The Option Value of Municipal Liquidity: Evidence from Federal Lending Cutoffs during COVID-19*

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Abstract

We estimate the option value of municipal liquidity by studying bond market activity and public sector hiring decisions when government budgets are severely distressed. Using a regression discontinuity (RD) design, we exploit lending eligibility population cutoffs introduced by the Federal sector's Municipal Liquidity Facility (MLF) to study the effects of an emergency liquidity option on yields, primary debt issuance, and public sector employment. We find that while the announcement of the liquidity option improved overall municipal bond market functioning, *lower-rated* issuers additionally benefited from direct access to the facility—their bonds traded at higher prices and were issued more frequently, suggesting a potential credit-risk sharing channel on top of the Fed's role as liquidity-provider of last resort. Local governments, by contrast, responded to emergency liquidity measures by recalling a greater share of service-providing government employees (mostly educational institution workers), but recalls were only sustained for *higher-rated* municipalities. This hiring responsiveness is consistent with the view that large government furloughs might have over-weighted the worst possible outcomes based on past experience. Together, the results suggest that municipalities would likely have been more distressed absent the emergency liquidity.

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1 Introduction

The State and Local sector is the dominant government service provider in the US, employing nearly 20 million workers. State and local government activity directly contributed 10.9 percent of GDP in 2019, well above the federal sector's direct contribution of 6.6 percent (BEA, 2020). A long-standing debate in public and macroeconomics concerns the optimal level of debt with respect to growth rates, yet less is known about the extent to which municipalities are liquidity constrained and thus what the value of lifting such constraints would entail for welfare, especially in periods of fiscal distress.

Particularly important to municipal functioning but often overlooked is the short-term municipal bond market, which can serve as a clearing house when expenditures and revenue receipts are temporarily misaligned. This happens, for example, when tax payments arrive on specific dates (e.g. quarterly), while payroll and other expenses are typically more continuous. In these circumstances, municipalities access short term funding markets to allow them to maintain smooth spending paths, secured with future revenue. These smoother municipal funding paths have at least two potential real economy benefits: they may curtail pro-cyclical fiscal policies that result in layoffs or furloughs; they can also support positive externalities emanating from high fiscal multipliers (Chodorow-Reich (2019), Yi (2020)) or the municipality's unique role as coordinating agent—especially salient after a natural disaster or health crisis when effective emergency services (such as hospitals reliant on public revenue) can reduce the duration of economic downturns (Auerbach et al. (2020)).

In this paper, we estimate the option value of municipal liquidity by studying bond market behavior and public sector hiring decisions when government budgets are severely distressed. Using a regression discontinuity (RD) design, we exploit lending eligibility cutoffs introduced by the Federal sector's Municipal Liquidity Facility (MLF) in April 2020 that granted direct Federal Reserve lending eligibility (via a Special Purpose Vehicle backstopped by an initial US Treasury equity investment) to city issuers with populations greater than 250,000, and county issuers with populations greater than 500,000.[1] Combining the cross-sectional variation from the RD with the timing of the policy announcement helps us overcome the empirical challenge of separately identifying the effects of one Federal intervention (MLF) from the many others that were simultaneously implemented around this period.[2] One such

[1]These cutoffs announced on April 27, 2020, expanded eligibility beyond the original April 9, 2020 cutoffs of 1m and 2m for cities and counties respectively, granting access to most but not all issuers (see Section 2 for further details). We call this the "option value" because despite finding large effects on yields, only two issuers actually made use of the facility from its inception through December 2020. In this sense, the RD can be thought of as more of an intent-to-treat estimator where the liquidity option is imperfectly taken up but the MLF option is fully salient.

[2]For example, on April 9th, the MLF was announced as part of a broader $2.3 trillion package including support for the Payroll Protection Program (PPP), Main Street Lending program, expansions to corporate credit facilities (PMCCF and SMCCF) as well as the Term Asset-Backed Securities Loan Facility (TALF). This underscores the difficulties in using timing alone to isolate the effects of the MLF, and the importance of using cross-sectional variation for identification.

confounder is the CARES Act Coronavirus Relief Fund (CRF) announced one month earlier which provided direct aid to cities and counties with populations over 500,000, thus sharing one of the two cutoffs used for MLF lending. The later timing of the MLF allows us to isolate effects separately from CRF aid, while the cross-sectional variation from the RD further minimizes selection concerns that would arise from comparing all eligible versus non-eligible issuers (endogenous correlates of city size which may also predict municipal performance).

We first document that overall secondary market yields and primary issuance for the most part returned to normal market functioning following the totality of Federal interventions that were introduced between mid-March and the end of April 2020, while low-rated investment grade bonds remained relatively distressed. We then estimate the effect of differential MLF access using municipal bond yields as an investor-perceived valuation of access to emergency liquidity, allowing us to learn how the market assessed the marginal borrower's liquidity constraint. We find that low-rated (A and BBB) city and county issuers' bonds traded at higher prices immediately following facility access, but did not respond to direct CARES aid announced over a month earlier using a similar eligibility cutoff.

Low-rated issuers that were narrowly eligible for emergency lending exhibited a yields decline (demand increase) of roughly 72 basis points relative to observationally equivalent issuers that narrowly missed eligibility—a sizable magnitude that closely resembles the overall market spread between BBB yields and higher-rated issuers. These differences in investor perceptions also translated to a large differential increase in primary issuance among low-rated issuers, but new bonds were not issued directly through the MLF. That is, while there were too few facility users to test whether the MLF *directly* facilitated additional revenue smoothing among issuers, their ability to issue at favorable borrowing costs on private markets clearly improved with the facility *option*. These results imply that absent the MLF, issuers would have likely been constrained in their ability to issue new debt throughout the crisis.

We then turn to real-side employment outcomes, and ask whether emergency liquidity induced local budget officers to retain more public sector employees or increase hours in light of mass furloughs and separations. We find that city and county governments recalled more service-providing government employees in response to the totality of both the MLF option and direct CARES aid, however decomposition tests suggest that hiring mostly responded to CARES aid and not the MLF option (consistent with recent work by Green and Loualiche (2020)). In our preferred specification, eligible issuers retained about 422 to 517 more local service-providing employees relative to observationally equivalent local governments in the two months following the cutoff announcement. On a baseline year-on-year decline of about 1,717 employees, this reflects a substantial recovery effect size of roughly 25% to 30%. Almost all of the retained service-providing employees were concentrated in *Education and*

Health Services, the majority of which is comprised of educational institution employees.[3] Consistent with priority hiring for education sector employees, year-on-year RD estimates are positive during lockdown months, revert to zero during summer months when school labor demand is low, with recalls sustaining beyond the summer for *high-rated* governments (the vast majority of issuers). That less fiscally constrained governments appear more elastic in hiring (especially while shutdowns were largely still in place) is consistent with a viewpoint that state and local governments may have over-weighted the worst possible outcomes based on past experience, furloughing education sector employees even though realized revenue shortfalls were far lower than originally anticipated.[4]

In the remainder of the paper, we probe why MLF access was perceived as more valuable lower down on the ratings distribution rather than viewed as neutral across the distribution, discuss implications for optimal policy. We use two further strategies to gauge the mechanism behind this result. We first estimate the probability that one of the three major nationally recognized statistical rating organizations (NRSROs) downgraded any of the issuer's bonds, and find modest evidence that downgrade probabilities differentially increased when the issuer was revealed as ineligible for facility access. Using a complementary asset price decomposition approach following Boyarchenko et al. (2020) and Schwert (2017), we also quantify the credit-risk channel over a broader set of issuers, which implies a non-trivial role for credit risk in determining yields during peak distress. These results suggest the presence of a potential credit-risk sharing channel beyond the Federal Reserve's role as liquidity provider of last resort—a willingness to credit-risk share even if not widely exercised in practice. Interestingly, though the facility terms target primary issuance of notes with maturities of up to 36 months, we find substantial impacts on long-term secondary market yields. One novel contribution of the paper is thus identifying a feedback channel between short-term primary issuance and long-term secondary market liquidity in the municipal sector, which to our knowledge, has not been previously documented. One example of how this could happen is short-term notes could be applied to long-term general obligation (GO) debt servicing. Short-term optionality could enter the operational budget affecting long term pass-through if localities have flexible constitutions or statues which permit such fungibility.[5]

Given our new estimate of credit-risk sharing, a parameter which enters a broader welfare calculus regarding the efficiency of macroeconomic stabilization policies, one natural question is when does municipal credit-risk sharing represent a socially efficient policy improvement? From a financial frictions

[3]One potential political economy interpretation for this concentration is that while furloughs and separations may have faced less political resistance in sectors with a clear rationale for lockdowns, a liquidity option may have induced the retention of some of these employees. Another relates to prioritizing unionized workers.

[4]This view was articulated in Sheiner (2021), which we discuss along with the evolving fiscal and jobs outlook of state and local governments in Appendix A.1.

[5]In related work, we are exploring whether we can leverage differences in these rules and features of outstanding long term bonds (such as revenue versus GO), to try to address this apparent short term to long term mechanism.

viewpoint, several influential papers have suggested that sub-optimal risk sharing could result from institutional investors only benefiting from local exemptions, and a home market bias that distorts the efficient spread of risk (Poterba (1989), Pirinsky and Wang (2011), Schwert (2017), Babina et al. (2021)). Other market structure constraints may also result in inefficient muni market pricing (Garrett (2020)). When such frictions are also heterogeneously correlated with the mean income of underlying geographies, inequality weights must also be taken into consideration. Future work will need to contextualize the magnitude of potential externalities and multipliers from government functioning (including the social value of public services), to help answer this question.

1.1 Relevant Literature

The municipal bond literature has broadly examined default risk, liquidity risk, and tax effects. Using MSRB transaction-level data, both Wang et al. (2008) and Schwert (2017) attribute a large proportion of the observed yield to default risk in municipal bond pricing. Using a sample from 1998 to 2015 (mostly expansion years), Schwert (2017) finds that credit-default risk accounts for between 74% and 84% of mean spreads; a strikingly large share.[6] Ang et al. (2014), however, use a different approach and find that liquidity accounts for 75% of the average municipal bond spread.[7] We interpret our findings as consistent with the Schwert (2017) results, as our estimates of MLF access imply a large investor perceived spread, but only among the lowest rated issuers, which we interpret as reflecting credit downgrade (and potentially, but not necessarily, default) risk. Our employment and credit downgrade results are also fully consistent with Adelino et al. (2017), who find that credit ratings impact borrowing constraints, and subsequently have real implications for employment and expenditure outcomes; as well as Cornaggia et al. (2018) who find a significant link between credit ratings, yields, and borrowing costs. We also view our credit-risk sharing results as related to Gao et al. (2019), who document that the regulatory default environment meaningfully interacts with borrowing costs and yields.

In a contemporaneous paper, Bi and Marsh (2020) also analyze the impact of varied fiscal and monetary policy interventions on municipal bond market performance in the wake of COVID-19. Bi and Marsh (2020) analyze daily time series effects around different announcements and do not focus explicitly on the MLF nor its associated cutoffs for identification. Like us, they focus differential high

[6]The estimated range of the credit-default risk component in Schwert (2017) is based on three approaches: measuring the default component as a residual after adjusting yields for tax and for liquidity components estimated from transactions data directly; measuring the default component using credit default swaps; and, using within-bond changes in spreads around pre-refunding events, when a risky bond becomes risk-free, while controlling for bond-specific unobservables using fixed effects.

[7]Ang et al. (2014) compute synthetic risk-free municipal bond yields using an estimated zero-coupon curve from pre-refunded municipal bonds, adjusting them to have the same liquidity as regular munis, and compute the credit risk component by taking the difference between regular municipal bond yields and yields computed using the synthetic discount rates.

credit-risk, and find that long-term low-rated bonds remained distressed beyond Federal interventions. By contrast, we find that this was only true when the MLF was not available to those issuers. Bordo and Duca (2021) further focus on the time series impact of the MLF announcement on yields spreads, and find that the MLF capped the growth of spreads by 5 to 8 percentage points. Li and Lu (2020) focus on the effects of shutdown announcements on offering yields (rather than trade prices), and find that initial offering yields increased in response to shutdowns, and decreased following facility announcements. Both Bordo and Duca (2021) and Li and Lu (2020) are consistent with our results on market functioning, however neither focus on the causal effects of MLF optionality on issuers. Finally, Fritsch et al. (2021) study entities that issued directly through the MLF, and similarly find that yields declined after announcements of intent to issue via the facility.

Our paper also builds heavily on methodological contributions from a series of municipal bond market papers. Harris and Piwowar (2006), Green et al. (2007b), Green et al. (2010), Green et al. (2007a), and Schultz (2012) study secondary market transaction costs, with the latter two papers focusing on the pricing and price dispersion of newly issued bonds. We also draw from work by Novy-Marx and Rauh (2012) quantifying the sovereign default channel. Lastly, related to evaluating Federal Reserve facilities, our methodological approach most closely links to work by Moore (2017) who analyzed the Term Auction Facility by comparing marginal winners and losers, and Luck and Zimmermann (2020) who find strong employment effects from quantitative easing (QE) policies via bank lending channels.

Finally, Auerbach et al. (2020) examine federal and state fiscal conditions in the context of the pandemic, both generating their own estimates and evaluating other papers. They find generally, that the pandemic has induced smaller tax losses may have been initially expected, for example, when imputing expected losses from the Great Recession. In Appendix A.1, we further provide a detailed summary of the evolution of state and local revenue and employment forecasts throughout the pandemic and during the sample frame of this analysis. One specific paper examining the effect of CARES Act aid on employment during COVID-19, Green and Loualiche (2020), finds that sales-tax reliant localities with stricter balanced budget requirements (BBRs) (proxied by lower rainy day funds) likely suffered greater job losses due to an inability to deficit spend across budget cycles, and it was in those cases that fiscal aid was more effective. This finding, in part, prompts us to include state fixed effects in our preferred specification to absorb variation across states emanating from the stringency of their underlying BBRs (however our main results do not rely on these fixed effects).

The rest of the paper proceeds as follows. Section 2 provides institutional background on municipal debt markets and the unique market distress exhibited during the COVID-19 pandemic. In Section 3, we discuss the main bond market and real economy data sources used, matching procedures and sample restrictions. Section 4 outlines our methodological approach and estimating equations. Section 5 presents the main results on bond-level outcomes and public sector employment, and mechanism tests. We provide sensitivity and robustness tests to our results in Section 6, and conclude with a discussion of what is learned from the main results in light of a perceived low take-up puzzle regarding the number of issuers that made use of the Municipal Liquidity Facility in Section 7.

2 Municipal Distress and Federal Interventions during COVID-19

Our empirical strategy leverages a unique policy implemented in response to the COVID-19 pandemic. In this section we discuss municipal bond market conditions in the run-up to the pandemic, how recent dynamics may have diverged from prior trends and contributed to peak disruption, and a detailed timeline of Federal sector interventions that supported municipal debt markets during this period.

2.1 Turmoil in Municipal Debt Markets

The Municipal Bond Market

The $3.8 trillion municipal bond market contains more than 50,000 issuers and 1 million individual bonds, making it approximately half the size of the corporate bond market with 10 times as many issuers. Roughly 90% of this market is exempt from federal income tax,[8] and more than 80% is rated investment grade. Consequently, municipal bond default rates have historically been low (Appleson et al., 2012). As of May 2020, 26% of outstanding debt was issued directly by state, city, county, and other local governments, 41% by utilities, service, and transit issuers, 21% by school districts, and 8% by public hospitals. Unlike treasury and corporate bond markets, 70% of municipal debt is held by retail investors (a third of which is in mutual funds and ETFs) seeking tax advantages associated with municipal bond returns.[9] Unlike the corporate sector, municipal debt is also commonly issued in deals containing many different tenors as independent bonds, facilitating more predictable budget smoothing.

Government issuers in this market are often required to balance their operating budgets, and can usually only borrow long term in order to finance infrastructure investments. In 2015, 48 of 50 US states had some form of balanced budget requirement (BBR), while 39 of 50 had strong constitutional or statutory requirements (Brookings Tax Policy Center, 2015), limiting deficit spending across fiscal

[8] Some bonds are "double-exempt" for local residents, applicable to tax liabilities at both Federal and State levels (e.g. CA state bonds for CA residents), whereas others are "triple-exempt" at the local level as well (e.g. New York City bonds).

