Federal Assistance and Municipal Borrowing: Unpacking the effects of the CARES Act on Government Liquidity Management

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Abstract

Access to cash can affect the ability of local governments to respond to crises. Federal aid to local governments can supply this directly, though the effectiveness on a dollar-per-dollar basis depends on its complementary or substitutability with local borrowing. Through this lens this examines the effects of the Coronavirus Relief Fund (CRF) on the local governments borrowing using a regression discontinuity design that exploits the quasi-experimental setting induced by the fund eligibility criterion imposed by the US Treasury. The findings indicate that recipient governments observed mild reductions in borrowing costs and increased their debt issuance on the primary market, with no significant spillovers to the secondary market. Moreover, this analysis provides some suggestive evidence on the liquidity management undertaken by local governments. It documents an increase in the issuance of short-term debt, at the expense of reductions on the issuance of longer-term bonds. Together, these findings shed some light on the mechanisms through which federal aid to local governments translates into improved borrowing conditions on the bond market.

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1 Introduction

The purpose of this paper is to study the effect of federal aid on local government borrowing during a macroeconomic crises. Historically, the US federal government has repeatedly provided aid to local governments during these crises. During the Great Recession, this aid was found in the Build America Bonds program as part of the American Recovery and Reinvestment Act of 2009. In the more recent COVID-19 pandemic, both the Coronavirus, Aid, Relief and Economic Security Act of 2020 (CARES Act) and the American Rescue Plan (ARP) of 2021 provided assistance to local governments for coping with the pandemic. A key purpose is that these federal aid programs can help local governments maintain or enact federally desirable policies, but this is not a guaranteed consequence of aid on a dollar-for-dollar basis. For instance, federal support to distressed governments could translate into positive outcomes on the municipal bond market if such support restores investor confidence in local government finances (Andrew Ang et al., 2010; Luby, 2012). On the other hand, if the federal deficit-supported aid displaces local debt, then the policy merely substitutes local for federal borrowing. Lastly, if the aid provision reveals information on the magnitude of the shock not previously incorporated by investors, then the aid could punish local governments accessing the debt market. To the extent that federal resources are limited, the effectiveness of the policies in terms of crowding out or crowding in local government responses is a policy-relevant concern to setting federal budget priorities.

Analyzing the developments on the municipal bond market during the first stages of the COVID-19 pandemic provides an ideal setting to examine this question as the lockdowns translated into liquidity shocks for local governments due to contraction of fiscal revenues and the unexpected hike in public health spending and, at the same time, the federal response to the crisis created a quasi-experimental setting in which some local governments obtained direct support from the federal government to cope with the negative effects of the crisis. At the onset of the COVID-19 pandemic, the US government enacted CARES Act which expanded federal spending by \$2.3 trillion, 11% of U.S Gross Domestic Product and almost 70% of federal revenues (Gordon, 2012). This policy included the creation of the Coronavirus Relief Fund (CRF) which distributed \$150 billion in assistance state and local governments covering COVID-related expenses, alleviating near-term fiscal pressures. In a nutshell, the US Treasury relied on a population criterion to allocate the funding from the CRF across state and local governments where no state received less than \$1.25

billion and, more importantly for the identification of this paper, all county and city governments with population above 500,000 received a direct payment from the Treasury, proportional to their population level and that was subtracted from the state's allocation.

The analysis presented on this paper exploits this feature of CRF to analyze the municipal debt outcomes both at the primary and secondary market outcomes and, by doing so, shedding some light on the extent to which federal support during crises episodes alleviates the distress experienced by local governments. To ensure the internal validity of the analysis, the comparisons carried out across the whole paper look at restricted set of county governments within a close distance to the cutoff for treatment assignment. The key assumption behind the validity of this comparison is that, within this bandwidth, assignment to treatment mimics the conditions of a randomized control trial (Lee and Lemieux, 2010).

The descriptive analysis of municipal debt outcomes of the counties analyzed in this paper reveals that CRF recipients observed a deterioration on their credit quality during the post-intervention period (defined in this paper as April 2020 - December 2021), thus underscoring the relevance of this mechanism on the determination of municipal debt outcomes. Findings from this exercise also show that governments that did not received direct aid from the Treasury increased in a larger magnitude their reliance on debt instruments of shorter maturities, thus providing suggestive evidence on the magnitude of the liquidity pressures experienced by local governments.

For the empirical examination, this paper implements a regression discontinuity design (RDD) to study the effects of federal support on the municipal debt market. The main econometric model is estimated using both a parametric estimator (i.e. fixed-effects OLS) and the non-parametric approach developed by Calonico et al. (2014), as well as linear and quadratic polynomial functions (Gelman and Imbens, 2019) on the running variable (i.e. county population). To preview the findings, results from the RDD suggest that CRF recipients observed improved conditions on the municipal bond market during the first stages of the COVID-19 pandemic. Bonds from these governments observed lower spreads at issue during the post-intervention period (which as defined below, encompasses the period between April 2020 and December 2021). Such estimates point towards a reduction between 6 and 9 basis points on primary market spreads (equivalent to 0.12-0.17x standard deviations of this variable during the post-intervention period), where the upper bound on those estimates (47 basis points) is still within the magnitude of a standard deviation. These governments also observed larger amounts of debt issued. Point estimates suggest an increase in per-capita debt issuance between 1.7 and 5.0 dollars (i.e. magnitude equivalent to 0.13-0.39x standard deviations). Taken together, these results show that governments on the treatment group issued larger amounts of debt at lower borrowing costs, which arguably played a relevant role in the way in which these governments coped with the health and economic crisis.

In contrast with the results on the primary market, estimates for secondary market outcomes are mixed and inconclusive. However, they do provide suggestive evidence that aligns with the findings on the primary market: bonds from governments on the treatment group observed lower spreads at trade and higher trading volume relative to their counterparts on the control group. Both the results for the primary and secondary market are robust to decisions around the bandwidth selection, as well as to the exclusion of county agencies and authorities from the pool of analyzed governments.

To examine the mechanisms through which this policy influenced outcomes on the bond market, the baseline model is extended to analyze treatment effect heterogeneity driven by the structure of the municipal yield curve (i.e. distribution of bonds by years to maturity) and county governments creditworthiness. Results from this exercise underline the relevance of the credit rating mechanism as they suggest significant reductions on the bond spreads of higher rated governments, relative to BBB bonds. These results show that, at the margin, lower rated governments (AA and A) observed larger spread reductions on the primary market. Similarly, this exercise provides some suggestive evidence on some substitution on debt issuance along the yield curve. CRF recipients increased per-capita debt issuance of instruments with shorter maturities while at the same time observed reductions on the issuance of longer-term instruments.[1] However, these estimates are not significant at traditional levels, hence interpretation should be done with caution. Results for the secondary market, on other hand, show some evidence on fly-to-safety behavior among investors as the estimates from this model imply a reduction on the trading volumes of debt

[1] As it will be described on the results at Figure 4 and Table 3, the inflection point on the change of the composition of bond issuance across maturities is found around the maturities above 10 years. Hence, for the rest of the paper I refer to shorter-term bonds (or bonds with shorter maturities) as bonds with time to maturity in the categories 0-2 years, 3-5 years, and 5-10 years. Similarly, I refer to longer-term bonds to bonds with time to maturity in the categories 10-15 years, 11-15 years, and +20 years.

with shorter maturities while, at the same time, there was an increase on the trading of longer-term bonds. In particular, estimates for bonds of maturity above 20 years imply that trading of these bonds was 14 cents per capita larger (i.e within 0.20x standard deviations of this variable, significant at the 5% level) for CRF recipients.

As a final robustness check, inspired by the intent-to-treat estimator proposed by Cellini et al. (2010) I extend the parametric estimator of the baseline RD model to analyze the dynamic heterogeneity of the treatment effect. This flexible estimator not only provides estimates on lagged policy (and anticipation) effects. Results from these estimates do not reveal significant heterogeneity and provide some suggestive evidence on mild policy spillovers driven by the implementation of the ARP, as this model estimates small to null reductions in primary spreads around the implementation time of this policy.

The remainder of the paper is organized as follows. Section 2 describe the theoretical underpinnings of the research question by positioning this paper with the outstanding literature, as well as describing the policy analyzed in this study. Section 3 describes the main data sources while Section 4 presents the results from the descriptive analysis. Section 5 details the implementation of the regression discontinuity and section 6 goes through the empirical results on RDD. Section 7 examines the robustness of the estimates across econometric specifications, heterogeneity driven by credit rating, maturity structure, and potential dynamic policy effects. Concluding remarks are presented at Section 8.

2 Literature Review

Exogenous shocks often translate into fiscal distress for state and local governments. The severity of such distress depends on the impact of the shock on revenue steams, and new spending requirements. Finance theory posits that issuers with stronger revenue-generation fundamentals face lower borrowing costs when accessing debt markets. Yet, turmoil in financial markets heightens the uncertainty around issuer's capability of repayment. Literature from both corporate and municipal finance has documented how issuers' response to economic shocks are moderated by their financial stance. Kahle and Stulz (2013) show that firms with weaker fundamentals experienced more pronounced shocks during the past financial crisis. Poterba and Rueben (2001) find a positive correlation between unexpected deficit shocks and high bond yields at a state level. Kriz and Wang (2016) analyzed municipal investor's risk preferences over the past financial crisis finding a substantial increase in short-term risk premium during the crisis, supporting theory build by Kriz (2004) suggesting that heightened risk-aversion derived from crisis events could led to an increase in municipal borrowing costs. The common theoretical underpinning linking this literature is the intuition from the market discipline hypothesis: over-indebtedness is constrained by market forces through higher interest rates (Bayoumi et al., 1995; Goldstein and Woglom, 1991).

Exogenous liquidity shocks may have several effects on government's incentives to issue debt. Governments facing revenue shortfalls or heightened spending needs could face such through deficit spending via the bond market. At the same time, uncertainty around the economic recovery could lead governments to postpone capital projects, thus delaying bond issues.

The Great Recession shed some light on municipal government's debt management strategies during a crisis episode. During 2009, municipal debt issuance reached its lowest level since 2002 (Martell and Kravchuk, 2012), where this contraction was sharper for some large city governments (The Pew Charitable Trusts, 2013). The decline would have been even greater if not for local government's use of Build America Bonds (Martell and Kravchuk, 2012) and the increase of refinancing operations that exploited the benefits from the low interest rate environment (The Pew Charitable Trusts, 2013).

Arguably, state and local governments response to crisis episodes is moderated by the actions undertaken by the federal government to cope with the economic shock. Fiscal decentralization literature argues macroeconomic stabilization is a role reserved for the federal government due to the constraints that state and local governments have to influence employment and prices (Oates, 2005). An implication of this idea is that the central government leads the response to unexpected economic shocks.

The Great Recession provided supportive evidence for federal funds channeled to the municipal debt market through the Build America Bonds program, authorized as part of the American Recovery and Reinvestment Act of 2009 (ARRA), as it lowered local government's borrowing costs (Andrew Ang et al., 2010; Liu and Denison, 2014), captured a relevant share of the market in 2009 (16%) and 2010 (27%), and were adopted by almost every state government (Luby, 2012). Furthermore, the structure of the program had a significant role in providing liquidity to distressed issuers. The direct subsidy given by the federal government increased investor's appetite for municipal instruments.

Federal policy also signals to investors on the role and commitment of the federal government in reducing the uncertainty on the municipal bond market. For instance, while the announcement of federal support might alleviate the turmoil observed in financial markets, the allocation and rules for allocating federal funds may differentiate the effects across subnational governments. Moreover, it is not clear how investors will react to federal policies. On one hand, additional funding to cope with the crisis might alleviate liquidity concerns, hence relieving pressures on municipal yields. On the other hand, the distribution federal support could signal investors which governments are more prone to experience larger economic dislocations.

The growing literature on the economic effects of the COVID-19 pandemic documents from different angles how state and local governments experienced and coped with the crisis. Massive contraction in economic activity derived from the lockdown was likley to have a significant negative effect on fiscal revenues. Gordon (2012) estimated that state personal income and sales tax revenues (i.e. the two more relevant sources of revenue of state governments) fell faster and more dramatically than the dropped in the past financial crisis.

Lockdowns lead to unusual sharp contractions in consumption relative to income in part due to direct fiscal stimulus to business and households. Consumption declines on health care, restaurants, entertainment, and lodgings were expected to reduce sales tax revenues in regions particularly reliant on those industries (Clemens and Veuger, 2020). Property tax revenues, in contrast, are likely to remain stable in the short-run due to the lags for property reassessments (Lutz et al., 2011). Early predictions by Chernick et al. (2020) anticipated contractions in city revenues (from all sources) between 5.5% and 9% , relative to counterfactual revenues had there not been a recession.

State and local governments actively turned to financial markets (Gillers, 2021) despite the generalized turmoil experienced at the onset of the pandemic (Baker et al., 2020; Haddad et al., 2021). This, could be potentially explained by the uncertainty surrounding the magnitude and severity of the shock for state and local governments. However, the large efforts undertaken by the federal government arguably prevented state and local governments from experiencing severe revenue shortfalls.

