Municipal Bond Market Effects of Credit Rating Dissemination

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Abstract

We examine the effect of dissemination on transaction costs in the municipal bond market by exploiting a regulatory change that made municipal credit rating information prepared by two of the three major credit rating agencies freely and publicly available on the Electronic Municipal Market Access (EMMA) database. We use a difference-in-differences framework that compares subsequent trading in bonds issued before the regulatory change, where the difference is whether rating information is provided on EMMA or not. We find that the dissemination of credit ratings reduced transaction costs primarily for retail investors. We also find that the dissemination effects are concentrated in bond purchases and are amplified when the information environment of the issuer is poor. Collectively, we interpret these results as indicating that the broad dissemination of credit rating information lowers transaction costs when information acquisition costs are high.

Keywords: Municipal bonds, credit ratings, information intermediaries.

JEL codes: G15, G18, M41.

Data Availability: Data are available from the public sources cited in the text.

1. Introduction

This paper examines the effect of credit rating dissemination on trading costs in the municipal bond market. This market is economically large with $4.2 trillion in holdings as of September 2020, but it is commonly characterized as opaque, fragmented, illiquid, and as a market that imposes high trading costs on its investors. Unlike corporations, state and local governments are not subject to the SEC's registration and strict reporting requirements (Naughton and Spamann, 2015). Therefore, financial disclosure by municipalities is often less reliable, less comparable, and less timely than information released by corporations. Prior research has found that credit ratings incorporate municipal financial information (e.g., Wallace 1981; Copeland and Ingram 1982; Edmonds et al., 2017; Dambra et al., 2022) thereby making them a valuable source of credit-relevant information.[1] However, these studies do not consider whether the dissemination of credit ratings impacts transaction costs in the municipal bond market.

Dissemination of information is distinct from disclosure, as the latter refers to the provision of information to the public while the former is about the channel entities use to communicate information. Prior studies have examined dissemination via multiple sources, including the business press and newswire services, conference calls, as well as more recently developed outlets such as corporate websites and social media (e.g., Blankespoor, deHaan, and Zhu, 2018). In general, this literature suggests that dissemination can reduce information acquisition and processing costs, but the capital market consequences of these reduced costs are unclear. On the one hand, increased dissemination can encourage investors to trade in the underlying security by reducing information acquisition costs, for example, by making it easier for investors to evaluate investment alternatives. On the other hand, increased dissemination can discourage costly

[1] See Feldstein and Fabozzi, supra note 72, at 223. Market participants have indicated that retail investors primarily focus on interest rate, maturity and credit rating.

information acquisition, thereby deterring more sophisticated investors from impounding such information into security prices through trading activities. These competing forces make the overall effect of increased dissemination difficult to predict, especially in the municipal bond market. When compared with the U.S. stock market, the municipal bond market has fewer information intermediaries, and is populated by a higher percentage of retail investors (e.g., Basu and Naughton, 2020). In addition, municipal bond market investors are generally focused on news about downside as opposed to upside risk. Therefore, it is also difficult to develop predictions about the effects of dissemination in the municipal bond market from U.S. stock market studies.[2]

In this paper, we exploit the differential provision of credit rating information via the Electronic Municipal Market Access (EMMA) database by the Municipal Securities Rulemaking Board (MSRB), the sole repository for continuing disclosures for municipal bond investors starting July 1, 2009, to examine the effect of dissemination in the municipal bond market. Although other financial disclosures have been available via EMMA since 2009, rating information was unavailable until November 2011. At that time, MSRB initiated the provision of ratings information for Standard and Poor's (S&P) and Fitch Ratings (Fitch), two of the three major agencies that rate municipal bonds.[3] The third major rating agency, Moody's Investors Service (Moody's), did not have their rating information provided through EMMA until 2015 because of concerns they raised about legal liability.[4] In our analyses, we identify bonds in the pre-

[2] The uncertainty of the effect of credit rating dissemination in our setting is also evident in practitioner comments. For example, while the Government Finance Officers Association stated that "We believe that there is nothing more relevant than making credit ratings available in one location, where the public can access the information quickly, efficiently, and at no cost to them", the rating agencies expressed concern that posting ratings data at no charge on EMMA is commercially untenable, does not appropriately account for the value of their intellectual property, and creates potential legal liability issues.
[3] See https://www.wsj.com/articles/BL-TOTALB-123 and https://www.sec.gov/rules/sro/msrb/2010/34-63086.pdf
[4] Moody's argued that because the rating information was available to the "general public", that there was the potential that the rating information would be misused. The MSRB worked with Moody's to alleviate this concern, resulting in the provision of Moody's ratings in 2015. We discuss this aspect of our setting in more detail in Section 2.

dissemination period that were rated by either S&P or Fitch but not Moody's (treated observations), and bonds that were rated only by Moody's (control observations), and employ a difference-in-differences specification to isolate the effect of dissemination on transaction costs in the municipal bond market.

This setting has several features that facilitate the identification of increased dissemination. First, it allows us to identify the effect of ratings dissemination without any underlying change in the issue's information set. Since the decision to post credit rating information from two of three major agencies via EMMA was undertaken based on a mutual discussion between the MSRB and the rating agencies, it is independent of issuer level fundamental information. In addition, we select our sample using bonds that were traded *before* the regulatory change, ensuring that the choice to be rated by a particular rating agency is independent of whether the rating information was later provided on EMMA. These two aspects of our setting and research design mitigate concerns about self-selection. Third, other bond-issue level information was already on EMMA starting in early 2009 (i.e., the start of our sample period). This aspect allows us to employ a research design where the only material change is the provision of ratings information, which allows us to better identify the effect of credit ratings dissemination independent of other financial information. Fourth, since EMMA is a third-party website, we can rule out the strategic dissemination of financial information independent of the underlying incentives of the issuers and rating agencies. Finally, the rating disclosure, at least in part, is a material item that should allow market participants to evaluate the relative quality of an issue (e.g., Cornaggia et al., 2018). This aspect of our setting helps to ensure that we have sufficient power to make meaningful conclusions.

Our main tests measure changes in transaction costs using dealer markups (e.g., Schultz, 2012; Cuny, 2018), which are akin to bid-ask spreads in stock markets.[5] Our research design measures the differential change in transaction costs for bonds where ratings information was newly disseminated via EMMA (i.e., bonds rated by either S&P or Fitch but not Moody's) with those where there was no dissemination (i.e., bonds rated only by Moody's). Our univariate results reveal an economically meaningful differential reduction in transaction costs from dissemination of approximately 18 basis points. We find similar results using a multivariate specification that controls for the determinants of transaction costs and issuer economic conditions, and by including year, bond, and trade-type (i.e., buy or sell) fixed effects. We also employ an entropy matching procedure, which further controls for potential differences in bond issuance characteristics across the treatment and control samples.[6] We continue to find an economically meaningful differential reduction in transaction costs from the dissemination of credit rating information.

Next, we conduct cross-sectional analyses to ascertain whether information acquisition costs are one of the mechanisms driving our results. First, we consider the differential effect of dissemination on transaction costs across retail and institutional investors. Compared to institutional investors, retail investors face greater information acquisition costs. Prior to the regulatory change, retail investors would likely need to manually inspect financial filings to create comparisons across different bonds as the only centralized source of credit rating information was from the rating agencies themselves, and this information typically required a subscription. We follow prior studies and use a transaction cost cutoff of $100,000 to separate trades into retail and

[5] In robustness tests, we also document a differential and significantly positive increase in turnover, count of trades, and log of dollar value of trades for treated observations, indicating that the dissemination of credit ratings increased liquidity in the municipal bond market.

[6] Our results are robust to using the a nearest neighbor matching procedure, which minimizes the distance between a vector of bond characteristics across treatment and control issuers to identify the best control firm for each treated firm.

institutional (e.g., MSRB 2020). Our analyses reveal that the differential reduction in transaction costs only occurred for retail transactions, indicating that the benefits of dissemination are concentrated in the retail investor group. We also split our sample into bond purchases and sales, and examine how the dissemination effect varies across these two transaction types. Consistent with prior studies that have documented that information acquisition costs are a more important determinant of bargaining power in bond purchases than in bond sales (e.g., Cuny, 2018), we find that the dissemination effect is concentrated in the bond purchase subsample.

Lastly, we provide additional support for the role of information acquisition costs by examining whether the effects we document are concentrated in issuers with worse information environments (e.g., Blankespoor et al., 2014). We suggest that information acquisition costs are higher for issuers with worse information environments, and hence the incremental value of the credit rating dissemination is higher for those issuers. We use three proxies for issuers' information environment: (1) whether the issuer is subject to a mandatory audit, (2) whether there is posted financial information about the issuer on EMMA, and (3) whether the issuer has received a financial reporting award from the Government Finance Officers Association. For each of these three proxies, we separate our sample into two subsamples and conduct separate analyses to estimate the dissemination effect for issuers with 'worse' versus 'better' information environments. Across each analysis, we find that the dissemination effects are stronger for issuers with worse information environments, and that the differences between those entities with 'worse' versus 'better' information environments are highly significant.

Overall, our cross-sectional analyses indicate that retail investors were the primary beneficiaries of the increased dissemination of credit rating information, as it was only retail investors who experienced a reduction in transaction costs. In addition, we find that the reduction in transaction costs arises primarily for bond purchases and is pronounced for issuers with worse information environments. Collectively, these results suggest that changes in information acquisition costs that arose from the regulatory change contributed to the changes in municipal bond market trading costs.

