Liquidity and Volatility in the Municipal Bond Market: Evidence from the Municipal Liquidity Facility and other early interventions

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Abstract

At the onset of the pandemic, the federal government stepped in with fiscal and monetary policy actions to assist distressed municipalities. In particular, the Municipal Liquidity Facility (MLF) was established to create a market maker of last resort. In this paper, we examine the extent to which the provision of federal aid to states and the implementation of the MLF was successful in promoting proper market functioning by looking at the plausibly causal effect of these policies on medium-term municipal market volatility. Leveraging a combination of financial instruments outside of the Municipal Bond market, and past observations of state bond yields, we use a synthetic control approach to compute counterfactual volatility series, had the federal government not intervened. Aligning with previous literature, our results suggest that, for A-rated bonds and other instruments of higher credit quality, federal support (both direct fiscal stimulus and liquidity backstops), lead to volatility reductions in the municipal market between 43% - 70% of the incremental volatility experienced by state issuers at the peak of the 2020 financial turmoil. We further examine variation on these effects driven by the magnitude of direct fiscal stimulus received by state governments, finding some suggestive evidence on the relevance of the MLF restoring confidence into markets, even when it observed a low take-up rate among states. Our paper highlights the role that government intervention has on financial markets' perception and the process of price formation in the secondary market, as well as the role of the Federal Reserve as a lender of last resort during episodes of distress.

Keywords: Municipal Bond Markets, Municipal Liquidity Facility, Lender of last resource, Financial Crisis, COVID.
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Highlights

- During March of 2020, the COVID-19 pandemic led to historically high volatility levels on the municipal bonds market. In the sample of bonds we analyze, trading in the secondary market increased 120% and yields at trade spiked 85.3 basis points relative to February 2020. Aiming to bring normalcy back, the federal government enacted the CARES Act which provided direct aid to state and local governments, and also established the Municipal Liquidity Facility (MLF), where eligible local governments could obtain resources from the Federal Reserve acting as a lender of last resort (LOLR).

- In this paper, we examine the role these policies played restoring investor's confidence on the municipal the market over the rest of 2020 and 2021. Using a synthetic control estimator, we compute market volatility measures for different levels of credit risk to build counterfactuals on the volatility of the market, had the federal government not provided aid to state governments.

- Our results provide direct evidence on the volatility reductions observed in the municipal market, associated with the provision of federal aid to state governments. Our estimation suggests that the MLF in conjunction with the announcement of direct fiscal support, reduced volatility in the municipal market for issuers rated A and above. Furthermore, we find that AA-rated issuers experienced larger volatility decreases.

- Point estimates from this analysis point towards medium-term volatility reductions (i.e. 15 months after the intervention) that range between 43% (A-rated) and 70% (AA-rated) of the incremental volatility that municipal bonds experienced at the peak of the turmoil on financial markets.

- We exploit variation on the fiscal stimulus provided to state governments during the pandemic to dissect individual effects from direct fiscal support to governments and the availability of the liquidity backstop provided through the MLF. We find suggestive evidence that supports the latter, underlining the role of the Federal Reserve as a LOLR and its influence restoring confidence on financial markets.

1 Introduction

At the onset of the COVID-19 pandemic, uncertainty about the short and long-term effects the pandemic would have on financial markets resulted in unexpected volatility shocks across financial markets. The municipal bond market was not the exception. During March 2020 municipal bond yields spiked in a couple of days period. Some expected state and local governments would experience severe distress due to dislocations in the bond market (Haughwout et al., 2022a). This is not a surprise considering the current available information, and what occurred during the Great Recession, where municipal bond markets largely dried up at their own onset. For instance, Green and Loualiche (2021) report that by May 2020, 1.5 million public employees (state and local) had been laid off. In another example, Gordon et al. (2020) documented declines in state personal income and sales tax revenues, sharper than the ones observed during the Great Recession.

The federal government plays a fundamental role providing fiscal aid to distressed governments and restoring confidence on financial markets. At the onset of the COVID-19 pandemic, during the last weeks of March 2020 to be precise, the US government launched unprecedented efforts to support the economy through the enactment of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, followed a couple weeks later by the Municipal Liquidity Facility (MLF) implemented by the Federal Reserve. Through these policies, all state governments received direct fiscal stimulus from the Treasury (through the Coronavirus Relief Fund, CRF) and were eligible to tap into the MLF, where the Federal Reserve could purchase short-term notes issued by governments with liquidity needs.

Literature looking at the effects of the MLF in the municipal bond market concurs in finding positive effects on municipal bond prices (Bernhardt et al., 2020; Haughwout et al., 2022b). Moreover, some works had suggested actions undertaken by the Federal Reserve restore confidence to the market and calmed the turmoil observed during March 2020 (Li and Lu, 2020), even when few government issuers directly tapped the MLF (Johnson et al., 2021). By the turn of 2021, after several months into the pandemic, scholars documented a fiscal resiliency from state and local governments and mentioned that the most catastrophic events that were expected for these entities were largely avoided (Clemens, 2022; GAO, 2021).

While most of the work done around the MLF concurs it calmed the municipal bond market, to the best of our knowledge, no study so far directly tests such a claim. In this paper, we extend the work done at this literature by looking at the role that the announcement of federal support to state governments, both through direct fiscal stimulus (i.e. the CARES Act) and through the liquidity backstop provided by the Federal Reserve (the MLF as well as other liquidity facilities), had on the volatility observed on the municipal bond market in the aftermath of the COVID-19 pandemic. This work provides new evidence on the extent to which federal policies implemented at the onset of the pandemic reduced the likelihood of short-term dislocations in financial markets due to heightened uncertainty and reduced liquidity.

Using a synthetic control approach, we compute time series of municipal market volatility, across different levels of credit risk, that aim to describe the market volatility, had the government not intervened through the CARES Act and the MLF. These synthetic volatility measures are built using (as donor units) pricing series of municipal bonds and non-muni financial assets, both unaffected by these policies. We include past measurements of general obligation municipal bond yields to model the issuer-specific determinants of volatility, as well as contemporaneous observations on prices of international sovereign bonds, commodities, and currencies to model the uncertainty surrounding investors on financial markets during the first months of the COVID-19 pandemic. The combination of these two information sources allow the synthetic control estimator to compute volatility series that mimic the volatility on the municipal market, had the mentioned policies were not implemented.

As we expect that the provision of federal aid to state governments provided a boost in confidence to investors, we hypothesize that achieving calm and normalcy in trades would trickle down through the distribution of credit quality. Accordingly, we segment our analysis using state credit ratings to obtain treatment effect estimates for the major credit rating categories (i.e. AAA, AA, A, BBB). That is, we compute volatility estimates for each rating category by looking at the distribution of state ratings in 2019, fixing the states on each group, and then compute the average intra-weekly volatility of bonds issued by states on the same rating group.

To contextualize the magnitude of the estimated treatment effects, we calculate measures of the excess (or incremental) volatility observed during March 2020 for each credit rating group. This allows us to express volatility reductions in terms of the incremental shock observed at the peak of the turmoil on this market. These results highlight that, overall, the announcement and provision of federal support had significant effects reducing the volatility in the municipal bond market. We find that state bonds with the highest credit rating (AAA) observed volatility reductions equivalent to 62.4% of the excess volatility computed for this group. For AA bonds and A-rated bonds the estimated ATE (as percentage of each group's excess volatility) is 70.1% and 43.6%, respectively. We do not find an effect for state bonds with a grade of BBB or lower. These results shed some light on the magnitude of the benefits for the municipal bond market associated with the provision of liquidity backstops and the announcement of federal support to governments during episodes of economic distress.

To assess the robustness of our main results, we conduct the analysis using a different criterion to build the credit rating groups (i.e. keeping fixed the credit risk of the bonds, rather than the issuing state). The results from this analysis confirm the effects of the policy reducing the volatility on the market across all groups rated A and above. Moreover, these results also align with the relative magnitude of the effects found at the baseline specification, suggesting that AA-rated issuers observed the larger volatility reductions, relative to AAA and A rated issuers. Estimates of this model, however, suggest the effect on the AAA group are considerably milder than the ones estimated by at the main results. In both sets of results, effects for the AA-rated group exceed the ones for the A-rated group.

Documenting smaller effects on the AAA group, relative to the AA-rated issuers, could be consistent with a scenario where the strong creditworthiness of AAA-rated instruments is enough to shield investors from the uncertainty that was present in other segments of the credit risk distribution and other asset classes. This aligns with our intuition that bonds with higher credit quality will return to normalcy first, while bonds with lower credit quality might remain in trouble.

We further examine this issue, and also test for the sensitivity of our main results to the length of the post-intervention window used to compute the ATE, by performing the analysis according to different values of this parameter. This analysis also highlight the dynamic effects the intervention had on the municipal market. The results from this exercise reveal that statistically significant policy effects are identified by the estimator following the sixth month after the intervention, both for the AAA and AA rated groups. For A-rated issuers, the effects are significant after December 2020 at the 5% level.

To shed some light on the role each type of policy had restoring confidence to the market, we examine heterogeneity on the estimated treatment effect driven by variation on the distribution of federal support (through capital grants) given to state governments during the first months of the pandemic, which mimics the distribution of aid channelled through the CRF. We separate the ATE from each credit rating group, between the states whose per-capita allocation of capital grant funding was above (or below) the median across states. We document mixed evidence across credit rating groups. Estimates for the AAA and A-rated groups suggest larger effects for states whose allocation was above the national median, while the results for the AA-rated group point towards the other direction. However, we interpret these results with caution as this segmentation within the rating groups lead to some comparisons across states that mimic a traditional case study.

This analysis reveals that AA-rated issuers with below-median explain the policy effects captured by the estimates of this group at the baseline specification. States on the below-median segment of the COVID-19 support distribution are larger states where these funds represented a lower proportion of their fiscal revenues. Hence, any effects driven by the provision of direct aid should be smaller for these states (compared to the effect for states that received larger transfers, relative to their size). Considering these points, we argue this result could be consistent with a model where the role of the Federal Reserve as a lender-of-last-resort had a larger influence easing the uncertainty on the municipal bond market. Moreover, since this rating group includes 25 of the 50 states in the country, it provides some suggestive evidence on the market-level effects of these policies.

2 Literature Review

There is growing literature looking at the public finance effects of COVID-19 policies and the Municipal Liquidity Facility (MLF) has received special attention among scholars. Several papers have examined from different angles the effect the MLF had on government's borrowing costs. Most of the studies have approached this question using time series or event study based methods that exploit the variation around the announcement of the intervention to capture the MLF effects on the municipal market. Relying on long time series data, Bordo and Duca (2021) suggest the MLF kept municipal spreads on tolerable levels. This study finds the MLF kept municipal bond rates from rising 9 percentage points in April 2020, and about 3 percentage points over the second and third quarters of 2020. Most of the variation in their analysis comes from historical data, with few observations of bond yields after the policy. Moreover, their analysis only looks at municipal bonds with high credit risk.

Fritsch et al. (2021) also examine the effect of the facility in municipal yields. These authors use an event study on an aggregate index of the municipal market, for different maturities. Unlike Bordo and Duca (2021), this article only focus on AAA-rated securities. Their study suggests a decrease of between 21 and 27 basis percentage points in bond spreads with maturity below 5 years. Furthermore, their estimates show that the reduction in spreads increases along with bond's maturity. Fritsch et al. (2021) also analyze the case studies of the two issuers that tapped the facility, finding positive effects of the policy in their borrowing costs. Their yields in the secondary market were significantly reduced when they communicate investors their intentions of using the MLF.

The literature around the MLF concurs on the swift effects of the policy calming the turmoil as borrowing costs returned to stable dynamics right after Fed's intervention in the municipal market. Some studies had looked at the variation surrounding the intervention. Bi and Marsh (2020) focused on narrow trading windows to analyze the effect of announcements on fiscal policy (i.e. CARES Act) and direct monetary policy actions (i.e. the MLF) on bond prices, particularly through liquidity and credit risk channels. Evidence from their study suggests these interventions successfully stabilized the market. Their study finds that credit risk concerns played a significant role in determining short-term yields at the onset of the pandemic, while not affecting the price of longer-term bonds. Authors argue this is likely a consequence of MLF's design, which primarily benefited short-term notes. These three articles rely on interrupted time series and on indices to approximate the effect of MLF on Muni-Bond market yields.

Moving into quasi-experimentl evidence, Johnson et al. (2021) uses a difference-in-differences model and an interrupted time series analysis to examine the effect the MLF had on municipal yields. In this paper, the authors also exploit the treatment assignment rule by comparing eligible governments with the rest of governments issuing debt during the pandemic period. Results from the difference-in-difference model point to no treatment effect of the policy, thus suggesting that eligibility status was not determinant for borrowing costs. Authors underscore the effect of the policy is channeled through FED's role of market maker of last resort. In contrast, we argue that a lender of last resort will

In a similar fashion, Haughwout et al. (2022a) exploited MLF's population eligibility cutoff to estimate the option value of municipal liquidity in secondary market yields, primary market issuance, and public sector employment. Their results suggest that eligible issuers traded, on average, 75 basis points lower than similar governments below the cutoff for treatment eligibility, Moreover, their probability of issuing debt on the primary market was larger by about 8 percentage points. Thus, having available liquidity eased the distress experienced by some local governments.

Both Johnson et al. (2021) and Haughwout et al. (2022a) argued for treatment effect heterogeneity by credit risk level (i.e. credit rating). Johnson et al. (2021) provides a comparison of the average spread between the government's borrowing costs through the municipal bond market and hypothetical MLF rates used as theoretical counterfactual, finding that lower-rated governments would have observed higher borrowing costs, had they tapped into the MLF. This backs authors' argument explaining the low take-up rate of the program: its high costs did not cover issuers' opportunity cost at the onset of the pandemic (March-April 2020). These results, however, contrast the findings of Haughwout et al. (2022a) where their empirical evidence suggests lower-rated issuers traded at higher prices and were issued more frequently. While the differences between these two sets of results could be explained by the segment of the market each paper looks at[1], both underline the underlying heterogeneity driven by credit risk.

[1] Johnson et al. (2021) focuses on short-term instruments issued by state and local governments, while Haughwout et al. (2022a) relies on both short and long term bonds issued only by local governments.

The MLF was by no means the first intervention of a central bank as a lender of last resort (LOLR). However, it was the first providing that sort of liquidity to the municipal bond market. In the context of banking, a large body of literature has found that the LOLR role of central banks provides stability to financial systems, starting in Greek and Roman times, but in modern crisis this role supports financial systems by supplying liquidity to stressed actors and by replacing the demand for liquidity when markets stop circulating (Alves et al., 2021; Calomiris et al., 2016; Garcia-de Andoain et al., 2016; Santos and Suarez, 2019).

Our research conducted so far highlights the causal mechanism of the liquidity facility improving borrowing costs of municipal issuers. Despite only two governments tapped MLF's resources (i.e. had direct exposure to the policy), there were overall effects in the municipal market. In this regard, scholars argue the policy contributed to reducing the turmoil in the financial market at the onset of the pandemic, supporting the idea of federal intervention being an effective tool to restore proper market functioning. In other words, as it became clear to investors that the Federal Reserve provided lending facilities to local governments, uncertainty around the price of municipal debt decreased. However, the literature has been quite vague exploring the causal mechanism behind the role of signaling by the federal government on the price formation process of municipal bonds. In this paper, we aim to fill such gap by directly looking at market volatility as the outcome variable of interest, in contrast to previous studies that had explored the effect this policy had on bond prices and yields (Bernhardt et al., 2021; Bordo and Duca, 2021; Haughwout et al., 2022b; Johnson et al., 2021). This is one of the main factors that differentiate our paper from the outstanding literature. Second, this paper explores policy effects in the months following the intervention, as opposed to the literature that focuses on the immediate effect observed on bond prices (Bi and Marsh, 2020; Li and Lu, 2020). This allows capturing any potential effects driven by the waves of COVID-19 cases and deaths experienced towards the end of 2020 and during 2021. Finally, we contribute to the literature that analyzes the role of lenders of last resort using quasi-experimental approximations in a context different from banking, which currently lacking in the literature.

3 Federal Support to State Governments

In response to the COVID-19 pandemic and to stabilize the economy at its onset, the U.S. government provided direct assistance to various economic sectors through the Coronavirus Aid, Relief, and Economic Security (CARES) Act. Through this act, the federal government allocated approximately $2.2 trillion to mitigate the adverse effects of the lockdown on the economy. To complement these actions, the Federal Reserve established several liquidity facilities that made available up to $2.3 trillion in loans.

The implementation of these policies took place in the weeks following March 13,2020, the US National Emergency Declaration. Here we enlist some of the main events related to the provision of federal aid to state and local governments.[2]

1. On March 20, 2020, the Federal Reserve expanded the Money Market Liquidity Facility (MMLF) so it included short-term municipal debt as eligible collateral at this facility. Three days later (March 23), a similar expansion was applied to the Commercial Paper Funding Facility (CPFF). Both measures aimed to provide a backstop to municipal instruments fostering their demand on the secondary market.

2. On March 24, 2020, Senate and Administration leaders came to an agreement on the CARES Act, that led to its passing on the Senate (on March 26) and its enactment on March 27. Support to state and local governments through the CARES Act mainly came through the Coronavirus Relief Fund (CRF), which allocated $139 billion to state and local governments, where $111.37 billion were direct transfers to state governments. States funds were distributed proportional to each state's population where no state received less than $1.25 billion. Funds to municipalities were distributed across counties and cities with population above 500,000.

3. On April 9, 2020, the Federal Reserve announced the implementation of the Municipal Liquidity Facility (MLF) to assist state and local governments manage cash flow stresses caused by the pandemic. The main purpose of the MLF was to enhance the liquidity of the primary short-term municipal securities market by purchasing up to $500 billion of short-term notes directly from eligible governments.[3] The facility determined that the proceeds from this program could be used to manage short-term impacts on the government's cash flows due to income tax deferrals, reductions of tax and other revenues, and increases in expenses associated with the management of the pandemic. To establish the MLF, the Federal Reserve received $35 billion in credit protection from the Treasury using funds appropriated by the CARES Act.

4. On April 27, 2020, the MLF was expanded to include all counties and cities with population above 500,000 and 250,000, respectively, and extended its duration until the end of 2020. Initially, all state governments and municipalities above 1,000,000 inhabitants were eligible to tap into the MLF, which would cease to purchase notes by the end of September 2020. By the end of April, all state governments had received

[2]Figure A.1 on the Appendix provides a summarizing visual.

[3]This includes: Tax anticipation notes (TANs), tax and revenue anticipation notes (TRANs), bond anticipation notes (BANs), revenue anticipation notes (RANs) and other similar short-term notes, provided that such notes mature no later than 36 months from the date of issuance.

support from both the Treasury and the Federal Reserve. [4]

Some of effects of these policies were visible even as they were still discussed by policymakers (Bi and Marsh, 2020). Results from their study show a significant decline in tax-adjusted municipal spreads after the announcement of a fiscal stimulus package in March 24, 2020. This reduction in spreads heightened after the unanimous passage of the CARES Act by the senate on March 26, and persisted until the implementation of the MLF. A key takeaway from Bi and Marsh (2020) is that investors reacted to the announcement of federal aid to the economy, instead of adjusting their behavior until the aid landed to states and municipalities funds.

This result is consistent with classical finance theory that underscores the role of signaling among investors and other financial market agents (Stigler, 1961). In particular, in terms of the role information has on the price formation process. A potential mechanism could be that the announcement of federal aid dissipated some of the uncertainty surrounding municipal issuers, thus alleviating the volatility observed during March 2020. Nonetheless, it remains unclear the extent to which the reductions in spreads were driven by differences in investors' perceptions between actions from the US Government (i.e. CRF and other sources of federal funding to states) and the Federal Reserve (MLF).

The main challenge to identify the effects between these two policies is that both were implemented around the same time, with similar rules for allocation and eligibility. Furthermore, even though there are differences in the take-up rate between these two policies (i.e. through the CRF, all state governments received direct aid from the Treasury, while only two governments tapped to the MLF), if the mechanism to financial markets is through the signaling provided by the announcement of support, then disentangling the effect of each policy relying on the timeline of events might not be possible.

This complexity calls for careful analysis on the effects of federal policies had on the municipal market. While our identification strategy does not alleviate this problem, its presence does not represents a threat to validity as our baseline results arguably capture the joint effect of the available information on the CRF and the MLF on the municipal bond market. As part of the robustness checks, we conduct an exercise that sheds some light on the heterogeneity of the joint effect of these policies driven by the magnitude of

[4] Support from the Federal government was channelled through several sources besides the CRF. Data from the Federal Funds Information of States (FFIS) shows that grant funding for state governments was channeled through 75 different funds, grants and programs part of several departments and agencies of the federal government. The CRF was perhaps the largest fund to states as it comprised 47% of total funding given to states by June 2020. FFIS data retrieved from the replication package of Green and Loualiche (2021).

the aid channelled, not only through the CRF, but through to all capital grant funding provided to states during the first stage of the pandemic. This allow us to examining the effect of direct fiscal support, relative to the liquidity backstop provided by the Federal Reserve.

