsured debt issues immediately before and after GASB 34-compliant ACFRs are released differs from distributions during adjacent time periods.

Insider expectations about GASB 34 consequences are unobservable. Thus, we exploit the results from testing H2 to infer managements' assessments of expected GASB 34 consequences. In particular, we classify governments that delay (accelerate) abnormal new debt issues as governments that expect relatively more favorable (adverse) consequences

⁹ Consistent with the notion that standardization can compromise financial reporting, the National Association of State Auditors, Comptrollers, and Treasurers initially expressed "strong objection" to GASB 34 through the comment letter process (letter #39). Notwithstanding these initial objections, all 50 state governments implemented GASB 34 according to the recommended implementation provisions.

¹⁰ Prior studies investigate specific GASB 34 provisions. Plummer et al. (2007) find that government-wide net assets disclosures provide decision-relevant information for 530 Texas school districts. Vermeer et al. (2011) consider infrastructure reporting by state governments following GASB 34 implementation. In contrast, we investigate debt issues and alternative financing sources before and after GASB 34 implementation for all state, local, and special district governments.

¹¹ GASB itself recognized the information advantage held by insider managers. Specifically, the preamble to GASB 34 states "the financial managers of governments are knowledgeable about the transactions, events, and conditions that are reflected in the government's financial report and of the fiscal policies that govern its operations. For the first time, those financial managers will be asked to share their insights [in a MD&A]" (GASB 1999, 2). Accordingly, Bloch (2016) finds in a survey of municipal analysts that the MD&A is among the most valuable new disclosures provided by GASB 34, with most reporting that GASB 34 improved the ability to evaluate government finances.

from GASB 34. If prospective investors and insurers rationally incorporate new GASB 34 information according to insider expectations and insider expectations are realized after GASB 34 compliant information is disclosed publicly, then we expect profiles of new debt issues change in at least two ways.

First, our reasoning suggests that GASB 34 consequences primarily affect the marginal cost of uninsured (rather than insured) debt. If so, the trade-off between the costs of issuing insured versus uninsured debt are perturbed. We expect governments that anticipate favorable GASB 34 consequences prior to GASB 34 implementation reduce reliance on debt insurance following GASB 34 implementation relative to governments that anticipate unfavorable consequences.¹² We use the term *insurance substitution* to describe such changes in the use of debt insurance.

H3 (Insurance Substitution): Governments expecting adverse GASB 34 consequences increase the relative use of insured new debt to total new debt following GASB 34, compared to governments expecting favorable GASB 34 consequences.

Second, our reasoning suggests that revelation of GASB 34 information has consequences for the marginal cost of debt. If so, the equilibrium trade-off between the cost of debt versus other financing alternatives is affected following GASB 34 implementation. We expect governments that anticipate favorable GASB 34 consequences prior to GASB 34 implementation substitute debt for alternative financing sources following GASB 34, relative to governments anticipating adverse consequences. We introduce the term *financing substitution* to describe this phenomenon.

H4 (Financing Substitution): Governments expecting adverse GASB 34 consequences increase the relative use of alternative financing sources to new debt following GASB 34, compared to governments expecting favorable GASB 34 consequences.

We emphasize that the behaviors posited by H3 and H4 do not result directly from managers *anticipating* GASB 34 consequences. Rather, changes in the use of debt insurance and in the substitution between alternative financing sources and debt financing result when pre-GASB 34 expectations are *realized*. Finally, expectations indicated by the foregoing hypotheses are attenuated possibilities that GASB 34 disclosures cause governments to enter or exit public debt markets post-GASB 34. For example, the ability to detect predicted GASB 34 changes in the use of third-party insurance is compromised if governments anticipating adverse GASB 34 consequences abandon the use of debt financing rather than increase the use of insurance.

IV. DATA

Data Sources and Sample Composition

Table 1, Panel A summarizes the sample selection process. We use the Mergent Municipal Bond Securities (Mergent) database to identify new municipal debt issues (including refinancing debt) for all state governments, local governments (i.e., cities and counties), and special district governments during the nine-year period July 1, 1998 through June 30, 2007 (FY1999 through FY2007). Excluding issues with missing identifiers (e.g., issuer name or state), with missing debt characteristics (e.g., principal amount), and bonds issued by Washington, DC, Native American tribes and reservations, and U.S. territories and protectorates leaves 1,369,494 bond issues (38,261 unique issuers). Excluding conduit issues where the primary obligator is a private entity (e.g., nonprofit hospitals and private schools) and not the issuing government yields 1,230,550 debt issues (34,562 issuers). This sample is used to investigate insurance effects (H3) and alternative financing sources (H4). We exclude insured issues when considering H1 and H2. Omitting insured issues yields 507,911 uninsured debt issues by 23,192 issuers. Panel B shows how we aggregate individual debt issues within a state boundary and quarter (year) to create 1,600 government-quarter (400 government-year) observations used to investigate H1 through H3 (H4).

Following prior studies (Baber and Gore 2008; Cuny 2016; Beck 2018; J. Cornaggia, K. Cornaggia, and Israelsen 2018), the sample includes both general obligation and revenue bonds.¹³ As a practical matter, the distinction between general obligation and revenue debt issues is not straightforward (Patton et al. 2022). For example, perhaps to demonstrate fiscal responsibility in political markets, state legislatures sometimes restrict the use of general obligation debt, and some states (e.g., Arizona, Indiana, and West Virginia) proscribe the use general obligation debt by the state

¹² Related, prior studies argue—and provide supporting evidence—that financial disclosure and municipal bond insurance are substitutes (Gore et al. 2004; Cuny 2016).

¹³ General obligation bonds are typically secured by the full faith, credit, and taxing power of a municipal issuer and serviced with tax revenues, whereas revenue bonds are secured by and serviced through a single specific revenue stream, such as revenue from highway or bridge tolls (Pew 2017, 11).

TABLE 1
Sample Selection

Panel A: Individual New Debt Issues

	<u>Number of Issues</u>	<u>Number of Issuers</u>
Universe of new debt issues by state and local governments from July 1, 1998 through June 30, 2007 in the Mergent database	1,369,494	38,261
Less: Conduit debt issues	(138,944)	(3,699)
Sample for tests of H3 (insurance substitution) and H4 (financing substitution)	1,230,550	34,562
Less: Insured issues	(722,639)	(11,370)
Sample of uninsured debt issues for all other analyses	507,911	23,192

Panel B: Data Aggregation for Empirical Analyses

	<u>Government-Quarter Observations</u>
Aggregation: 50 state boundaries × 9 years × 4 quarters/year	1,800
Less: Four FY2003 quarters when GASB 34-compliant ACFRs are first publicly available (50 state boundaries × 4 quarters/year)	(200)
Unit of analysis for investigating H1 through H3	1,600
	<u>Government-Year Observations</u>
Aggregation: 50 state boundaries × 9 years	450
Less: First year that GASB 34-compliant ACFRs were publicly disclosed (FY2003; 50 state boundaries × 1 year)	(50)
Unit of analysis for investigating H4	400

This table explains our sample selection.

government units entirely. To circumvent such restrictions, governments can and do issue revenue debt secured, like general obligation debt, by tax collections. In such cases, revenue debt does not differ substantively from general obligation debt.¹⁴ Moreover, governments can increase taxes to service general obligation debt, but can have limited ability to increase nontax revenues to service revenue debt. Thus, a case can be made that general obligation debt affords greater collateral security to investors than does revenue debt. Regardless, the relative importance of GASB 34 information to investors for each bond type is unclear *ex ante*.

We do not focus on changes in bond ratings or bond yields for both conceptual and empirical reasons. Conceptually, given governments' focus on providing services to constituents (rather than on generating residual claims), changes in debt service costs that result from new GASB 34 information affect the marketability of new debt to fund public services. For example, a favorably affected government could experience reduced marginal debt costs and elect to enter the public debt market or increase new debt issues until bond interest rates and debt ratings revert to politically and financially acceptable levels that existed before GASB 34. The opposite can happen for governments adversely affected following GASB 34—that is, governments that experience increases in marginal debt service costs either exit the public debt market or reduce the use of debt financing until interest rates and bond ratings achieve pre-GASB 34 levels. Such endogenous behavior offsets GASB 34 debt cost consequences realized through GASB 34, and therefore undermines the ability to detect both bond yield and bond rating changes that result from disclosing new GASB 34 information.

Moreover, as a practical matter, credit ratings and yields for new debt issues are missing from the Mergent data in a way that potentially biases time-series comparisons. On average, credit ratings are not reported for 61 percent of individual issuances during the four years before GASB 34-compliant ACFRs were publicly disclosed (FY1999 through FY2002) and 37 percent of individual issuances during the four years following (FY2004 through FY2007). Similarly, missing yields are 14 percent in the pre-34 period and 5 percent in the post-34 period (not tabulated). The possibility that

¹⁴ For example, revenue bonds can be secured by general purpose revenue pledges (e.g., property taxes). Moody's uses its general obligation bond methodology to rate the revenue bonds secured by such pledges (Moody's Investors Service 2016).

changes in the availability of interest rates and bond ratings for new debt issues are systematic raises concerns about biases in pre- versus post-GASB 34 comparisons.

Unit of Observation

We aggregate debt issues for all governments within a state to obtain 50 independent state-level observations for each quarter-year. This approach recognizes that state and local governments and special districts respond collaboratively to citizen demands for public services within (but not across) state boundaries (Pew Charitable Trusts (Pew) 2016, 2017). State and local governments and special districts can and do shift public service responsibilities and resulting financing between and among new and/or existing jurisdictions.¹⁵ Such shifting can be conspicuous (e.g., a general government creates a special district to administer a public service and the special district issues the debt required to fund the service) or subtle (e.g., a local government assumes a public service discontinued by the state government and issues related debt). Moreover, states differentially regulate and monitor financial practices of subordinate entities (Pew Charitable Trusts (Pew) 2013, 2016). In the end, subjurisdictions within a state are financially and politically interrelated and, as such, respond to financial reporting standardization collectively.¹⁶ Although some governmental accounting studies justifiably consider state and substate entities independently, doing so in our setting potentially obscures hypothesized effects. In contrast, even if subjurisdictions anticipating adverse consequences shift debt burdens to subjurisdictions anticipating favorable consequences, our approach still captures the increase in debt. To ease the exposition, we use the term *governments* when referencing the nexus of state and local governments and special districts located within a state's boundaries.¹⁷

Setting the Disclosure Period for GASB 34-Compliant ACFRs

Three features of the state and local governmental financial reporting environment are relevant for aligning the data. First, GASB 34 imposed staggered effective dates, requiring the largest governments (those with FY1999 revenues exceeding \$100 million) to use the new reporting framework for fiscal years ending on or after June 15, 2002. Governments with FY1999 revenues between \$10 and \$100 million (less than \$10 million) were required to implement for fiscal years ending on or after June 15, 2003 (2004). An empirical design that exploits the staggered effective dates is problematic owing to lack of reliable data about FY1999 GAAP-basis revenues, ACFR transmittal dates, and GASB 34 compliance for the full population of municipal issuers.

To gauge the economic importance of governments *likely* to be in each phase of GASB 34 implementation, we use revenues from the Census of Governments' Government Finance Survey (COG GFS) for a 1999 sample of cities and counties. Mean (median) population for entities required to implement GASB 34 in FY2002 is approximately 180,000 (60,000). In contrast, mean (median) population for likely FY2003 implementers is approximately 14,000 (4,000), and the mean (median) population for likely FY2004 implementers is 2,500 (407).¹⁸ Thus, although GASB 34 implementation unfolds over a three-year period, most entities likely to issue debt were likely required to first implement GASB 34 for the fiscal year ending on or after June 15, 2002.¹⁹

A second feature relevant for setting the timing of GASB 34 implementation is that fiscal year ends vary both between and within states. Ninety-two percent of state governments have June 30 fiscal year ends, and 62 percent

¹⁵ See footnote 1 for representative studies in the municipal finance literature that also aggregate debt issues within state boundaries. To provide specific examples, Walsh (2012); Cournoyer (2013); and Goodman and Leland (2019) document how state and local governments shift debt burdens from general governments to authorities that administer specific public services; Pew (2013, 2016) describe policies and practices of state assumption of subjurisdiction obligations; Greer and Denison (2016) find that states differ in how the debt burden is shared between state governments and local governments and that concentrating the overall debt burden at the state government level promotes fiscal stabilization at the cost of debt management being less responsive to local economic conditions; Baird (2017) describes Alaska's proposal to shift debt issues earmarked for education from the state government to local school districts; Shi and Hendrick (2020, abstract) aggregate total debt levels of state and local governments within a state and year to document that "over-borrowing bias emerges when the state fiscal base is shared by multiple general-purposes and special-purpose jurisdictions."