[9] Outstanding debt by sector calculated from Bloomberg, as of 5/19/2020. Retail share calculated from the Board of Governors, "Financial Accounts of the United States, Z.1, as of 1Q2020." Retail holdings calculated from MSRB.

cycles.[10] General Obligation (GO) bonds, which constitute approximately 30% of the long-term municipal market, are not secured by a specific revenue source but are instead backed by the "full faith and credit of the taxing authority" and typically finance capital projects like bridges and schools. The large remainder of the long term market (60%) is dominated by Revenue bonds (RB) issued by public enterprises and secured by defined revenue sources (such as transit revenue, airport feels, bridge tolls, etc.)[11]

Less well known but central to this paper, state and local governments also frequently leverage the $440 billion short-term municipal note market to bridge cash flow gaps within fiscal years. As localities depend on revenues (tax receipts, federal grants, the proceeds of bond issues and other revenues) that are only received at specific intervals, budget officers seek to smooth spending in anticipation of such receipts, and can do so by issuing TANs, RANs, BANs – tax anticipation notes, revenue anticipation notes, and bond anticipation notes. Other notes include tax and revenue anticipation notes (TRANs), tender option bonds (TOBs), and Variable Rate Demand Notes (VRDNs), the latter of which comprises roughly half of the short term market. Short-term notes are typically secured by the revenues expected to be received later in the fiscal year, and are paid off when said revenues arrive. A classic example is the proceeds from final settlements of state income tax returns due April 15 (the federal tax filing deadline), which can include taxable unearned income and capital gains (typically not withheld). Other examples include quarterly property tax receipts, or expected surges in airport fees during the holiday season.

Primary market interest rates at which governments issue new debt in this market are also strongly linked to secondary market yields. This is because primary market pricing is usually benchmarked to secondary market prices of similar bonds, and the willingness of dealers to underwrite bonds is impacted by market conditions (Boyarchenko et al., 2020). Price discovery occurs through submissions to exchanges, and is exceptionally low in this market in part due to the low volume of transacted trades (Green et al., 2010). In an average expansion year (such as 2019), muni bonds trade about 6,500 times a day with median trades of about $30,000, far less frequently and at lower volumes than corporate bonds.

Municipal Liquidity Crisis

While many of these features of the municipal market have remained relatively constant for several decades, one notable change in recent years has been the increasing share of muni holdings in mutual funds, which nearly doubled to $800 billion since 2010 (from 10% to 20% of the market) and by some measures now reflect one third of overall muni holdings (WSJ, Oct 2019). Importantly, municipal

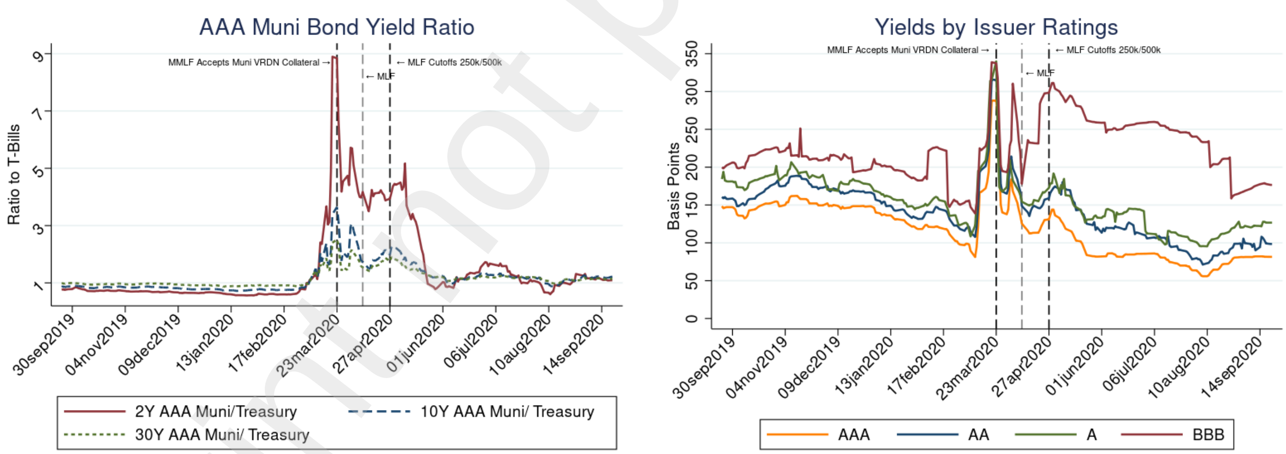
[10]Some states further prohibit or limit GO bond issues, including Arizona, Colorado, Idaho, Indiana, Iowa, Kansas, Kentucky, Nebraska, North Dakota, South Dakota, and Wyoming.
[11]MSRB, Bloomberg, calculated as of 5/21/2020.

holdings by mutual funds have also become more concentrated, with over 70% of muni mutual funds held by the 10 largest fund families (investment banks).[12]

After a record surge of muni inflows into mutual funds that netted $90 billion over the 12 months ending in February 2020, inflows reversed sharply in March 2020, with municipal bond mutual funds experiencing outflows of $43 billion in March (Cipriani et al., 2020a). In a rush for liquidity, this offloading was accompanied by sell-offs in other fixed income asset classes, not just municipal bonds. As explained in Cipriani et al. (2020a), "When funds experience outflows, fund managers must sell securities in order to have enough liquidity to meet redemptions. As a result, there was almost no demand for new issuance at the peak of the market disruption". Indeed, early on in the pandemic, some muni market journalists credited increased mutual fund concentration as one potential reason why municipal markets may have had an unexpectedly outsized role in the rush for liquidity during the COVID pandemic relative to prior downturns—the ensuing sell-off may have flooded the market due to seller concentration, bringing muni demand and new debt issuance to a near standstill by mid-March.[13]

Figure 1 below shows the performance of secondary market municipal bond yields during this period, and highlights the liquidity crisis that occurred in early- to mid-March.

Figure 1: Turmoil in Secondary Market Municipal Bond Yields during COVID-19



NOTES—Both panels use daily current yields (6-month) in the calculation of the dependent variable. The left panel is taken from the AAA BVAL curve from Bloomberg, while both plots' bonds ratings are calculated as averages across Moody's and S&P ratings (take directly from Bloomberg). The three announcements indicated by vertical dotted lines are described in detail in the text. *Source*: Bloomberg, originally calculated in Cipriani et al. (2020a).

The left panel shows mean weekly yields for the universe of secondary-market traded AAA muni bonds—the safest in the market—as a ratio to comparable US treasuries. In the period leading up

[12]Calculations from "Financial Accounts of the United States, Z.1, as of 1Q2020".

[13]According to the Wall Street Journal, one prominent fund attempted to offload up to $700 million in munis in a single day, which they note, was an unprecedented contrast to the usual $50,000 trades the market is accustomed to (WSJ, April 2020).

to the mid-March turmoil, municipal bonds were trading at yields below US treasuries (ratios below 1) due to their special tax-exempt status. The market then deteriorated in a steep yield rise until the first Federal intervention in the muni market was announced on March 23 (indicated by the left-most dotted line, and explained in detail in the subsequent subsection).[14] Yields spiked precipitously for all bond tenors but most sharply for short-run debt, reflecting deteriorating demand conditions as investors anticipated severe dislocation in the near term.[15] After a series of further Federal interventions, markets finally reverted back to relative market functioning by June. The right pane, however, reveals significant heterogeneity in this resumption to normalcy—the lowest rated investment-grade bonds (BBB) continued to exhibit distress despite recovery in lower risk credit bins. One potential explanation for these heterogeneous recovery rates is that investors may have struggled to assess the change in credit default risk for low-rated issuers weathering the first wave of the pandemic, consistent with notoriously low price discovery in municipal markets due to their low trading frequency.[16] As Green et al. (2010) note, illiquid assets may "rise faster than they fall". This differential recovery motivates our main heterogeneous specifications in which we examine effects separately according to pre-crisis creditworthiness levels, discussed in Section 4.

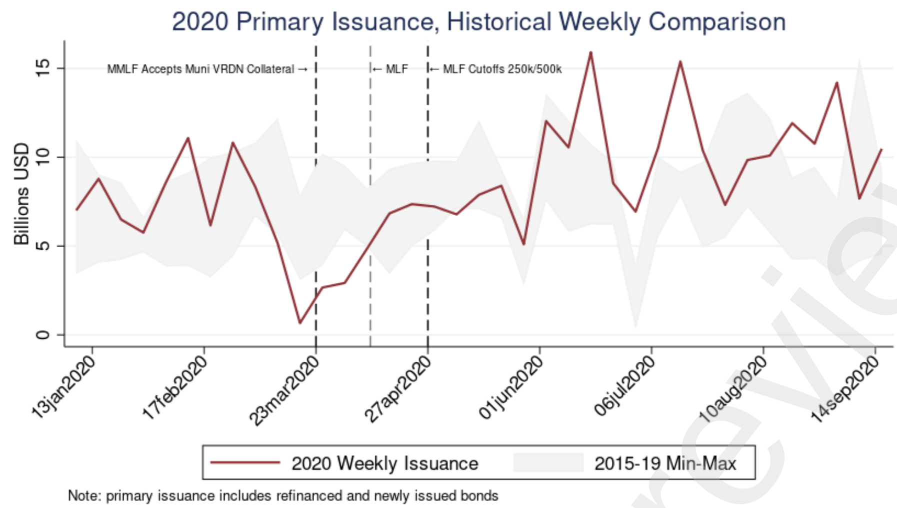
While secondary market yields provide an important lens for measuring financial distress, the ability of state and local governments to issue new municipal bonds on primary markets is also critically linked to secondary market performance. If demand is low for bonds already traded in the market, then the additional supply of new bonds (like those needed to bridge government cash flow shortfalls) may be both constrained, and potentially issued at higher cost than during an expansion—even in the presence of a monetary policy environment with lower prevailing interest rates. Local governments supplying new bonds in this environment may face increased intermediation fees from market makers and underwriters seeking to arrange initial offerings, and may have to resort to compensating investors with high interest rates for retaining illiquid positions or additional credit risk. Figure 2 below shows 2020 weekly new issuance as compared to the minimum and maximum issuance in the same weeks over the previous five years.

[14]This, as well as subsequent interventions relevant to the analysis and marked by the two additional dotted lines in Figure 1, are all described in further detail in the next subsection.

[15]It is common practice in the municipal bond market that newly issued bonds target an at-issuance coupon rate of 5% by modifying initial offering prices. Because coupon interest is fixed at issuance and comprises the numerator of the current yield metric shown in Figure 2 (with prices reflected in the denominator), large spikes in yields thus tend to reflect declining prices as demand shifts downward.

[16]One additional example of opaqueness in municipal debt markets relates to undisclosed private muni loans, extensively documented in Ivanov et al. (2021).

Figure 2: State and Local Government New Issuance Shortfalls during COVID-19



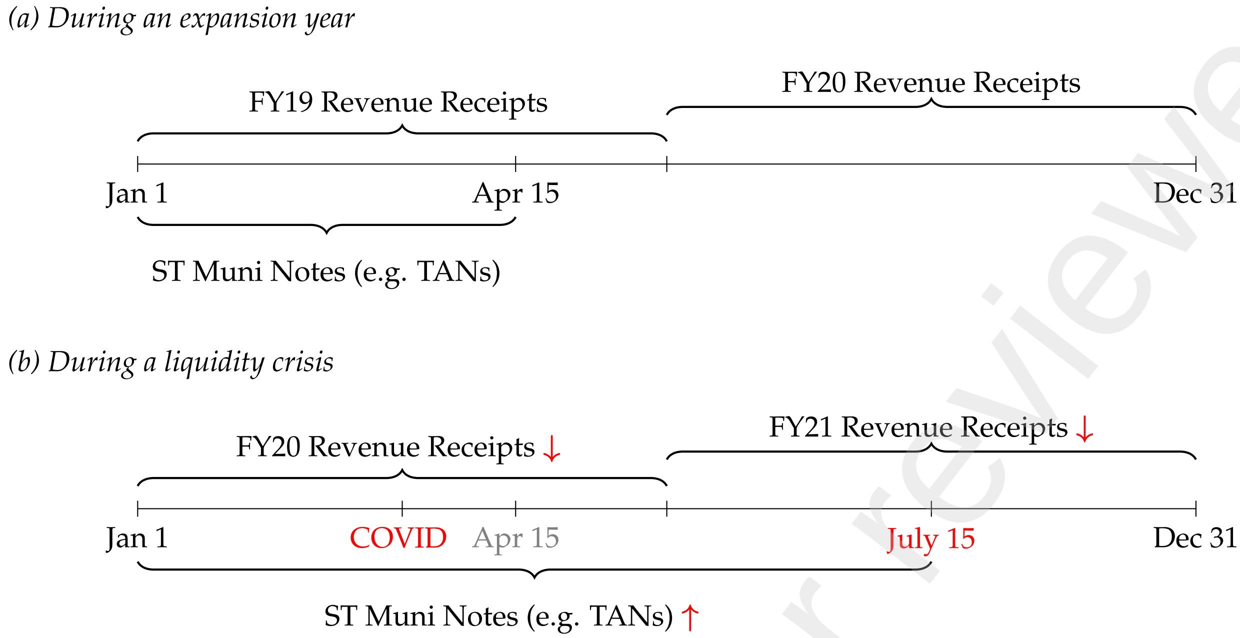
NOTES—Figure shows weekly total primary issuance in the municipal bond market throughout the 2020 calendar year, compared to the five-year historical maximum and minimum issuance in the same week. Weekly means are aligned beginning-of-week on Mondays (when most Federal interventions are announced), and may include refinanced bonds which appear as new CUSIPs. *Source:* Bloomberg, originally calculated in Cipriani et al. (2020a).

As the pandemic set in, governments endured a clear primary issuance shortfall relative to historical trend, which approached a market freezing point at its nadir around March 20th. After a series of Federal interventions, the trend then mean reverts and appears to compensate for missing issuance. Especially apparent is a significant issuance spike in mid-July, when local and state income tax receipts and non-withheld income such as capital gains were realized (deferred in 2020 due to the IRS federal tax deadline extension from April 15 to July 15, enacted as a matter of fiscal policy). Cumulatively, though at least the end of June, 2020 year-to-date primary issuance had narrowly outpaced its five-year historical levels, though as we discuss below, this uptick may not necessarily reflect the socially efficient level of primary issuance given the unusual supply needs of municipal issuers during the pandemic.

2.2 Pass-Through of Market Distress to Budgets and the Real Economy

To understand how municipal market liquidity crises impact state and local budgets and public sector hiring, consider two fiscal cycles spanning one calendar year, as shown in panel (a) of Figure 3.

Figure 3: Fiscal Cycles and Budget Needs during a Municipal Liquidity Crisis



In a regular calendar year (like 2019), state and local governments incur expenses such as payroll, debt service, etc., throughout this entire period, but only receive expected revenue at distinct intervals. One prominent example of an irregular revenue source is the proceeds from final settlements of state income tax returns, typically due April 15 (the federal tax filing deadline), which can include taxable unearned income (typically not withheld).[17] When governments are short of revenue yet have guaranteed sources of future receipts, they can issue short-term anticipation notes on municipal bond markets to help smooth out the temporary shortfall. Take for example, the state of New York, whose tax base includes large amounts of capital gains realized in the prior calendar year. Taxes on these amounts are typically received on April 15. New York may issue a 4-month tax anticipation note (TAN) in anticipation of this revenue on Jan 1, 2019, maturing April 30th, 2019 (one month after its fiscal cycle closes). This would generate bond proceeds to smooth out cash flow, and would be secured by revenue incoming on April 15, enabling the government to start spending part of the expected settlement amount in the interim.

Now consider what happens when an economy experiences a major income shock accompanied by a bond market liquidity crisis, as was the case with the COVID-19 pandemic and the muni market. There are three distinct ways in which this enters fiscal budgeting, shown in red in panel (b) of Figure 3. First, income receipts decline in both the current and upcoming fiscal cycles. While tax assessments may be revised downwards, 2021 budgets were for the most part already set when the COVID pandemic

[17]Other examples include quarterly property tax receipts, or expected surges in airport fees during the holiday season.

took hold. This led to a decline in the revenue that would normally cover these planned expenditures. Second, as a matter of fiscal policy, the IRS extended the 2020 federal tax deadline from April to July 15th, increasing the quantity of short-term notes governments desired to issue on the market to plug the additional cash flow gap. Third, while the temporary misalignment of expenditures and revenue needs (even if unexpectedly large in a specific year) are normally able to be remedied through the bond market, the simultaneous large financial sell off of fixed income assets described in Section 2 effectively drove investor demand for munis toward zero, leaving governments unable to borrow. This was the challenge faced by the US economy in mid-March of 2020; local governments could not borrow on private municipal markets despite their increasing need to do so because of demand conditions.

