The Effect of Credit Ratings on Disclosure: Evidence from the Recalibration of Moody's Municipal Ratings

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Abstract

This paper examines how credit rating levels affect municipal debt issuers' disclosure decisions. Using exogenous upgrades in credit rating levels caused by the recalibration of Moody's municipal ratings scale in 2010, we find that upgraded municipalities significantly reduce their disclosure of required continuing financial information, relative to unaffected municipalities. Consistent with a reduction in debtholders' demand for information driving these results, the reduction in disclosure is greater when municipal bonds are held by investors who relied more on disclosure ex ante. However, we also find that the reduction in disclosure does not manifest when issuers are monitored by underwriters with greater issuer-specific expertise and when issuers are subject to direct regulatory enforcement through the receipt of federal funding. Overall, our results suggest that higher credit rating levels lower investor demand for disclosure in the municipal market, and highlight the role of underwriters and direct regulatory enforcement in maintaining disclosure levels when investor demand is low.

(*JEL* G24, G28, H74, M40, M41)

Keywords: Municipal Bonds, Municipal Disclosure, Credit Ratings, Moody's Recalibration, Underwriters, Single Audit Act

1. Introduction

The municipal bond market is critical in funding the nation's infrastructure. As of 2018, over 44,000 state and local governments owed $3.7 trillion in municipal bonds outstanding to fund their daily operations and a wide variety of public projects, such as roads, schools, water systems, and hospitals (SIFMA [2018]). However, unlike the corporate environment, where an abundance of information is available through issuers' disclosures and various information intermediaries, this market is notoriously opaque. Although municipal bond issuers are required to file annual financial information, they often fail to provide investors with even basic financial statements after the initial offering, or provide these "continuing disclosures" with a significant delay (SEC [2016]).[1] Regulators have advocated for disclosure reform since the early 1900s (Zimmerman [1977]), and prior literature suggests that this lack of transparency benefits broker-dealers at the expense of household investors (e.g., Cuny [2018]) and furthers dealers' monopoly power (Green et al. [2006]).

The widespread lack of compliance with the requirement to file annual financial information calls for a better understanding of the determinants of municipal disclosure. In this paper, we examine the role of credit rating agencies in municipalities' continuing disclosure decisions. A distinct feature of the municipal market is the large presence of retail investors, who rely on credit ratings for their investment decisions (SEC [2012]).[2] Given this reliance, credit ratings are likely to affect these investors' demand for municipal disclosures. In particular, theory predicts that, as credit risk decreases, debtholders' payoffs become less sensitive to new information about issuers' economic fundamentals (e.g., Easton et al. [2009], Merton [1974]).

[1] For example, nearly 40% of municipalities failed to file any continuing disclosures in 2009 (Schmitt [2011]).
[2] Retail investors accounted for 67% of municipal bond holdings at the end of 2016 (44% direct holdings and 23% indirect holdings through mutual funds, money market funds and ETFs, according to the U.S. Flow of Funds Accounts quarterly data). The SEC's recent report on the municipal securities market states: "Although issuers disclose financial information in various disclosure documents available to investors, market participants noted that many investors nonetheless rely on municipal credit ratings" (SEC [2012], p. 52).

Consequently, an increase in credit rating levels that lowers debtholders' perception of credit risk should reduce their demand for information. To the extent that municipal issuers exercise discretion over providing continuing disclosure, they should respond by reducing the supply of disclosure.

While the theory is intuitive, municipal disclosure does not necessarily vary with credit rating levels. In this market, retail investors typically have limited information-processing abilities and hold bonds to maturity (SEC [2012]). These investors may thus have little demand for disclosure after the initial bond offering, implying that an increase in credit rating levels could have an insignificant effect on continuing disclosure. Further, municipalities may also respond to potential disclosure demands of their other constituents, such as citizens, which do not necessarily depend on credit rating levels. Finally, a number of gatekeepers jointly enforce municipalities' disclosure requirements and can act as countervailing forces against a reduction in continuing disclosure. Thus, the effect of credit ratings on municipal disclosure is unclear.

To identify the effect of municipal credit ratings on disclosure, we exploit the recalibration of Moody's municipal rating scale. In April 2010, Moody's recalibrated its ratings to the global ratings scale (GRS), which upgraded the rating levels of over 18,000 municipal entities. Importantly, the recalibration only represented a change in *scale* and did not result from changes in issuers' underlying credit risk or other economic fundamentals that could be related to their disclosure incentives.[3] Although issuers' fundamental credit risk remained unchanged, recent evidence shows that investors nevertheless believed that the increased rating levels represented a drop in credit risk (Adelino et al. [2017], Cornaggia et al. [2018], Beatty et al. [2019]). Based on our prediction, investors' perception of a reduction in credit risk reduces their demand for

[3] Moody's [2010] makes this point explicit in discussing the recalibration: "This recalibration does not reflect an improvement in credit quality or a change in our credit opinion for rated municipal debt issuers. Instead, the recalibration will align municipal ratings with their global scale equivalent" (p. 1).

disclosure, and municipalities thus disclose less.

Using this shock to credit rating levels, we examine whether rating upgrades alter municipalities' continuing disclosure of financial information, defined as the likelihood and frequency of disclosing any financial information after the initial offering.[4] We use a difference-in-differences design to compare the continuing disclosures of municipalities that experienced a rating upgrade from Moody's recalibration (our treatment group) to a control group of municipalities rated by S&P that were not recalibrated, as they were already rated on the GRS. Consistent with our prediction, we find that recalibrated municipalities provide significantly less continuing disclosure, relative to the control group. Specifically, the likelihood (frequency) of continuing financial disclosure declines by 5.7% (5.1%).

We next investigate how variation in investors' demand for information drives our results. Specifically, we examine whether our results are stronger for municipalities with investors who trade their bonds in the secondary market. Secondary market traders care about changes in issuers' credit risk after the initial offering and are more likely to demand continuing disclosure (particularly when credit risk is high). In contrast, buy-and-hold investors typically intend to hold their bonds to maturity and likely have little or no demand for continuing disclosure. This implies that higher credit rating levels are more likely to reduce secondary market traders' demand for information about credit risk. We thus expect that, following a ratings upgrade, the decrease in the demand for continuing disclosure is larger for issuers with secondary market traders (who had a greater ex ante demand for information) than for issuers with buy-and-hold investors. Consistent

[4] Debt issuers are expected to file continuing financial disclosures on the Municipal Securities Rulemaking Board's (MSRB) online dissemination platform after a primary offering (i.e., after a primary offering, issuers are expected to file annual financial disclosures to keep investors updated about their credit quality). We measure municipal disclosure broadly, using measures of both the existence and frequency of all continuing municipal financial filings (including audited financial statements, unaudited annual financial and operating data, interim financial information, budgets, and other miscellaneous filings). In supplemental analyses, we also use event-based disclosures and the timeliness and length of financial disclosure filings.

with our prediction, we find that municipalities with bonds traded on the secondary market are significantly more likely to reduce disclosure, following the recalibration, whereas the disclosure of municipalities with bonds held to maturity remains unchanged. This result suggests that the relation between credit ratings and continuing disclosure in the municipal market depends critically on the nature of the issuers' investor base and their demand for ongoing financial information.[5]

Our findings suggest that higher credit rating levels can lead to lower continuing disclosure through a reduction in investors' demand for information. However, as discussed below, the municipal market has a number of other gatekeepers that could impose costs on issuers that fail to comply with continuing disclosure requirements and provide a countervailing force against a decline in disclosure. We next examine the effectiveness of two such gatekeepers: (i) underwriters and (ii) direct regulatory enforcement related to federal funding through the Single Audit Act.

Unlike securities markets where the SEC directly oversees and enforces reporting requirements, the SEC cannot directly penalize municipalities that do not provide continuing disclosure. Rather, the SEC indirectly regulates municipal disclosure through its oversight of underwriters. According to Section 15c2-12 of the Exchange Act, prior to a bond issuance, underwriters must obtain and review issuers' signed commitment to file financial information annually after the initial offering. The underwriter can be held liable for lacking adequate due diligence if issuers do not provide continuing disclosure, which in turn can incentivize underwriters to monitor issuers' compliance with continuing disclosure requirements. However, enforcement actions against underwriters are rare, which can generate variation in the level of monitoring across underwriters. If underwriters are limited in their incentives or resources to

[5] We also find that, within issuers of bonds traded on the secondary market, the decline in continuing disclosure, following the recalibration, is significant for municipalities with both high levels of institutional and retail traders (see section IA.5 of the internet appendix). This result suggests that ratings levels affect the demand for disclosure of both retail and institutional investors.

monitor continuing disclosures, municipal issuers can potentially exert more discretion over their disclosure decisions.

We explore the role of underwriters as gatekeepers of continuing disclosure by testing how our results vary with several underwriter characteristics. First, we examine underwriters' expertise, which presumably enhances their ability to assess issuers' ex-ante commitment to continuing disclosure. We find that the decline in disclosure, following the recalibration, is significantly smaller for issuers with underwriters that specialize in the issuer's given bond sector and, to some extent, for issuers with local underwriters. This suggests that underwriters with greater issuer-specific expertise offset issuers' incentives to decrease continuing disclosure. Second, we examine competition among underwriters. Competition can either exacerbate conflicts of interest arising from the issuer-pay model and weaken disclosure enforcement or motivate underwriters to improve their reputation through improved disclosure enforcement. However, we do not find evidence that our results vary with underwriter competition, perhaps due to the strong segmentation of this market, which constrains entry (e.g., Butler [2008]).

We next examine the effectiveness of *direct* regulatory enforcement on continuing disclosure. Municipal issuers receiving federal grants over $750,000 are subject to the Single Audit Act, which requires grant recipients to provide the federal government with annual audited financial statements. Importantly, compliance with the act is monitored by Congress and federal program officials, and a failure to comply can result in withheld funding and other legal consequences. The marginal cost of filing financial statements that are already prepared per the government's requirement is likely to be low. Consistent with this reasoning, we find that our results are concentrated among issuers not subject to the Single Audit Act, and that grant recipients subject to the act do not significantly change their levels of disclosure, following the recalibration. This suggests that issuers subject to direct enforcement that can impose significant costs on non-

compliance conform to disclosure rules, regardless of changes in demand for information from bondholders.

Finally, we investigate two additional channels that could drive our results and perform a battery of robustness tests. First, we consider whether the recalibration changed the information content of Moody's credit ratings or resulted in a change in investor base. Our results suggest that these channels are not the primary drivers of our main empirical findings. Second, we address the concern that the difference in continuing disclosure between the treatment and control groups would exist absent the recalibration (i.e., a violation of the parallel-trends assumption). We provide evidence that the recalibration has a persistent effect on disclosure and that the treatment and control groups exhibit no differential trends in their cost of capital and credit ratings prior to the recalibration.[6] Finally, we show that our results are robust to using two alternative control groups: a sample of single-rated S&P issuers matched on a set of pre-recalibration covariates and a sample of recalibrated but non-upgraded issuers.

This paper contributes to our understanding of the role of credit rating agencies on municipalities' continuing disclosure decisions. Rating agencies are of particular interest to the municipal bond market, as household investors rely on credit ratings for investment decisions (SEC [2012]). Their role in the municipal information environment, however, is not well understood. Our findings suggest that higher credit rating levels reduce continuing disclosure through a reduction in investors' perception of credit risk. This indicates that a change in credit rating labels has consequences for municipal disclosure decisions and highlights the importance of credit rating agencies' gatekeeping role in the municipal bond market.

[6] Ideally, we would directly verify that there is no differential trend in continuing disclosure leading up to the recalibration. However, the municipal disclosure data become available one year prior to the recalibration, which prevents us from directly observing a pre-trend. Thus, we test whether our treatment and control groups are comparable in their cost of capital and credit ratings, two key observable dimensions of the information environment related to disclosure.

To our knowledge, our paper is also the first to examine the implications of the interplay between credit rating agencies and other gatekeepers, such as underwriters and other regulatory bodies, on municipal disclosure. Cross-sectional differences in enforcement by gatekeepers potentially generate substantial variation in issuers' compliance with providing annual continuing disclosure. Indeed, we find that the decline in municipal disclosure resulting from higher credit rating levels manifests in settings where (i) the underwriter has limited issuer-specific expertise and (ii) issuers are not subject to direct regulatory enforcement by government entities that award federal grants.

Our findings echo recent work in the corporate setting documenting that greater precision and higher levels of credit ratings lead to a decline in firms' voluntary disclosures (Sethuraman [2019], Basu et al. [2018]). While our results are broadly consistent with these studies, a municipality's decision to disclose *any* continuing financial information to an opaque secondary market differs markedly from a firm's decision to add voluntary information (such as management forecasts) to an already rich body of corporate disclosure. Arguably, it is not clear that municipalities would necessarily fail to provide required disclosures following a change in municipal credit rating levels. Our results demonstrate a novel tradeoff between capital market demands for information and the strength of regulatory enforcement mechanisms for municipal continuing disclosure decisions.

This study can inform ongoing regulatory efforts aimed at improving municipal disclosure. Our findings suggest that the opacity of the information environment at least partly reflects investors' weak demand for disclosure, particularly when credit ratings are relatively high. Consequently, regulatory efforts aimed at improving municipal disclosure may not level the playing field for market participants, as continuing disclosure may not be processed by certain investors. In this regard, our study speaks to the role of prior conditions on the effectiveness of

disclosure regulation. For example, prior literature finds that the effectiveness of securities regulation in improving transparency hinges on the strength of pre-existing regulatory environments (Christensen et al. [2016]). Our findings underscore the importance of also considering the heterogeneity in investors' demand for information when evaluating the benefits of improving disclosure compliance.

