

Separation in the Municipal Debt Market Following GASB 34 Implementation

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Abstract: This paper examines state and local governments' strategic behavior regarding debt issues in anticipation of, and in response to, Governmental Accounting Standards Board Statement No. 34 (GASB 34). GASB 34 facilitated financial reporting comparability among governments, and we consider whether this comparability created meaningful separation in the municipal debt market. We argue that governments anticipating favorable consequences delayed, while governments that anticipated adverse consequences accelerated, debt issues surrounding the GASB 34 implementation period. Post GASB 34, governments anticipating favorable consequences were less likely to use third-party debt insurance and more likely to pursue debt relative to alternative financing sources, relative to governments that anticipated adverse consequences. These findings are consistent with separation in the municipal debt market.

Keywords: governmental accounting standards; municipal debt market; GASB 34

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1. Introduction

We investigate how state and local governments respond to financial reporting and disclosure comparability anticipated by Government Accounting Standards Board Statement No. 34 (hereafter GASB 34).¹ Issued in June 1999 with implementation first required in June 2002, GASB 34 touches all important facets of external financial reporting and disclosure by state and local governments (Patton and Hutchison 2013). This standard provides the basic blueprint for a financial report, and the most significant requirements prescribe accrual-based accounting, consolidated presentation of governmental and business-type activities, and specific disclosures regarding management's discussion and analysis (MD&A). Despite the standard's importance, no study examines how these significant financial reporting and disclosure changes influenced governments' strategic behavior to issue publicly-traded debt.

We argue that anticipated financial comparability post-GASB 34 facilitates sorting and classifying municipal debt issuers. Some issuers expected to be favorably affected by the new accounting standard, whereas others expected to be adversely affected. As a result, increased financial comparability results in issuers behaving in a manner that reflects their expectations. Issuers being differentially affected by GASB 34 implementation results in separation in the municipal debt market.

Conventional empirical approaches in the governmental accounting literature focus on mean effects for samples of municipal debt issuers, such as issuers within states that have more

¹ GASB notes 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 1999, ¶344(e), emphasis added).

(or less) stringent government reporting requirements. If separation results in offsetting debt market consequences within the sample, such approaches may be unable to document the effects.² We differentiate our empirical approach by examining higher moments of debt issuance distributions to document evidence consistent with separation. In doing so, we identify governments that – based on their behavior just prior to GASB 34 implementation – anticipated primarily favorable versus adverse consequences. By acknowledging the potential for separation in our research design, our approach allows us document effects of GASB 34 that are heretofore unexplored by prior research.

To consider separation effects the municipal setting, it is important to acknowledge that state and local governments are a nexus of financially and politically interrelated (and often overlapping) units that collectively respond to citizen demands for public services. This perspective is justified both conceptually and pragmatically. The US Constitution implicitly delegates the creation, oversight, and control of governments within (but not across) state boundaries to state officials.³ Both prior research and anecdotal observation indicate how state and local government responsibility for providing services can shift between geographically overlapping entities, and that new overlapping entities can be created to administer and finance these services (e.g., Pew 2016; Pew 2017). For example, property taxes collected from occupants of a given address often finance debt at the city, county, state, and special district levels.⁴ Thus, overlapping governments are not independent with respect to their ability to raise revenues,

² If issuers' behaviors are associated with their expectations regarding GASB 34's consequences, then issuers expecting favorable consequences respond opposite to issuers expecting adverse consequences. These offsetting behaviors (and associated debt market consequences) may be undetectable in the aggregate.

³ To illustrate the relevance of the distinction to policy debate, the Pew Charitable Trust describes how “[s]tates, the primary government, can retain most of the power to borrow or share this responsibility with other entities.” Furthermore, “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; p 17).

⁴ <https://www.taxpolicycenter.org/briefing-book/how-do-state-and-local-property-taxes-work>

control expenditures, or issue debt. Related, special districts can be created (wholly or in part) to issue debt – often with a general government as the ultimate obligor (Pew 2017).

Such nuances are important to this investigation because GASB 34 consequences can be obfuscated when financial burdens shift among overlapping or financially related entities. State boundaries are the lowest level where we can meaningfully apply this perspective. Thus, we aggregate debt issued by all state and local governments within state boundaries during each time period.

This procedure has several advantages: it eliminates concerns about debt burden shifting, accommodates financial dependencies among governments within a state, and comprehensively reflects the non-federal government debt burden on individual citizens. Our characterization implies that from a lender perspective, the financial viability of a state government is relevant to assessments of debt issues by local governments within a state. Conversely, the financial viabilities of local governments within a state are relevant to assessments of state government debt issues.

The empirical analyses address characteristics of municipal debt issues during the nine-years, 1998 through 2007, that encompasses GASB 34 implementation. We posit that municipal debt issuers respond strategically to expected consequences of GASB 34 accounting and disclosure changes. Governments that anticipate favorable debt financing consequences from GASB 34 disclosures are expected to delay debt issues until the new standard is in place. Oppositely, governments that anticipate adverse debt financing consequences are expected to accelerate debt issues to before the new standard is effective.

Issuers have a variety of options in debt issuances, and we focus on what we view to be the most important strategic elements: when to issue debt (and how much to issue), whether to

obtain third-party insurance for debt issues, and the substitution of debt and taxes. We first document that new debt issue distributions following GASB 34 indicate greater dispersion, consistent with separation in municipal debt markets. We then consider how the use of third-party insurance and source of financing decisions (i.e., issuing debt versus raising taxes) differs between governments expecting favorable versus governments adverse consequences. We expect and find that governments expecting favorable consequences are less likely to rely on third-party insurance post-GASB 34. This finding is consistent with GASB 34 disclosures revealing positive financial information about these governments, such that they are less likely to benefit from debt insurance following GASB 34 implementation. We also expect and find that governments expecting favorable consequences are more likely than governments expecting adverse consequences to choose debt relative to tax financing post-GASB 34. We interpret this result as evidence that debt market positions improve for governments expecting favorable consequences relative to governments expecting adverse consequences following GASB 34 implementation.⁵ This finding supports further the proposition that GASB 34 disclosures reveal comparably positive (negative) financial information about governments expecting favorable (adverse) consequences. Results for the use of debt insurance and the substitution between debt and other financing sources are robust when we consider time trend differences between governments expecting favorable versus adverse consequences during the sample period.

This study contributes to the governmental accounting literature on several dimensions. While prior studies consider the role of accounting standards in governmental financial reporting decisions (Kido et al. 2012; Costello et al. 2016; Beck 2018), we are the first to document a

⁵ This finding is consistent with a lower cost of debt for governments expecting favorable consequences following GASB 34. As described in Section 3, debt costs are tangential to our focus on issuer (rather than investor) behavior and data limitations further preclude directly examining debt costs.

change in the distribution of governmental debt issues around an accounting standard change. Such separation in the municipal debt market supports the notion that some issuers experienced favorable, while others experienced adverse, consequences from GASB 34 accounting and disclosure changes. We also document that issuers behave strategically in anticipation of debt market consequences of GASB 34 implementation in terms of debt issue timing, the use of third-party insurance, and the use of debt relative to other financing sources to meet citizen demands for public services.

2. Financial Reporting by Governments

2.1 Governmental financial reporting prior to GASB Statement No. 34

Comprehensive annual financial reports (CAFRs) provide information on governmental general activities (e.g., revenues from collecting taxes and expenditures from providing core services, such as education and public safety) and enterprise activities (e.g., user fees charged to finance a specific public service). Historically, CAFRs exhibited considerable variation in scope and content across US state and local governments (Baber and Gore 2008). Prior to GASB 34, many (but not all) governments used only fund accounting to report governmental activities. Fund accounting, a modified cash basis of accounting that focuses on increases and decreases in financial resources during the current period, facilitates short-term stewardship and fiscal accountability by comparing annual budgets (organized by “funds” with designated purposes) to actual results (GASB 1999). Fund accounting’s short-term focus does not provide potential investors – for example, investors in long-term debt – with a long-term financial picture. Moreover, no accepted framework existed to guide the scope of activities included within funds, including which funds were reported in government CAFRs. Zimmerman (1977) aptly described

diversity in these (and other) governmental financial reporting practices that existed prior to GASB 34 as a “municipal accounting maze.”

Comparing governments’ financial information is more difficult in the absence of a unifying financial reporting framework and disclosure model (e.g., prior to GASB 34). We argue that the financial reporting framework that existed prior to GASB 34 undermined governments’ abilities to communicate credibly their financial quality to outsiders (Akerlof 1970; Spence 1973; Diamond and Verrecchia 1991). Voluntary accounting and disclosure practices coupled with independent audits are potential solutions to this information problem (Healy and Palepu 2001), but accrual-based financial reporting that incorporated long-term assets and obligations did not exist prior to GASB 34. To the point, auditors opine whether financial statements fairly present a government’s financial position, changes in financial position and cash flows, in conformity with U.S. generally accepted accounting principles. Accounting and reporting standards were differentially set and enforced both within and between states governments, however. Lacking a common framework, auditors typically indicated compliance with various state requirements.

This reporting environment undermined governments’ abilities to provide credible information to potential investors about consolidated government-wide activities or long-term assets and obligations. Even ignoring the auditability issue, financial information provides the greatest value to potential investors when it facilitates comparisons between investment alternatives. Such comparability is undermined when governments report using state-specific models. GASB summarizes these challenges by noting the following when describing financial reporting problems that GASB 34 was designed to address and resolve:

“The fragmented nature of governmental fund reporting makes it *difficult for most users* of the financial statements to *obtain a clear picture of total governmental activities*. Users

also found it difficult to obtain aggregated information about government services and the revenues raised to finance them because activities cross funds, and individual transactions can be divided and reported in more than one fund or fund type. In addition, governments have considerable flexibility both in the choice of funds for recognizing transactions and in moving resources from one fund to another. These features *reduce users' ability to compare information* reported for the same funds or fund types over time, as well as information reported for similar funds or fund types *across similar governments.*" (GASB 1999, ¶248; emphasis added).

2.2 GASB 34 implementation

GASB 34 was issued June 1999, with adoption required for large governments for fiscal years ending after June 15, 2002 (GASB 1999). The standard requires governments to prepare government-wide financial statements on an accrual accounting basis. Application of accrual accounting caused most governments to disclose long-term assets (e.g., infrastructure) and long-term liabilities (e.g., pensions and compensated absences) for the first time. The standard retains fund accounting for general governmental activities, and requires governments to compare actual fund revenues and expenditures with both originally approved and subsequently revised budgets. GASB 34 also requires a reconciliation of modified accrual and full accrual accounting information and prescribes a standard format for MD&A, infrastructure and debt details, and historical economic and demographic data.