2.3 Federal Interventions during COVID-19

> *We had to undertake very quickly to enter into the market, and our four principles that were guiding us in terms of our design were: speed to announcement and execution—do not let the perfect be the enemy of the good; ensure that State and local governments had access to liquidity for operating cash—this is what we heard overwhelmingly from individual issuers and associations like GFOA; restore market confidence and stability given the unprecedented liquidity crisis in the market; and finally, to your point, to design a uniformly applicable, transparent, easy-to-administer facility.*

—Kent Hiteshew, Deputy Associate Director for Financial Stability, Federal Reserve Board of Governors (on the choice of lending eligibility population cutoffs for the Municipal Liquidity Facility Congressional Oversight Commission, Examination of the Municipal Liquidity facility Established by the Federal Reserve Pursuant to the CARES Act, Sept 17, 2020.)

In light of both real municipality liquidity challenges and a municipal *market* liquidity crisis, several Federal Sector agencies undertook interventions to influence municipal bond markets. Here, we describe the three most prominent interventions that relate to this paper, indicated with vertical dotted lines in plots throughout the draft.

March 23, 2020: MMLF Accepts Some Types of Municipal Debt as Collateral: The Federal Reserve System expanded its recently announced Money Market Mutual Fund Liquidity Facility (MMLF)—which makes loans to financial institutions secured by high-quality collateral—to accept variable rate muni demand notes (VRDNs) as pledgable collateral.[18] As shown in Figure 1 and Figure 2, the announcement day

[18] The MMLF was established "to prevent outflows from prime and muni money market funds from turning into an industry-wide run, as happened in September 2008", and was supported by $10 billion of credit protection from the U.S. Treasury's Exchange Stabilization Fund. (Cipriani et al., 2020b). While the MMLF had accepted some forms of short-term municipal debt on March 20, the types of pledgble collateral were greatly expanded on March 23.

of this expansion corresponds almost exactly with the peak of municipal yields and other measures of market distress, which subsequently began a steady decline to normal market functioning as Federal demand for municipal assets pulled the market away from its sales freeze. Moving forward, we refer to the time period prior to this first announcement as the "pre-period" or placebo period when studying effects of subsequent facility announcements before any were yet established. However, it bears noting that two days earlier, the Internal Revenue Service (IRS) and US Treasury formally extended the Federal tax filing deadline from April 15 to July 15 to provide household tax payment relief on 2019 liability payments due in April.[19] Further, on this same day, several new Federal Reserve facilities (for example in the corporate bond market) were announced or expanded.[20] For these reasons, any time series analysis across these various announcements must be treated delicately as they have the potential to confound effects from a bundle of interventions. We thus interpret effects of the "totality of events" when analyzing dependent variables before the first (and after the last) of these announcements are made.

April 9, 2020: Municipal Liquidity Facility (MLF) Announced. As part of a broader $2.3 trillion support package that included support for the Payroll Protection Program (PPP), Main Street Lending program, expansions to corporate credit facilities (PMCCF and SMCCF) as well as the Term Asset-Backed Securities Loan Facility (TALF), the Municipal Liquidity Facility (MLF) was announced on April 9 "to help state and local governments better manage the cash flow pressures they are facing as a result of the increase in state and local government expenditures related to the COVID-19 pandemic and the delay and decrease of certain tax and other revenue."[21] The MLF was established with a $35b US Treasury initial equity investment (appropriated through the March 27 CARES act) as backstop for a maximum of $500b in direct Federal Reserve lending of short-term notes (TANs, RANs, TRANs, BANs) to municipal issuers via a Special Purpose Vehicle (SPV). As the quote above demonstrates, there was considerable concern in the design phase of the facility that facing up to 50,000 issuers would render the operational arm of the facility less effective, and risk a slower speed to intervention. Consequently, eligible issuers were initially limited to US states, cities with populations greater than 1 million, and counties with population greater than 2 million.

By targeting the largest issuers first (where the bulk of trading occurs), the thinking was that this push would help the market stabilize, and additional issuers could be covered by the parent state's ability to "downstream" to ineligible issuers, as described below.[22] Tenors were originally limited to 2-year maturities, and priced at a penalty rate to a private market index by issuer credit rating.[23] The

[19] See March 21, 2020 IRS Extension Announcement for details.

[20] See Federal Reserve System Board of Governors Press Release, March 23, 2020 "Federal Reserve announces extensive new measures to support the economy" for a description.

[21] See Federal Reserve Bank of New York MLF Page, April 9, 2020 and April 9, 2020, MLF Term Sheet for further details.

[22] See Examination of the Municipal Liquidity Facility, 09/17/20, Congressional Oversight Committee

[23] Details regarding the penalty pricing grid are shown in Appendix A.2.

lending eligibility cutoffs established here were also initially distinct from cutoffs used to determine direct CARES Act aid through the Coronavirus Relief Fund (CRF), through which cities and counties above 500,000 in population were eligible for direct aid—an important consideration we return to when interpreting effects on public sector employment.[24]

"Downstreaming", whereby for example, a state or higher-level government issues on behalf of a cash-flow managing instrumentality or political subdivision is permitted, but the extent of downstreaming is regulated by state legislative approval. Further, the MLF established maximum MLF borrowing caps for each issuer as 20% of each issuer's 2017 "Own-Source General and Utility Revenue" (OSGUR), which implicitly limits the extent of downstreaming.[25] In our setting, the presence of downstreaming will tend to attenuate any effects we find through our regression discontinuity analysis, as ineligible issuers are more likely to be beneficiaries of downstreaming when subdivisions do not already have direct MLF access. We thus interpret all RD estimates as a lower bound on the direct MLF access effect in this paper.

April 27, 2020: Municipal Liquidity Facility (MLF) Population Cutoffs Expanded. Central to our empirical strategy, on this day eligibility was expanded to cities with populations greater than 250,000, counties with populations greater than 500,000, and multi-state entities were included as eligible issuers.[26] Tenors were also extended to allow up to 3-year maturity notes to provide maneuvering room for localities in which Balanced Budget Requirements (which usually preclude deficit borrowing across fiscal cycles) are statutorily amendable or flexible. Terms regarding minimum credit ratings were also established—"an Eligible Issuer that is not a Multi-State Entity must have been rated at least BBB-/Baa3 as of April 8, 2020, by two or more major nationally recognized statistical rating organizations ("NRSROs"). An Eligible Issuer that is not a Multi-State Entity and that was rated at least BBB-/Baa3 as of April 8, 2020, but is subsequently downgraded, must be rated at least BB-/Ba3 by two or more major NRSROs at the time the Facility makes a purchase." (MLF Term Sheet, April 27, 2020). In other words, issuers are required to have had investment-grade ratings both prior to the pandemic and at time of issuance. Finally, this announcement added an explicit sunset date of December 31, 2020.

Extensions: Several notable extensions were announced thereafter, including the publication of the MLF pricing grid (May 11) and revision to the pricing grid (August 11). On June 3, the MLF was expanded to allow state governors to specially designate up to 2 ineligible cities or counties as eligible,

[24] Cities and counties with populations under 500,000 could receive CRF aid as well, but had to rely on their underlying states to downstream aid to their localities, which is potentially politicized and less certain.

[25] OSGUR caps use 2017 as a base as it reflects the latest year prior to 2020 in which the U.S. Census Bureau's Annual Survey of State and Local Government Finances administers a full government census (conducted every 5 years, with sampled surveys in the interim). For a list of OSGUR caps per issuer, see Federal Reserve Municipal Facility Limits.

[26] The MLF term sheet defines multi-state entity as an entity created by a compact between two or more States, approved by the United States Congress, acting pursuant to its power under the Compact Clause of the United States Constitution MLF Term Sheet, April 27, 2020. These include for example, authorities like the Port Authority of New York and New Jersey.

as well as two revenue-bond issuers (RBIs) per state.[27] The cap of governor-designated city and county issuers varies between 0 and 2 per state depending on the number of already-eligible cities and counties, as some small states had fewer eligible issuers according to the April 27 cutoffs.[28] Like downstreaming, to the extent that this Governor privilege is utilized when issuers do no already have direct access to the MLF, our RD strategy estimated should be interpreted as lower bounds on the true effect. Because these subsequent extensions were minor relative to earlier announcements, we label all activity beyond April 27 as the "post-period", analogous to how we defined the "pre-period" prior to March 23.

3 Data Sources and Underlying Variation

3.1 Linking Trades and Primary Issuance to Census Populations

We desire a dataset that allows us to link high-frequency municipal bond trades and new primary issuance to MLF-eligible (and ineligible) localities. Toward this end, we begin with the universe of unique Bloomberg issuers which are sorted by type (e.g. city, county, state, school district, etc.) at the 6-digit base CUSIP level. These contain both issuers with active debt, as well as small issuers who do not have (or never had) outstanding debt, and allow us to first flag all city and county issuers. We then acquire and clean the universe of secondary market municipal bond trades from the Municipal Securities Rulemaking Board (MSRB) via their Electronic Municipal Market Access (EMMA) service. Since 1998, MSRB has required registered dealers to report all municipal bond transactions, which include information about their 9-digit bond CUSIP (the first 6-digits of which correspond to base CUSIP), date and time of trade, price and yield, maturity, coupon, par volume traded, total amount of the bond, and a flag for whether the dealer bought from a customer, sold to a customer, or whether the transaction was an inter-dealer trade. Importantly, the detailed security description has full information on the issuer's name and geography.

For our regression discontinuity strategy, our main analysis period is contained in calendar year 2020, however to provide an additional year of pre-trend we keep all trades occurring after January 1, 2019, and end our sample frame on November 20, 2020. After restricting our analysis to city and county issuers, this results in 2,857,105 trades of 194,926 bonds across among 8,042 unique issuers in our MSRB-Bloomberg matched data.[29] To establish issuer lending eligibility cutoffs, the MLF makes use of two specific Census Bureau files: a 2018 population list for cities and towns, and a 2019 population list

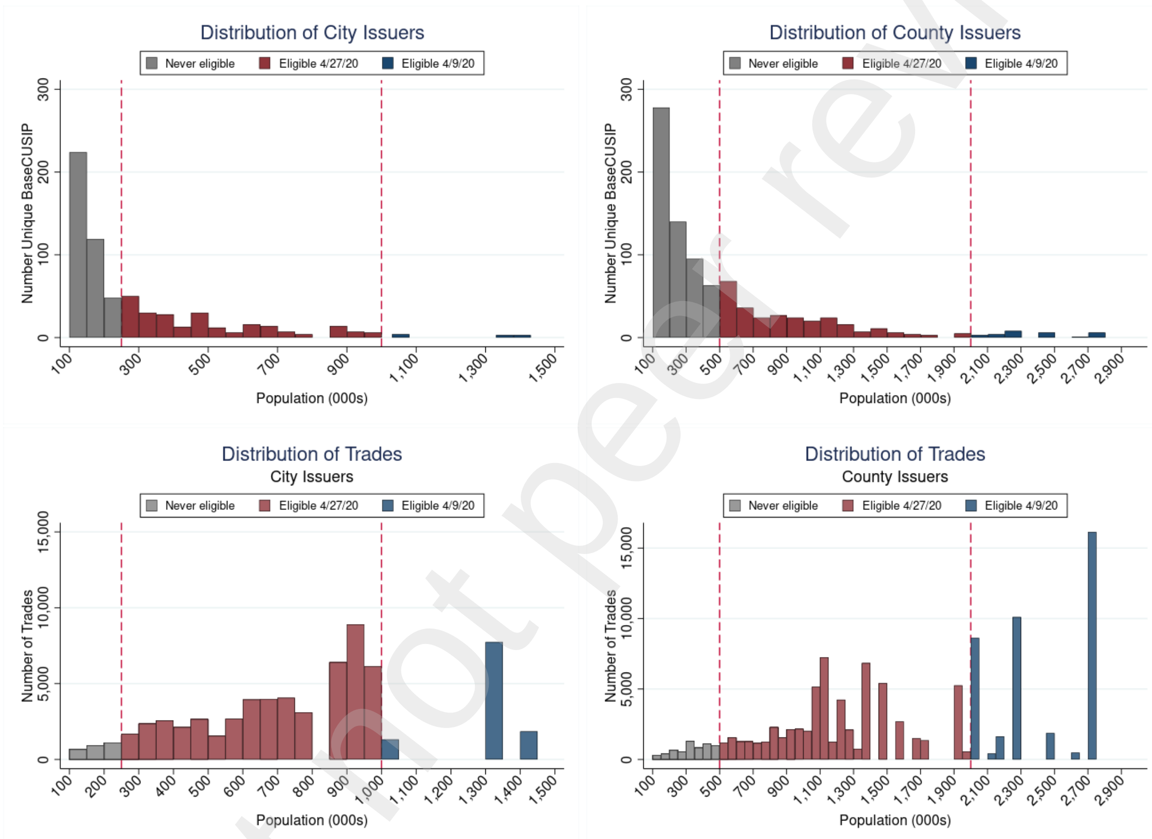
[27] While RBI issuer access to the MLF may be reflected in aggregate estimates of market yields, our main regression discontinuity design is restricted to cities and counties and thus remains insulated from this designation—we therefore do not focus on it.

[28] See MLF Term Sheet, June 3, 2020 for further details.

[29] Cities: 1,939,824 trades, 142,876 bonds, 6,121 issuers; Counties: 917,281 trades, 52,050 bonds, 1,921 issuers.

for counties.[30] These files contain detailed place (government) names and populations, however do not carry CUSIP identifiers. We thus extensively clean MSRB issuer names to match these Census place name lists, and are able to match 6,121 / 6,789 of MSRB city issuers and 1,921 / 1,938 of MSRB county issuers to populations respectively. Figure 4 below shows the distribution of matched city and county issuers and trades prior to all Federal announcements, focusing on municipalities with populations above 100,000 and cities less than 1.5 million for exposition.

Figure 4: Running Variable Distribution of Issuers and Trades around Lending Eligibility Cutoffs



NOTES—For exposition, cities and counties are shown for populations over 100,000; including 0 to 100,000 would result in a large spike of small issuers which are omitted here to focus on variation around the 4/27/20 cutoff. All data are restricted to trades prior to March 1, 2020, to demonstrate variation prior to the onset of the pandemic and choice of cutoffs. See text for how issuers, trades, and populations are merged together. *Source:* US Census Bureau, MSRB, Bloomberg

The first thing to note about this figure is that the initial MLF eligibility cutoff established on 4/9/20, while able to target the most traded bonds, ultimately resulted in very few eligible issuers (blue bars). Were this cutoff to have remained in place, 24 issuers with 7,559 outstanding bonds would have been

[30] U.S. Census Bureau, "Annual Estimates of the Resident Population: April 1, 2010 to July 1, 2018" for cities, as of April 6, 2020; and U.S. Census Bureau, "Population, Population Change, and Estimated Components of Population Change: April 1, 2010 to July 1, 2019" for counties, as of April 6, 2020. As these files cover populations greater than 50,000, we further use U.S. Census Bureau 2010-2019 Populations, All Places Files to fill in populations when less than 50,000. This name cleaning process is involved, and described in detail in Appendix A.3.

MLF-eligible. The revision of cutoffs to their 4/27/20 levels (red bars) expanded eligibility to a total of 204 issuers with 37,666 outstanding bonds (prior to the pandemic).[31] For these reasons, we focus all subsequent analysis on the 4/27/20 cutoffs, as any variation across the initial cutoffs would be smooth across the new thresholds. While a formal Frandsen (2017) test (analogous to a McCrary (2008) test for a discrete running variable (Cattaneo et al., 2018)) is provided in Section 6, the smooth mass around the new cutoffs combined with institutional details behind the selection of 250,000 and 500,000 as cutoffs (including their apparent use of round number heuristics) also provides preliminary evidence that cutoffs were not chosen to target specific types of issuers or anomalies in the population distribution.

While restricting the data to trading bonds provides an important lens into yields, we are also fundamentally interested in effects on new primary issuance. However bonds that are newly issued but do not trade (or trade sparsely or with a lag relative to issuance) would not appear in the MSRB data. For this reason, we turn to Mergent which contains the universe of municipal issuance. In addition to the 194,926 trading bonds in our MSRB post-2019 sample, from Mergent we are able to add an additional 8,491 bonds that are newly issued but do not trade.[32] Mergent also provides key bond characteristics, including offering amount, the source of funds and use of proceeds, coupon type (fixed, variable, or zero), the tax status of the coupon payments, callability and first call date, insurance status and the identity of the insurer, and pre-refunding status and timing. We discuss these variables, cleaning procedures, and tax adjustments, in Appendix A.4.

Credit Ratings

To analyze effects by creditworthiness, we construct time-varying issuer credit ratings from ratings data on their underlying bonds. We first acquire monthly ratings updates at the bond level from the three major nationally recognized statistical rating organization (NRSROs): Standard and Poor's (S&P), Moody's and Fitch.[33] At the CUSIP level, we ascribe all trades that occur within the month following each ratings observation their long-term, short-term and muni bond rating (when available). We then concord all long-term, short-term, and muni-ratings—each system specific to their NRSRO—to an aggregated S&P Ratings bin (AAA, AA, A, BBB, BB, CCC, C, D) using a custom concordance file.[34] Following Boyarchenko et al. (2020), we finally construct issuer plurality ratings within issuer-month, equal to the most common aggregated bond rating across NRSROs and CUSIPs that month. This allows

[31] Of all eligible MLF issuers, only Madison, WI, did not have trading bonds after Jan 1 2019 in our data. We otherwise have fully coverage of city and county eligible issuers.