¹⁶ To illustrate, Boise City, Idaho (2002, 6) notes in its 2002 ACFR that "cities are 'creatures of the state' authorized for organization by the Idaho Constitution."

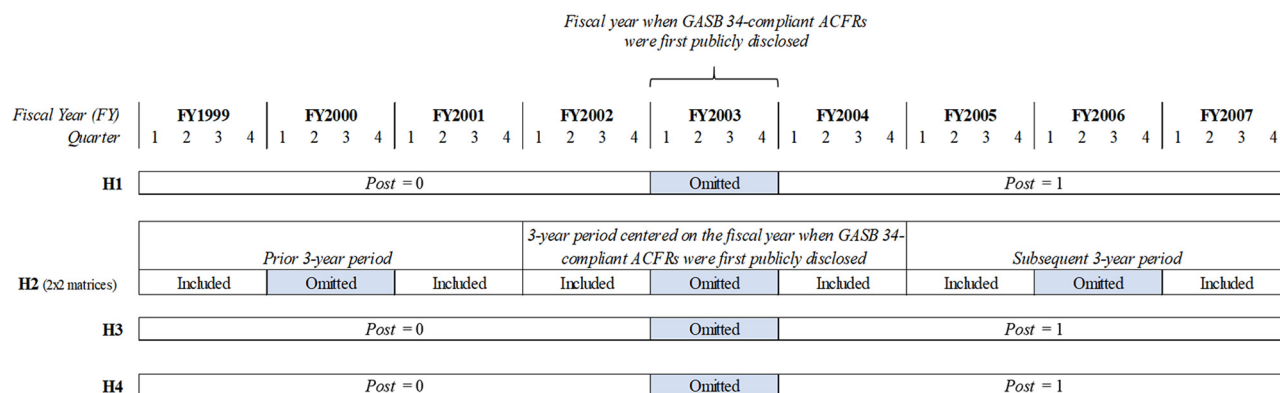
¹⁷ This characterization does not imply that subjurisdictions cannot be autonomous legal and political entities that issue and service debt independently. This characterization does imply, however, that informed financial assessments of a debt-issuing governmental entity require financial assessments of other governmental entities within the state.

¹⁸ Using census data to provide an example, Round Rock City, TX (located 21 miles north of Austin) had a 2000 population nearly identical to the median population for likely FY2002 implementers and issued debt during our sample period. In contrast, Castle Hills, TX (in the San Antonio metropolitan area), which has a 2000 population nearly identical to the median population for likely FY2003 implementers, did not issue debt during the sample period (<https://www.tsl.texas.gov/ref/abouttx/popcity12000.html>).

¹⁹ Many studies eliminate governments with small populations because these governments rarely issue debt (e.g., Baber and Gore 2008; Beck 2018), further supporting the proposition that most important debt issuing governments likely adopted GASB 34 during the first implementation period.

FIGURE 1

Timeline



This timeline illustrates how the sample (FY1999 through FY2007) is deployed for each hypothesis. Fiscal year refers to July 1 through June 30 (e.g., FY2003 is from July 1, 2002 through June 30, 2003). As GASB 34-compliant ACFRs for the largest governments became publicly available at various dates throughout FY2003, FY2003 is omitted when considering pre- versus post-GASB 34 periods.

Variables are defined in the [Appendix A](#).
(The full-color version is available online.)

(32 percent) of the largest city governments in each state have June 30 (December 31) fiscal year ends (not tabulated). The third feature relevant for considering disclosure dates is that, unlike publicly traded companies, governments face no federal requirement to disclose audited financial statements. Thus, the reporting lags between fiscal year ends and ACFR publication can vary even among governments with the same fiscal year end.

In deference to these features of the financial reporting landscape, we argue that GASB 34 information influences assessments by municipal debt market participants when the preponderance of economically significant issuers first provides GASB-34 compliant ACFRs publicly. To determine when this occurs, we obtain financial reports for all state governments and the largest city government within each state (based on 2000 Census population) for fiscal years ending on or after June 15, 2002. We confirm that all 50 state governments, and 35 of the 37 city governments for which we can obtain FY2002 ACFRs are GASB 34-compliant as of FY2002. We find further that 47 of the 50 state governments and 30 of the 37 largest city governments transmit GASB 34-compliant FY2002 ACFRs between July 1, 2002 and June 30, 2003 (FY2003) (not tabulated).²⁰ As the most economically significant governments provide a GASB-34 compliant ACFR at various dates throughout FY2003, we omit FY2003 observations when executing the empirical analyses. The four fiscal years prior to FY2003 (FY1999 to FY2002) comprise the pre-GASB 34 period ($Post = 0$) and four fiscal years following FY2003 (FY2004 to FY2007) comprise the post-GASB 34 period ($Post = 1$). [Figure 1](#) summarizes how observations are deployed by hypothesis.

Documenting Standardization Provided through GASB 34

Before addressing the hypotheses, we first examine the extent to which GASB 34 increased financial reporting standardization. We identify three specific provisions within GASB 34 that “create new information and...restructure much of the information that governments have presented in the past” ([GASB 1999](#), preface):²¹ (1) a MD&A that includes a standardized presentation of (i) changes in long-term assets and liabilities and (ii) revenues by source and expenditures by function ([GASB 1999](#), ¶11b and ¶11f); (2) disclosure of originally budgeted revenues and expenditures ([GASB 1999](#), ¶130); and (3) delineation of general fund balance reserve by purpose and amount ([GASB 1999](#), ¶84).

²⁰ We accessed various resources (e.g., online search, email and telephone requests, etc.) to obtain these financial reports. Two state governments (Oklahoma and Michigan) and five city governments (Boise City, ID; Portland, ME; New York City, NY; Columbus, OH; and Oklahoma City, OK) implement GASB 34 early in FY2001. Two city governments that did not implement GASB 34 during or before FY2002 (Billings, MT and Cheyenne, WY) implemented the standard in FY2003 (as required by the standard). We find mean of 209 (173) days between the fiscal year ends and transmittal dates for these state (largest city) governments in FY2002. These statistics are in line with other studies (e.g., [Governmental Accounting Standards Board \(GASB\) 2011](#); [C. Edmonds, J. Edmonds, B. Vermeer, and T. Vermeer 2017](#); [Henke and Maher 2016](#)).

²¹ These provisions are only a subset of the comprehensive standardization provided by GASB 34. We focus on voluntary disclosures that existed pre-GASB 34. For example, [Table 2](#) does not consider the GASB 34 requirement to present full accrual government-wide financial statements because no state government voluntarily provided such disclosure before GASB 34.

We hand collect data from two years of state government ACFRs—the first fiscal year that each government implemented GASB 34 and the last fiscal year prior—to investigate and quantify the extent that GASB 34 engendered standardization. These procedures, summarized in Table 2, reveal that in the year prior to (initial year of) GASB 34 implementation, 0 percent (100 percent) of state governments include standardized information about long-term assets, long-term liabilities, and full-accrual revenues by source and expenditures by function. We find also that 12 percent (90 percent) of state governments disclose original budgets for revenues and 14 percent (100 percent) disclose original budgets for expenditures in the year prior to (initial year of) GASB 34 implementation. Finally, 54 percent (92 percent) delineate general fund balance reserves by purpose and amount on the face of the balance sheet in the year prior to (initial year of) GASB 34 implementation. These comparisons indicate greater financial reporting standardization by state governments following GASB 34 implementation on dimensions GASB deemed important.

V. EMPIRICAL ANALYSES

Pre- versus Post-GASB 34 Distributions of New Uninsured Debt Issues (H1)

Using the full sample of 1,800 observations (50 states \times 36 quarters centered on the fiscal year GASB 34-compliant ACFRs are publicly disclosed), we estimate:

$$\text{UninsuredDebtPC}_{it} = \alpha_0 + \beta_1 \Delta \text{GSP}_{it} + \text{State Fixed Effects} + \text{Qtr Yr Fixed Effects} + \varepsilon_{it} \quad (1)$$

where $\text{UninsuredDebtPC}_{it}$ is the aggregate new uninsured debt per capita issued by governments within the boundaries of state i ($i = 1, \dots, 50$) in quarter-year t ($t = 1, \dots, 36$). Continuous variables are bounded at the 1st and 99th percentiles of distributions to attenuate the influence of extreme values. State fixed effects control for mean state-specific effects that potentially influence decisions to issue new debt, such as state oversight of localities. Quarter-year fixed effects control for time-variant effects that potentially influence decisions to issue new debt (e.g., macroeconomic factors including recessions and expansions). State i , quarter t percent change in gross state product (ΔGSP_{it}) controls for the possibility that macroeconomic factors influence government financing decisions (e.g., demographic or macroeconomic changes specific to state-quarters) differentially. We use the term *unexpected uninsured debt* to reference specification (1) residuals.

To consider H1, we compare distributions of unexpected uninsured debt issues for the pre-GASB 34 period (FY1999 through FY2002) versus the post-GASB 34 period (FY2004 through FY2007), excluding FY2003 observations as governments release GASB 34-compliant ACFRs at various dates during FY2003.²² If GASB 34 promotes separation, then we expect greater dispersion in unexpected uninsured debt for the post-GASB 34 distribution.

Figure 2, Panel A shows probability density functions (PDFs) and cumulative density functions (CDFs) for distributions of new unexpected uninsured debt before and after GASB 34 implementation. Panel B provides comparisons of distribution characteristics. The variance of unexpected uninsured debt after GASB 34 is more than twice the variance before GASB 34, supporting H1. The Brown-Forsythe (B-F) test, a nonparametric comparison of distribution variances, indicates the difference is statistically significant ($p < 0.001$). The two-sample Kolmogorov-Smirnoff (K-S) procedure, a nonparametric test of differences between CDFs, also indicates a statistically significant pre- versus post-GASB 34 difference ($p < 0.001$).²³

Timing of Unexpected Uninsured Debt Issues (H2)

Next, we use unexpected uninsured debt extracted from specification (1) to consider changes in time series patterns of new uninsured debt issues during the year before and after GASB 34 (H2). For each government, we cumulate quarterly unexpected uninsured debt for each fiscal year. We use the term *annualized unexpected uninsured debt* to describe this cumulated amount. We then partition the nine-year sample period (FY1999 through FY2007) into three-year periods. Note that the three-year period FY2002 through FY2004 centers on the fiscal year that GASB 34-compliant ACFRs were first publicly disclosed. For the first and third years in each three-year period, we sort the 50 governments according to whether annualized unexpected uninsured debt for the year is above or below the median.²⁴

²² Results for all analyses are robust to including FY2003 observations in the $Post = 1$ subgroup (not tabulated).

²³ To address the concern that budget adjustments during the economic expansion following the 2001 recession affect inferences, we verify that Figure 2 comparisons are robust to omitting FY2002 observations (not tabulated).

²⁴ Use of median residuals to classify governments facilitates statistical comparisons of distributions, as Chi-square tests require identical marginal distributions. Comparisons of distributions using mean or signed residuals are qualitatively similar (not tabulated).