These changes provide a common framework for financial reporting and promoted comparability among governments. Consistent with this characterization, public testimony by a State Farm Insurance Company representative during the GASB 34 deliberation process notes "...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" (Reardon 1997). If this characterization applies, then GASB 34 disclosure improvements presumably result in adverse consequences for "weaker borrowers" (i.e., reduced "subsidies") and favorable consequences for "better borrowers" (i.e.,

reduced “penalties”). We investigate how state and local governmental debt issuers respond to the financial reporting comparability anticipated, and ultimately provided, by GASB 34.

3. Hypotheses Development

We argue that GASB 34 promotes separation in municipal debt markets such that debt market positions improve for favorably affected governments strengthen, and weaken for adversely affected governments. If so, then incentives to use debt financing change accordingly following GASB 34 implementation. Thus, we expect greater dispersion of new debt per capita following GASB 34 implementation. Specifically, governments expecting favorable consequences should increase, while governments expecting adverse consequences should decrease, new debt issues post-GASB 34. As a consequence, we expect that the variance in the cross-sectional distribution of new debt issues increases.

We use the term *new debt effects* to reference pre- versus post-GASB 34 changes in new debt per capita dispersion. The following hypothesis, stated in the alternative form, applies.

Hypothesis 1 (New Debt Effects): Dispersion of the distribution of new debt issues increases following GASB 34 implementation.

Note that if governments are differentially affected, standard parametric approaches to empirical investigation can obscure GASB 34 consequences. We therefore consider higher moments of new debt issues distributions to address Hypothesis 1.

We next consider whether governments strategically modify debt issues in anticipation of GASB 34 disclosures. GASB 34 was issued in June 1999, with implementation not required until fiscal years ending after June 15, 2002. Governments had ample time to assess the potential expected consequences of their GASB 34-related disclosures. We posit that government officials are better informed than outsiders about the government’s financial condition, and therefore

behave strategically in debt markets. In particular, governments that expect favorable consequences have incentives (at the margin) to delay abnormal debt issues to post-GASB 34, whereas governments that expect adverse consequences have incentives to accelerate abnormal debt issues before GASB 34 implementation.

We introduce the term *anticipatory effects* to describe such strategic behavior. We expect that anticipatory effects manifest empirically as systematic changes in the time series distribution of new debt issues during the GASB 34 implementation period. Thus, we compare distributions of new debt issues during the GASB 34 implementation period with distributions in adjacent periods. Stated formally,

Hypothesis 2 (Anticipatory Effects): The distribution of new debt issues during the GASB 34 implementation period differs from distributions during adjacent periods.

Strategic issuance of debt implies rejection of the corresponding null hypothesis (that distributions are identical) in favor of distributions that indicate atypically greater frequencies in two categories. In particular, we expect relatively greater frequency of governments to exhibit (1) low debt issues immediately before, but high debt issues immediately following, GASB 34 and (2) high debt issues before, but low debt issues following, GASB 34. We exploit these debt issue patterns to classify governments that satisfy condition (1) as governments expecting favorable consequences, and governments that satisfy condition (2) as governments expecting adverse consequences.

Note that this approach presumes that government officials, being insiders, can better anticipate GASB 34's economic effects than outsiders (e.g., investors and analysts). If so, then we can infer expected GASB 34 consequences according to debt issuance behavior surrounding the implementation period (Akerlof 1970). This reasoning supports our decision to use debt issuances to infer GASB 34 consequences, rather than municipal debt costs or bond ratings.

To illustrate, if GASB 34 reduces debt costs, governments potentially respond by increasing their use of debt financing going forward. Such anticipated increases in debt use can return post-GASB 34 debt costs and debt ratings to pre-GASB 34 levels, such that GASB 34 effects are not detectable. Another practical point that undermines investigations involving debt costs and ratings relates to data availability. New debt yields and bond ratings are commonly missing from the data during the pre-GASB 34 period, as dealers have increasingly reported this information to data disseminators over time.⁶ This feature of the data introduces concerns about selection bias when directly examining changes in debt service costs over time.

Next, we consider how the use of bond insurance changed pre- to post-GASB 34 for governments expecting favorable versus adverse consequences. Governments potentially reduce their financing costs by purchasing bond insurance from a third party with superior credit quality. Both Gore et al. (2004) and Cuny (2016) document substitution between financial disclosure and the purchase of insurance. Consistent with arguments and evidence in these papers, we hypothesize that the favorable (adverse) disclosures provided by GASB 34 manifest as reduced (increased) use of debt insurance by governments expecting favorable (adverse) consequences following GASB 34 implementation. We use the term *insurance effects* when describing this expectation. Stated formally,

Hypothesis 3 (Insurance Effects): The use of debt insurance following GASB 34 implementation increases more for governments expecting adverse consequences than for governments expecting favorable consequences.

Lastly, we investigate the differential implications of GASB 34 on municipal financing decisions. While regulations, guidelines, and financial prudence govern the use of debt versus other financing alternatives (Musgrave and Musgrave 1989), changes in the cost of debt

⁶ For example, yield spreads are missing for 19% of new debt issued from FY1998 through FY2001, but for only 5% of new debt issued from FY2004 through FY2007 (untabulated).

financing that result from GASB 34 disclosures potentially influence governments' financing decisions at the margin. We expect governments expecting favorable consequences increase (and governments expecting adverse consequences decrease) their use of new debt relative to alternative financing sources following GASB 34 implementation. We use the term *substitution effects* to describe this phenomenon. Stated formally,

Hypothesis 4 (Substitution Effects): The use of new debt relative to alternative financing sources following GASB 34 implementation increases more for governments expecting favorable consequences than for governments expecting adverse consequences.

4. Data

4.1 Data sources and sample composition

We use the Mergent Municipal Bond Securities database to identify new public debt issues and new debt characteristics. Table 1 shows the sample selection process at the debt issue level. We begin with all Mergent-listed non-refunding debt issues during the nine-year period July 1, 1998 through June 30, 2007 (i.e., FY1999 through FY2007). Excluding issues with missing identifiers (e.g., issuer name or state), with missing debt characteristics (e.g., type of security or principal amount), and debt issued by Washington DC, Native American tribes and reservations, and US territories and protectorates yields 1,369,494 non-refunding debt issues by 38,261 unique issuers. We obtain 1,230,550 debt issues (34,562 issuers) after excluding conduit debt issued by governments on behalf of a private entity (e.g., nonprofit hospitals, private schools), because the private entity is typically the primary obligor. This sample, which includes insured debt issues, is used to investigate insurance effects (Hypothesis 3). When debt is insured, the third-party insurer's – not issuing government's – financial position is relevant to investors. Thus, when testing hypotheses 1 and 2, we exclude insured issues which yields a sample of 507,911 uninsured non-refunding issues by 23,192 issuers. We obtain state and local government

financing sources required to test Hypothesis 4 from the Census of Governments' Government Finance Survey data (COG GFS).

4.2 Unit of observation

We aggregate debt issues for all state and local governments within a state and quarter into a single unit of observation, yielding 50 observations per quarter-year. This unit of analysis is a fundamental aspect of our research design given our study's objective, which is to investigate whether GASB 34 contributed to separation in the municipal market. Such separation implies that because GASB 34 implementation affected issuers differentially, that issuers respond in potentially offsetting ways. Because state and local governments within a state are financially and politically interrelated, their responses to GASB 34 are not independent from one another. As a result, testing for evidence of separation using an individual state and/or local government unit of analysis is problematic

Our rationale for aggregating units to the state level rests on numerous factors stemming from the fact that states are constitutionally endowed with the ability to create and monitor subordinate governments located within (but not across) state boundaries.⁷ States vary in the extent that they guarantee, monitor, and oversee the issuance of subunit debt (Pew 2016). Further, there is both time-series and cross-section variation in the extent that public services, and financing required to support public services, are centralized at the state government level or decentralized to the local government level. State and local governments within a state collectively respond to citizen demands for public services, but constituents are typically

⁷ To illustrate, Boise City, Idaho notes in its 2002 CAFR that "...cities are 'creatures of the state' authorized for organization by the Idaho Constitution" (p. 6). Because of this, states can set and monitor financial practices of subordinate entities (e.g., cities, counties, cities, and special entities such as school districts and transportation authorities) (Pew 2013; 2016).

indifferent to which level of government (e.g., state versus local versus special district) provides public services.

Importantly, and as a result of this organizational architecture, state and local governments can and do shift service responsibilities and resulting financial burdens between geographically overlapping governmental units.⁸ This practice is often highlighted in property tax bills, which typically list the portion of taxes dedicated to debt service for a number of governmental entities that overlap a given address (e.g., city, county, state, special district). General governments can also create new, legally separate special districts through which to issue debt.

This shifting of financial burdens among governmental units in a state can be direct or indirect. To illustrate, state governments can issue debt to fund education directly, or can create legally separate state-level education authorities that issue debt to fund the same services. Alternatively, the state can reduce education funding, thereby indirectly shifting the responsibility to local governments that may or may not also create separate local-level debt-issuing education authorities. As a practical matter, municipal debt data sources do not link special districts to the general governments that created and oversee them (even when another government is the primary obligor). Therefore, determining the full debt burden for an individual state or local government is not straightforward (Malanga 2013). One strength of our measure is that it approximates the total, non-federal debt burden of the state's citizens. Further, if direct or

⁸ As examples, Walsh (2012), Shafroth (2013), and Goodman and Leland (2018) document how state and local governments shift debt burdens from general government units to authorities that administer specific public services. Baird (2017) describes Alaska's recent proposal to shift debt issues earmarked for education from the state government to local school districts. Pew (2013; 2016) describe policies and practices regarding state assumption of subunit obligations.

indirect within-state debt shifting occurs in response to GASB 34, our measure will not be affected.

Our arguments do not imply that substate units *cannot* be autonomous legal and political entities that issue and service debt independently. However, because of financial interdependencies among governments within a state, investors' financial assessment of one government issuing debt in a state potentially influences financial assessments of other governments in the state. In addition to the aforementioned concerns, we aggregate debt issues within a state-quarter to accommodate for political economies and organizational structures that distinguish the states and further compromise our ability to detect differential consequences of GASB 34 among individual entities. Based on these institutional attributes, we argue that the state is the lowest level where meaningful comparisons of debt obligations among governments can be made. To ease the exposition, we use the term *governments* when referencing the combined state government, special purpose governments, and local governments located within state boundaries.