Our final analysis exploits the later (June 2015) provision of Moody's ratings information as a second experimental setting. This analysis uses the same bonds as our main tests, but changes the control group to issues that were rated by S&P or Fitch and the treatment group to issues that were rated by only Moody's. This experimental setting lacks power relative to our main tests, as the set of information available to investors is only modestly changed by the addition of Moody's ratings to EMMA. In addition, since trading in municipal bonds decreases over the lifetime of the bond, this analysis uses relatively light trading data and hence the results should be interpreted with caution. Consistent with our main analyses, the results suggest that increased dissemination lowers transaction costs. However, the coefficients have a lower level of statistical significance and indicate an effect that is approximately half the economic magnitude of our main analysis.

Our paper contributes to a few areas of the literature. First, we contribute to those studies that examine the determinants of transaction costs in the municipal bond market. Green et al. (2007) suggests that dealers exercise substantial market power and sophisticated investors have access to price-relevant information thereby resulting in lower transaction costs. Schultz (2012) asserts that transaction costs are a function of interdealer volume as a percentage of the number of bonds and pre-trade transparency. In the accounting literature, Cuny (2018) documents that access to financial statement information enhances retail investors' bargaining power by reducing the difference in transaction costs that small municipal bond investors pay over large investors. We contribute to this literature by documenting that credit ratings dissemination is an important factor that also reduces transaction costs even when there are no changes in available financial statement information.

We also contribute to the literature that examines the economic content and real effects of municipal credit ratings. Prior research has shown that credit ratings incorporate municipal financial information thereby making them a valuable source of information (Wallace, 1981; Copeland and Ingram, 1982; Wilson and Howard, 1984; Edmonds et al., 2017; Cornaggia et al., 2018). Related studies have also documented that municipal credit ratings have implications for issuer decision making, such as disclosure choices (Gillette et al., 2020). While this literature has shown that the economic information in credit ratings affects market outcomes and issuer decision making, they do not consider the availability of credit rating information. We complement these studies by documenting that there are real effects associated with credit rating dissemination, and in particular, that increased dissemination moved the municipal bond market toward a level playing field in terms of transaction costs for all investors.

Lastly, we contribute to the information dissemination literature. This literature has shown that firms share information via alternative means, including twitter, corporate websites, mobile applications, and other social media (e.g., Blankespoor et al., 2014; Jung et al., 2018). Such dissemination has been found to help alleviate information asymmetry among investors and improve market liquidity (Blankespoor et al., 2014). However, prior studies examine dissemination in U.S. stock markets, which are structured very differently from the opaque and decentralized municipal bond market (e.g., Harris and Piwowar, 2006). We focus on one specific but important form of dissemination to show that credit ratings dissemination has implications for transaction costs in the municipal bond market. We also find evidence that the benefits of dissemination in the municipal bond market arise from a reduction in information acquisition costs, which is one of the potential mechanisms highlighted in the literature that studies U.S. stock markets.

The remainder of this paper is organized as follows. Section 2 describes the institutional setting. Section 3 summarizes the relevant literature and develops our hypothesis. The sample selection process is described in Section 4, followed by our main results in Section 5. Section 6 describes our robustness tests, and Section 7 concludes.

2. Institutional Setting

Municipal bonds are debt securities issued by states, cities, counties, and other governmental entities, such as school systems or water treatment facilities, to fund day-to-day obligations and to finance capital projects. The market is relatively large, with approximately $4.2 trillion in holdings as of September 2020. The majority of investors in the municipal bond market are retail investors who invest through direct holdings or mutual funds. However, due to the illiquid nature of the market, most trades are decentralized over-the-counter trades with broker dealers. As a result, retail investors rely on dealer quotes to glean information from the market prior to trading. The lack of transparency and the resulting information asymmetry between dealers and investors is one of the main drivers of transaction costs in the municipal bond market for retail investors.

This lack of transparency was also an important driver of the development of the Electronic Municipal Market Access (EMMA) database by the Municipal Securities Rulemaking Board (MSRB).[7] The database was initially launched as a pilot on March 31, 2008, at which time it served as a free integrated public display of documents and data produced by other pre-existing MSRB systems, including official statements and advance refunding documents (e.g., escrow deposit

[7] http://emma.msrb.org

agreements used in advance refunding) collected by the MSRB through its former Municipal Securities Information Library (MSIL) system as well as trade data collected by the MSRB's Real-Time Transaction Reporting System (RTRS). Prior to its launch, the information and documents displayed on the EMMA website were not generally freely or publicly available. On June 1, 2009, the official statement/advance refunding submission process, previously handled by the MSIL system, was migrated to the new EMMA Primary Market Disclosure Service. Since 2009, the EMMA website has been continuously updated to incorporate changes made to the various MSRB market transparency programs.

On May 20, 2010, MSRB filed with the U.S. Securities and Exchange Commission (SEC) a proposed rule change to amend the continuing disclosure service of the MSRB's Electronic Municipal Market Access system ("EMMA") to provide for the posting of credit rating information on the EMMA public website. Under the proposed rule change, MSRB would invite all NRSRO's to post their credit ratings "free of charge" on the EMMA website for investors. The objective was to make credit ratings information easily accessible on an equal basis to all participants, especially retail investors, in the municipal securities market.

The provision of credit ratings through EMMA meant that all investors can freely access credit rating information in a timely manner. Prior to this rule change, ratings were available through subscription-based services from the rating agencies. In addition, dissemination via EMMA enabled ratings information to be integrated with other details about a security or issue, and users were now able to use ratings criteria to identify municipal bonds through EMMA's search features on the website. The EMMA interface also allowed users to search among all municipal bonds by current long-term credit rating. The credit rating information on EMMA included any short-term rating, underlying or unenhanced rating, credit enhancement features, or other information such as forward-looking statements concerning the likelihood that a rating may change. Collectively, the new interface substantially reduced information acquisition costs for investors who did not previously have access to credit rating information through the rating agencies.

The SEC received two notable comments on the proposed rule change filed by the MSRB, one from the Government Finance Officers Association (GFOA) and the other from S&P. GFOA strongly supported the proposed rule as "something that is a true benefit to investors and the public." S&P, however, expressed concern that posting ratings data at MSRB, at no charge, is commercially untenable and does not appropriately account for the value of the NRSROs' intellectual property. They further believed that including credit rating and related information on the EMMA public website would offer only limited incremental value to investors in municipal securities. Despite S&P's concerns, the proposed rule was approved by SEC in October, 2010[8], and MSRB started displaying CUSIP[9] level credit ratings for municipal bonds from Fitch and S&P starting November 2011.

At the time the new rule was adopted, the MSRB also expressed its willingness to work with any NRSRO to provide assurance that the risk of misuse of its proprietary interests through disclosure on EMMA was minimal. This expression was largely in response to concerns raised by Moody's, who believed that such disclosures could create legal liability-related issues given that EMMA is available to the general public.[10] Moody's expressed the concern that unsophisticated investors would rely on the displayed rating information and that this reliance could create liability

[8] See https://www.sec.gov/rules/sro/msrb/2010/34-63086.pdf

[9] A CUSIP number is a unique identifier that stands for the Committee on Uniform Securities Identification Procedures. It is a nine-digit numeric or nine-character alphanumeric code that identifies a North American financial security for the purposes of facilitating clearing and settlement of trades.

[10] https://www.wsj.com/articles/BL-MBB-33860

if trades generated substantial losses. These concerns were sufficiently mitigated over the next several years, such that EMMA began to also display Moody's ratings in June 2015. Figure 1 provides a screenshot of the ratings displayed via the EMMA website. Investors can observe all the current ratings posted by a given issuer as well as compare across multiple issuers' ratings.

3. Literature Review and Hypothesis development

The main objective of this study is to examine the effect of disseminating credit rating information to municipal bond investors, which adds to the literature that examines the provision of municipal disclosures (e.g., Naughton and Spamann, 2015; Cuny, 2018; Dambra et al., 2022). Dissemination of information is distinct from disclosure, as the latter refers to the provision of information to the public while the former is about the channel entities use to communicate information. Prior studies have found that dissemination via business press (Bushee, Core, Guay, and Hamm, 2010; Rogers, Skinner, and Zechman, 2016), newswire services (Li, Ramesh, and Shen, 2011), conference calls (Frankel, Johnson, and Skinner, 1999; Bushee, Matsumoto, and Miller, 2003), and social media (Blankespoor, Miller, and White, 2014; Jung et al., 2018) has important consequences above and beyond the disclosure decision. Each of these studies examines dissemination in U.S. stock markets, which are structured very differently from the municipal debt market (e.g., Harris and Piwowar, 2006). In particular, the municipal debt market has fewer information intermediaries (e.g., Basu and Naughton, 2020) and a set of investors that are primarily focused on downside as opposed to upside risk (Gillette et al., 2020). In addition, the municipal debt market has a relatively high percentage of retail investors (Schwert, 2017; MSRB, 2018).

Our setting focuses on the broad dissemination of credit ratings information via EMMA, a third-party website which is the sole repository for continuing disclosures for municipal bond investors. Our study is related to Cuny (2018), who examines the effect of access to issuer financial statement information through EMMA. In general, prior research has found that credit ratings incorporate municipal financial information. Copeland and Ingram (1982) find a positive association between credit ratings and subsequent disclosure but only weak evidence that municipal financial statements predict credit ratings, suggesting that credit ratings are a leading indicator of what is disclosed in the financial statements. Consistent with this finding, Ingram, Brooks, and Copeland (1983) find that credit rating changes are more informative in the municipal bond market relative to the corporate market because municipal financial statements are produced with a greater lag and information processing costs are significantly higher. More recent papers have documented that municipal credit ratings continue to be relevant to investors and issuers (Cornaggia et al., 2018), and that credit ratings influence issuer decisions, such as those related to disclosure (Gillette et al., 2020; Basu et al., 2022).

