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 ratings affect debt issuers' disclosure decisions. Using the recalibration of Moody's municipal ratings scale in 2010 as an exogenous upgrade to credit ratings, we find that upgraded municipalities significantly reduced their disclosure of financial information relative to unaffected municipalities. Consistent with a reduction in debtholders' demand for information driving our results, we find that the reduction in disclosure is greater for issuers with debtholders that rely more on disclosure ex-ante. We also find a smaller reduction in disclosure for issuers with monitoring by local underwriters, and for issuers under direct regulatory oversight. Collectively, our results suggest that debtholders demand less information when they perceive credit risk to be lower, and that relying on market forces alone (e.g., debtholders and underwriters) may not achieve greater transparency in the municipal bond market as desired by regulators.

1. Introduction

A large literature in accounting examines the importance of credit ratings in providing information to investors, and particularly to debtholders.[1] Relatively less is understood about how credit ratings affect debt issuers' disclosure decisions.[2] In this paper, we provide novel evidence on this question by showing that higher credit ratings reduce debt issuers' disclosure due to a decrease in debtholders' demand for information. Unlike equity holders, debtholders' payoffs become less sensitive to new information about issuers' economic fundamentals as credit risk decreases (e.g., Easton, Monahan, and Vasvari [2009]). Consequently, we predict that an increase in credit ratings will reduce debtholders' demand for information, and issuers will respond to this lower demand by reducing the supply of disclosure. While theoretically intuitive, identifying this mechanism is empirically challenging because credit ratings are linked to debt issuers' economic fundamentals as well as equity holders' information demands, both of which affect issuers' disclosure decisions.

We identify how credit ratings affect disclosure through changes in debtholders' demand for information using the municipal bond market. This setting is advantageous for two reasons. First, the municipal bond setting is free from the influence of equity holders, which allows us to focus on the role of debtholders' demand for information. Second, we exploit the recalibration of Moody's municipal credit rating scale, which resulted in credit rating upgrades that lowered debtholders' *perception* of credit risk but were unrelated to changes in issuers' underlying fundamentals, including their actual credit risk. This setting enables us to identify the causal effect of credit rating upgrades on disclosure through changes in debtholders' demand for

[1] See, for example, Holthausen and Leftwich [1986], Hand et al. [1992], Goh and Ederington [1993], Hite and Warga [1997], Ederington and Goh [1998], Dichev and Piotroski [2001], Alp [2013], Cornaggia and Cornaggia [2013], and Cornaggia et al. [2018].

[2] For example, Beyer et al. [2010] state, "*Although much of the research to date has focused on equity valuation, it is an economic fact that the debt markets are a significant part of any developed economy. As such, research should consider the role of debt markets in the firm's information environment*" (p. 336).

information.

In April 2010, Moody's recalibrated their credit ratings to the Global Ratings Scale. Prior to the recalibration, municipal ratings were under the Municipal Ratings Scale and reflected only distance to distress. In 2010, Moody's incorporated loss given default into their municipal ratings, which led to ratings upgrades for over 18,000 municipal entities. The recalibration represented a scale change and did not result from changes in issuers' underlying credit risk and other economic fundamentals.[3] However, existing research documents that the ratings upgrade lowered investors' *perception* of issuers' credit risk because investors heavily rely on credit ratings when pricing municipal securities. Thus, investors in this market did not fully understand that the ratings change did not reflect changes in issuers' underlying credit risk.[4] This feature is important for our identification strategy, because the recalibration changed debtholders' demand for information while holding constant issuers' economic fundamentals that could be related to their other disclosure incentives.[5]

Using this shock to credit ratings, we examine whether rating upgrades alter municipalities' disclosure of financial information.[6] One feature of this market is that the SEC cannot directly require municipalities to disclose financial information, and there is significant variation in issuers' compliance with disclosure rules (e.g., nearly 40% of municipalities failed to file financial statements in 2009 (Schmitt [2011])). We use a difference–in–differences design to

[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).

[4] For example, see Adelino et al. [2017], Beatty et al. [2018], Cornaggia et al. [2018], and Feldstein and Fabozzi [2008].

[5] An increase in credit ratings that is related to a change in underlying fundamentals could also change issuers' incentives to supply disclosure independent of changes in debtholders' demand for information. For example, if an increase in credit ratings reflects increased performance, this could also affect issuers' incentives to disclose for a variety of reasons (e.g., issuers might prefer to disclose good news rather than bad news). In our setting, such changes in issuers' other incentives to disclose are held constant because the recalibration only affected debtholders' *perception* of credit risk, without affecting underlying fundamentals.

[6] We measure municipal disclosure broadly, using measures of both the existence and frequency of all municipal financial filings (including audited financial statements, unaudited annual financial and operating data, interim financial information, budgets, and other miscellaneous filings).

compare the financial reporting 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. S&P municipal ratings were always on the Global Ratings Scale, and arguably represent an ideal control group. Consistent with our prediction, we find that municipalities in our treatment group significantly reduced their disclosure of financial information after the recalibration relative to our control group.

We perform two cross-sectional tests to further corroborate whether a decrease in debtholders' demand for information is the driving force behind our results and to examine variation in this demand across investor types. First, we consider how the change in disclosure varies with investors' trading levels. We argue that investors are more likely to rely on financial information when they actively engage in trading than when they hold bonds to maturity. This implies that active investors are more likely to alter their demands for disclosure relative to buy-and-hold investors in response to ratings changes. Thus, following a ratings upgrade, active investors are likely to experience a larger decrease in their disclosure demands than buy-and-hold investors. Consistent with our prediction, we find that our results are stronger for municipalities with more frequently traded bonds.

Second, in light of our findings suggesting that active traders reduce their demand for information following a ratings upgrade, we consider variation in disclosure demand within active retail versus institutional traders. Unlike other fixed–income markets, retail investors dominate the municipals market because interest income from municipal securities is largely exempt from federal income taxes as well as state and local income taxes (SEC [2012]).[7] Given the high level of participation from retail investors in this market and their reliance on credit

[7] Retail investors account 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).

ratings, it raises the question whether and to what extent these investors demand disclosure. If retail investors have little demand for disclosure to begin with, then it is unlikely that ratings changes will significantly alter their demand for financial information. As a result, we should observe a smaller drop in disclosure for municipalities with more retail investors. However, we find that the drop in disclosure is similar across all quantiles of active retail investor trading. This suggests that retail investors' demand for information at least partially drives our results, consistent with recent evidence documenting that retail investors consume municipal disclosure (Cuny [2017]).

In light of the negative relation between credit ratings and disclosure and the wide variation in disclosure compliance across municipalities, we next examine the effect of enforcement in mitigating the decline in disclosure following ratings upgrades. A unique feature of the municipals market is that regulators lack direct oversight over issuers and enforce municipal disclosure by overseeing underwriters. To investigate the effectiveness of this indirect enforcement mechanism, we examine how changes in municipal disclosure following the recalibration vary with cross-sectional differences across underwriters. Consistent with local underwriters with closer ties to municipalities being more effective at enforcing disclosure than large national underwriters, we find that issuers with local underwriters decrease the disclosure of financial information significantly *less* following the recalibration.

We also examine instances where issuers' disclosure is subject to *direct* regulatory oversight. Specifically, municipal issuers that receive federal grants over $750,000 are subject to the Single Audit Act, which requires the provision of audited financial statements to the federal government. Consistent with direct regulatory oversight being effective, we find that federal grant recipients do not decrease disclosure following the recalibration. These results suggest that direct oversight is more effective in enforcing disclosure than indirect oversight through

alternative channels, such as underwriters.

We next investigate two additional channels that could be driving our results. First, Moody's recalibration could provide more information about issuers' credit quality, which would decrease investor demand for disclosure, leading municipalities to disclose less. We provide two tests to control for this potential channel. Our first test examines whether the recalibration had a differential effect on issuers of bonds that have a dual rating (by Moody's and S&P) compared to issuers rated solely by Moody's. Because S&P ratings already contain information about loss given default, any potential change in information content presumably would be greater for single-rated issuers than for dual-rated issuers with an S&P rating. Under this assumption, we expect our results to be stronger for single-rated issuers if the recalibration resulted in more informative ratings. However, we fail to find a differential effect between issuers of dual and single–rated bonds. Our second test directly controls for this channel by including measures of information asymmetry in our analysis, and our results remain robust. These findings make it unlikely that the information content channel is the primary driver of our results.

Second, we examine whether our results could be driven by a change in investor base. If certain types of investors demand more disclosure, a change in investor base following the recalibration could lead to changes in disclosure. We find that the recalibration did not 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 investors with differential disclosure demands.

Finally, we perform a battery of robustness tests to support the validity of our results. One specific concern is that the difference in the likelihood of disclosing financial information 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 had an immediate and persistent effect on disclosure, and that the treatment and control groups exhibit no

differential trend in their cost of capital and credit ratings prior to the recalibration.[8] Our results are also robust to using a control group matched on the level of government, bond yield, maturity, credit ratings, and the percentage of retail trades prior to the recalibration.

Our paper documents a novel mechanism linking entities' credit ratings to disclosure. Due to their nonlinear payoff function, a decrease in credit risk will reduce debtholders' demand for information, and issuers will respond to this lower demand by reducing the supply of disclosure. In contrast, the prevailing intuition in the literature linking cost of capital and disclosure does not consider debtholders' specific payoff function and is premised on settings with equity holders (e.g., Balakrishanan et al. [2014], Leuz and Schrand [2009]), who have fundamentally different payoff functions and information demands. For example, as credit risk decreases, equity holders' payoff may become more sensitive to new information about issuers' economic fundamentals (e.g., if lower credit risk increases perceived investment opportunities), which in turn could increase equity holders' demand and issuers' supply of disclosure.[9] Our study isolates the debtholder demand for information channel linking credit risk to disclosure in a market with exclusively debtholders.

Our findings contribute to the literature studying the effect of credit ratings on debt issuers' disclosure. While rating agencies are one of the central gatekeepers in debt markets, and a large literature finds a positive association between disclosure and credit ratings, [10] there is little evidence on how ratings influence borrowers' disclosure decisions. We find that credit ratings have a negative effect on disclosure. Existing literature studying the effect of credit

[8] Ideally, we would directly verify that there is no differential trend in municipal financial statement disclosure leading up to the recalibration. However, the municipal disclosure data becomes 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 cost of capital and credit ratings, two key observable dimensions of the information environment related to disclosure.