4.3 Setting the GASB 34 disclosure period

Our empirical design requires identifying the period when GASB 34-compliant financial statement first become publicly available. We expect that GASB 34 separation transpires in the debt market when the preponderance of economically significant issuers within the state disclose GASB-34 compliant financial statements, so that lenders can meaningfully compare municipal debt investment alternatives.

Three features of the governmental financial reporting environment are relevant for aligning the data relative to GASB 34 disclosures. First, GASB 34 imposed staggered implementation, requiring the largest governments – those with FY1999 revenues exceeding

\$100 million – to use the new reporting framework for fiscal years ending after June 15, 2002. Governments with FY1999 revenues between \$10 and 100 million (less than \$10 million) were required to implement GASB 34 for fiscal years ending after June 15, 2003 (2004). Thus, GASB 34 compliance unfolds over a two year period, with the largest and most economically significant governments required to disclose first.

Second, fiscal year ends vary across governments. Forty-six of 50 state governments have June 30 fiscal year ends, and 62% (32%) of the 50 largest cities in each state have June 30 (December 31) fiscal year ends. Issuers with a June fiscal year end are expected to disclose GASB-34 compliant financial information well in advance of issuers with a December fiscal year end.

Third, the time between a government’s fiscal year end and the date its CAFR becomes publicly available varies even among governments with the same fiscal year end. Unlike publicly traded companies, governments generally do not face regulatory deadlines for making audited financial statements publicly available.

To determine when the most economically important governments implemented GASB 34, we hand-collect CAFRs for all 50 state governments and for the largest city government within each state (based on 2000 Census population data) for the first fiscal year ending after June 15, 2002. All 50 state governments and 94% of the largest local governments for which we can obtain CAFRs are GASB 34-compliant as of FY2002.⁹ Appendix A provides a list of state (Panel A) and largest city (Panel B) governments, sorted in descending order by CAFR

⁹ We accessed various resources (e.g., online search, individual email request, phone requests, etc.) to obtain CAFRs for the largest local government within each state. We found CAFRs for the first fiscal year ending after June 15, 2002 for 32 of the largest local governments in each of the 50 states, 30 of which are GASB 34-compliant. No reliable data source provides FY1999 revenue for the population of individual municipal issuers, so GASB 34 implementation rules cannot be used directly.

transmittal date (i.e., the date the CAFR became publicly available) for the first fiscal year ending after June 15, 2002. We find that 92% of state governments and 83% of the largest city governments transmit GASB 34-compliant FY2002 CAFRs between July 1, 2002 and June 30, 2003.¹⁰

Based on these findings, we designate the period July 1, 2002 through June 30, 2003 (FY2003) as the GASB 34 “transition period,” and exclude data from this fiscal period from the empirical analyses. We label the four fiscal years prior to the transition period (FY1999 through FY2002) as the pre-GASB 34 period ($POST = 0$) and the four fiscal years following the transition period (FY2004 through FY2007) as the post-GASB 34 period ($POST = 1$). Figure 1 summarizes how we deploy observations to address each hypothesis.

5. Empirical Analyses

For ease of exposition, particularly given that our sample size varies by hypothesis, we present empirical results (including descriptive statistics, tests of mean differences, correlations, and regression results) by hypothesis.

5.1 Pre- versus Post- GASB 34 distributions of new debt issues (Hypothesis 1)

Separation in the municipal debt market implies that governments are differentially affected by GASB 34 implementation. If so, then procedures that focus on distribution means or medians may be unable to detect separation. We therefore compare moments of the distributions beyond the first order. Using our full sample of 1,800 observations (centered on the GASB 34 transition period), we estimate:

¹⁰ For these issuers, the mean (median) reporting lag is 221 (183) days for state governments and 183 (168) days for the largest city governments. These statistics are in line with estimates provided in other studies. GASB (2011) documents a mean (median) reporting lag of 199 (177) days for states and 181 (173) days for localities from 2006-2008; Edmonds et al. (2017) find a mean reporting lag of 152 days for bond-issuing cities and counties from 2004-2007; and Henke and Maher (2016) report a mean reporting lag of 174, 200, 135, and 198 days for city, county, special district, and state governments, respectively from 2014-2015.

$$[1] \quad \mathit{NEWDEBTPC}_{i,t} = \alpha_0 + \alpha_1 \mathit{GSP}_{i,t} + \mathit{STATE} \text{ fixed effects} + \mathit{QTRYR} \text{ fixed effects} + \varepsilon_{i,t}$$

where $\mathit{NEWDEBTPC}_{i,t}$ is the aggregate amount of new uninsured and non-refunding debt per capita issued by governments within the boundaries of state i ($i = 1, \dots, 50$) in quarter-year t ($t = 1, \dots, 32$). Table 2, Panel A indicates that mean (median) quarterly new debt per capita ($\mathit{NEWDEBTPC}_{i,t}$) is \$102.99 (\$73.43).

To consider state-specific and macroeconomic effects that influence new debt issues, specification [1] includes fixed effects for state and quarter-year fixed effects (STATE_i and QTRYR_t) and the state i , quarter t change in gross state product ($\mathit{GSP}_{i,t}$). The covariate GSP considers state-specific trends in the state macro-economy beyond what is considered by fixed effects. We interpret residuals $\varepsilon_{i,t}$ from specification [1], designated *unexpected new debt* in the following discussion, as the new debt per capita unexplained by the state and time fixed effects and the state-time specific quarterly change in gross state product. Continuous unbounded variables are winsorized at the 1st and 99th percentiles to mitigate the effects of outliers.¹¹

We compare distributions of unexpected new debt for the four-year pre-GASB 34 period (FY1999 through FY2002) versus the post-GASB 34 period (FY2004 through FY 2007). If GASB 34 promotes separation, then we expect the post-GASB 34 distribution to be more dispersed (e.g., exhibit higher variance) than the pre-GASB 34 distribution.

Figure 2 displays results for these comparisons. Panel A shows the probability density function (PDF) and cumulative density function (CDF) for distributions of unexpected new debt before and after GASB 34 implementation. Panel B shows distribution characteristics. Observe that the variance of unexpected new debt following GASB 34 implementation is more than twice the variance before GASB 34. Panel C shows related statistical tests. The Brown-Forsythe (B-F)

¹¹ Regression results are consistent when (1) using robust regression, and (2) eliminating observations with a Cook's distance greater than $4/n$ (Leone et al. 2019).

procedure, a nonparametric test of two distribution variances, indicates a statistically significant difference ($p < 0.001$). The two-sample Kolmogorov-Smirnoff (K-S) procedure, a nonparametric test of differences in distributions, also indicates statistically significant pre- versus post-GASB 34 comparisons ($p < 0.001$). These comparisons, considered both visually and statistically, indicate greater dispersion following GASB 34 implementation. Hence, the evidence supports our Hypothesis 1 prediction, that dispersion of unexpected new debt per capita increases following GASB 34 implementation, and the proposition that GASB 34 creates separation in the municipal debt market.

5.2 Anticipatory effects of GASB 34 – new debt issues (Hypothesis 2)

We use unexpected new debt extracted from specification [1] to address Hypothesis 2, which considers whether anticipatory effects manifest as changes in time series distributions of new debt issues during the GASB 34 implementation period. For each government, we cumulate four quarterly residuals to obtain *unexpected new debt per capita* for each fiscal year.

We then partition the nine-year FY1999 to FY2007 sample period into equal three-year periods: FY2002 through FY2004 (designated the *implementation period*) is centered between the adjacent three-year periods, FY1999 through FY2001 and FY2005 through FY2007. For the first and third years in each the three-year period, we classify the fifty governments according to whether cumulative unexpected new debt for the year is above or below the median. If governments modify debt issues in anticipation of expected GASB 34 consequences – that is, if Hypothesis 3 applies – then we anticipate an atypical classification of governments during the three-year implementation period relative to the three-year windows.

Table 2, Panels B and C present 2 x 2 classifications of the 50 governments according to whether cumulative annual unexpected new debt per capita exceeds distribution medians for the

first and third years within each three-year period.¹² Panel B shows the distribution of the 50 governments for the FY2001 through FY 2003 implementation period, centered on FY2002 GASB 34 implementation. If observations are equally distributed among the four cells, this suggests that governments do *not* systematically increase or decrease debt issues. We find that observations more frequently fall on the southwest-northeast diagonal than on the northwest-southeast diagonal. This result indicates that governments that decrease (increase) unexpected new debt during the year prior to GASB 34 implementation increase (decrease) new debt issues during the year following GASB 34 implementation.

To determine if the pattern of unexpected new debt surrounding GASB 34 implementation is unusual, we compare the 2 x 2 distribution during the implementation period, shown in Panel B, to 2 x 2 distributions during the adjacent three-year periods, shown in Panel C (FY1999 through FY2001 in Column 1, and FY2005 through FY2007 in Column 2). We find that the distribution during the implementation period differs from the distributions during the adjacent three-year periods. In Panel C, observations more frequently fall on the northwest-southeast diagonal, indicating that most states that issue unexpected new debt above (below) the median in the first year of the three-year period are still above (below) the median in the third year. The final two rows of Panel C indicate that differences between the implementation period distribution and the comparison distributions are statistically significant ($p < 0.01$ and $p < 0.10$, respectively).

Thus, we reject the null hypothesis that debt-issuing behavior during the implementation period and during the comparison period are the same in favor of the alternative Hypothesis 2

¹² Use of median (rather than mean or signed) residuals to classify governments facilitates statistical comparisons between 2 x 2 distributions. In particular, the chi-square tests require identical marginal distributions. Comparisons of distributions of the 50 governments (untabulated) using mean or signed residuals are similar qualitatively to what is shown in Table 2.

that distributions differ systematically. Furthermore, we find greater frequencies of governments increase (decrease) debt issues immediately before and then decrease (increase) debt issues immediately following GASB 34 implementation. We interpret this result as evidence that governments, who are better informed than outside investors about what GASB 34 disclosures will reveal, systematically modify debt issues in order to minimize debt service costs.

5.3 *Classifying governments according to strategic behavior*

We exploit this interpretation to distinguish governments that anticipate favorable investor comparisons that reduce debt costs following GASB 34 disclosures from governments that expect adverse investor comparisons that increase debt costs. We designate the 15 governments in the northeast cell in Table 2, Panel B as those expecting favorable consequences ($EXPECT_FAV = 1$), and the 15 governments in the southwest cell as those expecting adverse consequences ($EXPECT_ADV = 1$).¹³ Notice that 20 governments do not fall into either classification.

