

# 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 liquidity directly, though the effectiveness on a dollar-per-dollar basis depends on its complementary or substitutability with local borrowing. Through this lens this paper examines the effects of the Coronavirus Relief Fund (CRF) on 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 municipal securities market.

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# 1 Introduction

Historically, the U.S. federal government has repeatedly provided aid to local governments during macroeconomic 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. Federal aid can support local governments in maintaining the provision of public goods and services or in promoting policies that align with federal priorities.

However, this is not a guaranteed consequence of aid on a dollar-for-dollar basis, and the effect on municipal debt outcomes remains an empirical question. For instance, federal support to distressed governments could translate into positive outcomes on municipal debt markets if such support restores investor confidence in local government finances ([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. This could add pressure to local governments looking for debt financing during periods of economic instability. 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.

This paper examines the municipal bond market during the first stages of the COVID-19 pandemic to study the effect of federal aid on local government borrowing during macroeconomic crises. During this period, some of state and local governments coped with the liquidity pressures induced by the crisis (i.e., contraction of fiscal revenues, unexpected hike in public health spending) with direct federal aid channeled through the Coronavirus Relief Fund (CRF), the arm of the CARES Act that distributed \$150 billion in to assistance state and local governments covering COVID-related expenses, thereby alleviating near-term fiscal pressures. The U.S. 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.

This paper exploits this feature of the CRF to analyze municipal debt outcomes at both the primary and secondary market. This is achieved through the implementation of a regression discontinuity design (RDD) in a sample of bonds that were issued or traded between April 2020 and December 2021 by county governments whose population is within a close distance to the cutoff for CRF eligibility set by the U.S. Treasury. The key assumption behind the internal validity of this comparison is that, within this bandwidth, CRF eligibility mimics the conditions of a randomized control trial ([Lee and Lemieux, 2010](#)).

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 0.12-0.17 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 (i.e. magnitude equivalent to 0.13-0.39 standard deviations). Taken together, these results show that CRF recipients 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, 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 in the treatment group observed lower spreads at trade and higher trading volume relative to their counterparts in 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.

These results are robust to different assumptions on the bandwidth used to con-

struct the treatment and control groups, where the estimated policy effects were larger for the model with a narrower bandwidth, thereby suggesting that any potential extrapolation bias on the RDD is negative. I also find stronger effects when looking only at central county governments. This suggests that debt issued through county agencies, departments or authorities did not observed significant spillovers from this policy. This is consistent with a model where investor response to federal aid only extends to the direct recipient of the assistance.

To further examine this mechanism and study the role that state governments played in the distribution of CRF funding, as a robustness check I estimate the RDD excluding all county governments that received indirect CRF payments (i.e. when state governments transfer part of their CRF allocation to their county governments). The results from this analysis show smaller to null effects in primary market spreads, although with an increase between 13 and 18 basis points on secondary market spreads. At first sight, this could suggest that effects on primary market issuance were amplified by these indirect payments, thus highlighting how the interaction between federal and state policies could lead to improved outcomes in the bond market. Moreover, it aligns with a model where investors interpreted federal aid as a potential signal of expected larger economic dislocations.

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

analysis 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 analysis provides some suggestive evidence on a level of 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.<sup>1</sup> 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 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 (0.20 standard deviations) larger for CRF recipients.

These findings align with previous literature documenting positive effects of the announcement and implementation of the CARES Act and the Municipal Liquidity Facility on local government borrowing (Bi and Marsh, 2020; Haughwout et al., 2022b; Johnson et al., 2021). The results from this study document smaller effects on borrowing costs, potentially driven by the fact that this analysis looks at longer-

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<sup>1</sup>As it is described in Table 3 and Figure B.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.

term outcomes, whereas previous literature mainly focused on the immediate effects of the policy. The findings of this paper bring new evidence on how federal assistance could influence local borrowing long after its received by distressed governments.

This paper contributes to the public finance literature in three main ways. First, it adds to the literature on fiscal distress and borrowing costs ([Benson and Marks, 2007](#); [Johnson and Kriz, 2005](#); [Poterba and Rueben, 1997, 2001](#)) by documenting heterogeneity on local debt management during the most recent macroeconomic crisis. Second, it adds to the growing literature on the effects of federal policies enacted during the COVID-19 pandemic on state and local finances ([Gordon et al., 2020](#); [Green and Loualiche, 2021](#)) and on the municipal debt markets ([Bi and Marsh, 2020](#); [Fritsch et al., 2021](#); [Haughwout et al., 2022a](#); [Johnson et al., 2021](#)). Third, it contributes to the policy evaluation research by bringing fresh insights on the effectiveness of the federal policies implemented during the COVID-19 pandemic to mitigate the negative outcomes induced by the crisis.

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 4 describes the main data sources while Section 5 presents the results from the descriptive analysis. Section 6 details the implementation of the regression discontinuity and section 7 goes through the empirical results on RDD. Section 7.1 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

This section reviews some of the relevant literature on local governments response to fiscal distress due to macroeconomic crises and the extent to which such distress could be alleviated by federal support, as well as some of the recent literature on state and local government finances during the COVID-19 pandemic.

Macroeconomic shocks often translate into fiscal distress for state and local governments both through reductions in tax revenues due to the contraction of economic activity and increasing spending pressures in response to the slowdown ([Cromwell and Ihlanfeldt, 2015](#)). These shocks not only hinder governments ability to provide essential public services, but also translate into increased borrowing costs when accessing the debt market ([Kriz, 2004](#); [Poterba and Rueben, 2001](#)). Moreover, the severity of these financial market consequences depends on the strength of issuer's finances coming into the crisis, as well as investor perceptions on the effects of the shock and the outlook for recovery ([Cohen, 1989](#); [Kahle and Stulz, 2013](#)).

The effects on the market could be traced back to specific factors from both the supply and demand for municipal debt. From the issuer standpoint, when tax increases or spending cutbacks are not available, fiscal distress increases the incentives for deficit spending to cover fiscal gaps and counter-cyclical measures. However,



as implied by the market discipline hypothesis, these incentives are constrained by market forces through higher interest rates (Bayoumi et al., 1995; Goldstein and Woglom, 1991). From the investor standpoint, hikes on borrowing costs due to the crisis can only be explained by risk-averse investors (Kriz, 2004; Kriz and Wang, 2016). Moreover, if the crisis affects different sectors of the financial market, it could create fly-to-safety incentives that trigger a portfolio reallocation that could influence the demand for municipal debt. In this sense, a general concern around crisis episodes is whether the conditions on financial markets allow state and local governments to issue debt proportional to the extent of their fiscal needs.

For instance, in spite of the turmoil experienced in the municipal securities market during the Great Recession, state and local governments continued to borrow markedly, possibly motivated by the heightened fiscal stress experienced by governments (Fisher and Wassmer, 2014). One of the main explanations behind this phenomenon is the role that federal support played supporting municipal borrowing, in particular through the Build America Bonds (BAB) program which boosted the demand for municipal bonds through a direct subsidy equivalent to 35% of the bond coupon (Luby, 2012).

The evidence towards the benefits of the BAB program underline the relevance that federal policies have shaping the outcomes on the municipal market during episodes of distress. BAB bonds were adopted by almost every state government, capturing a relevant share of the market (16% in 2009 and 27% in 2010), and observing lower borrowing costs (Ang et al., 2010; Cestau et al., 2013; Liu and Denison,

2014; Luby, 2012). Nonetheless, this does not imply that federal intervention will always lead to improved outcomes on the municipal bond market. For instance, investors could interpret federal aid to distressed governments as either a signal of reduced liquidity pressures or as a harbinger of larger economic dislocations if such aid is allocated proportional to the expected magnitude of the shock. Hence, policy design plays a crucial role determining the effectiveness of federal interventions.