The literature analyzing the economic effects of COVID-19 policies, which this paper contributes to, has placed significant attention on the extent to which the Municipal Liquidity Facility (MLF), implemented by the Federal Reserve, eased distress among municipal issuers.

Some scholars document positive effects of the MLF by keeping municipal yields at tolerable levels (Bordo and Duca, 2021; Fritsch et al., 2021) and was successful calming the municipal bond market (Li and Lu, 2020). Empirical evidence by Bi and Marsh (2020) suggests the announcement of federal intervention via fiscal policy (CARES Act) and direct monetary actions by the Federal Reserve (MLF) helped stabilizing the municipal market. Authors argue the announcement of federal actions eased liquidity risks concerns among investors, thus lowering spreads on municipal bonds.

In particular, this paper adds to studies like Johnson et al. (2021) and Haughwout et al. (2022a) that used causal inference designs to examine the effect of the MLF on municipal debt outcomes, by exploiting the variation from the assignment rule used by the Federal Reserve to determine MLF eligibility. Johnson et al. (2021) estimated a difference-in-differences model comparing the borrowing costs of municipal governments, conditional on MLF eligibility. Authors find no significant effects of the MLF in borrowing costs in the primary market. However, since their study assessed the immediate market's reaction to the policy it overlooks heterogeneity driven by the type of eligible government (state, county, city), as well longer term effects. Haughwout et al. (2022a) uses a regression discontinuity design to examine the option value of municipal liquidity on primary market issuance, secondary market yields, and public sector employment. They do not find significant differences on the yields of secondary market transactions, except for low-rated issuers who experienced an average decrease of 75 basis points on the nominal yield. However, authors estimate an 8% increase on the probability of issuing primary market debt associated with MLF eligibility.

While there is plenty of research on the MLF, few studies had analyzed local governments' reaction to the CARES Act. Green and Loualiche (2021) stands out by its examination of the impact of CARES Act assistance in state government labor force. Using an instrumental variable approach, authors estimated that assistance through the Coronavirus Relief Fund to state governments prevented more than 400 thousand layoffs in April, and protected approximately one million job-months for state and local governments through August.

This paper attempts to narrow this gap in the literature by presenting evidence on the effects of direct federal assistance through the CARES Act on local government debt outcomes. By doing so, this paper contributes to the public finance literature in three main ways. First, it adds to the literature looking at the relation between fiscal stress and borrowing costs (Benson and Marks, 2007; Johnson and Kriz, 2005; Poterba and Rueben, 1997, 2001) by documenting heterogeneity on local government's debt policy response to the COVID-19 shock. Second, it brings new evidence to the literature looking at federal intervention on the municipal bond market (Fritsch et al., 2021; Haughwout et al., 2022b; Johnson et al., 2021). Third, it provides an assessment on the policy effects of the Coronavirus Relief Fund on local government's debt outcomes and issuing behavior. Finally, this study makes a particular contribution to the COVID-19 studies on the municipal bond market by considering from both 2020 and 2021 to assess the effectiveness of the implemented policies.

2.1 Policy Setting: Coronavirus Relief Fund

On March 13, 2020, the President declared a national emergency declaration for all states, tribes, territories, and the District of Columbia due to severity of the COVID-19 pandemic. In the following weeks, the US Congress designed and voted the CARES Act, a bill that implemented several programs to address issues related with the health emergency. The CARES Act was passed by Congress on March 25, 2020 and a couple day later (i.e. March 27, 2020) was signed into law by the President.

The CARES Act provided \$ 2.2 trillion in assistance to households, small businesses and subnational governments to cover expenses related with the coping of the health crisis. These funds were allocated, among other things, to expand and extend unemployment benefits, boost the stimulus checks program, increase health spending, provide loan guarantees for large businesses and governments, and, more importantly for this paper, to provide direct aid to state and local governments through the CRF which received an allocation of \$150 billion. This figure considered \$139 for state and municipal governments, \$8 billion for tribal governments and \$3 billion for territories. Allocations across state governments was determined by each state's population with the caveat that no state should receive less than \$1.25 billion. This resulted in a distribution where the smallest 21 states received this minimum allocation (Driessen, 2020; Gordon, 2012). This distribution implied a population cutoff for states where all the states with a population smaller than Connecticut's (i.e. 3.5 million people) received a fixed allocation.

The CRF included a mechanism to distribute part of each state's allocation to municipal governments through direct payments made by the Treasury. Eligibility for such payments was determined by a population threshold: counties and cities/towns whose population was more than 500,000 people were eligible to receive funds directly from the Treasury. To determine the distribution of funds across states, and the list of eligible governments, the Treasury used data from the U.S. Census Bureau's Population Estimates Program for 2019.

The Treasury identified 171 county and city governments eligible for direct assistance. When looking at the payments by the Treasury, I identify 154 local governments that received direct payments through the CRF: 118 counties and 36 cities. Transfers to local governments amounted for \$27.6 billion (19.88% of the allocation for state and local governments) where \$20.3 billion were received by county governments and \$7.3 billion by cities.

This paper, however, only focuses on the aid provided to county governments. To provide some context around the payments provided to county governments through this program, these 118 counties represent governments from 32 states, where 45% of them are located in California (15 counties), Florida (12), Texas (12), New Jersey (9), and Pennsylvania (6). In terms of the magnitude, the payments observed by these counties were in average \$159.2 per capita (with a standard deviation of \$63.1 per capita), and ranged between \$32.7 and \$577.6 per capita, where the largest per capita amounts were observed by the counties with lower levels of population.

The CARES Act limited the uses of CRF aid to only cover: i) necessary expenses incurred due to the health emergency, ii) expenses not accounted for on local budgets (as of March 27, 2020), iii) and expenses incurred between March 2020 and December 2022. In other words, the fungibility on these funds rules provided some discretion to governments on how to allocate the aid within their local budgets, which increased their spending capacity by reducing the liquidity pressures created by the fall in local tax revenues. This is one of the key features of the policy used for the analysis carried out in this paper. The fungibility of this aid mimics a cash transfer to distressed county governments, which could provide useful information to investors on the municipal bond market about the financial condition of these governments during the pandemic.

3 Data

The data for this analysis stems from several sources. The bond data from the primary market comes from IPREO where I considered the universe of all bonds issued by county governments (including agencies and authorities) between January 2019 and December 2021.[2] This data set contains yields at issue along with the main bond characteristics. Data from the secondary market was retrieved from the MSRB, accessed through Wharton Research Data Services. For the secondary market analysis, all the transactions observed between January 2019 and December for the active bonds issued since January 2002 were considered. This allows to capture a more comprehensive picture of the conditions experienced on the secondary market.

Dependent Variables: For the analysis of the primary and secondary municipal bond markets, I consider as main dependent variables the spreads at issue (primary market) and trade (secondary market), as well as the par amount issued (primary market) and traded (secondary market). These last two are expressed in dollars per-capita. Bond spreads are calculated as the simple subtraction of the municipal yields with the treasury yields for instruments of equivalent maturity at the date of issue (trade), hence providing a direct measurement (in percentage points) of the market risk premium assigned to the bonds when issued and in any given trade on the secondary market. The rationale for this measure is twofold. On one hand, monetary policy actions undertaken by the Federal Reserve during the analyzed period led to a decrease on the interest rate of the U.S. economy. The federal funds effective rate dropped from 1.58% in February 2020 to 0.05% in April 2020, and stayed under 0.10% for the remainder of 2020 and 2021. This exerted downward pressure on nominal yields during the period preceding the lockdown. By using the spread I am directly controlling for the direct effect that monetary and fiscal policy changes had on municipal borrowing costs. In addition, municipal-Treasury spreads provide a measurement of the credit risk-premium assigned by the market to each county issuer, hence measuring the extent to which investor's concerns on economic risks associated with the pandemic were eased by this policy. For these reasons, it

[2]Adhering to the criterion used at U.S. Annual Census of Local Governments, I consider consolidated county-city governments as city governments, hence I exclude them from the analysis.

is widely common among academics (Cornaggia et al., 2018; Denison, 2001; Poterba and Rueben, 2001) and practitioners to use them as measurement of the credit risk and borrowing costs.

Independent Variables: In accordance with the methodology employed by the Treasury, 2019 county population figures from the US Census are included on the analysis. CRF data was retrieved from the U.S. Treasury website in order to identify the governments that received direct assistance from the Treasury[3]. To account for the magnitude of the shock on the local economy, I incorporate county-month measurements of the unemployment rate from the Bureau of Labor Statistics.

In adherence to common practice in the public finance literature, throughout the analysis I consider the main variables commonly considered as explanatory factors in a bond pricing model. The predictors considered are: credit rating, years to maturity, offering type (i.e. competitive vs negotiated), coupon rate, a binary variable for general obligation bonds, and a binary variable to identify central government issuers from county agencies.[4] From the bond data (for both the primary and secondary market) I exclude the observations with missing information on the dependent variables or any of the main bond characteristics (see variables at Table 5). Moreover, I exclude from the analysis outlier observations on the dependent variables by removing the top and bottom 0.5% of the sample.

[3]Source: https://home.treasury.gov/system/files/136/Payments-to-States-and-Units-of-Local-Government.pdf

[4]Considering the rating assigned by Fitch, Standard and Poor's, and Moody's I first generated a continuous variables that takes values from 1-10, where bonds with higher ratings are assigned lower values. Hence, this variable measures increases in credit risk associated with deterioration on the credit rating. Then, using such variable I computed a credit rating measurement that takes the minimum rating from these three, and then builds a categorical variable that groups bond's credit ratings according to their letter category (i.e. AAA, AA, A, BBB), where the coding of this variable assigns a higher number of the lowest credit rating. For bonds without ratings from one or two agencies, only the observed ratings are considered. This grouping criterion implies that ratings AA-,AA, and AA+ are categorized together at AA. Same applies for A and BBB ratings.

4 Descriptive Analysis

4.1 Treatment and Control Group Definition

Identification of (sharp) regression discontinuity designs hinge on the assumption that, around the cutoff, assignment into treatment is as good as random, hence comparisons between observations within a small bandwidth around the cutoff should mimic a randomized experiment (Lee and Lemieux, 2010). To determine the bandwidth for the baseline analysis I use the methodology proposed by Imbens and Kalyanaraman (2012) and Calonico et al. (2014) to compute optimal bandwidths (common for both sides of the cutoff) for each dependent variable considering the observations of each month included on the post-intervention period. That is, for each month, compute the MSE optimal bandwidth from local-linear regressions on the dependent variables for the primary market observed during that month, and then take the median from such estimates. The result of this exercise leads to a bandwidth of +/- 142 thousand people around the cutoff. [5] This implies the analysis considers all the counties whose population in 2019 was between 358 and 642 thousand people, which lead to 27 counties (44 distinct issuers) on the treatment group and 50 counties (60 distinct issuers) on the control group.

A benefit of the methodology advanced by Imbens and Kalyanaraman (2012) and Calonico et al. (2014) is that allows for an optimal criterion to choose the treatment and control groups based on the characteristics of the sample, which in this case could vary across time. However, it comes at the expense of potentially adding bias to the estimation as variation if several governments enter/exit the analysis at different econometric specifications, thereby potentially inducing omitted variable bias concerns and reduced the comparability among coefficient estimates.[6] Considering these concerns, the analyses presented on this paper are based on a restricted set

[5] Bandwidths computed using data from the secondary market are considerably smaller than the ones from the primary market due to the larger sample size of the data from the secondary market. Hence, I consider only the ones computed using data from the primary market to ensure there are enough observations on each side of the cutoff for all the analyses. As a robustness check I present the main results using different choices of such bandwidth.

[6] To be clear, the set of governments described above correspond to the baseline models that look at the bonds issued (primary market) by county governments whose population is within the bandwidth during the post-intervention period. For the secondary market, the treatment group is comprised by 32 counties (76 distinct issuers) and the control group by 50 counties (124 distinct issuers).

of governments, hence providing cleaner measures of the sensitivity of the results to changes on the modeling assumptions.

To be specific, along the paper the period from January 2019 to March 2020 is referred as the pre-intervention period, while April 2020 - December 2021 as the post-intervention period. The motivation for looking at 15-months after the intervention for the baseline analysis relies on the observed dynamics of the municipal bond market. Local governments access financial markets following their spending and revenue collection cycle. Therefore, the timing in which issuers go to the market could differ across governments. To have a measure that encompasses accurately county governments' response to the policy, then the post-intervention horizon should include enough periods such that the governments on both arms of the study participate on the municipal bond market.

4.2 Pre-Treatment Balance

To examine the characteristics of the bonds and governments on each arm of the study Table 1 shows a balance table of the main dependent and independent variables used for the analysis for both pre and post intervention periods. Panel A reveals significant differences on the dependent variables during both periods of the study. During the pre-intervention period, bonds issued by CRF recipients observed spreads at issue 13 basis points lower compared to their counterparts on the control group. Moreover, the amount issued per capita was $ 2.5 lower for bonds on the treatment group. A similar story is documented for the secondary market were spreads and par traded per capita were lower for the governments on the treatment group.