[32] Among city and county issuers, non-traded bonds are generally constrained to localities with populations less than 25,000, and therefore newly issued bonds are largely irrelevant to our analysis of yields. We otherwise, are unable to match 25% of Mergent non-trading bonds to Census populations due to mismatches in Mergent issuer names and Census issuer names.

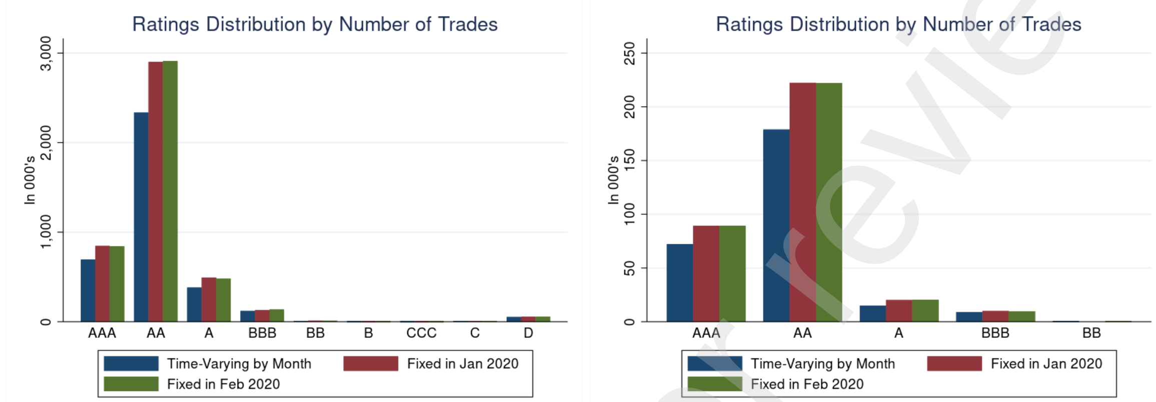
[33] These are assembled via Bloomberg.

[34] See Appendix A.5 for further details on these concordances and the construction of plurality ratings.

us to subset on pre-pandemic fixed issuer ratings when implementing heterogeneous specifications. In Figure 5 below, we show the resulting distribution of plurality ratings (at the trade-level) when fixing the rating to January (or February) 2020, and when letting the plurality rating vary by month.

Figure 5: Plurality Ratings Distribution for Cities and Counties

(a) All Cities and Counties: (b) Cities and Counties within 100k of Cutoff:



NOTES—Figure shows distributions of trades according to monthly plurality issuer ratings from January 1, 2019 to November 20, 2020. See text for details. *Source:* MSRB, S&P, Moody's, Fitch

This figure shows that the ratings distribution is both heavily skewed toward high-credit issuers, and relative stable over time. In panel (b) we show the distribution for an arbitrary bandwidth of 100,000 around city and county cutoffs, to show stability in the distribution to this sub-sample (used to highlight the key variation in the regression discontinuity design in the next sub-section). To the extent that there are monthly credit downgrades that occur differentially after the pandemic, or for MLF-eligible versus non-eligible issuers, we use disaggregated ratings and the bond level. This allows us to ask whether the probability that a bond was downgraded relative to the prior month changed overall, and heterogeneously by baseline plurality ratings (for example within January 2020 BBB's). Generally, we are able to match 96.2% of trading bonds to a monthly plurality rating, where 3.8% of the unmatched reflect the inability to find a plurality when ratings are split.

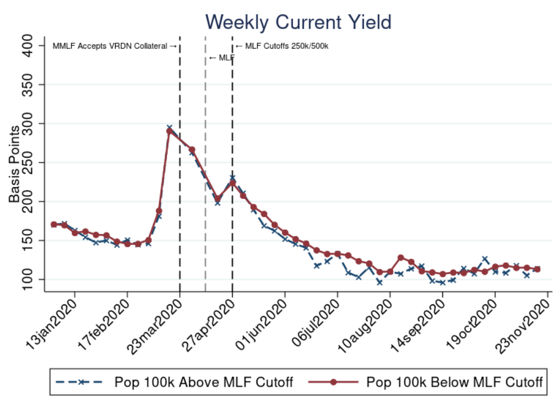
3.2 Underlying Variation in Yields

We now turn to highlighting the underlying variation in our matched sample. In our main regression discontinuity design specification, we choose the running variable's window of analysis using an optimal bandwidth selection procedure (following Calonico et al. (2014)) which takes choice and publication bias out of the econometrician's hands. In this section, we first show some of the key variation underlying our design using an intuitive symmetric population bandwidth of 100,000 below (ineligible) and 100,000 above (eligible) MLF population cutoffs. In Figure 6, we show unconditional weekly mean yields (i.e. without covariate adjustments) separately by issuers narrowly eligible for MLF (blue dashed series), and similar-sized issuers that narrowly miss MLF eligibility (red solid line).

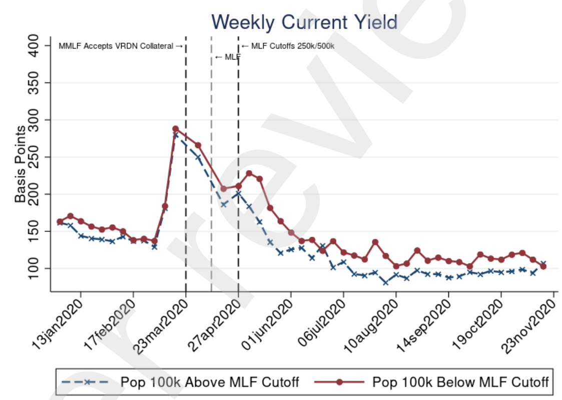
Figure 6: Mean Yields within 100k Population of MLF Eligibility Cutoffs

By geography:

(a) Cities:

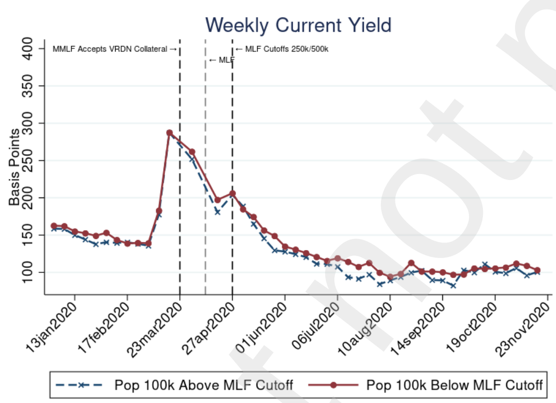


(b) Counties:

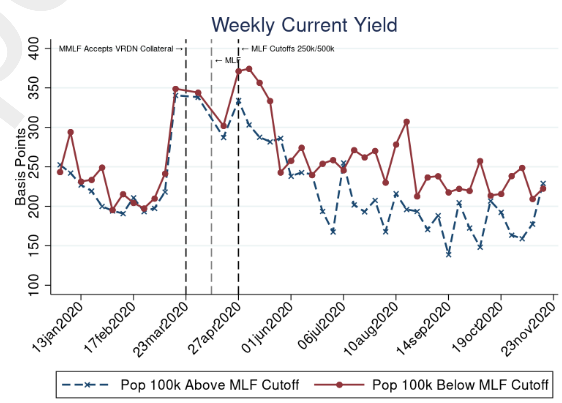


By credit worthiness:

(c) High-Rated (AAA & AA):



(d) Low-Rated (A & BBB):



NOTES—Figure shows mean weekly yields (pooled over buyer and seller prices) for eligible issuers with cutoff-relative populations between 0 and 100,000 inclusively (blue dashed series), and issuers with cutoff-relative populations greater than or equal to -100,000 and less than 0 (red solid series). Trades between announcements are pooled into a single period beginning on the announcement day. See text for sample restrictions and definitions. *Source:* MSRB, S&P, Moody's, Fitch

Panels (a) and (b) show city and county yields exhibited relatively parallel pretrends that rose together during the liquidity crisis in the run up to the first Federal intervention on March 23rd (left black dashed line).[35] Cities and counties continued to track each other throughout the initial MLF announcement (middle gray dashed line) until the announcement of the April 27 cutoffs—the main

[35]This is a necessary condition for any regression discontinuity design as the bandwidth approaches 0 in the limit; here we already begin to see observational equivalence (in the dependent variable) with an arbitrary symmetric cutoff of 100,000.

focus of this design.[36] Panel (a) shows investor-perceived measures of MLF access (yields) did not differentially materialize in cities after the third announcement's cutoffs were mandated. While there appears to be mild visual evidence that narrowly ineligible county bonds began trading at lower prices (higher yields) than eligible counties after the third announcement, this difference in not statistically significant in formal regressions. Both plots show full resumption to pre-crisis levels in the long run.

However, turning to panels (c) and (d), the main results of the paper become apparent from simply inspecting the path of these means across the ratings distribution. In these panels and subsequent analyses, we pool the two highest and two lowest investment-grade issuer plurality ratings from January 2020 (fixing issuer ratings at their pre-crisis levels) for statistical power—a choice motivated by the sparse BBB mass shown in panel (b) of Figure 5. While high-rated bonds share the same pattern as the overall market, among lower-rated bonds, a strong wedge begins to emerge exactly when the new cutoffs are made binding, immediately after the third announcement. Importantly, the timing of the yield wedge (exactly in the week the revision was announced) also minimizes concerns that chosen population cutoffs may have been used for other COVID-related policies, which would have appeared differentially in the pretrend before April 27. Of greatest concern, the March 27 CARES Act Coronavirus Relief Fund (CRF) also used a direct aid cutoff of 500,000 for both cities and counties—however both panel (b) which focuses on counties eligible at both dates, and the main effects in panel (d), provide initial evidence that yields did not respond to direct CARES aid, instead only responding to the MLF cutoff expansion.[37]

Analyzing the levels of these means, low-rated MLF-*ineligible* bonds (red series) remain significantly distressed beyond the last Federal intervention, in line with the aggregate BBB municipal market trend initially presented in Figure 1. By contrast, low-rated MLF-*eligible* bonds appear to recover fully to pre-crisis levels, mimicking the behavior of high-rated bonds when supported by the MLF.[38] In some weeks, this wedge appears to be larger than 100 basis points—roughly half the pre-crisis spread level. The question of how to interpret these differential effects that are only present among low-rated bonds, forms the basis of our formal analysis, and is discussed in Section 4.

Sample Restrictions

The above plots apply three sample restrictions, which are also applied throughout the estimation section: (1) we two-tail winsorize all yields at the 1% level; (2) we drop all yields with likely classical

[36]To ensure bin means do not overlap announcements, we mean-collapse all yields that fall within inter-announcement intervals to a single observation centered at each interval's midpoint (inclusive of the Monday of the each announcement).

[37]We probe this initial evidence more formally in subsequent robustness tests below.

[38]This pattern also emerges strongly when looking separately at A and BBB categories, as we show in additional results in Figure A.4 of Appendix A.7. We also examine whether selection on composition can explain these results in Figure A.8, which shows that while there is some evidence that revenue bonds are traded disproportionately following the announcement, this pattern is not sustained throughout the series.

measurement error (prices less than 50, and greater than 150, following Green et al. (2010));[39] (3) we drop any secondary market trades occurring within 60 days of primary issuance following Green et al. (2007a).[40] After imposing these restrictions, the pooled low-ratings sample (the smallest in our analysis and thus the one warranting the most power concerns) is comprised of 758 issuers, 7,207 bonds, and 94,104 trades in the post-period; with 4.76% of bonds falling with 100,000 population above the cutoff, and 3.54% within 100,000 below the cutoff.[41]

3.3 Public Sector Employment Data

We leverage an additional source to estimate effects on real-economy outcomes: local public sector employment and payroll by county-month and sector at eligible and non-eligible localities.

QCEW Government Employment data: We use the May 26, 2021 revised release of government employment data from the U.S. Census Bureau Quarterly Census of Employment and Wages (QCEW), which includes a monthly stock of all workers that were employed on the 12th day of the month through the end of 2020Q4 (through December 2020). The QCEW is based on ES-202 unemployment insurance filings at the state level, required for filing payroll taxes for federal UI-covered workers, so our sample requires a minimum level of part-time work for an employee to be included. This data covers roughly 90% of U.S. employees, excluding self-employed workers, some agricultural workers, and informal workers, and roughly 96% of state and local government workers (BLS-QCEW).[42] The QCEW is unique in its ability to provide a census of employees at a relatively granular sector-by-geography resolution. The most disaggregated geography in this data is at the county level, however public sector employees are also broken out by level of government: we use *Local Government Employees* (Ownership Title = 3) employed in *Total, All Industries* (NAICS = 10) as our preferred measure, which excludes state and federal employees but aggregates city and county employees at the county level. For roughly 67% of the data, sector-specific components of government employment are readily available, and allow us to separately estimate effects for goods-producing (NAICS = 101) and service-providing (NAICS = 102) public employees, the latter of which is largely comprised of *Education and Health Services* employees (NAICS = 1025).[43]

[39] Recall that prices at par, for the most part, vary closely around 100 at time of initial issuance.

[40] The results in Figure 6 are nearly identical without these restrictions.

[41] In Appendix A.6, we further enumerate the list of 43 A and BBB city and county issuers that fall within 100,000 in population of the cutoff, along with the number of bonds issued for each.

[42] Contractors are also covered by the QCEW (BLS), and reflect about 10% of local public sector employment (the sample studied in this paper), and include goods-producing employees. See Hyman (2018) for an explanation of sample coverage using similar data.

[43] We are able to confirm for this subset, that goods and services sector sub-components add up to aggregate employment totals, and thus consider these reliable. Cells that do not add up are withheld "so as to protect the identifiable information of respondents", and coded to missing in QCEW data as a small cell disclosure protection (`https://www.bls.gov/cew/questions-and-answers.htm` question 9).

4 Empirical Framework

We proceed with two complementary empirical strategies: a regression discontinuity (RD) design to estimate MLF access effects, and a spread decomposition approach to support our proposed mechanism hypothesis and generalize to the broader market. Here we discuss the former.

4.1 Regression Discontinuity Design with MLF-Eligible Populations

We estimate the option value of MLF access using a pooled regression discontinuity strategy, analyzing effects in two distinct periods: the pre-period (prior to March 23, 2020) and the post-period (after April 27, 2020), corresponding to pre-crisis or "placebo" and post-cutoff revision or "treatment" groups.[44]

$$\begin{aligned} Y_{n(bi)t} &= \alpha + \beta_t * \mathbb{1}(pop \geq cutoff)_i + \gamma_t * (pop - cutoff)_i \\ &+ \delta_t * \mathbb{1}(pop \geq cutoff)_i (pop - cutoff)_i + \mathbf{X}_{bit} + \varepsilon_{n(bi)t} \end{aligned} \quad (1)$$

Here, we track outcome $Y \in \{$Yields, 1(Primary Issuance), 1(Credit Downgrade), Public Sector Employment$\}$. When the outcome is yields, we measure trade n of bond b from issuer i in period t. $1(pop \geq cutoff)$ is an indicator variable that takes a value of 1 if the running variable (population relative to cutoff) is greater than or equal to zero. We use "relative population" as the running variable to be able to stack cities and counties with different cutoffs together in one specification. The γ and δ terms estimate separate polynomials slopes on each side of the cutoff, and capture the parametric relationship between city/county size and the outcome variable. α estimates the cutoff-intercept of the γ term's slope from the left-hand side, and has the convenient interpretation of representing the control group (MLF-ineligible) mean close to the cutoff. β_t is the parameter of interest, and estimates the difference in intercepts between the left polynomial and right polynomial at the cutoff, or the jump in the regression function at the cutoff.[45]

We follow Calonico et al. (2014) and Gray et al. (2020), in using a data-driven optimal bandwidth selection procedure to select asymmetric bandwidths on either side of the cutoff for estimating each polynomial, which subsequently helps determine the parameter of interest.[46] The procedure chooses bandwidth boundaries that minimize the integrated mean-squared error (IMSE) of the regression, which is linearly separable in bias and variance terms that are traded off in the bandwidth selection (smaller

[44] In Figure A.5 of Appendix A.7, we also provide estimates from a dynamic monthly RD specification.

[45] For continuous running variables, this builds on a well known statement that if the average potential outcomes are continuous functions of the running variable at the cutoff, the difference between the limits of the observed outcomes of two groups (as the score converges to the cutoff) is in fact the average treatment effect Hahn et al. (2001).