The municipal bond market consequences of credit rating dissemination are unclear. On the one hand, increased dissemination has the potential to encourage retail trading, as those investors will have more readily available information on which to base trading decisions, thus reducing information acquisition costs. On the other hand, increased dissemination of credit ratings could discourage costly information acquisition, and therefore dissuade more sophisticated investors from trading activities. Green et al. (2007) note that institutional investors play a critical role in establishing transaction costs in the municipal bond market. As a result, a reduction in institutional investor trading could increase information asymmetry and trading costs for all investors.

In addition, third-party dissemination of credit ratings may not have an impact on transaction costs for several reasons. First, investors can gather the same information from other sources, including rating agencies or issuer websites. Second, it may be the case that credit rating information displayed via EMMA is insufficient for retail investors to integrate credit risk into their trading decisions.[11] For example, Badoer and Demiroglu (2019) document that the dissemination of over-the-counter transactions in corporate debt securities increases the informational efficiency of bond prices and reduces the incremental information content of credit ratings. In our setting, this finding suggests that the availability of other financial information including trading data via EMMA could reduce the incremental informativeness of credit ratings. Lastly, the rating agencies suggested that the benefits of the proposed rule change to investors in municipal securities would not outweigh the burdens that it would impose on NRSROs that voluntarily provided such information.

Therefore, we state our hypotheses in null form due to the uncertainty as to whether credit rating dissemination will affect transaction costs in the municipal bond market.

H1: The dissemination of credit ratings has no effect on municipal bond transaction costs.

Prior studies have noted that investors face heterogeneous information acquisition costs. In our setting, institutional investors may have had relatively easy access to credit rating information before that information was disseminated on EMMA through subscriptions to the rating agencies. In contrast, retail investors may have found it difficult to acquire credit rating information in a timely manner, and may have faced substantially higher information acquisition costs in those cases where they wanted to compare ratings across multiple issues. These suggestions imply that credit rating dissemination may impact retail investor trading differently than institutional trading. However, it remains possible that no such difference manifests because retail investors may not

[11] S&P's comment letter on the proposed implementation of the ratings disclosure rule states that "existing disclosure is sufficient to enable investors to access S&P's ratings". S&P further stated that "such information may not be sufficiently tailored to meet the needs of retail investors".

change their trading behavior or because the underlying credit rating information is available from other sources. Therefore, we also state our next hypothesis in null form.

> H2a: *The dissemination of credit ratings does not have an incremental effect on municipal bond transaction costs for retail investors.*

The dissemination of credit rating information on EMMA reduces information acquisition costs, particularly in those cases where alternative sources of information that investors use to assess investment alternatives are untimely or poorly constructed. To the extent that information acquisition costs play an important role in trading decisions, there should be a greater dissemination effect on trading costs in issues where information acquisition costs are highest. Again, it is possible that investors will not ultimately change their trading behavior as the underlying information is available from other sources. Therefore, we also state our final hypothesis in null form.

> H2b: *The dissemination of credit ratings does not have an incremental effect on municipal bond transaction costs for issues with lower quality information environments.*

4. Sample Selection

We build our sample by first identifying the universe of municipal bonds that were issued between July 2009 and October 2011, the period after EMMA began displaying other financial information including continuing disclosures from the issuers and before it started displaying credit rating information.[12] This selection criteria allows us to identify the effect of credit rating dissemination while controlling for underlying disclosure requirements across issuers. We restrict our sample to bonds issued before November 2011 to ensure all issues have trading transactions in the pre-period and to mitigate the concern that selection bias may drive our results. In our setting, self-selection would occur if bond issuers chose a particular rating agency due to the fact that the

[12] According to SEC Rule 15c2-12.

rating information was displayed on EMMA. By restricting our sample to pre-November 2011 issues, all bonds in our sample were assigned to the treatment and control group before they were aware of the dissemination change, thus mitigating the self-selection issue. We obtain ratings and other bond level characteristics from Bloomberg. We also obtain secondary market bond transaction data from the MSRB between July 2009 through June 2014. This allows us to create an equidistant sample with two and half years before and after the initiation of public dissemination of credit ratings via EMMA.

Next, we apply liquidity restrictions that are necessary for us to implement our research design. First, in order to measure transaction costs and other trading activity measures, we require at least one inter-dealer trade on the same day as the investor transaction. Second, to facilitate difference-in-difference tests, we limit the sample of issues such that trading variables can be measured in both the pre- and post-period. Furthermore, we require that bonds be traded at least once by both retail and institutional investors in both the pre- and post-period. Thus, each bond in the sample has a minimum of four dealer-matched investor purchases or sales across the five-year sample period.

We define treatment observations as those bonds rated by S&P or Fitch but not by Moody's (i.e., those bonds where rating information becomes available on EMMA in November 2011) and control observations as bonds that are rated by Moody's but neither S&P nor Fitch (i.e., those bonds where rating information continues to be unavailable on EMMA for the entire sample period).[13] We exclude all other bonds that are not in the treatment group or the control groups. This removes transactions for bonds that are rated by both S&P and Moody’s, and by both Fitch

[13] In untabulated robustness tests, we confirm that our results are robust to several alternate ways of defining the treatment group. Specifically, our conclusions are unchanged if we define treatment issues as those (1) rated by both S&P and Fitch, (2) rated by S&P, which eliminates issues rated by Fitch alone, and (3) rated by Fitch, which eliminates issues rated by S&P alone.

and Moody's. We also exclude unrated bonds, which prior research has found to have different characteristics than rated bonds (e.g., Reeve and Herring 1986, Ziebell and Rivers 1992, Peppe and Unal 2021). The final trading sample comprises 433,353 transactions across 6,327 bonds issued by 2,447 unique issuers, with 311,673 trades (4,244 bonds) in the treatment group and 121,680 transactions (2,083 bonds) in the control group. The sample selection process is provided in Table 1.

5. Research Design and Results

5.1 Main Results

We examine the effect of credit rating dissemination on transaction costs in the municipal bond market using the following difference-in-difference research design:

$$TransactionCost_{b,t} = \alpha + \beta_1 Treated_{b,t} + \beta_2 Treated_{b,t} * Post_{,b,t} + \sum \gamma_j\, Controls + Fixed\ Effects + \varepsilon_{i,t} \quad (1)$$

We follow the methodology in Schultz (2012) and Cuny (2018) and proxy for transaction costs using the difference between a bond's inter-dealer price and the price paid by an investor. More specifically, we measure *TransactionCost* as the markup on any trade in bond *b* on date *t* using the formula below:

$$\text{Markup}_{b,t} = TradeSign_{b,t} * 10{,}000 * \text{Log}\left[\frac{\text{InvestorPrice}_{b,t}}{\text{AvgInterdealerPrice}_{b,t}}\right]$$

TradeSign is a buy/sell indicator equal to "1" if the trade is a customer purchase, "-1" if the trade is a customer sale, and "0" if the trade is inter-dealer. Markup can only be determined if there is both a transaction between an investor and dealer as well as at least one inter-dealer trade on the same day. The *InvestorPrice* is the purchase (sale) price paid (received) by an investor in bond *b* on date *t*. The *AvgInterdealerPrice* is the average price of all inter-dealer transactions in bond *b* on date *t*. The Markup captures the basis point difference between the inter-dealer price and the price at which investors purchase (sell) the same security on the same day. Following prior literature, negative values of *Markup* are dropped as those are uncommon and akin to a negative bid-ask spread (e.g., Chordia et al., 2001).[14] We winsorize all continuous variables at a 1% (99%) level to reduce the effect of outliers and possible data entry errors.[15]

We define *Treated* as an indicator that equals 1 for bonds rated by S&P or Fitch but not by Moody's and 0 for bonds that are rated by Moody's but neither S&P nor Fitch. *Post* is an indicator that equals 1 for transactions that take place after MSRB started posting S&P and Fitch credit ratings on November 21st, 2011. The standalone *Post* variable is subsumed by fixed effects. Our main coefficient of interest is β_2, the coefficient on the interaction term *Treated*Post*, which captures the differential change in *TransactionCost* across the treatment bond-years (i.e., S&P or Fitch ratings disseminated via EMMA) and control bond-years (i.e., Moody's ratings not disseminated via EMMA). A reduction in transaction costs will produce a negative coefficient on β_3 .

We use a wide array of control variables identified in the prior literature as correlated with transaction costs. We control for the time remaining to maturity (*Duration*), the time in the dealer's inventory (*Inventory*, an indicator that equals 1 if a purchase (sale) does not follow (precede) a sale (purchase) within one day of the trade date (e.g., Sirri, 2014)), trade size (*Log(TradeSize)*), the par value of all transactions in a bond during a given month (*Log(AggTrades)*), and whether the trade is a retail trade or institutional trade (*Retail*, measured used a transaction size cutoff of $100,000). We also include state GDP, *Log(StateGDP)*, and state personal income level, *Log(PersIncome)*,

[14] We drop 4.6% of trades (i.e., 20,918 out of 454,271 trades) due to a negative value for *Markup*.
[15] Our results are unchanged if we don't winsorize the variables.

to control for changes in issuer economic conditions. All variables are defined in Appendix A. Lastly, we include bond fixed effects, trade date fixed effects, and trade type fixed effects that identify whether the transaction is a customer purchase or sale.