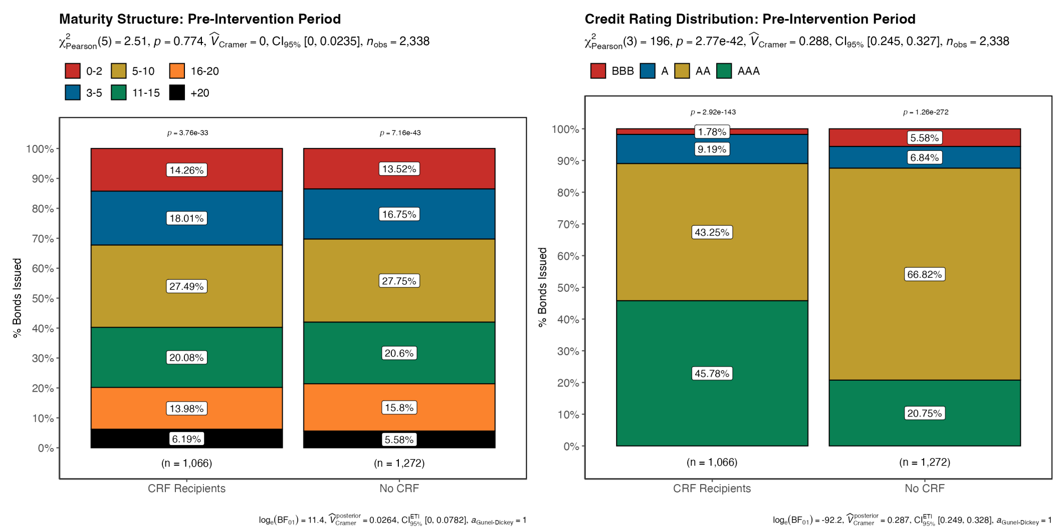
Table 1: Balance Table: Dependent and Independent Variables.

Variable	Pre-Intervention Period Control	Pre-Intervention Period Treatment	Pre-Intervention Period Mean Diff	Post-Intervention Period Control	Post-Intervention Period Treatment	Post-Intervention Period Mean Diff
Panel A: Dependent Variables						
Spread at Issue	0.0820	-0.0497	-0.1317***	0.3817	0.3726	-0.0091
	(0.5572)	(0.4727)	(0.0213)	(0.5241)	(0.5351)	(0.0188)
Amount Issued Per Capita	7.1220	4.6512	-2.4708***	7.4964	5.8880	-1.6085***
	(14.3861)	(9.5284)	(0.4979)	(13.0134)	(12.7902)	(0.4571)
Spread at Trade	0.2950	0.2103	-0.0847***	0.6402	0.4226	-0.2176***
	(0.8971)	(0.8782)	(0.0044)	(1.0243)	(0.8071)	(0.0040)
Amount Traded Per Capita	0.2892	0.2303	-0.0588***	0.2662	0.2394	-0.0268***
	(0.8308)	(0.7299)	(0.0038)	(0.8008)	(0.7753)	(0.0035)
Panel B: Independent Variables						
Coupon	3.9046	3.9668	0.0622	3.4068	3.3103	-0.0966+
	(1.2650)	(1.0868)	(0.0486)	(1.4488)	(1.4505)	(0.0514)
Credit Rating	3.3341	2.5666	-0.7675***	2.6578	2.9617	0.3039***
	(2.1201)	(1.8426)	(0.0820)	(1.6345)	(2.1241)	(0.0673)
Years to Maturity	9.9489	9.6839	-0.2650	8.8474	9.0466	0.1991
	(6.6512)	(6.6707)	(0.2766)	(6.1089)	(6.6232)	(0.2259)
Offering Type	0.3970	0.5460	0.1490***	0.5114	0.5427	0.0313+
	(0.4895)	(0.4981)	(0.0205)	(0.5000)	(0.4983)	(0.0177)
GO Bond	0.5197	0.5206	0.0010	0.6455	0.5644	-0.0810***
	(0.4998)	(0.4998)	(0.0208)	(0.4785)	(0.4960)	(0.0173)
Central Government	0.6824	0.7176	0.0352+	0.6535	0.6186	-0.0349*
	(0.4657)	(0.4504)	(0.0190)	(0.4760)	(0.4859)	(0.0170)
Unemployment Rate	3.3710	3.1562	-0.2148***	6.3645	5.8604	-0.5042***
	(0.8988)	(0.6838)	(0.0328)	(2.7607)	(2.7000)	(0.0967)

Note: This table shows the balance table across the treatment and control groups, for both the pre-intervention and post-intervention period. Columns Control and Treatment show the mean of each variable, with the standard deviation reported in parenthesis. The column Mean Diff shows the result of a t-test with the standard error reported in parenthesis.

Differences on municipal debt outcomes could be explained by variation in the main characteristics of the instruments issued, as well as factors explaining the financial conditions of the issuer governments. Panel B explore these differences and shows no significant differences on coupon rates and years to maturity during the pre-intervention period. The proportion of general obligation bonds, and bonds issued by central governments observed similar levels across groups. The results from the t-test reveals a significant difference on creditworthiness across groups, where CRF recipients were characterized by higher credit ratings. Figure 1 expands this analysis by showing a comparison on the distribution of bonds issued on the primary market during the pre-intervention period by credit rating and years to maturity. The panel on the left reveals depicts no significant differences on the maturity structure of CRF recipients and governments on the control group.The chi-squared association test yields a p-value of 0.774, and fails to reject the null hypothesis of independence across distributions. The panel on the right shows there are significant differences on the credit ratings. Issuers from CRF recipients counties observed 45.8% of their sample rated as AAA and 43.3% as AA, while bonds from issuers of non-CRF recipients were 20.7% AAA and 66.8% AA. Moreover, the control groups also observed a larger proportion of BBB. These findings align with the results from the t-tests on the continuous analog of these variables presented on Table 1, and altogether suggest that during the pre-intervention period governments on the control group observed a relatively riskier profile than their counterparts on the treatment group.

Figure 1: Pre-Treatment Comparison by Credit Rating and Years to Maturity



Notes: These panels compare bond issues by governments on the treat and control groups during the pre-treatment period. The bar-plots compare the distribution of bonds issued by maturity and credit rating between the treatment and control groups. Pearson statistic and corresponding p-value correspond to a Chi-squared association test where the null hypothesis is that the distribution by maturity (and credit rating) of the control group is independent to the distribution of the treatment group.

4.3 Post-Intervention Comparison

Figure 2 shows the distribution of the dependent variables across time. These panels show the average of the dependent variable as well as the area bounded by the inter-quartile range of group-by-month bond distribution. As depicted on both panels on the left, spreads on both primary and secondary markets spiked at the onset of the pandemic. This arguably reflects the perceived uncertainty on the market around the effects of the crisis on local economies and budgets. After the peak observed in

March-April 2020, bond spreads followed a stabilization process where pre-pandemic levels were not observed until the Q2-2021.

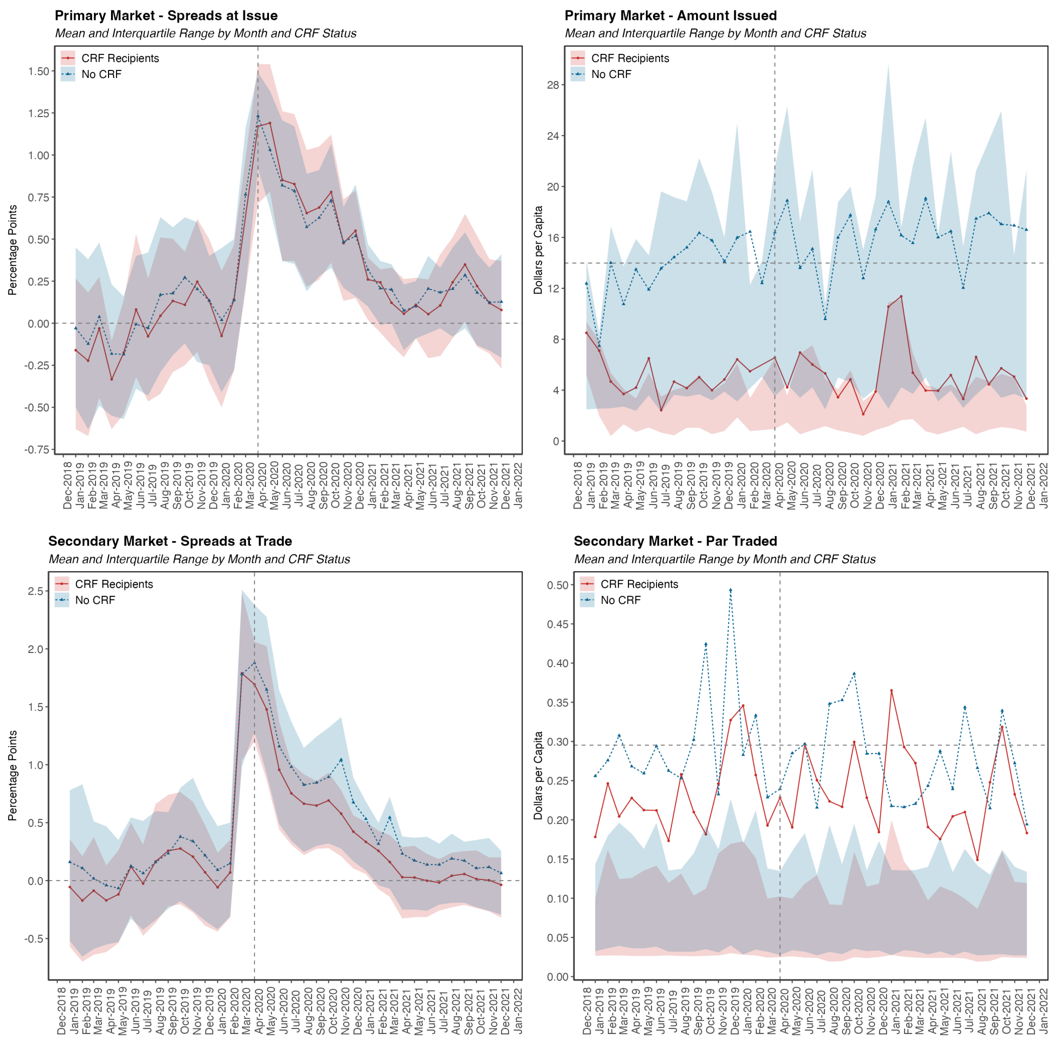
Despite the spike on primary market spreads experienced by CRF recipients at the onset of the pandemic, on the following months average bond spreads across groups remained on similar levels, although governments on the treatment group observed less variation on such spreads (captured by the more compact area bounded by the inter-quartile range). On the other hand, the average spread of CRF recipients on the secondary market remained below the one computed for the counties on the control groups during the whole post-intervention period. It stands out that for both primary and secondary markets, spreads for both groups were fluctuating close to zero and followed similar trends .

Panels on the right of Figure 2 show the par amounts of debt issued and traded on the primary and secondary markets, respectively. Visual inspection of both graphs suggests no clear trends neither on the pre-intervention or post-intervention periods. It stands out, however, the skewness of the distribution of the par-traded (per capita) on the secondary market. Average par traded for both groups is significantly above the inter-quartile range.

The right-hand side of Panel B in Table 1 shows the differences on the explanatory variables prevailed during the post-intervention period. In general, bonds from governments in the treatment group observed higher credit ratings, lower coupon rates, and were more likely to be placed through competitive sales. Similarly, bonds from the control group are more likely to be general obligation bonds or bonds issued by central county governments. It stands out that governments from the control group were characterized by an unemployment rate 50 basis points higher than their counterparts that received the CRF, which adds up to the potential risks priced by the market on the borrowing costs. While these factors altogether suggest that bonds on the control group could observe larger spreads, the t-test for the dependent variables shows there are no significant differences between bond spreads across arms of the study. Moreover, this difference is lower in magnitude relative to the one estimated for the pre-intervention period.