2. Background and Related Literature

2.1 Municipal Setting

The municipal market is substantially less regulated than many others (e.g., the corporate bond and equity markets) and is marked by a large degree of opacity and illiquidity (e.g., SEC [2016]). State and local governments are guaranteed state sovereignty in the U.S. Constitution. Consequently, municipal borrowers are exempt from the majority of federal regulations in the 1933 and 1934 Securities Acts, and issuers are not required to file quarterly or annual reports with the SEC.

In 1975, Congress established limited regulatory oversight for municipal securities, creating the MSRB and mandating the registration of municipal securities brokers and dealers (SEC [1994]). However, the Tower Amendment of 1975 prohibits the SEC and MSRB from requiring municipalities to furnish any information to the commission or prospective issuers either before or after the sale of securities. Nevertheless, the SEC regulates municipal disclosure directly through anti-fraud provisions and indirectly through the oversight of underwriters, brokers, and dealers via Section 15c2 of the Exchange Act.

In 1989, the SEC adopted Rule 15c2-12 to increase the transparency of the municipal market and the timeliness of financial information provided in primary offerings. Specifically, Rule 15c2-12 requires underwriters to obtain and review an official statement including certain

financial and operating information for primary offerings exceeding $1,000,000. In 1994, the SEC adopted amendments to Rule 15c2-12 to improve the quality of the *ongoing* or "continuing" information disclosed to market participants after the initial offering (SEC [2009]). Under the 1994 Amendments, underwriters and dealers are prohibited from purchasing or selling municipal securities, unless the municipality has signed a contract (i.e., a continuing disclosure agreement) to continue to provide annual financial and operating information after the initial offering and notify investors of the occurrence of specific material events (similar to 8–K filings). Although Rule 15c2-12 requires municipalities to provide annual financial information to investors, it does not set standards for the nature or quality of this information. The type and length of the required financial information (including whether it requires an audit) is decided in the continuing disclosure agreement negotiated with the underwriter, laying the ground for substantial variation across the types of disclosure that are filed by municipalities.[7]

Despite these efforts, municipal disclosure remains sparse, as nearly 40% of municipalities fail to file any continuing disclosures, indicating a widespread lack of compliance with Rule 15c2-12.[8] This lack of transparency is a growing concern among regulators, particularly in light of the large presence of retail investors in this market, due to the tax exemptions afforded by municipal securities. Regulators argue that this opaque information environment increases the difficulty of assessing the risk of municipal entities (including rising underfunded public pensions and healthcare costs) and privileges sophisticated investors and broker-dealers at the expense of retail investors. As a result, the SEC, MSRB, and FINRA have collectively proposed a number of recent

[7] In this regard, the municipal setting resembles some OTC settings, where firms are not subject to SEC reporting requirements. Specifically, these two markets are similar in (i) the level and heterogeneity of the type of disclosures across issuers and (ii) the rule that underwriters and broker-dealers must obtain and review financial information to comply with Rule 15c2-11 (e.g., Bruggemann et al. [2017], Bushee and Leuz [2005]).

[8] See Schmitt [2011] and our descriptive statistics in Table 2.

initiatives aimed at improving the transparency and liquidity of the municipal market.[9]

2.2 Related Literature

As outlined in GASB Concept Statement 1, the primary users of municipal financial disclosure are regulators, debtholders, and citizens. An emerging literature studies the determinants of municipal disclosure to illuminate the reasons behind the high levels of opacity and large variation in disclosure across issuers in this market. Prior literature documents that, conditional on the decision to disclose, higher disclosure quality reduces issuers' cost of debt (Baber and Gore [2008], Baber et al. [2013]) and GAAP regulation at the state level improves disclosure compliance, particularly for smaller issuers.[10] A recent study by Cheng et al. [2019] examines the role of peer effects in the municipal market, and finds that Moody's-rated issuers who were upgraded by fewer notches, relative to peers following the recalibration, provide timelier and more frequent disclosure to compete for capital.

However, many argue that disclosure is less useful in the municipal market, relative to the corporate bond market, as financial statements are less timely, less comparable, and less reliable. Copeland and Ingram [1982] find a positive association between credit ratings and subsequent disclosure but only weak evidence that municipal financial statements predict credit ratings, suggesting that municipal disclosure is not a timely measure of credit risk. Consistent with this finding, Ingram et al. [1983] find that credit rating changes are more informative in the municipal market, relative to the corporate market, because municipal financial statements are produced with a greater lag and information processing costs are significantly higher. In contrast, Marquette and

[9] For example, the SEC has proposed expanding the disclosure requirements under Rule 15c2-12 (SEC [2015]). In 2017, acting Chairman Michael Piwowar discussed repealing the Tower Amendment (SEC [2017]), and in 2018, the SEC required additional disclosure related to the material financial obligations of municipal issuers (SEC [2018]).

[10] Compliance with governmental accounting rules can be costly, particularly for small municipal issuers with limited resources. For example, Patrick [2010] documents that, while some large, urban governments are staffed by highly specialized accounting professionals, many small rural governments are staffed by part-time municipal secretaries with limited bookkeeping and accounting experience.

Wilson [1992] find that the municipal bond market anticipates future credit rating changes using publicly available information, suggesting that municipal disclosure is useful in pricing credit risk.

The literature also examines the role of municipal financial reporting in monitoring the performance of public officials by citizens. Several studies argue that elections provide incentives for politicians to be transparent when their actions increase the well-being of voters and to distort performance if transparency could decrease their chances of retaining power (e.g., Ingram and Copeland [1981], Peltzman [1992], Brender [2003], Kido et al. [2012]). Cuny [2016] evaluates the benefits of municipal disclosure, relative to political incentives, and finds that, while disclosure provides capital market benefits, politicians' re-election concerns inhibit the disclosure of bad news. Finally, Nakhmurina [2018] documents that state fiscal monitoring increases the quality of municipal disclosure.

Despite this breadth of evidence, there is very little knowledge on the role that various gatekeepers, such as credit rating agencies and underwriters, play in municipalities' disclosure decisions. Credit rating agencies are one of the central gatekeepers in debt markets, and a large literature in the corporate setting finds a positive association between disclosure and credit rating levels.[11] Two recent papers find causal evidence of a negative relation between the precision and levels of credit ratings and corporate voluntary disclosure (Sethuraman [2019]; Basu, Naughton, and Wang [2018]), consistent with our study. However, it is not obvious that these predictions would hold in the municipal market, due to differences in investor characteristics and the disclosure enforcement regime. For example, credit rating levels may not affect continuing disclosure when buy-and-hold investors with little demand for this information dominate the market. Moreover, compliance with continuing disclosure may vary with the heterogeneous types

[11] See, for example, Basu and Naughton [2018], Bonsall and Miller [2017], and Bozanic and Kraft [2017]. See also Cuny and Dube [2018] for a recent study of this link in the municipal market setting.

of enforcement that issuers face from other gatekeepers in this market, such as underwriters. Finally, a municipality's decision to disclose *any* continuing financial information to an opaque secondary market differs markedly from a firm's decision to add management forecasts to an already rich body of corporate disclosures. Arguably, it is not clear that a change in credit rating levels is a sufficient force to move disclosure in our setting.

3. Institutional Setting and Hypothesis Development

3.1. The Effect of Credit Rating Upgrades on Municipal Disclosure

To estimate the effect of credit rating levels on municipal bond issuers' continuing disclosure decisions, the primary empirical challenge is that both ratings and disclosure are endogenous to changes in issuers' economic fundamentals. To address this issue, we exploit a change in credit rating levels that is plausibly exogenous to changes in issuers' fundamentals using Moody's 2010 recalibration of their municipal ratings scale. We provide a detailed description of the institutional details surrounding the recalibration event in section IA.1 of the internet appendix.

Prior to the recalibration, Moody's employed a dual-class ratings system. Municipal bonds were rated under the municipal scale, which reflected distance to distress, defined as the likelihood that a municipality would require extraordinary support from a higher level of government to avoid default. In contrast, Moody's global rating scale (GRS) measures expected loss, defined as the probability of default multiplied by loss given default. In the aftermath of the financial crisis, Moody's faced significant legal and regulatory pressure to convert to the GRS. This pressure culminated in the Dodd Frank Bill in June 2010, mandating that all nationally recognized statistical ratings organizations (NRSROs) employ one consistent ratings scale across asset classes.[12] In

[12] In 2008, Congressional and SEC investigations as well as a lawsuit by the State of Connecticut raised concerns that Moody's was underrating its municipal debt, as the debt was actually less risky than implied by the municipal scale.

response to this mounting pressure, Moody's recalibrated its municipal ratings to the GRS over the month between April 16 and May 7, 2010.

To convert municipal ratings to the GRS, the recalibration incorporated loss given default into the existing ratings. Loss given default for municipal bonds is typically much lower, relative to other debt rated on the GRS, such as corporate bonds. Consequently, nearly 18,000 issuer and security combinations, covering about 70,000 ratings and representing over $2.2 trillion of municipal debt, were recalibrated upward, resulting in credit rating upgrades between zero to three notches.[13,14]

Two features of this event are key to our study. First, the recalibration did not reflect changes in debt issuers' underlying credit risk or other economic fundamentals but was merely a change in the ratings scale (Moody's [2010]). Consistent with this feature, Cornaggia et al. [2018] validate that S&P ratings did not move synchronously with Moody's ratings for dual-rated bonds, following the recalibration. This feature is important, because it allows us to hold the costs of disclosure—and thus issuers' other incentives to supply disclosure—constant. For example, a change in fundamentals can shift both investors' demand for information (e.g., a decrease in credit risk lowers debtholders' demand) and independently shift issuers' supply of information up or down for a variety of reasons (e.g., a decrease in credit risk represents good news, which can heighten issuers' incentives to disclose, ceteris paribus). Holding fundamentals constant mitigates the concern that issuers' supply of disclosure changes independently of bondholders' demand for disclosure.

[13] See the transition matrix in Table 1 of Moody's [2010] and in section IA.1 of the internet appendix for our sample. Further, Moody's [2010] explains that ratings on housing, healthcare, and other enterprise sectors may not change as a result of the recalibration. In addition, the recalibration did not affect short-term ratings, ratings below investment grade, or ratings that were already at the maximum level.

[14] A number of other studies also use the recalibration setting. Adelino et al. [2017] show that the recalibration reduced municipalities' financial constraints and increased government employment. Thus, similar to Cornaggia et al. [2018], the mechanism in their study is a decrease in the cost of debt resulting from investor reliance on ratings. Beatty et al. [2019] show that Moody's and Fitch charge higher fees and increase market share as a result of providing higher ratings, and it is unclear how this finding predicts changes in municipal disclosure.

Second, although the rating upgrades were uncorrelated with changes in underlying fundamentals, recent findings document that municipal investors nevertheless *perceive* the upgrades as a reduction in credit risk. For example, Cornaggia et al. [2018] document that credit spreads on upgraded bonds decrease between 19 and 33 basis points, compared to non-upgraded bonds.[15] The authors attribute their findings to investors' reliance on ratings in this setting.

These two features are useful in pinning down how credit ratings affect disclosure through changes in debtholders' demand for information. Specifically, disclosure provides information about credit risk and informs investors about future payoffs. Given debtholders' nonlinear payoff function, their payoffs are less sensitive to information about issuer fundamentals when credit risk is lower (e.g., Merton [1974], Easton et al. [2009], Lok and Richardson [2011]). Consequently, when investors perceive that credit risk has declined by observing and relying on a higher credit rating, they demand less disclosure.[16] We thus predict that, to the extent that municipalities exercise discretion over providing continuing disclosure, issuers experiencing an upgrade in rating levels, following the recalibration, decrease continuing disclosure, relative to unaffected issuers.

However, the unique institutional features of the municipal bond market suggest that this prediction is not obvious. First, municipal bond investors may have a low demand for disclosure to begin with. This market is dominated by household investors with limited information processing abilities who tend to hold bonds to maturity (SEC [2012]), suggesting that, on average, the demand for continuing disclosure after the initial offering could be limited—in particular because municipal disclosure is often unaudited and untimely (e.g., Cuny [2016], Copeland and Ingram [1982]), which potentially limits its usefulness. Second, disclosure still potentially plays

[15] We demonstrate that a similar result also holds in our sample (Table 10, Panel B). The collective evidence on the recalibration suggests that Moody's-rated municipal bonds were mispriced prior to the recalibration and the recalibration (at least partially) corrected the mispricing, resulting in persistently lower bond yields. See section IA.1 of the internet appendix for a detailed narrative.

[16] For example, municipal disclosure is likely less important to Aaa bondholders than to Bbb bondholders.

an important role for the municipalities' other constituents, including citizens and the various oversight bodies that monitor politicians at the federal, state, and local levels. Municipalities may not be willing to reduce disclosure entirely to the extent that they respond to the demands of these other constituents. Finally, as discussed in section 3.2.2, a number of gatekeepers jointly enforce municipal disclosures, such as underwriters and state and federal regulators. These third parties can impose costs on non-compliant issuers and act as countervailing forces against a reduction in continuing disclosure following credit rating upgrades.

3.2 Cross-Sectional Analyses

In this section, we discuss several cross-sectional analyses to (i) provide additional evidence consistent with debtholder demand for information being the mechanism behind our results, and (ii) explore how our results vary with issuers' enforcement environments.