[9] Alternatively, a higher credit rating could lower equity holders' payoff uncertainty, leading to a lower demand for information, *ceteris paribus*. In contrast to debtholders, the effect of higher ratings on equity holders' demand for information in ambiguous.

[10] See, for example, Basu and Naughton [2018], Bonsall and Miller [2017], Bozanic and Kraft [2017], Cuny and Dube [2018], and Kraft [2015b].

ratings on disclosure typically uses the corporate bond market, which commingles equity and debtholders' demand for information, while our setting is free from equity holders. More importantly, these studies focus on channels that differ markedly from ours. For example, Basu et al. [2018] argue that credit rating upgrades lower managers' incentives to commit to disclosure in the event of future bad news. This channel focuses on how disclosure benefits the manager, and does not consider debtholders' demand for information. In addition, Sethuraman [2018] studies how changes in the information content of credit ratings (due to shocks to rating agencies' reputation) lead to changes in investors' demand for voluntary disclosure, while we examine changes in the level of credit ratings on debt issuers' disclosure.

Our findings are also of interest to the literature studying the determinants of disclosure in the municipal bond market. Municipal disclosure of financial information is notoriously sparse compared to the corporate setting, leading academics and regulators to emphasize the importance of disclosure reform, especially in light of growing public pension obligations and rising healthcare costs (e.g., Novy-Marx and Rauh [2011a], [2011b]).[11] These concerns call for a better understanding of the drivers of opacity in this market. Our study shows that debtholders reduce their demand for information following a ratings increase, suggesting that credit rating levels are an important determinant of opacity.

Finally, our paper speaks to disclosure regulation in the municipal bond market. We examine disclosure outcomes in a market where the SEC cannot directly mandate financial disclosure and aims to increase transparency through underwriters. We document that this indirect enforcement mechanism is not always effective and show that direct regulatory oversight by the federal government is more effective. Taken together, our results suggest that relying on

[11] Regulators have advocated for disclosure reform since the early 1900s (Zimmerman [1977]). Cornaggia et al. [2018] use the recalibration setting to show that bondholders mechanistically rely on ratings to assess credit risk in the municipals market. In their abstract, they suggest that improvements in disclosure might mitigate this issue: "*Our results commend improved disclosure to mitigate mechanistic reliance on ratings and inefficiencies due to rating standards that vary across asset classes.*"

market forces alone (e.g., debtholders and underwriters) is unlikely to achieve greater levels of disclosure as desired by regulators in this market.

2. Background on Municipal Disclosure

State and local governments issue municipal bonds to fund public projects such as roads, schools, sewer systems, and hospitals. A recent SEC report states that the municipals securities market is "critical to building and maintaining the infrastructure of our nation" (SEC [2012], p. i). Despite its importance, the municipal bond market is marked by a large degree of opacity and illiquidity (e.g., Baber and Gore [2008], SEC [2015]). This is a growing concern for regulators, who argue that this opaque information environment increases the difficulty of assessing the underlying risk of municipal entities and benefits sophisticated investors and broker–dealers at the expense of retail investors.[12] Recently, the SEC, MSRB, and FINRA have collectively proposed a number of initiatives aimed at improving the transparency and liquidity of the municipals market.[13]

In 1975, Congress established limited regulatory oversight for municipal securities, creating the Municipal Securities Rulemaking Board (MSRB) and the mandatory registration of municipal securities brokers and dealers (SEC [1994]). However, because state and local governments are guaranteed state sovereignty in the U.S. Constitution, municipal borrowers are exempt from the majority of federal regulations in the 1933 and 1934 Securities Acts. 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.

[12] Unlike other fixed–income markets, retail investors dominate the municipals market because interest income from municipal securities is largely exempt from federal income taxes as well as state and local income taxes (SEC [2012]). However, the state and local tax exemption is typically restricted to residents of the issuing municipality. As a result, the municipals market is less attractive to institutional investors (with low marginal tax rates) who cannot fully utilize the tax exemptions that are priced into the securities (SEC [2012], Schwert [2017]).

[13] 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 2014 the SEC launched the Municipalities Continuing Disclosure Cooperation (MCDC) Initiative.

Nevertheless, the SEC has aimed to regulate municipal issuers directly through anti–fraud provisions and indirectly through their oversight of underwriters, brokers, and dealers through Section 15(c)(2) 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 the primary offering. 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 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 provide certain annual financial and operating information on an ongoing basis, and to notify investors in the event of the occurrence of specific material events (similar to 8–K filings).[14,15]

Despite the SEC's disclosure requirement, compliance with the provision of annual financial information appears to be sparse. Consequently, there is significant variation in the availability, timeliness, and completeness of financial information provided by municipalities compared to other markets (Baber and Gore [2008], SEC [2015]).[16] For example, nearly 40% of municipalities failed to file financial statements in 2009 (Schmitt [2011]).

[14] Material event notices include events such as failure to pay an interest or principal payment and rating changes.

[15] Notably, 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.

[16] The SEC's most recent enforcement action aimed at increasing compliance with Rule 15c2–12 is the Municipalities Continuing Disclosure Cooperation (MCDC) Initiative of 2014. This initiative provides issuers and underwriters with the opportunity to self-report errors in their bond offering documents regarding prior compliance with the continuing disclosure laws. This initiative (which occurred after our sample period) has increased the number of enforcement actions against underwriters and issuers.

3. Hypothesis Development

3.1. The effect of credit rating upgrades on municipal disclosure of financial information

As illustrated in Figure 1, debtholders' payoff function is nonlinear. When credit risk is lower, debtholders' payoff is less sensitive to information about fundamentals, and they benefit less from disclosure. Given this payoff function, as higher credit ratings signal lower credit risk, debtholders' demand for information declines. Debt issuers respond to this lower demand by reducing their supply of disclosure—especially if disclosure is costly. In the municipal bond setting, producing financial statements is particularly costly because issuers must convert their financial data reported using fund accounting into a statement of position using a different accounting method (such as U.S. GAAP). As municipalities are not subject to typical SEC disclosure requirements (such as SOX 404), municipal issuers are likely to have fewer systems and procedures in place to produce another set of financial statements.

It is important to note that our prediction is specific to debtholders. Equity holders have a different payoff function, which does not level out as credit risk decreases. As credit risk drops, their demand for information could either (a) remain unchanged, if the sensitivity of their payoff to underlying fundamentals remains unchanged; (b) increase, for example if lower credit risk increases perceived investment opportunities or better news, or (c) decrease, if lower credit risk leads to less uncertainty about expected future payoff. At a minimum, the presence of equity holders and the difference between equity and debtholders' payoff functions can confound the link between changes in credit ratings and disclosure. This is not an issue in the municipal bond market, because municipalities do not issue public equity. In contrast to studies that examine how shocks to the cost of capital are related to disclosure in the corporate setting, our economic mechanism is distinct in the sense that it focuses on changes in debtholders' information demands due to their specific payoff function.

3.2 Cross-Sectional Tests

3.2.1 The role of debtholders' demand for information

We perform two cross-sectional tests to further corroborate whether a decrease in debtholders' demand for information is the mechanism driving lower disclosure following a ratings upgrade and how this demand varies across investor types. In our first test, we examine variation in debtholders' demand for information based on their trading behavior. We argue that issuers of bonds that are infrequently traded on the secondary market are likely to experience smaller changes in their investors' demand for information following a ratings upgrade, because these investors likely adopt a buy-and-hold strategy and are less sensitive to information about short-term changes in issuers' economic fundamentals. In contrast, investors that actively trade municipal bonds are likely to rely on financial information for trading purposes and alter their demand for information following ratings changes. As a result, we predict that issuers of more frequently traded (i.e., more liquid) bonds are more likely to reduce disclosure following a rating upgrade.

Note that the extent to which we observe a larger reduction in disclosure from issuers of more liquid bonds also depends on whether active trading itself provides information to the market. If the information from informed investors is quickly impounded into the bond price, investors may rely less on financial disclosure. Finding that issuers with more liquid bonds reduce disclosure less would support this prediction. However, we also note that 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 [2017]).

In our second test, we examine variation in the amount of active trading performed by retail versus institutional investors. A distinguishing feature of our setting is that the municipal

bond market is dominated by retail investors. Given that retail investors are generally less sophisticated than institutional investors, they may have little demand for financial disclosure due to differences in their ability to process financial information. In this case, the decline in financial disclosure is likely to be driven by changes in institutional investors' information demands.[17]

However, some recent studies find evidence consistent with retail investors demanding municipal disclosure, despite their heavy reliance on ratings. The MSRB created the Electronic Municipal Market Access website (EMMA) to facilitate retail investors' access to information about firm fundamentals, and consistent with this notion, Cuny [2017] finds that this access to financial information allows retail investors to negotiate better prices with dealers. In addition, investors are likely to interpret disclosure as a positive signal, without necessarily processing its content. In other words, retail investors' demand for disclosure may simply stem from the information contained in the *choice* to disclose, consistent with the view that failing to disclose conveys bad news regarding issuers' fundamentals.[18]

In addition, retail investors may demand financial disclosure indirectly through other information channels. Recent evidence suggests that retail investors consider a variety of information sources when making their investment decisions, including press coverage and information provided by their broker-dealers (e.g., Cuny [2017]). These other sources of

[17] For example, Cornaggia et al. [2018] conclude that investors' response to Moody's rating recalibration is primarily driven by retail investors. However, Adelino et al. [2017] suggest that institutional investors also rely on ratings (i.e., due to ratings-based regulations, such as official regulations provided by Basel II and the National Association of Insurance Commissioners (NAIC), and/or internal investment management practices that restrict portfolio holdings based on ratings), and contributed to the drop in yields following the recalibration. Investment practices based on ratings are not uncommon among institutional investors (e.g., Chen et al. [2014] find that bonds that exogenously become investable by asset-class-sensitive institutional investors benefit from valuation changes). This suggests that institutional debtholders may also mechanistically interpret the recalibrated ratings as evidence of lower credit risk and decrease their demand for municipal disclosure following the recalibration.

[18] This alternative explanation for retail investors' demand for disclosure is consistent with investors screening on the presence of disclosure in their investment decisions. This explanation is appealing given that retail investors typically buy and sell municipal bonds through a broker-dealer due to the decentralized nature of this market, and broker-dealers are likely to screen investment opportunities for their retail clients based on compliance with financial disclosure regulations.

information are more prevalent when municipalities disclose financial information, thereby creating an indirect demand for information from retail investors.