TABLE 2

Comparison of GASB 34-Compliant Disclosures Presented in State Government ACFRs in the Year Prior to versus the Year of GASB 34 Implementation

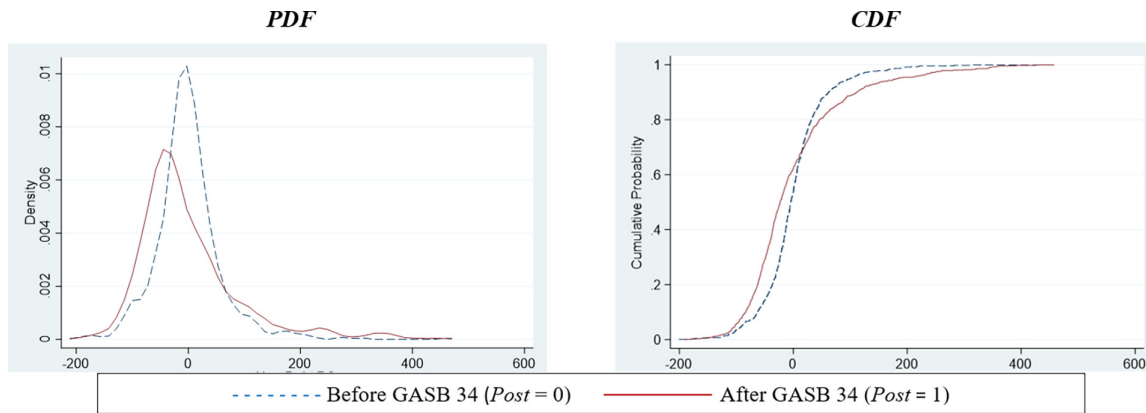
GASB 34 Requirement	Voluntary Disclosure in the Year Prior to GASB 34 Implementation		Compliance in the First Year of GASB 34 Implementation
	Exactly As GASB 34 Subsequently Required	Similar in Spirit to GASB 34 Requirements	
1: Provide MD&A	0%	100% provide a transmittal letter.	100%
1(a): MD&A includes quantification and description of year-over-year change in full accrual long-term capital assets	0%	4% provide <i>some</i> disclosure of changes in total long-term asset balances.	100% ^a
1(b): MD&A includes quantification and description of year-over-year change in long-term liabilities	0%	30% provide <i>some</i> disclosure of changes in long-term liability balances. Significant variance exists in categories included (e.g., general obligation bonds, governmental versus total, pension liabilities).	100% ^a
1(c): MD&A includes comparison of full accrual basis revenues by source and expenses by function	0%	94% of governments provide a similar disclosure using <u>modified</u> accrual basis accounting. Funds included in these comparisons vary substantially: <ul style="list-style-type: none"> • 16%: general fund only; • 52%: total governmental funds only; and • 26%: both general fund and total governmental funds. 	100%
2: Disclose original budget revenues and expenditures	12% for revenues and 14% for expenditures	NA	90% for revenues and 100% for expenditures
3: Disclose detailed reasons for the government general fund reserve balance	54% on the balance sheet and 44% only in the notes	NA	92% on the balance sheet and 8% only in the notes

^a Note that in FY2002, governments report the full accrual long-term assets and liabilities as of year-end (and not a year-over-year change, as changes are incalculable in the initial year).

FIGURE 2
Distributions of Unexpected New Uninsured Debt Per Capita

Panel A: PDF and CDF of the Specification (1) Residuals When $Y = UninsuredDebtPC_{it}$

$$UninsuredDebtPC_{it} = \alpha_0 + \beta_1 \Delta GSP_{it} + State\ Fixed\ Effects + Quarter - Year\ Fixed\ Effects + \varepsilon_{it}$$



Panel B: Descriptive Statistics for the Specification (1) Residuals When $Y = UninsuredDebtPC_{it}$, by Post

Moment	Before GASB 34 (Post = 0)	After GASB 34 (Post = 1)	B-F Statistic	K-S Statistic
Variance	3,516.821	8,295.210	55.272***	
Skewness	1.344	1.731		
Kurtosis	10.309	7.100		
				0.2175***

***, **, * Indicate parameters statistically significant at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively. This figure compares distributions of unexpected new uninsured debt per capita before ($Post = 0$) versus after ($Post = 1$) GASB 34 implementation. Panel A presents the PDF and CDF for residuals obtained from estimating OLS specification (1) where the dependent variable is $Y = UninsuredDebtPC_{it}$, defined as aggregate new uninsured debt per capita for government i in quarter-year t . Note that specification (1) includes quarter-year fixed effects, which ensures mean residuals (mean unexpected new uninsured debt per capita) = 0 during each of the pre- and post-GASB 34 periods. Panel B presents statistics for higher order moments of distributions. The B-F statistic rejects the null hypothesis that $Post = 0$ and $Post = 1$ distribution variances are equal. Variables are defined in Appendix A. (The full-color version is available online.)

Table 3, Panel B shows the 2×2 classifications of the 50 governments according to whether annualized unexpected uninsured debt exceeds distribution medians for the first and third years of each period. In contrast with an equal distribution, we find that distributions encompassing GASB 34 implementation more frequently concentrate on the southwest-northeast diagonal (15 and 15 governments) than on the northwest-southeast diagonal (ten and ten governments) (column (2)). Thus, governments that decrease (increase) unexpected uninsured debt issuance during the year prior to disclosing a GASB 34-compliant ACFR are more likely to increase (decrease) unexpected uninsured debt during the year following the GASB 34-compliant disclosure.

Next, we compare the 2×2 distribution during FY2002 through FY2004 (column (2)) with distributions for the three-year periods before (FY1999 through FY2001; column (1)) and after (FY2005 through FY2007; column (3)). Observations in columns (1) and (3) less frequently fall on the northeast-southwest diagonal. Chi-square tests indicate that the distribution during FY2002 through FY2004 (column (2)) differs reliably from the distributions in the adjacent three-year periods ($p < 0.001$ when compared with column (1) and $p = 0.089$ when compared with column (3)). Thus, we reject the null hypothesis that uninsured debt-issuing behavior surrounding the initial year of GASB 34-compliant ACFR disclosures is the same as the adjacent periods in favor of the H2 prediction that debt-issuing behavior is unusual during GASB 34 implementation.

Classifying Governments According to Strategic Behavior

We use the atypical distribution of new uninsured debt surrounding the GASB 34 implementation period to classify governments as expecting favorable or adverse consequences. Using the Table 3 results (Panel B, column (2)), we

TABLE 3
Unexpected New Uninsured Debt Per Capita

Panel A: Descriptive Statistics

Variables	n	Mean	Std. Dev.	P25	P50	P75
<i>UninsuredDebtPC_{it}</i>	1,800	102.994	98.591	38.142	73.425	132.404
ΔGSP_{it}	1,800	1.337	0.559	0.973	1.266	1.623

Panel B: Comparing Unexpected New Uninsured Debt Issues during Adjacent Three-Year Periods

Prior 3-Year Period (FY1999 through FY2001) (1)		3-Year Period Centered on the fiscal year when GASB 34-compliant ACFRs were first publicly disclosed (FY2002 through FY2004) (2)				Subsequent 3-Year Period (FY2005 through FY2007) (3)					
		Residual _{FY2001}		Residual _{FY2004}		Residual _{FY2007}					
		Below	Above	Below	Above	Below	Above				
Residual _{FY1999}	Below	16	9	Residual _{FY2002}	Below	10	15	Residual _{FY2005}	Below	13	12
	Above	9	16		Above	15	10		Above	12	13

Statistical Comparisons:

	χ^2 Statistic
Column (2) versus column (1):	12.500***
Column (2) versus column (3):	2.885*

(continued on next page)

presume the 15 governments in the northeast cell delay debt issues until after GASB 34 because they anticipate favorable GASB 34 consequences. We set the variable $ExpectFavorable_i = 1$ to distinguish these governments. Conversely, we presume the 15 governments in the southwest cell accelerate new uninsured debt issues because they anticipate adverse GASB 34 consequences. The variable $ExpectAdverse_i = 1$ distinguishes these governments. The remaining 20 governments are set as $ExpectFavorable_i = 0$ and $ExpectAdverse_i = 0$. $ExpectFavorable$ and $ExpectAdverse$ thus delineate the 50 aggregated governments into three time-invariant and mutually exclusive classifications. Panel C lists the governments included in each cell in Panel B. Governments listed in each cell during the three years centered on the release of GASB 34 disclosures (column (2)) differ substantially from the adjacent three-year periods, consistent with strategic behavior by governments specific to GASB 34 implementation.²⁵ We also construct a rank measure that varies directly with the extent that governments anticipate favorable GASB 34 consequences. We rank the 50 governments by the difference between annualized unexpected uninsured debt in FY2004 and FY2002 to obtain time-invariant $Rank_i = 50/50, 49/50, \dots, 1/50$. Higher values indicate a more favorable expectation about GASB 34 consequences.²⁶

We emphasize that the variables $ExpectFavorable$, $ExpectAdverse$, and $Rank$ indicate neither financial viability nor financial reporting quality of governments. Rather, the variables indicate insider expectations about how GASB 34 implementation influences outside investors' incremental assessments of creditworthiness within state boundaries. Managers of high (or low) financial quality governments can anticipate either favorable or adverse consequences from GASB 34-compliant disclosures at the margin. Managers' expectations of favorable (adverse) consequences following GASB 34 do not definitively indicate that a government is high (low) quality—only that GASB 34 disclosures improve

²⁵ For example, of the 15 governments classified as $ExpectFavorable = 1$, only one (Missouri) appears in the northeast cell in column (1) and only four (Delaware, Indiana, Missouri, and New Mexico) appear in the northeast cell in column (3). Of the 15 governments classified as $ExpectAdverse = 1$, only three (Georgia, Minnesota, and South Dakota) appear in the southwest cell in column (1) and only two (Kentucky and South Carolina) appear in the southwest cell in column (3).

²⁶ Note that $ExpectFavorable$ and $ExpectAdverse$ classifications cannot be inferred from $Rank$. To illustrate, Nebraska is classified as $Rank = 46/50$ but the state is not classified $ExpectFavorable = 1$ because its FY2002 unexpected new uninsured debt exceeds the FY2002 sample median. Thus, $Rank$ provides an alternative (albeit correlated) measure of strategic debt-issuing behavior.

TABLE 3 (continued)
Panel C: Governments Included in Each Cell within Each 2 × 2 Matrix Presented in Panel B
3-Year Period Centered on the Fiscal Year When GASB 34-Compliant ACFRs Were First Publicly Disclosed
Prior 3-Year Period (FY1999 through FY2001) (1) **Residual_{FY2001}** **Residual_{FY2004}** **Residual_{FY2007}**
Subsequent 3-Year Period (FY2005 through FY2007) (3)

Residual _{FY1999}		Residual _{FY2001}		Residual _{FY2002}		Residual _{FY2004}		Residual _{FY2005}		Residual _{FY2007}	
Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
Below	Arizona, California, Colorado, Delaware, Iowa, Indiana, Louisiana, Massachusetts, Maryland, Maine, Nebraska, New Jersey, New York, Rhode Island, Texas, Virginia	Alaska, Connecticut, Hawaii, Kansas, Kentucky, Michigan, Missouri, Ohio, South Carolina	Alaska, Florida, Kansas, Nevada, New York, Ohio, Oklahoma, Tennessee, Virginia, Washington	Alaska, Florida, Kansas, Nevada, New York, Ohio, Oklahoma, Tennessee, Virginia, Washington	California, Colorado, Delaware, Idaho, Illinois, Indiana, Massachusetts, Maine, Missouri, North Carolina, New Jersey, New Mexico, Oregon, Texas, Wyoming	Below	Above	Below	Above	Below	Above
Above	Arkansas, Georgia, Idaho, Minnesota, New Mexico, Oklahoma, South Dakota, Vermont, Wyoming	Alabama, Florida, Illinois, Mississippi, Montana, North Carolina, North Dakota, New Hampshire, Nevada, Oregon, Pennsylvania, Tennessee, Utah, Washington, Wisconsin, West Virginia	Alabama, Arizona, Connecticut, Georgia, Kentucky, Louisiana, Minnesota, Mississippi, Montana, North Dakota, North Carolina, New Hampshire, North Carolina, Rhode Island, Utah, Vermont	Alabama, Arizona, Connecticut, Georgia, Kentucky, Louisiana, Minnesota, Mississippi, Montana, North Dakota, North Carolina, Rhode Island, Utah, Vermont	Arkansas, Hawaii, Iowa, Maryland, Michigan, Nebraska, New Hampshire, Rhode Island, Utah, Vermont	Above	Below	Above	Below	Above	Below
Below	Arkansas, Iowa, Illinois, Maine, Mississippi, North Dakota, Nebraska, New Hampshire, Oregon, South Dakota, Utah, Vermont, West Virginia	Arkansas, Alabama, Arizona, Connecticut, Georgia, Hawaii, Iowa, Maryland, Michigan, Nebraska, New Hampshire, Rhode Island, Utah, Vermont	Arkansas, Alabama, Arizona, Connecticut, Georgia, Hawaii, Iowa, Maryland, Michigan, Nebraska, New Hampshire, Rhode Island, Utah, Vermont	Arkansas, Alabama, Arizona, Connecticut, Georgia, Hawaii, Iowa, Maryland, Michigan, Nebraska, New Hampshire, Rhode Island, Utah, Vermont	California, Colorado, Connecticut, Kansas, Louisiana, Massachusetts, Minnesota, Missouri, Montana, Nebraska, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Virginia, Washington, West Virginia, Wisconsin, Wyoming	Above	Below	Above	Below	Above	Below
Above	Florida, Hawaii, Idaho, Kentucky, Maryland, Michigan, Minnesota, Missouri, Montana, Nebraska, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Virginia, Washington, West Virginia, Wisconsin, Wyoming	California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming	California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming	California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming	California, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming	Below	Above	Below	Above	Below	Above