Governments on the control group issued more debt ($7.49 per capita) relative to the governments on the treatment group ($5.88 per capita). On the secondary market, bonds from governments on the treatment group traded, in average, 21 basis points lower than the bonds from the governments on the control group. Moreover,

Figure 2: Primary Market Spreads by Treatment Status during the Analysis Horizon



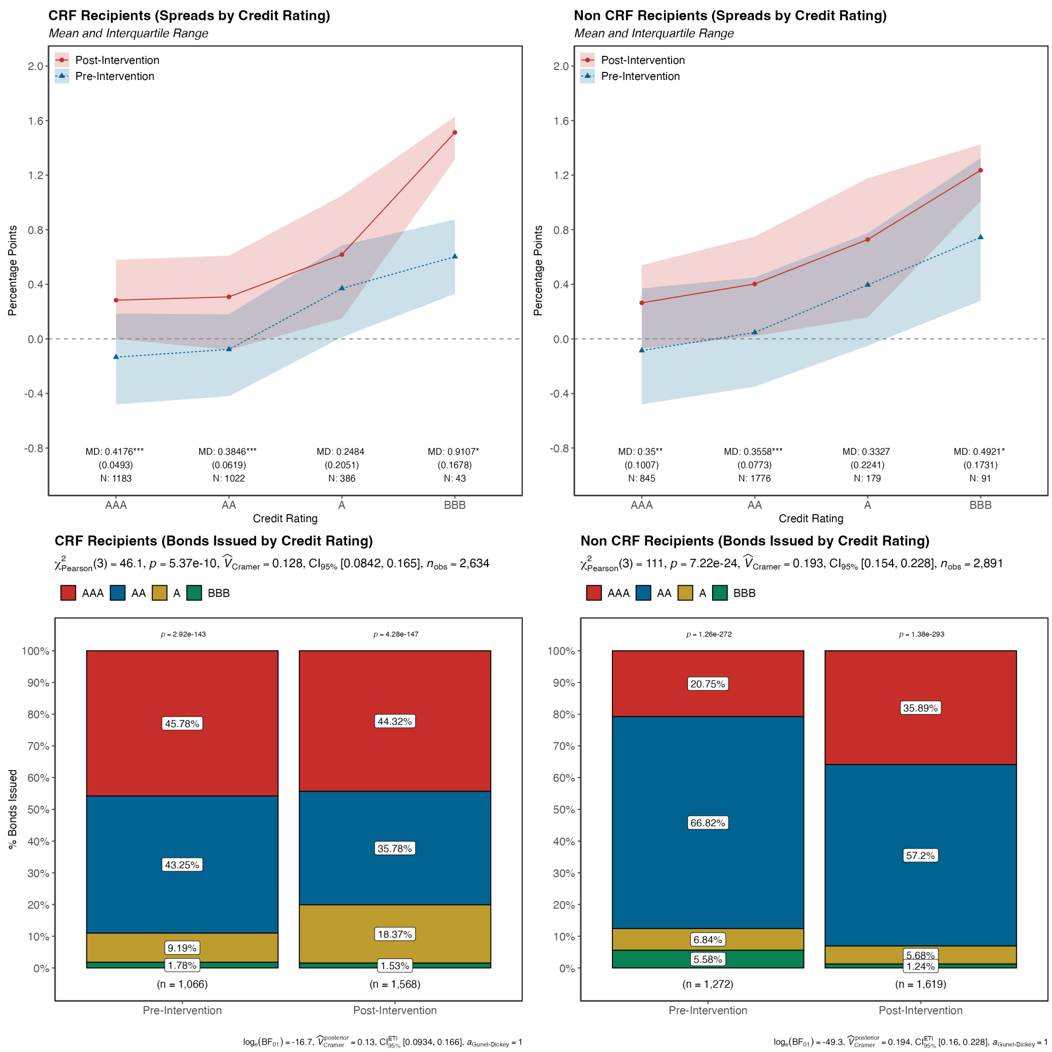
Notes: This graph shows the distribution of each dependent variable for each month between Jan-2019 and Dec-2021. The lines show the average for both treatment and control groups. The shaded areas show the inter-quartile range (i.e. distribution between the 25th and the 75th percentiles). Vertical dashed lines show the intervention month and separate the pre-intervention period from the post-intervention one. Horizontal gray dashed lines depict baseline comparisons. For the panels on the left (spreads) comparison is around zero (i.e. risk free rate), while for panels on the right (par issued/traded) the reference is the average of each dependent variable during the pre-treatment period.

the per capita par value of such trades was slightly lower (i.e. 2.68 cents) for the bonds on the treatment arm. A decrease on the difference across spreads, for instance, is consistent with a reduction in investor's risk premium on the control group, which in theory should be influenced by the (lack of) treatment. This provides some suggestive evidence on the magnitude of the effectiveness of federal policies aiming to restore confidence on the bond market. The results from the t-test for the post-intervention period capture the magnitude of the variation that the empirical model aims to explain due to the intervention and the main variables that predict outcomes on the municipal bond market.

Panel B in Table 1 also shows a change in the credit rating balance across groups. While during the pre-intervention period issuers on the treatment arm observe higher credit ratings, these deteriorated during the post-intervention period. Interestingly, governments on the control group experienced the opposite story: an increase on the credit ratings assigned at issue. Figure 3 depicts the comparison on the distribution of bonds issued by credit rating. The panels at the bottom dissect changes on the distribution before and after the implementation of the CARES Act, revealing that during the post-intervention period issuers on the treatment arm observed a deterioration on their credit quality. There was significant decrease in the proportion of AA-rated bonds, substituted by a rise in the proportion of A-rated bonds. Issuers on the control group, in contrast, observed a shift in the credit rating distribution towards higher ratings: an increase in the proportion of AAA bonds, accompanied by reductions in the proportions of the rest of the rating categories.

The top panels show an increase in spreads on the primary market during the post intervention period for all rating categories. With the exception of A-rated bonds, such increases were larger for the bonds issued by CRF recipient counties. In particular for BBB-rated bonds that observed an average difference of 91 basis points during the post-intervention period. Larger spread increases for the treatment group is consistent with the observed deterioration on the credit quality of the bonds during the post-intervention period. However, it challenges the expected effect of the CRF as it aimed to provided assistance to governments with arguably larger liquidity needs. These differences could be explained by investors expectations about the magnitude of the pandemic shock. To the extent that market expectations were pessimistic enough to offset the credit enhancement components of the policy, bonds on the treatment group could observe larger spikes on their primary market spreads, relative to their control group counterparts.

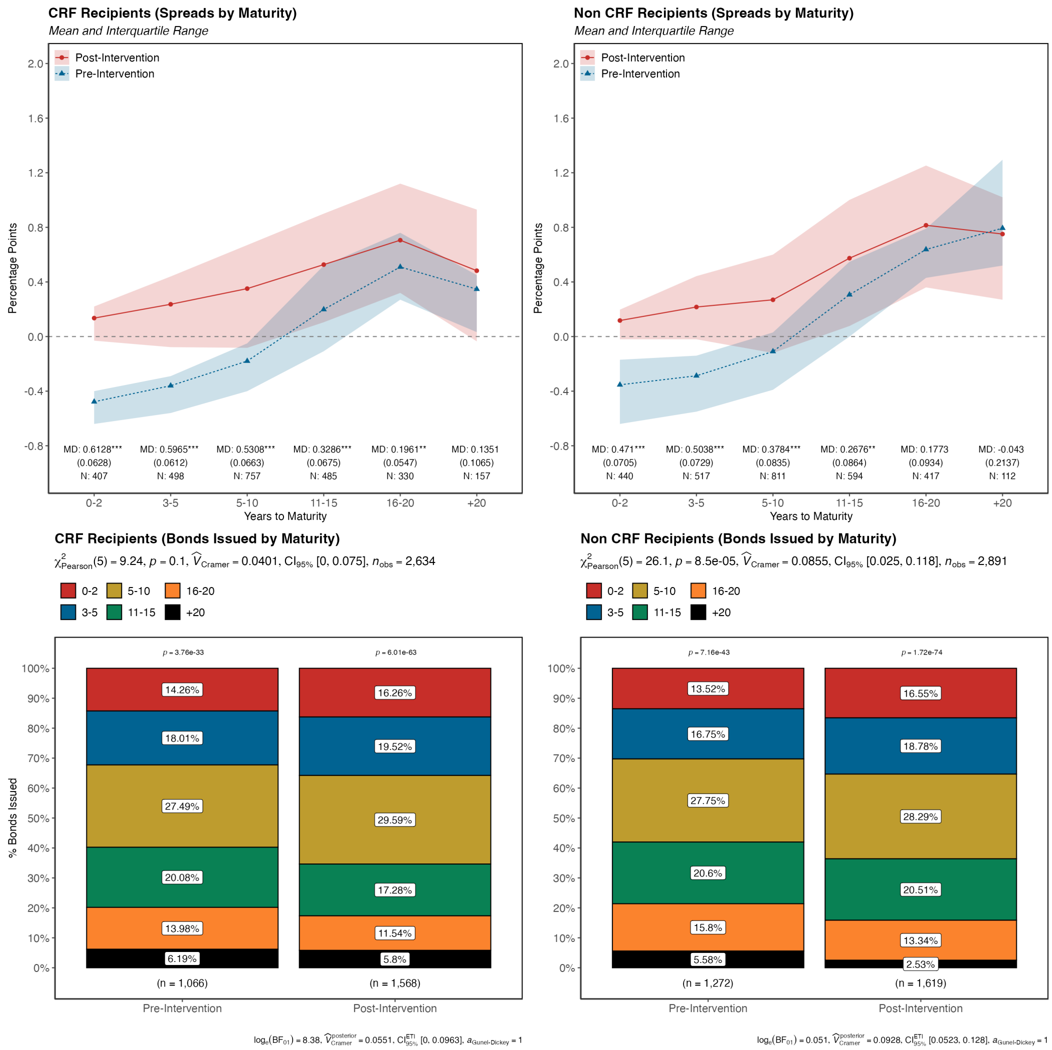
Figure 3: Primary Market Spreads by Treatment Status and Credit Rating



Notes: These panels compare bond issues by governments on the treat and control groups, before and after the intervention. Panels on the top compare the spreads at issue by credit rating. Lines correspond the average, while shaded areas bound the 25th and 75th percentiles, within the group-category-period. Coefficients reported at the bottom correspond to the unconditional mean difference. Clustered standard errors by county reported in parenthesis. Panels at the bottom compare the distribution of bonds issued by credit rating before and after the intervention. Pearson statistic and corresponding p-value correspond to a Chi-squared association test where the null hypothesis is that the distribution by credit rating before the intervention is independent to the distribution after the intervention.

Figure 4 performs a similar comparison but looking at differences across the maturity structure. The panels at the top show that for both groups primary market spreads during the post intervention period were higher across the yield curve, where (as expected from theory) investors assigned larger premiums to longer term debt. Coefficients reported at the bottom of the plot area show the results of a regression based t-test on the spreads before and after the intervention, holding constant the maturity category. The comparison of these coefficients across arms of the study shows that bonds at the treatment group observed larger spread increases during the post-intervention period for all maturities. Both groups documented an increase in the issuance of shorter-term debt (0-10 years) at the expense of a reduction on the issuance of longer-term bonds. However, these differences were more significant for governments on the control group (i.e. chi-squared test $p < 0.001$), relative to the treatment group (i.e. chi-squared test $p = 0.1$). At first sight, these results suggest that while governments in the control group increased their reliance on shorter-term debt increased more than their counterparts on the treatment group, this did not translated to higher spreads. In other words, while liquidity pressures arguably heightened and risk premiums increased, such increase was lower than the one recorded for the governments that received aid to cope with the crisis.

Figure 4: Primary Market Spreads by Treatment Status and Years to Maturity



Notes: These panels compare bond issues by governments on the treat and control groups, before and after the intervention. Panels on the top compare the spreads at issue by years to maturity. Lines correspond the average, while shaded areas bound the 25th and 75th percentiles, within the group-category-period. Coefficients reported at the bottom correspond to the unconditional mean difference. Clustered standard errors by county reported in parenthesis. Panels at the bottom compare the distribution of bonds issued by maturity before and after the intervention. Pearson statistic and corresponding p-value correspond to a Chi-squared association test where the null hypothesis is that the distribution by maturity before the intervention is independent to the distribution after the intervention.

5 Empirical Strategy

While the descriptive evidence presented in the previous section shed some light on the underlying factors driving the observed heterogeneity in debt outcomes before and after the intervention, it fails to isolate the effect of CRF. To address this concern, this section estimates the effect of direct federal assistance on municipal debt by implementing a sharp RDD that exploits the population criterion used by the Treasury for program eligibility and, therefore, treatment assignment. Equation 1 shows the statistical model of interest.

$$y_{igst} = \alpha_0 + \theta CRF_{gs} + \sum_p \beta_p pop_{gs}^p + \gamma X_{igst} + a_s + b_t + e_{igst} \tag{1}$$

y_{igst} denotes the dependent variable for bond i issued by government g from state s on date t. CRF_{gs} is a binary treatment variable, pop_{gs} denotes the population of county g. Adhering to common practice in RD designs, the running variable is defined as the distance between the population and the cutoff for treatment assignment (500,000), expressed in thousands of people. To account for instrument-specific factors that could determine municipal debt outcomes, X_{igst} is a vector of bond controls often used in bond pricing models and described in Table 1. To reduce sampling variability of the estimator (Lee and Lemieux, 2010), along with the control variables I include state a_s and month-by-year b_t fixed-effects. It is important to highlight that the datasets used for the analysis are not balanced panels of bonds across time. Observations on the data correspond to unique bond issues of county governments, hence having variation at the daily level. However, to clarify notation, b_t denotes month-by-year fixed-effects.