To test whether retail investors affect the change in demand for disclosure in our setting, we consider variation in our sample based on the percentage of active trading performed by retail versus institutional investors. If retail investors have a lower demand for financial information than institutional investors, then we should observe a smaller drop in disclosure for municipalities with a greater proportion of active retail trades following a ratings upgrade. In other words, if retail traders do not process financial disclosures at all, then we expect little variation in their demand as a result of the recalibration.

3.2.2 The role of municipal disclosure enforcement

In this section, we explore the role of underwriters and direct regulatory oversight in enforcing municipal disclosure. Underwriters are responsible for enforcing disclosure at the time of and subsequent to bond issuance. In fact, the SEC almost exclusively relies on underwriters to enforce municipal disclosure and has no direct enforcement power, except in the case of securities fraud. However, underwriters maximize their own profits, which may affect their decision to enforce disclosure compliance. For example, in a recent initiative to increase municipal disclosure compliance (the MCDC initiative of 2014), the SEC charged 72 underwriters representing 96% of the securities traded in the municipal market, suggesting that many non-compliance cases involve national underwriters.[19] Therefore, it is interesting to consider cross-sectional differences in underwriter characteristics that may predict their effectiveness in enforcing municipal disclosure, particularly in light of our earlier prediction that debt issuers reduce their disclosure following a ratings upgrade.

[19] The SEC charged and penalized 72 underwriters 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]).

The municipal bond underwriting business consists of a few national and many local underwriters. Prior literature finds that local underwriters in this market have a better understanding of local economic conditions and are considered more reputable (Butler [2008]). The geographic proximity of local underwriters may reduce the information asymmetry related to issuers' previous compliance with disclosure laws and improve the ability of local underwriters to perform their due diligence relative to national underwriters. This would suggest that local underwriters are more effective in enforcing disclosure. On the other hand, Daniels and Vijayakumar [2007] find that municipal bond issues managed by large underwriters have lower bond yields, suggesting that national underwriters may, in fact, be more reputable and may be more effective in enforcing disclosure. Further, national underwriters are likely to have more resources, experience, and human capital to perform their due diligence in addition to the incentive to protect their national reputation.

We also investigate the role of direct regulatory oversight in enforcing municipal disclosure. Municipal entities that receive federal funding in excess of $750,000 are subject to the Single Audit Act, requiring them to provide audited financial statements to the federal government. Contrary to the SEC, which cannot directly impose costs of non-compliance on the issuer, the federal government can withdraw funding in the event of non-compliance, and violations of the Single Audit Act may have legal consequences for the entities seeking federal funding. We test whether issuers under direct regulatory oversight through the Single Audit Act are less likely to drop disclosure following the recalibration.

4. Empirical Setting: Moody's Ratings Recalibration

To estimate the effect of credit ratings on bond issuers' disclosure, 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 ratings that is plausibly

exogenous to changes in issuers' fundamentals using Moody's 2010 recalibration of their municipal ratings scale.

Prior to the recalibration, Moody's employed a dual ratings system. Moody's rated municipalities according to a different methodology compared to other fixed income classes such as corporate bonds, financial institutions, and sovereign entities. Under the dual system, municipal ratings (i.e., the Municipal Ratings Scale) reflected only distance to distress, while other fixed income ratings (i.e., the Global Ratings Scale) reflected expected loss (equal to distance to distress multiplied by loss given default). Over a four–week period from April 16 to May 7, 2010, Moody's eliminated the dual rating system and incorporated loss given default into their municipal ratings, thereby aligning their ratings across all fixed income sectors.[20] Because municipal securities are often guaranteed by state and local tax revenue, loss given default on municipal bonds is very low on average. Incorporating loss given default into the new municipal ratings thereby led to ratings upgrades for over 18,000 municipal entities, representing over $2.2 trillion of municipal debt.[21]

The key feature of this event for our study is that the recalibration did not reflect changes in debt issuers' underlying credit risk and economic fundamentals, but merely a change in the ratings scale (Moody's [2010]).[22] 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

[20] The Dodd-Frank Bill in July 2010 mandated consistent ratings across asset classes, prompting Moody's to recalibrate their ratings scale.

[21] See the transition matrix in Table 1 of Moody's [2010]. 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.

[22] Moody's [2010] states, "*Market participants should not view the recalibration of municipal ratings as rating upgrades, but rather as a recalibration of the ratings to a different rating scale*" (p.1).

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.

Although the rating upgrades were uncorrelated with economic fundamentals, the collective evidence from prior studies suggests that municipal investors nonetheless perceived the ratings upgrades as reductions in credit risk. For example, Cornaggia et al. [2018] document that credit spreads on upgraded bonds decreased between 19 and 33 basis points compared to non–upgraded bonds.[23] This feature allows us to pin down how credit ratings affect disclosure through the channel of debtholders' demand for information, because the only change that occurs is debtholders' *perception* of credit risk.[24]

Another key feature of the recalibration for our tests is that it did not affect all issuers. S&P ratings were already calibrated to the Global Rating Scale and did not go through a recalibration. Thus, we use S&P-rated issuers as our control group in our analyses.[25]

5. Sample Selection and Variable Measurement

5.1 Sample

Table 1 describes our sample selection process. To construct our sample, 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–year window prior to

[23] We replicate this result in Table 9, Panel B, to validate that it holds in our sample

[24] Several prior studies utilize the recalibration setting. Cornaggia et al. [2018] use the recalibration setting to show that bondholders mechanistically rely on ratings to assess credit risk, and we view our paper as a natural and important extension of their study. Their results recommend improved disclosure to mitigate investors' reliance on credit ratings; however, we predict that this reliance on ratings actually contributes to *lower* levels of disclosure. 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 capital resulting from investor reliance on ratings. Finally, Beatty et al. [2018] 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.

[25] A potential concern is that issuers rated by S&P and Moody's may differ in economic characteristics that are related to disclosure. We perform several robustness tests in Section 8 to alleviate this concern.

Moody's recalibration (following Adelino et al. [2017]). Our sample period begins July 1, 2009 (when disclosure filings become available on MSRB) and ends June 30, 2014 (sample of 90,915 unique issuer-year observations).[26] 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 every municipal bond issue that had a "Change in Scale" on April 16, April 23, May 1, or May 7, 2010 from Mergent. 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).[27] 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.

To construct our state–level control variables, we require unemployment rates from the Bureau of Labor Statistics, Gross State Product and State Per Capita income from the Bureau of Economic Analysis, and the Housing Price Index from the Federal Housing Finance Agency. Finally, we use MSRB to obtain data on secondary market trading, and municipal disclosures through the Electronic Municipal Market Access system (EMMA).

Our final sample consists of 21,085 issuer–year observations (4,217 unique municipalities). The treatment group is comprised of 9,725 observations (1,945 unique municipalities) that experienced a rating upgrade from Moody's recalibration, and a control group of 11,360 observations (2,272 municipalities) that were not subject to Moody's

[26] 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.

[27] 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 health–care sectors) (e.g., Adelino et al. [2017]).

recalibration. 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.[28]

5.2 Measurement of municipal disclosure

Because the data on municipal disclosure filings begins in 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 municipalities' financial reporting prior to Moody's recalibration. Although this period includes the four–week recalibration event at the end (April 16, 2010 – May 7, 2010), municipalities in our sample require on average 188 days to issue financial reports after the beginning of their fiscal year (for example, a report issued in May 2010 typically covers the fiscal year starting December 2008).[29] As Moody's announced its recalibration in March 2010, it is unlikely that any effects on financial reporting would manifest prior to June 30, 2010.[30]

To construct our municipal disclosure variables, we obtain all municipal filings on EMMA for our sample period. Table 1, Panel B presents the distribution of municipal filings in our sample. The municipal filings in EMMA include specific filings of financial information according to Rule 15c2-12, along with a variety of other filings. Municipalities are required to file either audited financial statements (or 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). In addition, issuers are required to provide certain notices upon the realization of specific events, such a failure to provide required annual

[28] 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 lower amounts of debt. These differences are potentially relevant considerations when generalizing our results to the entire municipals market.

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

[30] Note that 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.

financial information, a notice of information provided to rating agencies, a notice of change in fiscal year, or a notice of change in accounting standards. Finally, issuers are also required to provide notices of any material events (MSRB [2014]).

To create a broad measure of financial disclosure, we use both the existence and frequency of all financial information filed with MSRB, including: (1) 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,[31] 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 given year, and *FinReporting_Freq* as the natural logarithm of the number of these filings. We also create existence and frequency variables for each individual measure of disclosure.[32]

5.3 Control variables

Our tests also include several control variables that may 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

[31] 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.

[32] As described above, municipalities also provide a number of event-specific filings on EMMA, such as notices of certain material events (e.g., the failure to pay an interest or principal payment and rating changes). We do not include such filings in our measure as they are typically provided conditional on the realization of a specific event.

covenant terms) (e.g., Banko and Zhou [2010], Green [2017]). Third, we compute the percentage of general obligation bonds (GO bonds) issued (*%GOBonds*). GO bonds are typically issued by lower risk municipalities with better quality collateral, which is likely to be correlated with disclosure behavior.

We also include several control variables to capture state–level economic characteristics that are typically correlated with disclosure. Specifically, we include Gross State Product (*GSP*), Per–Capita Income (*PCI*), unemployment rate (*Unemployment*), and the Housing Price Index (*HPI*) (a measure of state property tax revenues). We measure each variable by computing the state–year average. We winsorize all continuous variables at the top and bottom 1% levels.

6. Research Design and Results

6.1 Research design

We use 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:[33]

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

where our dependent variable is either *FinReporting*, a binary indicator variable equal to one if the municipality files financial information with MSRB, or *FinReporting_Freq*, the natural logarithm of the number of financial filings in a given year. *Treated* is an indicator variable equal to one for municipalities that experienced a rating upgrade from Moody's recalibration, and zero for municipalities in our control group, which issued bonds rated only by S&P in the four years prior to the recalibration. *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 5.3,

[33] 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]).

which we lag by one year relative to the disclosure of financial statements. In addition to these control variables, we also include various levels of fixed effects in our model. First, we include year fixed effects, allowing the common change in filing financial information to vary by year (for example, changes in regulatory enforcement). Note that year fixed effects absorb the main effect on *Post* in our estimations. A potential concern is that financial disclosure is influenced at the state level, or may only reflect the characteristics of certain sectors and levels of government. If these issuers were also more likely to be recalibrated, then our results could be driven by these issuer characteristics. To address this concern, we include state, 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 state, 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.