Baber, Beck, and Koester

***, **, * Indicate parameters statistically significant at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively. This table considers unexpected net unsecured debt issues by U.S. state and local governments. Panel A provides descriptive statistics. Panel B is constructed using residuals from estimating specification (1), which regresses $UninsuredDebtPC_{it}$ on ΔGSP_{it} , state fixed effects, and quarter-year fixed effects. We cumulate four quarter-year residuals to obtain one observation for each state-fiscal year. These 50 observations are sorted on the cumulative residual into two groups: above or below the within-fiscal year median value. Chi-square statistics compare the distribution centered on the year that GASB 34-compliant ACFRs were publicly disclosed (Panel B, column (2)) with distributions from the three-year period prior (Panel B, column (1)) and the three-year period after (Panel B, column (3)). Panel C lists the governments included in each cell with each 2 × 2 matrix presented in Panel B. Variables are defined in Appendix A.

(worsen) investors' perceptions about a government. Thus, pre-conceived notions of the *absolute* financial health of individual government units do not necessarily apply when thinking about these classifications.

Next, we estimate the following specifications to investigate separation in the municipal debt market post-GASB 34:

$$Y_{it} = \alpha_0 + \beta_1 Post_t * ExpectFavorable_i + \beta_2 Post_t * ExpectAdverse_i + Controls + \varepsilon_{it} \quad (2a)$$

$$Y_{it} = \alpha_0 + \beta_1 Post_t * Rank_i + Controls + \varepsilon_{it} \quad (2b)$$

where Y_{it} is the government i , quarter t new debt characteristic and $Post = 1$ distinguishes observations following GASB 34. Control variables are identical to those in specification (1). Notice that the quarter-year fixed effects subsume the $Post$ main effect, and state fixed effects subsume the $ExpectFavorable$, $ExpectAdverse$, and $Rank$ main effects.

In specification (2a), we compare parameter estimates on interactions and address the null hypothesis $\beta_1 = \beta_2$. Rejecting the null in favor of $\beta_1 \neq \beta_2$ is consistent with separation in the municipal debt market following GASB 34, conditional on insider expectations of GASB 34 effects. Specification (2b) employs an interaction between $Post$ and $Rank$ (which varies directly with the extent that governments anticipate favorable GASB 34 consequences). Rejecting the null hypothesis $\beta_1 = 0$ in favor of $\beta_1 \neq 0$ supports the proposition that expectations about GASB 34 consequences influence new debt characteristics.

Validating Measures of GASB 34 Expectations

Figure 3 juxtaposes mean annualized unexpected uninsured debt during the sample period for the 15 governments classified $ExpectFavorable = 1$ with the 15 governments classified $ExpectAdverse = 1$. As FY2002 and FY2004 data are used to construct these classifications, differences are greater for these years by construction. Notice, however, that differences persist during both the pre- and post-GASB 34 periods that are *not* used to construct $ExpectFavorable$ and $ExpectAdverse$. Specifically, the two groups have relatively stable and persistent unexpected uninsured debt both pre- and post-GASB 34. Moreover, governments classified as expecting adverse GASB 34 effects have greater unexpected uninsured debt during the pre-GASB 34 period than governments classified as expecting favorable GASB 34 effects. However, the opposite is true during the post-GASB 34 period if favorable or adverse expectations are realized. The figure shows that the directional difference between the $ExpectFavorable$ and $ExpectAdverse$ groups manifests precisely during FY2003, the year that GASB 34-compliant ACFRs are disclosed.²⁷

To consider the Figure 3 comparison statistically, we estimate specifications (2a) and (2b) using new uninsured debt per capita ($UninsuredDebtPC$) as the dependent variable. Table 4, Panel A displays descriptive statistics for the pooled sample of 1,600 observations.²⁸ Panel B indicates that both $ExpectFavorable$ and $ExpectAdverse$ governments increase uninsured new debt from the pre- to the post-GASB 34 periods. Consistent with Figure 3, the increase is more substantial for governments classified as anticipating favorable GASB 34 consequences. Mean $UninsuredDebtPC$ increases by \$91.90 (from \$78.99 to \$170.89; $p < 0.001$) for governments that expect favorable consequences, compared to only \$27.18 (from \$70.39 to \$97.57; $p < 0.001$) for governments that expect adverse consequences. Panel C indicates that $UninsuredDebtPC$ and ΔGSP vary directly (inversely) with $ExpectFavorable$ and $Rank$ ($ExpectAdverse$).²⁹

Finally, Panel D shows estimates for specifications (2a) and (2b). Consistent with univariate comparisons, we reject $\beta_1 = \beta_2$ in favor of $\beta_1 > \beta_2$ in column (1), which implies greater post-GASB 34 uninsured new debt issues by governments that expect favorable GASB 34 consequences. Comparisons of mean new uninsured debt per capita (\$102.53 in Table 4, Panel A) with both the $Post * ExpectFavorable$ interaction estimate $\beta_1 = \$43.84$ ($p = 0.011$) and the difference between the β_1 and β_2 estimates (\$69.39; $p = 0.000$) are significant economically. In column (2), the positive estimate $\beta_1 = 96.74$ ($p = 0.000$) on the interaction $Post * Rank$ also indicates that new uninsured debt per capita following GASB 34 varies directly with the extent that governments appear to anticipate favorable GASB 34 consequences.

A potential concern is that these results are attributable to FY2002 and FY2004 observations which are used to create $ExpectFavorable$, $ExpectAdverse$, and $Rank$. Thus, we verify that results are comparable when omitting FY2002 and FY2004

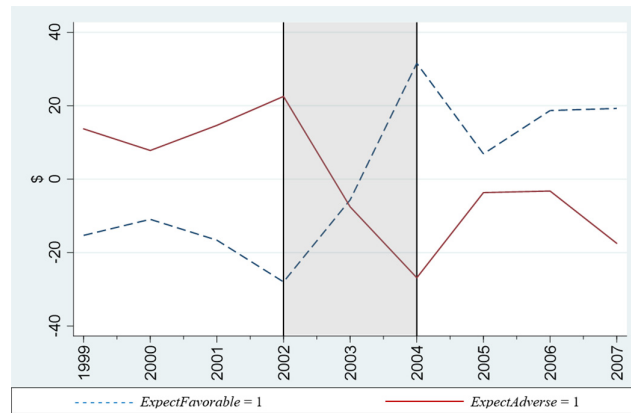
²⁷ Figure 3 displays mean *unexpected* new uninsured debt per capita, estimated using residuals from specification (1). As specification (1) includes state and quarter-year fixed effects, unexpected new uninsured debt indicates differences from state means. Thus, Figure 3 does not imply that governments expecting favorable consequences necessarily issue relatively less *total* debt per capita before and greater *total* debt per capita after GASB 34 implementation.

²⁸ Because sample sizes differ according to the hypotheses considered, we present descriptive statistics, univariate comparisons, correlations, and regression results by hypothesis.

²⁹ We compute $ExpectFavorable$, $ExpectAdverse$, and $Rank$ using FY2002 and FY2004 residuals from specification (1); correlations between these measures and ΔGSP are insignificant for FY2002 and FY2004 observations.

FIGURE 3

Validating *ExpectFavorable_i* and *ExpectAdverse_i* As Indicators of Strategic Behavior: Time-Series of Annual Unexpected New Uninsured Debt Per Capita for *ExpectFavorable_i* = 1 and *ExpectAdverse_i* = 1



This figure shows FY1999 through FY2007 annual unexpected new uninsured debt per capita (i.e., cumulated four quarter residuals from specification (1) which regresses $UninsuredDebtPC_{it}$ on ΔGSP_{it} , state fixed effects, and quarter-year fixed effects) for 15 governments expecting favorable consequences ($ExpectFavorable_i = 1$; represented by the blue dotted line) and 15 governments expecting adverse consequences ($ExpectAdverse_i = 1$; represented by the red solid line). The shaded section identifies the three-year period used to set the $ExpectFavorable_i$ and $ExpectAdverse_i$ classifications. Variables are defined in Appendix A. (The full-color version is available online.)

TABLE 4

Validating *ExpectFavorable_i* and *ExpectAdverse_i* As Indicators of Strategic Behavior

Panel A: Descriptive Statistics

Variables	n	Mean	Std. Dev.	P25	P50	P75
$UninsuredDebtPC_{it}$	1,600	102.531	98.480	37.736	73.398	132.241
$InsuredDebtPC_{it}$	1,600	106.369	101.937	35.684	78.976	143.476
$ExpectFavorable_i$	1,600	0.300	0.458	0.000	0.000	1.000
$ExpectAdverse_i$	1,600	0.300	0.458	0.000	0.000	1.000
$Rank_i$	1,600	0.510	0.289	0.260	0.510	0.760
ΔGSP_{it}	1,600	1.347	0.570	0.976	1.275	1.690

Panel B: Descriptive Statistics for $ExpectFavorable_i = 1$ and $ExpectAdverse_i = 1$

Variables	$ExpectFavorable_i = 1$			$ExpectAdverse_i = 1$		
	Post = 0	Post = 1	Difference	Post = 0	Post = 1	Difference
$UninsuredDebtPC_{it}$	78.988	170.892	91.904***	70.388	97.570	27.182***

Panel C: Correlations

Variables	(1)	(2)	(3)	(4)	(5)
(1) $UninsuredDebtPC_{it}$					
(2) $InsuredDebtPC_{it}$	0.226***				
(3) $ExpectFavorable_i$	0.149***	0.010			
(4) $ExpectAdverse_i$	-0.123***	-0.118***	-0.429***		
(5) $Rank_i$	0.171***	-0.022	0.694***	-0.676***	
(6) ΔGSP_{it}	0.022	0.054**	0.059**	-0.084***	0.061**

(continued on next page)

TABLE 4 (continued)

Panel D: Regression Results

Variables	<i>UninsuredDebtPC_{it}</i> (1)	<i>UninsuredDebtPC_{it}</i> (2)	<i>InsuredDebtPC_{it}</i> (3)	<i>InsuredDebtPC_{it}</i> (4)
<i>Post</i> * <i>ExpectFavorable_i</i> (β_1)	43.836** (0.011)		5.711 (0.586)	
<i>Post</i> * <i>ExpectAdverse_i</i> (β_2)	-20.553* (0.062)		-10.064 (0.199)	
<i>Post</i> * <i>Rank_i</i>		96.741*** (0.000)		9.115 (0.608)
ΔGSP_{it}	-5.834 (0.179)	-5.448 (0.220)	-2.797 (0.609)	-2.892 (0.598)
$\beta_1 = \beta_2$	69.389***		15.775	
F-statistic	21.205		2.001	
(p-value)	0.000		0.167	
State FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
n	1,600	1,600	1,600	1,600
R ²	0.411	0.414	0.456	0.455

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.