We also construct a ranked variable that varies directly with the extent that governments anticipate favorable GASB 34 consequences. For each state, we compute $RANK_i$ as the rank of unexpected new debt in FY2004 less unexpected new debt in FY2002. Values range from 1 through 50, with higher $RANK_i$ values indicating government i 's expectation of favorable consequences (at the margin) for delaying debt issues until the post-GASB 34 period.¹⁴

¹³ Governments expecting favorable consequences include California, Colorado, Delaware, Idaho, Illinois, Indiana, Massachusetts, Maine, Missouri, North Carolina, New Jersey, New Mexico, Oregon, Texas, and Wyoming. Governments expecting adverse consequences include Alabama, Arizona, Connecticut, Georgia, Kentucky, Louisiana, Minnesota, Mississippi, Montana, North Dakota, Pennsylvania, South Carolina, South Dakota, Wisconsin, and West Virginia.

¹⁴ Note that $EXPECT_FAV$ and $EXPECT_ADV$ classifications cannot be directly inferred from $RANK$. To illustrate, Nebraska governments exhibit the fifth highest increase in unexpected new debt per capita ($RANK=46$), but these governments are not classified as ones expecting favorable consequences because their unexpected new debt in FY2002 exceeds the sample median during FY2002. Thus, $RANK$ represents an alternative measure of behavior surrounding GASB 34 implementation.

To investigate separation in municipal debt market post-GASB 34 along the dimension of governments expecting favorable versus adverse consequences, we estimate regression specifications of the following form:

$$[2a] \quad Y_{i,t} = \beta_0 + \beta_1 (POST_t \times EXPECT_FAV_i) + \beta_2 (POST_t \times EXPECT_ADV_i) + \mathbf{Controls} + \varepsilon_{i,t}$$

$$[2b] \quad Y_{i,t} = \delta_0 + \delta_1 (POST_t \times RANK_i) + \mathbf{Controls} + \varepsilon_{i,t},$$

where $Y_{i,t}$ is the government i , quarter t new debt characteristic and $POST = 1$ distinguishes observations following GASB 34 implementation. Control variables are identical to those in specification [1]. Notice that the $POST$ main effect is subsumed by $QTRYR$ fixed effects and the $EXPECT_FAV$ and $EXPECT_ADV$ main effects are subsumed by $STATE$ fixed effects.

In specification [2a], we are interested in estimates of parameters β_1 and β_2 on the interactions ($POST \times EXPECT_FAV$) and ($POST \times EXPECT_ADV$). In particular, we address the null hypothesis $\beta_1 = \beta_2$. Rejecting the null in favor of $\beta_1 \neq \beta_2$ indicates differences between governments expecting favorable versus adverse consequences from GASB 34 disclosures, consistent with separation in the municipal debt market following GASB 34.

Specification [2b] employs variable $RANK$, which indicates the extent that governments anticipate favorable GASB 34 consequences. We therefore address the null hypothesis $\delta_1 = 0$. Rejecting the null in favor of $\delta_1 \neq 0$ supports the proposition that debt issuance changes conditional on government expectations about how GASB 34 affects their investor assessments.

5.4 Validating strategic behavior measures

Figure 3 juxtaposes mean annual unexpected new debt per capita for the 15 governments expecting favorable consequences and the 15 governments expecting favorable consequences during the sample period. As FY2002 and FY2004 data are used to classify and measure anticipatory behavior, the difference between these two types of governments is greater for these

years by construction. Notice, however, that differences exist during both the pre- and post-GASB 34 periods which are not used to construct the *EXPECT_FAV* and *EXPECT_ADV* measures. Specifically, the two groups have relatively stable unexpected debt per capita both pre- and post-GASB 34. However, governments expecting adverse consequences have greater unexpected new debt per capita FY1999 through FY2001 than governments expecting favorable consequences, while the opposite is true FY2005 through FY2007 period. These comparisons contradict the notion that results going forward are attributable to state-specific trends in the use of new debt financing.¹⁵

To consider Figure 3 statistically, we estimate specifications [2a] and [2b] for $Y = NEWDEBTPC$. In specification [2a], governments expecting favorable consequences are more likely to increase (and governments expecting adverse consequences are more likely to decrease) new debt per capita following GASB 34 implementation. Thus, rejecting the null hypothesis $\beta_1 = \beta_2$ in favor of $\beta_1 > \beta_2$ is consistent with separation in the municipal debt market following GASB 34 that incrementally benefits governments expecting favorable consequences relative to governments expecting adverse consequences.

Specification [2b] employs the variable *RANK*, which varies directly with the extent that governments anticipate favorable GASB 34 effects. Thus, rejecting the null hypothesis $\delta_1 = 0$ in favor of $\delta_1 > 0$ supports the proposition that GASB 34 disclosures incrementally benefit governments expecting favorable consequences relative to governments expecting adverse consequences. For both specifications, we exclude the three years used to construct the financial

¹⁵ Note that Figure 3 displays mean residuals from specification [1] which includes state fixed effects. Thus, the figure indicates differences from state means and does not imply that governments expecting favorable consequences issue less total debt per capita before and greater total debt per capita after GASB 34 implementation (relative to governments expecting adverse consequences).

quality measures (FY2002 through FY2004) so that differences between the two groups cannot be attributed to the construction of the independent variables.

Table 3, Panel A displays descriptive statistics for the pooled sample of 1,200 observations (50 governments for 12 quarters prior to and 12 quarters following the GASB 34 implementation period) used to estimate specifications [2a] and [2b]. Panel B indicates that both groups increase new debt per capita from the pre- to the post-GASB 34 periods. Consistent with evidence in Figure 3, the increase is more substantial for governments that anticipate favorable GASB 34 consequences. Mean unexpected new debt per capita increases \$95.67 (from \$74.99 to \$170.66) for governments expecting favorable consequences, while the mean increase for governments expecting adverse consequences is only \$44.88 (from \$61.07 to \$105.94). Panel C correlations reveal that *NEWDEBTPC* and *GSP* are positively associated with *EXPECT_FAV* and *RANK*, and negatively associated with *EXPECT_ADV*.

Panel D shows estimates for specifications [2a] and [2b]. Focusing on Column [1], the parameter estimates β_1 and β_2 indicate post-GASB 34 differences in the dependent variable for governments expecting favorable and governments expecting adverse consequences, relative to the governments in neither classification. Recall that we are specifically interested in comparisons between governments that anticipate favorable versus adverse consequences of GASB 34, which are considered by addressing the null hypothesis $\beta_1 = \beta_2$. Consistent with expectations, we reject $\beta_1 = \beta_2$ in favor of greater post-GASB 34 new debt issues by governments expecting favorable consequences ($\beta_1 > \beta_2$). Using the *NEWDEBTPC* \$100.93 sample mean new debt per capita as a benchmark, both the statistically significant estimate $\beta_1 = \$35.46$ on the (*POST*EXPECT_FAV*) interaction and the statistically significant difference between β_1 and β_2 (\$48.48) are significant economically.

Turning to Column 2, the statistically significant positive estimate δ_1 on the interaction (*POST* \times *RANK*) also indicates that new debt per capita following GASB 34 implementation varies directly with the extent to which governments anticipate favorable GASB 34 consequences.

To summarize, both Figure 3 and the Table 3 results validate the *EXPECT_ADV* and *EXPECT_FAV* measures and indicate behavior in anticipation of GASB 34 consequences. These findings also support the proposition that GASB 34 implementation facilitates economically substantive municipal debt market separation, conditioned on governments' anticipation of GASB 34 consequences.

5.5 The use of debt insurance (Hypothesis 3)

To consider insurance effects from GASB 34 implementation, we expand the sample to include new debt issues insured by third parties. For each state-quarter observation, we compute the fraction of new debt issues that are insured, measured as either value-weighted by dollar amount (*INSURED-VW*) or issue-weighted by number of issues (*INSURED-IW*). As all 50 governments issue either insured or uninsured new debt in each of the 32 sample quarters, we estimate specifications [2a] and [2b] for 1,600 observations (50 governments, 16 quarters before and 16 quarters after the excluded GASB 34 transition year FY2002) .

Results for these analyses are displayed in Table 4. Panel A indicates that about one-half of municipal debt issues (55.3% of the number of new debt issues and 49.0% of the value of new debt issues) are insured during the sample period. Panel B shows mean fractions of insured new debt for governments anticipating favorable and governments anticipating adverse consequences, both before and after GASB 34 implementation. Governments expecting adverse GASB 34 consequences experience economically substantial and statistically significant increases in the

fraction of insured new debt – mean 8.6 percent increases for the value-weighted and 13.8 percent increases for the issue-weighted measures. In contrast, pre- versus post-GASB 34 increases in the use of insured debt by governments expecting favorable consequences are less substantial, and only one of the two measures show a statistically significant increase. Panel C correlations provide mixed inferences. In column 1, *EXPECT_FAV* and *RANK* are negatively associated with *INSURED-VW* (and the correlation of *EXPECT_ADV* and *INSURED-VW* is insignificant). However, in column 2, we find that issue-weighted insured debt is negative (positively) associated with *EXPECT_ADV* (*EXPECT_FAV*), while the *RANK* variable continues to show a negative correlation. Our regression analyses allow us to examine these associations more thoroughly.

Panel D shows estimates for specifications [2a] and [2b]. For the *EXPECT_FAV* and *EXPECT_ADV* classifications shown in columns [1] and [2], we reject the null hypothesis $\beta_1 = \beta_2$ in favor of the alternative hypothesis ($\beta_1 < \beta_2$) that governments expecting favorable consequences issue relatively less insured debt following GASB 34. Notice that the effect is concentrated in the governments expecting adverse consequences. That is, the parameter estimate $\beta_2 < 0$ on the interaction *POST*EXPECT_ADV* is statistically significant, but the estimate β_1 for the interaction *POST*EXPECT_FAV* is not. Moreover, the estimates β_2 are economically significant, as both exceed the means displayed in Panel B by more than 10%. Also consistent with expectations, associations using the rank measure (*POST*RANK*) in columns [3] and [4] indicate that changes in the use of debt insurance following GASB 34 implementation vary inversely with the extent that governments anticipate favorable consequences from GASB 34.

Such results indicate governments that anticipate adverse GASB 34 consequences are more likely to use debt insurance following GASB 34 than governments expecting favorable consequences. We interpret these insurance effects as further support for the proposition that GASB 34 promotes separation in the municipal debt market.