6.2 Descriptive statistics

We provide descriptive statistics in Table 2 and discuss some variables of interest below. As noted by regulators and existing literature, municipal issuers often fail to disclose financial information to market participants (Baber and Gore [2008], Schmitt [2011], SEC [2015]). Our sample statistics verify this phenomenon. The mean of *FinReporting* is 0.632, indicating that only 63% of issuers provide financial statements to MSRB on average. About 46% of the observations in our sample receive a Moody's upgrade. About 32% of municipalities in our

sample issue bonds, averaging about $6.92 million over a given year. Of the bonds issued during our sample period, approximately 22% are callable and 23% are general obligation (GO) bonds. With respect to the economic control variables, annual Gross State Product is $520 billion on average. The average Per Capita Income and Housing Price Index (calculated relative to a benchmark of 100 beginning in 1991) is $40,040 and 192, respectively. Finally, unemployment is approximately 8% on average.

Figure 2 provides the geographic distribution of disclosure per state. The figure depicts the average of *FinReporting* for each state across our sample period of July 1, 2009 to June 30, 2014. The graph demonstrates significant heterogeneity in the frequency of 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 financial information is common across the U.S. municipal market.

6.3 Main results

Table 3 presents our results. Panel A presents results from univariate difference–in–differences regressions (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 ratings reduce municipal financial reporting, the difference–in–differences coefficient (β_1) is negative and 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 17.34 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, we expect that issuers of Moody's rated bonds, which have a lower rating prior to the recalibration, have marginally greater benefits of disclosure relative to issuers of bonds 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).[34] 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 multivariate difference–in–differences regressions with various levels of fixed effects, using *FinReporting* (*FinReporting_Freq*) as our dependent variable. We include year fixed effects in all regressions. We then sequentially add state 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) – (5) (coefficients between –0.069 and –0.070 in Panel A), with a slight drop in column (6) after including issuer fixed effects (–0.058 in Panel A). 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 year fixed effects absorb the common increasing

[34] 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 effect on disclosure is also sizable. The difference-in-differences coefficient in column (6) of Panel B indicates a 5.8% 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 9.2% 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 5.2% following a ratings upgrade relative to that of control issuers that did not experience a ratings upgrade.

Having obtained our main difference-in-differences estimates, we next explore which specific types of municipal filings drive our results. In Table 4, we present difference-in-differences results for each individual type of financial disclosure. The specifications mirror those in columns (6) of Table 3, Panels B and C, respectively, except that we replace our dependent variables, in turn, by each specific type of municipal disclosure. Panel A presents results from regressions using binary indicators equal to one if the issuer provided each respective disclosure type in a given year. Panel B presents results from estimates using the natural logarithm of one plus the number of each respective disclosure type provided by the issuer in a given year.

Both 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 in columns (1) – (3) in both panels). Columns (1) – (3) of Table 4, Panel A indicate that, following a ratings upgrade, issuers are 3.3% less likely to file audited financial statements, 2.4% less likely to file other annual financial information, and 0.4% less likely to file interim financial

information, respectively, relative to control issuers that did not experience a ratings upgrade. There does not seem to be any significant variation in the provision of budgets or other financial information after the recalibration, perhaps due to the scarce nature of such filings. Panel B documents similar results using detailed measures of the frequency of financial information filings as the dependent variables.

6.4 Cross–sectional tests

In this section, we describe the results from our cross–sectional predictions. Our first set of tests investigates how the effect of a rating upgrade on municipal disclosure varies with the heterogeneity in investors' demand for information. First, we examine variation in the demand for disclosure proxied by investors' trading activity. Specifically, we use the same specification as in column (6) of Table 3, Panels B and C, respectively, except that we add *Treated* x *Post* x *Liquidity* and *Post* x *Liquidity* to our regression, where *Liquidity* is a binary indicator variable equal to one for issuers of bonds that have been traded by investors on the secondary market in the four years prior to the recalibration.[35] If active investors have a greater demand for financial information, then we expect the effect of a rating upgrade on disclosure to be greater for issuers of more liquid bonds.

Second, we examine variation in levels of retail investor trading, within the sample of issuers with bonds that have been traded by investors on the secondary market (15,125 observations). We estimate the same specification as in column (6) of Table 3, Panels B and C, respectively, except that we add interactions of *Treated* x *Post* and *Post* with binary indicators for each quartile of the *percentage* of retail trades (*PctRetailQ1 – PctRetailQ4*) in 2009, where a retail trade is defined as a trade not exceeding $100,000, following prior literature (e.g., Edwards et al. [2007], Cuny [2017]). If retail investors have a lower demand for financial information,

[35] Following Green et al. [2006], we define secondary market trades as trades that occur 30 days after bond issuance.

then we expect a smaller drop in disclosure following the recalibration for issuers with higher levels of retail trades.

Our results appear in Table 5. Consistent with our expectation, Panel A shows a negative and strongly significant coefficient on *Treated* x *Post* x *Liquidity*, suggesting that issuers with more active investors are significantly more likely to drop disclosure following a rating upgrade. Notably, the coefficient on *Treated* x *Post* is not significantly different from zero, indicating that the effect of credit rating upgrades on the likelihood of disclosing financial statements is concentrated in issuers of liquid bonds, whose investors are most likely to rely on financial statements prior to the recalibration and reduce their demand for information after a credit rating upgrade. Within issuers of liquid bonds, the probability of filing financial information following a ratings upgrade is 7.4% lower relative to the control issuers that did not experience a ratings upgrade (*Treated* x *Post* x *Liquidity* + *Treated* x *Post* = -0.074, t-stat = -5.09), which is larger than the magnitude reported in our main results (5.8%, as reported in column (6) of Table 3, Panel B).

Panel B shows a negative and significant coefficient on *Treated* x *Post* for the lowest quartile of retail trades, and an incrementally insignificant coefficient for each subsequent quartile (i.e., the coefficients on *Treated* x *Post* x *PctRetailQ2* through *Treated* x *Post* x *PctRetailQ3* are not significantly different from zero). This suggests a significant post-recalibration drop in municipal financial information in each quartile of the distribution of the percentage of retail trades, consistent with a demand for disclosure from both active retail and active institutional investors.

Next, we perform two sets of cross-sectional tests to investigate how the effect of a rating upgrade on municipal disclosure varies with enforcement. First, we examine the effect of national versus local underwriters. We define an underwriter as “national” when the number of

states in which it operates is in the highest tercile of the distribution (30 states) in a given year. We estimate the same regressions as in column (6) of Table 3, Panels B and C, except that we add *Treated* x *Post* x *NationalUnd*, *Treated* x *NationalUnd*, *Post* x *NationalUnd* and *NationalUnd* to the regression, where *NationalUnd* is a binary indicator variable equal to one for municipalities that issued bonds with a national underwriter.

Second, we examine the effect of direct regulatory oversight. Issuers that receive federal grants exceeding $750,000 are subject to the Single Audit Act and are required to provide audited financial statements. We estimate the same regressions as in column (6) of Table 3, Panels B and C, except that we add *Treated* x *Post* x *SingleAudit* and *Post* x *SingleAudit* to the regression, where *SingleAudit* is a binary indicator variable equal to one for municipalities subject to direct regulatory enforcement under the Single Audit Act during our sample period.

Our results appear in Table 6. Panel A shows a negative and significant coefficient on *Treated* x *Post* x *NationalUnd*, suggesting that issuers with national underwriters decrease disclosure relatively more subsequent to ratings upgrades compared to issuers with local underwriters, who tend to have a closer connection to municipalities (Butler [2008]). This result suggests that local underwriters are better enforcers of municipal disclosure than national underwriters. All else equal, the difference-in-differences estimate for national underwriters is 4.7% larger (that is, more negative) relative to that for local underwriters (*Treated* x *Post* x *NationalUnd* = -0.047, t-stat = -1.74). However, neither type of underwriter is fully effective in enforcing disclosure, as evidenced by the significant negative difference-in-differences coefficient (*Treated* x *Post* = -0.040, t-stat = -2.64).

Panel B shows a positive and significant coefficient on *Treated* x *Post* x *SingleAudit*, indicating that treated issuers under direct regulatory oversight decrease disclosure significantly less following a ratings upgrade. In fact, these issuers experience no change in disclosure

following the ratings recalibration (*Treated* x *Post* x *SingleAudit* + *Treated* x *Post* = -0.012, *t*-stat = -0.60) relative to issuers in the control group also under direct regulatory oversight. Collectively, the results in Table 6 suggest that, while the effectiveness of indirect regulatory enforcement varies across underwriters, direct regulatory disclosure enforcement seems to be effective in preventing a decline in disclosure.

7. Additional channels

7.1 The informativeness of recalibrated credit ratings

A decline in financial reporting is also consistent with the alternative channel that the *information content* of the ratings increased as a result of the recalibration. More informative credit ratings could lead to a drop in investor demand for disclosure, providing an alternative explanation for our results. This section investigates this channel.

On 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. Moody's used a simple algorithm based on the pre–period rating and bond type to convert all municipal ratings over a four–week period, and the new component of the rating (loss given default) was publicly available prior to the recalibration event. Moreover, the recalibration resulted in an upgrade to the top three notches of the ratings classifications for most municipal bonds (Moody's [2010]). As a result, the recalibrated ratings are less granular, which makes it more difficult to assess default risk within a given rating. Thus, it is possible that the recalibration resulted in less informative ratings, at least for certain credit rating users.[36]

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

[36] For further discussion, see Beatty et al. [2018].

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 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. Note that 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).

Our results appear in Table 7, Panel A. We estimate the same specifications as in column (6) of Table 3, Panels B and C. In columns (1) and (3), our treatment group includes municipalities that issued bonds only rated by Moody's in the four years prior to the recalibration, and in columns (2) and (4), our treatment group includes municipalities that issued bonds rated by both Moody's and S&P in the four years prior to the recalibration. Consistent with our results in Table 3, in both samples, upgraded issuers are significantly less likely to file financial statements after the recalibration. Importantly, the difference between coefficients 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 the information content channel. 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, which changes investors' 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.[37]

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. Table 7, Panel B shows that our findings are robust to controlling for both of these measures of information asymmetry.[38] Overall, these tests suggest that the information content channel is not the primary driver of our results.