Estimation of this model is done through both parametric and non-parametric approaches on data considering only observations on the post-intervention period. The parametric approach consists in directly estimating Equation 1 with a fixed-effects estimator, with clustered standard errors at the county level. On the other hand, non-parametric estimation consists on the implementation of the estimator developed by Calonico et al. (2014), reporting bias-corrected estimates with robust standard errors. To adjust for the covariates and fixed-effects structure on the non-parametric model, I use as dependent variables the residuals from running Equation 1 without the treatment and population variables, and estimate the model without covariates on this residualized outcome.[7] Following Gelman and Imbens (2019), the statistical model considers both a linear and quadratic specification on the polynomial function of the running variable (i.e. $p = 1, 2$).

5.1 Threats to Validity

Eggers et al. (2018) point out two potential pitfalls for RDD where population is the policy assignment rule. First, the same population threshold could be used to determine the eligibility to other policies, hence possibly compounding the effects of the policies. In this case, the other main federal policy using population as assignment criterion was the Municipal Liquidity Facility which also established a 500,000 population threshold to determine county's eligibility. In short, through this facility the Federal Reserve established a financial mechanism to purchase short-term notes issued by eligible governments, according to the rules of the program. There were only two state governments that tapped into the facility: the State of Illinois, and the Metropolitan Transit Authority of the State of New York (Haughwout et al., 2022a). Considering both the CRF and the Municipal Liquidity Facility were implemented in April 2020 and followed the same eligibility criterion, disentangling the individual effects of each policy using the baseline RDD specification of this paper is not feasible. This requires to interpret the results with caution when drawing policy implications. Results from this model could be driven by the effect MLF's announcement had on municipal borrowing costs. However, given that no county government tapped into the MLF, then any confounding effects stemming from this policy are likely to be indirect. Moreover, the analysis conducted in this paper excludes short-term instruments (which was the financial tool provided by the Fed to local governments), thus any potential confounders are taking place through the spillovers between short-term and long-term debt instruments.

The second pitfall pointed by Eggers et al. (2018) is strategic manipulation of population reports made by county officials. This concern relates with the validity of the continuity assumption required for identification. In other words, government officials might alter their population estimate in order to land in the desired side of policy assignment. For the CRF, the Treasury used the 2019 Census population

[7] To be clear, the model is estimated assuming a triangular kernel. Standard errors are computed using the nearest neighbor (NN)-based variance estimator proposed by Abadie and Imbens (2008). I required for a minimum of 5 nearest neighbors for standard errors computation.

estimate, which was available before the policy was announced and implemented. Therefore, risks of sorting into any arm of the policy should be negligible. To address this concern, below I present statistical evidence for lack of manipulation by running a McCrary (2008) test.

5.2 Identification

One of the main strengths of the RDD is its close relation with the *gold standard* for program evaluation: randomized experiments (Lee and Lemieux, 2010). The key element for this claim, however, is the continuity assumption which requires that conditional mean function of the dependent variable is continuous at the cutoff. In the absence of non-random sorting, a comparison of a small neighborhood of units above and below the cutoff for treatment assignment, mimics the conditions of a randomized experiment. In short, the validity of the design hinges in the assumption that counties' assignment near the cutoff is as good as random.

To test the validity of this design, I adhere to the recommendations by Cattaneo et al. (2020) and perform a McCrary (2008) test on the running variable using the methodology from Cattaneo et al. (2018). Not rejecting the null hypothesis of continuity at the cutoff favors evidence for lack of manipulation. Intuitively, in the absence of systematic sorting the density of the running variable (i.e. population) should be continuous at the cutoff, hence a discontinuity at the cutoff provides evidences of self-selection or manipulation. This test is carried out on both the primary and secondary data sample of bonds during the post-intervention period. Local linear regressions are calculated using the observations within the chosen bandwidth to determine the treatment and control groups. For the baseline calculation of the local-linear regressions I consider a second order polynomial and a triangular kernel. Figure 6 in the Appendix provides a visual representation of the results of these tests. This graph shows a histogram of the running variable (i.e. 2019 population) along with the estimated polynomials to test discontinuity at the cutoff.

The p-values of the McCrary tests for the primary and secondary market data are estimated at 0.1148 and 0.2783, respectively. Therefore, the null hypothesis is not rejected and these results provide statistical evidence for no systematic manipulation of the running variable. This is not surprising considering no county could have anticipated the 2020 crisis and, moreover, the use of population as criterion for funds allocation. As a robustness check, I replicate this test using first and third order polynomials. The results for the primary market are somewhat sensitive to the choice of the polynomial as the null-hypothesis is rejected at traditional levels for the linear and cubic polynomials. On the other hand, the results on the sample from the secondary market are robust to the linear polynomial, but not to the cubic one.

6 Main Results

Table 2 shows the coefficient estimates for the Local Average Treatment Effect (LATE) from both the parametric and non-parametric estimation approaches. First two columns depict the results for the dependent variables on the primary market, while the last two for the secondary market. After removing the variation explained by the covariates and fixed effects structure imposed on Equation 1, point estimates from the non-parametric and parametric approaches suggest a decrease in bond spreads between 6.6 and 47.1 basis points, significant at the 5% level. Results from the parametric estimation align in the direction of the estimated effects by suggesting a decrease of 9.1 basis points on bond spreads, although these are not statistically significant. In terms of magnitude, estimates between 6.6 and 9.1 basis points are equivalent to 0.12-0.17x the observed standard deviation of bond spreads during this period. While the results from the quadratic specification are considerably larger than the ones from the linear polynomial, they are within one standard deviation from the mean.

The second column shows the results for the par amount issued. With the exception of the non-parametric quadratic specification, the rest of the models indicate a positive and significant increase on debt issuance on the primary market associated with the policy. These estimates suggest that CRF recipients increased their debt issuance between 1.75 and 5.07 dollars per capita, relative to the control group. To add some context to the magnitude of these estimates, they are within 0.12-0.17x standard deviations of this variable. These findings suggest that governments that received direct aid from the federal government observed lower borrowing costs and more participation on the municipal bond market during the post-intervention period.

The third and fourth columns show the LATE estimates of the CRF on the

Table 2: LATE Estimates of the CRF on the Municipal Bond Market

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
Panel A: Non-Parametric				
Linear	-0.066*	1.751*	0.085***	0.0141
	(0.0297)	(0.7711)	(0.0106)	(0.0108)
Quadratic	-0.4711*	-10.0827	-2.6152***	-0.316***
	(0.1887)	(7.0314)	(0.0723)	(0.0716)
Panel B: Parametric				
Linear	-0.0913	5.0732*	-0.4154	0.0744
	(0.0553)	(2.0702)	(0.3178)	(0.043)
Quadratic	-0.0907	4.8842*	-0.4084	0.0742
	(0.0579)	(2.0338)	(0.3122)	(0.043)
Mean Dep Var	0.3772	6.7051	0.5438	0.2543
SD Dep Var	0.5295	12.9271	0.9406	0.7897
Obs (Left Cutoff)	1619	1619	115698	115698
Obs (Right Cutoff)	1440	1440	82082	82082

Note: This table shows the coefficient estimates of the Local Average Treatment Effect for the dependent variables of interest. Each column shows the estimations from the non-parametric and parametric estimations, for both linear and quadratic polynomial specifications on the data during the post-intervention period. For the non-parametric estimation, bias corrected estimates with robust standard errors are reported. Parametric estimation reports standard errors clustered at the county level. All econometric specifications include control variables, state and month-by-year fixed effects. Spreads at issue and trade are expressed in percentage points and amount issued and traded are expressed in dollars per capita. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

secondary market. Coefficients on the spreads at trade from the non-parametric estimation are mixed between the linear and quadratic specifications. Results from the parametric approach suggest a decrease of 41 basis points, yet with large standard errors. Estimates for the effects on the par traded on the secondary market are small and not precisely estimated. With the exception of the non-parametric quadratic specification, all models suggest an increase between 1.4 and 7.4 cents on the amount traded per-capita. These results are inconclusive due to the sensibility of the non-parametric model to the polynomial specification, as due to the lack of precision on the coefficient estimates, despite the large sample size. Figure 7 at the Appendix shows visual representation of the regression discontinuity plots for each of the dependent variable for both the linear and quadratic polynomial.

Taken together these findings imply the effects of the CRF were more salient on the primary bond market, with mild to null spillovers on the secondary market. These conclusions underline the role of federal aid alleviating fiscal distress experienced by local governments during crisis episodes. The direction of the estimates at Table 2 are consistent with a theory where federal aid restored confidence among investors in the municipal bond market as credit spreads decreased for issuers that received the CRF payment and these counties observed more participation on the primary market.

7 Robustness Checks

To examine the robustness of the main results to some of the modeling assumptions of the analysis, in this section I replicate the results described at Table 2 in three main ways. First, I show the sensitivity of the LATE estimates to the selection of the bandwidth to determine the issuers that are part of the treatment and control groups. Arguably one of the main factors driving the estimated policy effects is the composition of the treatment and control groups. For the baseline analysis I consider a bandwidth of 142,000 people around the cutoff for CRF eligibility. There is an implicit trade-off between extrapolation bias and estimation precision in terms of the determination of the optimal bandwidth. Imposing stricter boundaries (i.e. reducing the distance to the cutoff) mitigates the extrapolation bias, albeit it comes at the expense of a decrease in the number of observations which hinders statistical inference.

In this section I relax this assumption and replicate Table 2 using two alternative bandwidths: 90, and 221 thousand people. These correspond to the bounds of the interquartile range on the distribution of the estimated bandwidths for primary spreads at each month of the pre-intervention period, and represents a variation of (-36%, + 55%) on the baseline bandwidth. Tables 6-7 on the appendix show the results from these exercises. Table 6 shows the results from the model with the smaller neighborhood around the cutoff. Overall, the results align with the findings at Table 2. Estimates for primary market suggest stronger reductions in bond spreads (between 12 and 23 basis points, approximately equivalent to 0.22-0.43x standard deviations) and a larger increase in debt issuance (between 2.0 and 8.7 dollars per capita) associated with the policy. Results for secondary market outcomes are still mixed. Evidence from Table 7 suggests that the baseline estimates do not observe relevant extrapolation bias concerns as the results from the model with a larger bandwidth align in direction, magnitude and precision with the coefficients at Table 2.

Second, Table 8 replicates the models at Table 2 only considering central county governments. The baseline specification is estimated on a sample of bonds that include instruments issued by county government organizations distinct to the central county government (e.g. authorities, agencies, trusts, etc). The main rationale to consider them on the baseline sample is given their dependence on county budgets to finance their operation, these could experience relevant spillovers from the provision of direct federal support. Since the inclusion of bonds from these government agencies could introduce some bias into the results since these issuers did not directly received aid from the federal government. Estimates for spreads on the primary market in general align with the baseline results, although the parametric estimation in this sample yields more precise coefficients relative to the non-parametric model. LATE estimates from the parametric model imply a reduction of 23-25 basis points, which are equivalent to approximately 0.46-0.51x the standard deviation of this variable. LATE estimates for spreads at trade for both the non-parametric and parametric approaches suggest a reduction on the borrowing costs between 23 and 200 basis points. While there is large variability on these results, it stands out that all align finding negative effects. Estimates for amount issued and traded per capita (columns 2 and 4) are mixed in this sample. Results from each estimation approach lead to coefficients with opposing signs and lack of statistical significance.

Third, to test the validity of the research design Table 9 on the appendix replicates Table 2 but estimating the model during the pre-intervention period, hence obtaining placebo estimates on the coefficients of interest. In theory, in the absence of the intervention and provided that around the cutoff assignment to treatment is as good as random, there should not be systematic differences between debt outcomes of governments above and below the cutoff for treatment assignment. Finding coefficient estimates indistinguishable from zero provides suggestive evidence on the internal validity of the research design. Intuitively, this means that there should not be differences between debt outcomes from governments in both arms of the study, before treatment exposure.

Coefficient estimates for the primary and secondary markets align with the direction of the main results. Placebo estimates for the primary market show coefficients closer to zero and not estimated at significant levels, thus providing suggestive evidence for the validity of the research design for these dependent variables. Estimates for the secondary market do find a significant differences on spreads and volume traded. This could suggest the estimates at Table 2 for this segment of the market could be overestimating the policy effect. This underlines that interpretation of the conclusions derived from the secondary market analysis should be done with caution.

7.1 Heterogeneity by Credit Rating and Years to Maturity

To examine heterogeneity on the effect driven by credit rating and time to maturity, I extend the parametric model at Equation 1 to include interactions with the categorical variables for credit rating and years to maturity. In this expanded model, $I(k = s)$ is an indicator variable that equals to one if bond i is member of category k, where k is the credit rating and years to maturity categories described at Section 4.

$$y_{igst} = \alpha_0 + \sum_h \theta_h (CRF_{gh} \times I(h = k)) + \sum_p \beta_p pop^p_{gs} + \gamma X_{igst} + a_s + b_t + e_{igst} \quad (2)$$

In this case, the coefficients of interest θ_h show the heterogeneous effect of the policy across rating and maturity categories. The reference (omitted) categories are BBB bonds and maturities between 0-2 years, respectively. The models for each heterogeneity analysis are estimated independently. Each panel at Table 3 shows the results from each model. Panels A and B depict the results for the primary market outcomes, while panels C and D for the secondary market. Aligned with the findings of the descriptive analysis, estimates from panel A suggest no significant differences on borrowing costs or amount of debt issued driven by the maturity of the issued instrument. LATE estimates for bond spreads imply larger reductions for longer term instruments.