5.6 The use of debt versus alternative financing (Hypothesis 4)

Hypothesis 4 is predicated on the notion that governments that experience reductions in debt service costs are more likely to substitute debt for alternative financing sources following the GASB 34 implementation period. We construct three measures of alternative financing: (1) tax revenue, computed as income, property, and sales taxes (designated *TAXRev*); (2) own source revenue, computed as taxes and most user fees (designated *OSRev*); and (3) total revenue, computed as all revenue including intergovernmental transfers (*TOTRev*). These measures are deflated by aggregate new debt issues (*NewDebt*) to obtain measures that indicate the relative use of debt versus revenue sources to finance government activities. We estimate specifications [2a] and [2b] for these three dependent variables.

To address Hypothesis 4, we use Census of Governments (COG GFS) data compiled for nearly 90,000 state and local governments and special districts. The Census surveys the entire population of governments in years ending in 2 and 7, and a sample of governments for intervening years.¹⁶ As the COG GFS reports annual (rather than quarterly) financial data, we aggregate by fiscal year to obtain 400 government-year observations (50 governments x 8 years), 200 observations each before and after GASB 34 implementation. We estimate specification [2a] and [2b], modified to include *YEAR* (rather than *QTRYR*) fixed effects, and the covariate *GSPAnnual* (the annual change in gross state product).

¹⁶ The sample is selected using the probability proportional to size method during intervening years, which ensures selection of all state governments and city governments with population greater than 75,000 (COG 2008).

Results are displayed as Table 5. Descriptive statistics in Panel A show mean and median ratios greater than one for all three variables, which indicates that the use of alternative (non-debt) financing substantially exceeds the use of debt financing. To illustrate, mean *TAXRev* indicates that each dollar of new debt corresponds to \$11.78 of annual tax revenue. Panel B indicates statistically significant declines the ratios (i.e., increases in the relative use of debt financing) following GASB 34 for governments expecting favorable consequences, but not for governments expecting adverse consequences. Panel C correlations suggest a strong positive association between the alternative financing measures (as expected, given that tax revenues are part of own source revenues, and own source revenues are part of total revenues). The correlations suggest that governments that anticipate favorable (adverse) consequences use less (more) revenues relative to debt.

Regression results are in Panel D. Columns [1] through [3] indicate statistically significant differences in use of debt relative to alternative financing sources following GASB 34 implementation between governments expecting favorable versus adverse consequences. In particular, we reject the null hypothesis $\beta_1 = \beta_2$ in favor of the alternative hypothesis ($\beta_1 < \beta_2$) that ratios decline more substantially (the use of debt increase) for governments expecting favorable consequences than for governments expecting adverse consequences. These effects are economically substantial relative to the descriptive statistics in Panel A. In particular, magnitudes of differences between the parameter estimates β_1 and β_2 on the interactions (*POST*EXPECT_FAV*) and (*POST*EXPECT_ADV*) are 43% to 50% of mean ratios displayed in Panel A.

Results using the *RANK* measure in Columns [4] through [6] confirm that ratios decline (i.e., the relative use of debt financing increases) more substantially as the extent that

government expectations of favorable consequences during the GASB 34 implementation period increase. Specifically, the parameter estimate δ_1 on the interaction ($POST \times RANK$) is negative and is statistically significant in two of the three dependent variables (one tailed $\alpha < 0.05$). We interpret Table 5 results as evidence consistent with separation in the municipal debt market following GASB 34 implementation.

5.7 Considering differing time trends amongst governments expecting favorable versus adverse consequences

One potential concern is that our regression results are simply reflecting differential time trends experienced by governments expecting favorable versus adverse consequences during the test period (and not a GASB 34 effect). To address this concern, we estimate regression specifications controlling for the time trend variable $TIMETREND = (1, \dots, 36)$:

$$[3] \quad Y_{i,t} = \lambda_0 + \lambda_1 (POST_t \times EXPECT_FAV_i) + \lambda_2 (POST_t \times EXPECT_ADV_i) \\ + \lambda_3 (TIMETREND_t \times EXPECT_FAV_i) + \lambda_4 (TIMETREND_t \times EXPECT_ADV_i) \\ + \mathbf{Controls} + \varepsilon_{i,t}$$

In specification [3], Y_{it} = a dependent variable considered in Tables 3, 4, or 5, and $TIMETREND = (17, \dots, 21)$ during the transition period is omitted. Like in our primary specifications, we control for gross state product (GSP or GSP_{Annual}), state fixed effects, and time (quarter-year or year) fixed effects. The state fixed effects subsume $EXPECT_FAV$ and $EXPECT_ADV$ main effects and the time fixed effects subsume the $POST$ and $TIMETREND$ main effects. The parameter estimates λ_3 and λ_4 indicate the mean trend for governments expecting favorable consequences and governments expected adverse consequences, respectively. We address the robustness of our main results by testing $\lambda_1 = \lambda_2$.

Table 6 displays parameter estimates for specifications of specification [3]. We reject $\lambda_1 = \lambda_2$ in all columns except for column 2, where $INSURED-VW$ is the dependent variable. All

remaining results are consistent with the regression results presented in Tables 3 through 5. Further, we fail to find a significant difference between trends for the two types of governments in any of the columns, as indicated by tests of $\lambda_3 = \lambda_4$ shown at the bottom of each column. Overall, the results from this robustness test suggests that our primary results do not simply reflect a differential time trend effect between governments that anticipate favorable versus adverse GASB 34 consequences.

6. Conclusion

We posit that greater financial reporting comparability from GASB 34 implementation elevates ability of state and local governments to credibly communicate financial information which, in turn, manifests as *separation* in the municipal debt market. Our empirical results support this position, providing evidence that governments expecting favorable consequences of GASB 34 delayed new debt issuances to after GASB 34. In contrast, governments expecting adverse consequences of GASB 34 accelerate debt issuances to before GASB 34. Governments anticipating favorable consequences issued greater debt per capita, used proportionally less debt insurance, and favored debt financing over other financing alternatives following GASB 34, relative to governments that expected adverse consequences from GASB 34. These results are consistent with a characterization wherein GASB 34 disclosures reveal substantive financial information about municipal debt issuers which could not be credibly communicated previously. Our results inform GASB deliberations currently underway regarding potential modifications to GASB 34 (GASB, 2020) by providing evidence that the standard promotes separation in the municipal debt market.

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Appendix A
State and Largest City Governments, Sorted by FY2002 CAFR Transmittal Date

Panel A: State governments

	<u>State</u>	<u>FY End</u>	<u>Transmittal Date</u>		<u>State</u>	<u>FY End</u>	<u>Transmittal Date</u>
1	Utah	6/30/2002	11/22/2002	26	Mississippi	6/30/2002	1/9/2003
2	Colorado	6/30/2002	11/26/2002	27	Montana	6/30/2002	1/16/2003
3	New Jersey	6/30/2002	11/27/2002	28	Tennessee	6/30/2002	1/17/2003
4	Delaware	6/30/2002	12/5/2002	29	West Virginia	6/30/2002	1/20/2003
5	Maryland	6/30/2002	12/6/2002	30	Louisiana	6/30/2002	1/22/2003
6	Minnesota	6/30/2002	12/6/2002	31	Ohio	6/30/2002	1/27/2003
7	Kansas	6/30/2002	12/9/2002	32	Arizona	6/30/2002	2/11/2003
8	North Carolina	6/30/2002	12/12/2002	33	California	6/30/2002	2/11/2003
9	Virginia	6/30/2002	12/12/2002	34	Arkansas	6/30/2002	2/21/2003
10	Wisconsin	6/30/2002	12/13/2002	35	Florida	6/30/2002	2/26/2003
11	Wyoming	6/30/2002	12/13/2002	36	South Carolina	6/30/2002	2/26/2003
12	Alaska	6/30/2002	12/15/2002	37	Pennsylvania	6/30/2002	2/27/2003
13	Nevada	6/30/2002	12/16/2002	38	Connecticut	6/30/2002	2/28/2003
14	New Hampshire	6/30/2002	12/16/2002	39	Texas	8/31/2002	2/28/2003
15	Washington	6/30/2002	12/17/2002	40	South Dakota	6/30/2002	3/24/2003
16	Idaho	6/30/2002	12/19/2002	41	Hawaii	6/30/2002	3/31/2003
17	North Dakota	6/30/2002	12/19/2002	42	Missouri	6/30/2002	3/31/2003
18	Oregon	6/30/2002	12/19/2002	43	Illinois	6/30/2002	4/15/2003
19	Iowa	6/30/2002	12/20/2002	44	Alabama	9/30/2002	5/30/2003
20	Kentucky	6/30/2002	12/20/2002	45	Maine	6/30/2002	6/1/2003
21	Nebraska	6/30/2002	12/20/2002	46	Georgia	6/30/2002	6/4/2003
22	Michigan	9/30/2002	12/26/2002	47	New York	3/31/2003	7/18/2003
23	Indiana	6/30/2002	12/30/2002	48	Rhode Island	6/30/2002	10/17/2003
24	Massachusetts	6/30/2002	12/30/2002	49	Vermont	6/30/2002	11/21/2003
25	Oklahoma	6/30/2002	12/31/2002	50	New Mexico	6/30/2002	12/19/2003

Appendix A (continued)
State and Largest City Governments, Sorted by FY2002 CAFR Transmittal Date

Panel B: Largest city governments

	<u>City</u>	<u>State</u>	<u>FY End</u>	<u>Transmittal Date</u>
1	Manchester	NH	06/30/02	10/01/02
2	Des Moines	IA	06/30/02	10/30/02
3	New York	NY	06/30/02	10/31/02
4	Oklahoma City	OK	06/30/02	11/14/02
5	Portland	ME	06/30/02	11/21/02
6	Charlotte	NC	06/30/02	12/10/02
7	Louisville	KY	06/30/02	12/11/02
8	Houston	TX	06/30/02	12/17/02
9	Boston	MA	06/30/02	12/27/02
10	Detroit	MI	06/30/02	12/30/02
11	Los Angeles	CA	06/30/02	01/10/03
12	Phoenix	AZ	06/30/02	01/10/03
13	Burlington	VT	06/30/02	01/17/03
14	Baltimore	MD	06/30/02	01/27/03
15	Boise City	ID	09/30/02	03/14/03
16	Jacksonville	FL	09/30/02	03/28/03
17	Honolulu	HI	06/30/02	04/02/03
18	Columbus	OH	12/31/02	04/28/03
19	Omaha	NE	12/31/02	05/02/03
20	Anchorage	AK	12/31/02	05/07/03
21	Wichita	KS	12/31/02	05/19/03
22	Fargo	ND	12/31/02	05/30/03
23	Sioux Falls	SD	12/31/02	06/01/03
24	Denver	CO	12/31/02	06/12/03
25	Minneapolis	MN	12/31/02	06/19/03
26	Indianapolis	IN	12/31/02	07/11/03
27	Seattle	WA	12/31/02	07/22/03
28	Milwaukee	WI	12/31/02	08/22/03
29	Atlanta	GA	12/31/02	12/29/03
30	Kansas City	MO	04/30/03	08/09/04

Notes: This appendix provides a list of state and largest city government, sorted in descending order by FY2002 CAFR transmittal date (i.e., the date the CAFR became publicly available). Information for state (largest city) governments is presented in Panel A (B). These dates were obtained through hand collection efforts, and reveals that 92% of state governments and 83% of the largest city governments preparing GASB 34-compliant CAFRs transmitted these CAFRs prior to June 30, 2003 (the shaded dates).