Table 2 provides descriptive statistics for our main sample. The mean value of *Treated* is 0.719, indicating a slightly larger sized sample of treatment than control observations. About 86.7 percent of transactions are made by retail investors (i.e., the trade size is under $100,000), which indicates that even though we restrict our sample to bonds that are traded at least once by both retail and institutional investors, the majority of transactions are conducted by retail investors. The *Post* variable has a value of 0.335, indicating that about 33.5 percent of the trades in our sample occur after the regulatory change. This value indicates that there are fewer trades as bonds age, consistent with other studies and anecdotal evidence.

We use the entropy balanced matching technique to match treatment and control observations (Hainmueller, 2012; McMullin and Schonberger, 2020; Basu et al., 2022). In our setting, this matching approach provides a way to reduce noise that would otherwise be present in our estimation if the average treatment observation is not sufficiently comparable to the average control observation. The entropy balancing technique preserves the full sample and ensures covariate balance between treatment and control observations by re-weighting observations such that the post-weighting mean and variance for treatment and control observations are virtually identical along rating controls. This approach ensures that our treatment and control samples are comparable based on observable bond characteristics, thus allowing us to reasonably identify markup changes as a response to the dissemination effect as opposed to inherent and unobservable differences in transaction costs across the treatment and control bonds. Our conclusions are the same if we use an unweighted panel.

We carry out entropy matching using a comprehensive set of bond characteristics, as the goal of this empirical strategy is to mitigate differences across observations in the treatment and control groups. The specific entropy matching variables we use are the bond's credit rating at issuance or the average of the S&P and Fitch ratings if it is rated by both agencies (*Avg Rating*), the percent of retail trading on the bond before the regulatory change (*Avg Retail*), the bond yield at issue (*Yield*), whether the bond is callable (*Callable*), issue amount (*Log (Amount)*), the issue's coupon rate (*Coupon*), the maturity of the bond (*Maturity*) and whether a bond is issued by a state, city or county government (*State_City_County*). Table 3 Panel A provides the mean of each variable across our treatment and control subsamples both before and after we employ entropy matching. Pre-matching, there are some significant differences across the two groups of observations. For example, the treated group appears to have slightly worse ratings (mean Avg Rating of 3.498 for the treated group compared with 3.507 for the control group), are more likely to be insured (14.4% is insured for the treated group compared with 11.4% for the control group), and more likely to have a call feature (mean value of *Callable* is 0.585 for the treated group compared with 0.561 for the control group). However, post-matching, there are no differences in either the mean or variance of any of the matching variables across the two groups of observations.

The regression results from equation (1) are presented in Table 3. Panel B provides the univariate results and Panel C provides the multivariate results. The results in Panel B show no significant difference in *Markup* between treated and control bonds during the pre-dissemination period. This evidence suggests that treated and control groups are comparable in terms of *Markup* during the pre-period. We also find a decrease in *Markup* during the post-period when compared to pre-period for both treated and control bonds. Further, the decrease in *Markup* is significantly higher for the treated group compared to the control group. This is evident from the significant difference in *Markup* between treated and control group during the post-period. The difference-in-differences estimator is also negative and significant and reflects an economically meaningful decline of about 18.7 basis points. The decline in *Markup* for the control observations could be due to a variety of factors, such as macroeconomic events. However, we believe that those factors are comparable across the two groups of observations.

Table 3 Panel C shows the multivariate results using equation (1). The dependent variable is *Markup* in columns (1), (2), and (3). All columns include bond and trade date fixed effects, column (2) and column (3) include trade type fixed effects (indicating whether the transaction is a purchase or sale), and column (3) fully interacts all control variables with the *Post* indicator. All specifications cluster standard errors by bond and trade date. Across the two columns, the coefficient on the *Treated * Post* interaction term is significantly negative. This coefficient value indicates that investors trading bonds rated by S&P or Fitch had a differentially lower markup in the post-dissemination period compared to those trading in bonds rated by Moody's. The effects we document are also economically meaningful. The coefficients on *Treated * Post* are negative and significant at 1% level (coefficient =-7.090 and t-statistic = 2.865 in column (1); coefficient =-6.191 and t-statistic = 2.585 in column (2); coefficient =-5.730 and t-statistic = 2.734 in column (3)), indicating that treated bonds have an approximately 5.7% to 7.1% relative decline in *Markup*.

5.2 Cross-sectional tests

We conduct three sets of analyses to provide insights into the role of information acquisition costs. First, we investigate whether the reduction in transaction costs is greater for retail investors, who likely face greater information acquisition costs. Following prior literature, we define trades with par values under $100,000 as retail trades and above $100,000 as institutional trades (e.g., Edwards et al., 2007; Schultz, 2001; Cuny, 2018). The regression results in Table 4 show that there is a significantly negative effect on *Markup* for retail investors but not for institutional investors (Coefficient on *Treated * Post* = -7.617 and t-statistic = 2.741 for retail trading sample; and coefficient = 1.650 and t-statistic = 0.747 for institutional trading sample). An F-test shows the difference in the coefficients on *Treated * Post* is significant at the 1% level. In terms of economic significance, retail investors exhibit a decline in *Markup* of about 7.617 basis points compared with virtually no change for institutional investors. Collectively, these results provide evidence that the dissemination effects are concentrated among retail investors, the group most likely to experience a decline in information acquisition costs as a result of the regulatory change.

Next, we disaggregate the overall aggregate effect of bond purchases and sales documented in Table 3 into the component that arises from purchases and the component that arises from sales. Prior studies have documented that information acquisition costs are a more important determinant of bargaining power in bond purchases than in bond sales (e.g., Cuny, 2018). In Table 5, we find that the coefficient on the interaction term *Treated * Post* is negative and significant for bond purchases but insignificant for bond sales. This result provides additional evidence that information acquisition costs contribute to our main results, since the effects we document are concentrated in those observations where information acquisition costs play a more important role.

Lastly, we conduct a set of cross-sectional tests that employ different proxies of the issuers' information environment. The intent is to identify the incremental effect for issuers with ex-ante inferior sources of information, as it is among these issuers where the incremental reduction in information acquisition costs for investors is likely to be the greatest. We use three proxies for issuers' information environment. The first cross-sectional variable is a binary variable that partitions the sample based on whether state law requires municipalities to be audited. *Mandatory*

Audit is an indicator that takes value 1 for bonds issued in a state where audit is mandated (Barber and Gore, 2008), and 0 otherwise. Consistent with prior studies, we expect that states that do not require mandatory audits will have issuers with poorer information environments, and hence issuers where the reduction in information acquisition costs is greater. The second cross-sectional variable identifies those issuers that did not post financial information on EMMA (e.g., an Annual Comprehensive Financial Report) during the sample period. Similarly, we expect issuers without posted financial information to experience a greater reduction in information acquisition costs. The third cross-section variable identifies those issuers that earned a GFOA certificate for financial reporting excellence. The GFOA certificate is awarded to municipal governments that satisfy financial reporting standards set by GFOA including GAAP compliance and additional non-GAAP disclosures. Compared to issuers that earn GFOA certificates, issuers that do not will have a lower quality of financial reporting and a worse information environment. For each of these three proxies, we separate our sample into two subsamples and conduct separate analyses to estimate the dissemination effect for issuers with 'worse' versus 'better' information environments.

Table 6 Panel A presents the mandatory audit results. Columns (1) and (2) examine and show the effect on *Markup* exists only for issuers in states that do not mandate audits (Coefficient on *Treated * Post* = -7.346 and t-statistic = 3.020 for non-mandatory audit sample; and coefficient = -1.324 and t-statistic = 0.344 for mandatory audit sample). An F-test shows the difference in the coefficients on *Treated * Post* is significant at the 1% level. In economic terms, bonds issued in non-mandatory audit states experience a relative decline in *Markup* of 7.3%. These results indicate that the main effects we document are concentrated in issuers that do not have mandatory audits, suggesting that the impact of dissemination on transaction costs is greater for issuers with a poor information environment.

Table 6 Panel B presents the GFOA certificate results. We obtained the list of 2013 GFOA certificate award recipients from the Governmental Finance Officers Association website.[16] We then use fuzzy match and hand-clean to identify the issuers who received the certificate. Columns (1) and (2) examine and show that the dissemination effect on *Markup* exists only for issuers with no GFOA certificate (Coefficient on *Treated * Post* = -5.574 and t-statistics = 2.262 for non-GFOA sample; and coefficient = -4.742 and t-statistics = 0.989 for GFOA sample). An F-test shows the difference in the coefficients on *Treated * Post* is significant at 1% level. Once again, these results indicate that the impact of dissemination on transaction costs is greater for issuers with a poor information environment.

Lastly, Table 6 Panel C presents the results when we separate the sample based on whether the issuer posts an Annual Comprehensive Financial Report (ACFR) on EMMA over the sample period. Columns (1) and (2) examine and show that the effect on *Markup* is much stronger for issuers that did not file a ACFR (Coefficient on *Treated * Post* = -23.426 and t-statistic = 4.002 for non- ACFR sample; and coefficient =-3.085 and t-statistic =1.287 for ACFR sample). An F-test shows the difference in the coefficients on *Treated * Post* is significant at 1% level. Overall, each of the three cross-sectional tests indicates that the impact of dissemination on transaction costs is greater for issuers with a poor information environment.

6. Additional Analyses

In this section, we provide additional support for our conclusions by documenting similar effects across three proxies for municipal bond market trading activity and across several alternative samples. We also examine the later addition of Moody's ratings to EMMA.

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[16] We use GFOA certificate award for 2013 fiscal year because that's the earliest year that we can get.