While the interpretation should be done with caution due to the large standard errors, the direction and magnitude of the coefficients on the primary market outcomes reveal some of the heterogeneity present on the policy effects. For instance, the monotonic relationship of the coefficient estimates on the amount issued suggest a substitution across the maturity structure. CRF recipient counties increased their debt issuance on short-term instruments at the expense of decreasing issuance of longer term bonds. This highlights the magnitude of the liquidity pressures experienced by local governments that despite experiencing a cash windfall through the CRF, increased their reliance on short-term instruments. At the same time, such instruments observed reductions of smaller magnitude on their spreads at issue. This is consistent with a scenario with heightened uncertainty on the economic recovery on the short-term, but with positive long-term expectations.

Panel B describes the results for the heterogeneity on the policy effects across the credit rating categories. Both the linear and quadratic specifications suggest significant reductions between 95 and 114 basis points on the primary market spreads for all bonds rated A and above, relative to BBB bonds (i.e. the omitted category). These are large effects as they are equivalent to approximately 2 standard deviations of the distribution of this variable in the post-intervention period. It should be noted that LATE estimates on the spreads of AA and A-rated bonds are slightly larger than the ones for AAA bonds, which suggest that, in the margin, lower rated issuers benefited more from the CRF payment. This is consistent with a theory where direct aid from the treasury served as a credit enhancement and reduced the premium charged by investors driven by the perceived credit quality of the issuer during the post-intervention period.

LATE estimates on the amount of debt issued suggest large and significant increases for bonds rated AA and above. For both these categories, the implied effect suggest an increase between 10.24 and 10.89 dollars per capita in the volume of debt issued. Despite these results are large, they are still within one standard deviation on this variable. Finding larger effects for higher rated bonds aligns with the idea that governments with stronger credit quality had more access to the bond market, and hence increased their capacity to engage in deficit spending during the post-intervention period.

Panels C and D show the results for the outcomes on the secondary market. Overall, coefficient estimates are not significant at traditional levels, despite the large sample size improves the estimation precision. Results from the interactions with the maturity categorical variable indicate that longer-term bonds observed larger decreases on bond spreads, as well as higher volumes on the trades on the secondary market. This is consistent with the findings on the primary market and provide some suggestive evidence on fly-to-safety behavior on investor's side. Point estimates suggest small reductions on the trading of shorter-term bonds, accompanied by an increase on the trading of long-term bonds (i.e. maturity greater than 20 years) of 14 cents per capita, significant at the 5%. In terms of magnitude, this increase is within 0.20x the standard deviation of this variable on the sample.

Results for the coefficients on credit ratings show that lower rated bonds were more benefited from the policy as they observed larger reductions on the spreads at trade, and increases on the amount per-capita traded. While this is consistent with a scenario where CRF payments served as a credit enhancement it challenges the fly-to-safety interpretation described above since the coefficients on the par amount traded suggest an increase in the trading of lower rated bonds. Taken together, these results provide some suggestive evidence on investor's perceptions around the recovery of the municipal bond market and, to which extent these were shaped by the provision of federal aid to distressed governments.

7.2 Dynamic Heterogeneity and Placebo Tests

To examine potential dynamic heterogeneity of the policy effects, inspired by the Intent-to-Treat estimator proposed by Cellini et al. (2010), I expand Equation 1 to include time-to-event interactions for the treatment variable and the polynomial function on the running variable.

$$y_{igst} = \alpha_0 + \sum_{\tau \in t} \left(\theta_\tau CRF_{gs} \times I(\tau = t) + \sum_{p} (\beta_\tau^p pop_{gs}^p \times I(\tau = t)) \right) + \gamma X_{igst} + a_s + b_t + e_{igst} \tag{3}$$

Unlike the previous models, this model estimated on the data that includes both the pre-intervention and post-intervention periods.[8] Furthermore, to account for the potential variation on the policy effects driven by the magnitude of the transfer observed by recipient governments, for this econometric specification the treatment variable is expressed as a continuous variable that equals to the observed payment per capita for the recipient county governments, and zero for their counterparts on the control group. Coefficients θ_τ of this flexible model mimic the coefficients from an event study as they capture potential lagged effects of the policy, as well as anticipation effects. Intuitively, the structure of this model is equivalent to an

[8]Since this model incorporates data from the pre-intervention period to the analysis, this leads to a slight recomposition on the number of issuers on both arms of the study. For this segment of the analysis, the treatment group is comprised by 31 counties (46 distinct issuers) and the control group by 46 counties (77 distinct issuers) for the primary market. On the other hand, for the secondary market the treatment group includes 32 counties (50 distinct issuers) and the control group 50 counties (132 distinct issuers).

stacked estimation of t independent RD models with the specification at Equation 1 on leads and lags of the dependent variable. Interacting the polynomial function on the running variable with the time to event dummy variables allows the model to have individual coefficients on the running variable, which translates into higher estimation precision as these coefficients capture the component on municipal bond outcomes that varies at the county level but is fixed within counties over time (Cellini et al., 2010).

Figure 5 show the point estimates of the LATE for aggregated outcome variables since the intervention until each of the months displayed at the graph. The shaded areas portrays the confidence intervals at the 5% level. The first panel shows the coefficient estimates for primary market spreads. These results align with the trends observed at Figure 2. CRF recipients observed a larger hike on their spreads during April 2020, the weeks following the enactment of the CARES Act. Estimates for the secondary market are close to zero and noisy. This is also consistent with the mixed results documented on the previous sections. [9]

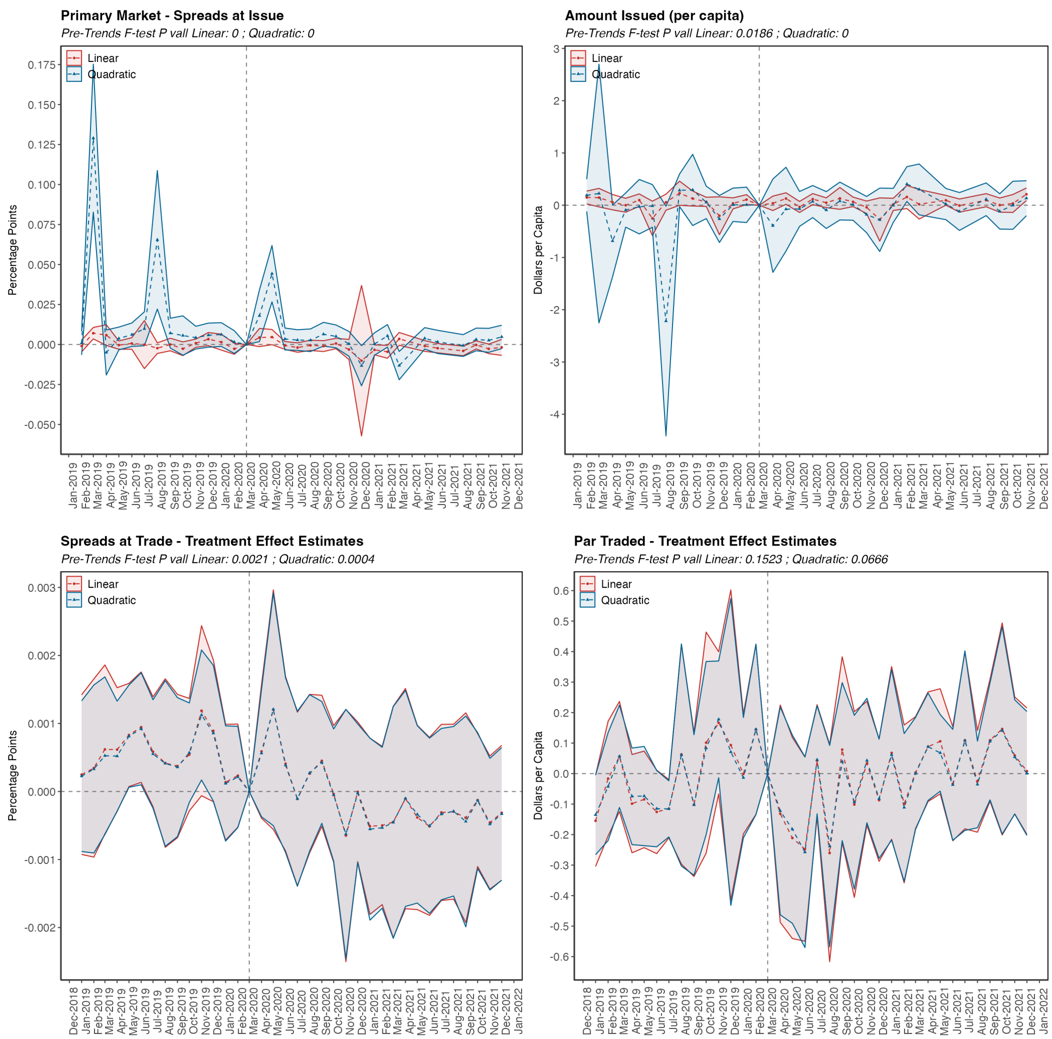
While the CRF was perhaps the first policy tool implemented by the US government to aid state and local governments to cope with the pandemic, it was followed by expansions of the CARES Act and other complementary policies. The America Rescue Plan Act of 2021 stands out as it provided support to subnational governments through the Coronavirus State and Local Fiscal Recovery Funds, which allocated \$65.1 billion to all county governments in the United States. Unlike the CRF, however, there was not an eligibility criterion for this aid. Therefore all local governments received some payment through this mechanism. Considering that all counties were influenced by this policy, there should not be relevant concerns associated with the effect identified by the RD to be confounded with the implementation of the America Rescue Plan. Furthermore, any effect of this policy should work in the same direction as the CRF since both are using population as the criterion to determine the magnitude of the aid received from the federal government. In such case, the LATE estimates reported in this analysis could be serve as a lower bound of the treatment effect of federal aid on municipal debt outcomes. Finally it should be noted that evidence at Figure 5 suggests that any potential spillovers from the America Rescue Plan are negligible since there is not a significant change in the treatment effect after the plan was presented into Congress (February 2021) and became law

[9] As a robustness check, I estimate these models including county fixed-effects. The estimates across models remained virtually unchanged, although there is a slight improvement on the precision of the coefficient estimates.

(March 2021).

Coefficients θ_τ during the pre-intervention period serve as placebo falsification tests for each period towards the intervention. Following the tradition of difference-in-difference designs, I falsify the null-hypothesis of coefficients θ_τ on the pre-intervention period being jointly equal to zero. In the absence of anticipation, the analysis should find evidence to not-reject the null-hypothesis. The subtitles of each panel at Figure 5 show the p-values of such F-test. The results of these tests show small p-values, which do not provide statistical support of the absence of anticipation during all the periods of the pre-intervention period. However, these results could be driven by the stringency of this test and the high variability of some coefficient estimates. Visual inspection of these coefficients shows that, for the most part, they are estimated close to zero hence providing evidence on the validity of the design. Analyzing at the individual coefficients for each month across models shows that the majority of them report p-values above 0.05. For instance, out of all the pre-intervention coefficients estimated across specifications for the model on primary market spreads, the average estimate was below 0.00035 basis points and 82% of them reported p-values above 0.05. Similar results are found for the models on the amount issued, and the outcomes on the secondary market. Taken together, these results shed some light on the internal validity of the research design.

Figure 5: Dynamic Treatment Effects



Note: These panels show the coefficient estimates θ from Equation 3, for both linear and quadratic ($p = 1, 2$) polynomial specifications. Shaded areas correspond to 95% confidence intervals computed with clustered standard errors at the county level.