The literature on the economic effects of the COVID-19 pandemic provides some recent evidence both on state and local governments' experiences during macroeconomic crises, as well as the role that federal intervention plays on the municipal market. Early research suggested that the massive contraction in economic activity derived from the lockdown would likely translate into a significant reduction in fiscal revenues. Gordon et al. (2020) 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 when compared to the past financial crisis. In particular, governments whose tax bases relied more on the consumption of health care, restaurants, entertainment, and lodgings were more likely to experience more pronounced revenue gaps (Clemens and Veuger, 2020). 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.

Notwithstanding the generalized turmoil experienced at the onset of the pandemic (Baker et al., 2020; Haddad et al., 2021), state and local governments' debt issuance sustained without significant disruptions (Gillers, 2021). Similar to the findings by

[Fisher and Wassmer \(2014\)](#), this could be explained by the magnitude of the fiscal distress experienced by state and local governments.

Unlike the response to the Great Recession, during the COVID-19 pandemic the federal intervention did not target the demand-side of the primary market. In contrast, most policies provided direct aid to state and local governments via intergovernmental transfers through the CARES Act, and lender-of-last-resort mechanisms through the Municipal Liquidity Facility (MLF). Recent literature looking at the effects of the MLF shows that this intervention brought calm to the market by easing the liquidity risk concerns among investors ([Bi and Marsh, 2020](#)), which further translated into lower borrowing costs ([Bordo and Duca, 2021](#); [Fritsch et al., 2021](#); [Li and Lu, 2020](#)). [Bi and Marsh \(2020\)](#) documents reduction in municipal bond spreads associated with the different announcements of federal interventions. Their study found an average decrease of 67 basis points following the announcement of the CARES Act. Moreover, they document larger reductions for short-term securities relative to long-term bonds.

However, some of these studies show that these borrowing costs reductions were mild to null. For instance, [Johnson et al. \(2021\)](#) estimated a difference-in-differences model comparing the borrowing costs of municipal governments, conditional on MLF eligibility. This study finds no significant effects of the MLF in borrowing costs in the primary market in the first quarter following the implementation of the policy. [Haughwout et al. \(2022b\)](#) 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, this paper estimates an 8% increase on the probability of issuing primary market debt associated with MLF eligibility.

Most of the academic research has focused on the MLF with only few studies looking at local government reactions to the CARES Act. [Green and Loualiche \(2021\)](#) stands out by its examination of the impact of CARES Act assistance on the state government labor force. Using an instrumental variable approach, authors estimated that assistance through the CRF to state governments prevented more than 400 thousand layoffs in April 2020, and protected approximately one million job-months for state and local governments through August 2020. Both [Haughwout et al. \(2022b\)](#) and [Bi and Marsh \(2020\)](#) provide robustness checks on their analyses looking at heterogeneity driven by the enactment of the CARES Act and the MLF. For example, [Haughwout et al. \(2022b\)](#) estimates effects of the announcement of these policies on employment, showing that the CARES aid had a larger influence in hiring and recall decisions than the MLF. Authors interpret this as evidence that CARES aid played a larger role in alleviating the distress on the economy, whereas the effects of the MLF were more salient on financial markets.

This paper contributes to filling this gap in the literature by directly testing the effects of the CRF on local government borrowing. The findings from this analysis shed some light on the role of federal aid as a credit enhancement for local gov-

ernments, and the extent to which it leads to crowd-in/crowd-out effects on local borrowing during macroeconomic crises. The following sections describe the characteristics of this policy, as well as some descriptive evidence that motivates the empirical analysis.

### **3 Policy Setting: Coronavirus Relief Fund**

On March 13, 2020, President Trump 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 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 was signed into law couple days later (i.e. March 27, 2020).

The CARES Act provided over \$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 to provide direct aid to state and local governments.

The CRF received an allocation of \$150 billion, where \$139 billion went for state and municipal governments, \$8 billion for tribal governments and \$3 billion for ter-

ritories. Allocations across state governments were 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 et al., 2020). 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 with population above 500,000 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.

Since most receiving local governments are counties, this paper focuses on the aid provided to such governments. To provide some context around the payments pro-

vided to county governments through this program, the CRF provided payments to 118 counties across 32 states. Forty-five percent 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 on 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.<sup>2</sup> While not providing unlimited discretion on the use of CRF funds, the rules did provide some discretion to governments on how to allocate the aid within their local budgets. This 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.

Even with this discretion, in a mid-August 2020 survey conducted by the Government Finance Officers Association (GFOA) CRF primary recipients reported that

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<sup>2</sup>At the outset, the CRF set December 2020 as the termination date. However, due to the ongoing health emergency, it was initially extended to December 2021 (in the Consolidated Appropriations Act) and subsequently extended to December 2022.

the restrictions on the use of CRF funds and the lack of guidance from the Treasury were among the main challenges faced by recipient governments. Furthermore, there were relevant concerns on whether the federal aid provided would be enough to cope with the crisis ([Haroon, 2020](#)). Lack of clarity on the rules established by the Treasury could translate into delayed spending of these funds, or into a sub-optimal allocation of funds within counties budgets, hence undermining the effectiveness of these transfers.

In response, the Treasury updated the CRF guidance 4 times (April, June, July and September 2020), where each time it improved the clarity on which expenses could be covered with these funds, thus dissipating the initial uncertainty around using the aid to cover payroll and public employees benefits ([Haroon, 2020](#)).

Despite these challenges, the funds were spent relatively fast. Data from the Treasury Office of Inspector General (OIG) shows that by September 30, 2020, 93.7% of the \$150 billion was already spent, thus indicating both the magnitude of local government needs, as well as their ability to map these resources into expenses that complied with Treasury requirements.

## 4 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.<sup>3</sup> 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 2021 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 computed as the difference between municipal yields and treasury yields for instruments of equivalent maturity at the date of issue or trade. Data on the daily treasury par yield rates is retrieved from the U.S. Treasury website. This provides 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 in U.S. interest rates. 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

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<sup>3</sup>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.

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 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<sup>4</sup>. 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 gov-

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<sup>4</sup>Source: <https://home.treasury.gov/system/files/136/Payments-to-States-and-Units-of-Local-Government.pdf>

ernment issuers from county agencies.<sup>5</sup> 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 A.1). Moreover, I exclude from the analysis outlier observations on the dependent variables by removing the top and bottom 0.5% of the sample.

## 5 Descriptive Analysis

### 5.1 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

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<sup>5</sup>Considering the rating assigned by Fitch, Standard and Poor's, and Moody's I first generated a continuous variable 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.

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.<sup>6</sup> 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) in the treatment group and 50 counties (60 distinct issuers) in the control group.

A benefit of the methodology advanced by [Imbens and Kalyanaraman \(2012\)](#) and [Calonico et al. \(2014\)](#) is that it 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 omitted variable bias concerns and reduces the comparability among coefficient estimates if several governments enter/exit the analysis at different econometric specifications. Considering these caveats, the analyses presented in this paper are based only on the bonds issued by governments whose population is within a fixed and consistent bandwidth around the population cutoff for CRF eligibility.<sup>7</sup>

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<sup>6</sup>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.

<sup>7</sup>To be clear, the set of governments described above correspond to the baseline specification that looks 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 unique issuers) and the control group by 50 counties (124 unique issuers).

To be specific, throughout 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.

## 5.2 Pre-Intervention 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 in 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 in the control group. Moreover, the amount issued per-capita was \$2.5 lower for bonds in the treatment group. A similar story is documented for the secondary market where spreads and par traded per-capita were lower for the governments in the treatment group.

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 explores these differences and

shows no significant differences in 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 of the distribution of bonds issued in the primary market during the pre-intervention period by credit rating and years to maturity. The panel on the left depicts no significant differences on the maturity structure of CRF recipients and governments in 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 group 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 in the control group observed a relatively riskier profile than their counterparts in the treatment group.