7.2 Changes in investor base

The municipal market remains dominated by direct household investment, where 44% of the market represents direct retail investment at the end of 2016 (SIFMA [2016]). Taken together with indirect investment through mutual funds, money market funds, and ETFs, 67% of municipal bonds are ultimately held 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 banking institutions and insurance companies increased proportionately.

In this section, we examine whether our results could be driven by a change in investor base. It is possible that the recalibration led to a change in the municipal investor base, and the new investor base has lower demands for financial disclosure. Thus, our observed decline in financial disclosure may be partially driven by changes in the mix of municipal investor types.

[37] Note that higher credit ratings can signal (a) lower uncertainty about issuers' future cash flow or (b) greater expected future cash flow. Thus, controlling for information asymmetry also suggests that channel (a) is the sole driver of our results.

[38] Note that this test is similar in spirit to Cornaggia et al.'s [2018] tests of changes in liquidity surrounding the recalibration. They find a temporary (90 day) increase in liquidity following the recalibration, and conclude "*If investor perception of bonds' credit quality increases when ratings are upgraded, then the price changes we observe could reflect lower liquidity premiums. A consequence of this hypothesis is that upgraded bonds should experience permanent increases in liquidity. This is not what we find. Although upgraded bonds' trading volume increases immediately after the recalibration relative to non-upgraded bonds, this increase is transitory.*" (p.2041)

Although our cross-sectional test in Table 5 Panel B does not find evidence of differential preferences for disclosure between retail versus institutional investors, , the possibility remains that investor mix can shift for other reasons, which may simultaneously affect disclosure.

We examine this possibility in Table 8 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 *PctInstitutional*, the percentage of institutional trades by issuer–year, where an institutional trade is defined as a trade exceeding $250,000.[39] Our sample requires the existence of trades on the secondary market over our sample period (sample of 14,939 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 investors.

8. Robustness tests

8.1 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 test the validity of this assumption.

First, we examine the difference in the likelihood of disclosing financial statements between our treatment and control groups in each reporting period after the recalibration, relative

[39] We follow prior literature and use market convention to distinguish between retail and institutional trading (e.g., Edwards et al. [2007], Cuny [2017]).

to the immediately preceding period. A potential concern is that our results may begin to manifest several years after the recalibration, which would suggest that they are not attributable to the recalibration event. Another potential concern is that our results are driven by temporary changes in liquidity around the recalibration event, as documented Cornaggia et al. [2018]. Observing trends in financial reporting after the recalibration can help alleviate these concerns.

We present our results in Table 9, Panel A. Specifically, 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 an immediate and 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.

Second, we examine the extent to which the treatment and control groups follow differential trends in their underlying economics. A drawback of our empirical setting is that the disclosure data begins one year prior to the recalibration. Ideally, we would verify that there is no differential trend in municipal financial reporting 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 financial reporting and thus in their financial reporting practices. Second, these tests allow us to corroborate whether municipalities that received a ratings upgrade experience lower cost of capital and a

"mechanical" increase in credit ratings resulting from the recalibration.

Our results appear in Table 9, Panel B. Column (1) estimates difference–in–differences in bond yields (*Yield*) in the four post–recalibration reporting periods (*Post1 – Post4*) and in the reporting period spanning July 1, 2008 – June 30, 2009 (*Pre1*), relative to our benchmark period (July 1, 2009 – June 30, 2010). We estimate this regression at the bond level, resulting in 152,528 observations. We compute *Yield* using the annual average bond yield weighted by trade size in the secondary market (i.e., trades occurring 30 days after the bond issuance). 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 year fixed effects, bond fixed effects (subsuming issuer fixed effects), and the natural logarithm of bond maturity interacted with year indicators (*BondMaturity*Year*) in the model. 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 bond issuance level and retain issuers that have at least one bond issuance before and one bond issuance after the recalibration to allow for a pre–post comparison, resulting in 3,581 observations. We also include year fixed effects and issuer fixed effects in the model. Consistent with a sharp increase in the credit ratings for upgraded issuers after the recalibration, we find that all *Treated* x *Post* coefficients are significantly positive. Importantly, in both columns, *Treated* x *Pre1* is 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 3 provides a visual representation of our three trend analyses.

8.2 Matching

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 could result in violations of the parallel trends assumption. For example, if issuers choose rating agencies for strategic reasons (e.g., they engage in ratings shopping), issuers with S&P ratings may differ from those with Moody's ratings. Our trends analysis described above helps alleviate this concern. Nevertheless, we further address this concern by matching treated issuers to control issuers based on a set of pre–recalibration covariates.

Beginning with our sample of municipalities (21,085 observations), we form one–to–one matched pairs by matching on issuer characteristics prior to Moody's recalibration (using our benchmark reporting period starting in 2009) using nearest neighbor matching. We match issuers on level of government (e.g., city, county, district, etc.), the secondary market trade–size–weighted average bond yield by issuer (*Yield*), the trade–size–weighted average bond maturity by issuer (*Maturity*), the average bond issuance credit–rating by issuer (*CreditRating*), and the percentage of retail trades (*PctRetail*).[40] Each treated issuer is matched to the single best–matched control issuer, with replacement, and unmatched treated issuers are dropped from the sample. This process results in an equal number of 8,395 treated and control observations.

Table 10 reports our results. In Panel A, we present the covariate balance between our treated and control observations in the year prior to Moody's recalibration (2009). Differences in means and medians are equal to zero for all covariates, suggesting that we have a good match between our treatment and control groups. In Panel B we present results from estimating our difference–in–differences design. We estimate the same specifications as in column (6) of Table

[40] Our results are also robust to matching on our other issuer-specific control variables, including debt issued, the percentage of callable bonds issued, and the percentage of general obligation bonds issued.

3, Panels B and C, respectively, except that we use our matched sample of treatment and control observations. We obtain negative and significant difference–in–differences coefficients across all of our specifications, indicating that our results are robust to this matched sample design.

8.3 Additional robustness tests

We perform several additional robustness tests, beginning with a falsification test on insured bonds. When issuers purchase bond insurance, the insurance company (e.g., Ambac) agrees to pay the remaining principal and interest payments to investors in the event of default. For this reason, the rating on insured bonds reflects the rating of the insurance company rather than the credit rating of the underlying municipality. Accordingly, issuers of insured bonds should be unaffected by the recalibration, and we expect to find no change in the disclosure of insured bonds after the recalibration. Our results, presented in the internet appendix, provide evidence consistent with this prediction.

Next, we examine the role of ex-ante information asymmetry between issuers and bondholders. Disclosure theory predicts that issuers with greater information asymmetry have greater capital market benefits from disclosure, which reduces the information asymmetry component of their cost of capital (e.g., Diamond and Verrecchia [1991]). In our setting, issuers with higher ex-ante information asymmetry likely benefit more from ratings upgrades through a greater reduction in yields, and are thus more likely to decrease disclosure. We measure ex-ante information asymmetry using issuers of revenue versus GO bonds and issuer size. Consistent with our prediction, we find that both issuers of revenue bonds and smaller issuers provide less financial disclosure following the ratings recalibration, which helps validate the role of information asymmetry in issuers' disclosure decisions in our setting. We present these results in the internet appendix.

Finally, we repeat our difference-in-differences analyses using the set of recalibrated non-

upgraded issuers as an alternative control group. A specific subset of Moody's-rated issuers did not experience a rating upgrade resulting from the recalibration. These issuers included certain housing, healthcare, and other enterprise sectors. In these sectors, issuers primarily derive their revenues from user fees and charges rather than tax revenues and receive little governmental support. Consequently, their ratings were benchmarked against non-municipal counterparts in other markets that were already rated on the Global Scale (e.g., for-profit healthcare and education companies, power utilities, etc.) (Moody's [2010]). Given the differences in the characteristics between upgraded and non-upgraded Moody's-rated issuers, we perform our analysis by matching this alternative control group to our treatment group on a set of issuer characteristics. Our results, presented in the internet appendix, show that our findings are robust to this alternative control group. However, the matching analysis significantly reduces the size of the control group, and we urge readers to interpret these results with caution.

9. Conclusion

Our paper examines how changes in credit ratings affect municipal borrowers' disclosure of financial information. Using Moody's recalibration of their ratings methodology in 2010, we find that upgraded municipalities are less likely to disclose financial information relative to municipalities that were rated by S&P and not recalibrated. This reduction in disclosure is larger for issuers with a higher ex–ante information demand from investors, and smaller for issuers with local underwriters who have better client-specific knowledge and for issuers under direct regulatory oversight. Taken together, our results suggest that higher credit ratings lower investors' demand for information, which reduces municipal issuers' disclosure. These results have important implications for the literature studying the link between credit ratings, credit risk, and disclosure.

Our study also provides timely evidence on the determinants of opacity in the municipals

market. Unlike corporate bondholders, municipal bondholders face a significantly more opaque information environment and rely heavily on credit ratings for their investment decisions. By showing that municipal issuers disclose less when they receive higher credit ratings, we document that credit ratings are an important determinant of disclosure in this market. Our findings are important in light of recent studies highlighting the need for increased disclosure regarding the rise in healthcare costs along with public pension obligations (e.g., Novy-Marx and Rauh [2011a], [2011b]), and the SEC's recent agenda to increase financial reporting and disclosure in the municipals market (SEC [2017]).

Although our empirical tests rely on the municipal bond market, the theory linking credit risk to debtholders' demand for information is based on their specific payoff function, and fully generalizable to other markets (e.g., corporate bonds). However, the reduction in disclosure following credit ratings upgrades is likely more applicable to the municipal bond setting, because municipal investors rely heavily on ratings for information to evaluate municipalities' credit risk. In contrast, the literature suggests that in the corporate sector credit ratings likely have a smaller effect on investors' perception of credit risk due to a richer information environment (e.g., Beaver et al. [2006]), which implies a smaller effect on debt issuers' disclosure decisions. Broadly speaking, our results are most likely to translate to settings where public debtholders dominate, such as firms relying primarily on public debt for external financing. In conclusion, our findings provide insights into the role of public debtholders and rating agencies in shaping the information environment, answering the call in Beyer et al. [2010] to shed light on the role of non-equity market participants on disclosure.