6.1 Trading Activity

We confirm that our findings persist when we use several alternative but intuitive measures of transaction costs: *Turnover* is defined as the ratio of monthly trading volume to the bond's amount outstanding. *Count of Trades* is the log of the number of trades occurring in a given month. Dollar Trades (*$Trades*) is defined as the natural logarithm of the dollar value of trades. Consistent with Schwert (2017), we estimate each of these measures on a monthly basis for each bond in our sample due to the general lack of liquidity in the municipal bond market. All variables are winsorized at the 1st and 99th percentiles to mitigate the impact of outliers. Summary statistics for the liquidity and trading activity variables are presented in Table 7 Panel A. For example, the mean count of trades on a given month is 4.546 trades while the median count is 2 trades per bond per month.

The multivariate regression results are presented in Table 7 Panel B. Given the different research design from equation (1), we include bond and year-month fixed effects, and standard errors are clustered by bond and year-month. We find a significant and positive increase in *Turnover* (Coefficient on *Treated * Post* = 0.023 and t-statistic = 3.276 in column (1)), *Count of Trades* (Coefficient on *Treated * Post* = 1.087 and t-statistic = 2.912 in column (2)), and *$Trades* (Coefficient on *Treated * Post* =0.206 and t-statistics = 3.432 in column (3)). The results collectively suggests that dissemination of credit ratings have a significant impact on reducing transaction costs in the municipal bond market.

6.2 Alternative Matching Procedure

We confirm that our results are robust to using nearest neighbor matching. This approach minimizes the (*Mahalanobis*) distance between a vector of observed covariates across treatment and control issuers to identify the best control firm for each treated firm. We select one matched control observation for each treated observation based on the observed bond characteristics at issuance. The matching variables are the same as used in the entropy matching technique: the bond's credit rating at issuance or the average of the S&P and Fitch ratings if it is rated by both agencies (*Avg Rating*), the percent of retail trading on the bond before the regulatory change (*Avg Retail*), the bond yield at issue (*Yield*), whether the bond is callable (*Callable*), the issue amount (*Log (Amount)*), the issue's coupon rate (*Coupon*), the maturity of the bond (*Maturity*) and whether a bond is issued by a state, city or county government (*State_City_County*).

The nearest neighbor matching procedure produces a smaller sample compared to the entropy matching technique, with 1,703 treatment issuers and 1,498 control issuers. The estimator allows control issuers to serve as matches more than once, which reduces the estimation bias but increases the variance when compared to matching without replacement (e.g., Almeida et al., 2016). The results in Table 8 are very similar to those in Table 3. The matching procedure produces two groups of observations with no statistical differences across bond characteristics. The univariate analysis produces a difference-in-differences estimate that is significantly negative and reflects an economically meaningful decline of about 13.9 basis points. In addition, the coefficients on *Treated * Post* interaction term are negative and significant in each multivariate specification at the 5% level.

6.3 Ratings Recalibration

In March 2010 Moody's and Fitch both recalibrated their U.S. Municipal Ratings Scale (a scale that was only used to rate only municipal bonds) to their Global Ratings Scale (a scale that was used to rate corporate, sovereign, and structured finance securities). This change was announced in late 2008 in response to regulatory pressure that arose in part because the default rates on municipal bonds were much lower than corporate issues with the same rating. Both agencies stressed in their press releases that the recalibration was not an improvement in underlying credit quality, and in fact any rating changes were purely mechanical. Since both Fitch (Treatment) and Moody's (Control) adopted this change at the same time, the potential effect is limited in our sample. Nonetheless, we ensure that our conclusions are unaffected by eliminating bonds that were impacted by the Moody's and Fitch recalibration[17]. In untabulated analyses, we continue to find results that are very similar to those in Table 3. The coefficients on *Treated * Post* are negative and significant at 1% level (coefficient =-7.573 and t-statistic = 3.059 in column (1); coefficient =-6.563 and t-statistic = 2.751 in column (2); coefficient =-5.824 and t-statistic = 2.805 in column (3)), indicating that treated bonds have an approximately 5.8% to 7.6% relative decline in *Markup*.

6.4 Dissemination of Moody's Ratings via EMMA

We provide further support for our conclusion on the municipal market effects of credit rating dissemination using the later addition of Moody's ratings to EMMA on June 30th, 2015. This update allows us to create a difference-in-difference research design to supplement our main tests. Under this new specification, our control group are the issues that were previously rated by either or both of S&P and Fitch, and our treatment group are the issues that were previously only rated by Moody's. In other words, our treatment and control groups are switched. Intuitively, the dissemination of Moody's ratings should provide a differential reduction in transaction costs for those issues rated by Moody's when compared to issues rated by either or both S&P and Fitch. However, it is likely that the overall differential effects are weaker, as investors already had ratings information for the other agencies. In simple terms, the first shock exploited in our main analyses

[17] We identified bonds whose ratings were adjusted by recalibration using issuers' event-announcements posted on EMMA.

likely led to trading that was focused on issues rated by S&P and Fitch, whereas the second shock where Moody's ratings were added may have spread trading across all rated bonds. We follow a similar research design as equation (1) as below:

$$Markup_{b,t} = \alpha + \beta_1 Treated\_Moody's_{b,t} + \beta_2 Treated\_Moody's_{b,t} * Post\_Moody's_{,b,t} + \sum \gamma_j Controls + Fixed\ Effects + \varepsilon_{i,t} \qquad (3)$$

Treated_Moody's is an indicator variable equal to one for bonds rated by only Moody's and equal to zero for bonds rated by either or both S&P and Fitch. *Post_Moody's* is an indicator variable that equals to one for transactions that take place after MSRB started posting ratings from Moody's on June 30th 2015, and zero otherwise. The standalone *Post_Moody's* variable is subsumed by the fixed effects we employ. While we use the same bonds that are in the main sample (i.e., bonds issued before credit ratings were made available on EMMA), we use a sample period that begins in June 2012 and ends in December 2016. Our main coefficient of interest is β_2, the coefficient on the interaction term *Treated_Moody's * Post_Moody's*, which captures the differential change in *Markup* across the treatment bond-years (i.e., Moody's ratings disseminated via EMMA) and control firm-years (i.e., S&P and Fitch ratings disseminated via EMMA). A negative value for β_3 indicates that the dissemination of Moody's ratings differentially lowers transaction costs for Moody's rated bonds.

Table 9 presents the results of Moody's dissemination. As with Table 3, the dependent variable is *Markup*. Both columns (1) and (2) include bond and trade date fixed effects, and column (2) includes trade type fixed effects that indicate whether the transaction is a bond purchase or bond sale. All specifications cluster standard errors by bond and trade date. In both columns, the coefficients on the *Treated_Moody's * Post_Moody's* interaction term are significantly negative (coefficient = -5.741 and t-statistic = 2.023 in column (1) and coefficient = -4.867, t-statistic =

2.038 in column (2)). These results indicate that investors trading in bonds rated by Moody's experience a differential decrease in *Markup* compared to those trading in bonds rated by S&P and Fitch. An important caveat to this analysis is that it is based on relatively light trading data, as transactions in municipal bonds decline over time and the bonds in this analysis were all issued prior to the S&P and Fitch dissemination event. In addition, the results are not as strong on either an economic basis or a statistical significance basis when compared with our main results. Therefore, while the results are reassuring, they should be interpreted with caution.

7. Conclusion

We examine how the dissemination of credit ratings affects transaction costs in the municipal bond market. We conclude that dissemination lowers transaction costs in part through its lowering of information acquisition costs. Our analysis shows a differential decline in transaction costs overall, and that this decline is primarily a product of retail trades, bond purchases, and trading in bonds of issuers with worse information environments. Collectively, our results have implications for the provision of information on EMMA. In particular, our results suggest that the MSRB should continue to focus its efforts on ensuring that information is provided on EMMA in a timely manner by municipal bond market participants in order to ensure a level playing field across different types of investors.

The effect of dissemination on other municipal bond market outcomes, such as trading profits, are not obvious from our study. While lower transaction costs may typically be expected to increase trading profits, it is also possible that certain traders may rely to their detriment on credit ratings. In this case, increased dissemination of credit ratings could exacerbate reliance and potentially lead to inferior bond selection and investment performance. In a recent working paper, deHaan et al. (2021) provide evidence consistent with over-reliance in a study of corporate bonds, where retail investors tend to select bonds in advance of defaults and downgrades thus leading to inferior investment performance. We leave the investigation of such effects in the municipal bond market to future research.