In the online appendix, I expand this analysis to explore differences on the dependent variables used for the analysis during the post-intervention period. This descriptive evidence highlights some of the liquidity management strategies implemented by local governments in response to the pandemic and showcases the variation in municipal debt outcomes ought to be explained by the econometric model.

## 6 Empirical Strategy

To estimate the effect of direct federal aid on municipal debt I implement a sharp RDD that exploits the population criterion used by the U.S. Treasury to determine CRF eligibility. Equation 1 shows the statistical model of interest.

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

$y_{igst}$  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. I include vector  $X_{igst}$  that includes the control variables 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 simplify notation,  $b_t$  denotes month-by-year fixed-effects.

Estimation of the baseline specification 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.<sup>8</sup> 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.  $p = 1, 2$ ).

## 6.1 Threats to Validity

There are two major threats to validity in RDD that use population thresholds (Eggers et al., 2018). 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

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<sup>8</sup>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.



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. These results should be interpreted with caution when drawing policy implications on this regard. Results from this model could be driven by the effect on the 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 notes (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 longer-term debt instruments.

The second threat is strategic manipulation of population reports made by county officials. This concern relates with the validity of the continuity assumption required for identification. The threat would arise if county government officials in 2020 could alter their 2019 Census population estimates in order to gain CRF eligibility. In this sense, risks of sorting into any arm of the policy should be negligible. To address this concern, I present statistical evidence for lack of manipulation by running a [McCrary \(2008\)](#) test (see [Figure A.1](#) in the Appendix).

Since the baseline model aims to estimate the effects of direct CRF payments on municipal debt outcomes, a potential threat to validity stems from the presence of

indirect CRF payments that could contaminate both the treatment and control arms of the study. To address this concern, as part of the robustness checks I estimate the model excluding bonds issued by governments that received indirect CRF transfers from their state governments. In other words, restricting the comparison between counties with direct CRF aid and counties with no CRF funding at all.

## 6.2 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 the 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. \(2018\)](#). Failing to reject the null hypothesis of continuity at the cutoff provides evidence of a 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 A.1 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. Hence, the null hypothesis is not rejected and these results provide 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.<sup>9</sup>

## 7 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,

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<sup>9</sup>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.

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 0.12-0.17 of 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 per-capita, relative to the control group. To add some context to the magnitude of these estimates, they are within 0.13-0.39 standard deviations of this variable. These findings suggest that governments that were eligible to receive 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

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 in 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 2 shows the 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 financing 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 better pricing and larger issuance on the primary market.

## 7.1 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 four 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 represent a variation of (-36%, + 55%) relative to the baseline bandwidth. Tables A.3-A.4 in the appendix show the results from these analyses. Table A.3 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 0.22-0.43

standard deviations) and a larger increase in debt issuance (between \$2.0 and \$8.7 per-capita) associated with the policy. Results for secondary market outcomes are still mixed. Evidence from Table A.4 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 A.5 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 receive 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 0.46-0.51 the standard deviation. 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 A.6 in 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.

One of the main external validity limitations of the analyses presented in this



paper is that they only capture the direct effects of the CRF on county government borrowing. 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 in the control arm of the study receiving aid from their home states.

Using data from the Office of the Inspector General (OIG) from the Treasury I identify the county governments that received indirect CRF payments (i.e. made by states and large cities). In total these indirect transfers mounted to approximately \$6.9 billion, which is equivalent to one third of direct CRF payments received by these governments. Moreover, within the issuers considered for the baseline specification, the distribution of governments that received indirect payments is relatively balanced (49% for the treatment group and 60% for the control group) and, more importantly, the probability of receiving these payments is not significantly correlated with CRF status, thus ruling out concerns about systematic biases driven by the presence of this indirect CRF aid.<sup>10</sup>

As the fourth robustness check, I estimate the baseline specification excluding the bonds issued by governments that received indirect CRF payments from both arms of the study. Furthermore, I test the extent to which the results of this robustness check

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<sup>10</sup>To test this I estimate a linear probability model on receiving indirect CRF payments explained by counties population (i.e. the allocation rule for direct payments) and the coefficient for this variable is not statistically significant.

are driven by the bandwidth assumption by showing results using the two alternative bandwidths described above. Tables A.7 - A.9 show the coefficient estimates from this exercise. These results show no significant effects on primary market spreads and mixed evidence for debt issuance. However, it stands out that these models suggest mild increases in secondary market spreads between 13 and 18 basis points. This is consistent with a theory where federal aid signaled investors that recipient governments could experience larger economic dislocations due to the pandemic, thereby increasing the risk premiums observed by these bonds on the secondary market. Furthermore, it underlines the extent to which indirect CRF payments improved the access to the primary bond market for distressed counties.<sup>11</sup>

## 7.2 Heterogeneity by Credit Rating and Years to Maturity

To examine heterogeneity of the effect driven by credit rating and time to maturity, I extend the parametric model at Equation 1 to include their interactions with CRF eligibility. 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 5.

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<sup>11</sup>To address the potential contamination in the control group induced by the presence of indirect CRF payments I estimate the model excluding bonds issued by governments with indirect CRF payments in the control group, but preserving those issuers on the treatment group. In other words, this comparison is between governments that received some form of CRF aid (i.e. direct, indirect or both) and issuers with no CRF assistance. The results of this exercise (at Table A.10) are virtually similar to the baseline specification with reductions in primary market spreads up to 34 basis points, and larger per-capita debt issuance, however these estimates are not significant at traditional levels. Similarly, while estimates for the secondary market are mixed they point towards mild increases in secondary market spreads and larger trading volumes for CRF-recipients.

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

In this case, the coefficients of interest  $\theta_h$  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, on 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 \$10.2 and \$10.9 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 the CRF 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 0.20 the standard deviation of this variable on the sample.

Results for the coefficients on credit ratings show that lower rated bonds were benefited more 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.

### 7.3 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.

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

Unlike the previous models, this model estimated on the data includes both the pre-intervention and post-intervention periods.<sup>12</sup> 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 in the control group. Coefficients  $\theta_{\tau}$  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 a stacked

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<sup>12</sup>Since this model incorporates data from the pre-intervention period to the analysis, this leads to a different distribution 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).

estimation of  $t$  independent RDD 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 3 shows 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 B.1. 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.<sup>13</sup>

While the CRF was perhaps the first policy tool implemented by the US government to directly 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 (ARP) of 2021 stands out as it provided support to subnational governments through the Coronavirus State and Local Fiscal Recovery

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<sup>13</sup>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.

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 RDD to be confounded with the implementation of the ARP. 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 3 suggests that any potential spillovers from the ARP 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 (March 2021).

Following the tradition of difference-in-difference designs, I examine coefficients  $\theta_\tau$  during the pre-intervention period to assess the validity of the research design. As described above, if the model does not suffer from omitted variable bias, coefficient estimates for the pre-intervention period should be close to zero, thus suggesting no pre-existing differences on the outcome variables prior to the intervention and reinforcing the internal validity of the RDD.

Visual inspection of Figure 3 lends some evidence towards this point since these coefficients oscillated around zero across models. It should be noted that models with quadratic polynomial specifications are more sensitive, however while this phe-



nomenon is more salient for the primary market outcomes, results for secondary market outcomes are virtually equivalent across specifications.

As a final robustness test I falsify the null-hypothesis of these pre-intervention coefficients being jointly equal to zero using a Wald test. Albeit the results of these tests reject the null-hypothesis of joint nullity, an individual review of the statistical significance of each coefficient shows that, for the most part, these are indistinguishable from zero at traditional levels. 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.