3.2.1 The role of debtholders' demand for information

Our first test investigates how investors' demand for information drives our results. Specifically, we examine variation in debtholders' demand for information, based on their trading behavior. We expect investors who trade bonds on the secondary market to demand *less* disclosure, following the recalibration, as they perceive that credit risk has declined and that their payoffs have become less sensitive to fundamental information. In contrast, buy-and-hold investors typically purchase bonds on the primary market with the intent of holding them to maturity. These investors may have little to no demand for continuing disclosure to begin with, suggesting that credit rating upgrades are unlikely to significantly decrease their demand for additional disclosure. We thus predict that following a ratings upgrade, the decrease in the demand for continuing disclosure is larger for issuers with secondary market traders than for issuers with predominantly

buy-and-hold investors.[17]

3.2.2 The role of other gatekeepers

Although the SEC cannot directly enforce financial disclosure from municipal entities, a number of other gatekeepers can impose costs on issuers who fail to provide investors with continuing disclosures. If these gatekeepers are effective in enforcing continuing disclosure, they can provide a countervailing force against issuers' incentives to decrease disclosure in response to a change in debtholders' demand for information. In this section, we examine the effectiveness of two specific gatekeepers.

3.2.2.1 The role of underwriters

We begin by exploring the role of underwriters. In this market, underwriters are instrumental in enforcing disclosure, because the SEC *indirectly* monitors continuing disclosure through its oversight of municipal underwriters. Specifically, under Rule 15c2-12, municipal underwriters must reasonably determine, through an independent assessment, that an issuer will fulfill its written commitment to provide continuing disclosure to the MSRB annually after the initial offering (SEC [2010]).[18] If issuers do not comply, then underwriters can be held liable for failing to perform adequate due diligence.[19] As a result, underwriters are likely to monitor continuing disclosure compliance or select disclosure-compliant issuers to avoid penalties from

[17] The extent to which we observe a larger reduction in disclosure from issuers of bonds traded on the secondary market also depends on whether trading itself provides information to the market. If the information from informed investors is quickly impounded into bond prices, investors may rely less on financial disclosure. Finding that issuers with more actively traded bonds reduce disclosure less would support this prediction. However, the municipal bond market is, in large part, highly illiquid, and prior literature suggests that significant transaction and information acquisition costs prevent prices from fully reflecting all available information (Cuny [2018]).

[18] As stated by the SEC, "Rule 15c2–12 is intended to enhance disclosure, and thereby reduce fraud, in the municipal securities market by establishing standards for obtaining, reviewing, and disseminating information about municipal securities by their underwriters" (SEC [2010]).

[19] For example, in the SEC's most recent initiative to increase municipal disclosure compliance (the MCDC initiative of 2014), the agency charged and penalized 72 underwriters, representing 96% of the securities traded in the municipal market, for selling bonds with offering documents containing materially false or misleading statements or omissions about issuers' compliance with continuing disclosure laws under Rule 15c2-12 (SEC [2016]). This suggests that many underwriters were not effective as monitors of continuing disclosure during our sample period.

the SEC. However, underwriters cannot directly enforce continuing disclosure after the initial offering. All they can do is refuse to underwrite future offerings if an issuer fails to comply with continuing disclosure. As enforcement actions against underwriters remain rare, their effectiveness in enforcing continuing disclosure potentially varies with their incentives and resources to evaluate and monitor issuers.[20] We explore how our results vary with the heterogeneity of several underwriter characteristics in Section 5.4.2.1.

3.2.2.2 The role of direct regulatory enforcement

We also investigate the role of *direct* regulatory enforcement on issuers' continuing disclosure. Nakhmurina [2018] demonstrates that fiscal monitoring at the state level improves the timeliness of municipal disclosures and generally improves reporting quality, which suggests that direct regulatory enforcement can improve issuers' compliance with disclosure requirements. We examine municipal entities that receive federal funding in excess of $750,000, which are subject to the Single Audit Act that requires issuers to provide annual audited financial statements to the federal government. Municipal compliance with the Single Audit Act is overseen by Congress, and a failure to comply can result in the withdrawal of funding and other legal consequences. Given the ability of federal agencies to directly enforce disclosure compliance, we expect that issuers subject to the Single Audit Act provide annual financial statements to the federal government. If these issuers prepare financial statements and receive an audit per the government's requirement, the marginal cost of disclosing these statements through EMMA is low. Consequently, we predict that issuers subject to the Single Audit Act are less likely to decrease disclosure following the recalibration.

[20] This differs from OTC equity settings, where disclosure requirements are enforced by the exchange and/or regulator (Rule 15c2-11).

4. Sample Selection and Variable Measurement

4.1 Sample

To construct our sample (see Table 1), we begin by obtaining municipal bond issuance data from Thomson Reuter's SDC Platinum. To ensure comparable treatment and control groups, we only include municipalities that issued debt rated by one of the top rating agencies (S&P, Moody's, and Fitch) in the four years prior to Moody's recalibration (following Adelino et al. [2017]). Our sample period begins July 1, 2009 (when continuing disclosure filings became available on MSRB), and ends June 30, 2014 (sample of 90,915 unique issuer–year observations).[21] Fitch also implemented a rating recalibration in the spring of 2010. However, because we lack data on this recalibration, we exclude municipalities that issued Fitch-rated bonds to avoid including issuers that underwent a different recalibration (51,675 observations).

To identify issuers affected by Moody's recalibration, we obtain data on recalibrated municipalities from Moody's recalibration files and data on bond issues from Mergent that had a "change in scale," indicating a recalibration. We exclude municipalities with bonds rated by Moody's that were not recalibrated and for which we do not have recalibration data from Mergent (10,030 observations).[22] Next, we exclude municipalities that issue only insured bonds (8,070), because the credit risk of insured bonds is tied to the insurance company rather than the issuer (Cuny [2016]). These bonds are thus likely to be unaffected by the recalibration. Finally, we obtain data on secondary market trading and municipal disclosures through the MSRB's Electronic Municipal Market Access system (EMMA).

Our final sample consists of 21,085 issuer-year observations (4,217 unique municipalities).

[21] Prior to 2008, municipalities could file financial information in a variety of online repositories. After 2008, all financial information must be submitted electronically to the MSRB's EMMA database.

[22] Consistent with prior literature, this excludes non-upgraded Moody's bond issuers from special districts already properly calibrated to the global rating scale from our sample (e.g., housing and healthcare sectors) (e.g., Adelino et al. [2017]).

Because we are interested in financial disclosure decisions at the municipal level, we conduct our analyses at the bond *issuer* level, as opposed to the issuance level (i.e., a municipality with multiple bond offerings during the year prepares one set of annual financial statements).[23] The treatment group is comprised of 9,725 issuer-year observations (1,945 issuers) that experienced a rating upgrade from Moody's recalibration and a control group of 11,360 issuer-year observations (2,272 issuers) that were not subject to Moody's recalibration. To ensure that we capture all municipalities that were affected by the recalibration, we include all issuers upgraded either at the issuer or the issuance level in the treatment group. Note that because we exclude Fitch-rated and non-upgraded Moody's-rated issuers from our sample, the control group consists of issuers that are exclusively rated by S&P.[24]

4.2 Measurement of Municipal Disclosure

Because the data on municipal continuing disclosure filings begins on July 1, 2009, we define July 1–June 30 as one reporting period to allow four full quarters in each period. Therefore our first reporting period runs from July 1, 2009–June 30, 2010, which we use as our benchmark to measure municipal issuers' continuing disclosure prior to Moody's recalibration. Although this period includes the four-week recalibration at the end (April 16, 2010–May 7, 2010), financial filings occur on average 223 days after the end of their fiscal year in our sample. (For example, an annual report issued in July 2010 typically covers the fiscal year ending December 2009.[25]) As Moody's announced its recalibration in March 2010, it is unlikely that any effects on disclosure

[23] Note that our results are robust to conducting our tests at the issuance level.

[24] These sample selection criteria result in a subsample of the SDC universe of issuers that are on average 8% more likely to provide disclosure (but disclose slightly less frequently), issue 4% more callable bonds, issue 11% less GO bonds, and issue less debt. These differences are potentially relevant considerations when generalizing our results to the entire municipal market.

[25] This is consistent with prior literature, which finds that municipalities typically take about seven months to file financial statements (e.g., Cuny [2016]).

would manifest prior to June 30, 2010.[26]

Table 1, Panel B, presents the distribution of municipal continuing disclosure in our sample. Continuing disclosure includes financial information filings according to Rule 15c2-12 as well as a variety of other filings and event-based disclosures. Specifically, municipalities are required to file either audited annual financial statements or audited comprehensive annual financial reports (CAFRs) when available or unaudited annual financial and operating data. Voluntary financial filings include interim financial statements, budgets, and other financial information (e.g., operating data). Other filings include certain notices, such as the failure to provide required annual financial information, that information was provided to a rating agency or that a change in fiscal year occurred. In addition, municipalities must provide notices of certain material events (e.g., principal and interest payment delinquencies, adverse tax opinions, bond calls, etc.) and may provide voluntary event-based disclosures (e.g., litigation and enforcement actions).

To create a broad measure of continuing financial disclosure, we use both the existence and frequency of all continuing disclosures of *financial* information filed with the MSRB, including (1) annual audited financial statements, (2) other annual financial information (annual unaudited financial statements or operating data filed by municipalities that did not file audited financial statements), (3) interim financial information (e.g., quarterly financial statements or monthly operating data), (4) budget filings,[27] and (5) other financial information (miscellaneous filings such as interim operating data). Specifically, we define *FinReporting* as a binary indicator variable equal to one if a municipality files *any* of the abovementioned financial documents in a

[26] In the event these effects do appear in our first reporting period, our prediction suggests that financial reporting would begin to decrease for the treated issuers, making it more difficult to detect a subsequent relative decrease in financial reporting.

[27] As EMMA does not always cleanly categorize budgets, we follow Cuny [2016] and classify filings as budgets if the filing date precedes the fiscal year-end.

given year and *FinReporting_Freq* as the natural logarithm of one plus the number of these filings. We also create existence and frequency variables for each individual measure of financial disclosures.

To provide a more holistic picture of municipal disclosure practices, we also supplement these variables with four additional measures of disclosure: the existence and frequency of event notices (*EventNotice* and *EventNotice_Freq*, respectively);[28] the timeliness of the issuer's first financial filing for a given fiscal year, defined as the number of days between the fiscal year-end date and the earliest financial filing date for that fiscal year, multiplied by –1 (*Timeliness*); and the length of the issuer's audited annual financial statements (*AuditedAnnualLength*) or other annual financial reports (*OtherAnnualLength*).[29]

4.3 Control variables

Our tests also include several control variables that prior literature finds to be correlated with financial reporting characteristics. First, we include a binary indicator variable for debt issuance in a given reporting period (*Issue*), and the natural logarithm of one plus the total amount of rated debt issued (*AmountIssued*). Issuers of larger amounts of debt are subject to greater scrutiny and are typically more transparent (e.g., Gore et al. [2004]). Second, we compute the percentage of callable bonds issued (*%CallableBonds*). Callable bonds tend to be issued by municipalities with higher information asymmetry, who would benefit from the option to refinance (i.e., if borrowers' prospects improve, they can call the bond and refinance at a lower interest rate and better covenant terms) (e.g., Banko and Zhou [2010], Green [2017]). Third, we compute the percentage of GO bonds issued (*%GOBonds*). GO bonds are typically issued by lower risk

[28] We do not include event notices in our main measure, as they are typically provided conditional on the realization of a specific event. However, because these filings are an important part of the issuers' continuing disclosure agreement and potentially represent an important component of issuers' information environment (and may be subject to the issuer's discretion), we supplement our main measures with measures of event-based disclosures. Including these filings in our main measure does not change our inferences and, in fact, strengthens our results (see footnote 35).
[29] We measure *Timeliness* up to June 30, 2013 to allow for one lead year of data for issuers to provide annual filings.

municipalities with better quality collateral, which is likely to be correlated with disclosure. We winsorize all continuous variables at the top and bottom 1% levels.

5. Research Design and Results

5.1 Research design

Our main analysis uses a difference-in-differences regression framework to assess the effect of Moody's ratings recalibration on the likelihood that municipalities disclose financial information. We estimate the following OLS model.[30]

$$FinancialReporting_{i,t} = \alpha_i + \beta_1 Treated_i \text{ x } Post_t + \beta_2 Treated_i + \beta_3 Post_t + \delta \mathbf{X}_{i,t} + \varepsilon_{i,t}, \quad (1)$$

where our dependent variable is either *FinReporting* or *FinReporting_Freq*, as defined above. *Treated* is an indicator variable equal to one for issuers that experienced a rating upgrade from Moody's recalibration (either at the issuer or issuance level) and zero for issuers in our control group. *Post* is a binary indicator variable equal to one in reporting periods beginning after Moody's recalibration (beginning July 1, 2010).

Next, we augment our model with a vector of control variables described in section 4.3. In addition to these control variables, we include various levels of fixed effects. First, we include state-by-year fixed effects, which helps rule out that our results are driven by changes at the state-year level that are correlated with issuers' financial reporting decisions and Moody's recalibration (e.g., regulatory changes at the state level or shocks to the local economy). Note that state-year fixed effects absorb the main effect on *Post* in our estimations and any state-year variables (e.g., unemployment). Another potential concern is that disclosure practices may reflect the characteristics of issuers in certain sectors and levels of government, which could drive our results,

[30] We use OLS regressions because of concerns about bias and consistency of fixed effect estimators in nonlinear maximum likelihood models (e.g., Greene [2004], Arellano and Hahn [2007]).

if these issuers were also more likely to be recalibrated. To address this concern, we also include issuer type and sector fixed effects in our model. Following Cornaggia et al. [2018], we also include issuer rating fixed effects prior to the recalibration to control for differences across issuers that may be correlated with credit ratings. We next add underwriter fixed effects to ensure that our results are not driven by differences in issuers' underwriters. Finally, we include issuer fixed effects, which control for any time-invariant issuer characteristics that may be driving our results (for example, the type of issuer). Note that issuer fixed effects subsume issuer type, sector, and credit rating fixed effects as well as the main effect of *Treated* in our estimations. We predict a negative and significant β_1 coefficient, indicating that issuers receiving a ratings upgrade are less likely to subsequently issue financial information, relative to issuers that are not upgraded.