[46] This method takes one choice parameter—bandwidth—out of the econometrician's hands, thus minimizing susceptibility to publication bias and "p-hacking".

bandwidths are less biased but higher variance; larger bandwidths are more precise but also more biased). We use a non-parametric first-order polynomial (hence the linear terms) with an asymmetric triangular kernel that weights observations closer to the cutoff with values closer to 1, and diminishing weights moving away from the cutoff (and weight=0 beyond the bandwidth boundary).[47] In practice, the choice of kernel has little sway over regression results, while the choice of bandwidth can be massively influential (Cattaneo et al. (2019a)). While a consistent estimate of β_t does not require covariates to be identified (and in fact, should be robust to estimation without covariates), in small samples, the precision of estimates can be improved by their inclusion. We thus include the **X** vector to account for time-invariant bond and issuer characteristics in subsequent regressions, however all results are robust to their exclusion. Lastly, we conservatively cluster standard errors on the running variable (relative population) so that inference is drawn from between-issuer differences rather than within-issuer—consistent with recommendations for a discrete running variable (Frandsen, 2017).

Importantly, we also estimate Equation 1 separately for the four pre-crisis plurality credit ratings types of issuers (AAA, AA, A and BBB), which we interpret as a reduced form test of risk-sharing. As discussed earlier, in our main estimation, we pool the two highest and two lowest investment-grade issuer plurality ratings from January 2020 for statistical power. Our hypothesis is that if the Federal sector directly shares credit-downgrade or credit-default risk, one would expect lower rated eligible and non-eligible issuers to have stronger price spreads due to comprising a higher share of bonds on the default margin. If instead there are neutral effects, this direct access most likely reflects an "added liquidity effect" on top what is already happening in the aggregate.

4.1.1 Identifying Assumptions and Balance Test

The key identifying assumption of the RD is that potential outcomes are smooth across the relative MLF eligibility population cutoffs, absent the cutoff announcement itself.[48] Any selection on the running variable could violate this via the "no manipulation" or "no sorting" requirement, which helps ensure that the RD is locally as good as random. However as we showed, this is unlikely to be problematic in our setting. Like randomized controlled trials however, the identifying assumption has a natural falsification test that helps support the validity of the assumption, should baseline data be available prior to the experiment itself.

In our context, we have collected many months of a pre-period placebo outcome data (prior to March 23, 2020), in which we can formally test this assumption on our trade-level dataset. In Table 1, we

[47] We provide the implied kernel weights from the IMSE-optimal bandwidth selection procedure in Figure A.6 of Appendix A.7.

[48] For a discrete running variable, the "local continuity" assumption is softened to "local randomization".

estimate Equation 1 for the pooled city and county sample in the period from Jan 1, 2020 to March 23, 2020, where each row reflects a separate regression in which a new optimal bandwidth is selected depending on the outcome variable, thus producing different observation counts per variable.

Table 1: RD Balance Table in Placebo Period

	Discontinuity	Standard Error	Control Mean	N (IMSE-bwdth)
Coupon Rate (b.p.)	-23.37	17.53	435.36	99,073
Security Price (per 100 par)	-1.97	1.12	109.95	109,817
Current Yield (b.p.)	-6.13	16.23	185.81	70,569
Δ Yield (Feb20-Jan20)	-0.03	0.05	-0.10	66,349
Δ Yield YoY (Jan20-Jan19)	-0.23	0.20	-0.94	54,808
Δ Yield YoY (Feb20-Feb19)	0.04	0.11	-1.01	53,284
Amount Outstanding (MM)	-19.40	191.01	474.46	90,172
Maturity Size (MM)	11.99	268.49	708.56	88,918
Tenor of Bond (Years)	0.35	0.90	12.89	111383
Remaining Duration of Bond (Years)	-0.20	0.86	8.26	96,340
Market Share of Issuer	0.03	0.10	0.18	91,623
Number of Securities by Issuer	-29.78	43.29	228.10	129,872
Par Traded of Bond (1000s)	38.40	35.91	91.70	94,622
S&P Ratings (1-7 scale)	0.08	0.14	5.60	89,519
Moody's Ratings (1-7 scale)	0.13	0.18	5.72	87,454
Fitch Ratings (1-7 scale)	-0.03	0.17	5.69	74,930
Time of Day of Trade (minute)	10.62**	4.85	768.33	101,467

NOTES—Table presents balance tests of covariates for MSRB active trades during the pre-period for the pooled sample. Each row corresponds to a separate regression with that characteristic as the dependent variable. The discontinuity measures the jump in the regression function at population eligibility 0 (relative to cutoff). The Control Mean denotes the mean of that characteristic immediately to the left of relative population 0. Each regression uses IMSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a triangular kernel to weight observations. Sample sizes vary depending on the bandwidth used. See Appendix A.4 for further details on variable definitions and construction. Standard errors are clustered by population relative to the cutoff (the running variable). *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. *Source:* MSRB, Bloomberg, S&P, Moody's, Fitch

Each regression is estimated unconditionally, without adding covariates on the right-hand side. "Discontinuity" and "Control Mean" columns correspond to β and α respectively from Equation 1.[49] Among the dependent variables listed, there are only few cases in which a significant break is detected in the placebo sample. When they are detected, the control mean suggests that they are not economically meaningful or large in magnitude, and thus simply a result of randomization error. Furthermore, estimates on the important yield variables, which include both a month-to-month and year-on-year trend variable, are particularly small relative to their control means.[50] While randomization errors are more likely to occur within smaller samples, such as estimates within lower-rated bins, the combination of balance in the placebo period with relatively parallel pretrends in dependent variables shown earlier,

[49] A full set of summary statistics can be found in Table A.5 of Appendix A.7.

[50] For more details on the how the remaining variables are constructed, see Appendix A.4.

suggests that the local randomization assumptions hold in this setting.

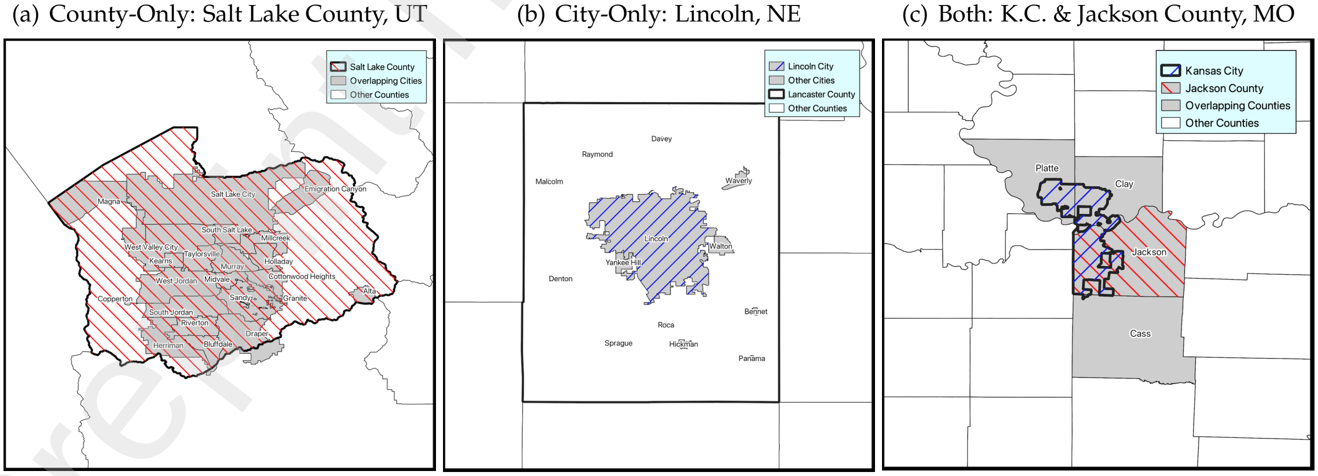
4.1.2 Differenced RD Modification for Employment Effects

We also desire to estimate the effects of MLF eligibility and related CARES Act aid on city and county public employment outcomes in our RD framework. However, data limitations only allow us to observe local public employment *combined* for city and county employees at the county-month level. That is, while the QCEW provides exceptional coverage of employment and breakdowns by sector, it does not decompose total local public employment into its city and county sub-components—consequently, any effort to estimate RD effects for counties could be made difficult by the possibility that an untreated county's public employment to the left of the county population cutoff is partially covered by an MLF-eligible *city*; symmetrically, an untreated city could be covered by an MLF-eligible county. Both of these cases would attenuate relative employment effects toward 0.

For these reasons, we opt for a strategy comparing combined city and county eligible *counties* on the right of the cutoff, with "neither-eligible" counties to the left of the cutoff, and propose a novel adaption in the spirit of "difference in discontinuity designs" to difference out any unintended selection concerns resulting from this measurement strategy. We first pre-sort cities and counties into four cases: (a) the county itself is the only MLF-eligible issuer within the entities spanned by the county; (b) only 1 city is MLF-eligible within the county, while the county itself is ineligible; (c) both the county and one of its underlying cities are MLF-eligible; (d) neither the county nor any of its contained cities are MLF-eligible. We show representative maps for cases (a), (b) and (c) in Figure 7 below.

Figure 7: Differenced RD Design with QCEW County-Level Employment: Three Treatment Cases

(a) County-Only: Salt Lake County, UT — (b) City-Only: Lincoln, NE — (c) Both: K.C. & Jackson County, MO



NOTES—In panel (a), Salt Lake County, UT, is eligible while none of its overlapping cities (nor unincorporated areas) is eligible; in panel (b), Lincoln, NE is eligible, but none of the other cities nor the county containing it (Lancaster County) are eligible; in panel (c), both Kansas City, MO, and Jackson County, MO, are eligible, and combined they span a super-region of four counties.

In our preferred strategy to estimate employment effects, we combine all case (a) "county-only" types (spanning 59/119 MLF-eligible counties) with case (c) "double-eligible" types (spanning 60/119 eligible counties) on the right of the cutoff of the *county* running variable, and compare these with case (d) "neither-eligible" types to the left of the cutoff along the same running variable.[51] The modified RD identification assumption is that potential outcomes are smooth around each running variable's *modified* sample. To the extent that these pre-sorted samples introduce further selection concerns, and to address the potential problem that any RD randomization error could magnify level differences emanating from differently sized geographies around the cutoff in these modified samples, our preferred specifications consider the first-difference change and percent change in employment (year-on-year) rather than its level. Year-on-year changes in employment in a given month should difference out any aggregation errors arising from this sorting procedure, and as county employment levels have a long right tail, focusing on changes also help reduce noise in these estimates.

In all employment regressions, we depart from our IMSE-optimal bandwidth selection procedure and instead use a fixed bandwidth due to the much smaller number of observations—an N between 700 and 900 (whereas yields RDs were estimated from tens and sometimes hundreds of thousands of observations). We choose a fixed bandwidth of 400,000 below the cutoff, and 600,000 above the cutoff, allowing us to take advantage of a full window size that spans localities of 1 million in population (between 100,000 and 1.1 million). As before, we employ triangular kernel that weights observations near the cutoff close to 1, and observations at the bandwidth boundary equal to 0. As we did for yields, we two-tail winsorize employment values at the 0.1% level prior to taking first differences. For consistency with yields results, we include state fixed effects in some regressions (and month fixed effects with the outcome is not year-on-year first differenced), however all employment effects are robust to being estimated unconditionally without covariates.

5 Results

We first present results from our regression discontinuity strategy on the effects of the MLF on secondary market yields, primary issuance, and real economy outcomes.

[51] In Appendix A.7, we also separately compare case (b) cities (spanning 21/86 eligible cities) against case (d) types along the *city* running variable (which requires constructing city employment by calculating the employment weight of each county spanned by the city). This sample has the added appeal that the city MLF eligibility cutoff (250,000) is distinct from the CARES eligibility cutoff (500,000), however power calculations in the Appendix show we are unable to detect large effect sizes. Furthermore, estimating effects on the city running variable also requires strong assumptions to assign county employment weights to multiple *untreated* cities within a given county.

5.1 RD Effects on Secondary Market Yields

Table 2 presents our main results on the investor-perceived option value of access to the MLF, estimated from secondary market municipal bond yields (in basis points).

Table 2: RD Estimates of MLF Access on Secondary Market Yields

	Discontinuity	Standard Error	Control Mean	N (IMSE-bwdth)
a. Pooled Post:				
Current Yield (Overall)	-19.26	19.51	156.77	187,976
City Only	-27.97	25.59	172.03	91,628
County Only	-15.06	23.74	134.06	53,874
High-Rated (AAA & AA)	-1.54	7.66	124.97	178,256
Low-Rated (A & BBB)	-72.28**	33.05	305.73	38,299
b. Pooled Pre (Placebo):				
Current Yield (Overall)	-13.25	12.17	194.13	70,569
City Only	-9.13	12.16	196.30	32,853
County Only	-21.61	19.17	191.88	28,325
High-Rated (AAA & AA)	-6.15	7.86	177.44	67,169
Low-Rated (A & BBB)	-24.87	21.45	263.11	12,756

NOTES—Table presents RD estimates of current yields (in basis points) for MSRB-active trades during each sample period. Each row corresponds to a separate regression with that characteristic as the dependent variable. The discontinuity measures the jump in the regression function at population eligibility 0 (relative to cutoff). The Control Mean denotes the mean of that characteristic immediately to the left of relative population 0. Each regression uses IMSE-optimal bandwidths calculated separately for each side of the cutoff and for each outcome, and a triangular kernel to weight observations. Sample sizes vary depending on the bandwidth used. State and calendar month fixed effects are included in all regressions. Standard errors are clustered by population relative to the cutoff (the running variable). See text for further sample restrictions. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$.

Panel (a) pools all trades post-April 27 into one treatment sample (reducing volatility), whereas panel (b) pools all pre-March 23 observations into one placebo sample (starting from January 1, 2020). Each row representatives a different sub-sample of issuers and separate regression, as discussed above. As before, the "discontinuity" and "control mean" columns correspond to β and α from Equation 1 respectively. In this regression table, we implement the sampling restrictions for yields discussed in Section 3, and include calendar month fixed effects to absorb variation in overall levels, and state fixed effects to reduce variability from differences in exemption rules and budget balance requirements (which can limit downstreaming) across states.[52]

Consistent with the event study plots, we fail to detect a statistically significant differential effect between eligible and ineligible issuers in the post-period overall, nor by city or county. The highest rated issuers appear to also be unaffected by the announcement of MLF eligibility.[53] Moving down

[52]State fixed effects are estimated using all data within the optimal bandwidth, spanning close to 758 issuers (the total number in the combined A and BBB group.)

[53]In regressions by ratings bin, we pool cities and counties together for statistical power.

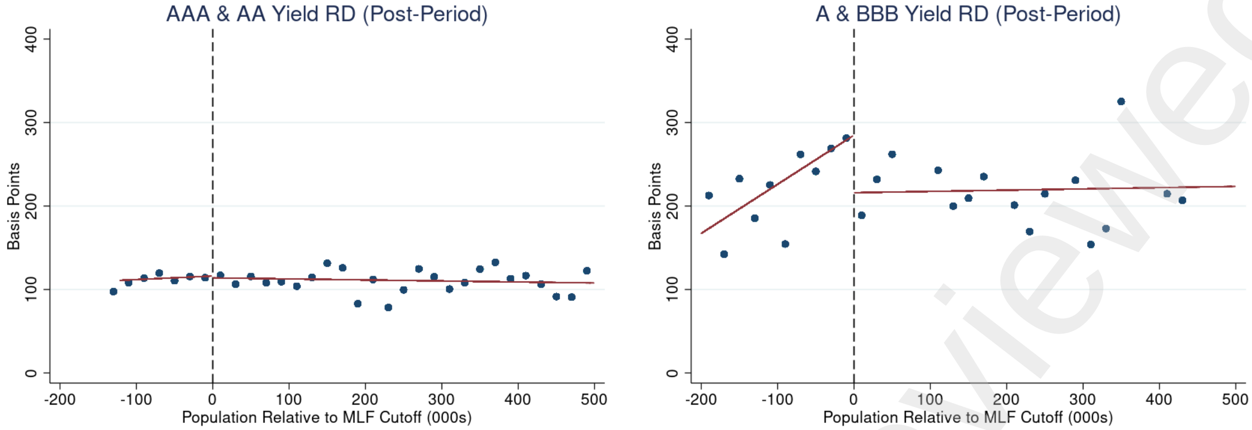
the ratings gradient however, we estimate a strong significantly significant yield spread for low-rated issuers. The point estimate suggests that narrowly MLF-eligible issuer bonds trade, on average, 72 basis points lower (i.e. at a higher price) than observationally equivalent issuers that narrowly miss the cutoff. This wedge represents about 23% to 28% of baseline yields (depending on whether one benchmarks to the pre-crisis (263.11) or post-crisis (305.73) period respectively), a magnitude that closely corresponds to the overall municipal market wedge between BBB yields and higher-rated issuers as shown initially in Figure 1. In fact, when estimated separately by BBB and A, effects among BBB-rated issuers are even stronger than this average (see additional results in Figure A.4 of Appendix A.7).