## 8 Conclusions

This paper studies the role of federal assistance on local government borrowing during periods of economic distress. Evidence from U.S. county governments during the COVID-19 pandemic suggests that federal aid produced crowd-in effects for local governments that enabled the provision of local services, and served as a credit enhancement to municipal issuers. CRF recipient governments observed mild reductions in their borrowing costs and increased their debt issuance on the primary

market, with no significant effects documented for the secondary market. The conclusions drawn from the RDD are robust to alternative decisions on the bandwidth used as inclusion criterion for the analysis, thus alleviating extrapolation bias concerns on the results.

Evidence from the robustness checks highlights how the effect of federal aid on local government borrowing is moderated by the vertical and horizontal interactions among governments. Results from the estimation including only central county governments predict stronger crowd-in and credit-enhancement effects of the CRF on municipal debt outcomes. This suggest mild to none indirect effects of federal aid on the debt issued by government agencies with a budgetary relationship with the recipient government.

Similarly, the results from the analysis that excludes governments that received indirect CRF transfers from their state governments show no significant effects on the primary market, but documents mild increases in bond spreads on the secondary market. In other words, investors assigned larger risk premiums to bonds issued by CRF recipient governments. This is consistent with a model where federal aid signaled investors that recipient governments were more likely to experience a stronger economic shock due to the crisis. At the same time, these results underline the crucial role played by state government aid during the pandemic, as it enabled local governments to issue debt at lower borrowing costs.

The examination of the treatment effect heterogeneity highlights credit quality

as one of the main mechanisms driving the results. On 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. Similarly, this analysis documents fly-to-safety behavior from investors on the secondary market since the increase in the trading volume of longer term bonds issued by CRF recipients was significant at traditional levels. 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.

Given the relative fungibility of CRF funding, for governments within the bandwidth for CRF eligibility this grant transfer mimicked a random liquidity shock during a period of economic distress and heightened uncertainty. Crowd-in effects of federal aid on government borrowing are consistent with a model where governments prefer to manage cash-flows via the financial markets rather than tapping into their cash reserves, when liquidity constraints become more stringent. This could be explained by governments incentives to preserve their creditworthiness and keep their borrowing costs stable ([Marlowe, 2011](#)). In this sense, results from this paper provide new evidence on the response of government borrowing to changes in their capitalization levels.

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 on 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 what 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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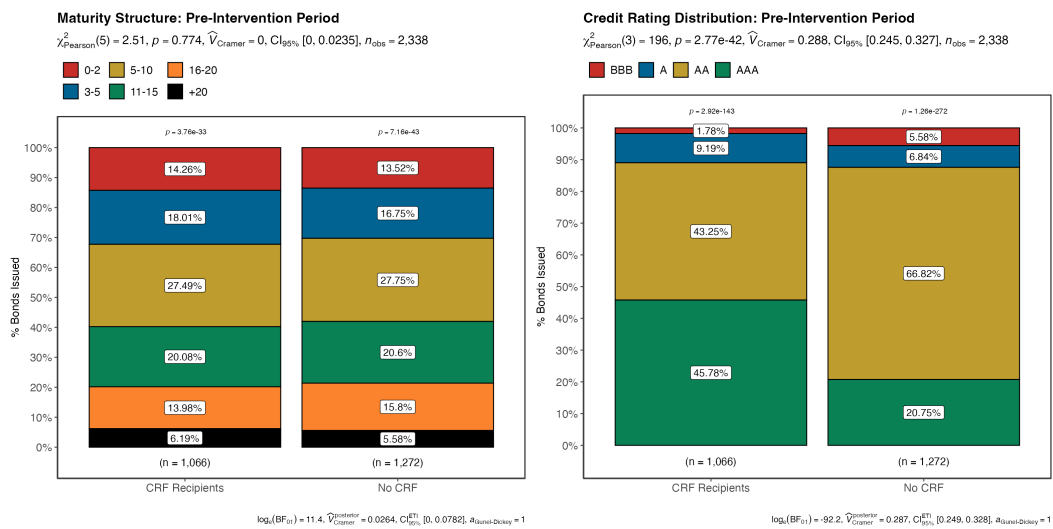
## 9 Tables and Figures

**Table 1:** Balance Table: Dependent and Independent Variables.

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

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



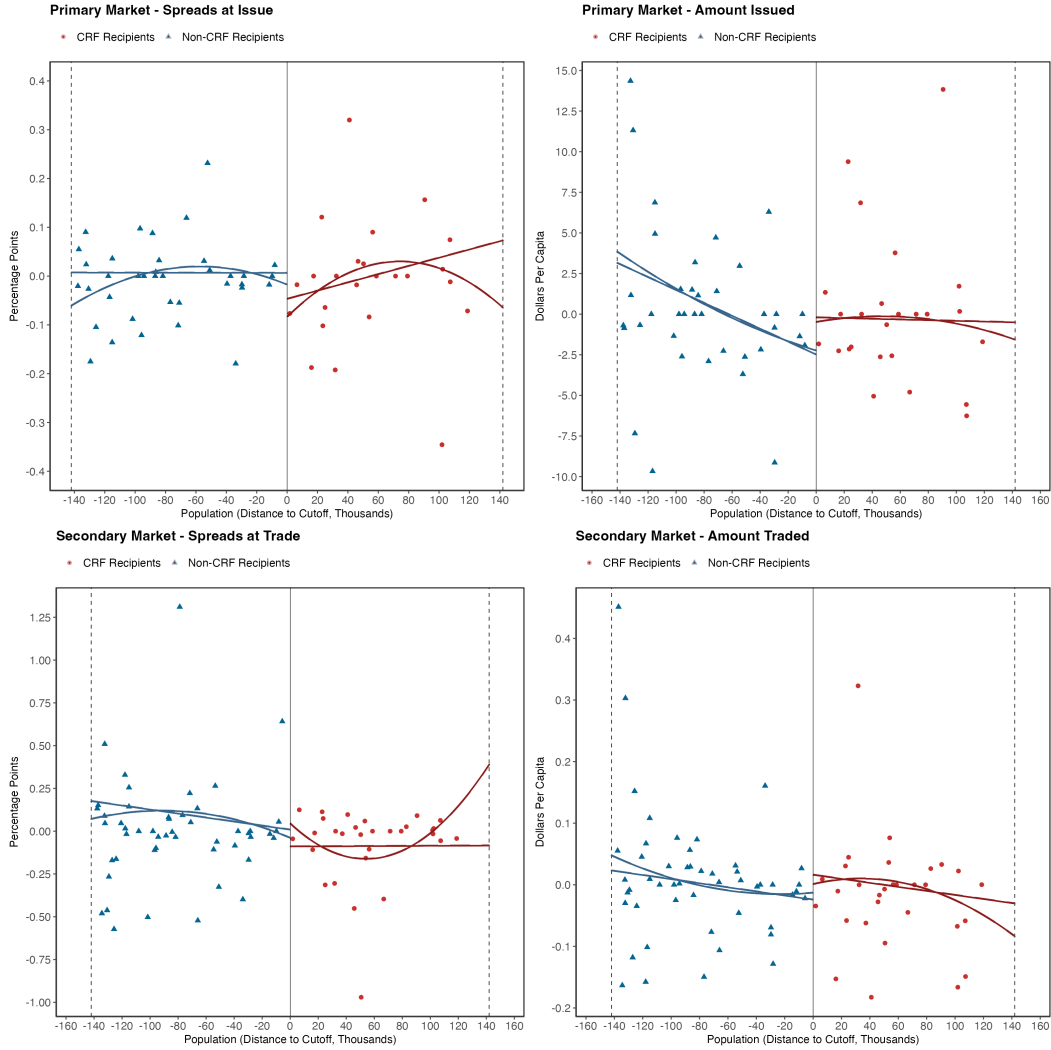
**Notes:** These panels compare bond issues by governments on the treat and control groups during the pre-treatment period. The bar-plots compare the distribution of bonds issued by maturity and credit rating between the treatment and control groups. Pearson statistic and corresponding p-value correspond 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.