Consistent with prior studies using the recalibration setting, we cluster standard errors at the issuer level (e.g., Adelino et al. [2017], Cornaggia et al. [2018]). This assumes that the factors driving issuers' disclosure decisions are independent across issuers. A potential concern with this assumption is that issuers within a given state could be subject to similar reporting regulations or local economic conditions. We include state-by-year fixed effects in all our regressions, which removes any variation that is common among issuers in a given state-year. To further address the concern that clustering at the issuer level might overstate statistical significance, we also cluster observations at the state level in robustness tests, which is very conservative as it allows for arbitrary correlation of the error term both cross-sectionally and temporally within a given state.[31]

5.2 Descriptive statistics

We provide descriptive statistics in Table 2 and discuss some variables of interest below. The mean of *FinReporting* is 0.632, indicating that only 63% of issuers provide financial

[31] Clustering at the state level produces results that are weaker but still statistically significant at conventional levels. We present and discuss these results in section IA.8 of the internet appendix.

statements to MSRB on average. This statistic verifies prior observations that municipal issuers often fail to disclose financial information to market participants (Baber and Gore [2008], Schmitt [2011], SEC [2015]). About 46% of the observations in our sample receive a Moody's upgrade and belong to the treatment group. Municipalities in our sample issue bonds 32% of the time with an average amount of $21 million (untabulated).

Figure 1 provides the geographic distribution of disclosure per state. The figure depicts the average of *FinReporting* for each state during our sample period of July 1, 2009, to June 30, 2014. The graph demonstrates significant heterogeneity in the frequency of continuing financial disclosure across states. While Texas and a few other states have average disclosure percentages above 75%, the disclosure rates of many states fall well below 50%. Overall, this figure suggests that the failure to provide continuing financial information is common across the U.S. municipal market.

5.3 Main results

Table 3 presents our results. Panel A presents results from univariate difference-in-differences (i.e., the model in Eqn. (1), excluding the vector of control variables and fixed effects). Columns (1) and (2) present results using *FinReporting* and *FinReporting_Freq* as our dependent variables, respectively. Consistent with our prediction that, all else equal, higher credit rating levels reduce municipal financial reporting, the difference-in-differences coefficient (β_1) is negative and highly significant across both specifications.

Turning to the main effects on *Treated* and *Post*, we make two points. First, the coefficient on *Treated* in columns (1) and (2) is positive and significant (coef. = 0.32 and 0.24, with *t*–stats = 22.16 and 17.56 respectively), indicating that, in the pre–recalibration period, issuers in our treatment group are significantly more likely to issue financial statements, relative to issuers in our control group. Note that our main hypothesis is consistent with a differential information

environment between the treatment and control issuers prior to the recalibration. That is, if investors rely on credit ratings, then we expect that issuers of Moody's rated bonds, which have a lower rating prior to the recalibration, have greater marginal benefits of disclosure, relative to issuers of bonds already calibrated to the global rating scale.

Second, consistent with the increased regulatory disclosure enforcement efforts and implementation of an online information repository that facilitated information dissemination over our sample period, there is an increasing trend in disclosure for issuers in both our treatment and control groups. (For example, in column (1), the coefficient on *Treated* x *Post* + *Post* = 0.07, *t*–stat = 8.81, and *Post* = 0.13, *t*–stat = 14.77, respectively.)[32] Our difference-in-differences results indicate that issuers in our treatment group increase the likelihood of disclosure significantly less after the recalibration, consistent with our hypothesis.

Panel B (Panel C) presents results from difference-in-differences regressions with various levels of fixed effects, using *FinReporting* (*FinReporting_Freq*) as our dependent variable. We sequentially add state-year fixed effects (column (1)), issuer type fixed effects (column (2)), issuer sector fixed effects (column (3)), issuer pre–recalibration credit rating fixed effects (column (4)), underwriter fixed effects (column (5)), and finally issuer fixed effects (column (6)). Across all specifications, the difference-in-differences coefficient (β_1) remains negative and highly significant. The magnitude of the coefficient remains similar in columns (1)–(3) (coefficients between –0.068 and –0.070 in Panel B), with a slight drop in columns (4)–(6) after including issuer rating, underwriter, and issuer fixed effects (coefficients between –0.059 and –0.057 in Panel B). Note that our fixed effects structure helps alleviate several potential concerns regarding the univariate analysis. For example, issuer fixed effects absorb differences in disclosure levels prior to the recalibration, and state-year fixed effects absorb any state-specific or common increasing

[32] This is consistent with the MSRB's finding of an increase in disclosure compliance over this period (MSRB [2014]).

trend in disclosure over our sample period. These factors are thus unlikely to affect our results.

The economic magnitude of the ratings upgrade on disclosure is sizable. The difference-in-differences coefficient in column (6) of Panel B indicates a 5.7% lower probability of filing financial information for issuers that experienced a ratings upgrade, relative to issuers that did not. This represents about 12% of the standard deviation and 9% of the mean of the financial reporting indicator (*FinReporting*) (0.48 and 0.63, respectively). Similarly, the difference-in-differences coefficient in column (6) of Panel C indicates that the frequency of issuers' financial information filings declined by 5.1% (=100*[exp(–0.052)–1]), following a ratings upgrade, relative to that of control issuers that did not experience a ratings upgrade.[33]

5.3.1 Results by type of municipal disclosure

Having obtained our main difference-in-differences estimates, we next explore which specific types of municipal financial filings drive our results, and whether the recalibration also affected other aspects of municipal disclosure. As described in Section 4.2, Rule 15c2-12 requires issuers to file audited annual financial statements (or CAFRs) when available, or otherwise unaudited annual financial and operating data. Interim financial statements, budgets, and other financial information are voluntary. Other filings include notices of certain material events. Disaggregating our results by type of disclosure can shed light on whether the variation is driven by voluntary disclosures, or those that are required.

In Table 4, we present our difference-in-differences results for each individual type of financial disclosure in Panels A and B (audited annual financial statements, other annual financial information, interim financial information, budget information and other financial information; using the binary indicator in Panel A and the natural logarithm of one plus the frequency of each

[33] Including event notices in our main measures does not change our inferences and, in fact, strengthens our main results: using the same specification as column (6) of Table 3 Panel B, the coefficient on *Treated* x *Post* is equal to –0.061 (*t*–stat = –4.50), when using the financial reporting indicator as the dependent variable, and –0.116 (*t*–stat = –6.41), when using the financial reporting frequency measure as the dependent variable.

disclosure in Panel B) and for our alternative disclosure measures in Panel C (*EventNotice*, *EventNotice_Freq*, *Timeliness*, *AuditedAnnualLength*, and *OtherAnnualLength*). Panels A and B suggest that the bulk of the variation in our main result comes from audited annual financial statement filings and filings of other annual and interim financial information (i.e., the difference-in-differences coefficient (β_1) is negative and significant for both the binary indicator measures in columns (1)–(3) on Panel A and the frequency measures in columns (1)–(3) of Panel B). There does not seem to be any significant variation in the provision of budgets or other financial information after the recalibration, perhaps because these filings are rare (representing 6.4% and 3.3% of financial filings, respectively, as shown in Table 1, Panel B).

Panel C demonstrates that both the likelihood and frequency of event notice filings declined significantly for recalibrated issuers, relative to the control group, consistent with our main result in Table 3. For example, column (1) shows that upgraded issuers are 11.1% less likely to file event notices, relative to the control group. Column (3) finds that recalibrated issuers are also less timely in filing continuing financial disclosures, relative to the control group (by about 25 days). Columns (4) and (5), however, show that the length of issuers' annual financial reports does not appear to differentially change with the recalibration between the treatment and control groups. In sum, although upgraded issuers decreased the likelihood, frequency and timeliness of their disclosures, following the recalibration, conditional on filing a report there appears to be no significant variation in its quality. One potential explanation is that these financial reports contain a large amount of boilerplate and investors in this market might view the issuers' disclosure *choice* as being an informative signal but do not necessarily process its content.

5.4 Cross-Sectional Tests

5.4.1 The role of debtholders' demand for information

Our first cross-sectional test investigates how our results vary with the heterogeneity in

investors' demand for information. We measure variation in investors' demand by their trading activity. Specifically, we estimate the same specification as in column (6) of Table 3, Panels B and C, separately for subsamples of issuers with and without bonds traded on the secondary market in the year prior to the recalibration.[34] Estimating separate regressions by subsample has the benefit of facilitating the interpretation of the coefficients and allowing the coefficients on all independent variables and fixed effects to vary within each subsample. Compared to buy-and-hold investors, we expect investors that trade bonds after the initial offering to be more likely to change their demand for information, following the recalibration. Thus, the effect of a rating upgrade on disclosure should be greater for issuers of bonds traded on the secondary market.[35]

Our results appear in Table 5. Consistent with our expectations, issuers with secondary market traders are significantly more likely to decrease disclosure, following a ratings upgrade. For example, within our sample of issuers with traded bonds, the likelihood of disclosure decreases by 7.7% (t–stat = –4.13) for upgraded issuers, relative to the control group, whereas the likelihood of disclosure does not change in our sample of issuers without traded bonds (coefficient = –0.008, t–stat = –0.28). The difference in the *Treated* x *Post* coefficients across the two subsamples is equal to –0.069 (*t*–stat = –2.11).[36] We draw similar inferences when using disclosure frequency as the dependent variable. This indicates that our results are concentrated within our sample of issuers with secondary market traders, suggesting that the relation between credit rating levels and disclosure depends on the issuer's investor base.

[34] Following Green et al. [2006], we define secondary market trades as trades that occur 30 days after bond issuance.
[35] In untabulated univariate analyses within our treatment group, we find that the likelihood and frequency of continuing disclosures prior to the recalibration are significantly greater for issuers with traded bonds, consistent with a greater investor demand for disclosure (59% and 61%, respectively). We also find that the level of these continuing disclosure measures increases significantly less after the recalibration for issuers with traded bonds (-31% and -24%, respectively).
[36] To test the difference in the treatment effect between both subsamples, we fully interact all the independent variables and fixed effects in our regression with a binary indicator variable flagging the sample of issuers with traded bonds (*SecMktTrading*). The coefficient on the triple-difference *Treated* x *Post* x *SecMktTrading* represents the difference in treatment effect between subsamples reported in the table. We use this approach in all subsequent tests of differences in treatment effect between subsamples.

5.4.2 The role of other gatekeepers

5.4.2.1 The role of underwriters

Next, we perform two sets of cross-sectional tests to investigate how our results vary with underwriter characteristics that could be associated with their enforcement of continuing disclosure: expertise and competition. The intensity of disclosure enforcement across underwriters is likely to vary for at least two reasons. First, assessing whether municipalities will comply with their continuing disclosure agreements as promised is costly and far from straightforward.[37] In response letters to the SEC, several underwriters characterized the requirement to forecast issuers' disclosure compliance as "labor intensive and costly, and even impossible" (SEC [2010], p. 94). Second, underwriters are paid by municipal issuers, and this potentially creates incentive problems for underwriters who may be willing to overlook the lack of disclosure in exchange for client business. To further our understanding of the potential drivers of underwriters' effectiveness as monitors of continuing disclosure, we explore how our results vary with several underwriter characteristics.

First, we examine whether greater expertise enables underwriters to monitor disclosure compliance more effectively. Given the opaque and fragmented nature of the municipal market, underwriters with more information regarding the issuer's bond sector and local political and economic environment are likely to provide a better forecast of issuers' commitment to continuing disclosure. We consider two dimensions of underwriters' expertise. The first dimension is bond-sector expertise. Underwriters who specialize in the issuer's given bond sector likely better understand that sector's economic outlook and standard financial reporting practices. We measure sector expertise within the two primary sectors of the municipal market: general obligation (GO)

[37] For example, it may not be possible for underwriters to determine whether a material event, which requires the disclosure of an event notice, occurred.

bonds and revenue bonds.[38]

The second dimension is the geographic proximity of the underwriter, captured by comparing local versus national underwriters. Prior literature suggests that local underwriters better understand local economic conditions and benefit from their knowledge of issuers' soft information, which may also enhance their local reputation. Consistent with this argument, Butler [2008] finds that local underwriters sell bonds at lower yields. This would suggest that local underwriters have greater issuer-specific expertise and are more effective in enforcing disclosure, relative to national underwriters. On the other hand, while national underwriters likely have lower local expertise, they may have more resources, experience, and human capital to perform their due diligence, particularly in analyzing issuers' hard information. Consistent with this argument, Daniels and Vijayakumar [2007] find that municipal bond issues managed by large underwriters have lower bond yields, suggesting they may be more effective monitors. We thus explore whether the reduction in disclosure, following the recalibration, is greater for issuers with local versus national underwriters.

In Table 6, Panel A, we estimate the same specification as in column (6) of Table 3, Panels B and C, separately for subsamples of issuers with underwriters that have varying dimensions of expertise. In columns (1)–(4), we measure expertise using the underwriter's bond-sector specialty. We define *Specialist* as a binary indicator variable equal to one if the underwriter's proportion of bond issuances in a reporting period is in the top quartile of the issuer's bond sector (GO or revenue bonds).[39] In columns (5)–(8), we measure expertise by whether the underwriter is local versus national. We define *Local* as a binary indicator variable equal to one when the municipality issues

[38] GO bonds typically cover municipalities' day-to-day operations and are backed by the full faith and credit of local governments (i.e., tax dollars), whereas the payment of revenue bonds is tied to the revenue stream of their underlying capital projects.