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Appendix A: Variable Definitions

Variable	Description	Data Source
Dependent Variables		
Markup	$TradeSign_{b,t} * 10{,}000 * \text{Log}\left[\frac{\text{InvestorPrice}_{b,t}}{\text{AvgInterdealerPrice}_{b,t}}\right]$ Trade Sign is a buy/sell indicator equal to "1" if the trade is a customer purchase, "-1" if the trade is a customer sale, and "0" if the trade is inter-dealer. The InvestorPrice is the purchase (sale) price of bond b on date t if at least one inter-dealer trade occurs on the same day. The *AvgInterdealerPrice* is the average price of all inter-dealer transactions in bond b on date t.	Constructed, MSRB
Turnover	Ratio of monthly trading volume to the outstanding bond amount	Constructed
Count of Trades	The natural log of the number of trades in a month	Constructed
$Trade	The natural log of the total dollar amount of trades per month	Constructed
Treatment Indicators		
Treated	Indicator set to one for bonds that are rated by S&P and Fitch but not by Moody's, and to zero for bonds that are rated by Moody's but neither S&P nor Fitch	
Post	Indicator set to one for trades that take place after MSRB started posting ratings from S&P and Fitch Ratings on November 21st 2011	
Treated_Moody’s	Indicator set to one for bonds that are rated by Moody's but neither S&P nor Fitch, and to zero for bonds that are rated by S&P and Fitch but not by Moody's	
Post_Moody’s	Indicator set to one for trades that take place after MSRB started posting Moody’s ratings on June30th 2015	
Control and Cross-sectional Variables		
Duration	Time remaining to maturity	MSRB
Log(TradeSize)	The natural log of the trade value of the transaction.	Constructed
Log(AggTrades)	The natural log of the trade value of all transactions in a bond during a given month	Constructed
Inventory	An indicator equals one if a purchase (sale) does not follow (precede) a sale (purchase) within one day of the trade date	Constructed
Log(StateGDP)	The natural log of State GDP	Constructed
Log(PersIncome)	The natural log of state personal income level	Constructed
Retail	Indicator variable set to one for trades with par values under $100,000	Constructed
Bond Purchase	Indicator variable set to one if the transaction is a bond purchase	MSRB
Mandatory Audit	Indicator variable set to one for issuers in states that the state law requires mandate audits, 0 for issuers in states that do not mandate audits	

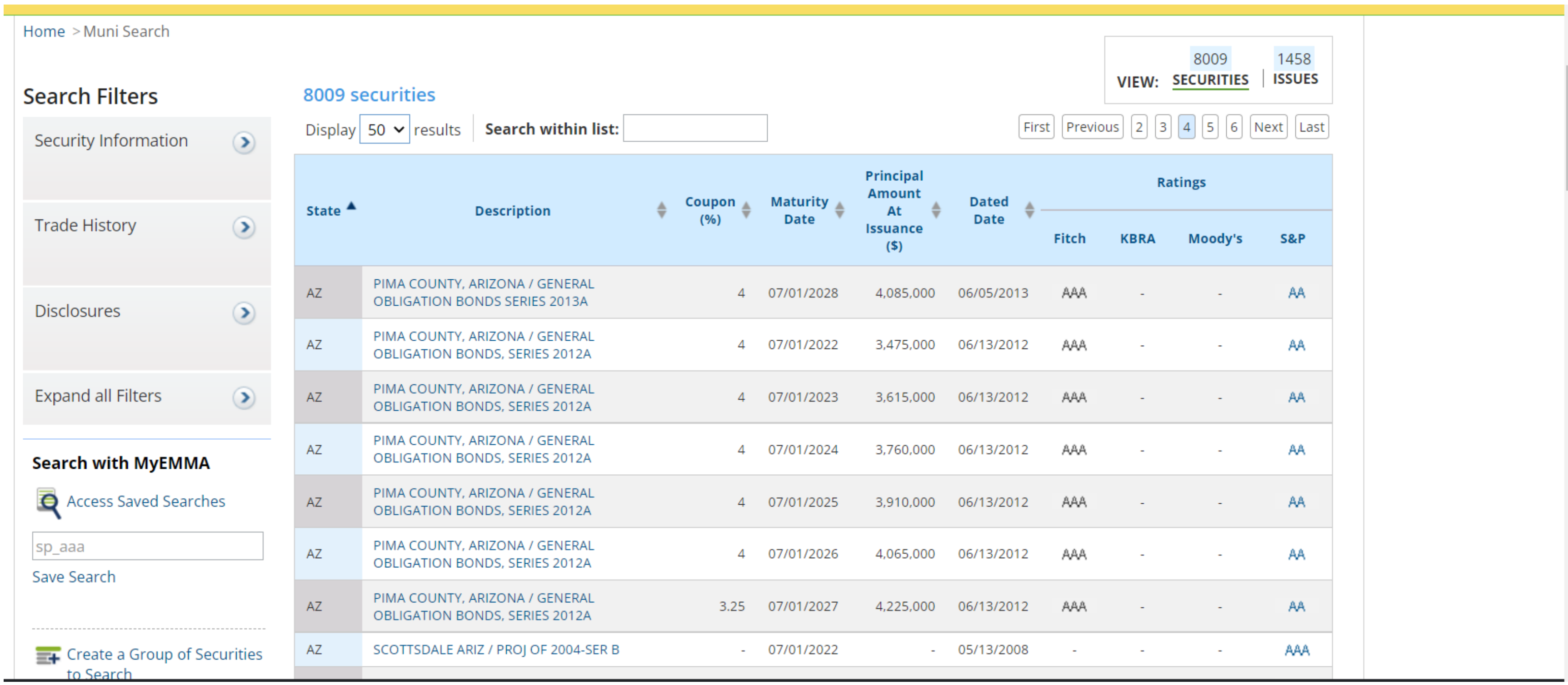
Variable	Description	Data Source
GFOA Award	Indicator variable set to one for issuers earned 2013 GFOA certificate, 0 for issuers did not receive GFOA certificate	GFOA Website
ACFR	Indicator variable set to one for issuers that have posted an Annual Comprehensive Financial Report (ACFR) on EMMA at least once, and zero for issuers that never posted ACFR on EMMA during the sample period	EMMA Website
Bond Characteristics		
Callable	Indicator variables set to one for callable bonds	Bloomberg
Yield	Yield of bond at issuance (in bps)	Bloomberg
Maturity	Years to Maturity	Bloomberg
Coupon	Bond coupon rate	Bloomberg
Log(Amount)	Natural logarithm of issue amount	Bloomberg
Insured	Indicator variables set to one for insured bonds, 0 otherwise	Bloomberg
Avg Retail	Average percent of retail trades in a given month	Constructed
Avg Rating	Equal to either the S&P, Fitch or Moody's rating, or the average of the Fitch and S&P rating if the issue is rated by both rating agencies	Bloomberg
State_City_County	Indicator variable set to one if a bond is issued by a state, city or county, 0 if by other governmental entities.	Bloomberg

Appendix B: Ratings Scale

The table describes categories for credit ratings, as well as the numerical scale used in the paper. Multiple numerical values for a single rating level represents the number assigned to ratings with a + qualifier, no qualifier, and a − qualifier, respectively for S&P and Fitch, and with a 1, 2 or 3 qualifier for Moody's. The source for ratings definitions is S&P Ratings Definitions from November 20, 2014 (http://www.standardandpoors.com/).

S&P Rating	**Moody's Rating**	**Fitch Rating**	**Assigned Value**	**Group Description**	**S&P Ratings Definition**
AAA	Aaa	AAA	1	Prime	An obligation rated 'AAA' has the highest rating assigned by S&P. The obligor's capacity to meet its financial commitment on the obligation is extremely strong.
AA	Aa	AA	2, 3, 4	High grade	An obligation rated 'AA' differs from the highest-rated obligations only to a small degree. The obligor's capacity to meet its financial commitment on the obligation is very strong.
A	A	A	5, 6, 7	Upper medium grade	An obligation rated 'A' is somewhat more susceptible to the adverse effects of changes in circumstances and economic conditions than obligations in higher-rated categories. However, the obligor's capacity to meet its financial commitment on the obligation is still strong.
BBB	Baa	BBB	8, 9, 10	Lower medium grade	An obligation rated 'BBB' exhibits adequate protection parameters. However, adverse economic conditions or changing circumstances are more likely to lead to a weakened capacity of the obligor to meet its financial commitment on the obligation.
BB	Ba	BB	11, 12, 13	Non-investment grade speculative	An obligation rated 'BB' is less Vulnerable to nonpayment than other speculative issues. However, it faces major ongoing uncertainties or exposure to adverse business, financial, or economic conditions which could lead to the obligor's inadequate capacity to meet its financial commitment on the obligation.
B	B	B	14, 15, 16	Highly speculative	An obligation rated 'B' is more Vulnerable to nonpayment than obligations rated 'BB', but the obligor currently has the capacity to meet its financial commitment on the obligation. Adverse business, financial, or economic conditions will likely impair the obligor's capacity or willingness to meet its financial commitment on the obligation.
CCC	Caa	CCC	17, 18, 19	Substantial risks	An obligation rated 'CCC' is currently Vulnerable to nonpayment, and is dependent upon favorable business, financial, and economic conditions for the obligor to meet its financial commitment on the obligation. In the event of adverse business, financial, or economic conditions, the obligor is not likely to have the capacity to meet its financial commitment on the obligation.
CC	Ca	CC	20	Extremely speculative	An obligation rated 'CC' is currently highly vulnerable to nonpayment. The 'CC' rating is used when a default has not yet occurred, but S&P expects default to be a virtual certainty, regardless of the anticipated time to default.
C	C	C	21	Default imminent	An obligation rated 'C' is currently highly vulnerable to nonpayment, and the obligation is expected to have lower relative seniority or lower ultimate recovery compared to obligations that are rated higher.