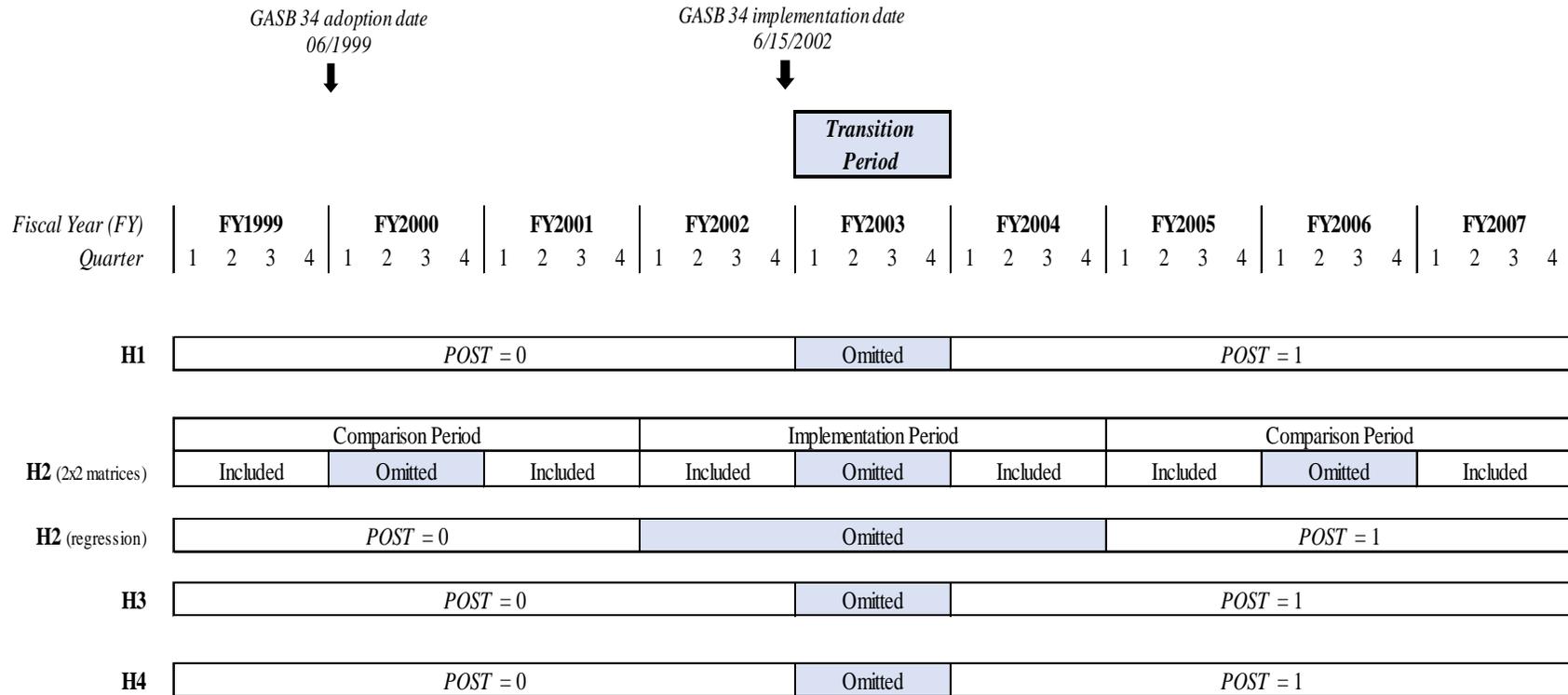
Appendix B Variable Definitions

Variable	Definition
<i>EXPECT_ADV</i>	Indicator that captures governments that expect adverse consequences from GASB 34 and accelerate their new debt issues to pre-GASB 34. Variable is set equal to one for governments with aggregate unexpected new debt per capita above (below) the median in the four quarters immediately preceding (following) GASB 34 transition (FY2003), and zero otherwise. Source: Mergent.
<i>EXPECT_FAV</i>	Indicator that captures governments that expect favorable consequences from GASB 34 and delay their new debt issues to post-GASB 34. Variable is set equal to one for observations with aggregate unexpected new debt per capita below (above) the median in the four quarters immediately preceding (following) GASB 34 transition (FY2002), and zero otherwise. Source: Mergent.
<i>GSP</i>	Quarterly percent change in gross state product for each state from quarter t to $t-1$ (in points). Source: Census.
<i>GPSAnnual</i>	Annual percent change in gross state product for each state from year t to $t-1$ (in points). Source: Census.
<i>INSURED-IW</i>	Number of debt issues by observation i in quarter t that are insured, divided by the total number of debt issues by observation i in quarter t . Source: Mergent.
<i>INSURED-VW</i>	Dollar amount of debt issues by observation i in quarter t that are insured, divided by the total dollar amount of debt issues by observation i in quarter t . Source: Mergent.
<i>NEWDEBTPC</i>	Total dollar amount of uninsured debt issues by observation i in quarter t , scaled by state population. Source: Mergent.
<i>OSRev</i>	Total own source (i.e., non-intergovernmental) revenue for each state in year t , scaled by aggregate <i>NEWDEBTPC</i> for the corresponding fiscal year. Source: Census.
<i>POST</i>	Indicator for post-GASB 34 transition period. Variable is set equal to one in FY2004-2007, and zero otherwise.
<i>QTRYR</i>	Fixed effect for quarter-year t .
<i>RANK</i>	Observation i 's rank in the amount computed as unexpected new debt per capita in FY2004 minus unexpected new debt per capita in FY2002. Values range from 1 through 50; higher values reflect a government's increasing expectation of favorable consequences from GASB 34 disclosures.
<i>STATE</i>	Fixed effect for state and local governments within state i .
<i>TAXRev</i>	Total tax revenue for each state in year t , scaled by aggregate <i>NEWDEBTPC</i> for the corresponding fiscal year. Source: Census.

<i>TOTRev</i>	Total revenue from all sources for each state in year t , scaled by aggregate <i>NEWDEBTPC</i> for the corresponding fiscal year. Source: Census.
<i>TIMETREND</i>	Number increasing (1, 2, 3...) each quarter (or year, in the case of Hypothesis 4 tests) throughout the duration of the sample period.

Notes: This appendix includes variable definitions. Variables are presented in alphabetical order.

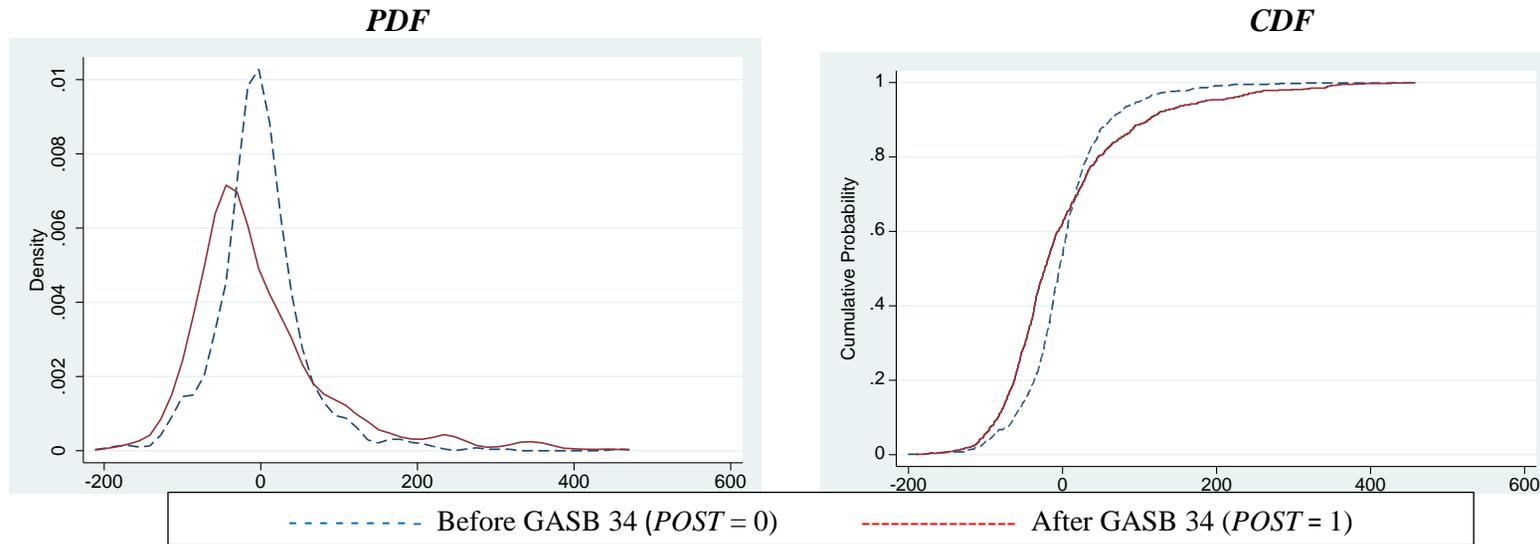
**Figure 1
Timeline**



Notes: This figure shows how FY1999 through FY2007 sample observations are partitioned for testing each hypothesis. Fiscal years are delineated as July 1 through June 30. GASB 34 implementation date refers to when all state and the largest local governments (i.e., those with FY1999 revenues greater than \$100 million) are required to implement GASB 34. Variables are described in Appendix B.