This table validates the three government classification measures (*ExpectFavorable_i*, *ExpectAdverse_i*, and *Rank_i*). Panel A provides descriptive statistics. Panel B compares the *UninsuredDebtPC_{it}* mean for *ExpectFavorable_i* = 1 (*ExpectAdverse_i* = 1) observations during the *Post* = 0 and *Post* = 1 periods. Panel C provides correlations. Panel D presents regression results. FY2002 through FY2004 observations are omitted as these years are used to create the three measures. In Panel D, controls include quarterly change in gross state product (ΔGSP_{it}) and state and quarter-year fixed effects; the *Post* main effect is subsumed by quarter-year fixed effects, and the *ExpectFavorable_i*, *ExpectAdverse_i*, and *Rank_i* main effects are subsumed by state fixed effects. Standard errors are clustered by state and by quarter-year. Two-tailed confidence levels shown parenthetically. Variables are defined in Appendix A.

observations to estimate specifications (2a) and (2b). Using this sample of 1,200 observations, the *Post* * *ExpectFavorable* coefficient is \$35.46 ($p = 0.068$), the *Post* * *ExpectAdverse* coefficient is $-\$13.02$ ($p = 0.350$), and the difference between the two coefficients is \$48.48 ($p = 0.004$), whereas the parameter estimate on *Post* * *Rank* is \$64.77 ($p = 0.006$) (not tabulated).

Finally, we expect that modifying new debt issues to exploit or avoid expected GASB 34 consequences manifests in uninsured, *but not insured*, debt issues. Consistent with this reasoning, columns (3) and (4) of Panel D replace the dependent variable in specifications (2a) and (2b) with new *insured* debt per capita (*InsuredDebtPC*) and show that *ExpectFavorable*, *ExpectAdverse*, and *Rank* measures are *not* associated with new insured debt issues. Such findings both justify our interpretation of the results in columns (1) and (2) and support the focus on uninsured debt to infer expected GASB 34 consequences.

In sum, both Figure 3 and Table 4 support the proposition that GASB 34 facilitates persistent separation in the municipal debt market. Analyses that follow considers post-GASB 34 changes in debt financing conditional on realization of expected GASB 34 consequences.

The Use of Debt Insurance (H3)

To consider how GASB 34 affected the use of debt insurance, Table 5 presents analyses for the percentage of *total* new debt insured by third parties (both value-weighted %*Insured-VW* and issue-weighted %*Insured-IW*). Panel A of the table indicates that, for the sample of 1,600 observations, about half of municipal debt issues (55.3 percent of the number of new debt issues and 49.0 percent of the value of new debt issues) are insured. Panel B shows univariate comparisons of mean measures of insured new debt pre-versus-post GASB 34, for the two classifications (*ExpectFavorable* = 1 and *ExpectAdverse* = 1). Governments that expect adverse GASB 34 consequences show economically and statistically significant increases in the relative use of insured new debt—a mean 8.6 percent increase ($p < 0.001$) for the value-weighted, and a mean 13.8 percent increase ($p < 0.001$) for the issue-weighted, measure. In contrast, increases in the use of insured debt are less substantial for governments anticipating favorable consequences. Panel C, column (1) indicates that *ExpectFavorable* and *Rank* vary inversely with %*Insured-VW*, but the correlation between *ExpectAdverse* and

TABLE 5
Insurance Substitution: Change in the Proportion of New Debt Issues That Are Insured

Panel A: Descriptive Statistics

Variables	n	Mean	Std. Dev.	P25	P50	P75
%Insured-VW _{it}	1,600	0.490	0.256	0.290	0.495	0.686
%Insured-IW _{it}	1,600	0.553	0.246	0.381	0.576	0.738
ExpectFavorable _i	1,600	0.300	0.458	0.000	0.000	1.000
ExpectAdverse _i	1,600	0.300	0.458	0.000	0.000	1.000
Rank _i	1,600	0.510	0.289	0.260	0.510	0.760
GSP	1,600	1.347	0.570	0.976	1.275	1.690

Panel B: Descriptive Statistics for ExpectFavorable_i = 1 and ExpectAdverse_i = 1

Variables	ExpectFavorable _i = 1			ExpectAdverse _i = 1		
	Post = 0	Post = 1	Difference	Post = 0	Post = 1	Difference
%Insured-VW _{it}	0.472	0.457	-0.015	0.461	0.548	0.086***
%Insured-IW _{it}	0.547	0.617	0.070***	0.454	0.592	0.138***

Panel C: Correlations

Variables	(1)	(2)	(3)	(4)	(5)
(1) %Insured-VW _{it}					
(2) %Insured-IW _{it}	0.622***				
(3) ExpectFavorable _i	-0.064***	0.077***			
(4) ExpectAdverse _i	0.037	-0.080***	-0.429***		
(5) Rank _i	-0.173***	-0.064**	0.694***	-0.676***	
(6) ΔGSP _{it}	0.006	0.052**	0.059**	-0.084***	0.061**

Panel D: Regression Results

Variables	%Insured-VW _{it} (1)	%Insured-IW _{it} (2)	%Insured-VW _{it} (3)	%Insured-IW _{it} (4)
Post * ExpectFavorable _i (β ₁)	-0.044 (0.142)	0.008 (0.781)		
Post * ExpectAdverse _i (β ₂)	0.057* (0.063)	0.076** (0.014)		
Post * Rank _i			-0.106* (0.098)	-0.082* (0.080)
ΔGSP _{it}	0.009 (0.605)	-0.000 (0.960)	0.009 (0.622)	-0.000 (0.972)
β ₁ = β ₂	0.101**	0.068**		
F-statistic	6.425	6.010		
(p-value)	(0.017)	(0.020)		
State FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
n	1,600	1,600	1,600	1,600
R ²	0.324	0.582	0.321	0.578

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.

This table considers the percentage of new debt issues that are insured (%Insured-VW_{it} and %Insured-IW_{it}). Panel A provides descriptive statistics. Panel B compares the %Insured-VW_{it} and %Insured-IW_{it} mean for ExpectFavorable_i = 1 (ExpectAdverse_i = 1) observations during the Post = 0 and Post = 1 periods. Panel C provides correlations. Panel D presents regression results. In Panel D, controls include quarterly change in gross state product (ΔGSP_{it}) and state and quarter-year fixed effects; the Post main effect is subsumed by quarter-year fixed effects, and the ExpectFavorable_i, ExpectAdverse_i, and Rank_i main effects are subsumed by state fixed effects. Standard errors are clustered by state and by quarter-year. Two-tailed confidence levels are shown parenthetically.

Variables are defined in [Appendix A](#).

%*Insured-VW* is not statistically significant. In column (2), however, issue-weighted insured debt is negatively (positively) associated with *ExpectAdverse* (*ExpectFavorable*), whereas *Rank* shows a negative correlation.

Panel D shows parameter estimates for the regression specifications (2a) and (2b). Results in columns (1) and (2) reject the null hypothesis $\beta_1 = \beta_2$ in favor of the alternative H3 ($\beta_1 < \beta_2$) that governments expecting adverse GASB 34 effects increase their use of insured debt following GASB 34 relative to governments expecting favorable consequences. Notice the parameter estimate β_2 on the interaction *Post * ExpectAdverse* is significantly positive in both column (1) (0.057; $p = 0.063$) and column (2) (0.076; $p = 0.014$), but the estimate β_1 for the interaction *Post * ExpectFavorable* is insignificant. This suggests that the insurance effect is concentrated in governments anticipating adverse consequences. Regarding economic significance, governments expecting adverse consequences increased the proportion of insured debt by 5.7 to 7.6 percentage points following GASB 34. Also consistent with expectations, parameter estimates on *Post * Rank* in column (3) (-0.106 ; $p = 0.098$) and column (4) (-0.082 ; $p = 0.080$) indicate that changes in use of debt insurance vary inversely with the extent that governments anticipate favorable GASB 34 consequences. We interpret these results as further support for the propositions that insider expectations about GASB 34 consequences are realized post-GASB 34, and that GASB 34 promotes separation in the municipal public debt market.

The Use of Debt versus Alternative Financing (H4)

H4 posits that, if pre-GASB 34 expectations are realized, governments expecting favorable GASB 34 consequences are more likely than governments expecting adverse consequences to substitute debt for alternative financing sources (e.g., taxes, fees, and transfers) following GASB 34. To test this hypothesis, we use COG GFS data for nearly 90,000 state and local governments and special districts to identify four alternative financing sources: (1) tax revenues (i.e., total income, property, and sales taxes), (2) user fee revenues, (3) intergovernmental transfers, and (4) the sum of these three alternative financing sources. We construct four variables that consider the extent governments substitute toward these alternative financing sources and away from public debt. Specifically, we create ratios with each financing source as the numerator and the sum of the alternative financing source and total new debt issues as the denominator. These computations yield four dependent variables—*TaxRev*, *FeesRev*, *IntgovtRev*, and *TotalRev*—representing the proportion of *total* new debt and alternative financing derived from the specified source. The variables are between 0 and 1 by construction, and values greater (less) than 0.50 indicate that a government's annual total alternative financing is more (less) than its annual total new debt issues. H4 predicts that these measures decrease (increase) for *ExpectFavorable* (*ExpectAdverse*) governments after GASB 34 implementation, and vary inversely with *Rank*.

As the COG GFS compiles data annually (not quarterly), we aggregate debt issues by fiscal year to obtain 400 government-year observations (50 governments \times eight years) – 200 observations before GASB 34 implementation and 200 observations after. We also modify specifications (2a) and (2b) replacing quarter-year fixed effects with year fixed effects and the quarterly change in gross state product with the annual change (ΔGSP_{Annual}).³⁰

Table 6, Panel A shows that mean and median values for the four dependent variables range from 0.77 to 0.92, indicating that governments use alternative financing to a greater extent than new debt financing. Panel B shows that governments expecting favorable consequences experienced statistically significant declines in the relative use of all alternative financing sources relative to total new debt following GASB 34. Governments expecting adverse consequences experienced a more modest decline in two of the four alternative financing sources relative to new total debt issues (and no significant change in the other two alternative financing sources). Panel C indicates the four alternative financing measures are positively correlated, and vary inversely (directly) with *ExpectFavorable* and *Rank* (*ExpectAdverse*). These correlations indicate that governments anticipating favorable (adverse) consequences use lesser (greater) alternative financing sources relative to new total debt issues.

Regression results in Panel D show that the parameter estimate on *Post * ExpectFavorable* is negative and statistically significant for all measures excluding *FeesRev*, which indicates that governments that expect favorable post-GASB 34 consequences decrease their use of alternative financing sources relative to new debt issues after GASB 34 (compared to the benchmark control group of governments). In contrast, the parameter estimate on *Post * ExpectAdverse* is positive and statistically significant for all four measures, consistent with governments that expect adverse GASB 34 consequences increasing the use of alternative financing sources relative to new debt issues after GASB 34. We reject the null hypothesis $\beta_1 = \beta_2$ in favor of the alternative hypothesis $\beta_1 < \beta_2$ in all four specifications. Panel E, which shows results

³⁰ COG GFS data include the entire population of governments in years ending in 2 and 7. In intervening years, the sample includes all state governments and city governments with populations greater than 75,000 (Census of Governments (COG) 2008) plus a random sample of smaller governments (stratified based on population). Year fixed effects subsume potential differences created from alternative sampling approaches in years ending in 2 or 7.