It is worth highlighting that including state fixed effects in our main specification also implicitly tax-adjusts yield estimates—since the above table is restricted to 2020, state fixed effects would account for any differential state tax rates and exemption rules on muni income accrued to the marginal investor residing in each state, who is presumed to take advantage of doubly-exempt bonds (i.e. muni interest income that is exempt from federal *and* state tax liabilities). In Section 6.3, we report the sensitivity of our RD results to directly adjusting tax-exempt bonds using NBER-TAXSIM effective rates for a representative high-wealth household investor in each state, as well as excluding state and month fixed effects, all of which show our results are qualitatively robust to excluding all covariates and do not rely on any specific tax adjustment method.[54]

Encouragingly, for all subgroups, estimates in the pre-period are close to zero. This suggests that balance is achieved even among these smaller sample sizes using only a parsimonious number of standard controls (state and month fixed effects). To explore the precision and sensitivity of our preferred estimates transparently, below we show RD scatter plots associated with high- and low-rated issuers.

[54]NBER-TAXSIM provides these rates net of local cross-deductibility rules, including mortgage interest deductions. The latest rates provided are for 2018, which we assume are the effective rates salient in 2020. See 2018 "Wages Federal Rate" for the effective rate applied to federal-only exempt bonds, and "Wages Total Rate" for the sum of federal and state effective tax rates applied to each state's underlying bonds (https://users.nber.org/~taxsim/state-rates/maxrate.html).

Figure 8: RD Plots of MLF Access Effect on Yields by Credit Worthiness



NOTES—Plots show regression slopes and intercepts from Equation 1 in the post period, overlaid on top of equally spaced pre-binned outcome data with a bin size of 20 (x-axis in thousands). Plots are shown over the optimal bandwidth selected using the IMSE-procedure, which produces (potentially) asymmetric optimal bandwidth boundaries for each sample. Plots correspond to Table 2, which includes state fixed effects as controls. Plot thus residualizes all yields by state-fixed effects (added back to their overall mean) prior to mean-collapsing by bin.

Here, we begin to see the pattern of which issuers investors appear most responsive to across the relative population distribution (the running variable). Among high-rated bonds, yields are neutral across city (and county) size distributions. The neutrality is preserved for low-rated issuers that are MLF-eligible, yet for the ineligible issuers, an upward-sloping relationship appears in which larger locations have higher yields, but only up until the cutoff. These estimates remain nearly identical when using tax-adjusted yields, and controlling for bond type (general obligation versus revenue bond), tenor length, remaining bond duration, as well as trade week, day of week, maturity size, and amount outstanding.[55] To the extent that there are concerns that the RD is picking up underlying volatility beyond these controls, we note that RD polynomial terms on either side of the cutoff account for any remaining differential volatility by relative population. This is further confirmed by placebo RD plots in Section 6 which do not show any strong pre-existing pattern in volatility.[56]

We interpret these results as suggestive of a setting in which pre-existing uncertainty in the pricing environment for low-rated municipal issuers (those with more bonds on the margin of default) may have been amplified by the pandemic. This credit risk seems to have been priced differentially by investors, depending on whether issuers had the ability to borrow from the Federal sector as a last resort lender.

[55]See sensitivity table shown in Section 6.3.

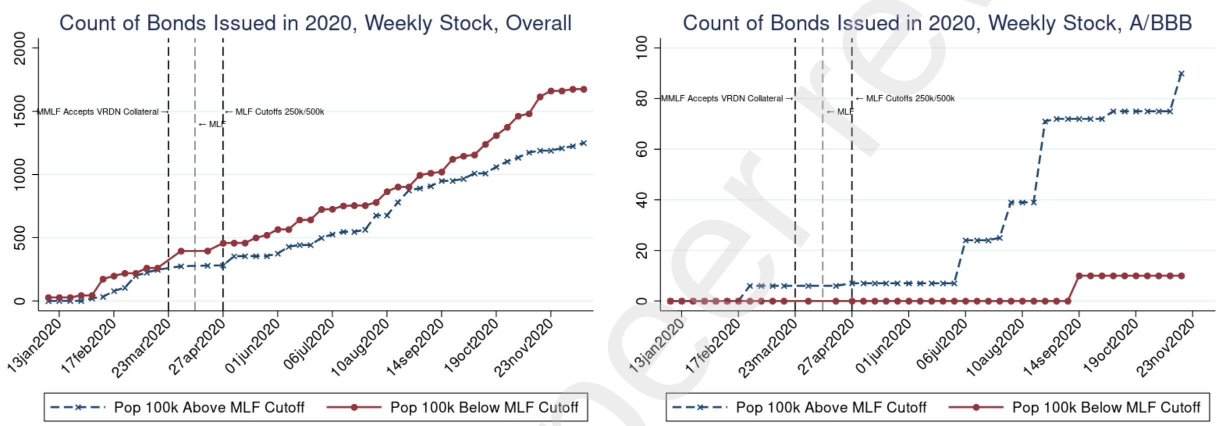
[56]In ongoing work, we are intend to further explore the influence of pre-refunded bonds and volume-weighted weights on the overall results.

5.2 RD Effects on Primary Issuance

In Figure 9 below, we first visually inspect whether investor perceptions which created a wedge in secondary market yields also induced additional new primary issuance of municipal notes or bonds, differentially for MLF-eligible issuers. Combining all new issuance from MSRB and Mergent data, we first show graphically the cumulative stock of bonds that were newly issued (defined here as having a first offering during calendar year 2020), by each week of the 2020 calendar year.[57]

Figure 9: New Primary Issuance within 100k Population of MLF Eligibility Cutoffs

(a) Cumulative New Issuance, All Ratings (b) Cumulative New Issuance, Low-Rated (A & BBB)



NOTES—Plots show new primary issuance as cumulative counts of newly issued bonds by MLF-eligible and ineligible issuers within a symmetric 100,000 population bandwidth. See text for further details. *Source:* MSRB, Mergent, Bloomberg

Panel (a) shows that issuers of all ratings (overall mean) within the symmetric bandwidth above and below the pooled cutoff shared roughly equivalent trends in new issuance, with a slightly higher level among lower population issuers due to the greater density of cities and counties of smaller populations versus larger populations. While overall issuance does appear to converge later on in September (where the two series intersect), this is not sustained as a strong pattern in the long run. By contrast, turning to low-rated issuers whose bonds we confirmed were trading at higher prices after the MLF cutoffs were revised in late April, we see a different pattern. First, the baseline level of new issuance in panel (b) is close to zero from January to July of 2020 in both samples due to the high inherent distress levels of these issuers, with the exception of one issuer above the cutoff that issued a series of differently-maturing bonds prior to the pandemic. In July, we observe bonds narrowly above the cutoff beginning to issue sooner, followed by a step function pattern of new issuances throughout 2020. The deferred timing of this issuance (in July) relative to the immediately binding effects on yields, may be linked to the beginning

[57] We choose a cumulative approach due to the lumpy nature and sparsity of primary issuance, however also formally test the probability that a bond is newly issued separately in the pre- and post-MLF periods below.

of fiscal budget cycles, or coincide with the deferred IRS tax filing deadline of July 15 which may have revealed a clearer picture of municipal revenue positions that put new issuance into motion.[58]

Importantly, only 5 low-rated city and county governments issued new bonds after the MLF cutoffs were announced, 4 issuers (on a denominator of 23) above the cutoff and 1 issuer (on a denominator of 20 issuers) below the cutoff (see Appendix A.6 for denominators). With the caveat that this does not provide bountiful variation to pick out a small signal from the noise, we now show the formal RD estimation associated with the probability that a bond was *ever* newly issued after (and before) the MLF cutoffs were announced. To do this we collapse each CUSIP (bond or note) into a single post- and pre-MLF period observation, and assign each bond a value of 1 if it was newly issued during that period, and 0 otherwise. Here, we depart from optimal bandwidths due to the limited size of the collapsed dataset, and show results without covariates for transparency. Each regression uses a fixed bandwidth of 400,000 below and 600,000 above the cutoff, allowing us to take advantage of a full window size that spans localities of 1 million in population (between 100,000 and 1.1 million), and a triangular kernel to weight observations close to the cutoff with weight=1, and closer to the bandwidth boundary with weight=0.

Table 3: RD Estimates of MLF Access on New Primary Issuance

	Discontinuity	Standard Error	Control Mean	N (Fixed-bwdth)
a. Pooled Post:				
Prob(CUSIP Issued in 27apr-20nov), Overall	0.08**	0.04	0.11	83,100
Prob(CUSIP Issued in 27apr-20nov), A & BBB	0.25**	0.11	-0.08	7,753
b. Pooled Pre (Placebo):				
Prob(CUSIP Issued in 01jan-23mar), Overall	0.07**	0.03	0.02	45,451
Prob(CUSIP Issued in 01jan-23mar), A & BBB	0.11	0.08	-0.10	3,977

NOTES—Table presents RD estimates of the probability of new primary issuance in each period. Each row corresponds to a separate regression with that characteristic as the dependent variable. The discontinuity measures the jump in the regression function at population eligibility 0 (relative to cutoff). The Control Mean denotes the mean of that characteristic immediately to the left of relative population 0. Sample sizes vary depending on the bandwidth used. Standard errors are clustered by population relative to the cutoff (the running variable). See text for further sample restrictions. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$.

Table 3 shows that the probability a bond is issued increases by 8 percentage points (on a baseline share of 11 percent), which is largely driven by an increase of 25 percentage points among low-rated bonds. Due to the sparse nature of primary issuance, we do not have ample variation to disentangle these effects completely from differential issuance in the placebo period—indeed, in panel (b) larger municipalities are 7 percentage points more likely to issue in the pre-period relative to marginally smaller ones—however rather than interpreting the point estimates here directly, we take this as strong qualitative evidence that having MLF optionality encouraged primary issuance on private markets, and

[58]To rule out seasonal mean reversion in primary issuance as a key driver of these results, we also report the distribution of 2019 new issuance as a benchmark expansion year in Figure A.7 of Appendix A.7, which does not provide any evidence that seasonal mean reversion is a key factor.

investors priced that ability to issue in secondary markets as shown earlier.

5.3 Differenced RD Effects on Public Sector Employment

We next turn to effects on local public sector employment using the differenced RD modification strategy discussed above. In Table 4 below, we combine all case (a) "county-only" and case (c) "double-eligible" types (spanning 60/119 eligible counties) on the right of the cutoff of the *county* running variable, and compare these with case (d) "neither-eligible" types to the left of the cutoff along the same running variable. Here again, each row represents a separate RD regression. As discussed above, unlike our yields analysis, here our preferred outcome variable is differenced year-on-year such that post-period results reflect pooled annual changes in county local government employment for May and June (between 2019 and 2020), while the placebo shows the same effects for differenced January and February pooled (columns (3) and (4)). As described previously, we estimate effects separately for goods- and service-providing public sector employees, however we can only do so for the 67% of the sample in which these sector breakdowns are reliably available, which results in different observation counts.

Table 4: RD Estimates of MLF Access on Public Sector Employment

	Emp. (1)	Emp. (2)	Δ Emp. (3)	Δ Emp. (4)	% Δ Emp. (5)	% Δ Emp. (6)	N (fixed-bwdth)
a. Pooled Post:							
Overall Employment	-477	323	325	297	1.19	1.18	945
	(1,016)	(1,854)	(239)	(223)	(0.96)	(0.83)	
– Goods Employment	-42	-49	1	2	2.61	3.93	248
	(63)	(37)	(5)	(5)	(3.85)	(4.27)	
– Services Employment	-412	-666	422*	517**	1.61	1.69**	711
	(1,134)	(2,001)	(238)	(242)	(1.00)	(0.85)	
b. Pooled Pre (Placebo):							
Overall Employment	-828	189	53	58	0.23	0.26	946
	(1,042)	(1,995)	(84)	(82)	(0.41)	(0.37)	
– Goods Employment	-44	-51	-1	-1	-0.38	0.51	248
	(64)	(38)	(3)	(3)	(2.23)	(2.76)	
– Services Employment	-762	-896	41	26	0.10	0.04	712
	(1,128)	(2,188)	(98)	(93)	(0.50)	(0.41)	
Month FEs		X					
State FEs		X		X		X	
Control Mean (post): Overall Employment	18,835	13,346	-1,717	-1,259	-8.06	-8.13	
Control Mean (post): Goods Employment	162	127	-7	-5	-6.44	-7.16	
Control Mean (post): Services Employment	18,506	14,054	-1,674	-1,349	-7.96	-8.58	

NOTES—Table presents RD estimates of employment measure in column header and sample period. Each row corresponds to a separate regression with that characteristic as the dependent variable. The discontinuity measures the jump in the regression function at population eligibility 0 (relative to cutoff). The Control Mean denotes the mean of that characteristic immediately to the left of relative population 0. Each regression uses a fixed bandwidth with a triangular kernel to weight observations (see Section 4 for details). Percents in columns (5) and (6) range from 0 to 100. Standard errors are conservatively clustered by population relative to the cutoff (the running variable); as such, inference is drawn across unique issuers. *** p≤0.01, ** p≤0.05, * p≤0.1.

We begin our interpretation by analyzing employment effects in levels in columns (1) and (2). Notably, while we fail to detect statistically significant MLF effects in levels, employment is highly volatile in the pre-crisis (placebo) period. Importantly, the control mean in these columns (marginally-ineligible counties) indicates our sample counties initially had roughly 18,835 local government employees on average—heavily skewed to the right by large counties.[59] The small effect sizes contrasted with large baseline means also suggest that any effects on levels are likely estimated with noise. Given this, in conjunction with our main selection concern that subsetting counties to isolate precise employment effects may generate unintended imbalances in baseline characteristics, we turn to using year-on-year differences in our preferred specification in column (3). Here, the unconditional control group shows counties experienced an average annual decline of roughly 1,717 local public sector employees in May and June of 2020 (relative to the same months in 2019).

While effects on overall employment are not significant at conventional levels, the point estimates for service-providing public employment sectors in columns (3) and (4) suggest MLF counties increased local government service employment by 422 to 517 employees relative to the control group. Interpreting estimates with respect to the control mean in column (3), this amounts to an average employfment decline of 1,200 to 1,295 employees relative to a baseline decline of 1,717 employees for ineligible counties—an effect size of 24.6% to 30.1%, indicating a striking degree of employment smoothing in response to emergency liquidity.[60]

Panels (a) and (b) in Figure 10 below, show RD scatter plots associated with our preferred estimates of employment changes (column (4) of Table 4), both overall and for service-providing sectors.[61]

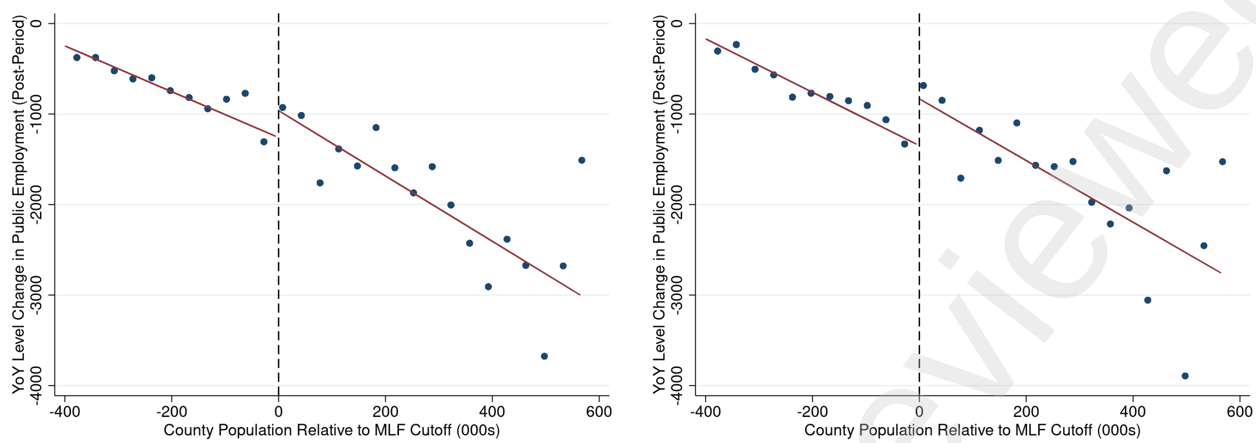
[59] Baseline means vary across models because the "control group" is the intercept of the RD polynomial from the left-hand side of the cutoff, which may vary when a control adjusts the optimal bandwidth over which the polynomial is projected.