Figure 2
Distributional Properties of Unexpected New Debt Per Capita

Panel A: PDF and CDF of the specification [1] residuals

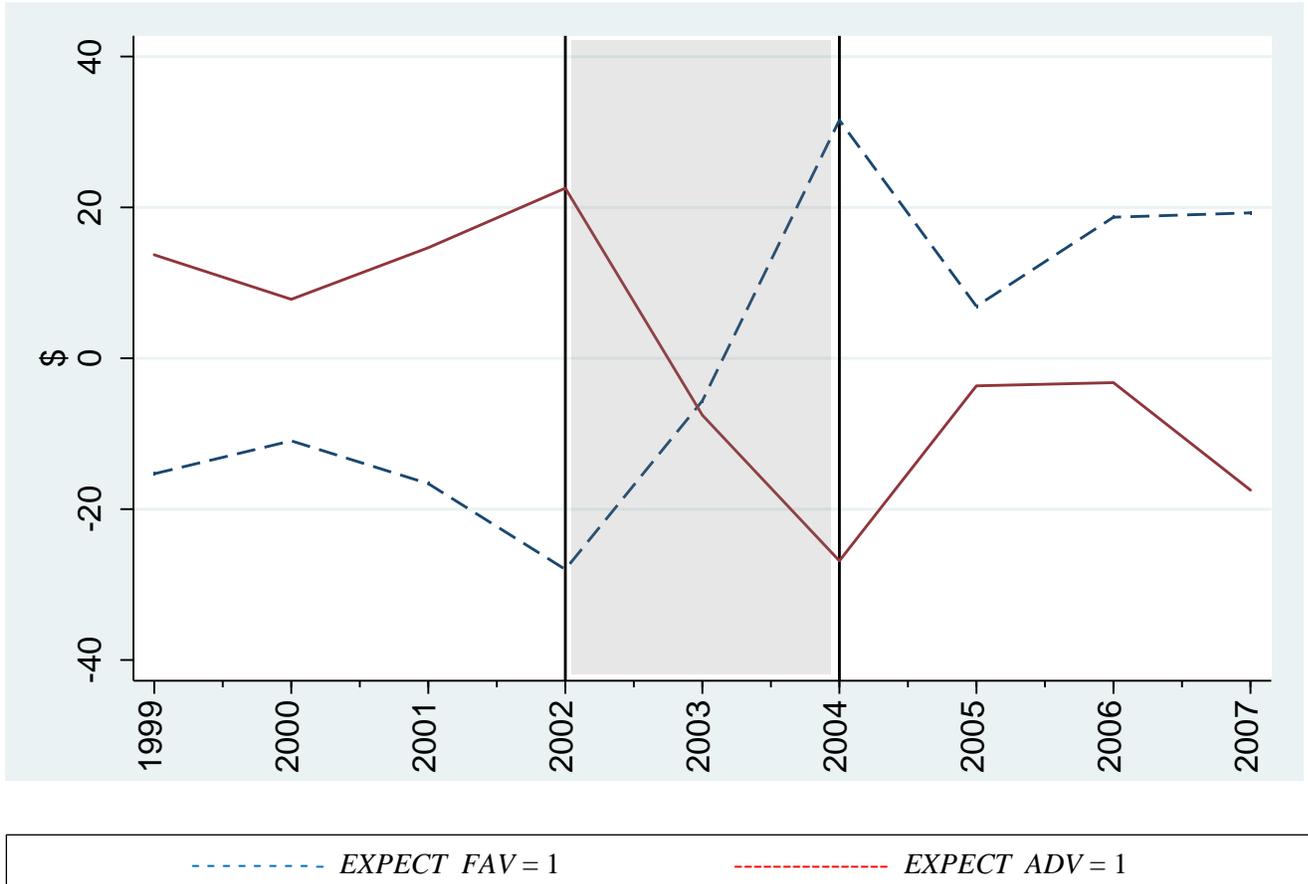


Panel B: Descriptive statistics for the Specification [1] residuals, by POST subgroup

Moment	Before GASB 34 (<i>POST</i> = 0)	After GASB 34 (<i>POST</i> = 1)	B-F Statistic
2 nd moment: Variance	3,516.821	8,295.210	55.272***
3 rd moment: Skewness	1.344	1.731	
4 th moment: Kurtosis	10.309	7.100	

Notes: Panel A presents PDF and CDF distributions of residuals from specification [1], estimated using a pooled sample. Specification [1] regresses *NEWDEBTPC* on *GSP*, state fixed effects, and quarter-year fixed effects. Note that due to the properties of OLS, including quarter-year fixed effects in specification [1] results in mean residual value for each quarter-year (and thus for the *POST* = 0 and the *POST* = 1 time periods) equal to zero. Residuals are plotted separately for *POST* = 0 and *POST* = 1. Panel B presents higher order moment descriptive statistics of the specification [1] residuals, separately for *POST* = 0 and *POST* = 1. The B-F statistic tests the null hypothesis that the distribution variance is equal in the *POST* = 0 and *POST* = 1 subsamples. Variables are defined in Appendix B.

Figure 3
Annual Unexpected New Debt Per Capita, for *EXPECT_FAV=1* and *EXPECT_ADV=1*



Notes: This figure graphs the annual unexpected new debt per capita (i.e., the residuals from specification [1] which regresses *NEWDEBTPC* on *GSP*, state fixed effects, and quarter-year fixed effects) for the 15 governments expecting favorable consequences (*EXPECT_FAV=1*; represented by the blue dotted line) and the 15 governments expecting adverse consequences (*EXPECT_ADV=1*; represented by the red solid line) from FY1999 through FY2007. The gray shading identifies the three-year period used to set the *EXPECT_FAV* and *EXPECT_ADV* variables. Variables are described in Appendix B.

Table 1
Sample Selection

	<u>Issues</u>	<u>Issuers</u>
Non-refunding 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	<u>(138,944)</u>	<u>(3,699)</u>
Sample for tests of Hypothesis 3 (insurance effects)	1,230,550	34,562
Less: Insured issues	<u>(722,639)</u>	<u>(11,370)</u>
Sample of uninsured debt issues for tests of all other hypotheses	507,911	23,192
		<u>Observations</u>
Aggregation: 50 state boundaries x 9 years x 4 quarters/year		1,800
Less: GASB 34 implementation year FY2003 (50 state boundaries x 4 quarters/year)		<u>(200)</u>
		1,600

Notes: This table describes the sample selection procedure using the Mergent data file of municipal debt issues. Variables are defined in Appendix B.

Table 2
Anticipatory Effects: Changes in Unexpected New Debt Per Capita

Panel A: Descriptive statistics (N = 1,800 used to estimate specification [1])

Variable	Mean	SD	P25	P50	P75
<i>NEWDEBTPC</i>	102.994	98.591	38.142	73.425	132.404
<i>GSP</i>	1.337	0.559	0.973	1.266	1.623

Panel B: Distributions of unexpected new debt issues during the GASB 34 implementation period

Implementation Period Centered on GASB 34 Transition Year (FY2002 vs. FY2004)		
Residual _{FY2002}	Residual _{FY2004}	
	Below	Above
Below Median	10	15
Above Median	15	10

Panel C: Distributions of unexpected new debt issues in adjacent three-year comparison periods

Pre-GASB 34 Period (FY1999 vs. FY2001) [1]			Post-GASB 34 Period (FY2005 vs. FY2007) [2]		
Residual _{FY1999}	Residual _{FY2001}		Residual _{FY2005}	Residual _{FY2007}	
	Below	Above		Below	Above
Below Median	16	9	Below Median	13	12
Above Median	9	16	Above Median	12	13

Comparisons:

Test period [Panel B] vs. pre-GASB 34 period [Panel C, Column 1]:	<u>X² Statistic</u> 12.500***
Test period [Panel B] vs. post-GASB 34 period [Panel C, Column 2]:	2.885*

Notes: This table considers anticipatory actions by US state and local governments regarding unexpected new debt issues. Panel B is constructed using residuals from estimating specification [1], which regresses *NEWDEBTPC* on *GSP*, state fixed effects, and quarter-year fixed effects. We cumulate four quarter-year residuals to obtain an observation for all governments within a state's boundaries for each fiscal year. These 50 observations are sorted on this cumulative residual value into two groups: above or below the within-fiscal year median value. Chi-squared statistics compare the test period distribution (Panel B) with distributions from adjacent counterfactual three-year periods (Panel C). ***, **, * indicate parameters statistically significant at the 1, 5, and 10 percent levels (two-tailed tests). Variables are described in Appendix B.

Table 3
Validating Anticipation Measures

Panel A: Descriptive statistics (N = 1,200)

Variables	Mean	SD	P25	P50	P75
<i>NEWDEBTPC</i>	100.933	97.809	36.133	72.346	131.458
<i>RANK</i>	0.510	0.289	0.260	0.510	0.760
<i>GSP</i>	1.372	0.564	1.004	1.303	1.722

Panel B: Descriptive statistics for EXPECT_FAV = 1 and EXPECT_ADV = 1

Variable	<i>EXPECT_FAV = 1</i>			<i>EXPECT_ADV = 1</i>		
	<i>POST=0</i>	<i>POST=1</i>	difference	<i>POST=0</i>	<i>POST=1</i>	difference
<i>NEWDEBTPC</i>	74.989	170.656	95.667***	61.066	105.941	44.875***

Panel C: Correlations (N=1,200)

Variables	[1]	[2]	[3]	[4]
[1] <i>NEWDEBTPC</i>				
[2] <i>EXPECT_FAV</i>	0.147***			
[3] <i>EXPECT_ADV</i>	-0.117***	-0.429***		
[4] <i>RANK</i>	0.158***	0.694***	-0.676***	
[5] <i>GSP</i>	0.028	0.084***	-0.099***	0.095***

Table 3 (continued)
Validating Anticipation Measures

Panel D: Regression results (N=1,200)

[2a] $NEWDEBTPC_{i,t} = \beta_0 + \beta_1 (POST_t \times EXPECT_FAV_i) + \beta_2 (POST_t \times EXPECT_ADV_i) + \text{Controls} + \varepsilon_{i,t}$

[2b] $NEWDEBTPC_{i,t} = \delta_0 + \delta_1 (POST_t \times RANK_i) + \text{Controls} + \varepsilon_{i,t}$,

Variables	Specification [2a] NEWDEBTPC [1]	Specification [2b] NEWDEBTPC [2]
<i>POST*EXPECT_FAV</i> (β_1)	35.464* (0.068)	
<i>POST*EXPECT_ADV</i> (β_2)	-13.016 (0.350)	
<i>POST*RANK</i> (δ_1)		64.767*** (0.006)
<i>GSP</i>	-12.301** (0.023)	-12.464** (0.032)
$\beta_1 = \beta_2$	48.480	
F-statistic	10.281***	
(p-value)	(0.004)	
State FE	Y	Y
Quarter-Year FE	Y	Y
R ²	0.455	0.454

Notes: Analyses of new debt issues per capita (*NEWDEBTPC*). FY2002 through FY2004 observations are omitted as these years set the three financial quality measures (*EXPECT_FAV*, *EXPECT_ADV*, and *RANK*). Standard errors are clustered by state and by quarter-year. Two-tailed p-values shown parenthetically. Designations ***, **, * indicate parameters statistically significant at 1, 5, and 10 percent (two-tailed tests). Variable definitions in Appendix B.