Figure 1: EMMA Display of Ratings Information



State	Description	Coupon (%)	Maturity Date	Principal Amount At Issuance ($)	Dated Date	Ratings: Fitch	KBRA	Moody's	S&P
AZ	PIMA COUNTY, ARIZONA / GENERAL OBLIGATION BONDS SERIES 2013A	4	07/01/2028	4,085,000	06/05/2013	AAA	-	-	AA
AZ	PIMA COUNTY, ARIZONA / GENERAL OBLIGATION BONDS, SERIES 2012A	4	07/01/2022	3,475,000	06/13/2012	AAA	-	-	AA
AZ	PIMA COUNTY, ARIZONA / GENERAL OBLIGATION BONDS, SERIES 2012A	4	07/01/2023	3,615,000	06/13/2012	AAA	-	-	AA
AZ	PIMA COUNTY, ARIZONA / GENERAL OBLIGATION BONDS, SERIES 2012A	4	07/01/2024	3,760,000	06/13/2012	AAA	-	-	AA
AZ	PIMA COUNTY, ARIZONA / GENERAL OBLIGATION BONDS, SERIES 2012A	4	07/01/2025	3,910,000	06/13/2012	AAA	-	-	AA
AZ	PIMA COUNTY, ARIZONA / GENERAL OBLIGATION BONDS, SERIES 2012A	4	07/01/2026	4,065,000	06/13/2012	AAA	-	-	AA
AZ	PIMA COUNTY, ARIZONA / GENERAL OBLIGATION BONDS, SERIES 2012A	3.25	07/01/2027	4,225,000	06/13/2012	AAA	-	-	AA
AZ	SCOTTSDALE ARIZ / PROJ OF 2004-SER B	-	07/01/2022	-	05/13/2008	-	-	-	AAA

Table 1: Sample Selection

Sample Selection Criteria	Number of Trades	Number of Unique Bonds	Number of Unique Issuers
Bonds issued between July 2009 and Oct 2011, and traded between July 2009 and June 2014	12,247,937	309,400	16,665
Liquidity Restrictions:			
Bond without dealer-matched customer purchase or sell	7,460,454	119,311	3,235
Bonds not traded by both retail and institutional investors in both the pre- and post-period	1,820,059	165,450	8,635
Bond Type Restrictions:			
Bonds rated by both S&P and Moody's or by both Fitch and Moody's	2,455,131	18,011	2,197
Unrated bonds	32,015	250	127
Bond issues with missing information	46,925	51	24
Final Sample	**433,353**	**6,327**	**2,447**

This table presents sample selection process. Treated sample includes bonds rated by S&P, Fitch or both, and control sample includes bonds rated only by Moody's.

Table 2: Summary Statistics

Variable	*N (Trades)*	*Mean*	*Std. Dev.*	*P25*	*Median*	*P75*
Main Variables:						
Treated	433,353	0.719	0.449	0.000	1.000	1.000
Post	433,353	0.335	0.472	0.000	0.000	1.000
Dependent Variable:						
Markup	433,353	134.953	97.233	41.378	136.011	209.523
Control Variables:						
Log(AggTrades)	433,353	14.219	2.246	12.468	14.389	16.014
Duration	433,353	19.139	8.303	13.000	20.000	26.000
Log(TradeSize)	433,353	10.285	1.264	9.210	10.127	10.82
Inventory	433,353	0.675	0.468	0.000	1.000	1.000
Log(StateGDP)	433,353	13.160	0.979	12.489	13.162	14.079
Log(PersIncome)	433,353	10.657	0.147	10.547	10.647	10.745
Cross-sectional Variables:						
Retail	433,353	0.867	0.339	1.000	1.000	1.000
Bond Purchase	433,353	0.821	0.383	1.000	1.000	1.000
GFOA Award	433,353	0.132	0.338	0.000	0.000	0.000
Mandatory Audit	433,353	0.287	0.452	0.000	0.000	1.000
ACFR	433,353	0.913	0.282	1.000	1.000	1.000

Table 2 presents descriptive statistics for the variables used in the regression analyses for the full sample, which consists of 433,353 daily trades representing 6,327 unique bond issuances from 2,447 issuers for the period 2009-2014. All variables are defined in Appendix A.

Table 3: The Effect of Dissemination on Markup

Panel A: Entropy Balanced Matching Procedure

	Pre-Matching			*Post-Matching*		
	Treated (a)	Control (b)	Diff (a-b)	Treated (c)	Control (d)	Diff (c-d)
Callable	0.585	0.561	0.024*	0.585	0.585	0.000
Yield	4.264	3.899	0.365**	4.264	4.261	0.003
Maturity	2.506	2.424	0.082**	2.506	2.505	0.001
Coupon	4.399	4.091	0.308**	4.399	4.398	0.001
Log(Amount)	14.968	14.645	0.323**	14.97	14.97	0.000
Insured	0.144	0.114	0.03***	0.144	0.144	0.000
Avg Retail	0.708	0.716	-0.008	0.708	0.708	0.000
Avg Rating	3.498	3.507	-0.009	3.498	3.498	0.000
State_City_County	0.267	0.322	0.055**	0.267	0.268	-0.001
Number of Bonds	4,244	2,083		4,244	2,083	

Panel B: Univariate Results

		Markup		
		Pre (a)	Post (b)	(b)-(a)
Treated=1	(i)	157.848 *N=208,206*	92.717 *N=103,467*	(65.131)***
Treated=0	(ii)	154.754 *N=80,152*	108.371 *N=41,528*	(46.383)***
	(i)-(ii)	3.094	(15.654)***	(18.748)***

Table 3 (continued)

Panel C: Multivariate Results

	(1)	(2)	(3)
Dependent variable	*Markup*	*Markup*	*Markup*
Treated * Post	-7.090***	-6.191***	-5.730***
	(2.865)	(2.585)	(2.734)
Control Variables:			
Log(AggTrades)	7.110***	4.474***	2.145***
	(20.559)	(11.738)	(4.696)
Duration	9.981**	8.467**	8.909**
	(2.251)	(2.175)	(2.263)
Log(TradeSize)	-11.608***	-9.203***	-11.336***
	(14.789)	(16.345)	(18.801)
Inventory	-7.930***	-12.570***	-10.987***
	(7.074)	(10.855)	(7.094)
Log(StateGDP)	-16.734	-6.954	-57.034
	(0.277)	(0.119)	(0.928)
Log(PersIncome)	51.668	55.089	38.051
	(0.888)	(0.996)	(0.697)
Retail	24.630***	20.356***	23.503***
	(14.729)	(11.064)	(13.784)
Fully Interacted	No	No	yes
Adj R-squared	0.388	0.418	0.423
Number of Trades	433,353	433,353	433,353
Bond Fixed Effects	Yes	Yes	Yes
Trade Date Fixed Effects	Yes	Yes	Yes
Trade Type Fixed Effects	No	Yes	Yes

This table reports analyses of the effect of dissemination on *Markup*. Panel A reports univariate results and Panel B reports multivariate results for the differential effect of S&P and Fitch rated firms (Treatment group) over Moody's rated issuers (Control group) on *Markup*. Column (2) is a fully interacted specification that interacts *Post* with each of the control variables. All columns present OLS coefficient estimates, and t-statistics based on robust standard errors clustered by bond and trade date. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% p-levels (two-tailed), respectively.

Table 4: Differential Effect for Retail versus Institutional Investors

	(1)	(2)
Dependent variable	*Markup-Retail*	*Markup-Institutional*
Treated * Post	-7.617***	1.650
	(2.741)	(0.747)
F-stat	P = 0.000	
Control Variables:		
Log(AggTrades)	4.255***	-0.654
	(9.020)	(1.198)
Duration	9.784**	2.083
	(2.528)	(0.866)
Log(TradeSize)	-5.809***	-17.075***
	(11.190)	(20.885)
Inventory	-12.372***	-11.222***
	(10.526)	(8.719)
Log(StateGDP)	25.627	-106.525**
	(0.397)	(2.411)
Log(PersIncome)	26.359	180.359***
	(0.418)	(3.692)
Adj R-squared	0.400	0.344
Number of Trades	375,884	57,427
Bond Fixed Effects	Yes	Yes
Trade Date Fixed Effects	Yes	Yes
Trade Type Fixed Effects	Yes	Yes

This table reports analyses of the effect of dissemination on *Markup* separately for retail versus institutional investors. Column (1) restricts to retail trades and Column (2) restricts to institutional trades. All columns present OLS coefficient estimates, and t-statistics based on robust standard errors clustered by bond and trade date. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% p-levels (two-tailed), respectively.

Table 5: Differential Effect for Bond Sales versus Bond Purchases

Dependent variable	(1) *Markup -Purchase*	(2) *Markup -Sale*
Treated * Post	-5.957**	2.069
	(2.110)	(0.780)
F-stat	P = 0.000	
Control Variables:		
Log(AggTrades)	3.833***	-0.203
	(7.941)	(0.411)
Duration	5.050	13.656***
	(1.545)	(3.178)
Log(TradeSize)	-6.991***	-9.087***
	(9.958)	(7.629)
Inventory	-13.328***	-13.631***
	(8.757)	(7.479)
Retail Trading	26.474***	-1.335
	(13.605)	(0.665)
Log(StateGDP)	-20.563	-131.313**
	(0.317)	(2.169)
Log(PersIncome)	-37.931	183.055***
	(0.549)	(2.677)
Adj R-squared	0.404	0.259
Number of Trades	355,934	76,798
Bond Fixed Effects	Yes	Yes
Trade Date Fixed Effects	Yes	Yes
Trade Type Fixed Effects	Yes	Yes

This table reports analyses of the effect of dissemination on Markup separately for bond purchases versus bond sales. Column (1) restricts to bond purchases and Column (2) restricts to bond sales. All columns present OLS coefficient estimates, and t-statistics based on robust standard errors clustered by bond and trade date. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% p-levels (two-tailed), respectively.