Table 3: Treatment Effect Heterogeneity by Credit Rating and Years to Maturity

Variable	Spread (1)	Spread (2)	Amount (1)	Amount (2)
Panel A: PM-Years to Maturity				
3-5	-0.0112	-0.0086	0.9771	1.0442
	(0.032)	(0.0327)	(2.2011)	(2.1798)
5-10	0.0298	0.032	0.7753	0.8329
	(0.0605)	(0.0606)	(2.1669)	(2.1519)
11-15	-0.0201	-0.0183	0.1319	0.1804
	(0.0859)	(0.0863)	(2.2331)	(2.2234)
16-20	-0.0841	-0.0822	-0.0978	-0.0501
	(0.0936)	(0.0933)	(2.5196)	(2.5081)
+20	-0.193	-0.1825	-8.7971	-8.5248
	(0.1304)	(0.1305)	(13.6596)	(13.5597)
Panel B: PM-Credit Rating				
AAA	-0.9599***	-0.9813***	10.7081*	10.8928*
	(0.1918)	(0.2049)	(4.7264)	(4.6565)
AA	-1.0689***	-1.114***	10.2448*	10.6344*
	(0.1919)	(0.2124)	(3.8896)	(4.2341)
A	-0.968***	-1.0174***	8.0134	8.4395
	(0.2657)	(0.2759)	(5.8051)	(5.7059)
Panel C: SM-Years to Maturity				
3-5	0.0091	0.0012	0.0012	0.0004
	(0.0308)	(0.0309)	(0.0141)	(0.0145)
5-10	-0.104	-0.1114	-0.0256	-0.0263
	(0.0627)	(0.0641)	(0.0197)	(0.0196)
11-15	-0.0043	-0.0112	-0.032	-0.0327
	(0.0585)	(0.0596)	(0.0385)	(0.0382)
16-20	-0.2441	-0.2594	0.0664	0.0649
	(0.2149)	(0.22)	(0.0443)	(0.0437)
+20	-0.2866	-0.3078	0.1445*	0.1425*
	(0.258)	(0.2642)	(0.0704)	(0.0709)
Panel D: SM-Credit Rating				
AAA	-0.5077	-0.4576	-0.0301	-0.0296
	(0.3901)	(0.4057)	(0.1293)	(0.132)
AA	-0.5629	-0.5624	0.0249	0.0249
	(0.4058)	(0.4079)	(0.1212)	(0.1212)
A	-0.6137	-0.6194	0.1813	0.1813
	(0.4415)	(0.4432)	(0.0917)	(0.0915)
Specification	Linear	Quadratic	Linear	Quadratic
Mean Dep Var	0.3772	0.3772	6.7051	6.7051
Std Dev Dep Var	0.5295	0.5295	12.9271	12.9271

Note: This table shows the estimates of coefficients θ_s from Equation 2 under the parametric estimation. Each panel shows the results from independent models on the dependent variables of interest. PM: Primary Market. SM: Secondary Market. Clustered standard errors at the county level are reported in parenthesis. Spreads at issue and trade are expressed in percentage points and amount issued and traded are expressed in dollars per capita. All econometric specifications include control variables, state and month-by-year fixed effects. Spreads at issue and trade are expressed in percentage points and amount issued and traded are expressed in dollars per capita. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

8 Conclusions

This paper reviews outcomes on the municipal bond market around the COVID-19 pandemic, drawing comparisons between county governments that received direct aid from the US Treasury through the CRF and governments that did not. Broadly, the findings indicate that recipient governments observed mild reductions in their borrowing costs and increased their debt issuance on the primary market, with no significant spillovers to the secondary market. This indicates that federal aid produced crowd-in effects for local governments that enabled the provision of local services. Moreover, this analysis provides some suggestive evidence on the liquidity management undertaken by local governments. It documents an increase in the issuance of short-term debt, at the expense of reductions on the issuance of longer-term bonds.

The descriptive analysis shows that, despite governments on the treatment group observed higher credit quality coming to the pandemic, during the post-intervention period they observed a significant deterioration on their average credit quality. This could be consistent with a scenario of heightened uncertainty around the medium term effects of the pandemic, where the market assigned higher risk premiums to bonds issued by governments more likely to experience adverse fiscal and economic conditions.

Despite this change on the credit quality of governments in CRF recipient counties, the main results of this paper show these governments observed improved conditions when accessing the debt market during the post-intervention period. They observed mild borrowing costs decreases and, at the same time, increased their per-capita debt issuance. The examination of the treatment effect heterogeneity highlights credit quality as one of the main mechanisms driving the results. At the margin, lower rated governments benefited more from the policy as they experienced larger borrowing costs reductions, even-though these bonds documented a smaller increase in per-capita debt issuance .

Both the descriptive and empirical analyses show that governments increased their reliance on shorter-term instruments, at the expense of reducing longer-term debt issuance. The descriptive analysis shows this dynamic was present for both arms of the study, where the increases seemed to be larger for governments on the control group and they documented a statistically significant change on the bond distribution across the yield curve between the pre and post-intervention periods (see Figure 4). The empirical analysis, while lacking statistical precision at traditional levels, implies the shift towards shorter-term debt was large for issuers on the treatment arm of the study. However the borrowing cost reduction on these bonds was lower compared to the one estimated for longer-term debt. This could be consistent with a scenario where market expectations for a short-term economic recovery were relatively low. Together, these findings underline the magnitude of the liquidity pressures experienced by local governments and how these were managed through municipal debt policy.

Results on the secondary market analysis are mixed and not conclusive, although the evidence aligns with some of the conclusions derived for the primary market, where investors traded bonds from CRF recipient governments at a higher volume and at lower spreads. Moreover, the heterogeneity by maturity structure analysis shows evidence on fly-to-safety behavior since trading volume increased for longer term bonds, at the expense of reductions on shorter-term bonds. Yet, the results from the heterogeneity driven by credit quality challenge this interpretation as the estimates show an increase on the trading of lower rated bonds (although these show large standard errors that hinder the validity of these conclusions).

One of the main external validity limitations of the analyses presented on this paper is that they only captures the direct effects of the CRF on municipal governments. While the eligibility rule implied that only governments with population above 500,000 experienced the treatment, state governments could distribute some of the funds from their allocation across their local governments. This could translate in some CRF recipients observing a larger positive liquidity shock on their finances, while at the same time could imply some governments on the control arm of the study receiving aid from their home states. Further research on this area could extend the analysis presented in this paper to incorporate the second order effects driven by other sources of support.

This paper adds to the growing literature of studies examining the effect of COVID-19 policies on local government finances and the municipal bond market, and provides an example on the influence the federal government has on shaping the outcomes of local governments in financial markets. While this study focused only county governments as the unit of analysis, we could expect to observe similar dynamics on state government and city government debt. It remains unclear, however, to which extent the magnitude of the policy effects varies across levels of government. Further research could shed some light on the role the federalist arrangement between state and local governments play on moderating the effect of federal aid to subnational governments.

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9 Data Appendix

Table 4: CARES Act Allocations and Payments to State and Local Governments, Billion of USD

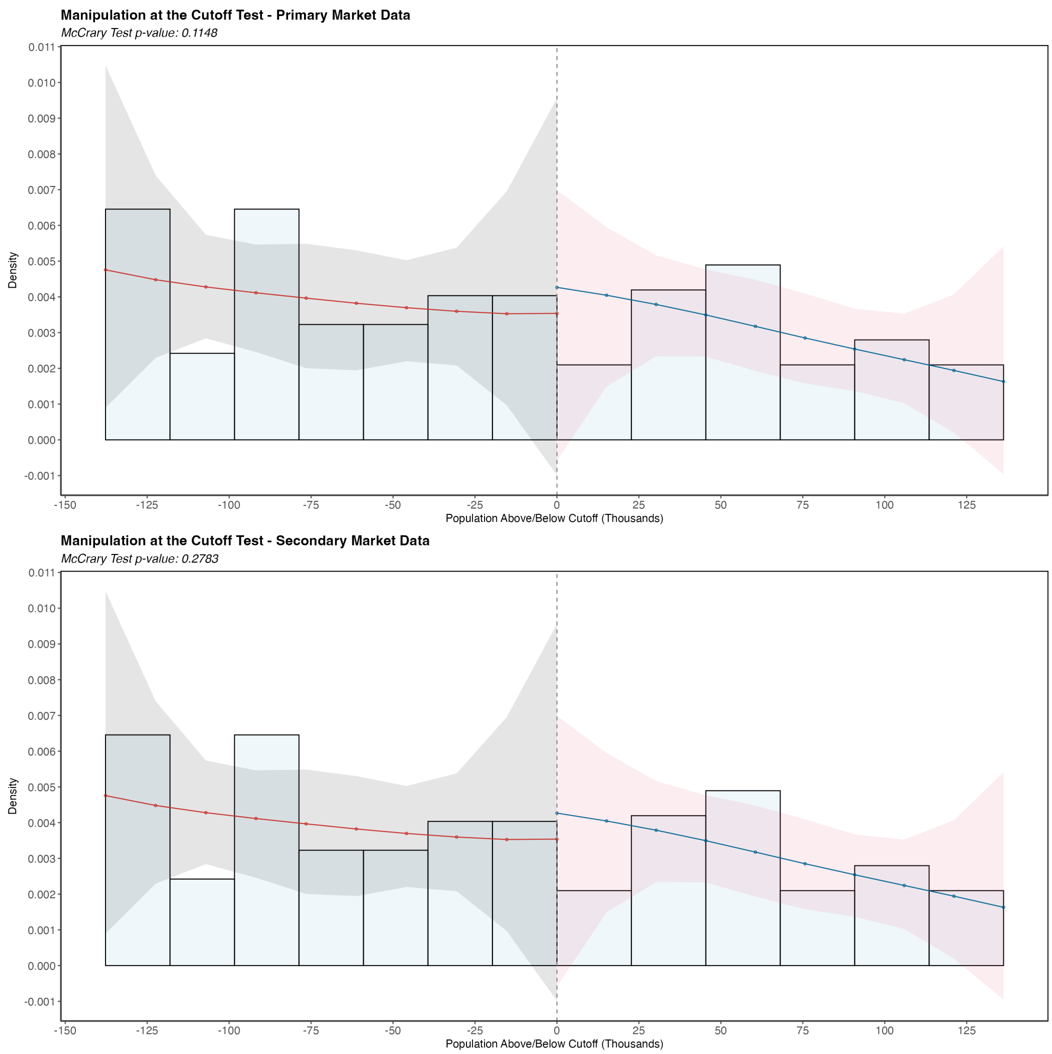
State	Total Allocation	Payment to State	Payment to Local Govs
Total	139.0000	111.3737	27.6263
California	15.3213	9.5256	5.7957
Texas	11.2435	8.0383	3.2051
Florida	8.3282	5.8558	2.4724
New York	7.5433	5.1356	2.4077
Pennsylvania	4.9641	3.9352	1.0289
Illinois	4.9136	3.5189	1.3947
Ohio	4.5326	3.7541	0.7785
Georgia	4.1170	3.5029	0.6141
North Carolina	4.0669	3.5854	0.4815
Michigan	3.8725	3.0807	0.7918
New Jersey	3.4442	2.3939	1.0503
Virginia	3.3097	3.1095	0.2002
Washington	2.9528	2.1671	0.7857
Arizona	2.8224	1.8570	0.9654
Massachusetts	2.6726	2.4608	0.2118
Tennessee	2.6481	2.3634	0.2847
Indiana	2.6105	2.4422	0.1683
Missouri	2.3799	2.0837	0.2962
Maryland	2.3443	1.6533	0.6910
Wisconsin	2.2577	1.9973	0.2604
Colorado	2.2330	1.6738	0.5592
Minnesota	2.1868	1.8699	0.3169
South Carolina	1.9965	1.9051	0.0914
Alabama	1.9013	1.7863	0.1149
Louisiana	1.8026	1.8026	0.0000
Kentucky	1.7324	1.5986	0.1338
Oregon	1.6355	1.3885	0.2470
Oklahoma	1.5344	1.2591	0.2753
Connecticut	1.3825	1.3825	0.0000
Alaska	1.2500	1.2500	0.0000
Arkansas	1.2500	1.2500	0.0000
Delaware	1.2500	0.9272	0.3228
Hawaii	1.2500	0.8628	0.3872
Idaho	1.2500	1.2500	0.0000
Iowa	1.2500	1.2500	0.0000
Kansas	1.2500	1.0341	0.2159
Maine	1.2500	1.2500	0.0000
Mississippi	1.2500	1.2500	0.0000
Montana	1.2500	1.2500	0.0000
Nebraska	1.2500	1.0839	0.1661
Nevada	1.2500	0.8361	0.4139
New Hampshire	1.2500	1.2500	0.0000
New Mexico	1.2500	1.0678	0.1822
North Dakota	1.2500	1.2500	0.0000
Rhode Island	1.2500	1.2500	0.0000
South Dakota	1.2500	1.2500	0.0000
Utah	1.2500	0.9348	0.3152
Vermont	1.2500	1.2500	0.0000
West Virginia	1.2500	1.2500	0.0000
Wyoming	1.2500	1.2500	0.0000

Note: This table shows the state allocations that each state received as part of the Coronavirus Relief Fund. Payment to state shows the amount directly transferred to state governments, while Payment to Local Governments shows the total amount of resources channeled directly to counties and cities, and that was subtracted from state's total allocation. Local governments from states where the payment to state equals the total allocation (e.g. Lousiana, Connecticut, Alaska, Arkansas, Idaho, Iowa, North Dakota, Rhode Island, Vermont, West Virginia, and Wyoming), did not received direct aid from the Treasury through this policy.