4 Data

4.1 Treated Series

For the analysis, we retrieved a sample of 9,273 bonds issued by state governments (including agencies and authorities at the state level) from January 2019 to December 2021 from Bloomberg LP. The variables of interest obtained from this data source are issue date and the credit rating at issue given by three of the main credit rating agencies. Using each bond's CUSIP, information on secondary market transactions is retrieved from the Municipal Securities Rulemaking Board, accessed through Wharton Research Data Services.[5] We use the yield at trade in the secondary market as the price variable to study the volatility of the municipal bond market.

We grouped the bonds in our sample into four rating categories: AAA, AA, A, and BBB. To build these categories we considered the rating assigned at issue by three main credit rating agencies: Fitch Ratings, Standard and Poor's, and Moody's. From these three ratings, we choose the lowest one. In this categorization, we grouped without the plus or minus sign that goes along with the rating. For instance, category AA includes all AA+, AA, and AA- bonds.

To create these categories, we considered the distribution of credit ratings across state governments observed during 2019. The rationale for this classification is to create cohorts with a fixed set of states according to their creditworthiness measured before the intervention took place, and at the beginning of the window of analysis. In our sample, some state governments were not as active during 2019 or there was no information on the rating assigned by these 3 agencies. We exclude them from our analysis.[6]

[5] This sample includes bonds issued between 2019 and 2021. However, the shock experienced during 2020 arguably affected all outstanding municipal debt including bonds issued before. So long there are no significant differences between the volatility observed on bonds issued before our analysis window, then our results should be representative of overall effects observed in the market.

[6] These states are: Alabama, Alaska, Kansas, Kentucky, Michigan, Missouri, Montana, New Hampshire, New Jersey, North Dakota, Oklahoma, South Dakota, and Wyoming. See Figure A.3

Using these four categories as criteria, for each date in which a transaction was observed, we compute the average yield at trade. The volatility measure used as main outcome for the analysis is the average intra-week volatility observed in a given month. Sequentially, the process to obtain these measures stems from: 1) Obtaining the daily yields; 2) Calculating the weekly standard deviation of those yields; 3) Averaging the weekly standard deviation in the month. This leads to the monthly average of the intra-week volatility. Finally, we normalize, in order to obtain a measure free outcome, that also helps with the synthetic control methodology. For this normalization we use the pre-covid sample mean and standard deviation of the intra-week volatility.

4.2 Donor Pool

One of the challenges in identifying the intervention effect on the municipal market is to disentangle the uncertainty driven by systemic factors (i.e. common to all asset classes and financial markets) from the one specific to the municipal bond market. For the empirical analysis we implement a synthetic control estimator, which requires a donor pool of untreated units to construct synthetic volatility series that mimic the volatility observed in the municipal market, in the absence of the intervention. This donor pool contains financial instruments that are unaffected by the intervention from the federal government, and close enough to the treated units to provide some representation of what would have happened to municipal securities in the absence of the policy. For this purpose, we consider two different of information sources. First, from Bloomberg LP we obtained time series data for sovereign bond yields from both developed and developing economies. Additionally, we included indices following the main stock markets and commodities . These led to a set of 80 financial time series. Ideally, with this sample, we could identify uncertainty in the municipal market driven by systemic factors.

For the second uncertainty source, we look at data coming from Standard & Poor's Municipal General Obligation Bond Indices, retrieved from Capital IQ. These indices are based on pricing data from general obligation bonds issued by state governments. This sample covers 32 state governments, with daily pricing data from 2013 to the end of 2018. The price variable considered for this data is the yield-to-worst. To dissect the factors driving the volatility of the municipal market during this period, we created four overlapping 36-month cohorts: i) 2013-2015, ii) 2014-2016, iii) 2015 - 2017, and iv) 2016 - 2018. This approach leads to a set of 128 donors. The implicit assumption from this approach is that we treat as different donors, series from the same state government coming from each time cohort. In theory, these variables should allow us to capture structural volatility drivers

on the Appendix.

from the municipal market exclusively. Altogether, the donor pool contains 208 individual donors, adding the 80 previously described from independent financial time series. We compute the monthly volatility measure for each donor following the same process used for the treated units.

Intuitively, each source of information provides useful variation to construct a synthetic counterfactual. Donors from the past observations of municipal bond yields should provide variation to explain the structural factors shaping the volatility of the municipal bond market. On the other hand, donors from the contemporaneous observations of other financial instruments should provide variation to capture the magnitude of the volatility hike observed in global financial markets, so the synthetic counterfactual can build estimates that model the turmoil in the municipal market, had the federal government not intervened. We use the same three steps plus normalization we apply to the treated series. In this case, we pick the month number 15 to separate the pre-intervention mean, to match for how long we follow the treated units.

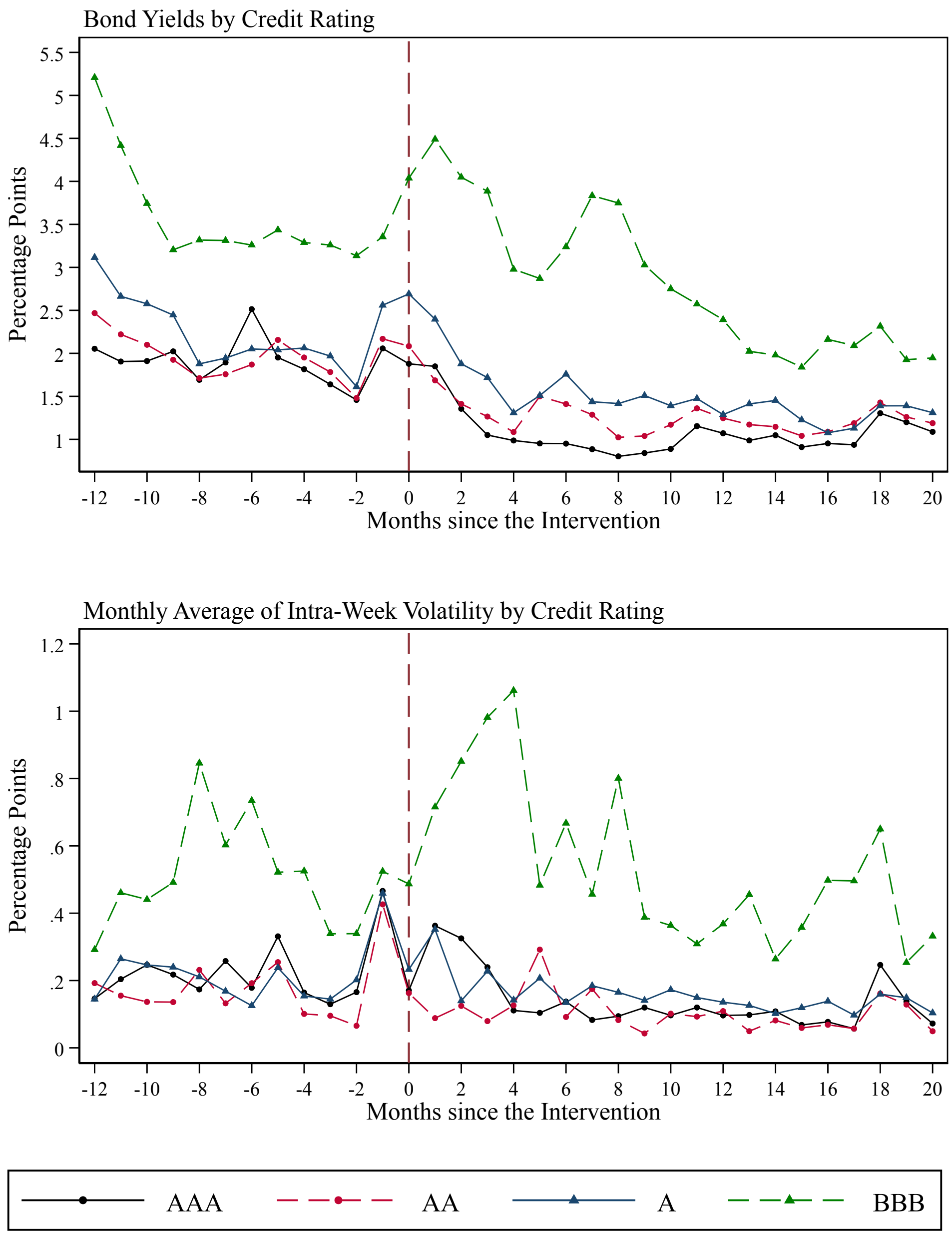
4.3 Descriptive Statistics

Now we examine the main statistics of the sample used for the empirical analysis. Figure 1 depicts two panels. The top panel shows the average yield at trade[7] observed in the secondary market for each credit rating category. The bottom panel shows the volatility measure, the monthly average of the intra-week standard deviation in yields, associated with each variable on the left panel. The variables on the bottom panel are the main dependent variables of interest in this paper. Both panels illustrate the increase in volatility experienced upon the emergency declaration during March 2020 and further suggest there were heterogeneous responses to the shock across rating categories. The top panel highlights the sudden increase in yields observed around March 2020, whose effects dissipated during the periods following the turmoil. The bottom panel shows a more nuanced view of the distress experienced during March 2020, where it seems that the magnitude of the adjustment as well as the time before the dust settled down after the turmoil differed across credit rating categories.

To examine statistical differences in the volatility levels across rating groups, we conducted a t-test on the average yield and volatility observed during the periods surrounding the announcement of federal intervention.This comparison looks for differences between the

[7]According to the WRDS Dictionary: yield at trade is based on a “yield to worst,” that may be realized on an investment in the security based on the settlement date, price, interest rate on the security and the remaining period until maturity or earlier redemption.

Figure 1: Yields and Volatility in the Secondary Bond Market



Note: The top panel shows the average yield observed by bonds issued at each month around the intervention of the federal government on the municipal bond market for each credit rating category. The bottom panel describes the monthly average of intra-week volatility observed during the same period. Source: Authors' calculations with Bloomberg and MSRB data.

periods preceding and following federal intervention. The analysis window used for this exercise and for the main specification of this paper considers 12 months before policy implementation (April 2019 - March 2020) and the 15 months following the intervention (i.e. April 2020 - June 2021).[8] The results of this descriptive analysis show a larger decrease in bond yields for higher-rated issuers. The states in our AAA rating category observed, on average, a decrease of 79.6 basis points in the yield at trade in the months following federal intervention in the municipal bond market. Issuers in the AA and A rating categories experienced a reduction of 64 and 60 basis points, respectively. This reduction across state issuers, however, is partially explained by monetary policy actions undertaken by the Federal Reserve during such period. To illustrate this, recall the federal funds effective rate decreased from 1.58% observed in February 2020 to 0.05% in April 2020, and remained below 0.10% for the rest of 2020 and 2021. While this factor might explain the decreasing trend in the yields depicted in the left panel of Figure 1, it stands out the negative relationship between the magnitude of the yield decrease and the credit rating category. This suggests that riskier instruments experienced lower reductions in borrowing costs after the Federal Reserve's intervention. This descriptive evidence underlines the potential heterogeneity of the policy across rating categories.

Columns (4) to (6) in Table 1 show the results of the unconditional mean comparison for the volatility measures of each rating category. Similar to the trend observed in yields, the analysis finds larger volatility reductions for higher-rated issuers. For example, for the states in our AAA rating group the average volatility between March 2019 and March 2020 was 0.223%, and 0.151% from April 2020 to December 2021. This implies a difference of 7.2 basis points. BBB-rated bonds stand out in our analysis as they observed different behavior, both before and after federal intervention. The bottom panel in Figure 1 clearly shows an increasing trend in the volatility for this rating category. Furthermore, it remained high after the policy was implemented, in contrast to the dynamics observed among the other analyzed rating categories. Estimates in Column (6) from Table 1 imply a volatility increase equivalent to 6.7 basis points (relative to the average volatility observed in the pre-intervention period), although this is not statistically significant. As we explore ahead, this empirical observation is consistent with our treatment effect estimates.

Now we examine the series from the donor pool. Figure 2 summarizes the two types of donors used to build the synthetic counterfactual. The top panel shows the average

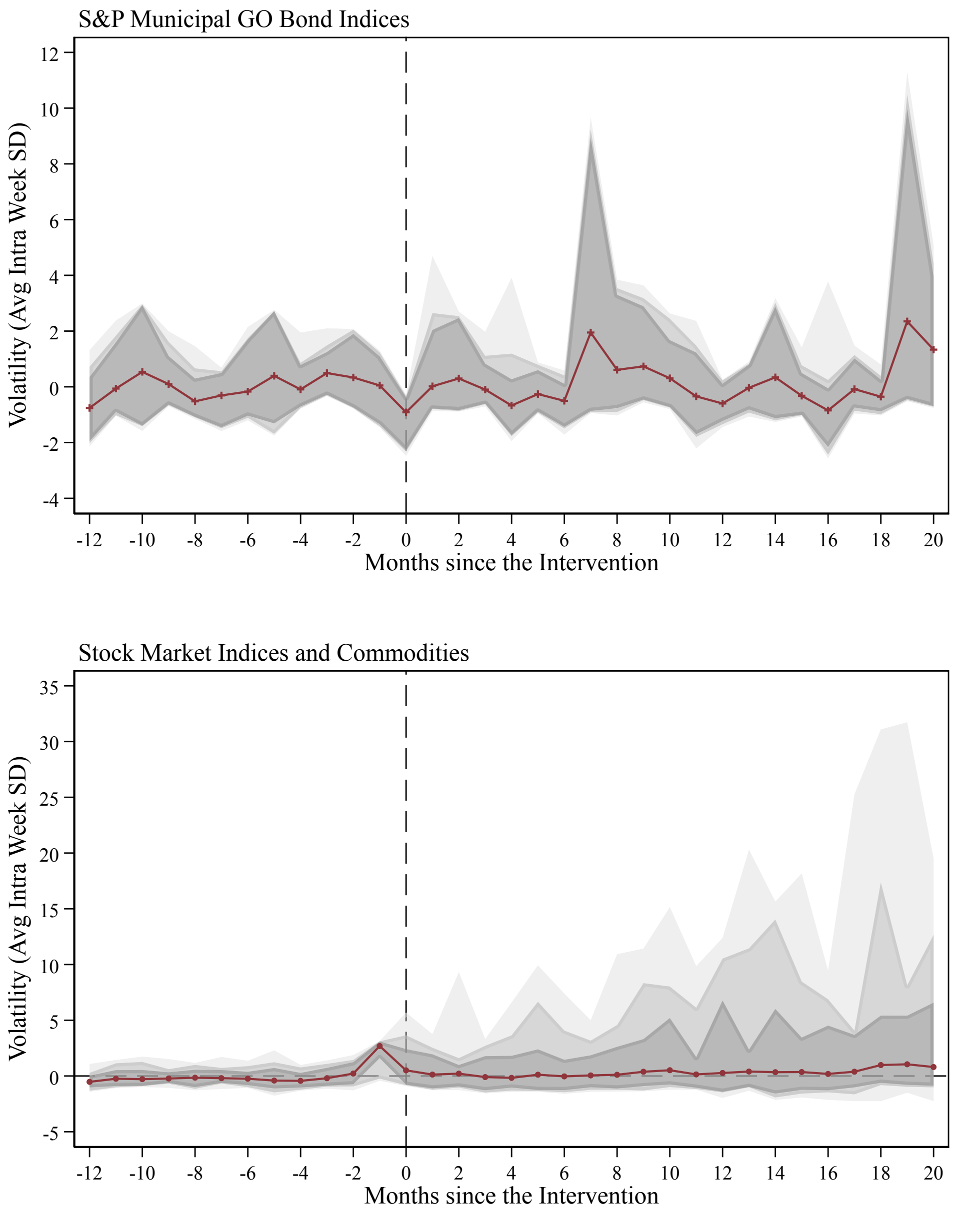
[8] In our bond sample we document low levels of trading for bonds from A-rated issuers between January 2019 and March 2019, to the extent that we are not able to compute volatility measures for this period. To keep the same analysis window for all treated units, we do not consider these three months in the pre-intervention period. As part of the robustness checks, we relax this assumption and perform a similar analysis with a symmetric window of 15 months before and after the intervention. See Section 7.1.

Table 1: Yields and Monthly intra-week Volatility Pre and Post Federal Intervention

	(1)	(2)	(3)	(4)	(5)	(6)
	Yield			Volatility		
	Pre	Post	Diff	Pre	Post	Diff
AAA	1.9095	1.1133	-0.7961***	0.2234	0.1511	-0.0723
	(0.2620)	(0.3332)	(0.1145)	(0.0947)	(0.0877)	(0.0355)
AA	1.9662	1.3259	-0.6402***	0.1765	0.1133	-0.0632
	(0.2695)	(0.2769)	(0.1056)	(0.0964)	(0.0610)	(0.0319)
A	2.2431	1.6429	-0.6002**	0.2164	0.1741	-0.0423
	(0.4260)	(0.4044)	(0.1613)	(0.0894)	(0.0619)	(0.0303)
BBB	3.5782	3.1920	-0.3862	0.5097	0.5767	0.0670
	(0.6192)	(0.7820)	(0.2696)	(0.1612)	(0.2524)	(0.0800)
Donors (Munis)	1.7816	1.8027	0.0210	0.0222	0.0197	-0.0024***
	(0.6401)	(0.5735)	(0.0209)	(0.0151)	(0.0100)	(0.0004)
Donors (Others)	4730.74	4848.55	117.81	49.48	48.17	-1.31
	(10058.85)	(10400.02)	(442.19)	(139.61)	(110.16)	(5.51)

Note: This table presents the results from a paired t-test comparing the yield and volatility of each credit rating category, before and after federal intervention. Yields expressed in percentage points. For example, the average yield of issuers in the AAA-rated group for the pre-treatment period is 1.90%. Volatility is expressed in the same units as it is the monthly average of the intra-week standard deviation of yields at trade. We use April 2020 as the intervention period (i.e. April 2020 is the first month on the post-intervention period). Therefore, the comparison is done between 12 months from the pre-intervention period and 15 months from the post-intervention period. Donors (munis) and donors (others) show the comparison for the units part of the donor pool. For the case of muni donors (i.e. series from the S&P Municipal General Obligation Bond Indices) we consider as intervention period the 12th month of each cohort. In other words, the comparison is done across the same set of experiment-months (i.e. relative to the intervention). Standard errors are reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels.

Figure 2: Volatility Estimates for Donor Units



Note: The top panel shows the historic volatility estimates of state-issued bonds coming from Standard & Poor's Municipal General Obligation Bond Indices. The bottom panel shows contemporaneous volatility estimates for financial instruments tracking stock markets and commodity prices. The solid red line shows the average volatility observed in each month of the graph. Gray areas surrounding such line depict the volatility distribution across donors at each period by showing the percentiles of said distribution at the 10%, 5%, and 1% level.

volatility historically observed by the Standard & Poor's Municipal General Obligation Bond Indices following the 32 governments part of our 128 donor pool of municipal issuers. As mentioned above, we partitioned this data into non-overlapping 36-month cohorts. Hence, we reference Figure 2 in relative time. This allows accounting for potential seasonal effects lurking behind the bond price-generating process, and from a standpoint of randomization inference, this provides a sharp zero effect distribution to contrast any changes in municipal bond markets(Abadie, 2021; Cavallo et al., 2013; Hagemann, 2019; MacKinnon et al., 2023). The solid red line shows the average volatility observed across all 128 donors at experiment-month t. The gray area surrounding the line shows the percentiles of the observed volatility at each period at the 10%, 5% and 1%. As we can see, for the most part, the volatility distribution across issuers remains stable over time, observing a hike on the 7th month after the intervention. Since we are normalizing the time using April 2020 as the intervention period, this aligns with the spike around November 2020 observed in Figure 1). We also conduct paired t-tests for the average difference in the volatility before and after the intervention at the last two rows of Table 1. To be specific, this comparison shows the mean difference for yields and volatility between the intervals (March t - March $t+1$) and (April $t+1$ - July $t+2$) where in this notation t represents the years from 2013 to 2018. These results suggest small differences (less than one basis point) between the volatility levels observed in the periods preceding the theoretical intervention time, relative to the period after it. To the best of our knowledge, there are no systematic factors that could be driving the trend observed at Figure 2, which aligns with the results at Table 1. Therefore, we believe this is not a relevant concern in terms of the adequacy of the donor pool, as it unlikely to be a dormant factor on the time series, that could confound the dynamic treatment effect. In case this presents a threat to the validity of our estimates, it could lead to underestimating the average treatment effect, since we will be using donor units with relatively high volatility levels in the post-period and our results would be lower bound of the true treatment effect.

The panel at the bottom of Figure 2 shows the observed volatility for all the units (i.e. financial instruments) that form part of the contemporaneous measures of volatility of our donor pool. As before, the gray area shows the percentiles of the volatility distribution for each month in the graph. This graph depicts how the turmoil experienced during March 2020 dissipated over the following months. The average volatility (solid red line) seems to return to levels similar to the ones observed in the pre-intervention period. Nonetheless, it stands out there were financial instruments in our sample whose volatility levels remained high after March 2020. From this variation, we aim to capture how global financial markets reacted to pandemic shock over time, in order to contextualize the jump observed in the municipal bond market. The last row at Table 1 shows the results of the average difference in the volatility estimates before and after the intervention. We do not observe statistically significant differences. This provides suggestive evidence of the adequacy of using these financial instruments as donor units.