Table 4
Change in the Use of Insured Debt

Panel A: Descriptive statistics (N = 1,600)

Variables	Mean	SD	P25	P50	P75
<i>INSURED-VW</i>	0.490	0.256	0.290	0.495	0.686
<i>INSURED-IW</i>	0.553	0.246	0.381	0.576	0.738
<i>GSP</i>	1.347	0.570	0.976	1.275	1.690

Panel B: Descriptive statistics for EXPECT_FAV = 1 and EXPECT_ADV = 1

Variables	<i>EXPECT_FAV = 1</i>			<i>EXPECT_ADV = 1</i>		
	<i>POST=0</i>	<i>POST=1</i>	difference	<i>POST=0</i>	<i>POST=1</i>	difference
<i>INSURED-VW</i>	0.472	0.457	-0.015	0.461	0.548	0.086***
<i>INSURED-IW</i>	0.547	0.617	0.070***	0.454	0.592	0.138***

Panel C: Correlations (N = 1,600)

Variables	[1]	[2]	[3]	[4]	[5]
[1] <i>INSURED-VW</i>					
[2] <i>INSURED-IW</i>	0.622***				
[3] <i>EXPECT_FAV</i>	-0.064***	0.077***			
[4] <i>EXPECT_ADV</i>	0.037	-0.080***	-0.429***		
[5] <i>RANK</i>	-0.173***	-0.064**	0.694***	-0.676***	
[6] <i>GSP</i>	0.006	0.052**	0.059**	-0.084***	0.061**

Table 4 (continued)
Change in the Use of Insured Debt

Panel D: Regression results (N=1,600)

[2a] $INSURED_{i,t} = \beta_0 + \beta_1 (POST_t \times EXPECT_FAV_i) + \beta_2 (POST_t \times EXPECT_ADV_i) + \text{Controls} + \varepsilon_{i,t}$

[2b] $INSURED_{i,t} = \delta_0 + \delta_1 (POST_t \times RANK_i) + \text{Controls} + \varepsilon_{i,t}$,

Variables	Specification [2a]		Specification [2b]	
	INSURED-VW [1]	INSURED-IW [2]	INSURED-VW [3]	INSURED-IW [4]
<i>POST*EXPECT_FAV</i> (β_1)	-0.044 (0.142)	0.008 (0.781)		
<i>POST*EXPECT_ADV</i> (β_2)	0.057* (0.063)	0.076** (0.014)		
<i>POST*RANK</i> (δ_1)			-0.106* (0.098)	-0.082* (0.080)
<i>GSP</i>	0.009 (0.605)	-0.000 (0.960)	0.009 (0.622)	-0.000 (0.972)
$\beta_1 = \beta_2$	0.101	0.068		
F-statistic	6.425**	6.010**		
(p-value)	(0.017)	(0.020)		
State FE	Y	Y	Y	Y
Quarter-Year FE	Y	Y	Y	Y
R ²	0.324	0.582	0.321	0.578

Notes: Analyses of percentage of new debt issues that are insured (*INSURED-VW* and *INSURED-IW*). In Panel C, the *POST* main effect is subsumed by quarter-year fixed effects, and the *EXPECT_FAV*, *EXPECT_ADV*, and *RANK* main effects are subsumed by state fixed effects. Standard errors are clustered by state and by quarter-year. Two-tailed p-values are shown parenthetically. ***, **, * indicate statistical significance at 1, 5, and 10 percent levels (two-tailed), respectively. Variables are in Appendix B.

Table 5
Relative Use of Debt versus Alternative Financing Sources

Panel A: Descriptive statistics (N = 400)

Variables	Mean	SD	P25	P50	P75
<i>TAXRev</i>	11.780	8.745	6.261	8.869	13.405
<i>OSRev</i>	22.382	17.382	11.739	17.050	25.669
<i>TOTRev</i>	32.084	24.727	16.950	24.464	37.070
<i>GSPAnnual</i>	5.476	2.232	4.064	5.277	6.764

Panel B: Descriptive statistics for EXPECT_FAV = 1 and EXPECT_ADV = 1

Variables	<i>EXPECT_FAV = 1</i>			<i>EXPECT_ADV = 1</i>		
	<i>POST=0</i>	<i>POST=1</i>	difference	<i>POST=0</i>	<i>POST=1</i>	difference
<i>TAXRev</i>	12.651	6.757	-5.894***	13.835	13.217	-0.618
<i>OSRev</i>	23.323	13.294	-10.028***	25.368	26.444	1.076
<i>TOTRev</i>	33.393	19.014	-14.379***	37.195	38.933	1.737

Panel C: Correlations (N=400)

Variables	[1]	[2]	[3]	[4]	[5]	[6]	[6]
[1] <i>TAXRev</i>							
[2] <i>OSRev</i>	0.992***						
[3] <i>TOTRev</i>	0.956***	0.955***					
[4] <i>EXPECT_FAV</i>	-0.156***	-0.154***	-0.156***				
[5] <i>EXPECT_ADV</i>	0.159***	0.133***	0.131***	-0.429***			
[6] <i>RANK</i>	-0.159***	-0.161***	-0.126**	0.694***	-0.676***		
[7] <i>GSPAnnual</i>	0.045	0.051	0.033	0.007	-0.048	0.008	

Table 5 (continued)
Relative Use of Debt versus Alternative Financing Sources

Panel D: Regression results (N=400)

[2a] $RATIO_{i,t} = \beta_0 + \beta_1 (POST_t \times EXPECT_FAV_i) + \beta_2 (POST_t \times EXPECT_ADV_i) + \text{Controls} + \varepsilon_{i,t}$

[2b] $RATIO_{i,t} = \delta_0 + \delta_1 (POST_t \times RANK_i) + \text{Controls} + \varepsilon_{i,t}$

Variables	Specification [2a]			Specification [2b]		
	TAXRev [1]	OSRev [2]	TOTRev [3]	TAXRev [4]	OSRev [5]	TOTRev [6]
<i>POST*EXPECT_FAV</i> (β_1)	-2.439 (0.144)	-5.453 (0.131)	-8.028 (0.103)			
<i>POST*EXPECT_ADV</i> (β_2)	2.728 (0.120)	5.625 (0.123)	7.988 (0.128)			
<i>POST*RANK</i> (δ_1)				-6.957 (0.117)	-15.839* (0.100)	-22.996* (0.098)
<i>GSPAnnual</i>	0.252 (0.241)	0.061 (0.829)	0.229 (0.511)	0.245 (0.287)	0.043 (0.891)	0.204 (0.618)
$\beta_1 = \beta_2$	5.167	11.078	16.016			
F-statistic	5.71**	7.849**	7.344**			
(p-value)	(0.048)	(0.026)	(0.048)			
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
R ²	0.640	0.607	0.630	0.640	0.609	0.632

Notes: Analyses of three ratios reflecting the use of debt relative to other financing sources (*TaxRev*, *OSRev*, and *TOTRev*) computed and analyzed as annual (not quarterly) observations. Standard errors are clustered by state and by year. Two-tailed p-values are shown parenthetically. ***, **, * indicate parameters statistically significant at 1, 5, and 10 percent respectively (two-tailed tests). Variables are in Appendix B.

Table 6
Controlling for Differences in Time Trends between
Governments that Expect Favorable versus Adverse Consequences of GASB 34

$$Y_{i,t} = \lambda_0 + \lambda_1 (POST_t \times EXPECT_FAV_i) + \lambda_2 (POST_t \times EXPECT_ADV_i) + \lambda_3 (TIMETREND_t \times EXPECT_FAV_i) + \lambda_4 (TIMETREND_t \times EXPECT_ADV_i) + \text{Controls} + \varepsilon_{i,t}$$

Variables	<i>INSURED-VW</i> [1]	<i>INSURED-IW</i> [2]	<i>TAXRev</i> [3]	<i>OSRev</i> [4]	<i>TOTRev</i> [5]
<i>POST*EXPECT_FAV</i> (λ_1)	-0.108*** (0.007)	0.026 (0.587)	-4.718* (0.088)	-16.168* (0.092)	-11.187* (0.084)
<i>POST*EXPECT_ADV</i> (λ_2)	0.053 (0.220)	0.047 (0.209)	3.861 (0.255)	9.631 (0.419)	5.209 (0.524)
<i>TIMETREND*EXPECT_FAV</i> (λ_3)	0.003 (0.117)	-0.001 (0.514)	0.570 (0.187)	2.037 (0.177)	1.435 (0.157)
<i>TIMETREND*EXPECT_ADV</i> (λ_4)	0.000 (0.944)	0.001 (0.409)	-0.227 (0.746)	-0.329 (0.884)	0.083 (0.959)
<i>GSP</i> (λ_5)	0.010 (0.541)	-0.001 (0.891)			
<i>GSPAnnual</i> (λ_5)			0.255 (0.234)	0.243 (0.521)	0.072 (0.813)
$\lambda_1 = \lambda_2$	0.161	0.021	8.641	25.799	16.396
F-statistic	6.446**	0.192	5.596**	6.093**	5.610**
(p-value)	(0.016)	(0.664)	(0.050)	(0.043)	(0.050)
$\lambda_3 = \lambda_4$	-0.003	0.002	-0.797	-2.366	1.352
F-statistic	0.866	1.412	1.367	1.817	1.218
(p-value)	(0.359)	(0.244)	(0.281)	(0.220)	(0.306)
State FE	Y	Y	Y	Y	Y
N	1,600	1,600	400	400	400
R ²	0.324	0.580	0.641	0.632	0.608

Notes: This table presents regression results from estimating specification [3]. Standard errors are clustered by state and by quarter-year (excluding in Columns 4 through 6, when they are clustered by state and by year). Two-tailed p-values are shown parenthetically. Designations ***, **, * indicate parameters statistically significant at the 1, 5, and 10 percent levels (two-tailed tests). Variables are defined in Appendix B.