[39] Thus, "specialists" are defined as underwriters that are either in the bottom or top quartile of the distribution of the percentage of GO (or revenue) bonds, leaving 50% of the distribution effectively classified as "non-specialists."

bonds with a local underwriter, defined as an underwriter that operates in the two bottom terciles of the number of states in a reporting period.[40]

Our results show that issuers with *Specialist* underwriters are significantly *less* likely to reduce disclosure, following a ratings upgrade. For example, the likelihood of disclosure for upgraded issuers with *Specialist* underwriters does not change following the recalibration, relative to the control group (coefficient = –0.013, t–stat = –0.58), whereas it decreases by 8.6% (t–stat = –4.61) for issuers with non-*Specialist* underwriters. The difference in the *Treated* x *Post* coefficients across the two subsamples is equal to 0.073 (*t*–stat = 2.57). Using disclosure frequency as the dependent variable, we draw similar inferences for issuers with both *Specialist* and *Local* underwriters as proxies for underwriter expertise. However, we do not find a significant difference in the *Treated* x *Post* coefficients across subsamples of issuers with local versus national underwriters when using disclosure likelihood as our dependent variable (coefficient = 0.042, t–stat = 1.36), so these results should be interpreted with caution.

Second, we examine whether competition among underwriters relates to their effectiveness as disclosure monitors. On the one hand, competition can exacerbate underwriters' conflicts of interest. Increased competition enhances issuers' ability to shop for an underwriter that will cater to their demands (e.g., Becker and Milbourn [2011]). As a result, increased competition may create greater incentives for underwriters to ignore disclosure noncompliance, reducing their effectiveness as disclosure monitors. On the other hand, greater competition can incentivize underwriters to more closely monitor their clients' continuing disclosure and select more disclosure-compliant issuers to improve their reputation and distinguish themselves from their peers. We examine how the reduction in continuing disclosure, following the recalibration, varies

[40] The median number of states underwriters operate in is 11. We chose the top tercile as our cutoff for "national" underwriters (and thus the bottom two terciles as our definition of "local" underwriters), which corresponds to underwriters operating in at least 30 states. Our results yield very similar coefficients and statistical significance on *Treated* x *Post* when using the top quartile (38 states) to define national underwriters.

with underwriter competition.

In Panel B, we estimate the same specification as in Panel A for different measures of underwriter competition. In columns (1)–(4), we measure competition using the underwriter's market share (*MktShr*), a binary indicator variable equal to one if the underwriter's proportion of bond issuance dollars in a given state-year exceeds the sample median. In columns (5)–(8), we measure competition using the underwriter's Herfindahl index (*Herf*), a binary indicator variable equal to one if the underwriter's Herfindahl index in a given state-year exceeds the sample median, where the Herfindahl index is the sum of squared underwriter market share in a given state–year. Across all specifications, we find no evidence of a significant difference in the *Treated* x *Post* coefficients across subsamples of issuers with high versus low competition.

Taken together, this evidence suggests that underwriters with client-specific knowledge play an important role as gatekeepers of continuing disclosure, but that underwriter competition has no effect on this gatekeeping role, perhaps due to the strong segmentation of this market, which constrains competitive entry (e.g., Butler [2008]).

5.4.2.2 The role of direct regulatory oversight

Finally, we examine how our results vary with direct regulatory oversight over issuers' continuing disclosure. Municipalities receiving federal grants exceeding $750,000 are subject to the Single Audit Act and must provide annual audited financial statements to the federal government. We estimate the same regressions as in column (6) of Table 3, Panels B and C, separately for subsamples of municipalities that are and are not subject to direct regulatory enforcement under the Single Audit Act during our sample period. Our results in Table 7 show that upgraded issuers subject to the act decrease disclosure significantly less, following the recalibration. For example, the likelihood of disclosure for upgraded issuers subject to the Single Audit Act does not change following the recalibration, relative to the control group (coefficient =

0.000, t–stat = 0.02), whereas it decreases by 9.0% (t–stat = –5.23) for issuers that are not subject to the act. The difference in the *Treated* x *Post* coefficients across the two subsamples is equal to 0.090 (*t*–stat = 3.24). Our inferences are similar when using disclosure frequency as the dependent variable. This suggests that direct regulatory disclosure enforcement is effective in preventing a decline in continuing disclosure after rating increases.

6. Additional channels and robustness tests

6.1 The information content channel

A decline in continuing disclosure, following the recalibration, is also consistent with the alternative channel that the *information content* of the ratings increased, leading to a lower investor demand for disclosure as opposed to merely reflecting a change in the ratings scale. In this section, we investigate this channel. On the one hand, the institutional details surrounding Moody's recalibration suggest that the recalibration is unlikely to alter—and may even reduce—the information content of credit ratings. The recalibrated ratings became less granular, which makes it more difficult to assess default risk within a given rating.[41]

On the other hand, investors may have *perceived* that the change in scale increased the rating's information content, thus decreasing their demand for disclosure. We test this conjecture by examining whether our results differ between issuers of single-rated bonds and issuers of dual-rated bonds (rated by both Moody's and S&P). Recall that, before Moody's recalibration, S&P ratings were already calibrated to the global rating scale (and thus contained information about loss given default). If Moody's recalibration resulted in more informative credit ratings, then we would expect the effect of the recalibration to be weaker for issuers of bonds rated by both

[41] Please refer to section IA.1 of the internet appendix for a detailed narrative of Moody's recalibration and its implications for the information content of the ratings.

Moody's and S&P than for bonds rated only by Moody's, as investors in dual-rated bonds would be able to glean information about loss given default from the S&P rating.[42]

Our results appear in Table 8, Panel A. We estimate the same specifications as in column (6) of Table 3, Panels B and C, except that we replace *Treated* with *Treated_OnlyMoodys*, a binary indicator for treated issuers that issued exclusively Moody's rated bonds in the four years prior to the recalibration and zero otherwise, and *Treated_MoodysS&P*, a binary indicator for treated issuers that issued exclusively bonds rated by both Moody's and S&P in the four years prior to the recalibration and zero otherwise. Consistent with our results in Table 3, both sets of treated issuers are significantly less likely to file financial information after the recalibration, relative to the control group. Although the coefficient on *Treated_OnlyMoodys* x *Post* is larger, relative to the coefficient on *Treated_MoodysS&P* x *Post*, the difference is not significantly different from zero, suggesting that our results do not differ between single- and dual-rated issuers.

Our next test attempts to directly control for changes in information content. According to this channel, a change in the information content of Moody's ratings should lower debtholders' uncertainty about the likelihood of future debt repayment (i.e., information asymmetry between issuers and debtholders), which would change their demand for information and issuers' disclosure incentives. Therefore, if a change in information content is the primary driver of our results, then we should expect a large attenuation of our main results after controlling for information asymmetry. Following Schwert [2017], we employ two measures of information asymmetry (described in Appendix A): (1) volume (*Volume*) and (2) the price impact of trades (*PriceImpact*), similar to Amihud's [2002] measure. To avoid losing observations and ensure that we perform this robustness test on the sample that generates our main results, we also include *MissingObs*, a binary

[42] A necessary assumption behind this interpretation is that any change in information content for single-rated issuers is greater than for dual-rated issuers (i.e., even if S&P is learning from the new Moody's rating and improves the information content of its own rating, the change in information content is still greater for the single-rated issuers who never benefited from a second rating than for dual-rated issuers who had an S&P rating).

indicator variable equal to one if each corresponding information asymmetry variable is missing. Table 8, Panel B, shows that our findings are robust to controlling for either measure of information asymmetry, as the coefficients change very little and their statistical significance is virtually unchanged.[43] Overall, these tests suggest that the information content channel is not the primary driver of our results.

6.2 Changes in investor base

In this section, we examine whether our results could be driven by a change in investor base. The recalibration may have led to a change in the municipal investor base, and the new investor base has lower demands for financial disclosure.[44] We examine this possibility in Table 9 by testing whether the relative percentage of retail and institutional trades shifted after the recalibration. Specifically, we estimate the same specifications as in column (6) of Table 3, Panel B, except that we replace our dependent variable with measures of investor composition. In column (1), our dependent variable is *PctRetail*, the percentage of retail trades by issuer-year, where a retail trade is defined as a trade less than or equal to $100,000. In column (2), our dependent variable is *PctInstit*, the percentage of large institutional trades by issuer-year, where a large institutional trade is defined as a trade exceeding $250,000.[45] Our sample requires the existence of trades on the secondary market over our sample period (sample of 11,650 observations). In both specifications, the coefficient on *Treated* x *Post* is insignificantly different from zero, suggesting that the recalibration did not significantly change the percentage of trades by retail or institutional investors, making it unlikely that our results are due to a change in the mix of different types of

[43] In additional robustness tests, we also fail to find a differential change in liquidity between the treatment and control groups, following the recalibration (see section IA.7 of the internet appendix). This result suggests that our findings are not driven by issuers adjusting their disclosure policies in light of changes in market liquidity.

[44] Sixty-seven percent of municipal bonds were held directly or indirectly by retail investors at the end of 2016. In our sample period, direct household investment declined from 52% of holdings in 2009 ($1.99 trillion) to 45% ($1.70 trillion) in 2014 (MSRB [2017], SIFMA [2016]). During this period, indirect household investment also declined slightly, while investment from banks and insurance companies increased proportionately.

[45] We follow prior literature and use market convention to distinguish between retail and institutional trading (e.g., Edwards et al. [2007], Cuny [2018]).

investors.

6.3 Trends around the recalibration

Our inferences rely on the identifying assumption that, absent the recalibration, the *change* in financial reporting for upgraded issuers would not have been different from the *change* in financial reporting for unaffected issuers (i.e., the parallel-trends assumption). In this section, we provide evidence consistent with the validity of this assumption.

First, we examine the difference in the likelihood of continuing disclosure between our treatment and control groups in each reporting period after the recalibration, relative to the immediately preceding period. A potential concern is that our results may only manifest in a single year after the recalibration, which would suggest that they are not attributable to the event. We present our results in Table 10, Panel A where we estimate the same model as in column (6) of Table 3, Panels B and C, respectively, except that we interact *Treated* with each of the post-recalibration reporting periods (*Post1* – *Post4*). Consistent with ratings upgrades having a persistent effect on municipalities' likelihood and frequency of disclosure, we find that all coefficients on our *Treated* x *Post* interaction terms are negative and significant, suggesting that our results are not driven by a single period. Interestingly, the effect is relatively more pronounced in reporting period *Post4*. One potential explanation is that it takes some time for municipalities to adjust their disclosure practices.[46]

Second, we examine the extent to which the treatment and control groups follow differential trends in their underlying economics leading up to the recalibration. A drawback of

[46] We believe that this is consistent with the nature of disclosure in the municipal setting. Given that financial statements are typically filed seven months after year-end, our tests might pick up some financial statements from previous years, particularly in reporting period *Post1*. Furthermore, in contrast to disclosures in information-rich environments, disclosure decisions in the municipal setting likely take longer to adjust. For example, issuers may have set multiyear budgets for producing financial statements and signed contracts with accounting staff and internal and external auditors. In addition, municipalities that did not issue bonds over several years following the recalibration may have taken time to become aware of the recalibration and its implications for debtholders' demand for disclosure—particularly governments with limited accounting expertise. Thus, the effect on disclosure decisions can take some time to materialize.

our empirical setting is that the disclosure data begin one year prior to the recalibration. Ideally, we would verify that there is no differential trend in continuing disclosure leading up to the recalibration. Instead, we test whether our treatment and control groups are comparable along two key observable dimensions of the information environment related to disclosure: bond yields and credit ratings. The advantage of using these measures is twofold. First, prior literature suggests that the cost of capital and credit ratings are related to municipalities' disclosure (e.g., Baber and Gore [2008], Cuny and Dube [2018]). Finding no differential trend in the cost of capital or credit ratings between our treatment and control groups prior to the recalibration mitigates concerns that there exists a differential pre-trend in the capital market benefits of disclosure and thus in their financial reporting practices. Second, these tests allow us to corroborate whether municipalities received ratings upgrades and experienced a drop in the cost of capital, following the recalibration, as expected.

Our results appear in Table 10, Panel B. Column (1) estimates difference-in-differences in bond yields (*Yield*) in the seven reporting periods around the recalibration event spanning July 1, 2006–June 30, 2014 (*Pre4 – Pre2* and *Post1 – Post4*), relative to our benchmark period (July 1, 2009–June 30, 2010 = *Pre1*). We estimate this regression at the bond level. We retain only uninsured bonds, resulting in a sample of 95,455 observations. We compute *Yield* using the annual average bond yield weighted by trade size in the secondary market. We control for binary indicators for rating agencies (*Moody* and *S&P*) and a binary indicator variable for whether bonds in our control group switched ratings from S&P to Moody's (*SwitchToMoodys*). We also include state-by-year fixed effects, bond fixed effects (subsuming issuer fixed effects), and bond maturity as well as logged bond amount interacted with year indicators (*Maturity* x *Year* and *Amount* x *Year*) to allow for credit risk to vary over time. Consistent with a sharp drop in bond yields for upgraded issuers after the recalibration, we find that all *Treated* x *Post* coefficients are significantly

negative.

Column (2) estimates the same specification as in column (1), using the credit rating of bond issuances in the primary market as our dependent variable (*CreditRating*). We estimate this regression at the issuer-year level, resulting in 4,893 observations. We also include state-by-year fixed effects, underwriter fixed effects, and issuer fixed effects in the model. Consistent with a sharp increase in credit ratings for upgraded issuers after the recalibration, we find that all *Treated* x *Post* coefficients are significantly positive.