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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 – June30).
FinReporting_Freq	Natural logarithm of the number of financial filings provided in a given reporting period.
Treated	Binary indicator variable equal to one for municipalities that experienced a rating upgrade from Moody's recalibration, 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 variable equal to if the issuer issued debt in a given reporting period.
AmountIssued	Natural logarithm of 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.
GSP	State–year average of Gross State Product (in units of $100 million).
PCI	State–year average Per–Capita Income (in units of $100).
HPI	State–year average House Price Index (in units of 100).
Unemployment	State–year average unemployment rate (in percent).
Other variables:	
Liquidity	Binary indicator variable equal to one for issuers of bonds that have been traded by investors on the secondary market in the four years prior to the recalibration.
PctRetailQx	Binary indicator for quartile x of the percentage of retail trades in the reporting period starting prior to the recalibration (July 1, 2009 – June 30, 2010), where a retail trade is defined as a trade not exceeding $100,000.
NationalUnd	Binary indicator variable equal to one when the municipality issues bonds with a national underwriter, defined as an underwriter that operates in the top tercile of the number of states in a reporting period.
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 trades in a reporting period.
PriceImpact	The median of daily price impact in a reporting period, multiplied by one million. The daily price impact is 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 on day t, P_j is the price of a bond in trade j, and Q_j is the par amount of trade j.
PctRetail	The percentage of retail trades by issuer–year, where a retail trade is defined as a trade not exceeding $100,000.
PctInstitutional	The percentage of institutional trades by issuer–year, where an institutional trade is defined as a trade exceeding $250,000.

Figure 1. Bond payoff function

This figure illustrates the payoff of a bond with a total principal plus interest payment of $10 as a function of the underlying asset. The underlying asset value is presented on the horizontal axis and the bond payoff on the vertical axis.

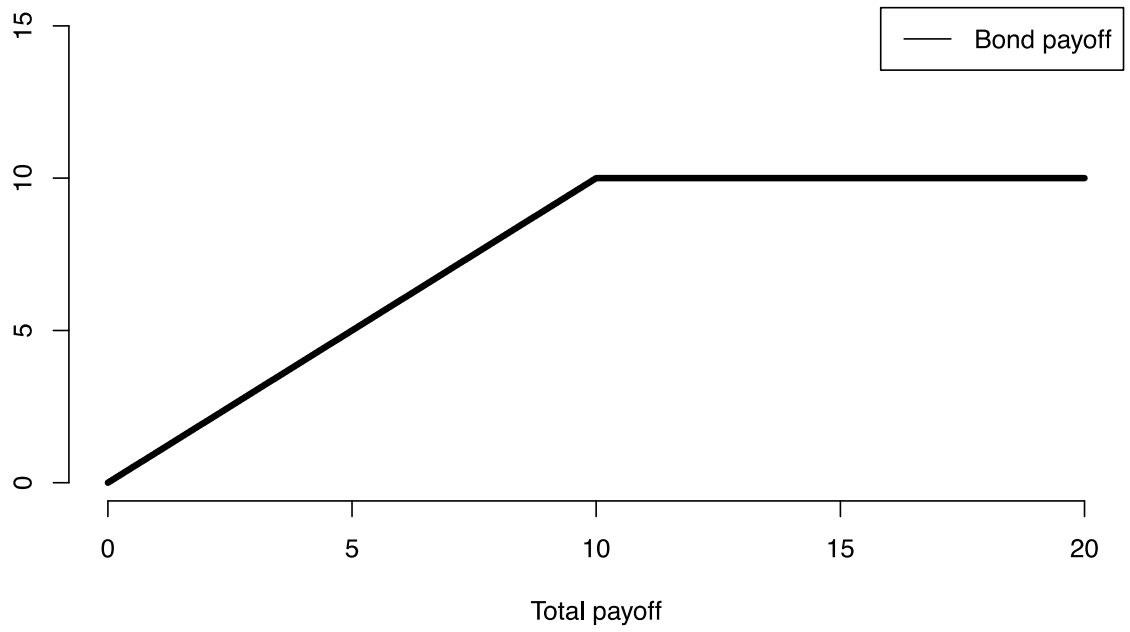


Figure 2. Geographic distribution of financial reporting

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.

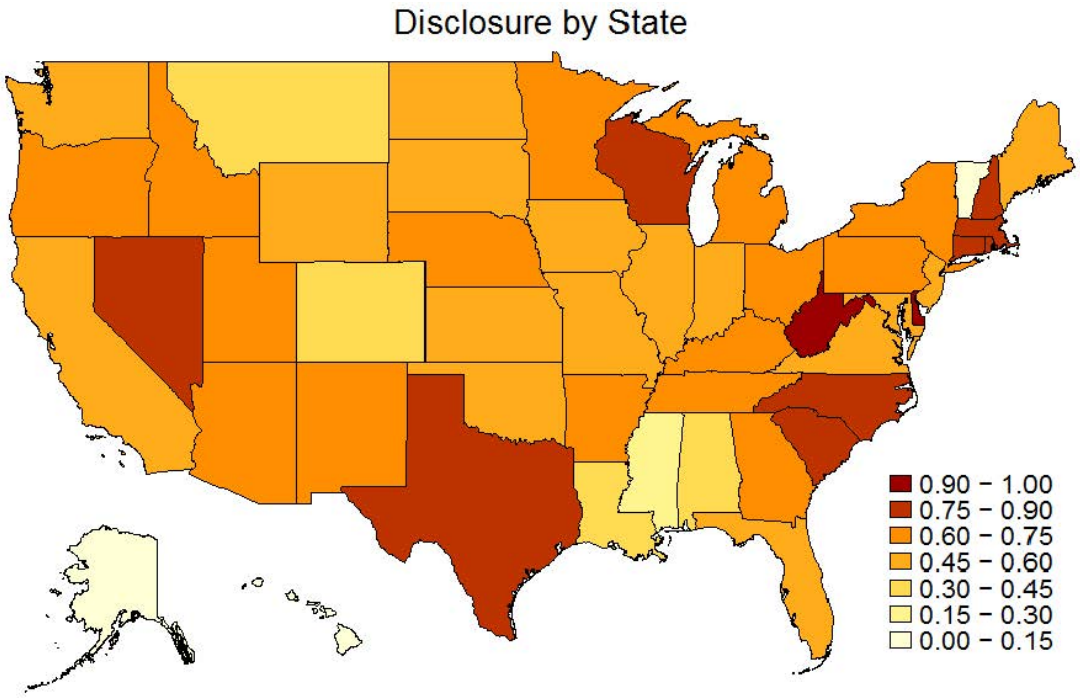
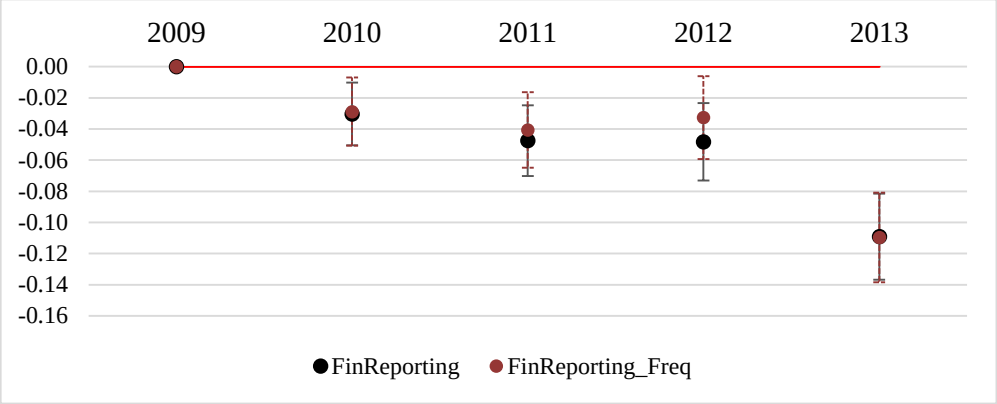


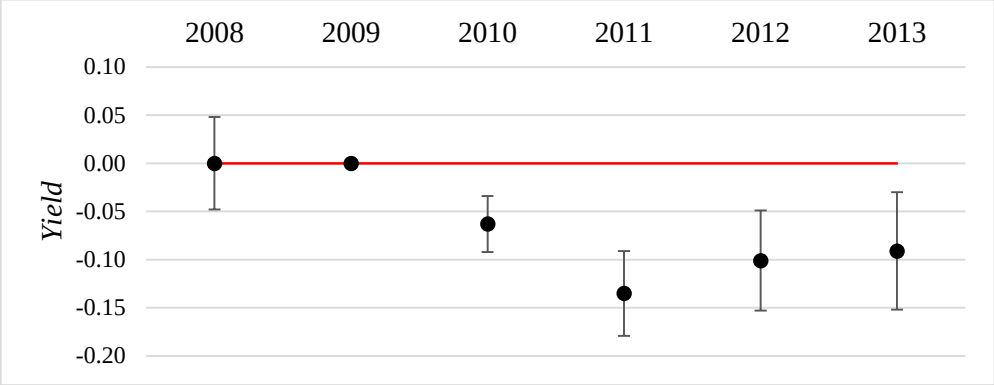
Figure 3. Trends around the ratings recalibration

This figure presents differences in *FinReporting* and *FinReporting_Freq* (Panel A), in bond yield (Panel B), and in issuance credit ratings (Panel C) between our treatment and control groups around Moody's recalibration, relative to our benchmark reporting period (2009). The difference–in–differences coefficients in Panel A are reported in Table 9, Panel A. The difference–in–differences coefficients in Panels B and C are reported in Table 9, Panel B, columns (1) and (2), respectively. Our treatment group includes municipalities that experienced a rating upgrade from the Moody's recalibration, and our control group includes municipalities that issued debt only rated by S&P in the four years prior to the recalibration. Each reporting period runs from July 1 – June 30. 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

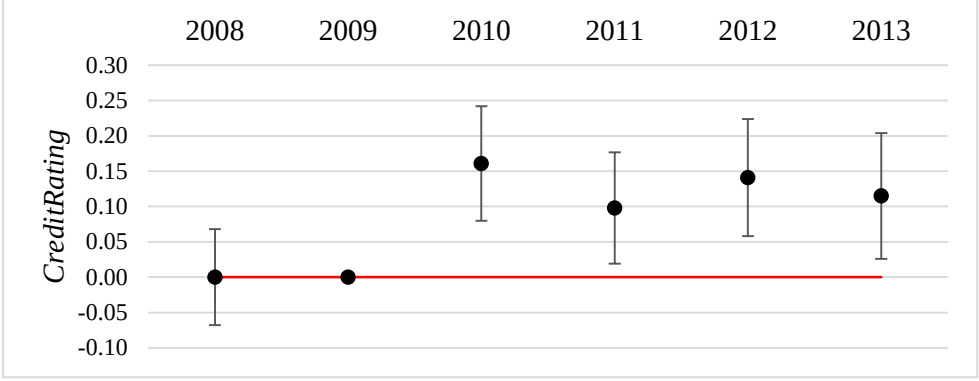


Table 1. Sample selection

This table presents the selection of our sample. 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	%
Financial information filings		
Audited annual financial statements	10,280	0.547
Other annual financial information	4,926	0.263
Interim financial information	578	0.031
Budget	1,634	0.087
Other financial information	581	0.031
Other filings		
Failure to provide required annual financial information	692	0.037
Information provided to rating agency	29	0.002
Change in fiscal year/timing of annual disclosure	13	0.001
Other	11	0.001
Total filings	18,744	1.000

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.