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

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric</b>				
Linear	-0.066** (0.0297)	1.751** (0.7711)	0.0857*** (0.0106)	0.0139 (0.0109)
Quadratic	-0.4711** (0.1887)	-10.0827 (7.0314)	-2.6375*** (0.0721)	-0.2932*** (0.072)
<b>Panel B: Parametric Estimation</b>				
Linear	-0.0913 (0.0553)	5.0732** (2.0702)	-0.4129 (0.3179)	0.074* (0.043)
Quadratic	-0.0907 (0.0579)	4.8842** (2.0338)	-0.4045 (0.3115)	0.0736* (0.0429)
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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Figure 2:** 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.

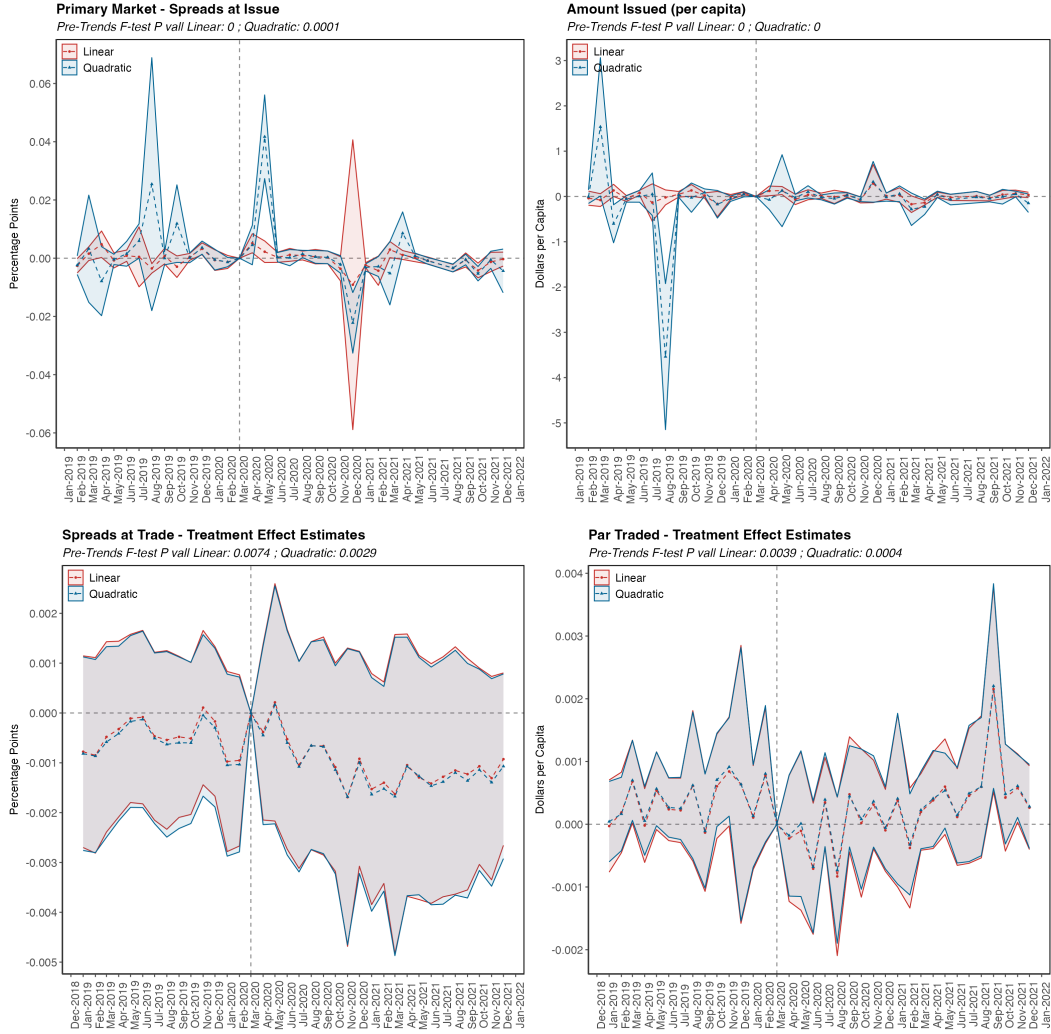
**Table 3:** Treatment Effect Heterogeneity by Credit Rating and Years to Maturity

Variable	Spread (1)	Spread (2)	Amount (1)	Amount (2)
<b>Panel A: PM-Years to Maturity</b>				
3-5	-0.0112 (0.032)	-0.0086 (0.0327)	0.9771 (2.2011)	1.0442 (2.1798)
5-10	0.0298 (0.0605)	0.032 (0.0606)	0.7753 (2.1669)	0.8329 (2.1519)
11-15	-0.0201 (0.0859)	-0.0183 (0.0863)	0.1319 (2.2331)	0.1804 (2.2234)
16-20	-0.0841 (0.0936)	-0.0822 (0.0933)	-0.0978 (2.5196)	-0.0501 (2.5081)
+20	-0.193 (0.1304)	-0.1825 (0.1305)	-8.7971 (13.6596)	-8.5248 (13.5597)
<b>Panel B: PM-Credit Rating</b>				
AAA	-0.9599*** (0.1918)	-0.9813*** (0.2049)	10.7081** (4.7261)	10.8928** (4.657)
AA	-1.0689*** (0.1919)	-1.114*** (0.2124)	10.2448** (3.8895)	10.6344** (4.2342)
A	-0.968*** (0.2657)	-1.0174*** (0.2759)	8.0134 (5.8052)	8.4395 (5.7057)
<b>Panel C: SM-Years to Maturity</b>				
3-5	0.0116 (0.0297)	0.0037 (0.0297)	0.0005 (0.0143)	-0.0001 (0.0146)
5-10	-0.0999 (0.062)	-0.1073* (0.0633)	-0.0265 (0.0199)	-0.027 (0.0197)
11-15	-0.0013 (0.0579)	-0.008 (0.0589)	-0.032 (0.0388)	-0.0325 (0.0386)
16-20	-0.239 (0.2148)	-0.2543 (0.2197)	0.062 (0.0449)	0.0609 (0.0443)
+20	-0.2843 (0.2582)	-0.3057 (0.2642)	0.1435** (0.0704)	0.142** (0.0709)
<b>Panel D: SM-Credit Rating</b>				
AAA	-0.5049 (0.3888)	-0.4544 (0.4042)	-0.0309 (0.1304)	-0.0314 (0.1328)
AA	-0.5574 (0.4044)	-0.5568 (0.4066)	0.0253 (0.1214)	0.0253 (0.1214)
A	-0.6092 (0.4403)	-0.6151 (0.4421)	0.1825* (0.0922)	0.1825* (0.0921)
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  $\theta_s$  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. \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ .



Figure 3: Dynamic Treatment Effects



**Note:** These panels show the coefficient estimates  $\theta$  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.

# A Appendix

**Table A.1:** Descriptive Statistics

Variable	Mean	SD	Min	P25	P50	P75	Max	N
<b>Panel A: Primary Market</b>								
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
<b>Panel B: Secondary Market</b>								
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

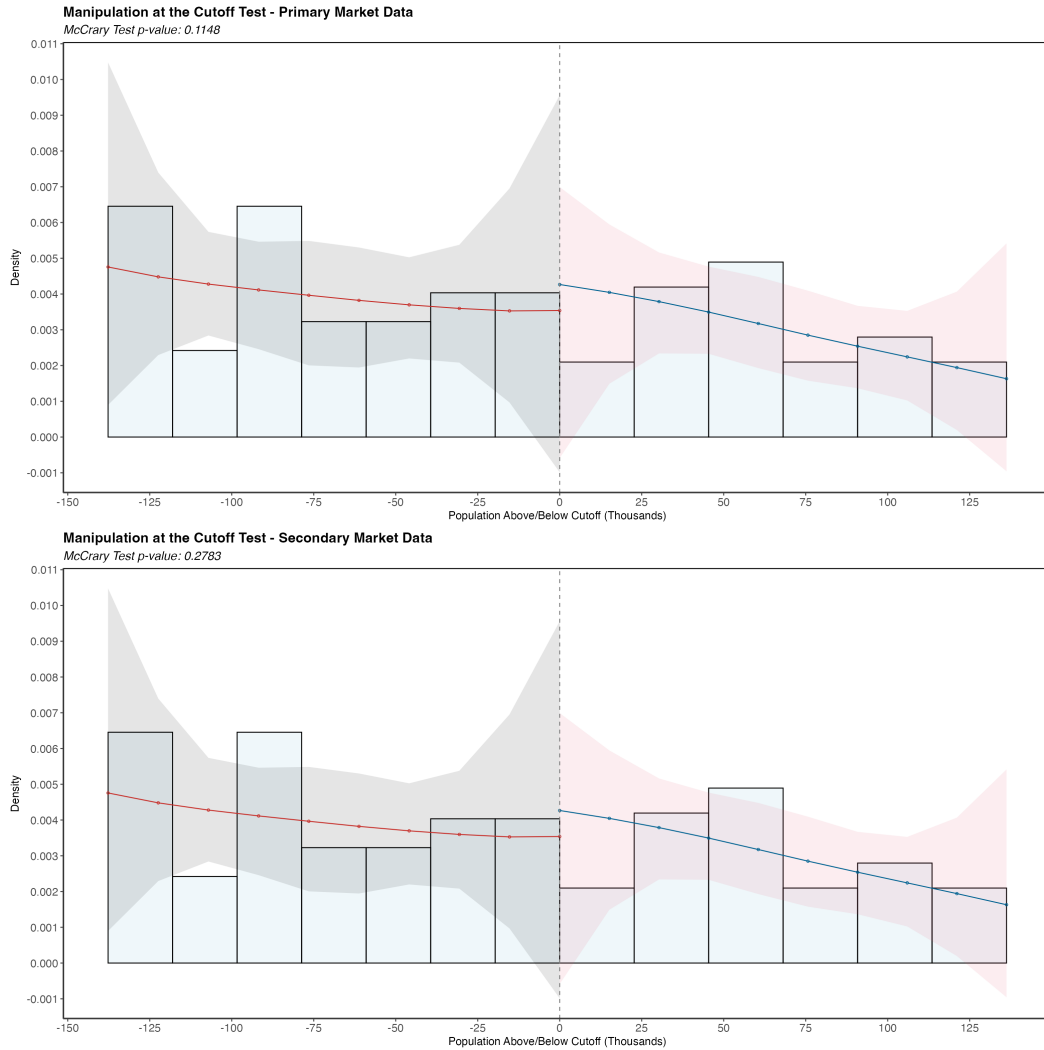
**Note:** This table shows the descriptive statistics of the samples used for the primary and secondary market analysis. Spreads, coupon rate, and the unemployment rate are expressed in percentage points and amounts (issued and traded) in dollars per capita. Offering Type, GO Bond and Central Government 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 A.2: 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. Louisiana, 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.

Figure A.1: 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.

**Table A.3:** LATE Estimates of the CRF on the Municipal Bond Market (Bandwidth = 90K)

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric</b>				
Linear	-0.122*** (0.0348)	2.0563* (0.8468)	-0.1936*** (0.013)	-0.0073 (0.0132)
Quadratic	-1.4567*** (0.4362)	-23.5114 (16.662)	1.8227*** (0.1221)	-0.5106*** (0.1073)
<b>Panel B: Parametric</b>				
Linear	-0.1858 (0.1026)	8.763* (3.8046)	0.1468 (0.2258)	0.0783 (0.0547)
Quadratic	-0.2326* (0.1019)	7.1787** (2.6133)	0.1369 (0.2274)	0.0799 (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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.4:** LATE Estimates of the CRF on the Municipal Bond Market (Bandwidth = 221K)

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric</b>				
Linear	-0.0727* (0.029)	0.9516 (0.7716)	0.0778*** (0.0105)	0.0093 (0.0108)
Quadratic	-0.4514* (0.1849)	-7.5199 (7.0466)	-3.1384*** (0.0712)	-0.2907*** (0.0696)
<b>Panel B: Parametric</b>				
Linear	-0.0913 (0.0553)	5.0732* (2.0702)	-0.4154 (0.3178)	0.0744 (0.043)
Quadratic	-0.0907 (0.0579)	4.8842* (2.0338)	-0.4084 (0.3122)	0.0742 (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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.5:** LATE Estimates of the CRF on the Municipal Bond Market - Only Central County Governments

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric</b>				
Linear	-0.0305 (0.0378)	-1.0945 (1.0154)	-0.231*** (0.0127)	-0.0467** (0.0182)
Quadratic	-0.3976 (0.2672)	-4.316 (8.7396)	-2.0399*** (0.089)	-0.4323*** (0.1059)
<b>Panel B: Parametric Estimation</b>				
Linear	-0.2346** (0.1112)	3.2395 (4.6124)	-0.584* (0.3135)	0.0938 (0.0663)
Quadratic	-0.2584** (0.0966)	2.4895 (4.6091)	-0.5356** (0.2674)	0.0875 (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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.6:** Robustness Checks: Placebo Estimates on the LATE

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric</b>				
Linear	-0.029 (0.0324)	1.4842 (0.9819)	0.1315*** (0.0129)	0.0265** (0.0116)
Quadratic	-0.2298 (0.1992)	10.7008 (7.6214)	-0.5157*** (0.0791)	-0.293*** (0.0805)
<b>Panel B: Parametric Estimation</b>				
Linear	-0.0949 (0.0859)	4.9162** (2.4537)	0.0145 (0.0922)	0.055 (0.0565)
Quadratic	-0.0935 (0.0836)	5.0143* (2.5278)	0.0205 (0.0894)	0.049 (0.0546)
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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .



**Table A.7:** LATE Estimates of the CRF on the Municipal Bond Market - Excluding Indirect CRF Recipients

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric Estimation</b>				
Linear	0.0001 (0.0334)	2.0262** (0.8875)	0.1687*** (0.012)	0.0027 (0.016)
Quadratic	0.5054 (0.3094)	-24.3275** (11.2658)	-1.2404*** (0.1079)	-0.7887*** (0.1503)
<b>Panel B: Parametric Estimation</b>				
Linear	-0.172 (0.1622)	2.3675 (5.6895)	0.1119 (0.1172)	0.2388* (0.1196)
Quadratic	-0.2383 (0.1703)	4.5635 (6.1778)	0.164 (0.1097)	0.2225* (0.1125)
Mean Dep Var	0.3909	7.9781	0.4502	0.2795
SD Dep Var	0.5197	12.8112	0.8685	0.8622
Obs (Left Cutoff)	758	758	43043	43043
Obs (Right Cutoff)	701	701	45609	45609

**Note:** This table shows the coefficient estimates of the Local Average Treatment Effect for the dependent variables of interest on the sample of bonds excluding indirect CRF recipients from both the treatment and control arms of the study. 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.8:** LATE Estimates of the CRF on the Municipal Bond Market - Excluding Indirect CRF Recipients (Bandwidth = 90K)