Table 5 depicts the descriptive statistics of the sample used for the empirical analysis. For example, the first row shows that the average TIC observed in the baseline post-intervention period for county governments issuing bonds in the municipal bond market was 137.84 basis points higher than the average yield observed for Treasury securities in that period. The second and third rows show the count of bond issues and the total amount of debt issued by a county government during the post-intervention period, respectively. Both variables are expressed in logarithms to smooth the underlying variance observed in the data. For reference, the raw values these variables observed imply the average county government in my sample issued 21.52 bonds for a total of $ 58.52 million.

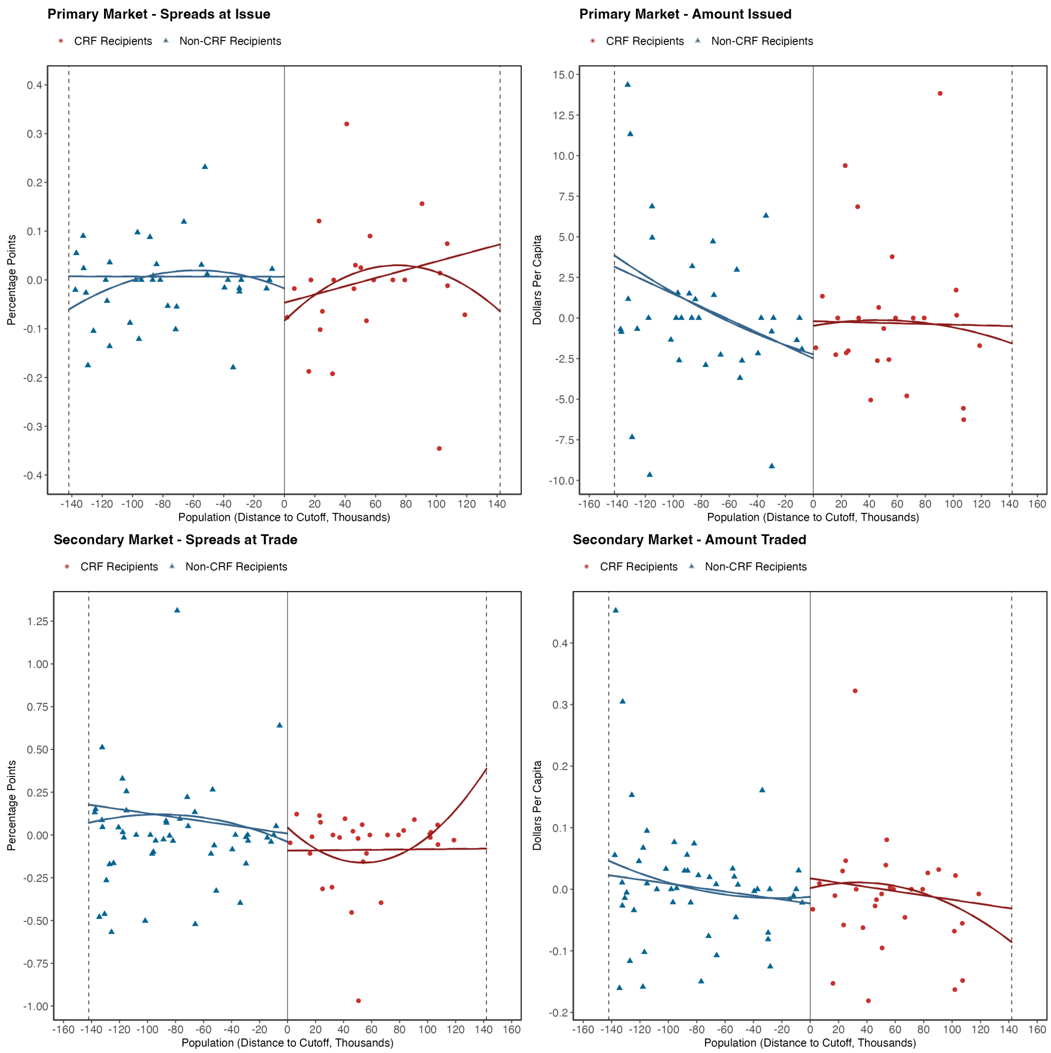
Similarly, Table 5 shows that the average county government issued bonds that were A-rated, with a coupon rate of 2.85%, and an average maturity of 7.49 years. The average government in my sample observed an unemployment rate of 6.95% in the baseline post-intervention period. Regarding fiscal structure, I observe that in average, counties active in the municipal market during this period draw 21% of their fiscal revenues from the sales tax, and 72% from the property tax. Furthermore, 91% of its spending is channeled through current expenditures. Finally, the last row shows the descriptive statistics of the running variable used for the analysis, which highlights that approximately 84% of the counties active in the market were below the population cutoff and, thus, not eligible for direct assistance from the Treasury through the CRF.

Figure 6: Manipulation at the Cutoff Test



Note: This figure shows the histogram of the running variable (i.e. population) and shows the estimated polynomial for each side of the cutoff, along with its confidence intervals at the 95% of significance. These intervals are represented as the shaded areas on the graph. Units on the vertical axis represent the density of the running variable. Observations in red correspond to governments in the control group, while observations in blue to units from the treatment group.

Figure 7: Regression Discontinuity Plots - Non Parametric Estimation



Note: These figures display the scatter binned plots of the dependent variables around the cutoff for treatment assignment, as well as the results from the non-parametric estimation of the statistical model at Equation 1. The gray dashed lines show the optimal bandwidth used for the estimation of the Local Average Treatment Effect. Both linear and quadratic estimations are reported. The top-left scatter-plot (spreads at issue) restricts the vertical axis to exclude an outlier observation that obscures the visualization results.

Table 5: Descriptive Statistics

Variable	Mean	SD	Min	P25	P50	P75	Max	N
Panel A: Primary Market								
Spread at Issue	0.2269	0.5558	-0.93	-0.18	0.14	0.58	2.27	5525
Amount Issued Per Capita	6.4048	12.7385	0.0722	1.3529	3.2381	6.7978	195.2708	5525
Coupon	3.602	1.3746	0	2.471	4	5	5	5525
Credit Rating	2.8822	1.958	1	1	3	4	10	5525
Years to Maturity	9.3189	6.5066	0	4	8	14	39	5525
Offering Type	0.5006	0.5	0	0	1	1	1	5525
GO Bond	0.5694	0.4952	0	0	1	1	1	5525
Central Government	0.6626	0.4729	0	0	1	1	1	5525
Unemployment Rate	4.9132	2.5674	1.8	3.1	4.4	5.8	17.4	5525
Panel B: Secondary Market								
Spread at Trade	0.4172	0.9293	-2.708	-0.21	0.236	0.808	4.414	373144
Amount Traded Per Capita	0.2585	0.7894	0.008	0.0271	0.0564	0.138	10.1146	373144

Note: This table shows the descriptive statistics of the samples used for the primary and secondary market analysis. Spreads, coupon rate, and the unemployment rate are expressed in percentage points and amounts (issued an traded) in dollars per capita. Offering Type, GO Bond and Central Government are dummy variables that equal to one if the bond sale was competitive, the bond is a general obligation bond, and was issued by the central county government, respectively.

Table 6: LATE Estimates of the CRF on the Municipal Bond Market (Bandwidth = 90K)

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
Panel A: Non-Parametric				
Linear	-0.122***	2.0563*	-0.1936***	-0.0073
	(0.0348)	(0.8468)	(0.013)	(0.0132)
Quadratic	-1.4567***	-23.5114	1.8227***	-0.5106***
	(0.4362)	(16.662)	(0.1221)	(0.1073)
Panel B: Parametric				
Linear	-0.1858	8.763*	0.1468	0.0783
	(0.1026)	(3.8046)	(0.2258)	(0.0547)
Quadratic	-0.2326*	7.1787**	0.1369	0.0799
	(0.1019)	(2.6133)	(0.2274)	(0.0563)
Mean Dep Var	0.4367	6.6966	0.5943	0.252
SD Dep Var	0.5402	12.4442	0.9836	0.7779
Obs (Left Cutoff)	1117	1117	76170	76170
Obs (Right Cutoff)	1012	1012	57652	57652

Note: This table shows the coefficient estimates of the Local Average Treatment Effect for the dependent variables of interest, on the sample of bonds of all issuers with a population within 90 thousand people from the cutoff. Each column shows the estimations from the non-parametric and parametric estimations, for both linear and quadratic polynomial specifications on the data during the post-intervention period. For the non-parametric estimation, bias corrected estimates with robust standard errors are reported. Parametric estimation reports standard errors clustered at the county level. All econometric specifications include control variables, state and month-by-year fixed effects. Spreads at issue and trade are expressed in percentage points and amount issued and traded are expressed in dollars per capita. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 7: LATE Estimates of the CRF on the Municipal Bond Market (Bandwidth = 221K)

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
Panel A: Non-Parametric				
Linear	-0.0727*	0.9516	0.0778***	0.0093
	(0.029)	(0.7716)	(0.0105)	(0.0108)
Quadratic	-0.4514*	-7.5199	-3.1384***	-0.2907***
	(0.1849)	(7.0466)	(0.0712)	(0.0696)
Panel B: Parametric				
Linear	-0.0913	5.0732*	-0.4154	0.0744
	(0.0553)	(2.0702)	(0.3178)	(0.043)
Quadratic	-0.0907	4.8842*	-0.4084	0.0742
	(0.0579)	(2.0338)	(0.3122)	(0.043)
Mean Dep Var	0.3958	6.5797	0.5445	0.2582
SD Dep Var	0.533	12.4497	0.9353	0.7978
Obs (Left Cutoff)	3130	3130	123691	123691
Obs (Right Cutoff)	1736	1736	88717	88717

Note: This table shows the coefficient estimates of the Local Average Treatment Effect for the dependent variables of interest, on the sample of bonds of all issuers with a population within 221 thousand people from the cutoff. Each column shows the estimations from the non-parametric and parametric estimations, for both linear and quadratic polynomial specifications on the data during the post-intervention period. For the non-parametric estimation, bias corrected estimates with robust standard errors are reported. Parametric estimation reports standard errors clustered at the county level. All econometric specifications include control variables, state and month-by-year fixed effects. Spreads at issue and trade are expressed in percentage points and amount issued and traded are expressed in dollars per capita. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 8: LATE Estimates of the CRF on the Municipal Bond Market - Only Central County Governments

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
Panel A: Non-Parametric				
Linear	-0.0305	-1.0945	-0.2301***	-0.0466*
	(0.0378)	(1.0154)	(0.0127)	(0.0181)
Quadratic	-0.3976	-4.316	-2.0331***	-0.433***
	(0.2672)	(8.7396)	(0.0891)	(0.1053)
Panel B: Parametric				
Linear	-0.2346*	3.2395	-0.5842	0.0939
	(0.1112)	(4.6124)	(0.3139)	(0.0663)
Quadratic	-0.2584*	2.4895	-0.5355*	0.0878
	(0.0966)	(4.6091)	(0.2678)	(0.0693)
Mean Dep Var	0.3368	7.2556	0.4833	0.267
SD Dep Var	0.4975	12.5913	0.8759	0.8204
Obs (Left Cutoff)	1058	1058	76896	76896
Obs (Right Cutoff)	876	876	49474	49474

Note: This table shows the coefficient estimates of the Local Average Treatment Effect for the dependent variables of interest on the sample of bonds considering only central county government issuers. Each column shows the estimations from the non-parametric and parametric estimations, for both linear and quadratic polynomial specifications on the data during the post-intervention period. For the non-parametric estimation, bias corrected estimates with robust standard errors are reported. Parametric estimation reports standard errors clustered at the county level. All econometric specifications include control variables, state and month-by-year fixed effects. Spreads at issue and trade are expressed in percentage points and amount issued and traded are expressed in dollars per capita. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 9: Robustness Checks: Placebo Estimates on the LATE

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
Panel A: Non-Parametric				
Linear	-0.029	1.4842	0.1307***	0.0286*
	(0.0324)	(0.9819)	(0.0129)	(0.0115)
Quadratic	-0.2298	10.7008	-0.5077***	-0.3324***
	(0.1992)	(7.6214)	(0.0793)	(0.0796)
Panel B: Parametric				
Linear	-0.0949	4.9162*	0.0121	0.0583
	(0.0859)	(2.4537)	(0.0923)	(0.0525)
Quadratic	-0.0935	5.0143	0.0174	0.0536
	(0.0836)	(2.5278)	(0.0896)	(0.051)
Mean Dep Var	0.0219	5.9954	0.2582	0.2636
SD Dep Var	0.5244	12.4678	0.8899	0.789
Obs (Left Cutoff)	1272	1272	93529	93529
Obs (Right Cutoff)	998	998	63630	63630

Note: This table shows the coefficient estimates of the Local Average Treatment Effect for the dependent variables of interest. Each column shows the estimations from the non-parametric and parametric estimations, for both linear and quadratic polynomial specifications on the data during the post-intervention period. For the non-parametric estimation, bias corrected estimates with robust standard errors are reported. Parametric estimation reports standard errors clustered at the county level. All econometric specifications include control variables, state and month-by-year fixed effects. Spreads at issue and trade are expressed in percentage points and amount issued and traded are expressed in dollars per capita. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.