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	0.530	0.475	0.000	0.693	0.693
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
GSP	21,085	52.056	47.130	18.588	28.240	64.270
PCI	21,085	400.404	52.668	364.250	395.340	431.780
HPI	21,085	1.922	0.294	1.758	1.892	2.039
Unemployment	21,085	7.980	1.934	6.575	8.058	9.525
Liquidity	21,085	0.762	0.426	1.000	1.000	1.000
NationalUnd	21,085	0.426	0.495	0.000	0.000	1.000
SingleAudit	21,085	0.350	0.477	0.000	0.000	1.000

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

This table presents difference–in–differences estimates of municipal financial reporting around the Moody's ratings recalibration. *Treated* is a binary indicator variable equal to one for issuers that experienced a rating upgrade from the Moody's 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–difference regressions. 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 multivariate difference–in–differences regressions with control variables and various levels of fixed effects, using *FinReporting* as our dependent variable. Panel C presents results from multivariate 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 regressions

Dependent variable:	*FinReporting*	*FinReporting_Freq*
	(1)	(2)
Treated x *Post*	–0.055***	–0.044***
	(–4.53)	(–3.52)
Treated	0.320***	0.243***
	(22.16)	(17.34)
Post	0.128***	0.116***
	(14.77)	(13.38)
Intercept	0.401***	0.342***
	(39.03)	(34.36)
Observations	21,085	21,085
R^2 (%)	9.0	5.4

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

Panel B. Multivariate difference–in–differences regressions: Financial reporting indicator

Dependent variable:	*FinReporting*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x *Post*	–0.069***	–0.070***	–0.070***	–0.069***	–0.069***	–0.058***
	(–5.64)	(–5.74)	(–5.71)	(–5.67)	(–5.51)	(–4.75)
Treated	0.311***	0.317***	0.314***	0.304***	0.295***	
	(20.47)	(20.76)	(20.54)	(19.46)	(18.42)	
Control variables						
Issue	0.072***	0.074***	0.074***	0.077***	0.084***	–0.009
	(3.39)	(3.54)	(3.53)	(3.67)	(3.97)	(–0.48)
AmountIssued	0.006	0.003	0.003	0.002	0.000	0.011**
	(1.00)	(0.57)	(0.50)	(0.26)	(0.08)	(2.28)
%CallableBonds	0.003	0.008	0.007	0.005	–0.009	–0.023*
	(0.23)	(0.58)	(0.49)	(0.33)	(–0.66)	(–1.90)
%GOBonds	–0.015	–0.005	–0.004	–0.007	–0.008	0.027**
	(–1.00)	(–0.33)	(–0.26)	(–0.47)	(–0.54)	(2.08)
GSP	–0.004***	–0.004***	–0.004***	–0.004***	–0.003***	–0.003***
	(–4.10)	(–4.09)	(–4.08)	(–4.07)	(–3.55)	(–3.40)
PCI	0.000	0.000	0.000	0.000	0.000	0.000
	(1.13)	(1.15)	(1.15)	(1.14)	(0.94)	(1.05)
HPI	0.053	0.051	0.052	0.052	0.051	0.051
	(1.22)	(1.18)	(1.19)	(1.20)	(1.14)	(1.15)
Unemployment	0.004	0.004	0.004	0.004	0.008	0.004
	(0.64)	(0.63)	(0.62)	(0.62)	(1.24)	(0.69)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	No
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 (%)	14.7	15.2	15.6	16.1	19.7	68.1

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

Panel C. Multivariate difference–in–differences regressions: Financial reporting frequency

Dependent variable:	*FinReporting_Freq*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x *Post*	–0.062***	–0.063***	–0.063***	–0.062***	–0.064***	–0.052***
	(–4.86)	(–4.95)	(–4.94)	(–4.89)	(–4.97)	(–4.11)
Treated	0.238***	0.245***	0.247***	0.236***	0.228***	
	(15.86)	(16.38)	(16.55)	(15.51)	(14.70)	
Control variables						
Issue	0.054**	0.053**	0.055**	0.058***	0.077***	0.014
	(2.38)	(2.40)	(2.49)	(2.63)	(3.44)	(0.71)
AmountIssued	0.034***	0.030***	0.029***	0.027***	0.021***	0.016***
	(4.58)	(4.10)	(4.07)	(3.81)	(3.06)	(2.92)
%CallableBonds	0.002	0.003	–0.001	–0.003	–0.012	–0.014
	(0.16)	(0.24)	(–0.08)	(–0.23)	(–0.84)	(–1.08)
%GOBonds	–0.050***	–0.022	–0.016	–0.018	–0.024	0.023
	(–2.89)	(–1.30)	(–0.97)	(–1.12)	(–1.44)	(1.58)
GSP	–0.004***	–0.004***	–0.004***	–0.004***	–0.004***	–0.003***
	(–4.51)	(–4.48)	(–4.48)	(–4.46)	(–4.15)	(–3.31)
PCI	0.001**	0.001**	0.001**	0.001**	0.001**	0.001**
	(2.18)	(2.21)	(2.20)	(2.20)	(2.11)	(2.22)
HPI	0.014	0.012	0.012	0.012	0.006	0.001
	(0.32)	(0.26)	(0.25)	(0.27)	(0.13)	(0.02)
Unemployment	0.011*	0.011*	0.011*	0.011*	0.014**	0.012*
	(1.84)	(1.82)	(1.82)	(1.81)	(2.13)	(1.90)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	No
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 (%)	10.0	10.9	12.1	12.7	16.0	61.1

Table 4. Difference–in–differences by type 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 mirror those in column (6) of Table 3, Panels B and C, respectively, except that we replace our dependent variable, in turn, by the following variables: Column (1): audited annual financial statement filings; column (2): other annual financial information filings; column (3): interim financial information filings; column (4): budget filings; and column (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 year. Panel B presents results from estimates using the natural logarithm of one plus the number of each type of filing in a given year. All other variables are as defined in Appendix A. For parsimony, we do not tabulate coefficients on control variables (full tables are presented in 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. Types of disclosure indicators

Dependent variable:	*Audited Annual Financial Statements* (1)	*Other Annual Financial Information* (2)	*Interim Financial Information* (3)	*Budget* (4)	*Other Financial Information* (5)
Treated x *Post*	–0.033**	–0.024**	–0.004*	–0.003	–0.000
	(–2.52)	(–2.08)	(–1.92)	(–0.45)	(–0.04)
Control variables	Yes	Yes	Yes	Yes	Yes
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 (%)	57.6	58.8	67.0	50.3	40.6

Table 4. Difference–in–differences by type of disclosure (cont'd)

Panel B. Types of disclosure frequency

Dependent variable:	*Audited Annual Financial Statements Frequency* (1)	*Other Annual Financial Information Frequency* (2)	*Interim Financial Information Frequency* (3)	*Budget Frequency* (4)	*Other Financial Information Frequency* (5)
Treated x *Post*	–0.038***	–0.017*	–0.005*	0.002	0.000
	(–3.28)	(–1.88)	(–1.82)	(0.30)	(0.03)
Control variables	Yes	Yes	Yes	Yes	Yes
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 (%)	53.5	56.2	74.7	51.1	42.3

Table 5. Cross–sectional tests: The role of heterogeneity in investors' demand for information

This table presents results from examining whether the difference–in–differences estimates of municipal financial reporting around the Moody's ratings recalibration vary with the heterogeneity in investors' demand for financial information. In Panel A, we estimate the same specification as in column (6) of Table 3, Panels B and C, respectively, except that we add *Treated* x *Post* x *Liquidity* and *Post* x *Liquidity* to the regression where *Liquidity* is a binary indicator variable equal to one for issuers of bonds that have been traded by investors on the secondary market in the four years prior to the recalibration. In Panel B, we estimate the same specification as in Panel A within the sample of issuers of bonds that have been traded by investors on the secondary market in the four years prior to the recalibration (15,125 observations), except that we add interactions of *Treated* x *Post* and *Post* with binary indicators for each quartile of the percentage of retail trades (*PctRetailQ1 – PctRetailQ4*) in the reporting period prior to the recalibration (July 1, 2009 – June 30, 2010), where a retail trade is defined as a trade not exceeding $100,000. All variables are as defined in Appendix A. For parsimony, we do not tabulate coefficients on control variables (full tables are presented in 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. Issuer liquidity

Dependent variable:	*FinReporting*	*FinReporting_Freq*
	(1)	(2)
Treated x *Post* x *Liquidity*	–0.108***	–0.114***
	(–2.77)	(–3.20)
Treated x *Post*	0.034	0.044
	(0.93)	(1.37)
Post x *Liquidity*	0.028	0.030*
	(1.62)	(1.83)
Control variables	Yes	Yes
Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	21,085	21,085
R^2 (%)	68.1	61.1

Table 5. Cross–sectional tests: The role of heterogeneity in investors' demand for information (cont'd)

Panel B. Percentage of retail trades

Dependent variable:	*FinReporting* (1)	*FinReporting_Freq* (2)
Treated x *Post*	–0.067**	–0.079***
	(–2.37)	(–3.10)
Treated x *Post* x *PctRetailQ2*	0.005	0.039
	(0.11)	(0.92)
Treated x *Post* x *PctRetailQ3*	–0.043	–0.034
	(–1.03)	(–0.75)
Treated x *Post* x *PctRetailQ4*	–0.013	0.006
	(–0.30)	(0.14)
Post x *PctRetailQ2*	0.002	–0.034
	(0.04)	(–0.97)
Post x *PctRetailQ3*	0.018	0.032
	(0.51)	(0.85)
Post x *PctRetailQ4*	0.013	–0.016
Control variables	Yes	Yes
Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	15,125	15,125
R^2 (%)	56.9	52.1