Importantly, in both columns, the coefficients on *Treated* x *Pre2* through *Treated* x *Pre4* are not significantly different from zero, suggesting that our treatment observations did not experience a differential trend in cost of capital or credit ratings leading up to the recalibration. Figure 2 provides a visual representation of our three trend analyses.

6.4 Additional robustness tests

We perform several additional robustness tests presented in the internet appendix, starting with a matched-sample approach. Our difference-in-differences analysis uses a treatment group of Moody's-rated issuers that were upgraded through the recalibration and a control group of S&P-only rated issuers. A potential concern is that our control group differs from the treatment group along unobserved dimensions, which would violate the parallel-trends assumption. To help address this concern, we repeat our difference-in-differences analyses, using two alternative control groups. First, we match treated issuers to control issuers based on a set of pre-recalibration covariates. Second, we match treated issuers to control issuers in the set of recalibrated non-upgraded municipalities. Our results are robust to both of these alternative control groups (see sections IA.2 and IA.3 of the internet appendix).

Next, we perform a falsification test, using insured bonds. The rating on insured bonds reflects the rating of the insurance company rather than the credit rating of the underlying

municipality, and accordingly issuers of insured bonds should be unaffected by the recalibration. Our results provide evidence consistent with this prediction, as we do not find a significant change in disclosure for issuers with insured bonds (see section IA.4 of the internet appendix).

Finally, we test how our results vary with the amount of the treated issuers' credit rating upgrades and bond maturity. Consistent with our expectations, we find that the treatment effect is greater for issuers that were upgraded the most and increases with the maturity of the issuer's bonds, as longer maturity may expose investors to greater credit risk and increase their ex ante demand for information (see section IA.6 of the internet appendix).

7. Conclusion

The municipal bond market is critical to funding state and local government employment and capital expenditures, yet the information environment is opaque. An important feature of this market is the large presence of investors who tend to rely on credit ratings for their investment decisions (SEC [2012]), highlighting the importance of credit rating agencies' role as information intermediaries. In this paper, we examine the role of rating levels on disclosure and how this relation varies as a function of the investor characteristics and unique enforcement mechanisms present in this market.

Using Moody's recalibration of their ratings methodology in 2010, we find that upgraded municipalities are less likely to disclose continuing financial information, relative to municipalities that were rated by S&P and not recalibrated. However, we show that this reduction in disclosure can be mitigated by other disclosure enforcement mechanisms. Specifically, in lieu of direct enforcement, the SEC relies on underwriters to encourage continuing disclosure. We find that disclosure declines less for debt issuers with underwriters that have greater client-specific knowledge, but we do not find evidence that competition among underwriters affects issuers'

decision to reduce disclosure. We also find that disclosure does not decline for issuers under direct regulatory oversight related to federal funding through the Single Audit Act. Taken together, our results suggest that, while higher credit ratings lower investors' demand for information and issuers' disclosure, the type of investor and disclosure enforcement significantly influences this effect.

Understanding the determinants of disclosure in the municipal market is important, as many claim that the rise in public pension obligations and healthcare costs will increase municipal default rates in the future (e.g., Novy-Marx and Rauh [2011a], [2011b], Moody's [2017]). Moreover, the opaque information environment benefits broker-dealers at the expense of the vast majority of household investors who dominate this market. While the SEC continues to launch new initiatives aimed at improving continuing disclosure, our results suggest that the opacity of the information environment partly reflects investors' demand. This begs the question of whether improving issuers' continuing disclosure compliance will necessarily result in the benefits expected by the SEC or academic research suggesting that improved disclosure might mitigate investors' mechanistic reliance on ratings in poor information environments (Cornaggia et al. [2018]). If investors do not process additional financial information when ratings are relatively high, then an increase in continuing disclosure compliance will not necessarily improve their bargaining power with broker-dealers or reduce their reliance on credit ratings. Overall, our study highlights the importance of credit rating agencies on the transparency of the municipal market, which should be of interest to academics as well as regulators such as the SEC and MSRB.

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Appendix A. Variable definitions

FinReporting	Binary indicator variable equal to one if the municipality filed financial information in a given reporting period (July 1–June 30), including (1) annual audited financial statements, (2) other annual financial information (annual unaudited financial statements or operating data filed by municipalities that did not file audited financial statements), (3) interim financial information (e.g., quarterly financial statements or monthly operating data), (4) budget filings, and (5) other financial information (miscellaneous filings such as interim operating data).
FinReporting_Freq	Natural logarithm of one plus the number of financial filings provided in a given reporting period.
EventNotices	Binary indicator variable equal to one if the municipality filed an event notice in a given reporting period.
EventNotice_Freq	Natural logarithm of one plus the number of event notices filed in a given reporting period.
Timeliness	Number of days between the municipality's fiscal year-end date and its first financial filing for that fiscal year, multiplied by –1.
AuditedAnnualLength	Natural logarithm of one plus the number of pages of a municipality's audited annual financial statements.
OtherAnnualLength	Natural logarithm of one plus the number of pages of a municipality's other annual financial reports (annual unaudited financial statements or operating data filed by municipalities that did not file audited financial statements).
Treated	Binary indicator variable equal to one for municipalities that experienced a rating upgrade from Moody's recalibration (either at the issuer and/or the bond level) and zero for municipalities that issued debt rated only by S&P over the four years prior to Moody's recalibration.
Post	Binary indicator variable equal to one in reporting periods beginning after the recalibration (reporting periods beginning July 1, 2010).
Control variables:	
Issue	Binary indicator equal to one if the issuer issued debt in a reporting period.
AmountIssued	Natural logarithm of one plus the amount of debt issued in a reporting period.
%CallableBonds	The percentage of callable bonds issued in a reporting period.
%GOBonds	The percentage of general obligation (GO) bonds issued in a reporting period.
Other variables:	
SecMktTrading	Binary indicator variable equal to one for issuers of bonds that were traded by investors on the secondary market in the year prior to the recalibration, where secondary market trades refer to trades executed after one month following the initial bond offering.
Specialist	Binary indicator variable equal to one if the underwriter's proportion of bond issuances that belong to the issuer's bond sector (GO or revenue bonds) in a given reporting period is in the top quartile.
Local	Binary indicator variable equal to one if the issuer's underwriter is local, defined as operating in the two bottom terciles of the number of states in a given reporting period.
MktShr	Binary indicator variable equal to one if the underwriter's proportion of bond issuance (in dollars) in a given state-year exceeds the sample median.
Herf	Binary indicator variable equal to one if the underwriter's the Herfindahl index in a given state-year exceeds the sample median, where the Herfindahl index is the sum of squared underwriter market share (*MktShr*) in a given state-year.

Appendix A. Variable definitions (cont'd)

Control variables (cont'd):	
SingleAudit	Binary indicator variable equal to one if the issuer is subject to an audit under the Single Audit Act during our sample period.
Volume	The natural logarithm of one plus the number of secondary market trades in a reporting period, where secondary market trades refer to trades executed after one month following the initial bond offering.
PriceImpact	The average daily price impact in a reporting period, multiplied by one million, measured as $\frac{1}{N_t}\sum_j \frac{\left\lvert\frac{P_j - P_{j-1}}{P_{j-1}}\right\rvert}{Q_j}$, where N_t is the number of trades in reporting period t, P_j is the price of a bond in trade j, and Q_j is the par amount of trade j.
MissingObs	Binary indicator variable equal to one if the information asymmetry variable *(Volume* or *PriceImpact)* is missing.
PctRetail	The percentage of retail trades by issuer-year, where a retail trade is defined as a trade not exceeding $100,000.
PctInstit	The percentage of large institutional trades by issuer-year, where an institutional trade is defined as a trade exceeding $250,000.

Figure 1. Geographic distribution of financial reporting

This figure presents the geographic distribution of financial disclosure from July 1, 2009, through June 30, 2014. The graph plots the average disclosure (the mean of *FinReporting*) of each state during the sample period.

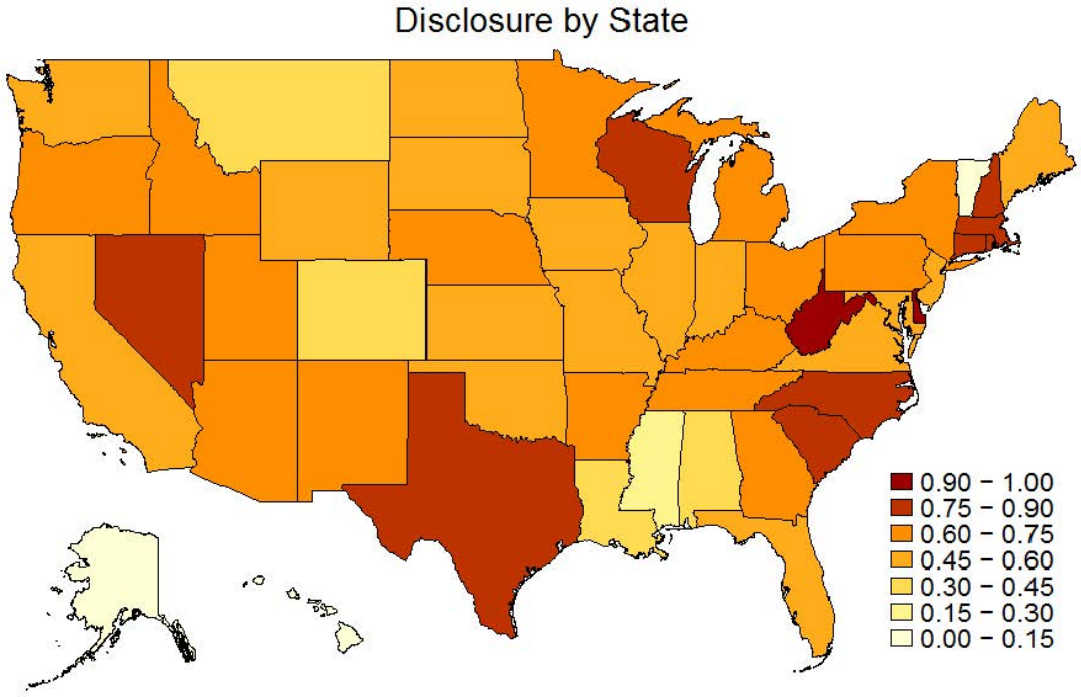
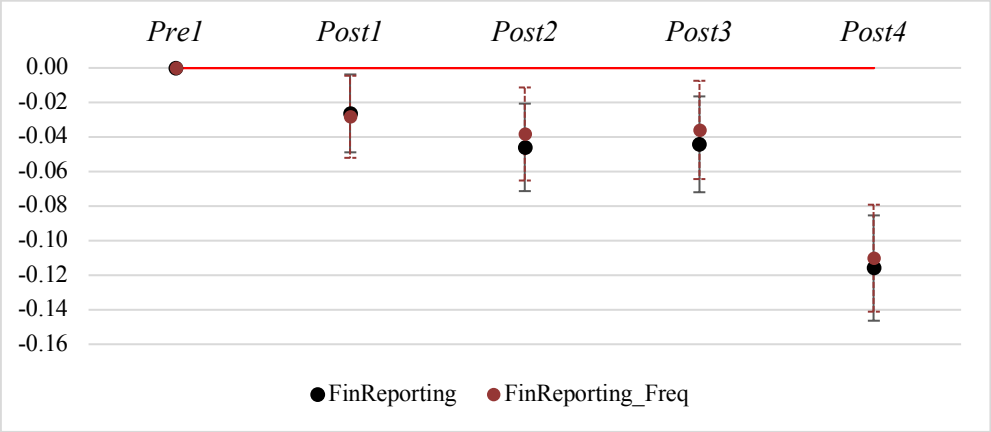


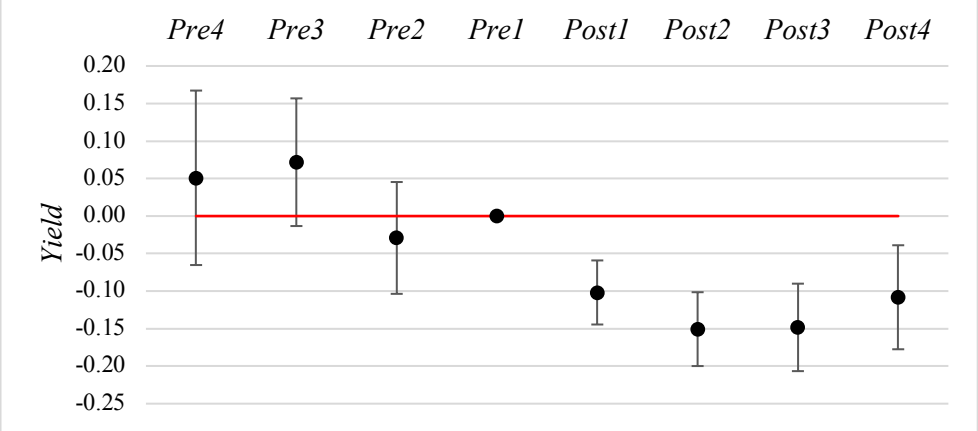
Figure 2. Trends around the ratings recalibration

This figure presents differences in *FinReporting* and *FinReporting_Freq* (Panel A), in bond yields (Panel B), and in credit ratings (Panel C) between our treatment and control groups around Moody's recalibration, relative to our benchmark reporting period (2009 = *Pre1*), where each reporting period runs from July 1–June 30. The difference-in-differences coefficients in Panel A are reported in Table 10, Panel A. The difference-in-differences coefficients in Panels B and C are reported in Table 10, Panel B, columns (1) and (2), respectively. The bars represent 90% confidence intervals.