[60] In percentages, column (6) shows a 1.69 percentage point gain in employees on a baseline loss of 7.96%; a similar effect size of 21.2%.

[61] RD plots associated with unconditional effects from column (3) of Table 4 yield nearly identical figures.

Figure 10: RD Plots of MLF Access Effect on Public Sector Employment

(a) Δ Overall Employment Post-MLF (b) Δ Services Employment Post-MLF



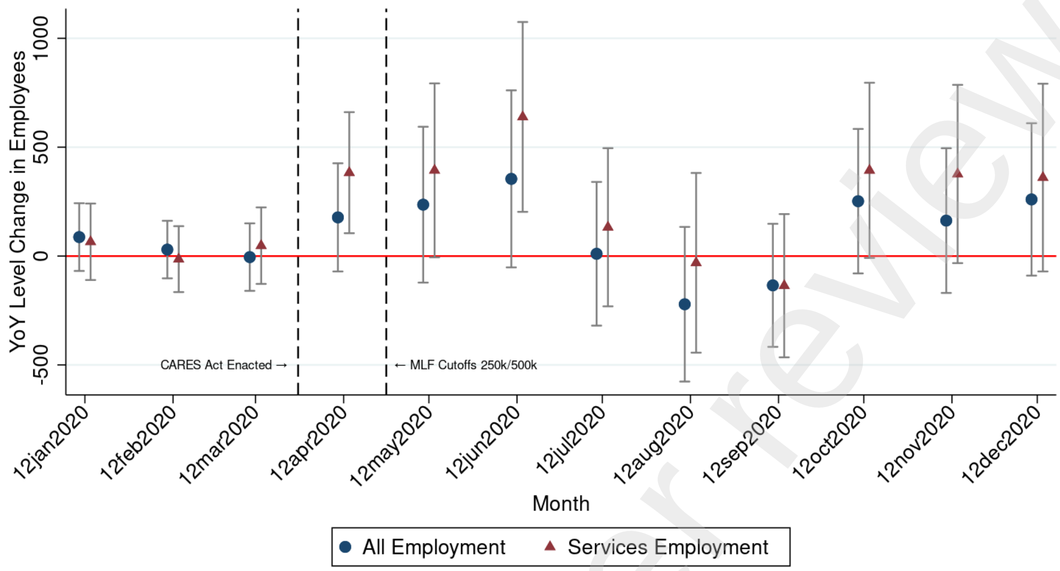
NOTES—Plots show regression slopes and intercepts from Equation 1 in the post-period (May and June 2020 pooled), overlaid on top of equally spaced pre-binned outcome data with a bin size of 35 (x-axis in thousands). Plots are shown over a fixed 1 million population wide bandwidth on the running variable. Plots first residualize employment of state-fixed effects (added back to their overall mean) prior to mean-collapsing by bin.

These plots first reveal a largely linear and negative employment-county size gradient for local public employees—the larger the locality, the larger the year-on-year level decline in employment. Interestingly, the same plots in the placebo period produce the opposite slope—the largest counties also had the largest growth in public employment in the run up to the pandemic, albeit with a milder gradient (see placebo effects in Section 6). While noisy due to the sparseness of the QCEW data, the plots reveal a modest upward level shift in overall employment (though not statistically significant at conventional levels in Table 4) when MLF liquidity is available, and a more pronounced shift for service-providing sectors, largely comprising employees at educational institutions.

5.3.1 Decomposition of MLF lending vs. CARES aid Employment Effects

To this point, we have interpreted May and June 2020 public sector employment effects at the April 27 MLF eligibility cutoffs (250,000 for cities, 500,000 for counties) as being fully attributable to the MLF emergency lending option. However, as noted at the outset, the CARES Act announced on March 27 also provided for direct aid eligibility for cities and counties with populations over 500,000 (whereas those under this cutoff had to rely on more uncertain state downstreaming), raising the possibility that earlier estimates capture a combination of both direct aid and lending eligibility—though it may be argued that both reflect similar liquidity interventions and thus likely share the same theoretical sign on employment. To begin to disentangle the separate contributions of MLF optionality and direct CARES Act aid, in Figure 11 we provide dynamic monthly post-period estimates corresponding to column (4) from Table 4, leveraging the differential timing of CARES and MLF cutoff announcements.

Figure 11: Differenced RD Effects on Local Government Employment by Month



NOTES—Figure plots RD coefficients and 90% confidence intervals estimated using the specification from Table 4 column (4), with QCEW 12th of the month employment data. See Table 4 notes for further details.

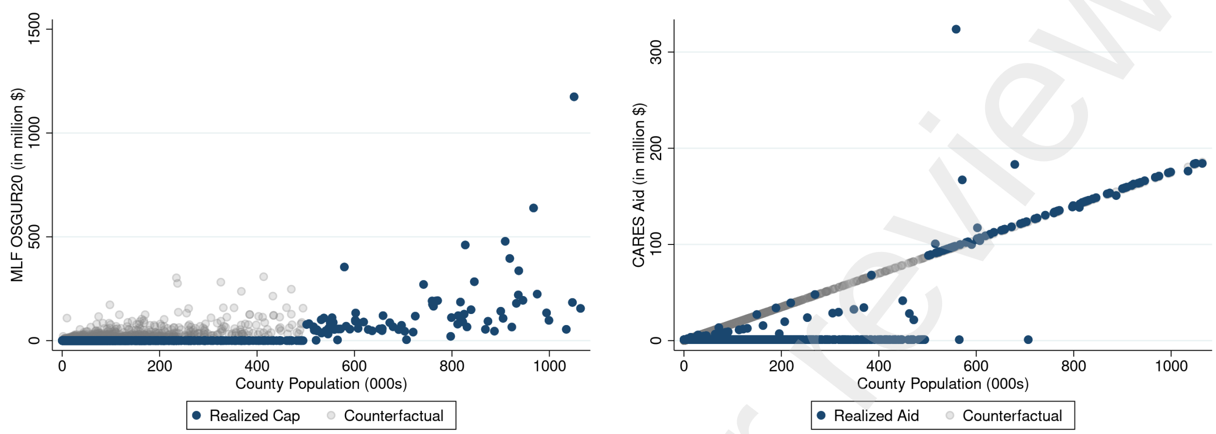
Plotting RD estimates dynamically presents an important observation on April 12th—employees recalled several weeks after the CARES provisions were enacted, but also several weeks before MLF population cutoffs were revised. In the data, all CRF aid was fully dispersed (and assumed to be salient to and anticipated by budget officers) between April 15 and May 6. We thus interpret the departure from zero that we detect on April 12th as the employment effect of anticipated (non-downstreamed) CARES aid, whereas May and June employment effects reflect the totality of all realized aid as well as the MLF option. The similar point estimates between April and May provide initial suggestive evidence that CARES aid may have been more material in affecting hiring and recall decisions, however the effects of the MLF expansion may also have taken some time to set in. These dynamics also reveal an interesting pattern related to the school-year calendar, a feature we return to when interpreting these effects. Finally, it bears mentioning that monthly estimates in placebo months (January to March) are also reassuringly concentrated around zero prior to the onset of the pandemic in the United States.

To more formally gauge the robustness of the result that CARES aid appears to be more consequential than the MLF lending option for hiring, we consider a decomposition which leverages variation in the dosage response of the MLF treatment relative to CARES aid. To see this variation clearly, in Figure 12, we first show variation in MLF lending caps, calculated as 20% of each government's 2017

”own-source general and utility revenue” (OSGUR) as per MLF regulations,[62] and the amount of direct aid apportioned to each government through the CARES Act based on the CRF population formula.[63]

Figure 12: Variation in MLF Lending Caps and direct CARES Aid Allocations

(a) MLF Lending Caps (20% of OSGUR) (b) CARES Aid CRF Allocation



NOTES—Panels use different scales. Blue filled observations in panel (a) show MLF lending caps (based on 2017 OSGUR) over the county running variable, where grey hollow observations indicate counterfactual cap amounts that would have been implemented were there no cutoff. Blue observations in panel (b) show realized direct aid allocations calculated from the CRF population formula (see footnote 59) plus $1, combined with data on downstreamed aid from states to counties on the left of the cutoff. Grey observations in panel (b) denote counterfactual aid were the population cutoff to have been perfectly binding across the entire running variable.

The two different scales of the y-axes in Figure 12 underscore that counties at the 500,000 cutoff were eligible for both an outsized amount of *potential* lending, and a much smaller but still material amount of direct aid, making comparisons difficult. While the kinked nature of the CARES population formula appears at first to provide a potential test in which a regression kink design (RKD) may help discern between these two policies, the underlying kink is in fact a flat schedule in the variation of interest—aid per person. That is, when both the outcome variable and treatment variable are highly correlated with the running variable (as it is here), there is in fact no kink in the denominator since the aid allocation formula is based on population itself, and therefore no meaningful variation in aid to leverage. Instead, we therefore opt for an alternative dosage response interaction test that uses variation in $\widehat{OSGUR}$ in panel (a) of Figure 12 (inclusive of counterfactual values, denoted with a hat), allowing us to compare effects at the cutoff for issuers that would have been eligible for more lending, relative to similar $\widehat{OSGUR}$

[62]See April 9, 2020, MLF Term Sheet for further details on MLF lending cap calculation.

[63]According to the CARES Act CRF Allocation Formula, no population can be double-covered by both its underlying city and county for aid, however each entity above the cutoff can independently claim direct aid on behalf of its constituents. We use direct CARES aid data received by either cities or counties from USASpending.gov (the US Treasury’s official tracker of fiscal expenditures). However, this analysis requires total county aid, which is comprised of direct county aid in addition to direct aid paid to a county’s underlying cities. Toward this end, we split the direct city aid across counties based on the county CRF cap were it to have accessed the aid directly. Because aid received is calculated with the “latest” population vintage, we estimate the county-level allocation with minor error. On the left of the cutoff, we are able to complement direct aid with indirect downstreamed aid to county governments from NACO, however do not have data on downstreamed aid to cities.

levels below the cutoff (indicated by grey dots to the left).

Toward this end, we alter our equation of interest slightly where g now denotes geography, and t the period of interest, and decompose the main effect into an overall level shift (β_{1t}) while controlling for the marginal effect of additional MLF lending dollars—the CARES effect when the $\widehat{OSGUR}20$ cap (20% of $\widehat{OSGUR}$) is set to zero—and an explicit term accounting for the marginal effect of an additional MLF cap dollar of lending (β_{2t}). Assuming the marginal MLF effect is economically meaningful relative to the indicator value of being announced as MLF-eligible, (β_{1t}) can be interpreted as the part of the total effect attributable to CARES aid.

$$\begin{aligned} Y_{gt} = \alpha &+ \beta_{1t} * \mathbb{1}(pop \geq cutoff)_g + \beta_{2t} * \mathbb{1}(pop \geq cutoff)_g \widehat{OSGUR}20_g + \gamma_t * (pop - cutoff)_g \\ &+ \delta_t * \mathbb{1}(pop \geq cutoff)_g (pop - cutoff)_g + \phi_t * \widehat{OSGUR}20_g + \mathbf{X}_{bit} + \varepsilon_{gt} \end{aligned} \tag{2}$$

In Table 5 below, we show the results from our dosage response test, where columns (1) and (4) use $\widehat{OSGUR}20$ as the main interaction term, (2) and (5) normalize $\widehat{OSGUR}20$ by realized CARES aid as indicated by filled blue observations in Figure 12 (adding 1 dollar to values equal to 0 left of the cutoff), and $\widehat{OSGUR}20$ normalized by counterfactual CARES aid were it to have been allocated formulaically across the entire running variable, including left of the cutoff. In all three of these alternative formulations using both of our two preferred outcomes, we fail to detect a statistically significant marginal effect of increasing the MLF cap relative to statistically significant effects on the level shift term associated with direct CARES aid. If we were to interpret the point estimates on the interaction term at face value, they would also only amount at most, to around 8% of the overall effect.

Table 5: MLF vs. CARES Dosage Response Decomposition of Employment Effects

	YoY Level Changes			YoY Percent Changes		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	-1473.0***	-1417.2***	-1412.3***	-10.09***	-9.967***	-9.400***
	(200.3)	(192.3)	(180.0)	(0.715)	(0.665)	(0.538)
1(pop $\geq$ cutoff)	620.3*	554.0*	531.9*	3.021**	2.959**	2.168**
	(266.3)	(272.2)	(261.2)	(0.930)	(0.933)	(0.810)
1(pop $\geq$ cutoff)*$\widehat{OSGUR}20$	-0.678			-0.0110		
	(1.408)			(0.00747)		
1(pop $\geq$ cutoff)*$\widehat{OSGUR}20/CARES$		39.88			0.143	
		(91.57)			(0.286)	
1(pop $\geq$ cutoff)*$\widehat{OSGUR}20/\widehat{CARES}$			36.71			-0.104
			(140.6)			(0.461)
(pop - cutoff)	-3.279***	-3.164***	-3.074***	-0.00587**	-0.00548*	-0.00311
	(0.681)	(0.670)	(0.644)	(0.00220)	(0.00215)	(0.00214)
1(pop $\geq$ cutoff)*(pop - cutoff)	-0.227	-0.218	-0.384	0.00269	0.00391	0.000710
	(1.173)	(1.113)	(1.102)	(0.00353)	(0.00327)	(0.00334)
$\widehat{OSGUR}20$	1.135			0.0153*		
	(0.998)			(0.00713)		
$\widehat{OSGUR}20/CARES$		0.594			0.0156*	
		(0.993)			(0.00664)	
$\widehat{OSGUR}20/\widehat{CARES}$			41.10			0.674*
			(43.44)			(0.333)
N	684	684	684	684	684	684

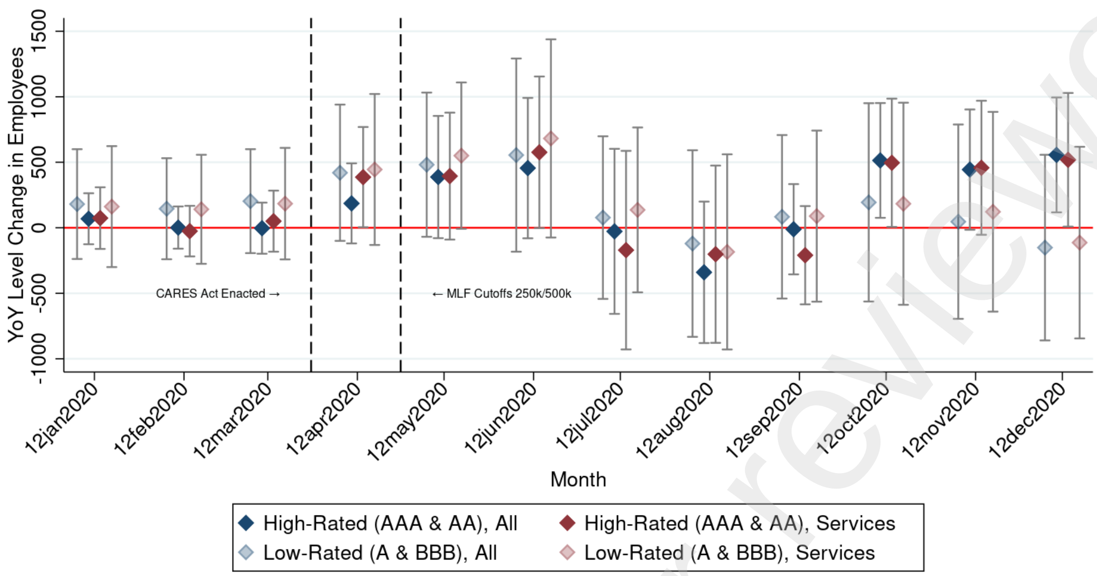
NOTES—Table presents estimates from Equation 2. Percents in columns (4) - (6) range from 0 to 100. Standard errors are clustered by population relative to the cutoff (the running variable). See Table 4 notes for further details. *** p$\leq$0.01, ** p$\leq$0.05, * p$\leq$0.1.

In conjunction with prior evidence that employment effects percolated prior to the MLF cutoff revision, and a lack of statistically significant total employment effects when analyzing the cutoff associated with cities (250,000) which was not subject to CARES (despite the fact that there seems to be strong evidence that effects on yields remain at this cutoff), we we take this as evidence that CARES aid induced the majority of the employment effects, while the MLF was more relevant for financial markets.

5.3.2 Employment Effects by Credit Ratings and Calendar Month

One interesting pattern that arose in Figure 11 was that the employment response to the totality of direct CARES aid and MLF lending eligibility was only positive in non-summer months. We further examine this result, adding heterogeneity by January 2020 plurality ratings in Figure 13.