5 Research Design

To estimate the effect that federal support to state and local governments had on alleviating the volatility observed in the municipal market, we use the synthetic control estimator proposed by Abadie and Gardeazabal (2003) to construct a counterfactual series of the volatility in the municipal market (for each credit rating category) as a weighted average of the volatility of past observations of the volatility on GO debt instruments and contemporaneous observations of the volatility on stock market indices and commodities. In this study we adhere to the methodological approach proposed at Abadie et al. (2010) where a non-negativity constraint is imposed on the donor weights, as well as a restriction that these must sum one.

In our study, the dependent variable y_{it} measures secondary market volatility of rating category $i = \{AAA, AA, A, BBB\}$ during month-year t. The donor pool includes volatility measures of $k = 208$ donors described at Section 4, and reported individually in the Appendix. Following Abadie (2021), we normalized all variables using their sample specific pre-intervention mean and standard deviation. This provides a unit-free comparison that parses out differences specific to each financial instrument, which mitigates the high variability observed across donor units, as well as differences in the units in which each time series is expressed. When computing the treatment effect for each rating category we reverse this adjustment so we have clean comparisons between the observed and synthetic volatility, expressed in the units of each outcome variable. We consider lagged observations of the volatility measurement as predictor variables to assess the goodness of fit in the pre-intervention period. In other words, the predictors used to evaluate the model's goodness of fit are observations y_{it} for each period t from April 2019 to March 2020. This strategy aligns with work by Cavallo et al. (2013); Hagemann (2019); MacKinnon et al. (2023) and others following their work and emphasizing randomization inference.

We consider April 2020 as the month were the intervention takes place, hence is the first month from the post-treatment period.[9] This implies that, under this specification, we

[9]Considering that most of the information about the federal relief to state and local governments was released during the last weeks of March and that the MLF implementation took place in April 9, and that we use monthly volatility measures for the analysis, we consider April 2020 as the intervention month. We do not use March 2020 as intervention month since the volatility observed during the first half of this month captures the heightened uncertainty in the markets that prevailed before the Declaration of Emergency in March 13, 2020.

cannot directly identify heterogeneity on the treatment effect that could shed some light on the individual influence that each policy had on the market. Hence, the treatment effects derived on the main results of this paper arguably capture the joint effect of the provision of federal support to state governments through the CARES Act, and the liquidity backstop provided by the Federal Reserve through the MLF. As part of the robustness checks, in Section 7.2 we use variation on the funds provided to states during the first stage of the pandemic to potentially dissect some of the effects of each type of policy tool.

Intuitively, the synthetic control estimator chooses the donor weights w_k that minimize the distance between the time series of the treated unit (i.e. volatility in the municipal market) and the time series of weighted (donor weights) average of the volatility of the donor units. In other words, the synthetic control estimator optimizes the pre-treatment fit of the variable of interest. Distance, in this case, is measured as the weighted (relevance weights) Euclidean distance between the past 12 monthly volatility observations preceding the intervention.

From a policy evaluation standpoint, the parameter of interest is the average treatment effect (ATE). In terms of potential outcomes, this parameter captures the average difference in the volatility of municipal issuers, conditional on being exposed to the intervention, $\tau_i = E[y_i(1) - y_i(0)]$. The synthetic control estimator directly computes this statistic by taking the difference between the outcome's time series y_{it} and the synthetic control $\hat{y}_{it}$. ATEs are computed as the average of $\hat{\tau}_{it}$ in the 15 months following the intervention, including the intervention period.[10]

5.1 Statistical Inference

Following Cavallo et al. (2013), for statistical inference we construct the empirical (placebo) distribution of the ATE by applying the synthetic control estimator described above to the volatility measure of each unit in the donor pool. The estimation of each placebo unit uses as donors the rest of the units in the donor pool. Provided there are no spillovers to the donor pool (i.e. exposure to the intervention), then the placebo distribution of the ATE, $\hat{\tau}_{it}$, should be centered around zero and reflect the sampling variability of the

[10] To clarify, we estimate four independent synthetic controls using the same donor pool and predictor variables and obtain four time series for the treatment effect $\hat{\tau}_{it}$, according to Equation 1.

$$\hat{\tau}_{it} = y_{it} - \hat{y}_{it} = \left(y_{it} - \left(\sum_{k=1}^{208} w_k^* \cdot y_{kt} \right) \right) \tag{1}$$

ATE statistic, hence we can use it to test the reliability of our ATE estimates. The reliance on randomization inference is assumption free with respect to the distribution of the effectsm and instead constrasts against a sharp null from a demanding zero effect distribution (Abadie, 2021; Hagemann, 2019; MacKinnon et al., 2023).

To ensure inference is done from a distribution where the statistical model replicates accurately the pre-treatment period, we dropped all the placebo units whose RMSPE is above 0.15[11]. This assumption derives in dropping 15.86% of the donor pool. Thus the placebo distribution used for inference contains observations from 175 donor units.

If the implementation of CARES Act and the MLF restored confidence to the markets, then the synthetic volatility series should show the heightened uncertainty still present in other financial markets, that would have characterized the volatility in the municipal market, in the absence of these policies. This implies the theorized ATE is negative. Aligned with this research hypothesis, for inference we compute left-tail rank-based p-values. In simple terms, we count the number of times the value of $\hat{\tau}_{it}$ exceeds the ATE estimated for each placebo unit. We illustrate the statistical reliability of our estimates with the *smokeplots* at Figure 4 where the faint gray areas delimit the distribution of the placebos at traditional confidence levels around the treatment effect predictions from our model. To complement this analysis, Figure A.4 in the Appendix shows the visual representation of this permutation test.

5.2 Identification and Threats to Validity

Abadie (2021) delineates the data requirements and identification assumptions the synthetic control estimator requires to capture the ATE of the intervention. Identification of this method requires that units in the donor group are not affected by the analyzed intervention. This is because the synthetic counterfactual is computed as a weighted average of the volatility measures of the donor units.

Spillovers and anticipation effects are related to violations of the Stable Unit Treatment Value Assumption (SUTVA) common to all causal inference methods. A violation of this

[11]The Root Mean Square Prediction Error (RMSPE) is given by the adjusted weighted average Euclidean distance between the synthetic prediction and the real series in the pre-treatment period. The lower the RMSPE the better fit the synthetic series offers to follow the real series. This works as a goodness of fit in the pre-intervention period where we want the synthetic series to replicate the real series as closely as possible. We only allow for donors for which we can establish good counterfactuals.

assumption could translate into attenuation bias in our estimates if the announcement of federal aid to state and local governments influenced the volatility of some of the financial instruments that form part of the donor pool. We consider this assumption likely holds in our case due to the nature of the two data sources used to build the donor pool. On one hand, past observations of GO debt volatility are unlikely influenced by federal intervention as this strategy surged as a response to the COVID-19 pandemic (i.e. an unanticipated shock to the economy). The cohort closest to the intervention in our donor pool covers the period 2016-2018, which ended 15 months before the intervention took place. Hence, we do not expect anticipation effects to be present in these volatility measurements. Furthermore, visual inspection from the trends depicted in Figure 2 and the results at the bottom of Table 1 suggest no relevant differences in the volatility trends at the window centered around March of each two-year cohort.

On the other hand, we do not include financial instruments issued by institutions located in the United States as part of the donors that provide volatility measurements contemporaneous to the policy. Hence, none of the units in this leg of the donor pool were eligible for any of the liquidity facilities implemented by the Federal Reserve nor any of the aid programs established as part of the CARES Act. We consider there are no relevant concerns of geographic spillovers nor anticipation effects in this leg of the donor pool since, to the best of our knowledge, the information regarding the implementation of federal policies, including the MLF and other liquidity facilities, was disclosed publicly to all market participants at the same time.

To the extent that the provision of federal aid to state and local governments led to a change in the behavior of agents (i.e. investors and issuers) on the international sovereign debt market and the commodities market, then our estimates could be biased as the post-intervention volatility measures of units in this leg of the donor pool could be influenced by the treatment effect the synthetic control aims to identify. For instance, if investors on instruments considered in our donor pool somehow anticipated a prompt reaction from the US government as a response to the pandemic that could influence the performance of such instruments, then we could observe volatility hikes in periods preceding the intervention that the analyzed policy could explain. However, given the unanticipated nature of the COVID-19 shock, we do not believe this is a significant concern.

The synthetic control estimator also needs to satisfy a convex hull condition that requires that units in the donor pool can replicate the outcomes observed by the treated units. Our strategy of implementing the synthetic control estimator on standardized units works toward the compliance of this assumption (Abadie, 2021; Hollingsworth and Wing, 2022). Aligned with this objective, the baseline specification considers an analysis window of 12 months before and 15 months after the intervention. Given the data characteristics of our sample, this is the largest analysis window we can define without reducing significantly the donor units measuring past observations of municipal issuer's volatility.[12].

In summary, our empirical approach hinges on the assumption that the data-generating process of municipal market volatility could be characterized by factors specific to municipal issuers (i.e. fiscal health, and economic performance), and by factors common to all financial instruments. With this framework in mind, our donor pool is comprised of volatility measurements stemming from two main groups: i) past observations of the municipal bond market, and ii) contemporaneous observations of financial markets. The first group aims to parse out uncertainty explained by time-varying factors specific to the municipal bond market, hence mitigating risks posed by potential dormant factors. The second category, on the other hand, looks to match the reaction to the pandemic shock observed across financial markets. We could be subject to omitted variable bias if there are volatility drivers on the municipal bond market that are not common to the volatility drivers of the financial instruments (i.e. sovereign bonds, stock markets, commodities) we considered for the donor pool.

6 Main Results

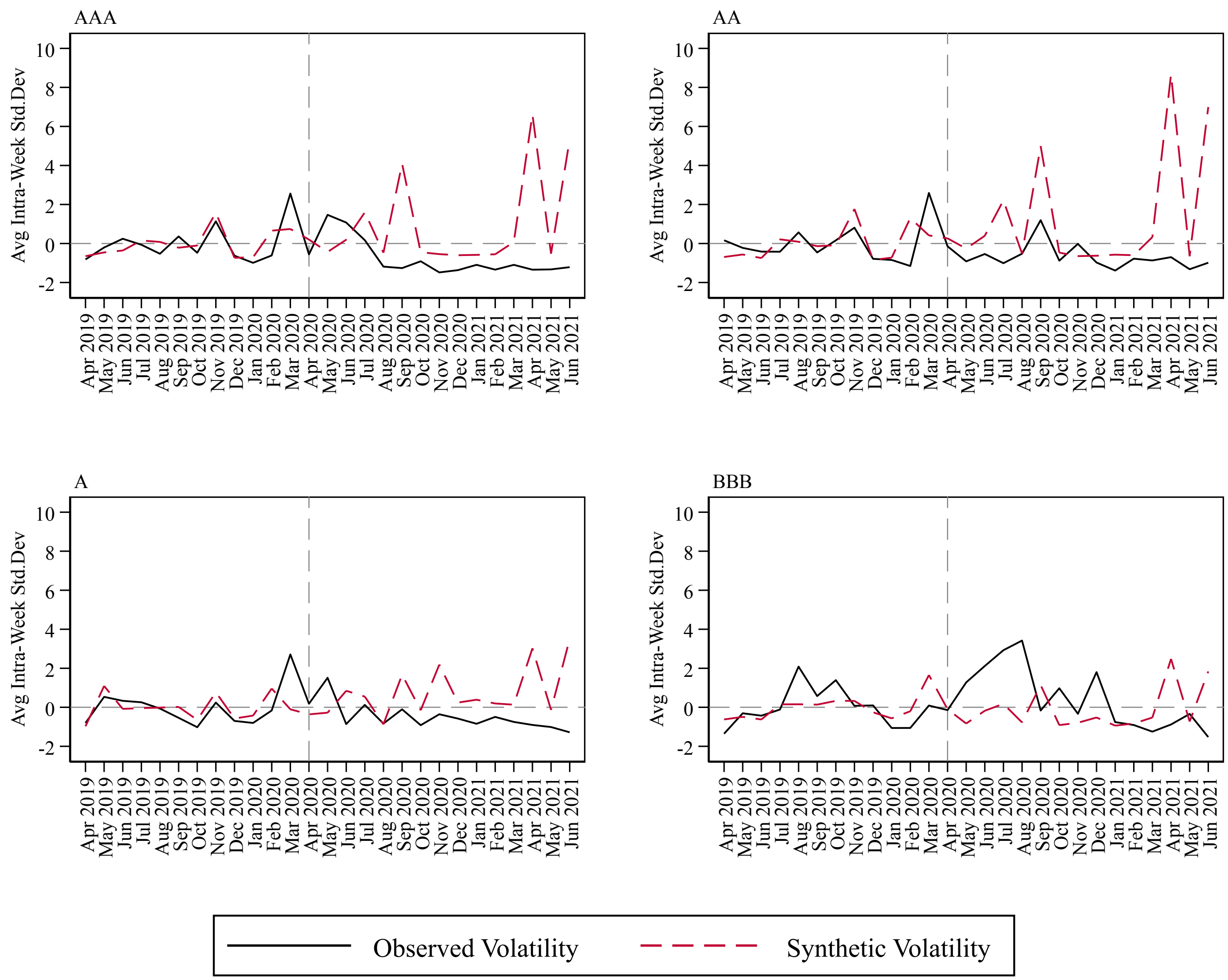
Figure 3 shows a visual representation of the observed volatility (black solid line) against the synthetic counterfactual (red dashed line) computed using the synthetic control estimator. Four panels are included, one for each credit rating group. From visual inspection of the observed volatility levels, it is clear that towards the pandemic there was a significant increase in the uncertainty surrounding the municipal bond market, reaching its maximum level during March 2020. Moreover, it seems that such uncertainty dissipated after the announcement and implementation of federal aid to state governments since the observed volatility returned to levels similar to the ones observed during the pre-treatment period. The synthetic control (red dashed line) shows a prediction of the volatility in the municipal market for the states that comprise each credit rating group, had the federal government not intervened in the market. Hence, the difference between these two lines captures the effect of the intervention easing the turmoil experienced during March 2020. For issuers at the AAA and AA rating groups, the synthetic control predicts levels of volatility larger than the ones observed after the intervention. In other words, our estimates suggest the interventions of April 2020 led to a reduction of the observed volatility levels for these

[12]Increasing the length of the pre-intervention period would require a different definition of the 2-year cohorts used for the analysis such that we ensure non-overlapping groups. The natural trade-off of this approach is a reduction in the number of donor units used to build the counterfactual

rating categories.

For states in the A-rated group we document a similar story, although there are not visible differences between the observed and synthetic volatility during the first six months following the intervention. The story told by the fourth panel, however, is rather different as the observed volatility was higher than the synthetic prediction for the first 9 months after the provision of direct aid to state governments. While this could suggest the policy

Figure 3: Average Volatility in the Municipal Market: Observed vs Synthetic Volatility



Note: Each panel shows both the time series for the observed and the synthetic normalized series of the monthly average intra-week volatility in the secondary market for each credit rating category. Normalization using the procedure described in Section 4.

led to an increase in the observed volatility of issuers in this group, it is worth noting that this group is built upon trades of bonds issued by the states of Illinois and New Jersey (see map at Figure A.3). Works by Haughwout et al. (2022a) and Fritsch et al. (2021) have noted Illinois benefited from the facility by successfully issuing \$3.2 billion in notes (i.e. approximately one-third of state's maximum eligible borrowing) at a 3.8% coupon rate, but also by observing lower borrowing costs in the following months. This could be one of the factors driving the observed volatility during this period.[13]

Table 2 presents the ATE estimates for each credit rating category. We document large and statistically significant volatility reduction for the states in the AAA and AA-rated groups. For instance, results in column (1) imply a point estimate of $\hat{\tau}^{AAA}$ = -0.1652, significant at the 1% level. In other words, provided the pandemic shock to Munibond markets of March 2020, the volatility AAA-rated states would have been 16.5 basis points higher had the actions of April 2024 not taken place. Conversely, had the federal government not intervened through the liquidity facility. We compute the ATE on unnormalized series of the treatment effect (see the transformation explained in Section 4). This allow us to express the effect in basis points from the yield.

To add some perspective to this result, we calculate measures of the incremental volatility experienced during March 2020 relative to the average volatility recorded in the pre-intervention period. This statistic provides a baseline measure of the magnitude of the turmoil experienced during the declaration of emergency and the lockdown. On the other hand, the ATE summarizes the volatility change in the 15 months following the intervention from the federal government. We then take the ratio of the ATE and this measure of the excess (or incremental) volatility at the onset of the pandemic. Intuitively, this statistic approximates the reduction in volatility attributed to the intervention, as the percentage of the incremental volatility documented during March 2020. For example, our computation of incremental volatility for AAA issuers is 26.4 basis points. This implies the volatility reduction induced by the policy was equivalent to 62.4% of the volatility increase experienced when uncertainty in financial markets was arguably at is peak. Another way to interpret our results is that by the 15th month following the intervention, the uncertainty dissipated by the policy reached 62.4% of the uncertainty observed during March 2020. The magnitude of this change points toward significant policy effects and highlights the central bank's ability to restore confidence in financial markets and dissipate uncertainty.

We document a similar story for AA-rated issuers. ATE estimates suggest a volatility reduction of 19.1 basis points, significant at the 1%. Expressing the ATE in terms of

[13]On June 2, 2020, the state government of Illinois announced it would tap the MLF by issuing \$1.2 billion. This transaction concluded in December 2020, when the state disbursed the resources from that bond issue. See Figure A.1 in the Appendix for further reference.

the incremental volatility observed during March 2020, our model implies the volatility reduction equivalent of 70.16%. This estimate is larger than the one for AAA-rated issuers, even when the excess volatility during the pandemic is slightly larger for the AA-rated group. This is explained by the magnitude of their treatment effect, which highlights reduce the uncertainty reduction experienced by this segment of the market. Estimates for A-rated issuers also align with this trend, implying a decrease in volatility associated with the implementation of federal policies. The coefficient for this group suggests a volatility reduction of 11.5 basis points with a p-value lower than 0.05, and equivalent to 43.57% to the excess volatility experienced by this rating group.

For BBB-rated governments ATE estimates are smaller and lack statistical significance. While noisy, the point estimates for these groups confirm the negative relationship between credit rating and implied volatility reductions observed for higher-rated governments. Altogether, these results suggest that higher-rated issuers benefited more from the announcement of federal aid than lower-rated issuers.

To assess our model's goodness of fit we adhere to Abadie et al. (2010) and look at the root mean squared prediction error (RMSPE) on the pre-intervention period. We report these statistics in Table 2 as well. Small statistics suggest the synthetic estimate fits adequately the observed volatility in the pre-treatment period. The magnitude of these statistics for this specification align with the visual inspection of Figure 3 where the differences between the treated and synthetic series during the pre-intervention period seem to be negligible across all credit categories.

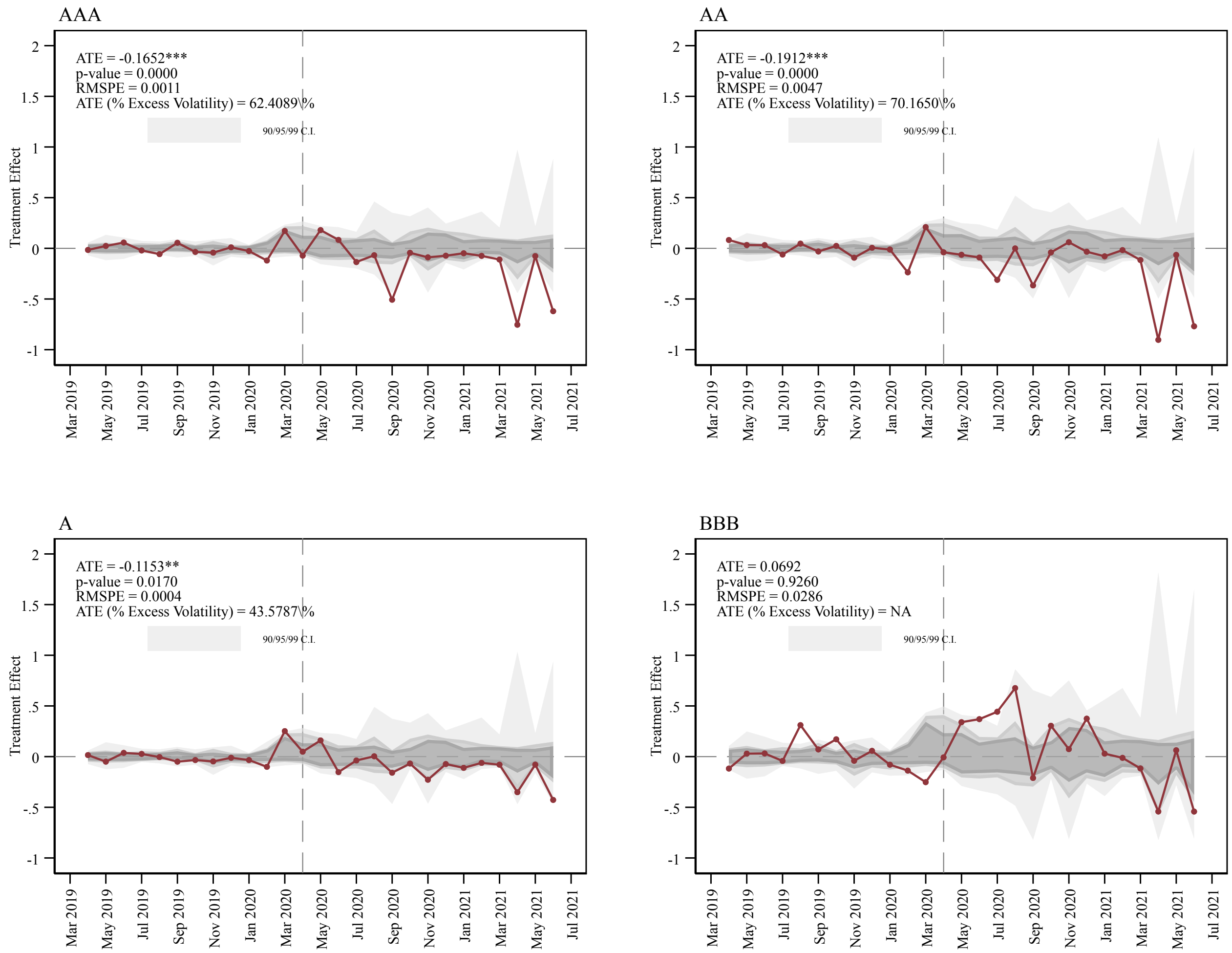
To summarize the results of this model, Figure 4 shows the treatment effect estimates (along with the placebo distribution) across the analysis window for each rating category. These *smokeplots* summarize the goodness-of-fit of the model, the magnitude and dynamics of the treatment effect, and the statistical reliability of the treatment effect estimates. This visualization of the results is useful to analyze the temporal heterogeneity in the treatment effect estimates. For instance, for issuers in both the AAA and AA-rated groups, significant volatility reductions appeared six months after the intervention. Treatment effect estimates for the A-rated category are within the 95% confidence region for most of the post-treatment period, thus confirming the significance of this ATE estimate at this level. For BBB-rated issuers, the treatment effect predicts a volatility increase induced by the policy. However, this change is within the treatment effects found at the placebo distribution of this estimate, thus explaining its lack of statistical power.