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric Estimation</b>				
Linear	-0.0029 (0.0416)	-0.3027 (1.1194)	0.1318*** (0.0142)	-0.0608*** (0.0209)
Quadratic	-1.3837 (1.382)	-2.8847 (65.157)	3.3754*** (0.4399)	-3.1102*** (0.7688)
<b>Panel B: Parametric Estimation</b>				
Linear	-0.0113 (0.1876)	9.368 (5.6185)	0.1834** (0.0835)	0.1667* (0.0897)
Quadratic	-0.027 (0.1467)	8.7181** (3.6627)	0.1831** (0.0801)	0.1768* (0.0894)
Mean Dep Var	0.4163	7.9846	0.4444	0.2757
SD Dep Var	0.5318	12.6195	0.852	0.8452
Obs (Left Cutoff)	589	589	32432	32432
Obs (Right Cutoff)	672	672	40016	40016

**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, but excluding indirect CRF recipients from both the treatment and control arms of the study. 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.9:** LATE Estimates of the CRF on the Municipal Bond Market - Excluding Indirect CRF Recipients (Bandwidth = 221K)

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric Estimation</b>				
Linear	-0.0065 (0.0339)	0.8873 (0.9083)	0.1687*** (0.012)	0.0027 (0.016)
Quadratic	0.1297 (0.3116)	-32.3242*** (11.5278)	-1.2404*** (0.1079)	-0.7887*** (0.1503)
<b>Panel B: Parametric Estimation</b>				
Linear	-0.172 (0.1622)	2.3675 (5.6895)	0.1119 (0.1172)	0.2388* (0.1196)
Quadratic	-0.2383 (0.1703)	4.5635 (6.1778)	0.164 (0.1097)	0.2225* (0.1125)
Mean Dep Var	0.3933	7.3309	0.4502	0.2795
SD Dep Var	0.5391	12.0221	0.8685	0.8622
Obs (Left Cutoff)	1548	1548	43043	43043
Obs (Right Cutoff)	758	758	45609	45609

**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, but excluding indirect CRF recipients from both the treatment and control arms of the study. 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.10:** LATE Estimates of the CRF on the Municipal Bond Market - Excluding Indirect CRF Recipients from Control Group

Model	Spread Issue	Amount Issued	Spread Trade	Amount Traded
<b>Panel A: Non-Parametric Estimation</b>				
Linear	-0.0053 (0.0351)	4.8284*** (0.9478)	0.1986*** (0.0111)	0.046*** (0.0157)
Quadratic	-0.3401 (0.3252)	-51.4345*** (11.2314)	-1.7455*** (0.1072)	-0.7989*** (0.1484)
<b>Panel B: Parametric Estimation</b>				
Linear	-0.1379 (0.1174)	3.5336 (2.6517)	0.0123 (0.0845)	0.1015 (0.0764)
Quadratic	-0.1461 (0.1101)	3.0528 (3.5678)	0.0077 (0.0992)	0.1728** (0.0765)
Mean Dep Var	0.3811	6.7543	0.4429	0.2619
SD Dep Var	0.5332	12.5162	0.8478	0.8237
Obs (Left Cutoff)	758	758	43043	43043
Obs (Right Cutoff)	1549	1549	88145	88145

**Note:** This table shows the coefficient estimates of the Local Average Treatment Effect for the dependent variables of interest on the sample of bonds excluding indirect CRF recipients only from the control arm of the study. 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

## B Online Appendix

### B.1 Post-Intervention Comparison

Figure B.1 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 (i.e., 15 months after the peak, for both primary and secondary markets).

Despite the spike on primary and secondary 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. The primary market spreads for the treatment group demonstrated less variation in comparison to the control group. This is evidenced by the more compact area bounded by the inter-quartile range. Conversely, for the secondary market, the average spreads for the treatment group were lower than their counterparts in the control group and also exhibited less variation.

Panels on the right of Figure B.1 show the amounts of debt issued and traded on the primary and secondary markets at par value, respectively. A visual inspection

of both graphs suggests no clear trends neither on the pre-intervention or post-intervention periods. However, the distribution of the par-traded (per capita) on the secondary market exhibits a notable skewness. The average par traded for both groups is significantly above the interquartile range. This implies that the distribution of secondary market trades might contain outlier observations of large trades which are pulling the average outside the interquartile range.

The right-hand side of Panel B in Table 1 shows that differences in bond characteristics 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.

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

bonds from the governments on the control group. Moreover, 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 in 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 B.2 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 provide 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 B.3 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 re-



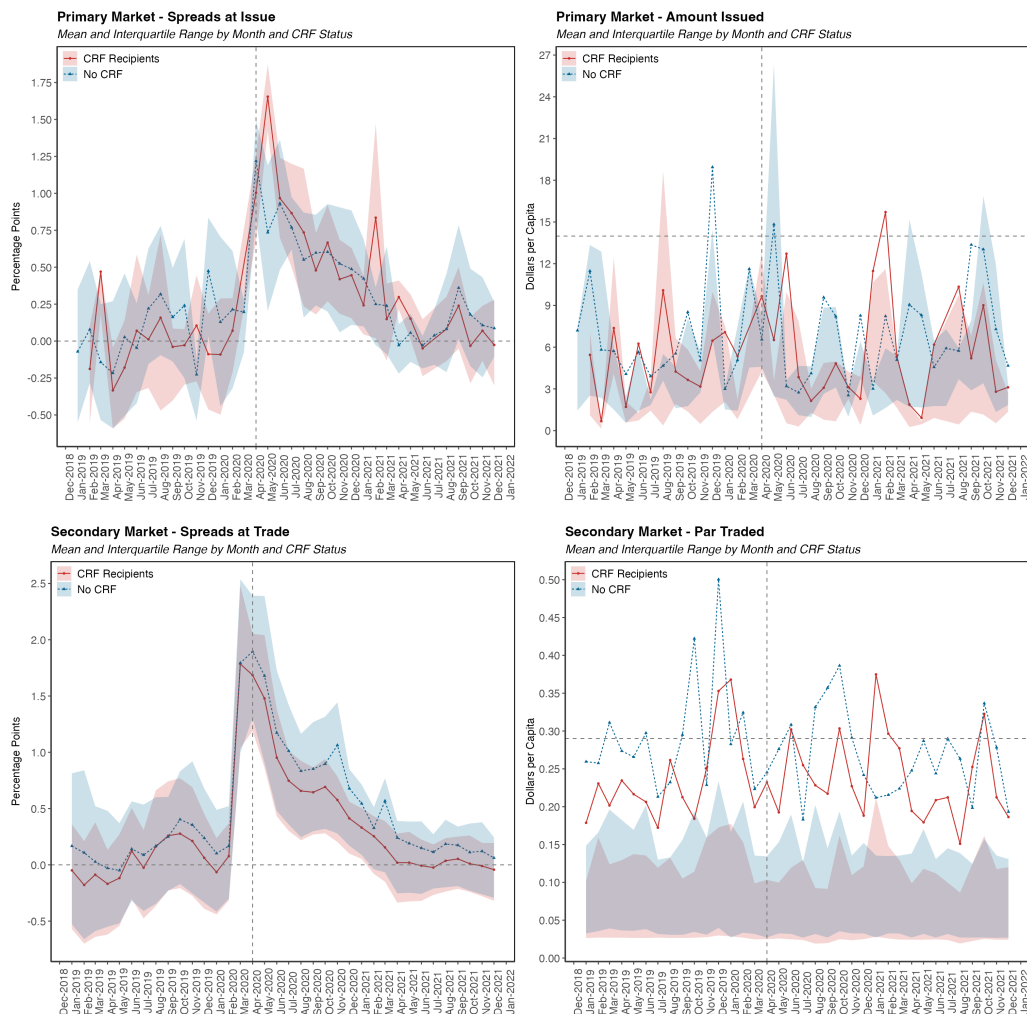
duction on the issuance of longer-term bonds. However, these differences were more significant for governments on the control group (i.e. chi-squared test  $p < 0.001$ ), relative to the treatment group (i.e. chi-squared test  $p = 0.1$ ). At first sight, these results suggest that while governments 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.