Panel A: Financial reporting trends after the recalibration



Panel B: Yield trends around the recalibration



Panel C: Credit rating trends around the recalibration

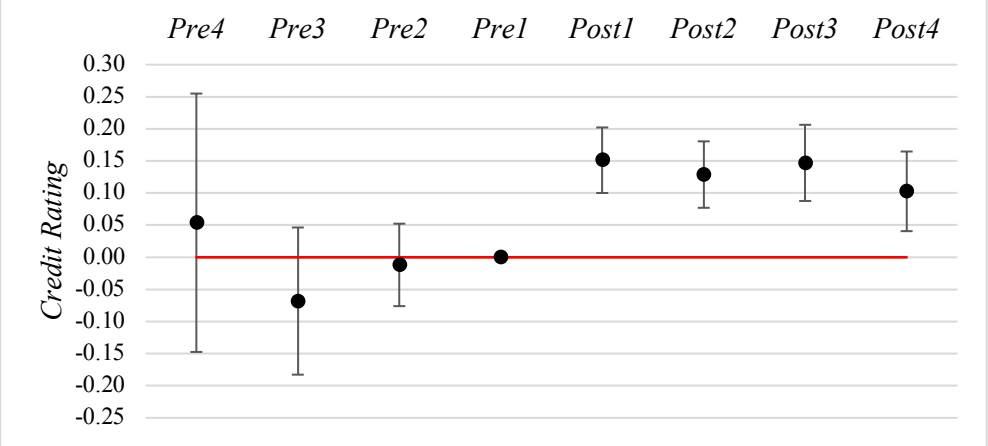


Table 1. Sample selection

This table presents the selection of our sample in Panel A and municipal continuing disclosures of financial information and event notices over our sample period in Panel B. Our sample runs from July 1, 2009, through June 30, 2014, and each reporting period runs from July 1–June 30.

Panel A. Sample of observations

Sample	Observations
Panel of issuers on Thomson SDC Platinum (from July 1, 2009–June 30, 2014) that issued rated debt in the four years prior to the recalibration	90,915
Less issuers of debt rated by Fitch	(51,675)
Less Moody's rated issuers without recalibration data in Mergent	(10,030)
Less issuers of exclusively insured bonds	(8,070)
Less observations without data on control variables	(55)
Total sample of issuer-years	21,085

Panel B. Municipal filings on MSRB

Filing type	Number	%
Audited annual financial statements	10,280	0.588
Other annual financial information	4,926	0.282
Interim financial information	578	0.033
Budget	1,114	0.064
Other financial information	581	0.033
Total financial filings	17,479	1.000
Event notices	15,996	
Total filings	33,475	

Table 2. Descriptive statistics

This table presents descriptive statistics for the variables used in our main analyses. Our sample runs from July 1, 2009 through June 30, 2014, and each reporting period runs from July 1–June 30. All variables are defined in Appendix A.

Variable	N Obs.	Mean	Std	25th	Median	75th
FinReporting	21,085	0.632	0.482	0.000	1.000	1.000
FinReporting_Freq	21,085	0.521	0.466	0.000	0.693	0.693
EventNotice	21,085	0.324	0.468	0.000	0.000	1.000
EventNotice_Freq	21,085	0.331	0.536	0.000	0.000	0.693
Timeliness	11,913	–223.2	218.1	–260.0	–182.0	–157.0
AuditedAnnualLength	8,568	4.322	0.693	3.970	4.290	4.828
OtherAnnualLength	8,653	3.007	1.292	1.946	2.773	4.143
Treated	21,085	0.461	0.499	0.000	0.000	1.000
Post	21,085	0.800	0.400	1.000	1.000	1.000
Issue	21,085	0.323	0.468	0.000	0.000	1.000
AmountIssued	21,085	0.745	1.254	0.000	0.000	1.395
%CallableBonds	21,085	0.222	0.403	0.000	0.000	0.000
%GOBonds	21,085	0.227	0.415	0.000	0.000	0.000
SecMktTrading	21,085	0.586	0.492	0.000	1.000	1.000
Sector	21,085	0.374	0.484	0.000	0.000	1.000
Local	21,085	0.684	0.465	0.000	1.000	1.000
MktShr	21,085	0.499	0.500	0.000	0.000	1.000
Herf	21,085	0.498	0.500	0.000	0.000	1.000
SingleAudit	21,085	0.350	0.477	0.000	0.000	1.000
Volume	14,939	2.809	1.481	1.609	2.639	3.850
PriceImpact	13,300	1.210	1.335	0.481	0.838	1.413
PctRetail	11,650	0.772	0.237	0.700	0.833	0.935
PctInstit	11,650	0.088	0.163	0.000	0.029	0.100

Table 3. Difference-in-differences in financial reporting around the ratings recalibration

This table presents difference-in-differences estimates of municipal financial reporting around Moody's ratings recalibration. *Treated* is a binary indicator variable equal to one for issuers that experienced a rating upgrade from the recalibration and zero for municipalities that issued debt rated only by S&P in the four year prior to the recalibration. *Post* is a binary indicator variable equal to one in reporting periods starting after the recalibration (beginning July 1, 2010). Panel A presents results from univariate difference-in-differences. Column (1) presents results from regressions using *FinReporting* as our dependent variable, and column (2) presents results from regressions using *FinReporting_Freq* as our dependent variable. Panel B presents results from difference-in-differences regressions with control variables and various levels of fixed effects, using *FinReporting* as our dependent variable. Panel C presents results from difference-in-differences regressions with control variables and various levels of fixed effects, using *FinReporting_Freq* as our dependent variable. All variables are as defined in Appendix A. *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Panel A. Univariate difference-in-differences

Dependent variable:	*FinReporting* (1)	*FinReporting_Freq* (2)
Treated x *Post*	–0.055***	–0.047***
	(–4.53)	(–3.88)
Treated	0.320***	0.244***
	(22.16)	(17.56)
Post	0.128***	0.111***
	(14.77)	(13.02)
Intercept	0.401***	0.338***
	(39.03)	(34.40)
Observations	21,085	21,085
R^2 (%)	9.0	5.5

Table 3. Difference-in-differences in financial reporting around the ratings recalibration (cont'd)

Panel B. Difference-in-differences regressions: Financial reporting indicator

Dependent variable:	*FinReporting*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x *Post*	–0.068***	–0.070***	–0.070***	–0.059***	–0.057***	–0.057***
	(–4.99)	(–5.10)	(–5.08)	(–4.33)	(–4.15)	(–4.20)
Treated	0.310***	0.316***	0.314***	0.250***	0.235***	
	(19.60)	(19.87)	(19.70)	(15.51)	(14.35)	
Control variables						
Issue	0.069***	0.072***	0.071***	–0.003	0.016	–0.018
	(3.23)	(3.36)	(3.33)	(–0.14)	(0.79)	(–0.98)
AmountIssued	0.007	0.004	0.004	0.013**	0.010*	0.013**
	(1.15)	(0.72)	(0.68)	(2.31)	(1.76)	(2.52)
%CallableBonds	0.006	0.011	0.010	0.005	–0.008	–0.017
	(0.45)	(0.80)	(0.72)	(0.40)	(–0.62)	(–1.37)
%GOBonds	–0.015	–0.004	–0.003	–0.001	–0.003	0.031**
	(–0.96)	(–0.29)	(–0.21)	(–0.08)	(–0.20)	(2.40)
State x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Issuer type FE	No	Yes	Yes	Yes	Yes	No
Sector FE	No	No	Yes	Yes	Yes	No
Issuer rating FE	No	No	No	Yes	Yes	No
Underwriter FE	No	No	No	No	Yes	Yes
Issuer FE	No	No	No	No	No	Yes
Observations	21,085	21,085	21,085	21,085	21,085	21,085
R^2 (%)	15.7	16.2	16.5	22.9	26.5	69.0

Table 3. Difference-in-differences in financial reporting around the ratings recalibration (cont'd)

Panel C. Difference-in-differences regressions: Financial reporting frequency

Dependent variable:	*FinReporting_Freq*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x *Post*	–0.060***	–0.062***	–0.062***	–0.053***	–0.053***	–0.052***
	(–4.36)	(–4.50)	(–4.50)	(–3.87)	(–3.86)	(–3.83)
Treated	0.232***	0.240***	0.242***	0.190***	0.176***	
	(15.17)	(15.74)	(15.91)	(12.36)	(11.27)	
Control variables						
Issue	0.058***	0.057***	0.058***	–0.001	0.027	0.012
	(2.61)	(2.60)	(2.67)	(–0.05)	(1.24)	(0.59)
AmountIssued	0.034***	0.029***	0.029***	0.036***	0.027***	0.016***
	(4.62)	(4.12)	(4.10)	(5.26)	(4.17)	(2.93)
%CallableBonds	0.003	0.004	–0.001	–0.005	–0.014	–0.009
	(0.19)	(0.27)	(–0.08)	(–0.39)	(–0.97)	(–0.66)
%GOBonds	–0.057***	–0.027	–0.020	–0.018	–0.024	0.024*
	(–3.27)	(–1.59)	(–1.21)	(–1.16)	(–1.49)	(1.68)
State x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Issuer type FE	No	Yes	Yes	Yes	Yes	No
Sector FE	No	No	Yes	Yes	Yes	No
Issuer rating FE	No	No	No	Yes	Yes	No
Underwriter FE	No	No	No	No	Yes	Yes
Issuer FE	No	No	No	No	No	Yes
Observations	21,085	21,085	21,085	21,085	21,085	21,085
R^2 (%)	11.2	12.2	13.4	18.0	21.5	62.3

Table 4. Alternative measures of disclosure

This table presents difference-in-differences estimates of municipal financial reporting around Moody's ratings recalibration for each individual type of municipal disclosure. The specifications in Panels A and B mirror those in column (6) of Table 3, Panel B and C, except that we replace our dependent variable, in turn, by the following variables: Columns (1): audited annual financial statement filings; columns (2): other annual financial information filings; columns (3): interim financial information filings; columns (4): budget filings; and columns (5): other financial information filings. Panel A presents results from regressions using binary indicators equal to one if the issuer provided each respective type of filing in a given reporting period. Panel B presents results from estimates using the natural logarithm of one plus the number of each type of filing in a given reporting period. For parsimony, we do not tabulate coefficients on control variables. (Full tables are presented in section IA.9 of the internet appendix). The specifications in Panel C mirror those in column (6) of Table 3, Panels B and C, except that we replace our dependent variable by *EventNotice* in column (1), *EventNotice_Freq* in column (2), *AuditedAnnualLength* in column (3), *OtherAnnualLength* in column (4), and *Timeliness* in column (5), as defined in Appendix A. *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Panel A. Difference-in-differences by type of financial filing indicators

Dependent variable: Financial filing type indicators					
	Audited Annual Financial Statements	*Other Annual Financial Information*	*Interim Financial Information*	*Budget*	*Other Financial Information*
	(1)	(2)	(3)	(4)	(5)
Treated x *Post*	–0.034**	–0.032**	–0.006**	0.008	–0.001
	(–2.35)	(–2.49)	(–2.12)	(1.04)	(–0.15)
Control variables	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes	Yes
Observations	21,085	21,085	21,085	21,085	21,085
R^2 (%)	58.9	60.0	67.7	52.6	41.8

Panel B. Difference-in-differences by type of financial filing frequency

Dependent variable: Financial filing type frequency					
	Audited Annual Financial Statements	*Other Annual Financial Information*	*Interim Financial Information*	*Budget*	*Other Financial Information*
	(1)	(2)	(3)	(4)	(5)
Treated x *Post*	–0.032**	–0.023**	–0.007*	0.006	–0.001
	(–2.55)	(–2.31)	(–1.88)	(1.04)	(–0.24)
Control variables	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes	Yes
Observations	21,085	21,085	21,085	21,085	21,085
R^2 (%)	55.0	57.4	75.3	52.6	43.6

Table 4. Alternative measures of disclosure (cont'd)

Panel C. Difference-in-differences for alternative measures of municipal disclosure

Dependent variable:	*EventNotice* (1)	*EventNotice_Freq* (2)	*Timeliness* (3)	*Audited AnnualLength* (4)	*Other AnnualLength* (5)
Treated x *Post*	–0.111***	–0.104***	–24.959***	0.024	–0.023
	(–6.81)	(–5.91)	(–2.68)	(0.67)	(–0.36)
Control variables					
Issue	0.179***	0.171***	2.867	–0.016	–0.068
	(8.12)	(6.90)	(0.25)	(–0.50)	(–0.76)
AmountIssued	0.012*	0.038***	1.989	0.012	0.031
	(1.89)	(5.11)	(0.68)	(1.56)	(1.36)
%CallableBonds	–0.109***	–0.119***	–3.023	–0.025	–0.065
	(–7.29)	(–6.94)	(–0.42)	(–1.23)	(–1.27)
%GOBonds	0.062***	0.083***	–0.648	0.006	0.074
	(4.00)	(4.68)	(–0.08)	(0.28)	(1.18)
Control variables	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes	Yes
Observations	21,085	21,085	11,913	8,568	8,653
R^2 (%)	54.6	60.7	68.5	85.1	75.1