Table 2: Average Treatment Effects: MLF impact on Municipal Volatility

	(1)	(2)	(3)	(4)
	AAA	AA	A	BBB
Average Treatment Effect (a)	-0.1652***	-0.1912***	-0.1153**	0.0692
	(0.0484)	(0.0493)	(0.0458)	(0.0825)
Historic Volatility (b)	0.2014	0.1539	0.1944	0.5084
Volatility March 2020 (c)	0.4661	0.4263	0.4591	0.5244
Excess Volatility (d = c - b)	0.2647	0.2724	0.2647	0.0160
ATE, % Excess Volatility (e = a/d)	62.41%	70.17%	43.58%	NA
P-Value (Left Tail)	0.0000	0.0000	0.0170	0.9260
RMSPE	0.0011	0.0047	0.0004	0.0286

Note: Each column shows the results of the synthetic control estimator for each credit rating category. The Average Treatment Effect (a) is computed using the 15 months following the intervention. Corresponds to the average of the treatment effect calculated on volatility series expressed in percentage points, that is reverting the normalization process described in Section 4. Hence, ATE is expressed in percentage points. ATE standard errors from placebo inference are reported in parentheses. Statistical significance is determined using a left-tail rank-based p-value (i.e. count the number of times the ATE of the treated unit is smaller (to the left) than the ATE for each unit on the placebo distribution. A */**/*** indicates significance at the 10%, 5%, and 1% levels, respectively. Historic volatility (b) shows the average volatility observed during the pre-intervention period, excluding March 2020. Volatility March 2020 (c) is reported below the Historic Volatility (b). We define Excess Volatility as the simple difference between the volatility recorded in March 2020 and the historic volatility (d = c-b). ATE, % Excess Volatility expresses the ATE as a percentage of the excess volatility (e = a/d). NA is reported when the result of this computation exceeds 300% in absolute value due to the small value of the Excess Volatility. RMSPE corresponds to the root mean squared prediction error of the synthetic control estimator.

Figure 4: Treatment Effect Estimates by Credit Rating Category



Note: Each panel shows treatment effect estimates $\hat{\tau}_t$ for each credit rating category (solid red line). Treatment effect estimates from normalized series of the monthly average intra-week volatility in the secondary market for each credit rating category. Normalization using the procedure described in Section 4.The shaded area surrounding the line depicts the distribution of the placebo distribution for each period on the analysis window. Shaded areas correspond to the percentiles of the placebo distribution for each period. Percentiles at 1%,5%, and 10% are displayed in gray scales. Following Table 2, reported ATE is expressed in basis points (i.e. reverting the normalization described above).

7 Robustness Checks and Treatment Effect Heterogeneity

7.1 Analysis with fixed credit risk profile

The baseline specification examines the volatility of state issuers by creating categories using the credit rating distribution observed in 2019. This approach provides a clear identification of the intervention effect by fixing the credit risk profile of states at levels measured at the beginning of the window of analysis. In other words, it mitigates the effect that changes in the state's creditworthiness during pre-treatment period could have on our ATE estimates. However, it comes at the expense of limiting the interpretation of our results in terms of credit risk, which could be one of the main mechanisms driving the heterogeneity of the policy effect. For instance, bond issues from the same state could observe different credit ratings at issue depending on the characteristics of the debt (e.g. coupon rate, maturity, repayment pledge). To the extent there is enough variability in the credit ratings assigned to bonds issued by the same state, then our categorization could obscure the differences in the policy effects driven by credit risk.

To assess the robustness of our results to this problem, we employ a different grouping criterion to build the credit rating categories used to compute the volatility estimates on the municipal market. For this exercise, we consider the credit rating assigned at issue to build the volatility estimates for each group. Under this categorization, the treated series measures the volatility in the secondary market of all bonds with the same credit rating assigned at issue, thus allowing variation one the distribution of states at each rating group across time. For example, in our sample bonds, the average rating assigned to bonds issued by states like Arizona and North Carolina observed a change between 2019 and 2020. Furthermore, we also observe average rating changes at securities issued by states like Florida, Indiana, and Mississippi between 2020 and 2021. Moreover, this flexible categorization overcomes the problem highlighted in Section 4 (i.e. low trading levels for A-rated bonds during Q1 2019) and allows us to compute volatility measurements for this period as well. Hence, as part of this robustness check, we change the length of the window of analysis to be a symmetric 15-month window centered around March 2020.

Figure A.6 and Table A.3 in the Appendix show the results from this estimation. While we also document a negative ATE for the AAA group, this is considerably smaller compared to the one presented at Table 2. The reduction implied by this model for this group is equivalent to 15.54% of the excess volatility observed by this group during March 2020. Under this specification, we also find large and significant volatility decreases for the AA and A rating groups. The magnitude of these coefficients suggest the uncertainty dissipated by the implementation of the policies during the post-intervention period was around 90% the excess volatility of these groups. Like in the baseline specification, we do not find significant results for BBB-rated bonds.

These results confirm large ATEs for bonds rated A and above. However, the story implied by these coefficients suggest more pronounced policy effects for AA and A, while milder for AAA bonds. Mechanically, this is explained by the trend estimated for the synthetic volatility of AAA bonds, which suggests this group should have experienced low volatility levels, regardless of the implementation of the CARES Act and the MLF. This is consistent with a model where the creditworthiness of these bonds provided enough certainty to investors to the extent that it kept volatility around pre-intervention levels. Effects are larger for the AA and A rating groups since the synthetic series computed for this groups suggest the volatility observed by these groups would had been significantly more pronounced in the absence of federal policies that restored confidence to the market.

Altogether, these results suggest the effects of the intervention were more widespread than previously estimated. All bonds rated above BBB observed lower volatility levels due to the intervention. Estimates for AAA bonds suggest milder policy effects that, while are still significant at the 5% level, show slightly larger standard errors. When comparing these estimates with the ones obtained at Table 2, the larger magnitude documented at this specification could suggest the direction of any potential omitted variable bias associated with state-specific characteristics is negative, thus leading to a possible overestimation of the ATE.

7.2 Heterogeneity driven by direct federal support

As described in Section 3, the enactment of the CARES Act and the implementation of the MLF happened within 14 days from each other. Moreover, both policies had similar eligibility criteria for state and local governments to the extent that all state governments received direct aid from the Treasury through the CRF and were eligible to participate in the MLF. There are few efforts in the literature that aim at disentangling the effect of each policy (Bi and Marsh, 2020) , which could shed some relevant light on the debate around the effectiveness of federal intervention through fiscal channels (i.e. the federal government) or via the monetary authority (i.e. the Federal Reserve).

Bi and Marsh (2020) acknowledge that the proximity of these two policies adds complexity to the analysis of the MLF as it hard to separate the incremental effect that the announcement of the facility had on investor's behavior, which was already adjusting to the informational nudge given by the CARES Act. Nonetheless, their analysis fails to separate individual effects from each policy as it measures the cumulative reduction in municipal spreads observed until the occurrence of some policy event.

In this section we aim to shed some light on the policy effects driven by the intervention through fiscal channels by looking at heterogeneity in the ATE driven by the distribution of funds to states during during the first months of the pandemic. For that purpose, we use the FFIS data on the estimated funding for the COVID-19 pandemic, which is available at the replication package of Green and Loualiche (2021). This data source shows the transfers to state governments from the federal government through the different funds and programs used by the federal government to channel resources to the states. This data shows the grant funding given by June 23, 2020, which mounted to $317 billion. Forty-seven percent of these funds ($150 billion) were transferred by the Treasury through the CRF. The rest was executed through several programs from the Department of Education, the CDC, the Department of Transportation, among others.

Using the 2019 population estimates from the Census, we computed per capita estimates of federal grant funding given to states until June 23, 2020. Then, we segment the states between those above (and below) the median amount of per capita federal grant funding for COVID-19. For consistency with the main results, we adhere to that credit rating categorization (i.e. cohorts of states fixed across time). In other words, we first classify the states using the credit rating groups at Figure A.3 and then we segment each category according to their relative position on the federal aid distribution. This approach allows to examine heterogeneity in the treatment effect potentially driven by the magnitude of federal aid to state governments, while keeping constant state's credit risk.

An implication of the proposed categorization is that states with above median transfers are, in most cases, the same that received the minimum CRF allocation of $1.25 billion. To clarify, the CRF allocation rule was proportional to each state's population with the caveat that no state received less than $1.25 billion. In practice, this implied that all states with population below 3.3 million inhabitants (i.e. Utah is the state at the observed population threshold that groups the states between the minimum allocation and the one proportional to population). This implies a non-linear relationship between total grant funding and state's population. Figure A.9 in the Appendix displays this relationship. In sum, states with above median grant transfers are, in general, states with relatively small population, and vice-versa.[14]

[14] As an additional robustness check, we ran this analysis using the distribution of the CRF to analyze the heterogeneity within each rating category. For this specification, instead of using the median as threshold, we considered the allocation rules of the CRF and split the sample of

Table A.4 on the Appendix shows the results from this exercise. First, it stands out that, with the exception of the BBB-rated group, measures of excess volatility during March 2020 were larger for the states below the median, compared to the states that received aid above the median. Since below median states are characterized by larger populations, they could had been surrounded by higher uncertainty on the negative effects of the pandemic (e.g. places more densely populated could observe larger economic dislocations). Figure A.11 on the Appendix shows the distribution of states according to this criterion.

Aligned with the main specification, we document significant volatility reductions for issuers rated A and above. AAA issuers, both above and below the median of the distribution of federal COVID-19 support, observed a volatility decrease significant at the 1% level. Point ATE estimates for the AAA-rated group suggest a larger effect for the above-median segment, with a volatility reduction of 24.8 basis points, relative to the counterfactual scenario without the announcement of federal aid. The ATE for the below-median AAA segment implies a volatility reduction of 18.5 basis points. When expressing these results as percentage of the excess volatility observed during March 2020, the implied reduction for the above-median segment (i.e. states with larger populations) is twice as large to the estimated for the below-median segment.

Results from AA and A-rated issuers show mixed evidence on which segments of these rating categories capture most of the policy effects. ATE estimates for the AA-rated group find a larger effect for the below-median segment, a volatility decrease of 16.9 basis points equivalent to 63.3% of the excess volatility estimated for this group. For the A-rated group the story is different as significant volatility reductions are only observed for the above-median segment. Point estimates for this subgroup imply a negative treatment effect equivalent to 81.1% of the excess volatility observed during March 2020. For the counterparts of the each rating category, interestingly, the ATE estimates are indistinguishable from zero. Panels at Figures A.13 and A.14 reveal this result is caused by the low volatility levels that characterize the synthetic estimates for these subgroups.

Observing larger volatility reductions for the above-median segment is consistent with a scenario where direct support from the federal government is the main factor providing certainty to the financial market. As shown in Figure A.9, states on the above-median segment observe significantly larger amounts of per capita federal grant funding for COVID-

states between those that received the minimum $1.25 billion allocation and those that received a payment above this amount. This strategy aligns with the analysis at Green and Loualiche (2021). The results from this exercise do not differ significantly from the ones presented at Table A.4 and these are available upon request. This is explained due to the similarity between the distribution of states using each criteria. To exemplify this, across the 50 states and DC, the correlation between the CRF allocation and total grant funding for COVID-19 (both per capita) is 0.94. See the map at Figure A.11 for further reference.

19. The financial buffer provided to these states by the federal government represented a larger proportion of their budgets, while at the same time such states could be less exposed to some of the uncertainty surrounding the effects of the pandemic since they are characterized by lower levels of population density.

On the other hand, documenting larger volatility reductions for the below-median segment could be explained by a more prominent role of the Federal Reserve as a LOLR. For states in this segment of the distribution, total grant federal funding represented a lower proportion of their fiscal revenues: for below-median states was, in average, 12.39% of state's fiscal revenues, while for above-median states was 16.31%. See Figure A.10. Observing larger policy effects for the segment of the distribution with lower per-capita funding could be consistent with a story where investor's response was directly influenced by the saliency of the information available about the effects of the pandemic on the economy. The below-median segment is composed by the larger states that were on the spotlight during the early developments of the pandemic. Moreover, given the size of these states, they arguably represent a larger proportion of the municipal bond market. With this in consideration, it could be the case that the below-median segment manifests larger volatility reductions. It should be noted that there are no significant differences on the magnitude of the economic shock experienced by state governments across segments of the per-capita funding distribution, thus we rule out this as a factor potentially driving this heterogeneity on the effect.

Figures A.13 - A.18 on the Appendix show the results of this exercise. In general, we do not find consistent evidence to support any of the interpretations above for the full market. Evidence from the AAA and A-rated groups suggest larger policy effects for the above-median segment of the distribution of per-capita federal grant funding to state governments. However, these results should be interpreted carefully as, in some cases, the estimated synthetic volatility series only represent one or few states. The AAA group includes 8 states (6 below-median, 2 above-median), and the A-rated group 3 states (1 below-median, 2 above-median). See Figure A.12 for reference.

Estimates from the AA-rated group (which cover 25 states) are less influenced by these concern as there are several states in each segment of the distribution (12 states below-median and 13 states above median). These results imply larger treatment effects for the states at the below-median segments (larger states). Considering this, it is likely the case that the analysis within this rating group shows stronger internal validity than the one conducted for the other groups. Moreover, since it covers half the states on the country, it arguably provides results that represent more accurately the overall effects at the market level. While this evidence alone is not sufficient to formulate a conclusion on which type of policy was more effective bringing calm to the market, it does provides some suggestive evidence on the relevance of making liquidity backstops available to issuers, when there is turmoil on financial markets. Finally, we highlight that results for both segments of the BBB-rated group are not significant. This confirms the findings for this rating group at the other specifications estimated in this paper.

7.3 Persistence and Dynamic Treatment Effects

The ATE presented at all scenarios in this paper is calculated considering the 15 months following the intervention. In other words, that statistic approximates the reduction in volatility induced by these policies observed between April 2020 and August 2021. The trends on the treatment effect series shown in Figure 4 underline the sensitivity of the results to the length of the post-intervention period used to compute ATEs.

In this section, we directly examine this by computing the ATE across different lengths of the post-intervention period. We conduct this analysis using the treatment effect series from our main specification. The results of this exercise are displayed in Figure A.5. The solid line represents each point estimate of the ATE for the post-treatment period that begins in April 2020 and ends in month t. Hence, the point estimates of the ATE at Table 2 are found at the last month of this graph.

This visualization allows us to identify similarities and differences between the treatment effect estimates across credit rating categories. For instance, for all rating categories we observe the ATE decreases as the length of the measurement window increases. This could be a consequence of observing larger treatment effect estimates observed towards the end of the baseline window at Figure 4. It also stands out the differences on the trends observed between the ATE for each rating groups. ATE estimates for the AAA rated group suggest the reduction in volatility implied by these policies is observed until September 2020, whereas for states in the AA-rated group the reduction is significant since the third month after the intervention period.

To assess the statistical significance of this exercise, we build placebo distributions of the ATE for each window length. This allows to compare whether the magnitude of the ATE differs from the estimated placebos, as the time to evaluate the treatment effects of the policies increases. The percentiles of the placebo distribution are shown as the shaded areas (smoke) surrounding the ATE estimates. Rejection of the null hypothesis is visualized when the point estimates of the ATE are below the lowest percentiles of placebo distribution.

Results from this exercise suggest the effectiveness of the policy reported for the AAA group at Table 2 is visible since September 2020, as the ATEs reach significance at the 1% level since this month. For AA-rated issuers the ATE identifies significant volatility decreases following the third month after the intervention. Visual inspection of Figure A.5 reveals that by the sixth month after the intervention, our synthetic control estimator would have identified significant treatment effects for the AAA and AA rated groups. For A-rated issuers, the ATE observes a decreasing trend that becomes significant at the 5% level around November 2020. This summarizes the sensitivity of our baseline results to choices on the duration of the post-intervention period.

8 Conclusions

Financial markets experienced severe distress in the onset of the COVID-19 pandemic, creating adverse conditions for state and local governments looking for debt during this period. This paper studies the extent to which federal aid to state and local governments (or at least the signal it provides to financial markets) has on alleviating the volatility and dissipating the uncertainty that surrounds bond prices in crisis episodes. Using a synthetic control approach, we analyze the effect of the announcement and implementation of the MLF on secondary market transactions of bonds issued by state governments. Our estimates suggest federal intervention successfully alleviated market volatility for all issuers A-rated and above. Results from our analysis suggest that volatility reductions in the 15-months following the lockdown, are in the range of 43%-70% of the incremental volatility observed by municipal issuers when the turmoil of financial markets was at its peak. We document heterogeneous effects across credit risk levels. Across all our specifications, AA-rated bonds observed the largest reduction in volatility due to the intervention. However, we find that BBB-rated bonds observed higher volatility after MLF's implementation, although these coefficients are indistinguishable from zero at conventional statistical significance levels.

Our results present new evidence on the role the Federal Reserve as a LOLR, and the extent to which the MLF was successful restoring confidence on the municipal bond market, even when only two governments tapped into this policy. By looking at the heterogeneity in the treatment effect potentially driven by the magnitude of direct federal support to state governments, we attempt to disentangle the latter one from influence of the informational nudge provided by the Federal Reserve. We find that states whose per-capita capital grant aid for COVID-19 was below the national median (i.e. mostly bigger states) observe larger volatility reductions. Since the support to these states was lower in terms of their revenues (and hence their potential fiscal needs during the first stage of the pandemic), we argue this provides some suggestive evidence on the relevance informational role the Federal Reserve plays for investors on the secondary municipal market.

These results are relevant in light of other shocks were the Federal government has shown a less active role in the recovery of Municipal financial markets. For instance, some have pointed to the implications of the lack of action from the Federal government during the Great Recession and in particular among most vulnerable communities (Green and Loualiche, 2021; Rodden, 2023). The combination of policies to assist local governments, including a direct interventions in financial markets, such as the MLF which in turn provided an alternative as a Lender of Last Resort (LOLR), or the CARES Act, largely prevented a sluggish recovery. This is true even if the MLF was not necessarily tapped into, as its mere presence brings trust to potential investors and serves as a federal insurance, were local governments to experience hardship.