Table 6. Cross–sectional tests: The role of disclosure enforcement

This table presents results from examining whether the difference–in–differences estimates of municipal financial reporting around the Moody's ratings recalibration vary with differences in disclosure enforcement. In Panel A, we estimate the same specification as in column (6) of Table 3, Panels B and C, respectively, except that we add *Treated* x *Post* x *NationalUnd*, *Treated* x *NationalUnd*, *Post* x *NationalUnd* and *NationalUnd* to the regression, where *NationalUnd* is a binary indicator variable equal to one for municipalities that issued bonds with national underwriters. In Panel B, we estimate the same specification as in Panel A, except that we add *Treated* x *Post* x *SingleAudit* and *Post* x *SingleAudit*, where *SingleAudit* is a binary indicator variable equal to one for municipalities that are mandated to provide audited financial statements under the Single Audit Act during our sample period. All variables are as defined in Appendix A. For parsimony, we do not tabulate coefficients on control variables (full tables are presented in 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. National underwriters

Dependent variable:	*FinReporting*	*FinReporting_Freq*
	(1)	(2)
Treated x *Post* x *NationalUnd*	–0.047*	–0.055*
	(–1.74)	(–1.90)
Treated x *Post*	–0.040***	–0.035**
	(–2.64)	(–2.24)
Treated x *NationalUnd*	0.054*	0.050
	(1.76)	(1.56)
Post x *NationalUnd*	0.003	0.033
	(0.12)	(1.61)
NationalUnd	–0.021	–0.017
	(–0.73)	(–0.62)
Control variables	Yes	Yes
Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	21,085	21,085
R^2 (%)	68.1	61.1

Table 6. Cross–sectional tests: The role of disclosure enforcement (cont'd)

Panel B. Single Audits

Dependent variable:	*FinReporting*	*FinReporting_Freq*
	(1)	(2)
Treated x *Post* x *SingleAudit*	0.070***	0.069***
	(2.76)	(2.61)
Treated x *Post*	–0.082***	–0.076***
	(–5.43)	(–4.83)
Post x *SingleAudit*	–0.042**	–0.018
	(–2.29)	(–1.01)
Control variables	Yes	Yes
Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	21,085	21,085
R^2 (%)	68.1	61.1

Table 7. Information content channel

This table presents difference–in–differences estimates of regressions of municipal financial reporting around the 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 *TreatedAlt*. In columns (1) and (3), *TreatedAlt = Only_Moodys*, a binary indicator variable equal to one for issuers that experienced a rating upgrade from the Moody's recalibration and issued exclusively Moody's rated bonds in the four years prior to the recalibration, and zero for municipalities that issued debt rated only by S&P in the four year prior to the recalibration (sample of 16,340 observations). In columns (2) and (4), *TreatedAlt = Moodys_S&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 for municipalities that issued debt rated only by S&P in the four year prior to the recalibration (sample of 16,105 observations). For parsimony, we do not tabulate coefficients on control variables (full tables are presented in 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 median of daily price impact in a reporting period, multiplied by one million. 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*	
TreatedAlt =	*Only_Moodys*	*Moodys_S&P*	*Only_Moodys*	*Moodys_S&P*
	(1)	(2)	(3)	(4)
TreatedAlt x *Post*	–0.057***	–0.062***	–0.068***	–0.039**
	(–3.90)	(–4.16)	(–4.59)	(–2.34)
Control variables	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Underwriter FE	Yes	Yes	Yes	Yes
Issuer FE	Yes	Yes	Yes	Yes
Observations	16,340	16,105	16,340	16,105
R^2 (%)	68.7	69.3	61.8	63.4

Table 7. 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***	–0.057***	–0.048***	–0.051***
	(–4.41)	(–4.67)	(–3.84)	(–4.04)
Control variables				
InfoAsymmetry	0.039***	0.005	0.028***	0.004
	(6.90)	(–1.01)	(4.61)	(–0.79)
MissingObs	–0.038***	–0.067***	–0.047***	–0.058***
	(–2.75)	(–5.58)	(–3.44)	(–4.88)
Issue	–0.005	–0.019	0.018	0.006
	(–0.25)	(–1.02)	(0.89)	(0.29)
AmountIssued	0.010**	0.013***	0.016***	0.018***
	(2.10)	(2.66)	(2.77)	(3.21)
%CallableBonds	–0.024**	–0.024**	–0.015	–0.015
	(–2.01)	(–1.98)	(–1.13)	(–1.14)
%GOBonds	0.026**	0.026**	0.022	0.022
	(2.03)	(2.00)	(1.52)	(1.51)
GSP	–0.003***	–0.003***	–0.003***	–0.003***
	(–3.30)	(–3.44)	(–3.21)	(–3.34)
PCI	0.000	0.000	0.001**	0.001**
	(0.82)	(1.02)	(2.05)	(2.19)
HPI	0.049	0.051	–0.001	0.001
	(1.13)	(1.16)	(–0.03)	(0.02)
Unemployment	0.004	0.004	0.012*	0.012*
	(0.72)	(0.67)	(1.93)	(1.88)
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	68.2	61.3	61.2

Table 8. Changes in investor base

This table presents difference–in–differences estimates of the percentage of retail and institutional investor trades around the 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 *PctInstitutional*, the percentage of institutional trades by issuer–year, where an institutional trade is defined as a trade exceeding $250,000. Our sample requires the existence of trades on the secondary market over our sample period (sample of 14,939 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* (1)	*PctInstitutional* (2)
Treated x *Post*	–0.015	0.004
	(–1.56)	(0.60)
Control variables		
Issue	0.001	–0.004
	(0.09)	(–0.36)
AmountIssued	–0.005	0.004*
	(–1.48)	(1.81)
%CallableBonds	0.013	–0.007
	(1.49)	(–1.09)
%GOBonds	0.010	–0.004
	(1.10)	(–0.68)
GSP	–0.002**	0.001
	(–2.50)	(1.49)
PCI	0.000	0.000
	(0.43)	(0.02)
HPI	0.031	0.013
	(1.06)	(0.64)
Unemployment	0.008*	–0.002
Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	14,939	14,939
R^2 (%)	62.2	59.5

Table 9. Trends around the recalibration

This table analyses trends around the 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. For parsimony, we do not tabulate coefficients on control variables (full tables are presented in the internet appendix). Panel B, column (1) presents estimates from a differences–in–differences regression of bond yields (*Yield*) on binary indicators for the four post–recalibration reporting periods (*Post1 – Post4*) and the period preceding our benchmark period: July 1, 2008 – June 30, 2009 (*Pre1*). We compute *Yield* as the secondary market trade–size–weighted annual average bond yield. We estimate this regression at the bond level (sample of 152,528 bond–year observations). We control for binary indicators for rating agencies (*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*), year fixed effects, bond fixed effects, and the natural logarithm of bond maturity interacted with year indicators (*Maturity*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 and require at least one bond issuance pre– and post–recalibration (sample of 3,581 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*	–0.030**	–0.029**
	(–2.47)	(–2.17)
Treated x *Post2*	–0.047***	–0.041***
	(–3.45)	(–2.76)
Treated x *Post3*	–0.048***	–0.033**
	(–3.19)	(–2.01)
Treated x *Post4*	–0.109***	–0.110***
	(–6.48)	(–6.28)
Control variables	Yes	Yes
Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	21,085	21,085
R^2 (%)	68.1	61.2

Table 9. Trends around the recalibration (cont'd)

Panel B: Yield and credit rating trends around the recalibration

Dependent variable:	*Yield*	*CreditRating*
	(1)	(2)
Treated x *Pre1*	0.007	–0.010
	(0.25)	(–0.24)
Treated x *Post1*	–0.063***	0.161***
	(–3.55)	(3.26)
Treated x *Post2*	–0.135***	0.098**
	(–4.99)	(2.04)
Treated x *Post3*	–0.101***	0.141***
	(–3.18)	(2.79)
Treated x *Post4*	–0.091**	0.115**
	(–2.47)	(2.12)
Control variables		
S&P	0.055***	0.078
	(2.89)	(1.25)
Moodys	0.070***	0.018
	(3.61)	(0.26)
SwitchToMoodys	–0.088	0.236
	(–0.62)	(1.51)
Year FE	Yes	Yes
Bond FE	Yes	No
Issuer FE	No	Yes
Maturity*Year FE	Yes	No
Observations	152,528	3,581
R^2 (%)	89.6	83.1

Table 10. Matched sample

This table presents our analysis using issuers that experienced a rating upgrade from the Moody's ratings recalibration (*Treatment Group*) and their matched counterpart issuers that only issued debt rated by S&P in the four years prior to the recalibration (*Control Group*). We match issuers in the reporting period starting prior to the recalibration (July 1, 2009 – June 30, 2010) on the following variables: issuer type (state, city, county, etc.); the trade–size–weighted average bond yield (*Yield*), the trade–size–weighted average bond maturity (*Maturity*), the average bond issuance credit–rating (*CreditRating*), where ratings are coded from 0–9, with 9 being the highest rating (AAA), and the percentage of retail trades (*PctRetail*), where a retail trade is defined as a trade not exceeding $100,000. Panel A presents cross–sample differences in mean and median values of the variables used to match treatment and control issuers in the pre–recalibration reporting period (sample of 1,679 treatment and control observations). Panel B presents estimates for the same specification as in column (6) of Table 3, Panels B and C, respectively, using our treatment group and a matched control group (sample of 8,395 treatment and 8,395 control observations). All other variables are as defined in Appendix A. For parsimony, we do not tabulate coefficients on control variables (full tables are presented in 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: Covariate balance

	Treatment group n = 1,679		Control group n = 1,679		Diff. in	Diff. in
Variable	Mean	Median	Mean	Median	means	medians
Yield	2.625	2.559	2.625	2.559	0.000	0.000
Maturity	8.638	7.688	8.638	7.688	0.000	0.000
CreditRating	7.565	8.000	7.565	8.000	0.000	0.000
PctRetail	0.744	0.816	0.744	0.816	0.000	0.000

Panel B: Difference–in–differences regressions

Dependent variable:	*FinReporting* (1)	*FinReporting_Freq* (2)
Treated x *Post*	–0.061***	–0.066***
	(–2.68)	(–2.76)
Control variables	Yes	Yes
Year FE	Yes	Yes
Underwriter FE	Yes	Yes
Issuer FE	Yes	Yes
Observations	16,790	16,790
R^2 (%)	55.9	49.2