TABLE 6
Financing Substitution: Use of Alternative Financing Sources Relative to New Debt Issues

Panel A: Descriptive Statistics

Variables	n	Mean	Std. Dev.	P25	P50	P75
<i>TotalRev_{it}</i>	400	0.915	0.035	0.894	0.918	0.940
<i>TaxRev_{it}</i>	400	0.806	0.071	0.759	0.808	0.858
<i>FeesRev_{it}</i>	400	0.777	0.089	0.718	0.784	0.835
<i>IntgovtRev_{it}</i>	400	0.765	0.093	0.705	0.770	0.829
<i>ExpectFavorable_i</i>	400	0.300	0.459	0.000	0.000	1.000
<i>ExpectAdverse</i>	400	0.300	0.459	0.000	0.000	1.000
<i>Rank_i</i>	400	0.510	0.289	0.260	0.510	0.760
<i>GSPAnnual_{it}</i>	400	5.476	2.232	4.064	5.277	6.764

Panel B: Descriptive Statistics for *ExpectFavorable_i = 1* and *ExpectAdverse_i = 1*

Variables	<i>ExpectFavorable_i = 1</i>			<i>ExpectAdverse_i = 1</i>		
	<i>Post = 0</i>	<i>Post = 1</i>	Difference	<i>Post = 0</i>	<i>Post = 1</i>	Difference
<i>TotalRev_{it}</i>	0.923	0.894	-0.029***	0.929	0.920	-0.009
<i>TaxRev_{it}</i>	0.831	0.758	-0.073***	0.837	0.807	-0.030***
<i>FeesRev_{it}</i>	0.780	0.747	-0.033*	0.790	0.791	0.001
<i>IntgovtRev_{it}</i>	0.786	0.711	-0.075***	0.808	0.783	-0.026*

Panel C: Correlations

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) <i>TotalRev_{it}</i>							
(2) <i>TaxRev_{it}</i>	0.933***						
(3) <i>FeesRev_{it}</i>	0.911***	0.758***					
(4) <i>IntgovtRev_{it}</i>	0.956***	0.886***	0.819***				
(5) <i>ExpectFavorable_i</i>	-0.126**	-0.103**	-0.099**	-0.113**			
(6) <i>ExpectAdverse_i</i>	0.164***	0.151***	0.101**	0.215***	-0.429***		
(7) <i>Rank_i</i>	-0.088*	-0.031	-0.072	-0.088*	0.694***	-0.676***	
(8) Δ <i>GSPAnnual_{it}</i>	0.007	0.005	0.100**	-0.037	0.007	-0.048	0.008

Panel D: Regression Results Using Binary Classifications

Variables	<i>TotalRev_{it}</i> (1)	<i>TaxRev_{it}</i> (2)	<i>FeesRev_{it}</i> (3)	<i>IntgovtRev_{it}</i> (4)
<i>Post * ExpectFavorable_i</i> (β_1)	-0.013** (0.045)	-0.031** (0.026)	-0.018 (0.136)	-0.030** (0.048)
<i>Post * ExpectAdverse_i</i> (β_2)	0.007*** (0.008)	0.010* (0.093)	0.016** (0.030)	0.018** (0.028)
Δ <i>GSPAnnual_{it}</i>	0.001 (0.259)	0.003** (0.049)	0.001 (0.712)	0.002 (0.122)
$\beta_1 = \beta_2$	0.020***	0.041***	0.034**	0.048***
F-statistic	14.812	13.223	6.704	15.630
(p-value)	0.006	0.008	0.036	0.006
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
n	400	400	400	400
R ²	0.747	0.741	0.738	0.812

(continued on next page)

TABLE 6 (continued)

Panel E: Regression Results Using Continuous Classification

Variables	<i>TotalRev_{it}</i> (1)	<i>TaxRev_{it}</i> (2)	<i>FeesRev_{it}</i> (3)	<i>IntgovtRev_{it}</i> (4)
<i>Post</i> * <i>Rank_i</i>	-0.026** (0.028)	-0.056** (0.030)	-0.054* (0.071)	-0.064** (0.019)
Δ <i>GSPAnnual_{it}</i>	0.001 (0.285)	0.003* (0.052)	0.001 (0.757)	0.002 (0.139)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
n	400	400	400	400
R ²	0.702	0.68	0.71	0.738

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.

This table considers the use of alternative financing sources relative to debt (*TotalRev_{it}*, *TaxRev_{it}*, *FeesRev_{it}*, and *IntgovtRev_{it}*), computed and analyzed as annual observations. Panel A provides descriptive statistics. Panel B compares the *TotalRev_{it}*, *TaxRev_{it}*, *FeesRev_{it}*, and *IntgovtRev_{it}* mean for *ExpectFavorable_i* = 1 (*ExpectAdverse_i* = 1) observations during the *Post* = 0 and *Post* = 1 periods. Panel C provides correlations. Panels D and E present regression results. In Panels D and E, controls include annual change in gross state product (Δ *GSPAnnual*) and state and year fixed effects; the *Post* main effect is subsumed by year fixed effects, and the *ExpectFavorable_i*, *ExpectAdverse_i*, and *Rank_i* main effects are subsumed by state fixed effects. Standard errors are clustered by state and by year. Two-tailed confidence levels are shown parenthetically.

Variables are defined in Appendix A.

using *Rank*, corroborates the Panel D results.³¹ As a whole, the evidence in Table 6 supports H4 and is consistent with separation in the municipal debt market following GASB 34.

VI. ADDITIONAL ANALYSES

Falsification Procedures

We first execute random assignment procedures that consider whether the core results in Tables 5 and 6 are statistically reliable and not attributable to chance or to differences not considered directly in the research design (Heß 2017; White and Webb 2021; deHaan and Glover 2023). Table 7, column (1) lists the comparisons of interest from each table and column considered in the core analyses. We begin by estimating 1,000 iterations of specifications (2a) and (2b) with *Post* = 1 randomly assigned to quarter-years (years) without replacement. Column (2) indicates the proportion of the 1,000 iterations where the difference between the placebo parameter estimates $\beta_1 = \beta_2$ exceeds the difference obtained from the core analysis (or where the placebo effect of *Post* * *Rank* exceeds the estimate obtained from the core analysis). Proportions are near zero, indicating that core results in Tables 5 and 6 are statistically reliable.

Next, we estimate 1,000 iterations of specifications (2a) and (2b) with 15 governments randomly assigned as *ExpectFavorable* = 1 and a different 15 governments randomly assigned as *ExpectAdverse* = 1 (or all governments randomly assigned a *Rank* value of 1 to 50) without replacement. We report the proportions where placebo effects exceed those obtained in core results in column (3). Proportions are near zero, again indicating statistically reliable core results in Tables 5 and 6.

To ensure that the primary results are not attributable to economic cycles, we replicate the analyses using statistically independent nine-year periods FY1989 through FY1997 (centered on FY1993) and FY2009 through FY2017 (centered on FY2013) that do not encompass GASB 34 adoption.³² For each time period, we use the procedures outlined in Table 3 (i.e., the 2 × 2 matrix sort) to classify governments according to expected GASB 34 consequences, and then test H3 and H4 using these alternative indicators. Table 7, column (4) ((5)) shows significance levels for the parameter estimates of interest when re-estimating the regressions using the FY1989 through FY1997 (FY2009 through FY2017) sample period. Results are statistically significant for only one of 24 tests, which suggests that the primary results are unique to the GASB 34 implementation period.³³

Finally, the analyses centers around FY2003, which immediately follows the recession that ended in November 2001 (i.e., during FY2002). To consider the potential impact of recessionary economic conditions, we replicate the

³¹ Results (not tabulated) are robust when specifications include the inverse of the deflator used to compute the dependent variables.

³² These time periods are set a full decade prior and subsequent to our primary sample period to retain consistency with COG GFS survey cycles and with state and local election cycles. Elections typically occur on even-numbered years, and are associated with changes in both debt issues and alternative financing sources (Baber and Sen 1986).

³³ Associations are also not statistically significant when applying the government classifications shown in Table 3 (Panel C, columns (1), (2), or (3)) to these alternative sample periods (not tabulated).

TABLE 7
Additional Analyses: Falsification Procedures

(1)	Random Assignment of:		Different Sample Periods	
	<i>Post</i> (2)	<i>ExpectFavorable</i> , <i>ExpectAdverse</i> , or <i>Rank</i> (3)	FY1989– FY1997 (4)	FY2009– FY2017 (5)
<u>Table 5, Panel D (considering the change in proportion of new debt issues that are insured)</u>				
(1) <i>Post</i> * <i>ExpectFavorable</i> _{<i>i</i>} = <i>Post</i> * <i>ExpectAdverse</i> _{<i>i</i>} column (1)	0.000	0.003	0.049**	0.521
(2) <i>Post</i> * <i>ExpectFavorable</i> _{<i>i</i>} = <i>Post</i> * <i>ExpectAdverse</i> _{<i>i</i>} column (2)	0.001	0.045	0.386	0.365
(3) <i>Post</i> * <i>Rank</i> _{<i>i</i>} column (3)	0.023	0.044	0.265	0.508
(4) <i>Post</i> * <i>Rank</i> _{<i>i</i>} column (4)	0.007	0.036	0.714	0.133
<u>Table 6, Panel D (considering the use of alternative financing sources relative to new debt issues)</u>				
(5) <i>Post</i> * <i>ExpectFavorable</i> _{<i>i</i>} = <i>Post</i> * <i>ExpectAdverse</i> _{<i>i</i>} column (1)	0.005	0.000	0.654	0.514
(6) <i>Post</i> * <i>ExpectFavorable</i> _{<i>i</i>} = <i>Post</i> * <i>ExpectAdverse</i> _{<i>i</i>} column (2)	0.003	0.001	0.855	0.454
(7) <i>Post</i> * <i>ExpectFavorable</i> _{<i>i</i>} = <i>Post</i> * <i>ExpectAdverse</i> _{<i>i</i>} column (3)	0.002	0.008	0.665	0.272
(8) <i>Post</i> * <i>ExpectFavorable</i> _{<i>i</i>} = <i>Post</i> * <i>ExpectAdverse</i> _{<i>i</i>} column (4)	0.009	0.001	0.766	0.306
<u>Table 6, Panel E (considering the use of alternative financing sources relative to new debt issues)</u>				
(9) <i>Post</i> * <i>Rank</i> _{<i>i</i>} column (1)	0.013	0.000	0.659	0.415
(10) <i>Post</i> * <i>Rank</i> _{<i>i</i>} column (2)	0.013	0.000	0.692	0.333
(11) <i>Post</i> * <i>Rank</i> _{<i>i</i>} column (3)	0.033	0.001	0.370	0.214
(12) <i>Post</i> * <i>Rank</i> _{<i>i</i>} column (4)	0.011	0.000	0.580	0.215

***, **, * Indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.

This table reports the confidence levels from four sets of placebo tests applied to the regressions presented in Tables 5 and 6. Column (1) lists the comparison of interest from each table and column considered in the core analyses. Columns (2) and (3) indicate the proportion of 1,000 iterations where the placebo estimate exceeds the core analysis estimate. In column (2), the variable *Post* is randomly assigned during the nine-year sample period (FY1999 through FY2007). In column (3), 15 governments are randomly assigned as *ExpectFavorable* = 1 and 15 governments are randomly assigned as *ExpectAdverse* = 1 (or all governments are randomly assigned a *Rank* value 1,...,50). Column (4) ((5)) indicates the parametric statistical significances for placebo specifications for FY1989–FY1997 (FY2009–FY2017), prior (subsequent) to the FY1999–FY2007 sample period used in the core analyses.

analyses (including setting new measures *ExpectFavorable*, *ExpectAdverse*, and *Rank*) separately using (1) a sample period centered on FY2010 (which follows the end of the June 2009 (FY2009) recession, and (2) state government revenues (which potentially correlate with recession-induced economic fluctuations). For both procedures, we fail to replicate the core results. Results (not tabulated) for these procedures alleviate concerns that results are attributable to economic cycles rather than accounting standardization.

Analysis Using Classifications Based on Pre-GASB 34 Voluntary Disclosures

To ensure that *ExpectFavorable* or *ExpectAdverse* categorizations do not simply capture governments' pre-GASB 34 disclosure practices, we classify observations according to whether the state government voluntarily provided disclosures prior to GASB 34 that were consistent with, or similar to, disclosures subsequently required by GASB 34 (as shown in Table 2). We fail to find evidence consistent with our core analyses when using these alternative classifications.

Analysis considering Time Trends in the Data

To ensure that the primary results do not reflect a time trend, we estimate Tables 5 and 6 specifications that include interactions between variables of interest and a time trend variable (results not tabulated).³⁴ These interactions are

³⁴ The nine-year sample period includes 36 quarters of data. The time trend variable begins at 1 and increases to 36 when the dependent variable is *Insured-IW* and *Insured-VW* (with values set equal to 17, 18, 19, and 20 omitted because FY2003 quarterly observations are omitted). The time trend variable begins at 1 and increases to 9 when the dependent variable is *TotalRev*, *TaxRev*, *FeesRev*, and *IntgovtRev* (with values set equal to 5 omitted because FY2003 observations are omitted).

statistically insignificant in ten of 12 specifications. More importantly, the primary results are robust to including time trends in the specifications.