Table 6: Cross-sectional Effect based on Information Environment

Panel A – Conditional on Mandatory Audit States

	(1)	(2)
Dependent variable	*Markup- No Mandatory Audit*	*Markup- Mandatory Audit*
Treated * Post	-7.346***	-1.324
	(3.020)	(0.344)
F-stat	P= 0.000	
Control Variables:		
Log(AggTrades)	4.534***	4.537***
	(10.573)	(7.760)
Duration	6.768	-1.079
	(1.509)	(0.205)
Log(TradeSize)	-8.669***	-9.994***
	(13.163)	(14.220)
Inventory	-14.100***	-10.933***
	(9.798)	(7.226)
Retail Trading	20.341***	20.520***
	(9.254)	(9.680)
Log(StateGDP)	57.168	-424.001***
	(1.171)	(4.425)
Log(PersIncome)	-0.924	29.955
	(0.015)	(0.328)
Adj R-squared	0.417	0.463
Number of Trades	308,870	124,483
Bond Fixed Effects	Yes	Yes
Trade Date Fixed Effects	Yes	Yes
Trade Type Fixed Effects	Yes	Yes

Table 6 (continued)

Panel B – Conditional on GFOA Awards

	(1)	(2)
Dependent variable	*Markup- No GFOA Award*	*Markup- GFOA Award*
Treated * Post	-5.574**	-4.742
	(2.262)	(0.989)
F-stat	P=0.000	
Control Variables:		
Log(AggTrades)	4.676***	3.667***
	(11.862)	(4.883)
Duration	10.218**	-20.978
	(2.419)	(1.117)
Log(TradeSize)	-9.113***	-9.464***
`	(15.004)	(10.652)
Inventory	-12.629***	-12.812***
	(10.131)	(6.571)
Retail Trading	20.796***	17.352***
	(10.379)	(7.103)
Log(StateGDP)	-35.780	157.546**
	(0.587)	(2.099)
Log(PersIncome)	96.039*	-259.988**
	(1.707)	(2.279)
Adj R-squared	0.410	0.536
Number of Trades	376,252	57,098
Bond Fixed Effects	Yes	Yes
Trade Date Fixed Effects	Yes	Yes
Trade Type Fixed Effects	Yes	Yes

Table 6 (continued)

Panel C – Conditional on ACFR Issuance

	(1)	(2)
Dependent variable	*Markup-* *No ACFR*	*Markup-* *ACFR*
Treated * Post	-23.426***	-3.085
	(4.002)	(1.287)
F-stat	P= 0.000	
Control Variables:		
Log(AggTrades)	4.517***	4.378***
	(4.613)	(10.576)
Duration	19.055**	4.672
	(2.394)	(1.304)
Log(TradeSize)	-10.314***	-8.847***
	(9.905)	(14.814)
Inventory	-21.320***	-11.275***
	(6.230)	(11.823)
Retail Trading	24.337***	19.843***
	(4.765)	(10.051)
Log(StateGDP)	-228.528**	44.540
	(2.397)	(0.886)
Log(PersIncome)	80.749	22.962
	(0.645)	(0.427)
Adj R-squared	0.488	0.420
Number of Trades	37,653	395,691
Bond Fixed Effects	Yes	Yes
Trade Date Fixed Effects	Yes	Yes
Trade Type Fixed Effects	Yes	Yes

This table reports analyses of the effect of dissemination on *Markup* separately in two groups based on proxies for the issuers information environment: for Mandatory audit states vs Non mandatory audit states (Panel A), for Bonds with GFOA awards vs Bonds with no GFOA awards (Panel B), and for Bonds that issued an Annual Comprehensive Financial Report (ACFR) vs Bonds with no ACFR Issuance on EMMA (Panel C). All columns present OLS coefficient estimates and t-statistics based on robust standard errors clustered by bond and trade date. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% p-levels (two-tailed), respectively.

Table 7: Effect on Trading Activity

Panel A: Trading Activity Variables

Variable	*N (Bond-Month)*	*Mean*	*Std. Dev.*	*P25*	*Median*	*P75*
Turnover	71,149	0.115	0.217	0.005	0.02	0.097
Count Trades	71,149	4.546	7.294	1	2	4
Log ($ Trades)	71,149	11.714	1.606	10.597	11.513	12.821

Panel B: Effect on Trading Activity

Dependent variable	(1) *Turnover*	(2) *Count of Trades*	(3) *$Trades*
Treated * Post	0.023***	1.087***	0.206***
	(3.276)	(2.912)	(3.432)
Control Variables:			
Post	-0.046***	-2.005***	-0.574***
	(6.869)	(7.041)	(9.986)
Duration	0.005	0.073	0.035
	(0.570)	(0.312)	(0.910)
Inventory	-0.046***	-2.733***	-0.838***
	(16.489)	(14.578)	(39.174)
Log(StateGDP)	0.131	-0.253	0.526
	(0.923)	(0.043)	(0.415)
Log(PersIncome)	-0.228	5.811	-0.371
	(1.528)	(0.911)	(0.392)
Retail	-0.191***	1.073***	-2.438***
	(39.445)	(6.237)	(111.602)
Adjusted R-squared	0.462	0.270	0.478
Observations (Bond-Year-Month)	71,149	71,149	71,149
Bond Fixed Effects	Yes	Yes	Yes
Year-Month Fixed Effects	Yes	Yes	Yes

This table reports analyses of the effect of dissemination on three alternative measures of municipal bond market liquidity. Panel A shows descriptive statistics for the trading activity variables aggregated at the bond-month level. Panel B shows the regression results. All specifications are executed at the bond-month level. All columns present OLS coefficient estimates, and t-statistics based on robust standard errors clustered by bond and year-month. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% p-levels (two-tailed), respectively.

Table 8: The Effect of Dissemination on Markup using Nearest Neighbor Matched Sample

Panel A: Nearest Neighbor Matching Procedure

	Pre-Matching			*Post-Matching*		
	Treated (a)	Control (b)	Diff (a-b)	Treated (c)	Control (d)	Diff (c-d)
Callable	0.585	0.561	0.024*	0.598	0.585	0.013
Yield	4.264	3.899	0.365**	3.943	3.853	0.09*
Maturity	2.506	2.424	0.082**	2.468	2.44	0.028
Coupon	4.399	4.091	0.308**	4.305	4.199	0.106*
Log(Amount)	14.968	14.645	0.323**	14.738	14.671	0.067*
Insured	0.144	0.114	0.03***	0.027	0.037	-0.01
Avg Retail	0.708	0.716	-0.008	0.735	0.736	-0.001
Avg Rating	3.498	3.507	-0.009	3.479	3.418	0.061
State_City_County	0.267	0.322	0.055**	0.315	0.328	-0.013
Number of Bonds	4,244	2,083		1,703	1,498	

Panel B: Univariate Results

		Markup		
		Pre (a)	Post (b)	(b)-(a)
Treated=1	(i)	157.036 *N=70,328*	86.937 *N=33,046*	(70.099)***
Treated=0	(ii)	149.547 *N=56,850*	93.319 *N=28,602*	(56.228)***
	(i)-(ii)	7.489*	(6.382)**	(13.871)***

Table 8 (continued)

Panel C: Multivariate Results

Dependent variable	(1) *Markup*	(2) *Markup*	(3) *Markup*
Treated * Post	-7.053***	-6.069**	-5.757**
	(2.749)	(2.373)	(2.364)
Control Variables:			
Log(AggTrades)	8.030***	4.839***	2.396***
	(18.863)	(11.732)	(4.607)
Duration	4.206	7.089	0.527
	(0.499)	(0.742)	(0.059)
Log(TradeSize)	-13.231***	-10.469***	-12.330***
	(27.079)	(22.454)	(22.400)
Inventory	-6.584***	-12.082***	-8.676***
	(5.622)	(10.691)	(5.094)
Log(StateGDP)	-7.690	3.528	31.010
	(0.161)	(0.076)	(0.615)
Log(PersIncome)	-65.796	-49.389	-69.890
	(1.123)	(0.842)	(1.207)
Retail	20.419***	15.488***	19.542***
	(16.831)	(13.928)	(13.562)
Fully Interacted	No	No	Yes
Adj R-squared	0.423	0.462	0.468
Number of Trades	188,826	188,826	188,826
Bond Fixed Effects	Yes	Yes	Yes
Trade Date Fixed Effects	Yes	Yes	Yes
Trade Type Fixed Effects	No	Yes	Yes

This table reports analyses of the effect of dissemination on *Markup* using nearest neighbor matched sample. Panel A reports univariate results and Panel B reports multivariate results for the differential effect of S&P and Fitch rated firms (Treatment group) over Moody's rated issuers (Control group) on *Markup*. Column (2) is a fully interacted specification that interacts *Post* with each of the control variables. All columns present OLS coefficient estimates, and t-statistics based on robust standard errors clustered by bond and trade date. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% p-levels (two-tailed), respectively.

Table 9: Effect of Moody's Rating Dissemination on Transaction Costs

	(1)	(2)
Dependent variable	*Markup*	*Markup*
Treated_Moody's * Post_Moody's	-5.741**	-4.867**
	(2.023)	(2.038)
Control Variables:		
Log(AggTrades)	5.432***	3.983***
	(8.205)	(7.451)
Duration	0.748	1.017
	(0.376)	(0.542)
Log(TradeSize)	-6.694***	-5.241***
	(7.487)	(7.266)
Inventory	-12.342***	-13.403***
	(10.457)	(13.864)
Log(StateGDP)	-0.229	-7.670
	(0.004)	(0.174)
Log(PersIncome)	-6.629	7.001
	(0.109)	(0.138)
Retail	15.780***	13.167***
	(8.593)	(8.996)
Adj R-squared	0.225	0.280
Number of Trades	218,656	218,656
Bond Fixed Effects	Yes	Yes
Trade Date Fixed Effects	Yes	Yes
Trade Type Fixed Effects	No	Yes

This table reports analyses of the effect of dissemination on *Markup* for the later introduction of Moody's ratings on EMMA. *Post_Moody's* is an indicator that takes the value of one for trading after June 30th 2015. The pre-Moody's sample period begins after the dissemination of S&P and Fitch ratings on EMMA. All columns present OLS coefficient estimates and t-statistics based on robust standard errors clustered by bond and trade date. Variables are defined in Appendix A. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% p-levels (two-tailed), respectively.