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9 Appendix

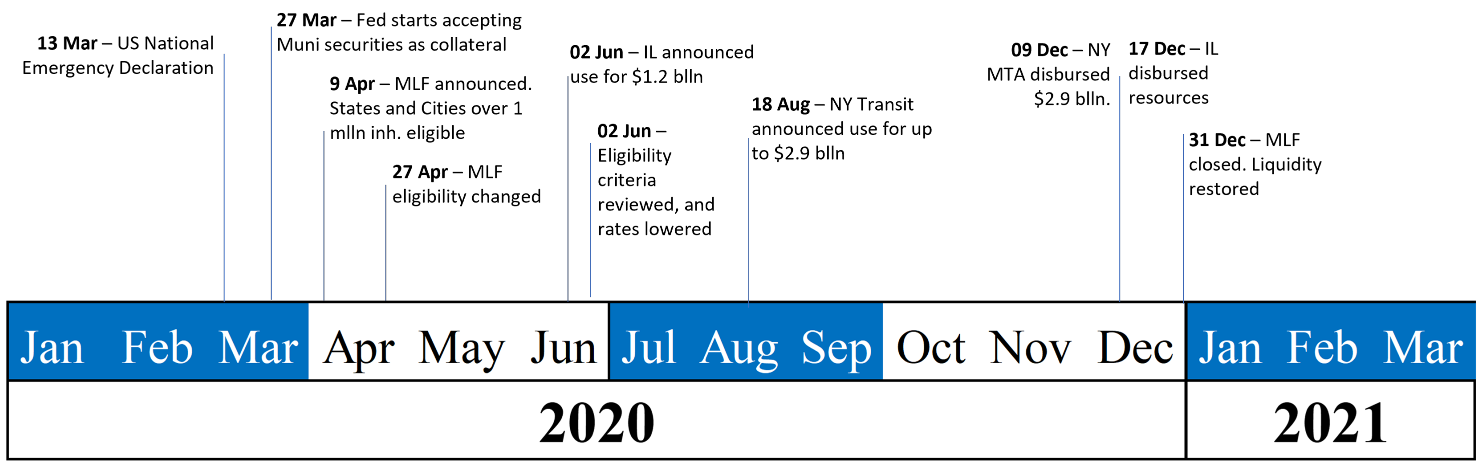
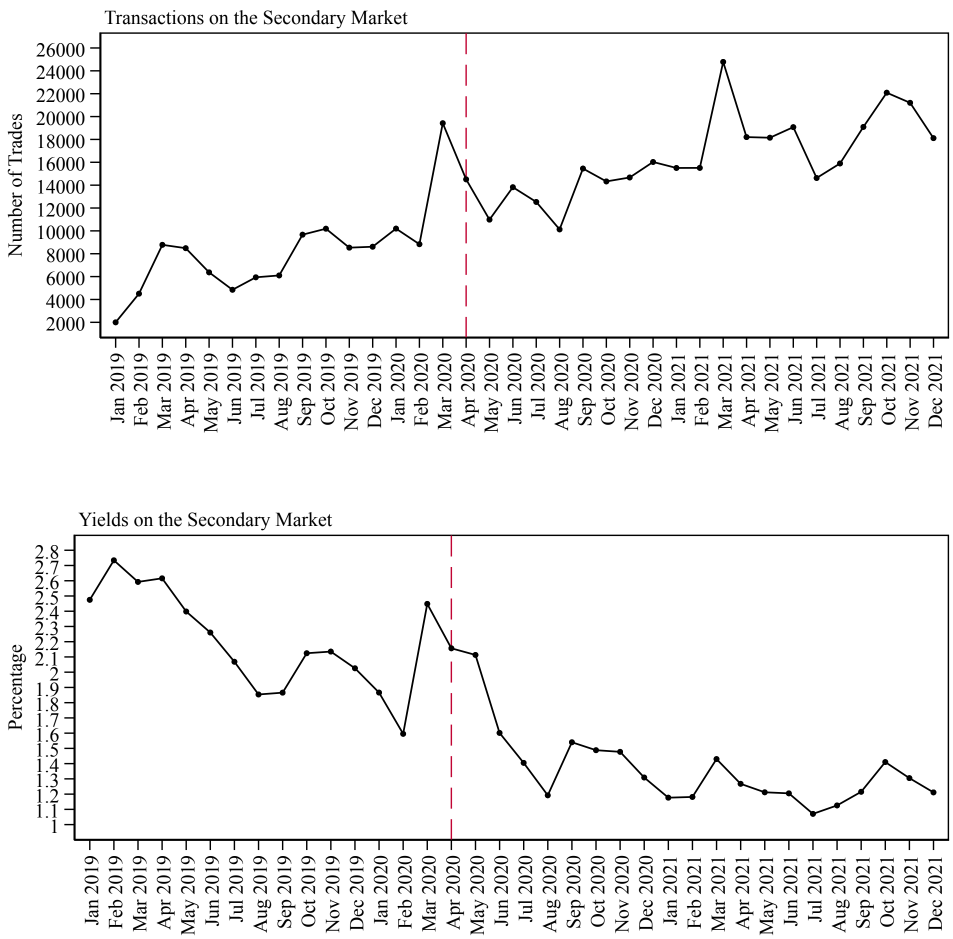


Figure A.1: Municipal Liquidity Facility Implementation - Main Events

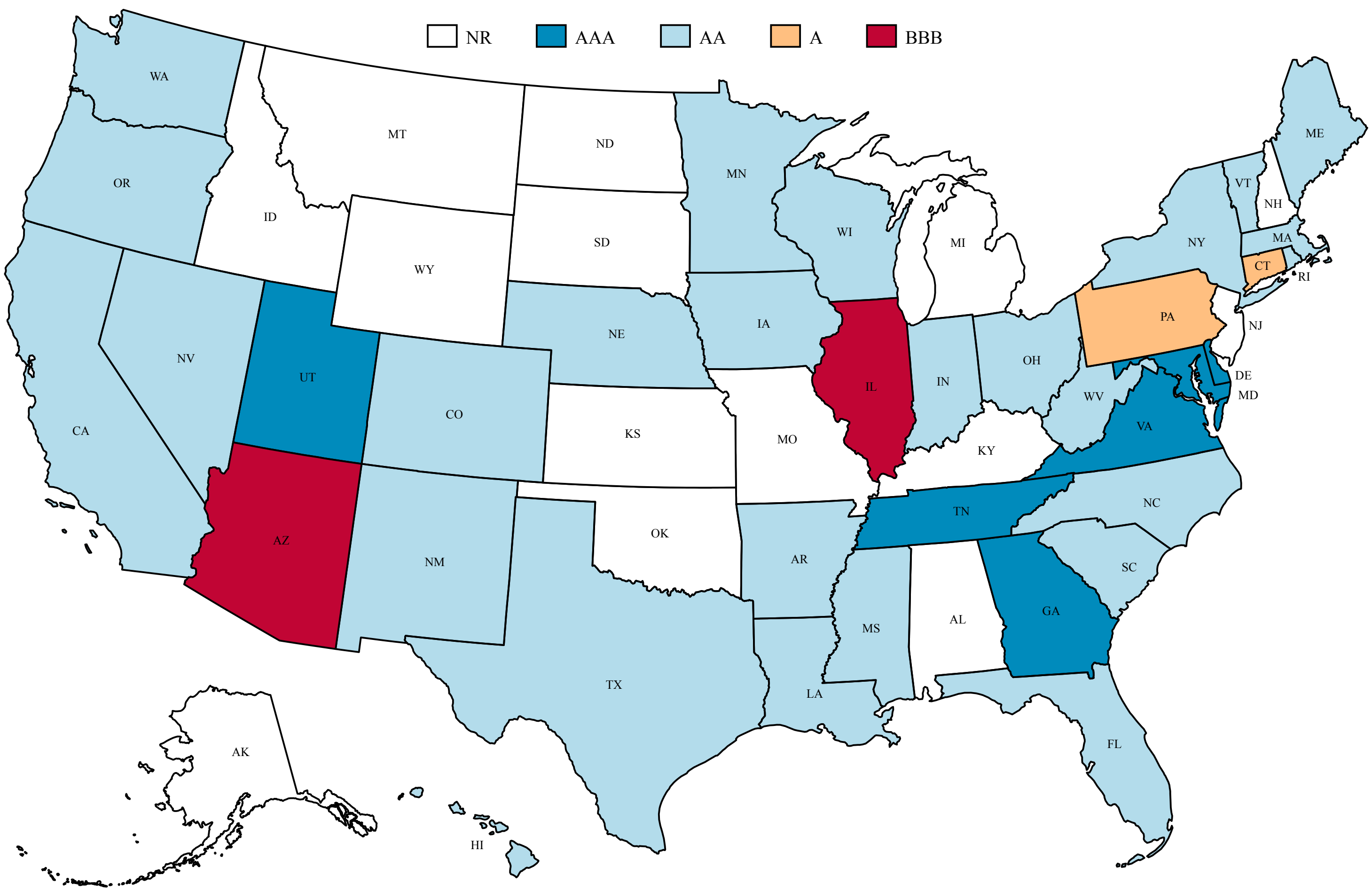
Note: This figure shows the timeline of the main events that led to the enactment of the MLF, as well as the relevant announcements regarding the policy. For the empirical analysis, we consider April 9, 2020 as the implementation date.

Figure A.2: Secondary Bond Market - State Issuers Active Between 2019 and 2021



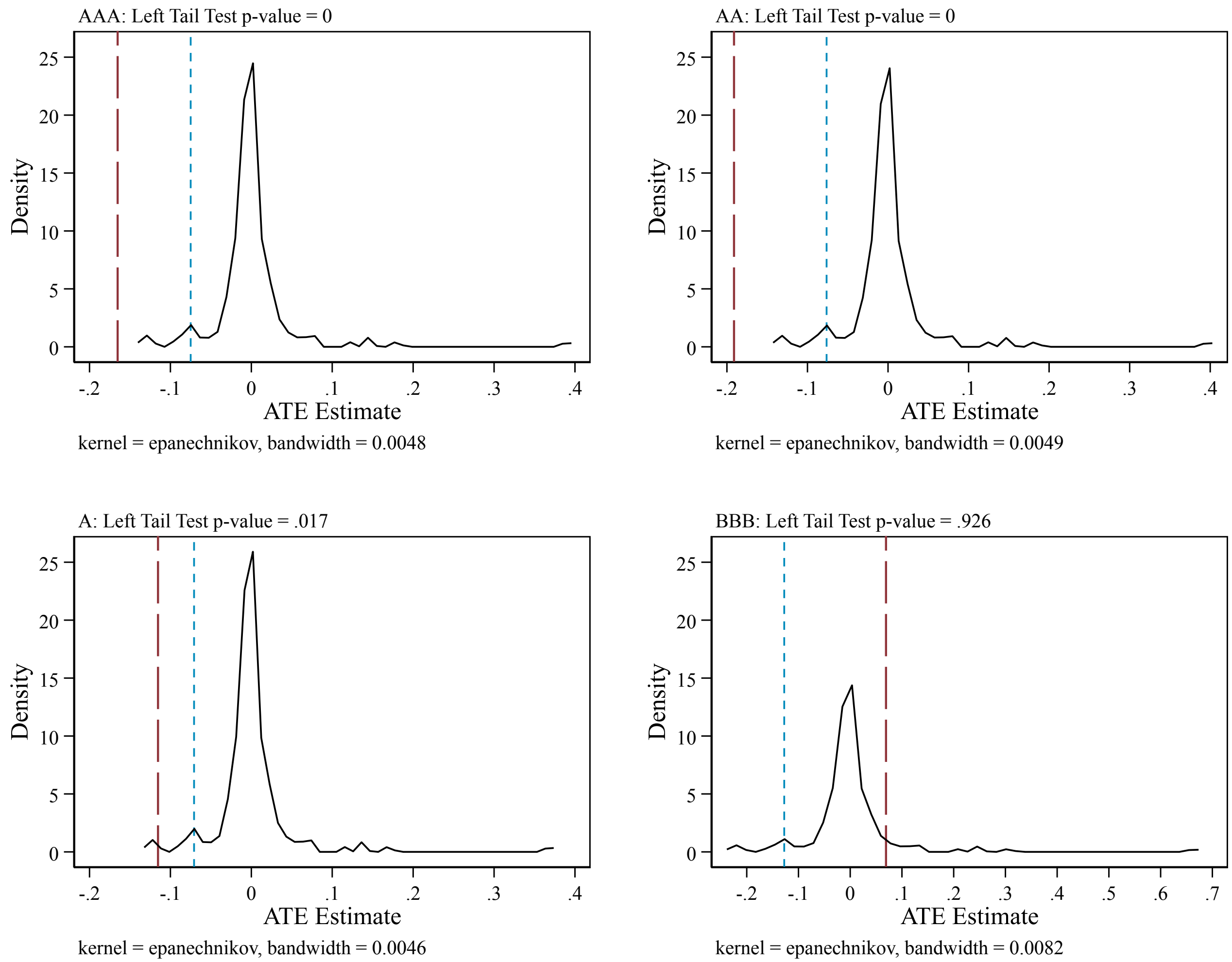
Note: This graph shows the aggregated trends on the bond sample considered for this analysis. This sample include bonds issued by state issuers between 2019 and 2021. These panels show secondary market metrics of such bonds across time. The panel on the top shows the number of trades observed in each month, while the panel on the bottom shows the average yield at trade for each month. The dashed red line shows April 2020, the month of the implementation of the Municipal Liquidity Facility, right after the enactment of the CARES Act.

Figure A.3: State Grouping by Average Credit Rating, 2019



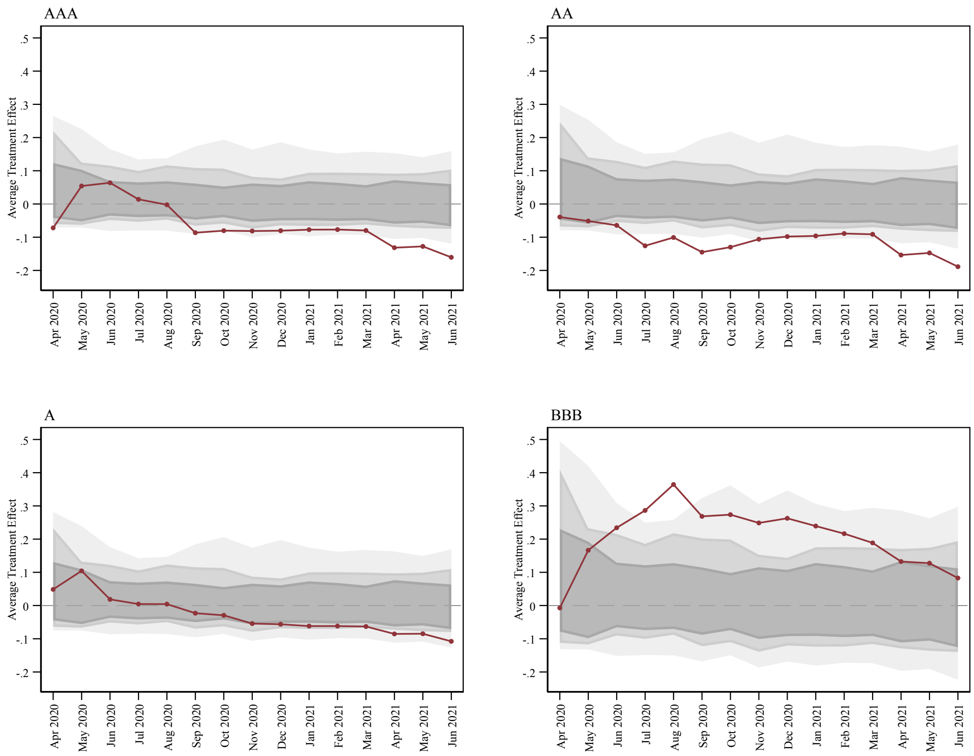
Note: This map shows the average credit rating assigned at issue observed by each state during 2019. The average is rounded to the nearest category. This map shows the composition of rating categories for the main results of this paper.

Figure A.4: Placebo Distribution of ATE Estimates by Credit Rating Group



Note:
This graph provides a visual representation of the computation of the left-sided rank-based p-values for each Average Treatment Effect estimate. The black line shows the kernel density of the placebo distribution, where the placebos are adjusted to the mean and standard deviation of each treated unit, during the pre-intervention period. The red dashed vertical line shows the point estimate of the Average Treatment Effect. The blue dashed line shows the 5% percentile of the placebo distribution, thus showing the threshold for rejection of the null hypothesis for the left-sided one-tail test. Statistical significance at the 5% level is reached so long the Average Treatment Effect is to the left of this threshold.

Figure A.5: Effect Persistence: Estimates by Credit Rating Category



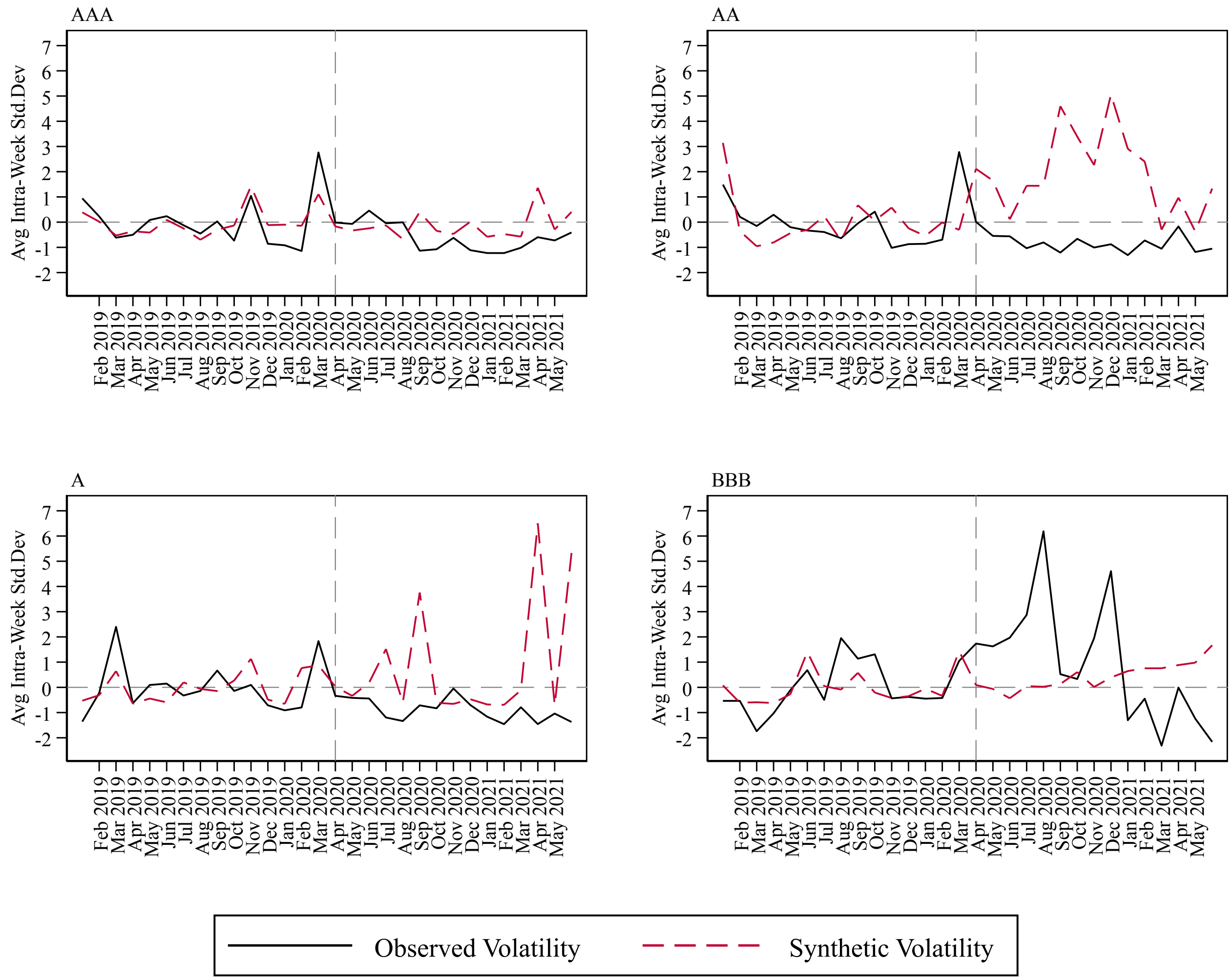
Note: This graph describes the cumulative treatment effect by showing monthly point estimates of the average treatment effect as the window to compute such statistic increases. The gray shaded areas show the percentiles of the placebo distribution of the ATE for each window. Percentiles at 1%,5%, and 10% are displayed in gray scales. The rejection zone of the null hypothesis ($ATE = 0$) is outside the lower bound of the shaded areas.