Analysis Using Classifications Constructed from State Governance Characteristics

To consider whether *ExpectFavorable* and *ExpectAdverse* proxy for characteristics advanced in prior studies, we classify the governments according to whether the state has (1) GAAP reporting requirements for all or some levels of government (state, county, city/town, and school districts) (National Association of State Auditors, Comptrollers, and Treasurers (NASACT) 2014; Khumawhala and Neely 2008); (2) a fiscal monitoring program for local governments (Nakhmurina 2024); (3) restrictions on municipal bankruptcy (Moldogaziev, Kioko, and Hildreth 2017); (4) tax and expenditure limitations imposed on local governments (Mullins and Wallin 2004); (5) requirements for local governments to submit official statements (i.e., bond disclosure documents) to the state finance office prior to debt issues; and (6) an appointed (as opposed to elected) comptroller/treasurer.³⁵ Augmenting the Tables 5 and 6 specifications to include interactions of these measures with *Post* typically reveals no statistically significant associations for these measures and does not materially alter the primary results (not tabulated).

Analyses Restricted to State Level Entities

We replicate all analyses using data for state level entities to consider the extent that aggregating data for all state, local, and special district entities within a state's boundary influences the core results. We create new state-level measures of expected GASB 34 consequences (*ExpectFavorable_{State}*, *ExpectAdverse_{State}*, and *Rank_{State}*) using uninsured state issues only. Not surprisingly, given that state level entities issue approximately 40 percent of the debt during our sample period (not tabulated), we find approximately 73 percent of governments fall in the same classification as in the primary analyses.³⁶ Table 8, Panels A, B, and C shows that tests of H3 and H4 using the state entity sample yield results of similar magnitudes and statistical reliability as the primary results in ten of 12 specifications.

Analysis Using Yields on State Level Bond Issues

We also use state level data to investigate whether associations using bond yield premiums conform with expectations of GASB 34 consequences considered using new state debt issues. Recall that we do not investigate bond yields in the primary analyses owing to data limitations and the endogeneity of yields with the decision to issue new debt. Even so, most state entities issue new debt with nonmissing yields at least annually, and we consider yield premiums using these limited data.³⁷ If expectations about GASB 34 effects manifest as realized changes in debt service costs, then we anticipate differences in yield premiums on state bond issues consistent with state level measures of expected GASB 34 consequences. For each uninsured state level bond issue with available data, we compute the yield less the prevailing taxable maturity-matched U.S. Treasury yield at the issue date. We estimate regression specifications (2a) and (2b) using the value-weighted mean of the yield premiums for each state-year (*YieldPremium*) as the dependent variable. Results are displayed in Table 8, Panel D. Column (1) indicates that governments anticipating favorable GASB 34 consequences experience yield premiums reductions after GASB 34 relative to governments expecting adverse consequences ($p = 0.056$). Results for the *Rank_{State}* measure displayed in column (2) indicate a similar association. These results are robust to including the total amount of debt issued, the ratio of general obligation debt to total debt issued, the ratio of callable debt to total debt issued, the value-weighted time to maturity, and the value-weighted average bond ratings as control variables (not tabulated). As a whole, this evidence suggests that insider expectations about GASB 34 consequences correlate predictably with realized changes in state entity debt service costs following GASB 34 implementation.

³⁵ State controller (treasurer) data are available at [https://ballotpedia.org/Controller_\(state_executive_office\)](https://ballotpedia.org/Controller_(state_executive_office)) ([https://ballotpedia.org/Treasurer_\(state_executive_office\)](https://ballotpedia.org/Treasurer_(state_executive_office))). These data are researched, written, and verified by Lucy Burns Institute staff. The sole statistically significant correlation (0.30; p -value = 0.04) is between *ExpectFavorable* and the state having an appointed comptroller/treasurer.

³⁶ The 15 state governments classified as *ExpectFavorable_{State_i}* are Arizona, California, Colorado, Idaho, Illinois, Massachusetts, Maryland, Maine, Michigan, North Carolina, New Jersey, New Mexico, Texas, Vermont, and Wyoming. The 15 state governments classified as *ExpectAdverse_{State_i}* are Alabama, Arkansas, Connecticut, Georgia, Hawaii, Kentucky, Minnesota, Mississippi, Montana, North Dakota, Oklahoma, Pennsylvania, South Dakota, Virginia, and West Virginia. Italics indicate the 22 governments classified into the same group for the state-level analyses for the primary analyses (22/30 = 73.3 percent).

³⁷ We aggregate by year because many states do not issue debt every quarter-year, but most issue debt every year. Data are available for 394 of 400 state-years during the sample period. Missing observations are Alaska (2005, 2007), Nebraska (1999), North Dakota (2007), Tennessee (2002), and Wyoming (2007).

TABLE 8
Analyses Using State Level Data

Panel A: Testing H3 (Insurance Substitution)

Variables	%Insured-VW _{Stateit}	%Insured-IW _{Stateit}	%Insured-VW _{Stateit}	%Insured-IW _{Stateit}
(1)	(2)	(3)	(4)	
$Post_t * ExpectFavorable_{Statei} (\beta_1)$	-0.039 (0.115)	0.001 (0.962)		
$Post_t * ExpectAdverse_{Statei} (\beta_2)$	0.016 (0.415)	0.008 (0.614)		
$Post_t * Rank_{Statei}$			-0.100*** (0.008)	-0.033 (0.133)
ΔGSP_{it}	0.010 (0.150)	0.012* (0.092)	0.010 (0.161)	0.012 (0.112)
$\beta_1 = \beta_2$	0.055**	0.007		
F-statistic	4.48	0.244		
(p-value)	0.042	0.624		
State FE	Yes	Yes	Yes	Yes
Quarter-year FE	Yes	Yes	Yes	Yes
n	1,600	1,600	1,600	1,600
R ²	0.267	0.433	0.269	0.434

Panel B: Testing H4 (Financing Substitution) with Binary Independent Variables

Variables	TotalRev _{Stateit}	TaxRev _{Stateit}	FeesRev _{Stateit}	IntgovtRev _{Stateit}
(1)	(2)	(3)	(4)	
$Post_t * ExpectFavorable_{Statei} (\beta_1)$	-0.015* (0.091)	-0.030 (0.137)	-0.033 (0.117)	-0.032 (0.183)
$Post_t * ExpectAdverse_{Statei} (\beta_2)$	0.009* (0.091)	0.022** (0.048)	0.015 (0.455)	0.028* (0.086)
$\Delta GSP_{Annual_{it}}$	0.001 (0.325)	0.003 (0.152)	0.002 (0.404)	0.003 (0.279)
$\beta_1 = \beta_2$	0.024**	0.032**	0.048**	0.060***
F-statistic	10.729	8.711	5.843	7.936
(p-value)	0.014	0.021	0.046	0.026
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
n	400	400	400	400
R ²	0.634	0.618	0.611	0.693

Panel C: Testing H4 (Financing Substitution) with a Continuous Independent Variable

Variables	TotalRev _{Stateit}	TaxRev _{Stateit}	FeesRev _{Stateit}	IntgovtRev _{Stateit}
(1)	(2)	(3)	(4)	
$Post_t * Rank_{Statei}$	-0.035*** (0.008)	-0.079** (0.010)	-0.068** (0.038)	-0.094** (0.013)
$\Delta GSP_{Annual_{it}}$	0.001 (0.262)	0.003 (0.121)	0.002 (0.347)	0.003 (0.243)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
n	400	400	400	400
R ²	0.637	0.623	0.611	0.698

(continued on next page)

TABLE 8 (continued)

Panel D: Yield Premium Analyses

Variables	<i>YieldPremium_{Stateit}</i> (1)	<i>YieldPremium_{Stateit}</i> (2)
$Post_t * ExpectFavorable_{Stateit} (\beta_1)$	-0.148 (0.160)	
$Post_t * ExpectAdverse_{Stateit} (\beta_2)$	0.113 (0.166)	
$Post_t * Rank_{Stateit}$		-0.373** (0.026)
$\Delta GSPAnnual_{it}$	-0.009 (0.386)	-0.008 (0.406)
$\beta_1 = \beta_2$	0.261*	
F-statistic	5.25	
(p-value)	(0.056)	
State FE	Yes	Yes
Year FE	Yes	Yes
n	394	394
R ²	0.465	0.467

***, **, * Indicate parameters statistically significant at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.

This table shows regression results estimated using uninsured debt issues by only state level entities. Panel A (B and C) presents estimates from specifications that consider H3 (H4) (i.e., replication of results presented in Table 5, Panel D (Table 6, Panels D and E) but using only state level entities data). Panel D considers the value-weighted average yield premia ($YieldPremium_{it}$) for new uninsured debt issues by only state level entities. The $Post$ main effect is subsumed by quarter-year (year) fixed effects in Panel A (B through D) and the $ExpectFavorable_{Stateit}$, $ExpectAdverse_{Stateit}$, and $Rank_{Stateit}$ main effects are subsumed by state fixed effects. Standard errors are clustered by state and by quarter-year (year) in Panel A (B through D). Two-tailed confidence levels are shown parenthetically.

Variables are defined similarly as in Appendix A, with the $State$ subscript indicating the variable is created using only state entity debt or state entity alternative financing sources.

VII. CONCLUDING REMARKS

Evidence provided in the study supports the proposition that mandatory financial reporting and disclosure standardization provided through GASB 34 fosters separation in the municipal debt market. Atypical debt issuing behavior surrounding the year that governments first disclose GASB 34-compliant ACFRs suggest a characterization where government decision-makers strategically incorporate expectations about GASB 34 consequences into their financing decisions. These insider expectations manifest as changes in characteristics of new municipal debt issues following GASB 34 implementation. Governments expecting favorable consequences were more likely to substitute (1) toward uninsured debt and away from insured debt, and (2) toward debt financing and away from financing alternatives following GASB 34, relative to governments expecting adverse consequences. The evidence informs recent and on-going GASB deliberations regarding changes to GASB 34 financial reporting requirements.³⁸

REFERENCES

- Akerlof, G. 1970. The market for “lemons”: Quality uncertainty and the market mechanism. *The Quarterly Journal of Economics* 84 (3): 488–500. <https://doi.org/10.2307/1879431>
- Armstrong, C., M. Barth, A. Jagolinzer, and E. Riedl. 2010. Market reaction to the adoption of IFRS in Europe. *The Accounting Review* 85 (1): 31–61. <https://doi.org/10.2308/accr.2010.85.1.31>
- Baber, W., and A. Gore. 2008. Consequences of GAAP disclosure regulation: Evidence from municipal debt issues. *The Accounting Review* 83 (3): 565–592. <https://doi.org/10.2308/accr.2008.83.3.565>
- Baber, W., and P. Sen. 1986. The political process and the use of debt by state governments. *Public Choice* 48 (3): 201–215. <https://doi.org/10.1007/BF00051618>

³⁸ <https://www.gasb.org/page/PageContent?pageId=/projects/financial-reporting-modelreexamination-of-statements-34-35-37.html>