Table 5. Cross-sectional tests: The role of debtholders' demand for information

This table presents results from examining whether the difference-in-differences estimates of municipal financial reporting around Moody's ratings recalibration vary with the heterogeneity in investors' demand for financial information. We estimate the same specification as in column (6) of Table 3, Panels B and C, respectively, for separate subsamples of issuers with and without bonds traded on the secondary market. (*SecMktTrading* is a binary indicator variable equal to one for issuers of bonds that have been traded by investors on the secondary market in the year prior to the recalibration.) All variables are defined in Appendix A. For parsimony, we do not tabulate coefficients on main effects and control variables. (Full tables are presented in section IA.9 of the internet appendix.) *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Dependent variable:	*FinReporting*		*FinReporting_Freq*	
	SecMktTrading = 1	*SecMktTrading* = 0	*SecMktTrading* = 1	*SecMktTrading* = 0
	(1)	(2)	(3)	(4)
Treated x *Post*	–0.077***	–0.008	–0.073***	–0.010
	(–4.13)	(–0.28)	(–3.77)	(–0.43)
Difference	–0.069**		–0.063**	
	(–2.11)		(–2.03)	
Control variables	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes
Observations	12,365	8,720	12,365	8,720
R^2 (%)	56.8	71.8	53.2	65.5

Table 6. Cross-sectional tests: The role of underwriters

This table presents results from examining whether the difference-in-differences estimates of municipal financial reporting around Moody's ratings recalibration vary with differences in underwriter characteristics. In Panel A, we estimate the same specification as in column (6) of Table 3, Panels B and C, respectively, for separate subsamples of underwriter expertise, measured by *Specialist* in columns (1)–(4) and *Local* in columns (5)–(8). *Specialist* is a binary indicator variable equal to one if the underwriter's proportion of bond issuances that belong to the issuer's bond sector (GO or revenue bonds) in a reporting period is in the top quartile, and *Local* is a binary indicator variable equal to one if the issuer's underwriter is local, defined as operating in the two bottom terciles of the number of states in a given reporting period. In Panel B, we estimate the same specification as in Panel A, for separate subsamples of underwriter competition, measured by *MktShr* in columns (1)–(4) and *Herf* in columns (5)–(8). *MktShr* is a binary indicator variable equal to one if the underwriter's proportion of bond issuance in dollars in a given state-year exceeds the sample median, and *Herf* is a binary indicator variable equal to one if the underwriter's Herfindahl index in a given state-year exceeds the sample median, where the Herfindahl index is the sum of squared market share in a given state-year. For parsimony, we do not tabulate coefficients on main effects or control variables. (Full tables are presented in section IA.9 of the internet appendix). *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Panel A. Underwriter expertise

Dependent variable:	*FinReporting*		*FinReporting_Freq*	
Underwriter expertise:	*Specialist = 1*	*Specialist = 0*	*Specialist = 1*	*Specialist = 0*
	(1)	(2)	(3)	(4)
Treated x *Post*	–0.013	–0.086***	–0.005	–0.073***
	(–0.58)	(–4.61)	(–0.20)	(–4.07)
Difference	0.073**		0.068**	
	(2.57)		(2.36)	
Observations	7,886	13,199	7,886	13,199
R^2 (%)	74.4	70.7	68.8	64.7

Dependent variable:	*FinReporting*		*FinReporting_Freq*	
Underwriter expertise:	*Local = 1*	*Local = 0*	*Local = 1*	*Local = 0*
	(5)	(6)	(7)	(8)
Treated x *Post*	–0.027	–0.069***	–0.019	–0.091***
	(–1.53)	(–2.74)	(–1.12)	(–3.29)
Difference	0.042		0.072**	
	(1.36)		(2.23)	
Observations	14,431	6,654	14,431	6,654
R^2 (%)	70.6	75.0	63.9	69.3
Control variables	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes

Table 6. Cross-sectional tests: The role of underwriters (cont'd)

Panel B. Underwriter competition

Dependent variable:	*FinReporting*		*FinReporting_Freq*	
Underwriter competition:	*MktShr = 1*	*MktShr = 0*	*MktShr = 1*	*MktShr = 0*
	(1)	(2)	(3)	(4)
Treated x *Post*	–0.056***	–0.033*	–0.056***	–0.030
	(–2.72)	(–1.67)	(–2.62)	(–1.53)
Difference	–0.023		–0.026	
	(–0.78)		(–0.88)	
Observations	10,527	10,558	10,527	10,558
R^2 (%)	72.0	73.5	66.7	67.0

Dependent variable:	*FinReporting*		*FinReporting_Freq*	
Underwriter competition:	*Herf = 1*	*Herf = 0*	*Herf = 1*	*Herf = 0*
	(5)	(6)	(7)	(8)
Treated x *Post*	–0.046**	–0.060***	–0.069***	–0.039**
	(–2.12)	(–3.23)	(–3.12)	(–2.19)
Difference	0.014		–0.030	
	(0.51)		(–1.05)	
Observations	10,499	10,586	10,499	10,586
R^2 (%)	71.7	71.5	64.8	66.4
Control variables	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes

Table 7. Cross-sectional tests: The role of direct regulatory enforcement

This table presents results from examining whether the difference-in-differences estimates of municipal financial reporting around Moody's ratings recalibration vary with direct regulatory enforcement related to federal funding under the Single Audit Act. We estimate the same specification as in column (6) of Table 3, Panels B and C, separately for subsamples of issuers subject and not subject to the Single Audit Act. (*SingleAudit* is a binary indicator variable equal to one for municipalities subject to the Single Audit Act during our sample period.) All variables are as defined in Appendix A. For parsimony, we do not tabulate coefficients on main effects or control variables. (Full tables are presented in section IA.9 of the internet appendix.) *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Dependent variable:	*FinReporting*		*FinReporting_Freq*	
	SingleAudit = 1	*SingleAudit* = 0	*SingleAudit* = 1	*SingleAudit* = 0
	(1)	(2)	(3)	(4)
Treated x *Post*	0.000	–0.090***	–0.011	–0.072***
	(0.02)	(–5.23)	(–0.48)	(–4.18)
Difference	0.090***		0.061**	
	(3.24)		(2.13)	
Control variables	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes
Observations	7,380	13,705	7,380	13,705
R^2 (%)	73.4	67.7	66.0	61.7

Table 8. Information content channel

This table presents difference-in-differences estimates of regressions of municipal financial reporting around Moody's ratings recalibration. In Panel A, we use the same specifications as in column (6) of Table 3, Panels B and C, respectively, except that we replace *Treated* with *Treated_OnlyMoodys* (a binary indicator variable equal to one for issuers that experienced a rating upgrade from Moody's recalibration and issued exclusively Moody's rated bonds in the four years prior to the recalibration and zero otherwise) and *Treated_MoodysS&P* (a binary indicator variable equal to one for issuers that experienced a rating upgrade from the Moody's recalibration and issued exclusively bonds rated by both Moody's and S&P in the four years prior to the recalibration and zero otherwise). For parsimony, we do not tabulate coefficients on control variables. (Full tables are presented in section IA.9 of the internet appendix). In Panel B, we use the same specifications as in column (6) of Table 3, Panel B and C, respectively, except that we control for measures of information asymmetry. In columns (1) and (3), information asymmetry is *Volume*, measured as the natural logarithm of one plus the number of trades in a reporting period. In columns (2) and (4), information asymmetry is *PriceImpact*, measured as the average daily price impact in a reporting period, multiplied by one million. *MissingObs* is a binary indicator variable equal to one if the information asymmetry variable (*Volume* or *PriceImpact*) is missing in our sample. All other variables are as defined in Appendix A. *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Panel A. Single vs. dual-rated issuers

Dependent variable:	*FinReporting*	*FinReporting_Freq*
	(1)	(2)
Treated_OnlyMoodys x *Post*	–0.059***	–0.064***
	(–3.40)	(–3.83)
Treated_MoodysS&P x *Post*	–0.055***	–0.043***
	(–3.53)	(–2.59)
Difference	–0.004	–0.021
	(–0.22)	(–1.11)
Control variables	Yes	Yes
State x Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	21,085	21,085
R^2 (%)	69.0	62.3

Table 8. Information content channel (cont'd)

Panel B. Controlling for information asymmetry

Dependent variable:	*FinReporting*		*FinReporting_Freq*	
InfoAsymmetry =	*Volume*	*PriceImpact*	*Volume*	*PriceImpact*
	(1)	(2)	(3)	(4)
Treated x *Post*	–0.053***	–0.052***	–0.055***	–0.053***
	(–4.43)	(–4.23)	(–4.59)	(–4.33)
Control variables				
InfoAsymmetry	0.039***	0.029***	–0.003	–0.001
	(6.95)	(4.90)	(–1.14)	(–0.37)
MissingObs	–0.038***	–0.043***	–0.081***	–0.068***
	(–2.74)	(–3.27)	(–7.20)	(–6.21)
Issue	–0.005	0.018	–0.006	0.017
	(–0.29)	(0.91)	(–0.33)	(0.87)
AmountIssued	0.010**	0.014**	0.010**	0.014**
	(2.06)	(2.54)	(2.07)	(2.56)
%CallableBonds	–0.024**	–0.012	–0.024**	–0.013
	(–1.97)	(–0.94)	(–1.98)	(–0.97)
%GOBonds	0.027**	0.024*	0.028**	0.025*
	(2.11)	(1.68)	(2.20)	(1.76)
State x Year FE	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes
Observations	21,085	21,085	21,085	21,085
R^2 (%)	68.4	61.4	68.2	61.3

Table 9. Changes in investor base

This table presents difference-in-differences estimates of the percentage of retail and institutional investor trades around Moody's ratings recalibration. In column (1), we estimate the same specifications as in Table 3, Panel B, except that we replace our dependent variable with *PctRetail*, the percentage of retail trades by issuer-year, where a retail trade is defined as a trade not exceeding $100,000. In column (2), we estimate the same specifications as in Table 3, Panel B, except that we replace our dependent variable with *PctInstit*, the percentage of large institutional trades by issuer-year, where a large institutional trade is defined as a trade exceeding $250,000. Our sample requires the existence of trades on the secondary market in each issuer-year (sample of 11,650 observations). All other variables are as defined in Appendix A. *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two-tail), respectively.

Dependent variable:	*PctRetail*	*PctInstit*
	(1)	(2)
Treated x *Post*	–0.012	0.008
	(–1.32)	(1.28)
Control variables		
Issue	0.001	–0.004
	(0.08)	(–0.49)
AmountIssued	–0.006*	0.004*
	(–1.91)	(1.76)
%CallableBonds	0.017**	–0.006
	(2.15)	(–1.08)
%GOBonds	0.017**	–0.006
	(2.12)	(–1.14)
State x Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	11,650	11,650
R^2 (%)	59.5	56.6

Table 10. Trends around the recalibration

This table analyses trends around Moody's ratings recalibration. Panel A estimates the same specification as in column (6) of Table 3, Panels B and C, respectively, except that we interact *Treated* with each of the four reporting periods (*Post1–Post4*) beginning after the recalibration (July 1, 2010). Each reporting period runs from July 1–June 30. Our benchmark reporting period runs from July 1, 2009–June 30, 2010 = *Pre1*. For parsimony, we do not tabulate coefficients on control variables. (Full tables are presented in section IA.9 of the internet appendix.) Panel B, column (1) presents a differences-in-differences regression of bond yields (*Yield*) on binary indicators for the four post-recalibration reporting periods (*Post1–Post4*) and the three reporting periods preceding our benchmark period (*Pre4–Pre2*). We compute *Yield* as the secondary market trade-size-weighted annual average bond yield. We estimate this regression at the bond level (sample of 95,455 bond–year observations) and control for binary indicators for rating agency (*Moody* and *S&P*), a binary indicator variable for whether the bonds in our control group switched ratings from S&P to Moody's (*SwitchToMoodys*), state-year fixed effects, bond fixed effects, and bond maturity as well as logged bond amount interacted with year indicators (*Maturity* x *Year* and *Amount* x *Year*). Panel B, column (2) estimates the same specification as in column (1), using the rating of bond issuances in the primary market as our dependent variable (*CreditRating*), where ratings are coded from 0–9, with 9 being the highest rating (AAA). We estimate this regression at the issuer-year level (sample of 4,893 observations). *t*–statistics appear in parentheses and are based on standard errors clustered by issuer. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively.

Panel A: Difference-in-differences in financial reporting by reporting period

Dependent variable:	*FinReporting*	*FinReporting_Freq*
	(1)	(2)
Treated x *Post1*	–0.026*	–0.028*
	(–1.92)	(–1.96)
Treated x *Post2*	–0.046***	–0.038**
	(–2.99)	(–2.34)
Treated x *Post3*	–0.044***	–0.036**
	(–2.62)	(–2.07)
Treated x *Post4*	–0.116***	–0.110***
	(–6.25)	(–5.85)
Control variables	Yes	Yes
State x Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	21,085	21,085
R^2 (%)	69.1	62.4

Table 10. Trends around the recalibration (cont'd)

Panel B: Yield and credit rating trends around the recalibration

Dependent variable:	*Yield* (1)	*CreditRating* (2)
Treated x *Pre4*	0.051	0.054
	(0.72)	(0.44)
Treated x *Pre3*	0.072	–0.068
	(1.39)	(–0.98)
Treated x *Pre2*	–0.029	–0.012
	(–0.64)	(–0.30)
Treated x *Post1*	–0.102***	0.151***
	(–3.92)	(4.88)
Treated x *Post2*	–0.151***	0.129***
	(–5.04)	(4.10)
Treated x *Post3*	–0.148***	0.147***
	(–4.19)	(4.08)
Treated x *Post4*	–0.108**	0.103***
	(–2.57)	(2.73)
Control variables		
S&P	0.021	0.111**
	(0.74)	(2.55)
Moodys	–0.046	–0.028
	(–1.63)	(–0.74)
SwitchToMoodys	0.103	0.213
	(0.97)	(1.34)
State x Year FE	Yes	Yes
Underwriter FE	No	Yes
Issuer FE	No	Yes
Bond FE	Yes	No
Maturity x Year FE	Yes	No
Amount x Year FE	Yes	No
Observations	95,455	4,893
R^2 (%)	92.1	92.5