Table A.3: Robustness Check (Fixed Credit Risk Profile): Average Treatment Effects

	(1)	(2)	(3)	(4)
	AAA	AA	A	BBB
Average Treatment Effect (a)	-0.0386*	-0.2506***	-0.1548***	0.0311
	(0.0560)	(0.0613)	(0.0589)	(0.0673)
Historic Volatility (b)	0.1585	0.1395	0.2091	0.3812
Volatility March 2020 (c)	0.4068	0.4129	0.3823	0.4943
Excess Volatility (d = c - b)	0.2482	0.2734	0.1732	0.1131
ATE, % Excess Volatility (e = a/d)	15.5496%	91.6673%	89.3744%	27.4608%
P-Value (Left Tail)	0.0860	0.0060	0.0060	0.9140
RMSPE	0.0017	0.0051	0.0026	0.0146

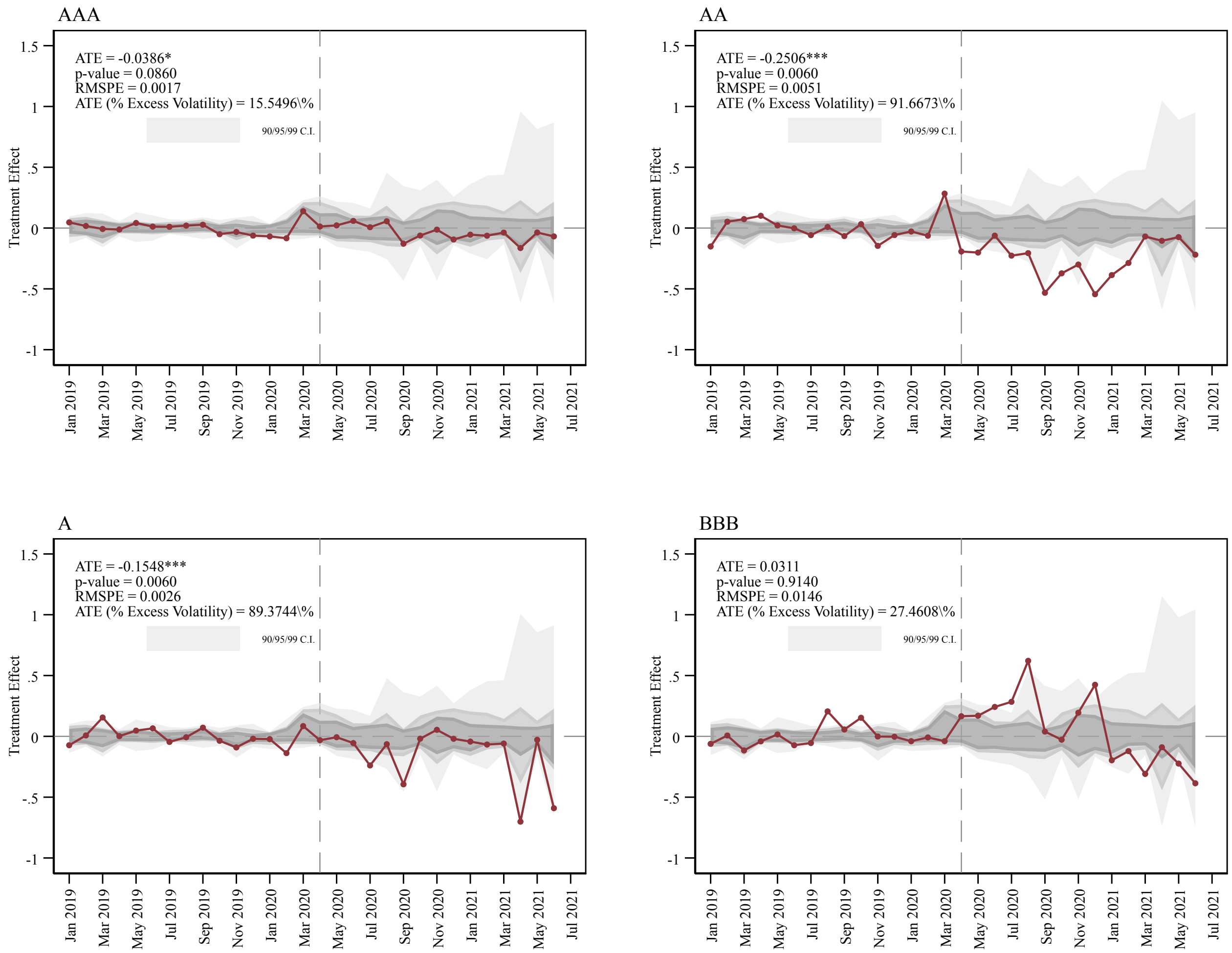
Note: Each column shows the results of the synthetic control estimator for each credit rating category. Average Treatment Effect (ATE) is computed using the 15 months following the intervention. Corresponds to the average of the treatment effect calculated on volatility series expressed in percentage points, that is reverting the normalization process described in Section 4. Hence, ATE is expressed in percentage points. Standard errors are reported in parentheses. These correspond to the standard deviation of the placebo distribution of each coefficient. Statistical significance is determined using a left-tail rank-based p-value (i.e. count the number of times the ATE of the treated unit is smaller (to the left) than the ATE for each unit on the placebo distribution. A */**/*** indicates significance at the 10%, 5%, and 1% levels, respectively. Historic volatility (b) shows the average volatility observed during the pre-intervention period, excluding March 2020. Volatility March 2020 (c) is reported below the Historic Volatility (b). We define Excess Volatility as the simple difference between the volatility recorded in March 2020 and the historic volatility (d = c-b). ATE, % Excess Volatility expresses the ATE as a percentage of the excess volatility (e = a/d). NA is reported when the result of this computation exceeds 300% in absolute value due to the small value of the Excess Volatility. RMSPE corresponds to the root mean squared prediction error of the synthetic control estimator.

Figure A.6: Robustness Check (Fixed Credit Risk Profile): Observed vs Synthetic Volatility



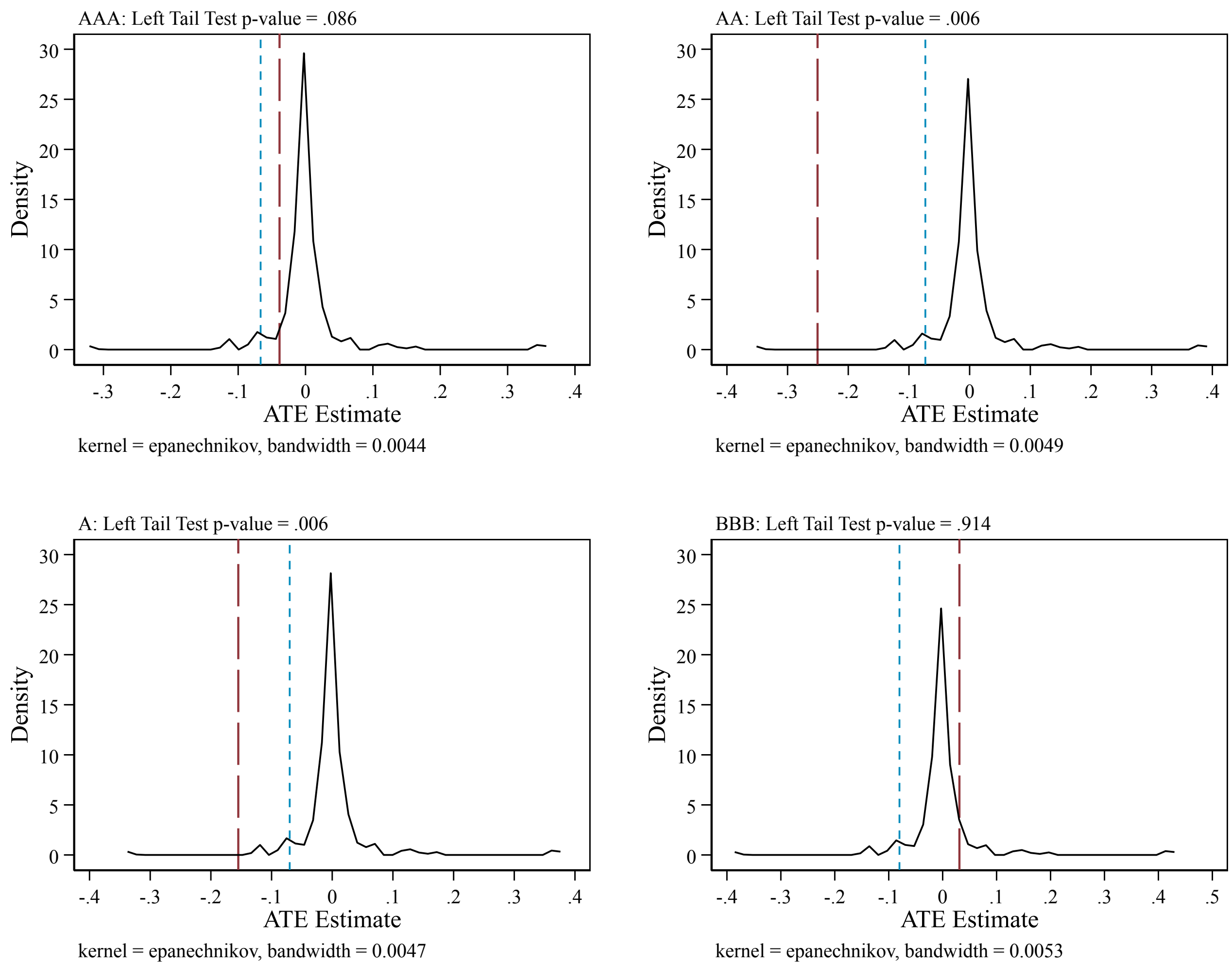
Note: Each panel shows both the time series for the observed and the synthetic series of the average weekly volatility in the secondary market for each credit rating category.

Figure A.7: Robustness Check (Fixed Credit Risk Profile): Treatment Effect Estimates



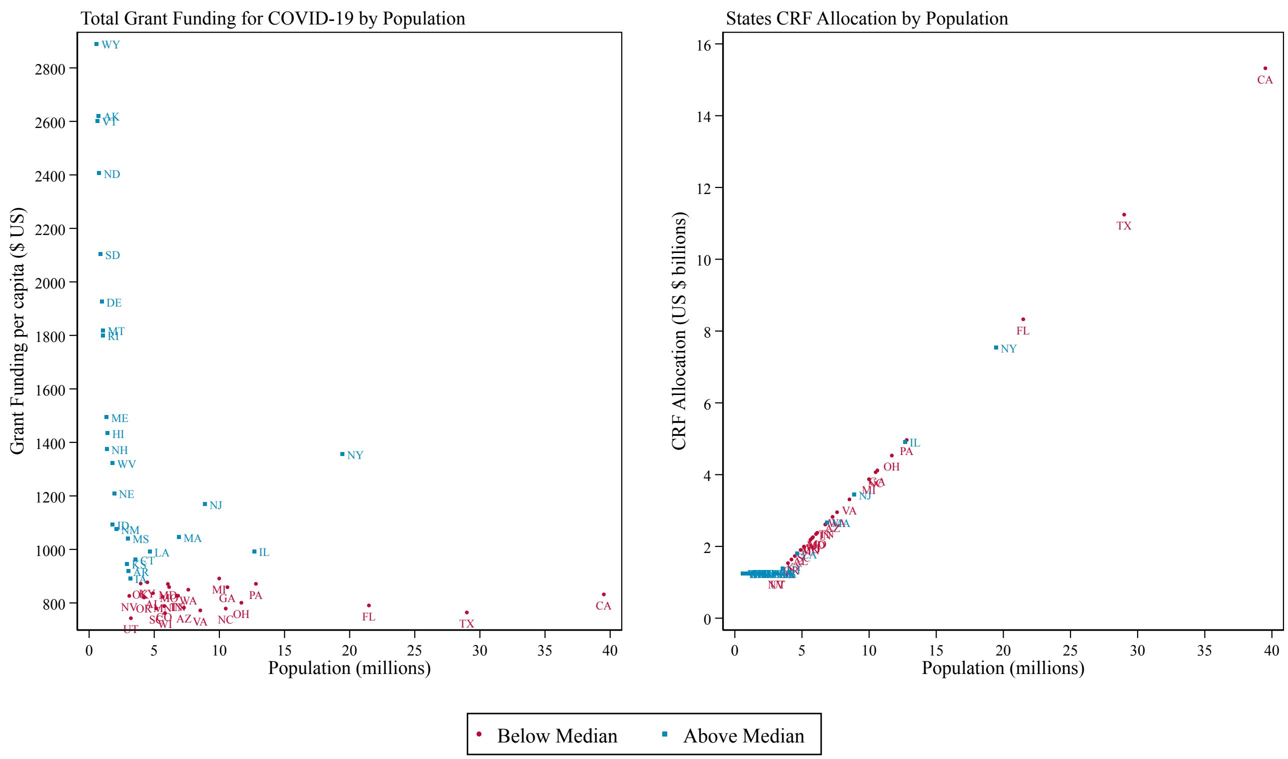
Note: Each panel shows treatment effect estimates $\hat{\tau}_t$ for each credit rating category (solid red line). The shaded area surrounding the line depicts the distribution of the placebo distribution for each period on the analysis window. Shaded areas correspond to the percentiles of the placebo distribution for each period. Percentiles at 1%,5%, and 10% are displayed in gray scales .

Figure A.8: Robustness Check (Fixed Credit Risk Profile): Placebo Distribution of ATE Estimates by Credit Rating Group



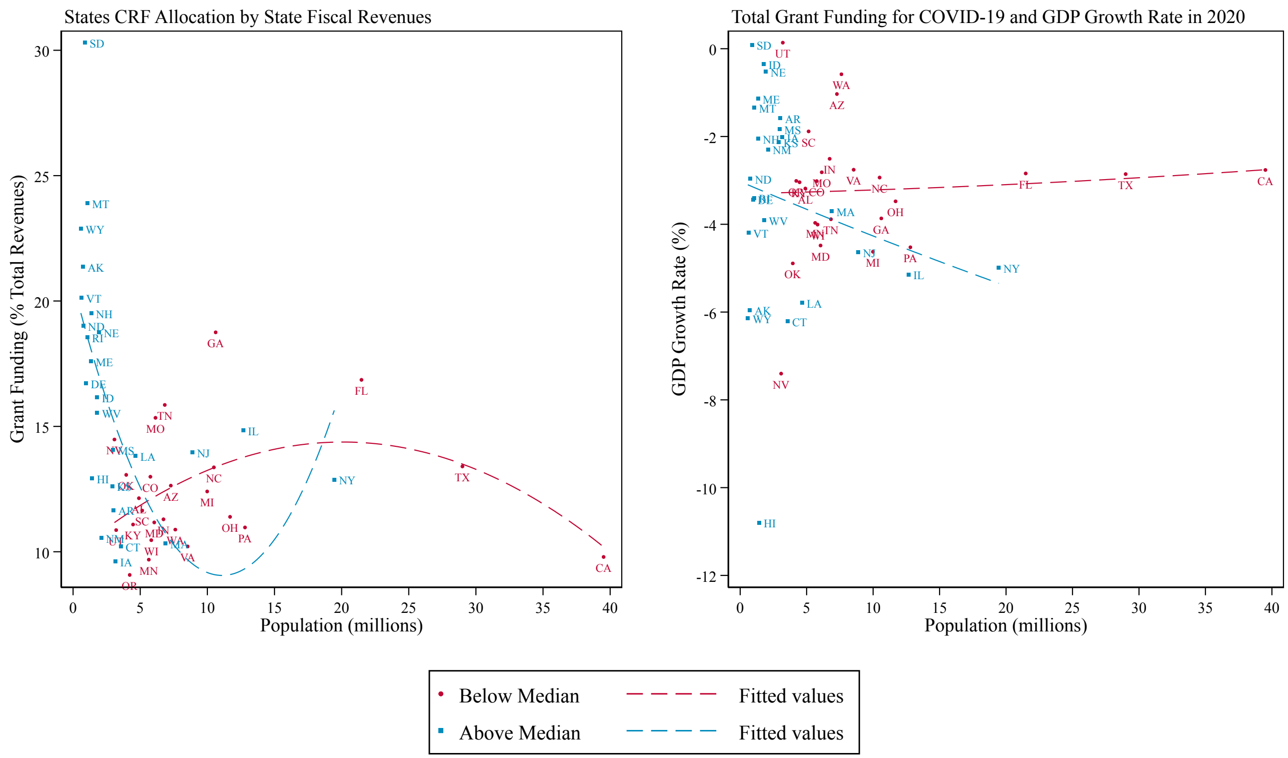
Note: This graph provides a visual representation of the computation of the left-sided rank-based p-values for each Average Treatment Effect estimate. The black line shows the kernel density of the placebo distribution, where the placebos are adjusted to the mean and standard deviation of each treated unit, during the pre-intervention period. The red dashed vertical line shows the point estimate of the Average Treatment Effect. The blue dashed line shows the 5% percentile of the placebo distribution, thus showing the threshold for rejection of the null hypothesis for the left-sided one-tail test. Statistical significance at the 5% level is reached so long the Average Treatment Effect is to the left of this threshold.

Figure A.9: COVID-19 Federal Funding for States (1)



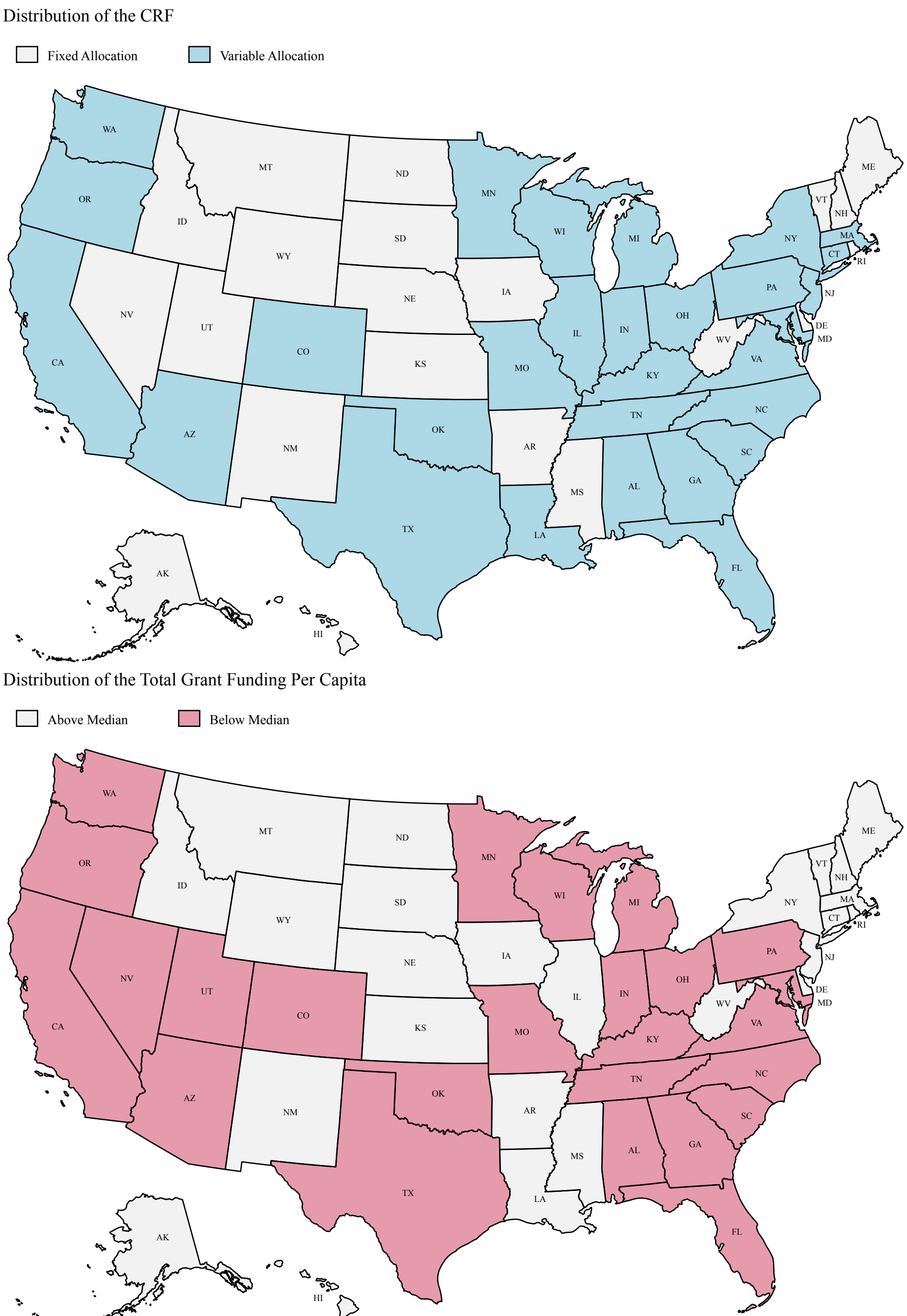
Note: Authors elaboration using FFIS data retrieved from the replication package of Green and Loualiche (2021). The panel on the left shows the estimates for per capita federal grant funding given to states plotted against each state's population level. The negative relationship shows that larger states observed lower per-capita funding. The panel on the right shows the correlation between total CRF allocation to state governments and population levels.