- Bahl, R., and W. Duncombe. 1993. State and local debt burdens in the 1980s: A study in contrast. *Public Administration Review* 53 (1): 31–40. <https://doi.org/10.2307/977274>
- Baird, A. 2017. Lawmakers consider measure shifting school debt from state to local governments. *Alaska's News Source* (February 28). <https://www.ktuu.com/content/news/Lawmakers-adopt-measure-shifting-school-debt-costs-from-state-to-local-governments-415046033.html>
- Barth, M., and K. Schipper. 2008. Financial reporting transparency. *Journal of Accounting, Auditing & Finance* 23 (2): 173–190. <https://doi.org/10.1177/0148558X0802300203>
- Beck, A. 2018. Opportunistic financial reporting around municipal bond issues. *Review of Accounting Studies* 23 (3): 785–826. <https://doi.org/10.1007/s11142-018-9454-2>
- Bloch, R. 2016. Assessing the impact of GASB statement no. 34: The perceptions of municipal bond analysts. *Municipal Finance Journal* 37 (2): 51–71.
- Boise City, Idaho. 2002. Comprehensive annual financial report for the fiscal year September 30, 2002.
- Callahan, C., and T. Waymire. 2015. The GASB no. 34 impact of budget-to-actual variances on bond ratings: Evidence from U.S. cities. *Journal of Governmental & Nonprofit Accounting* 4 (1): 32–52. <https://doi.org/10.2308/ogna-51001>
- Census of Governments (COG). 2008. Historical Overview of U.S. Census Bureau Data Collection Activities about Governments. Washington, DC: COG.
- Cornaggia, J., K. Cornaggia, and R. Israelsen. 2018. Credit ratings and the cost of municipal financing. *The Review of Financial Studies* 31 (6): 2038–2079. <https://doi.org/10.1093/rfs/hhx094>
- Costello, A., R. Petacchi, and J. Weber. 2017. The impact of balanced budget restrictions on states' fiscal actions. *The Accounting Review* 92 (1): 51–71. <https://doi.org/10.2308/accr-51521>
- Cournoyer, C. 2013. The secret tax explosion. *Governing* (August 27). <https://www.governing.com/archive/col-secret-tax-explosion.html>
- Cuny, C. 2016. Voluntary disclosure incentives: Evidence from the municipal bond market. *Journal of Accounting and Economics* 62 (1): 87–102. <https://doi.org/10.1016/j.jacceco.2016.04.004>
- deHaan, E., and A. Glover. 2023. Trading hours and retail investment performance. Graduate School of Business Stanford University and Foster School of Business, University of Washington (Working paper). https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4486721
- Denison, D., M. Hackbart, and M. Moody. 2009. Intrastate competition for debt resources. *Public Finance Review* 37 (3): 269–288. <https://doi.org/10.1177/1091142108323486>
- Diamond, D. W., and R. E. Verrecchia. 1991. Disclosure, liquidity, and the cost of capital. *The Journal of Finance* 46 (4): 1325–1359. <https://doi.org/10.1111/j.1540-6261.1991.tb04620.x>
- Edmonds, C., J. Edmonds, B. Vermeer, and T. Vermeer. 2017. Does timeliness of financial information matter in the governmental sector? *Journal of Accounting and Public Policy* 36 (2): 163–176. <https://doi.org/10.1016/j.jaccpubpol.2017.02.002>
- Fisher, R., and R. Wassmer. 2014. The issuance of state and local debt during the United States great recession. *National Tax Journal* 67 (1): 113–150. <https://doi.org/10.17310/ntj.2014.1.04>
- Goodman, C., and S. Leland. 2019. Do cities and counties attempt to circumvent changes in their autonomy by creating special districts? *The American Review of Public Administration* 49 (2): 203–217. <https://doi.org/10.1177/0275074018804665>
- Gore, A., K. Sachs, and C. Trzcinka. 2004. Financial disclosure and bond insurance. *The Journal of Law and Economics* 47 (1): 275–306. <https://doi.org/10.1086/380472>
- Governmental Accounting Standards Board (GASB). 1999. Statement No. 34: Basic Financial Statements—And Management's Discussion and Analysis—For State and Local Governments. Norwalk, CT: GASB.
- Governmental Accounting Standards Board (GASB). 2011. The Timeliness of Financial Reporting by State and Local Governments Compared with the Needs of Users. A GASB Research Brief. Norwalk, CT: GASB.
- Governmental Accounting Standards Board (GASB). 2021. Statement No. 98: The Annual Comprehensive Financial Report. Norwalk, CT: GASB.
- Governmental Accounting Standards Board (GASB). 2023. Project Pages: Financial Reporting Model—Reexamination of Statements 34, 35, 37, 41, and 46 and Interpretation 6. Norwalk, CT: GASB.
- Greer, R. 2015. Overlapping local government debt and the fiscal common. *Public Finance Review* 43 (6): 762–785. <https://doi.org/10.1177/1091142114545678>
- Greer, R., and D. Denison. 2016. Determinants of debt concentration at the state level. *Public Budgeting & Finance* 36 (4): 111–130. <https://doi.org/10.1111/pbaf.12118>
- Hackbart, M., and J. Leigland. 1990. State debt management policy: A national survey. *Public Budgeting & Finance* 10 (1): 37–54. <https://doi.org/10.1111/1540-5850.00849>
- Healy, P., and K. G. Palepu. 2001. Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature. *Journal of Accounting and Economics* 31 (1–3): 405–440. [https://doi.org/10.1016/S0165-4101\(01\)00018-0](https://doi.org/10.1016/S0165-4101(01)00018-0)
- Healy, P., and J. Wahlen. 1999. A review of the earnings management literature and its implications for standard setting. *Accounting Horizons* 13 (4): 365–383. <https://doi.org/10.2308/acch.1999.13.4.365>

- Henke, T., and J. Maher. 2016. Government reporting timeliness and municipal credit market implications. *Journal of Governmental & Nonprofit Accounting* 5 (1): 1–24. <https://doi.org/10.2308/ogna-51601>
- Heß, S. 2017. Randomization inference with Stata: A guide and software. *The Stata Journal: Promoting Communications on Statistics and Stata* 17 (3): 630–651. <https://doi.org/10.1177/1536867X1701700306>
- Jacob, K. 2004. Credit-rating effects of GASB 34. *Public Management* 86 (9): 26–28.
- Johnson, C. 1999. State government credit quality: Down, but not out! *Public Administration Review* 59 (3): 243–249. <https://doi.org/10.2307/3109952>
- Khumawhala, S. B., and D. G. Neely. 2008. Compliance with governmental accounting standards. Governmental Accounting Standards Board (Working paper).
- Kido, N., R. Petacchi, and J. Weber. 2012. The influence of elections on the accounting choices of governmental entities. *Journal of Accounting Research* 50 (2): 443–476. <https://doi.org/10.1111/j.1475-679X.2012.00447.x>
- McDonough, R. P., and C. J. Yan. 2021. Maintaining maintenance: The real effects of financial reporting for infrastructure. Rutgers University (Working paper). https://www.brookings.edu/wp-content/uploads/2021/05/McDonough-Yan-Paper_July2021.pdf
- Moldogaziev, T., S. Kioko, and W. Hildreth. 2017. Impact of bankruptcy eligibility requirements and statutory liens on borrowing costs. *Public Budgeting & Finance* 37 (4): 47–73. <https://doi.org/10.1111/pbaf.12175>
- Moody's Investors Service. 2016. *Rating Methodology: Special Assessment/Special Property Tax (Non-Ad Valorem) Debt (Report no. 1044931)*. New York, NY: Moody's.
- Mullins, D., and B. Wallin. 2004. Tax and expenditure limitations: Introduction and overview. *Public Budgeting & Finance* 24 (4): 2–15. <https://doi.org/10.1111/j.0275-1100.2004.00344.x>
- Musgrave, R. A., and P. B. Musgrave. 1989. *Public Finance in Theory and Practice*, 5th edition. New York, NY: McGraw-Hill.
- Nakhmurina, A. 2024. Does fiscal monitoring make better governments? Evidence from US municipalities. *The Accounting Review* (forthcoming). <https://doi.org/10.2308/TAR-2020-0251>
- National Association of State Auditors, Comptrollers, and Treasurers (NASACT). 2014. *State Comptrollers Technical Activities and Functions*. Lexington, KY: NASACT.
- Patton, T., and P. Hutchison. 2013. Historical development of the financial reporting model for state and local governments in the United States from late 1800s to 1999. *The Accounting Historians Journal* 40 (2): 21–53. <https://doi.org/10.2308/0148-4184.40.2.21>
- Patton, T., S. Patton, and T. Waymire. 2022. *Accounting for Governmental & Nonprofit Organizations*, 2nd edition. Naperville, IL: Cambridge Business Publisher.
- Pew Charitable Trusts (Pew). 2013. *The State Role in Local Government Financial Distress*. Washington, DC: Pew.
- Pew Charitable Trusts (Pew). 2016. *State Strategies to Detect Local Fiscal Distress*. Washington, DC: Pew.
- Pew Charitable Trusts (Pew). 2017. *Strategies for Managing State Debt*. Washington, DC: Pew.
- Plummer, E., P. D. Hutchison, and T. K. Patton. 2007. GASB no. 34's governmental financial reporting model: Evidence on its information relevance. *The Accounting Review* 82 (1): 205–240. <https://doi.org/10.2308/accr.2007.82.1.205>
- Regens, J., and T. Lauth. 1992. Buy now, pay later: Trends in state indebtedness, 1950–1989. *Public Administration Review* 52 (2): 157–161. <https://doi.org/10.2307/976469>
- Shi, Y., and R. Hendrick. 2020. The problem of the fiscal common-pool: Is there an overlap effect on state and local debt? *Journal of Public Budgeting, Accounting & Financial Management* 32 (2): 137–157. <https://doi.org/10.1108/JPBAFM-11-2019-0176>
- Spence, M. 1973. Job market signaling. *The Quarterly Journal of Economics* 87 (3): 355–374. <https://doi.org/10.2307/1882010>
- Vermeer, T., T. Patton, and A. Styles. 2011. Reporting of general infrastructure assets under GASB statement no. 34. *Accounting Horizons* 25 (2): 381–407. <https://doi.org/10.2308/acch-10029>
- Walsh, M. 2012. With no vote, taxpayers stuck with tab on bonds. *The New York Times* (June 25). <https://www.nytimes.com/2012/06/26/business/surprised-taxpayers-are-paying-for-bonds-they-did-not-vote-on.html>
- White, R., and M. Webb. 2021. Randomization inference for accounting researchers. *Journal of Financial Reporting* 6 (2): 129–141. <https://doi.org/10.2308/JFR-2021-006>

APPENDIX A
Variable Definitions

Variables	Definition
ΔGSP_{it}	Quarterly percent change in state i gross state product from quarter $t-1$ to t . (Source: Bureau of Economic Analysis)
$\Delta GPSAnnual_{it}$	Annual percent change in state i gross state product from year $t-1$ to t . (Source: Bureau of Economic Analysis)
$\%Insured-IW_{it}$	Number of government i , quarter t public debt issues that are insured, divided by the total number of debt issues within government i in quarter t . (Source: Mergent)
$\%Insured-VW_{it}$	Dollar amount of government i , quarter t public debt issues that are insured, divided by the total amount of government i , quarter t debt issues. (Source: Mergent)
$ExpectAdverse_i$	Indicator set equal to 1 for the 15 governments (i) with annualized unexpected new debt per capita above the median in FY2002 and below the median in FY2004, and 0 otherwise. (Source: Mergent)
$ExpectFavorable_i$	Indicator set equal to 1 for the 15 governments (i) with annualized unexpected new uninsured debt per capita below the median in FY2002 and above the median in FY2004, and 0 otherwise. (Source: Mergent)
$FeesRev_{it}$	Government i , year t fees revenue (source: COG GFS), scaled by the sum of fees revenue and government i , year t total new public debt issues (source: Mergent) within government i in year t .
$InsuredDebtPC_{it}$	Dollar amount of government i , quarter t insured public debt issues scaled by state population. (Source: Mergent)
$IntgovtRev_{it}$	Government i , year t intergovernmental transfers (source: COG GFS), scaled by the sum of intergovernmental transfers and government i , quarter t total new public debt issues. (Source: Mergent)
$Post$	Indicator set equal to 1 for FY2004 through FY2007 that distinguishes the post-GASB 34 period, and 0 otherwise.
$Rank$	Government i 's rank as a function of the difference in unexpected new uninsured debt per capita in FY2004 and FY2002. Values range from 1/50 through 50/50; higher values reflect a government's increasingly favorable expected consequences of post-GASB 34 comparisons.
$TaxRev_{it}$	Government i , year t tax revenue (source: COG GFS), scaled by the sum of tax revenue and government i , year t total new public debt issues. (Source: Mergent)
$TotalDebtPC_{it}$	Dollar amount of government i , quarter t total public debt (uninsured and insured) issues scaled by state population. (Source: Mergent)
$TotalRev_{it}$	Government i , year t revenue from all sources (source: COG GFS), scaled by the sum of revenue from all sources and government i , year t total new public debt issues. (Source: Mergent)
$UninsuredDebtPC_{it}$	Dollar amount of government i , quarter t uninsured public debt issues scaled by state population. (Source: Mergent)
$YieldPremium_{it}$	Value-weighted average yield premium for new debt issues calculated using only new debt issues by state entities (i.e., the sample that remains after eliminating local governments and special district governments).

Variables are displayed in alphabetical order.