This analysis provides some evidence of 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. Despite governments in the treatment group possessing higher credit quality at the start of the pandemic, during the post-intervention period they observed a deterioration on their average credit quality. This is 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 countries, 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 cost decreases and, at the same time, increased their per-capita debt issuance.

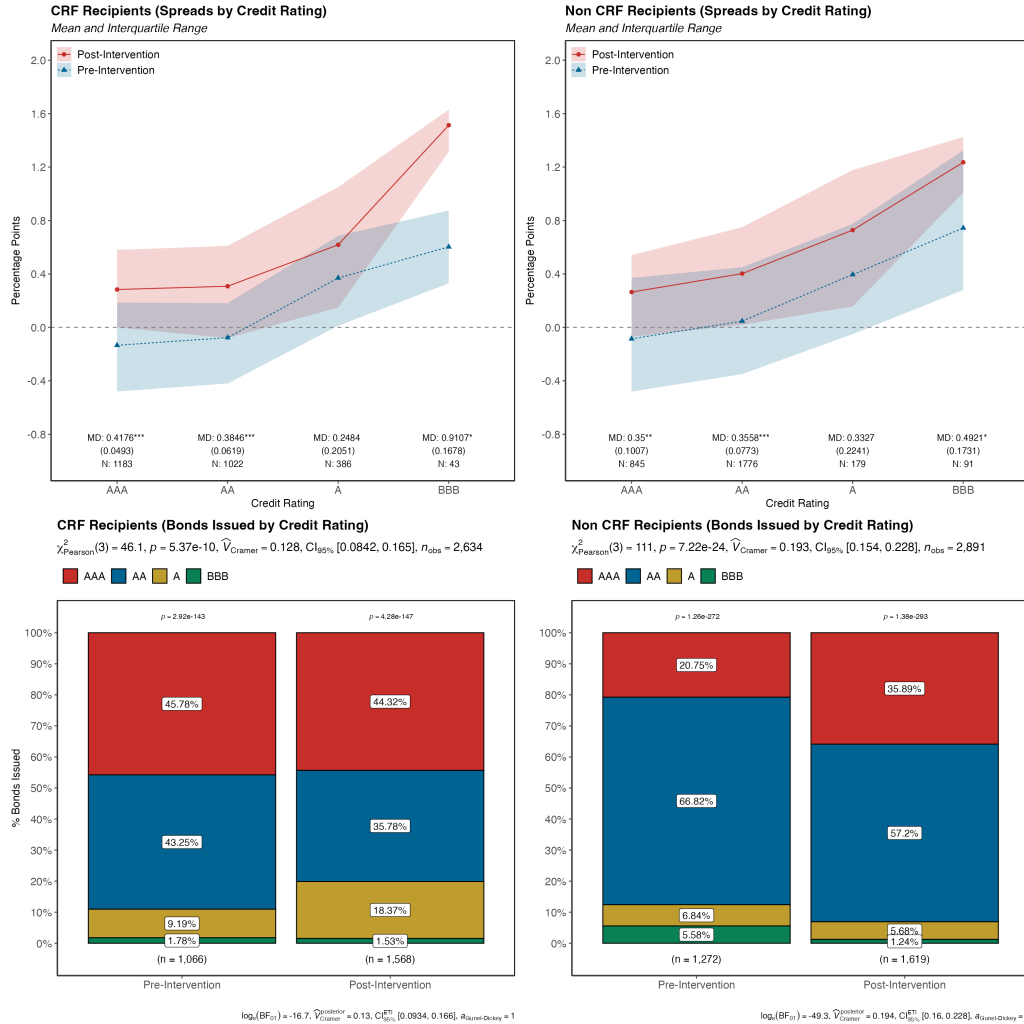
This descriptive evidence shows that governments increased their reliance on shorter-term bonds, at the expense of reducing longer-term debt issuance. This increased was larger for governments in the control group and they documented a statistically significant change on the bond distribution across the yield curve between the pre and post-intervention periods (see [Figure B.3](#)).

**Figure B.1: Primary Market Spreads by Treatment Status during the Analysis Horizon**



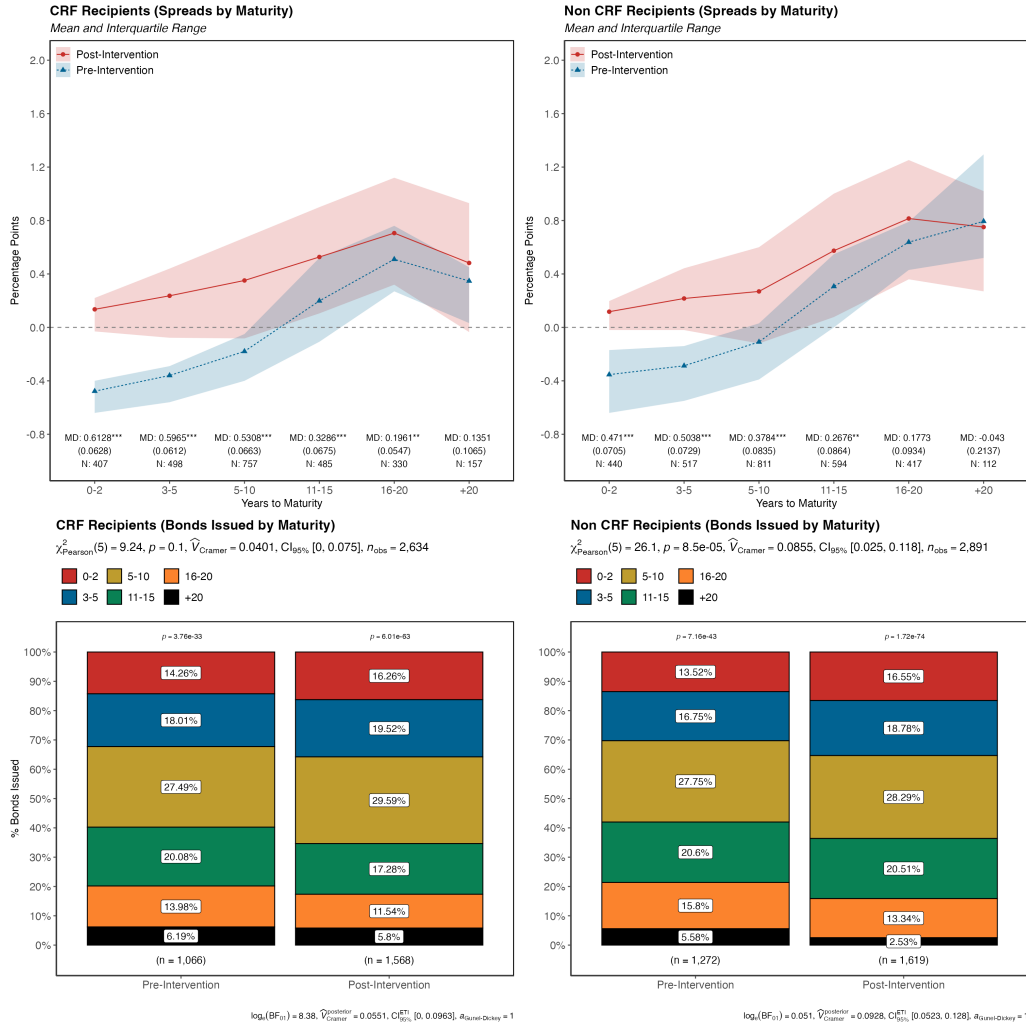
**Notes:** This graph shows the distribution of each dependent variable for each month between Jan-2019 and Dec-2021. The lines show the average for both treatment and control groups. The shaded areas show the inter-quartile range (i.e. distribution between the 25th and the 75th percentiles). Vertical 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.

**Figure B.2:** 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 are 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 B.3: 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.