Figure A.10: COVID-19 Federal Funding for States (2)



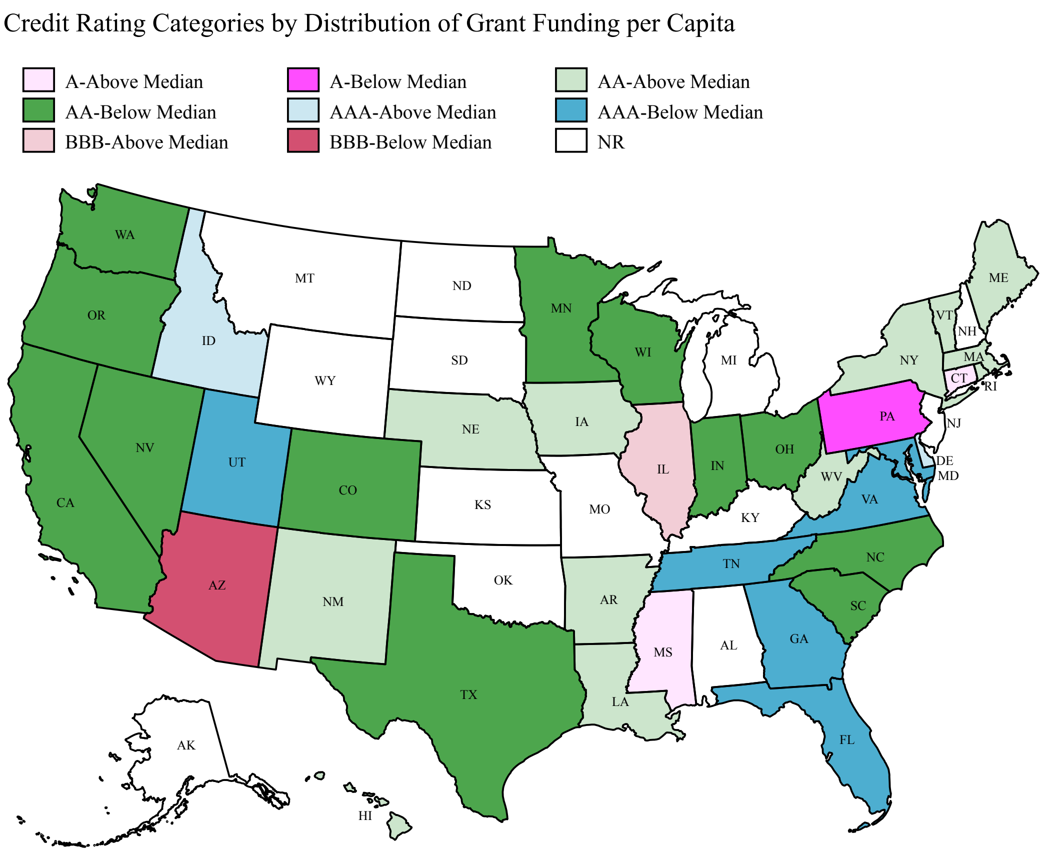
Note: Authors elaboration using FFIS data retrieved from the replication package of Green and Loualiche (2021). The panel on the left shows the estimates for federal grant funding given to states, expressed as a percentage of total fiscal revenues observed during 2020, plotted against each state's population level. The negative relationship shows that larger states observed lower COVID-19 funding, in terms of their fiscal revenues. Data of total revenues comes from the Annual Survey of Local Government Finances, retrieved from Pierson et al. (2015). The panel on the right shows on the vertical axis the economic shock observed by state governments (i.e. YoY economic growth rate for 2020) and plot it against population levels to distinguish states across the distribution of per-capita COVID-19 funding.

Figure A.11: COVID-19 Federal Funding for States



Note: Authors elaboration using FFIS data retrieved from the replication package of Green and Loualiche (2021). The first map shows the distribution of states according to the type of CRF allocation they received. Fixed allocation refers to the minimum $1.25 billion payment. The second map shows the categorization of states according to their position on the distribution of per-capita federal grant funding.

Figure A.12: Credit Rating Groups and COVID-19 Grant Funding



Note: Authors elaboration using FFIS data retrieved from the replication package of Green and Loualiche (2021). This map shows the distribution of states across the credit rating categories described at Figure A.3 and the distribution of per-capita grant funding showed at A.11.

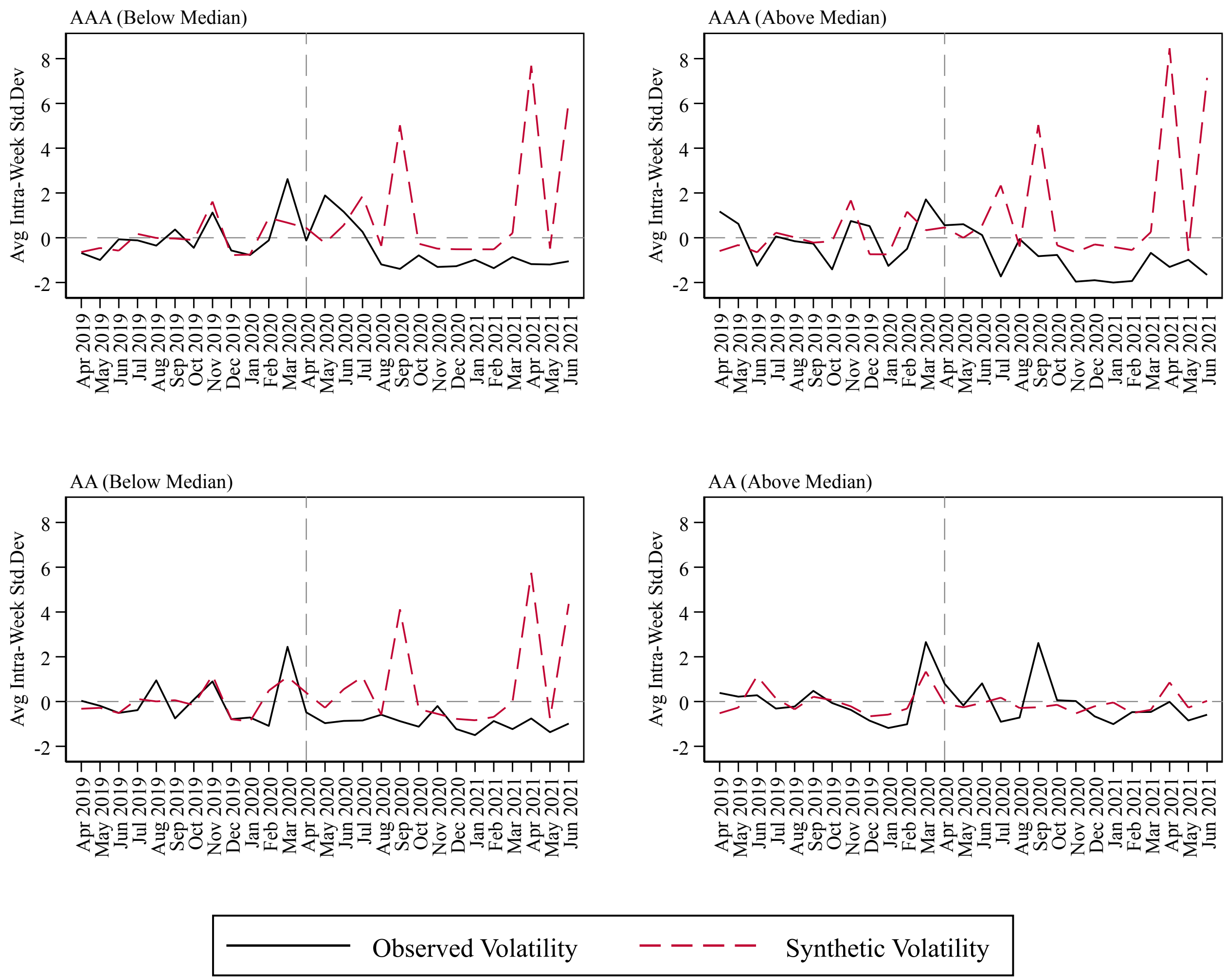
Table A.4: Average Treatment Effects: Heterogeneity by CRF Allocation

	AAA (Below Median)	AAA (Above Median)	AA (Below Median)	AA (Above Median)
Average Treatment Effect (a)	-0.1858***	-0.2428***	-0.1697***	0.0006
	(0.0499)	(0.0518)	(0.0514)	(0.0447)
Historic Volatility (b)	0.1995	0.3183	0.1676	0.1518
Volatility March 2020 (c)	0.4783	0.5074	0.4355	0.4048
Excess Volatility (d = c - b)	0.2788	0.1891	0.2678	0.2530
ATE, % Excess Volatility (e = a/d)	66.6290%	128.3999%	63.3718%	0.2569%
P-Value (Left Tail)	0.0000	0.0000	0.0000	0.5830
RMSPE	0.0015	0.0076	0.0042	0.0016

	A (Below Median)	A (Above Median)	BBB (Below Median)	BBB (Above Median)
Average Treatment Effect (a)	0.0462	-0.1921***	0.4337	-0.0578
	(0.0623)	(0.0539)	(0.0855)	(0.0806)
Historic Volatility (b)	0.2224	0.2393	0.4569	0.3683
Volatility March 2020 (c)	0.6065	0.4761	0.5395	0.5565
Excess Volatility (d = c - b)	0.3841	0.2369	0.0826	0.1882
ATE, % Excess Volatility (e = a/d)	12.0388%	81.0983%	NA	30.7029%
P-Value (Left Tail)	0.9260	0.0000	0.9940	0.0910
RMSPE	0.0005	0.0027	0.0125	0.0115

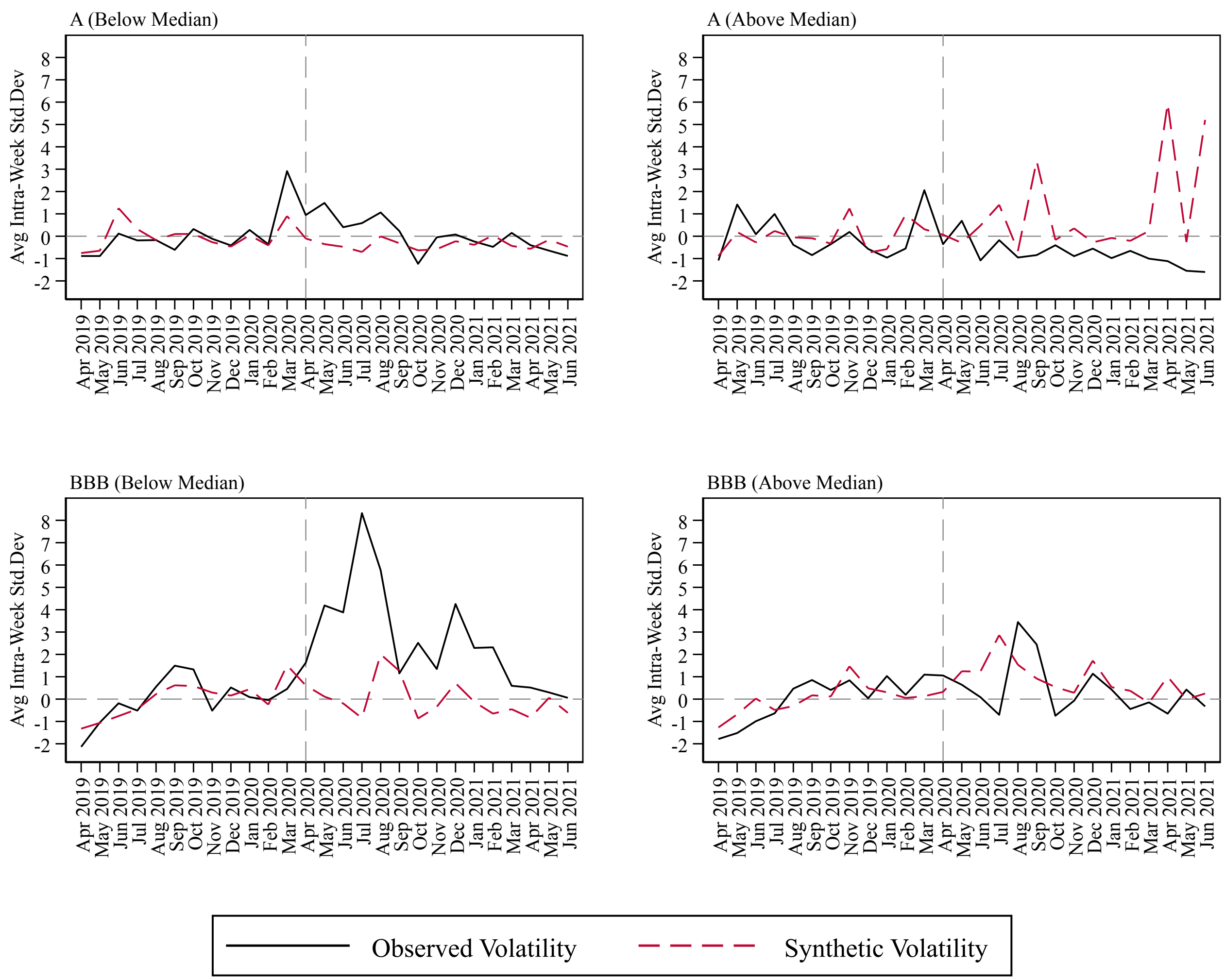
Note: Each column shows the results of the synthetic control estimator for each credit rating category. Average Treatment Effect (ATE) is computed using the 15 months following the intervention. Corresponds to the average of the treatment effect calculated on volatility series expressed in percentage points, that is reverting the normalization process described in Section 4. Hence, ATE is expressed in percentage points. Standard errors are reported in parentheses. These correspond to the standard deviation of the placebo distribution of each coefficient. Statistical significance is determined using a left-tail rank-based p-value (i.e. count the number of times the ATE of the treated unit is smaller (to the left) than the ATE for each unit on the placebo distribution. A */**/*** indicates significance at the 10%, 5%, and 1% levels, respectively. Historic volatility (b) shows the average volatility observed during the pre-intervention period, excluding March 2020. Volatility March 2020 (c) is reported below the Historic Volatility (b). We define Excess Volatility as the simple difference between the volatility recorded in March 2020 and the historic volatility (d = c-b). ATE, % Excess Volatility expresses the ATE as a percentage of the excess volatility (e = a/d). NA is reported when the result of this computation exceeds 300% in absolute value due to the small value of the Excess Volatility. RMSPE corresponds to the root mean squared prediction error of the synthetic control estimator.

Figure A.13: Robustness Check (Heterogeneity by CRF Allocation): Observed vs Synthetic Volatility (1)



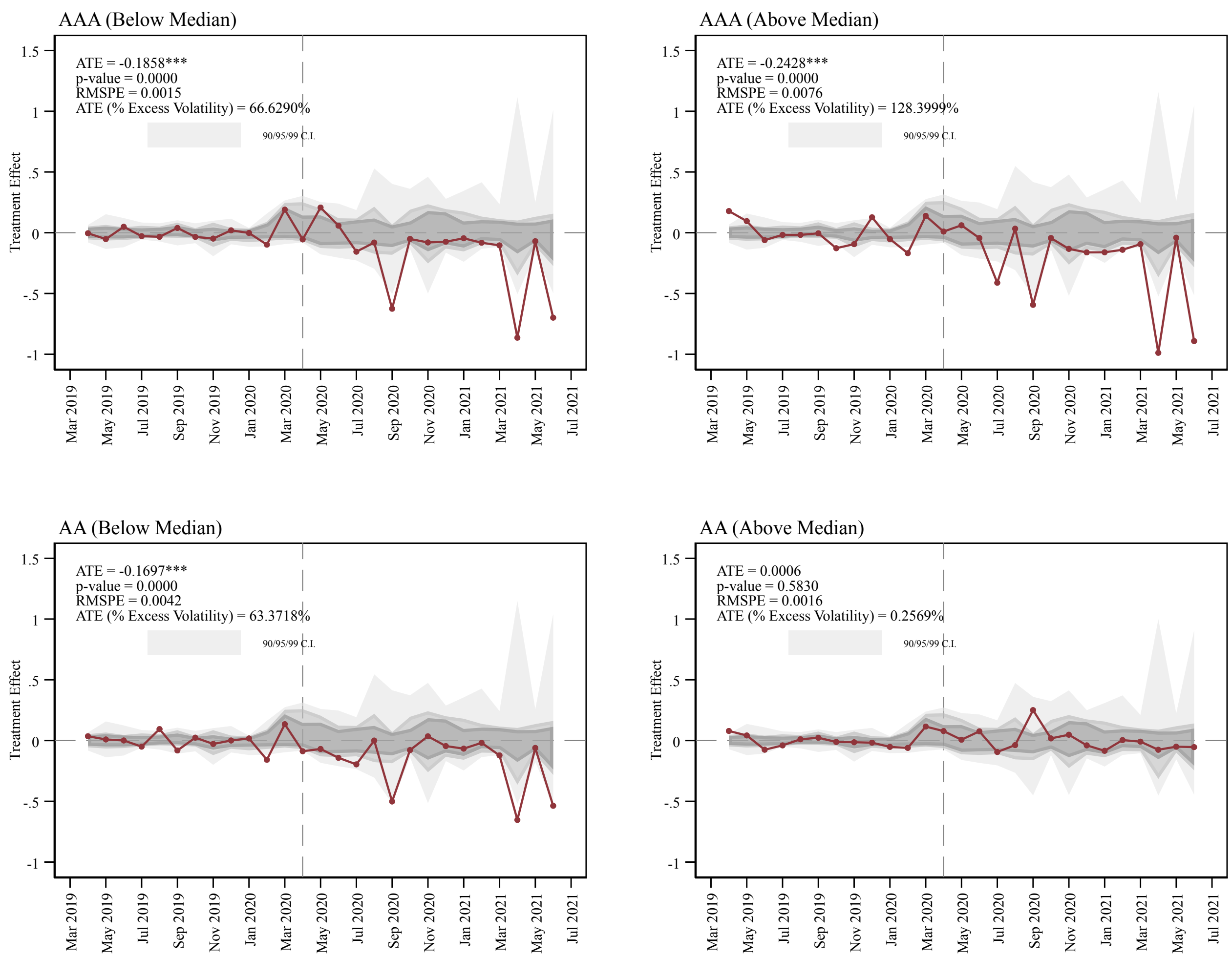
Note: Each panel shows both the time series for the observed and the synthetic series of the average weekly volatility in the secondary market for each credit rating category.

Figure A.14: Robustness Check (Heterogeneity by CRF Allocation): Observed vs Synthetic Volatility (2)



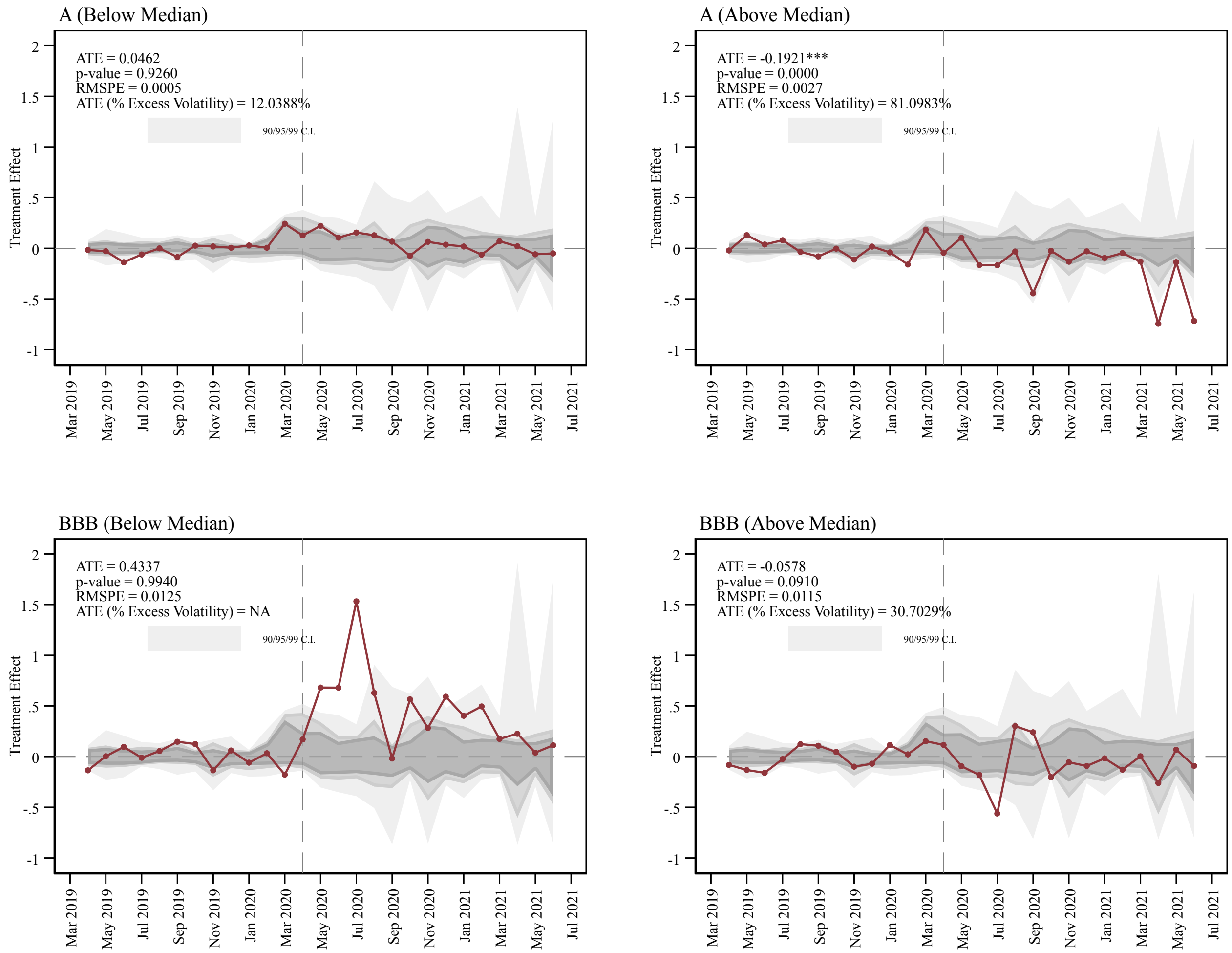
Note: Each panel shows both the time series for the observed and the synthetic series of the average weekly volatility in the secondary market for each credit rating category.

Figure A.15: Robustness Check (Fixed Credit Risk Profile): Treatment Effect Estimates (1)



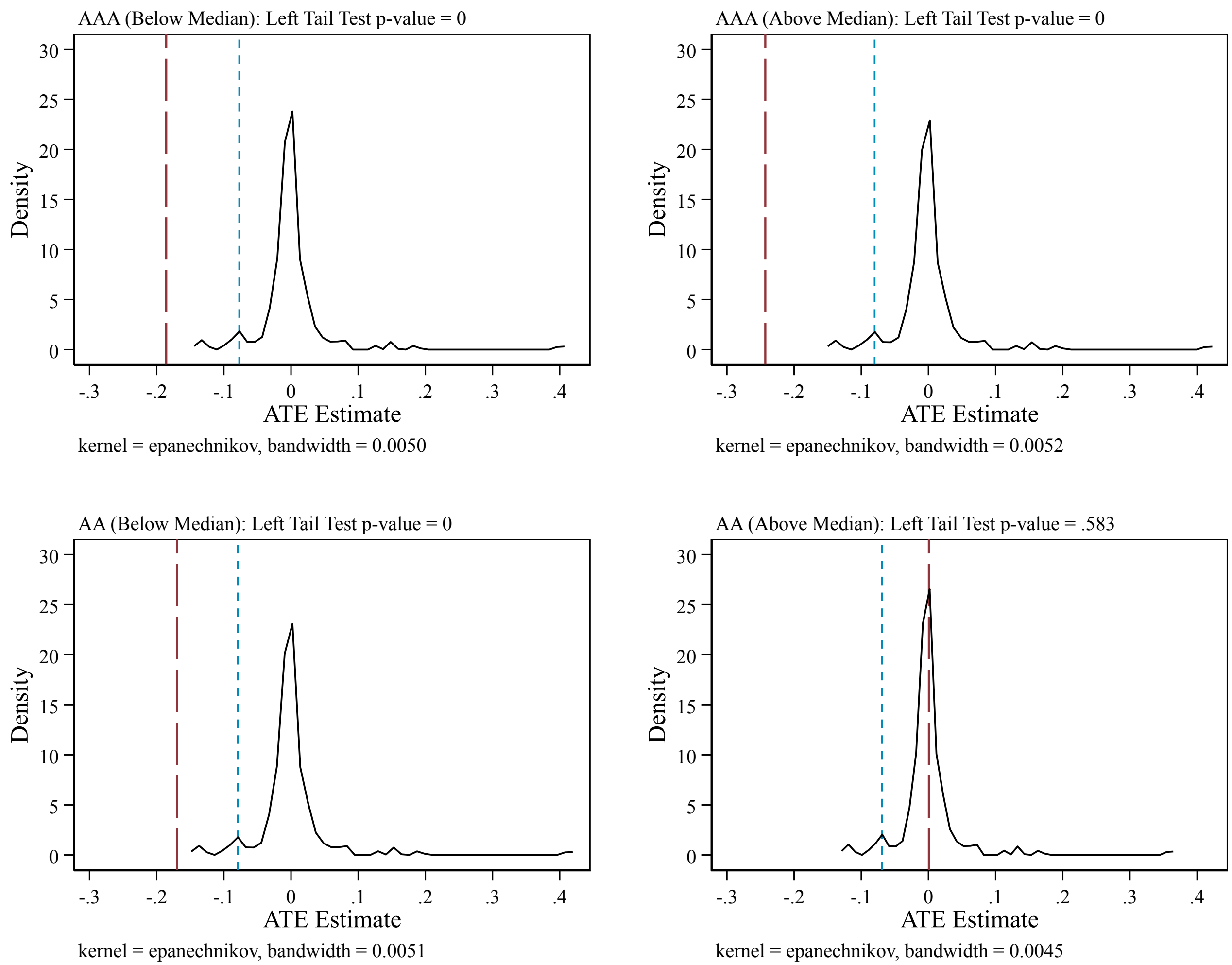
Note: Each panel shows treatment effect estimates $\hat{\tau}_t$ for each credit rating category (solid red line). Treatment effect estimates from normalized series of the monthly average intra-week volatility in the secondary market for each credit rating category. Normalization using the procedure described in Section 4.The shaded area surrounding the line depicts the distribution of the placebo distribution for each period on the analysis window. Shaded areas correspond to the percentiles of the placebo distribution for each period. Percentiles at 1%,5%, and 10% are displayed in gray scales. Following Table 2, reported ATE is expressed in basis points (i.e. reverting the normalization described above).

Figure A.16: Robustness Check (Fixed Credit Risk Profile): Treatment Effect Estimates (2)



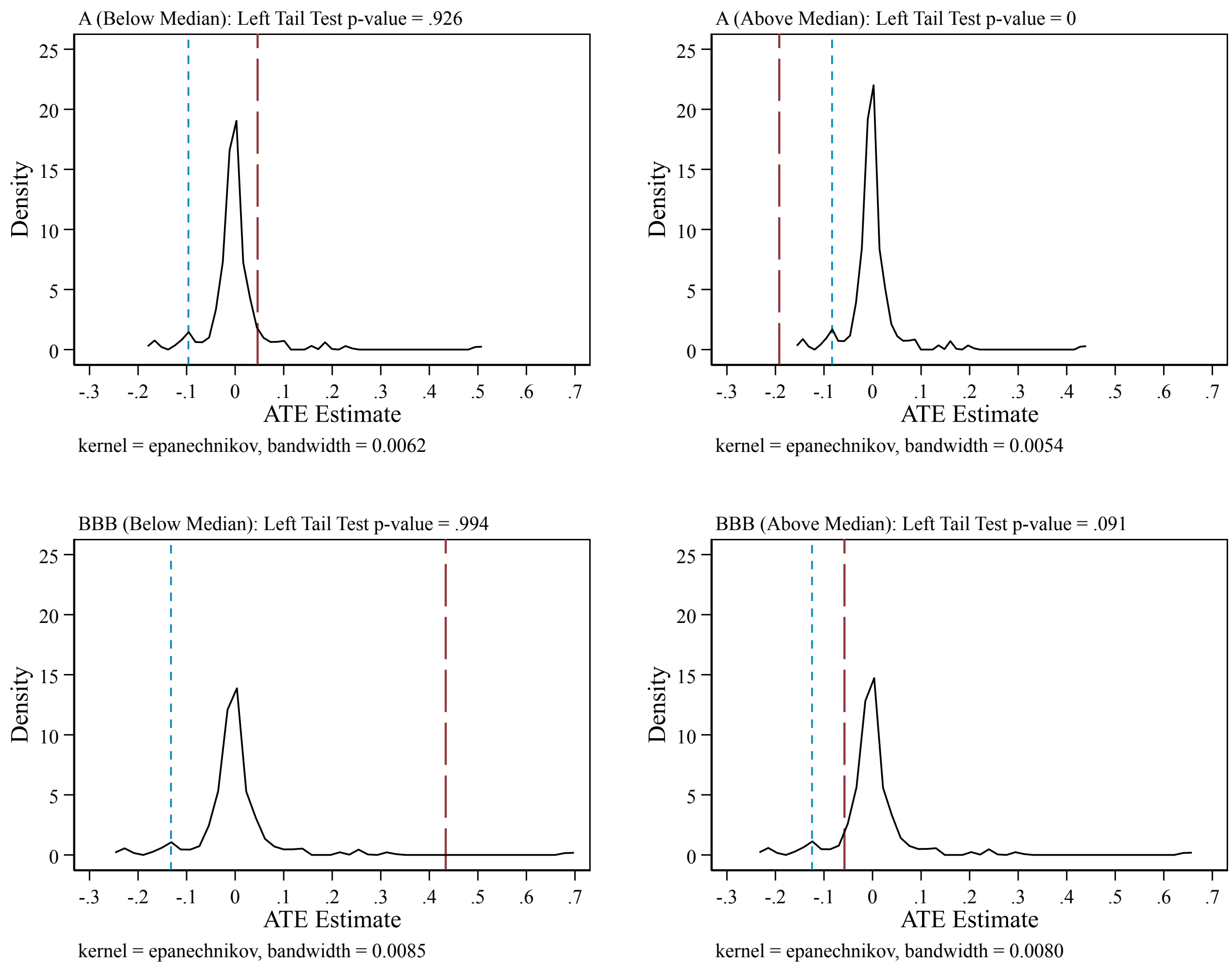
Note: Each panel shows treatment effect estimates $\hat{\tau}_t$ for each credit rating category (solid red line). Treatment effect estimates from normalized series of the monthly average intra-week volatility in the secondary market for each credit rating category. Normalization using the procedure described in Section 4.The shaded area surrounding the line depicts the distribution of the placebo distribution for each period on the analysis window. Shaded areas correspond to the percentiles of the placebo distribution for each period. Percentiles at 1%,5%, and 10% are displayed in gray scales. Following Table 2, reported ATE is expressed in basis points (i.e. reverting the normalization described above).

Figure A.17: Robustness Check (Heterogeneity by CRF Allocation):Placebo Distribution of ATE Estimates by Credit Rating Group (1)



Note: This graph provides a visual representation of the computation of the left-sided rank-based p-values for each Average Treatment Effect estimate. The black line shows the kernel density of the placebo distribution, where the placebos are adjusted to the mean and standard deviation of each treated unit, during the pre-intervention period. The red dashed vertical line shows the point estimate of the Average Treatment Effect. The blue dashed line shows the 5% percentile of the placebo distribution, thus showing the threshold for rejection of the null hypothesis for the left-sided one-tail test. Statistical significance at the 5% level is reached so long the Average Treatment Effect is to the left of this threshold.

Figure A.18: Robustness Check (Heterogeneity by CRF Allocation):Placebo Distribution of ATE Estimates by Credit Rating Group (2)



Note: This graph provides a visual representation of the computation of the left-sided rank-based p-values for each Average Treatment Effect estimate. The black line shows the kernel density of the placebo distribution, where the placebos are adjusted to the mean and standard deviation of each treated unit, during the pre-intervention period. The red dashed vertical line shows the point estimate of the Average Treatment Effect. The blue dashed line shows the 5% percentile of the placebo distribution, thus showing the threshold for rejection of the null hypothesis for the left-sided one-tail test. Statistical significance at the 5% level is reached so long the Average Treatment Effect is to the left of this threshold.

Table A.5: List of Financial Instruments used for the Donor Pool

Bloomberg Ticker/State	Description
CTATS10Y GOVT	Sovereign Bond: Austria (10Y)
CTATS3Y GOVT	Sovereign Bond: Austria (3Y)
CTBEF3Y GOVT	Sovereign Bond: Belgium (3Y)
CTBEF9Y GOVT	Sovereign Bond: Belgium (9Y)
CTBRL3Y GOVT	Sovereign Bond: Brazil (3Y)
CTBRL5Y GOVT	Sovereign Bond: Brazil (5Y)
CTBRLII30T GOVT	Sovereign Bond: Brazil (30Y)
CTCAD10Y Govt	Sovereign Bond: Canada (10Y)
CTCHF10Y Govt	Sovereign Bond: Switzerland (10Y)
CTCLP10Y Govt	Sovereign Bond: Chile (10Y)
CTCNY10Y Govt	Sovereign Bond: China (10Y)
CTCNY3Y GOVT	Sovereign Bond: China (3Y)
CTCOP10Y Govt	Sovereign Bond: Colombia (10Y)
CTCZK3Y GOVT	Sovereign Bond: Czech (3Y)
CTDEM9Y GOVT	Sovereign Bond: Germany (9Y)
CTDEMII5Y	Sovereign Bond: Germany (5Y)
CTDKK3Y GOVT	Sovereign Bond: Denmark (3Y)
CTDKK5Y GOVT	Sovereign Bond: Denmark (5Y)
CTDOP10Y Govt	Sovereign Bond: Dominican Republic (10Y)
CTESP9Y GOVT	Sovereign Bond: Spain (9Y)
CTEURHR9Y GOVT	Sovereign Bond: Croatia (9Y)
CTEURLT2Y GOVT	Sovereign Bond: Lithuania (2Y)
CTEURRO6Y GOVT	Sovereign Bond: Romania (6Y)
CTFIM10Y Govt	Sovereign Bond: Finland (10Y)
CTFIM10Y GOVT	Sovereign Bond: Finland (10Y)
CTFRF9Y Govt	Sovereign Bond: France (9Y)
CTGBP10Y GOVT	Sovereign Bond: England (10Y)
CTGBPII5Y GOVT	Sovereign Bond: England (5Y)
CTGRD20Y GOVT	Sovereign Bond: Greece (20Y)
CTGTQ20Y GOVT	Sovereign Bond: Guatemala (20Y)
CTHKD5Y GOVT	Sovereign Bond: Hong Kong (5Y)
CTIEP3Y GOVT	Sovereign Bond: Ireland (3Y)
CTINR2Y GOVT	Sovereign Bond: India (2Y)
CTINR9Y GOVT	Sovereign Bond: India (9Y)
CTITL10Y Govt	Sovereign Bond: Italy (10Y)
CTJMD10Y Govt	Sovereign Bond: Jamaica (10Y)

Continued on next page

Table A.5: List of Financial Instruments used for the Donor Pool

Bloomberg Ticker/State	Description
CTJPYII5Y GOVT	Sovereign Bond: Japan (5Y)
CTKRW10Y Govt	Sovereign Bond: South Korea (10Y)
CTLVL2Y GOVT	Sovereign Bond: Latvia (2Y)
CTMRY5Y GOVT	Sovereign Bond: Malasyia (5Y)
CTMXN10Y Govt	Sovereign Bond: Mexico (10Y)
CTNGN10Y Govt	Sovereign Bond: Nigeria (10Y)
CTNGN2Y GOVT	Sovereign Bond: Nigeria (2Y)
CTNOK3Y GOVT	Sovereign Bond: Norway (3Y)
CTNZD5Y GOVT	Sovereign Bond: New Zealand (5Y)
CTPEN10Y Govt	Sovereign Bond: Peru (10Y)
CTPHP2Y GOVT	Sovereign Bond: Phillipines (2Y)
CTPLN2Y GOVT	Sovereign Bond: Poland (2Y)
CTPTE10Y Govt	Sovereign Bond: Portugal (10Y)
CTRUB10Y Govt	Sovereign Bond: Russia (10Y)
CTRUB3Y GOVT	Sovereign Bond: Russia (3Y)
CTSAR10Y GOVT	Sovereign Bond: Saudi Arabia (10Y)
CTSAR2Y GOVT	Sovereign Bond: Saudi Arabia (2Y)
CTSKK10Y Govt	Sovereign Bond: Slovakia (10Y)
CTSKK5Y GOVT	Sovereign Bond: Slovakia (5Y)
CTUAH3Y GOVT	Sovereign Bond: Ukraine (3Y)
CTYHB2Y GOVT	Sovereign Bond: Thailand (2Y)
CTZAR10Y GOVT	Sovereign Bond: South Africa (10Y)
DL1 Comdty	Commodity Price: Ethanol
FC1 Comdty	Commodity Price: Feeder Cattle
GC1 Comdty	Commodity Price: Gold
HG1 Comdty	Commodity Price: Copper
HO1 Comdty	Commodity Price: Heating Oil
INDU Index	Commodity Price: Indu
JO1 Comdty	Commodity Price: Orange Juice
JX1 Comdty	Commodity Price: Kerosene
KC1 Comdty	Commodity Price: Coffee
LB1 Comdty	Commodity Price: Lumber
LC1 Comdty	Commodity Price: Live Cattle
LH1 Comdty	Commodity Price: Lean Hogs
NG1 Comdty	Commodity Price: Natural Gas
O 1 Comdty	Commodity Price: Oats

Continued on next page

Table A.5: List of Financial Instruments used for the Donor Pool

Bloomberg Ticker/State	Description
OR1 Comdty	Commodity Price: Rubber
QS1 Comdty	Commodity Price: Gasoil
RR1 Comdty	Commodity Price: Rough Rice
RS1 Comdty	Commodity Price: Canola
S1 Comdty	Commodity Price: Soybean
SBA Comdty	Commodity Price: Sba
SI1 Comdty	Commodity Price: Silver
SM1 Comdty	Commodity Price: Soybean Meal
WA Comdty	Commodity Price: Wa
XB1 Comdty	Commodity Price: Gasoline
XPTUSD Currncy	Commodity Price: Platinum
BO1 Comdty	Commodity Price: Soybean Oil
C1 Comdty	Commodity Price: Corn
CC1 Comdty	Commodity Price: Cocoa
CLA Comdty	Commodity Price: Wti
CLA Comdty	Commodity Price: Crude Oil
COA Comdty	Commodity Price: Brent
COAL IN Equity	Commodity Price: Coal
FTSEMIB Index	Stock Market Index: Italy
MEXBOL Index	Stock Market Index: Mexico
NKY Index	Stock Market Index: Japan
SPX Index	Stock Market Index: United States
DAX Index	Stock Market Index: Germany
CAC Index	Stock Market Index: France
AEX Index	Stock Market Index: Netherlands
IBEX Index	Stock Market Index: Spain
OMX Index	Stock Market Index: Sweden
SMI Index	Stock Market Index: Switzerland
BSX Index	Stock Market Index: Bermuda
IGPA Index	Stock Market Index: Chile
BUX Index	Stock Market Index: Hungary
RTSI Index	Stock Market Index: Russia
SAX Index	Stock Market Index: Slovakia
EGX30 Index	Stock Market Index: Egypt
KSE100 Index	Stock Market Index: Pakistan
NSE200 Index	Stock Market Index: Kenya

Continued on next page

Table A.5: List of Financial Instruments used for the Donor Pool

Bloomberg Ticker/State	Description
HIS Index	Stock Market Index: Hong Kong
KOSPI Index	Stock Market Index: South Korea
SENSEX Index	Stock Market Index: India
SHSZ300 Index	Stock Market Index: Shanghai
AS51 Index	Stock Market Index: Australia
ATX Index	Stock Market Index: Austria
ASE Index	Stock Market Index: Greece
BEL20 Index	Stock Market Index: Belgium
OMXC25 Index	Stock Market Index: Denmark
HEX Index	Stock Market Index: Finland
ICEXI Index	Stock Market Index: Iceland
CROX Index	Stock Market Index: Croatia
CTXEUR Index	Stock Market Index: Czesch
ROTXL Index	Stock Market Index: Romania
UTXEUR Index	Stock Market Index: Ukraine
TPX Index	Stock Market Index: Japan
Alabama	S&P Municipal Bond Index: Alabama General Obligation Index
Colorado	S&P Municipal Bond Index: Colorado General Obligation Index
California	S&P Municipal Bond Index: California General Obligation Index
Georgia	S&P Municipal Bond Index: Georgia General Obligation Index
Connecticut	S&P Municipal Bond Index: Connecticut General Obligation Index
Florida	S&P Municipal Bond Index: Florida General Obligation Index
Illinois	S&P Municipal Bond Index: Illinois General Obligation Index
Delaware	S&P Municipal Bond Index: Delaware General Obligation Index
Maine	S&P Municipal Bond Index: Maine General Obligation Index
Louisiana	S&P Municipal Bond Index: Louisiana General Obligation Index
Maryland	S&P Municipal Bond Index: Maryland General Obligation Index
Indiana	S&P Municipal Bond Index: Indiana General Obligation Index
Massachusetts	S&P Municipal Bond Index: Massachusetts General Obligation Index
Michigan	S&P Municipal Bond Index: Michigan General Obligation Index
Minnesota	S&P Municipal Bond Index: Minnesota General Obligation Index
Missouri	S&P Municipal Bond Index: Missouri General Obligation Index
Nebraska	S&P Municipal Bond Index: Nebraska General Obligation Index
Nevada	S&P Municipal Bond Index: Nevada General Obligation Index
New Jersey	S&P Municipal Bond Index: New Jersey General Obligation Index
New Mexico	S&P Municipal Bond Index: New Mexico General Obligation Index

Continued on next page

Table A.5: List of Financial Instruments used for the Donor Pool

Bloomberg Ticker/State	Description
Ohio	S&P Municipal Bond Index: Ohio General Obligation Index
Oklahoma	S&P Municipal Bond Index: Oklahoma General Obligation Index
Oregon	S&P Municipal Bond Index: Oregon General Obligation Index
Pennsylvania	S&P Municipal Bond Index: Pennsylvania General Obligation Index
Rhode Island	S&P Municipal Bond Index: Rhode Island General Obligation Index
South Carolina	S&P Municipal Bond Index: South Carolina General Obligation Index
Tennessee	S&P Municipal Bond Index: Tennessee General Obligation Index
Texas	S&P Municipal Bond Index: Texas General Obligation Index
Utah	S&P Municipal Bond Index: Utah General Obligation Index
Virginia	S&P Municipal Bond Index: Virginia General Obligation Index
Washington	S&P Municipal Bond Index: Washington General Obligation Index
Wisconsin	S&P Municipal Bond Index: Wisconsin General Obligation Index

Note: S&P Municipal Bond Indices correspond to the General Obligation index of each state, where the pricing variable considered is yield to worst.