Figure 13: RD Effects on Local Government Employment by Ratings and Month



NOTES—-Figure plots RD coefficients and 90% confidence intervals estimated using the specification from Table 4 column (3) with QCEW 12th of the month employment data, separately estimated by January 2020 plurality ratings. Here we use the specification omitting state fixed effects so not to over-absorb differences in ratings that may differentially load on to states). See Table 4 notes for further details.

When examining effects separately by ratings, we first confirm the general pattern of only detecting positive effects from April to June, and October to December. This school year pattern is broadly consistent with the notion that emergency liquidity—whether aid or a lending option—supported educational institution recalls, which is particularly striking given that the majority of schools were still under lockdown from April to June. Interestingly, Figure 13 also reveals that these employment returns were only sustained in the long run when the underlying government was a highly-rated issuer (which as we showed earlier, reflects the majority of municipal bond issuers).

If indeed it is the case that the least fiscally constrained issuers were most responsive to the additional liquidity, both during the months of April to June when government shutdowns were still widely in place, and through the close of the 2020 calendar year, this is consistent with the viewpoint that state and local governments may have over-weighted the worst possible outcomes based on past crisis experience, furloughing education sector employees even though realized revenue shortfalls were far lower than originally anticipated.[64]

[64] This view was articulated in Sheiner (2021), which we discuss along with the evolving fiscal and jobs outlook of state and local governments in Appendix A.1.

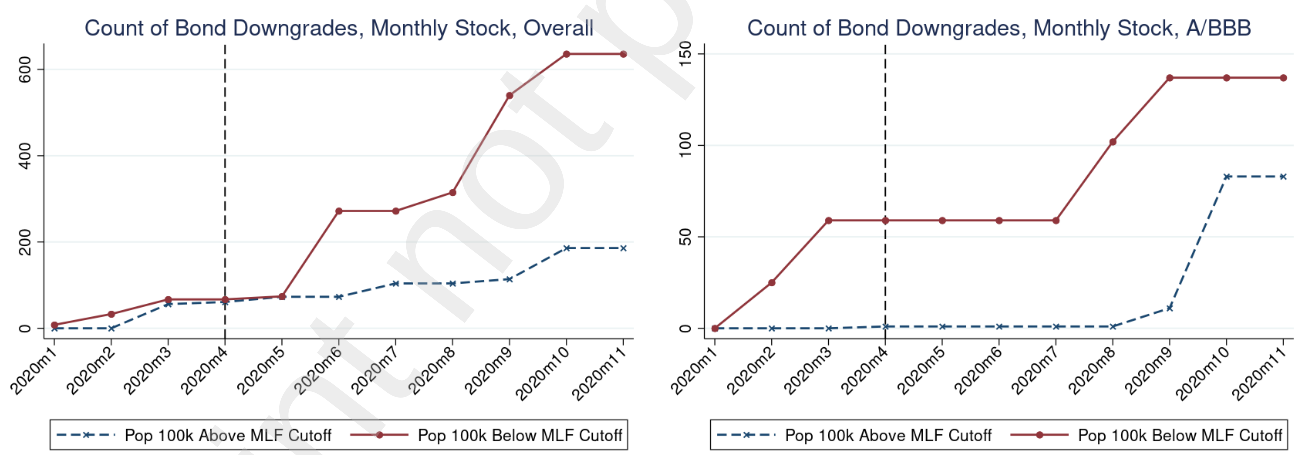
5.4 Pricing Credit Risk or Liquidity?

5.4.1 RD Effects on Credit Downgrade Probability

Above, we showed that MLF access impacted investor-perceived yields, but only at the low end of the credit ratings distribution. This prompts the question, what exactly are investors pricing? To address this question, we first explore the possibility that ratings agencies disproportionately downgraded issuers that narrowly miss the cutoff (or do not downgrade issuers if supported by the MLF), and investors either price MLF access similarly to NRSROs, or in response to them. While this is a very specific type of credit risk measure, we expect it to be broadly correlated with default risk. Toward this end, we first construct a balanced month-by-bond panel, and count how many times a bond received a downgrade from any of the three major NRSROs relative to its rating in the previous month. Analogous to earlier event study plots, in Figure 14 we focus on the arbitrary symmetric bandwidth 100,000 in population around each cutoff, and report cumulative downgrades separately by the underlying issuer's plurality rating in January 2020.

Figure 14: Credit Rating Downgrades within 100k Population of MLF Eligibility Cutoffs

(a) Cumulative Downgrades, All Ratings (b) Cumulative Downgrades, Low-Rated (A & BBB)



NOTES—Panels (a) and (b) count the number of times a bond is downgraded relative to the prior month for eligible issuers with cutoff-relative populations between 0 and 100,000 inclusively (blue dashed series), and issuers with cutoff-relative populations greater than or equal to -100,000 and less than 0 (red solid series).

Panel (a) shows that *independent* of MLF eligibility, while the number of downgraded bonds moved in tandem throughout April and May, by June of 2020, issuers that were revealed to be ineligible and therefore without an emergency liquidity option (red solid series), were downgraded at a faster rate than MLF-eligible issuers' bonds. The number of MLF-ineligible issuer bond downgrades rose from roughly

100 to 600 by November of 2020, whereas downgrades were rarer for issuers that had MLF optionality.[65] Panel (b) shows a noisier but related pattern among low-rated issuers: in August, low-rated ineligible issuers began to experience more downgrades (rising to roughly 150 downgrades by November), and accounted for about 25% of the aggregate downgrade pattern—a large share relative to the number of total issuers and bonds that are in the sample. One interpretation for why there may have been downgrades across the ratings distribution, but these were only priced into yields on the low end, is that underlying mal-priced credit risk may be particularly exposed during a crisis for issuers that are closer to the default margin. Both credit rating agencies (NRSROs) and private investors may have responded to the newly revealed risks differently, depending on whether a given issuer had an emergency credit-risk sharing option, such as access to the MLF.

While these patterns are striking, such rare events are difficult to detect statistically due to the majority of muni bonds exhibiting no change in ratings over the analysis period. In Table 6 below, we formalize this notion by estimating whether a bond was downgraded (relative to the previous month) in the post-MLF or pre-MLF period, and how many bonds on average were downgraded during this period. Similar to our analysis of primary issuance, here we collapse the data to two observations per bond—one in the pre-MLF period and one in the post-MLF period.

Table 6: RD Estimates of MLF Access on Credit Rating Downgrades

	Discontinuity	Standard Error	Control Mean	N (Fixed-bwdth)
a. Pooled Post:				
Pr(Downgrade), Overall (27apr-20nov)	-0.03	0.04	0.06	139,479
Pr(Downgrade), A & BBB (27apr-20nov)	0.06	0.16	0.16	11,756
Number of Downgrades, Overall (27apr-20nov)	-0.03	0.04	0.06	139,479
Number of Downgrades, A & BBB (27apr-20nov)	0.05	0.16	0.16	11,756
b. Pooled Pre (Placebo):				
Pr(Downgrade), Overall (01jan-23mar)	-0.02	0.01	0.02	119,200
Pr(Downgrade), A & BBB (01jan-23mar)	-0.05	0.05	0.05	10,046
Number of Downgrades, Overall (01jan-23mar)	-0.02	0.01	0.02	119,200
Number of Downgrades, A & BBB (01jan-23mar)	-0.05	0.05	0.05	10,046

NOTES—Table presents RD estimates of the probability that a bond was downgraded in the period of interest (post-MLF or pre-MLF), and the number of downgrades each bond may have received in the same period. Each row corresponds to a separate regression with that characteristic as the dependent variable. The discontinuity measures the jump in the regression function at population eligibility 0 (relative to cutoff). The Control Mean denotes the mean of that characteristic immediately to the left of relative population 0. Sample sizes vary depending on the bandwidth used. Standard errors are clustered by population relative to the cutoff (the running variable). See text for further sample restrictions. *** p≤0.01, ** p≤0.05, * p≤0.1.

While the table reveals that in the post-period, bonds of all ratings have negative coefficients associated with a lower downgrade rate for eligible issuers, these cannot be detected statistically against

[65] This pattern also persists when looking at the cumulative share of total bonds that are newly issued, which accounts for concerns that effects may be mechanically related to a greater number of issuers below the cutoff.

the vast majority of bonds that experience no change in ratings. For low-rated issuers, we in fact report positive point estimates, which suggests that the number of downgrades among MLF eligible issuers represented a larger share of total bonds than downgrades among ineligible issuers. Nevertheless, neither is statistically different from zero, and both are estimated with noise. We thus cautiously interpret these results in conjunction with the overall plots, as providing modest evidence that MLF-eligible issuers had lower downgrade rates than observationally equivalent ineligible issuers.

5.4.2 Spreads Decomposition: Asset Price Approach

In progress.

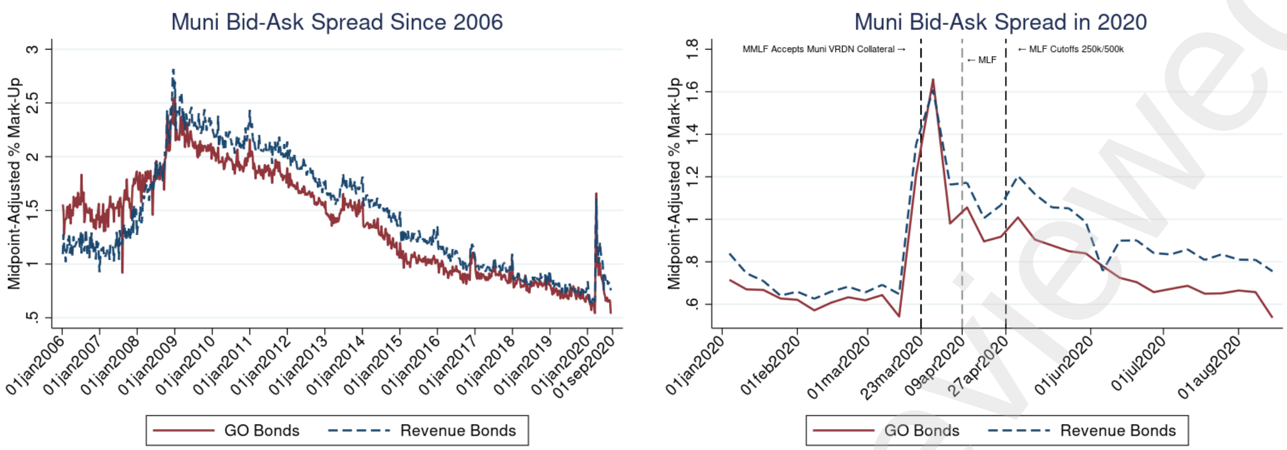
In this section we broaden our sample, and we examine the evolution of the municipal yield spread components pre- and post-COVID-19 market turmoil. Municipal bond spreads—the difference in yields between defaultable bonds and government securities of comparable maturity—reflect credit risk, liquidity risk, and tax advantages of holding them. Ang et al. (2014), Wang et al. (2008), and Schwert (2017) are some of the papers that study municipal bond spreads. Our approach is closest to Schwert (2017).

Calculating liquidity at the bond-level in the municipal bond market is not straightforward. A muni bond is traded less than three times per year. One of the liquidity measures that we utilize for our analysis is the effective bid-ask spread. This measure requires both a client's buy and a sell in the same time period for which the measure is being calculated. Because of the infrequent trading, we consider all clients' buys and sells within the week. Then we calculate the weighted-average price of the buys and the sells, and the bid-ask spread for bond b in week t is then:

$$BAS_{b,t} = \sum_{i \in t} \frac{(w_{it}^{b} p_{it}^{b} - w_{it}^{s} p_{it}^{a})}{0.5 * (w_{it}^{b} p_{it}^{b} + w_{it}^{s} p_{it}^{a})} \tag{3}$$

where w_{it} is the weight based on the size of trade i relative to the total volume traded.

Figure 15: Illiquidity Evolution in Municipal Bond Markets



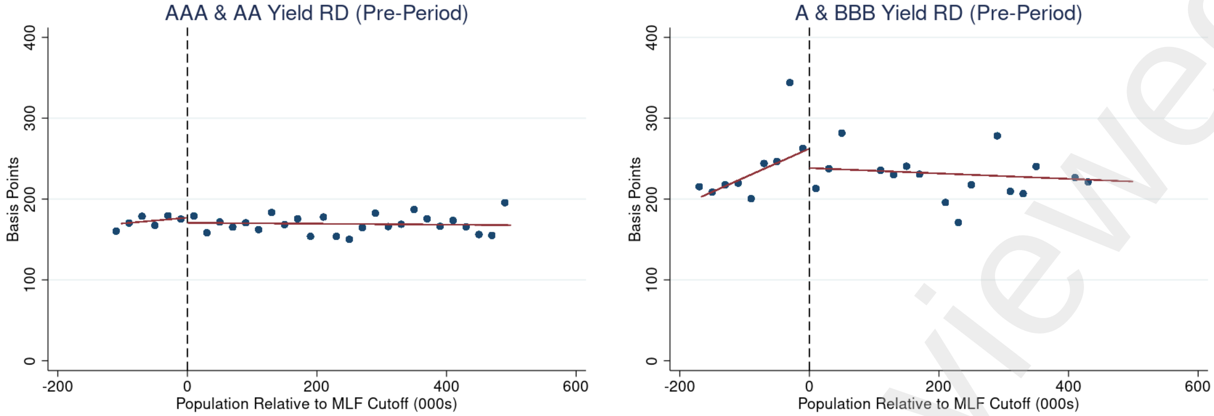
NOTES—Figure shows one common measure of illiquidity in fixed income markets, the bid-ask spread, calculated as a the volume-weighted difference between transacted bid and ask (buy and sell) muni yield prices, adjusted by the midpoint level of the two prices to adjust for heterogeneous mark-ups at varied levels. (see Equation 3 for details). Weekly mean bid-ask spreads are calculated over the universe of transacted municipal bond trades from 2006 through September 2020, and are aligned beginning-of-week on Mondays (when most Federal interventions occur). The right panel zooms in on calendar year 2020, and shows movement around three Federal interventions (indicated by dotted lines), described in detail in Section 2. Source: MSRB and Mergent

6 Robustness and Sensitivity

6.1 Placebo RD Plots Prior to Crisis

Below we show RD estimate plots associated with prior regression output shown in the main body of the text, using ur preferred specification that includes state and month fixed effects to absorb volatility given the small sample size involved in cutting results by credit ratings bin. Figure 16 shows yield RD plots for the placebo period by creditworthiness, while Figure 10 shows employment RD plots for the placebo period using our two difference in discontinuity strategies. These plots show relative smoothness across the cutoff, consistent with the tables showing no statistically significant RD effect prior to the policies being implemented.

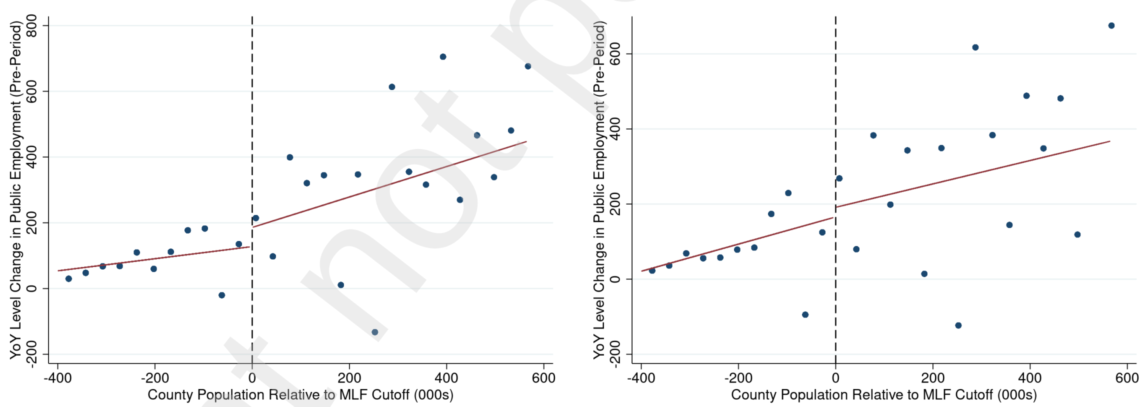
Figure 16: RD Placebo Plots of MLF Access Effect on Yields by Credit Worthiness



NOTES—Plots show regression slopes and intercepts from Equation 1 in the pre-period, overlaid on top of equally spaced pre-binned outcome data with a bin size of 20 (x-axis in thousands). Plots are shown over the optimal bandwidth selected using the IMSE-procedure, which produces (potentially) asymmetric optimal bandwidth boundaries for each sample. Plots correspond to Table 2, which includes state and calendar month fixed effects as controls. Plot thus residualizes all yields by state-fixed effects (added back to their overall mean) prior to mean-collapsing by bin.

Figure 17: RD Placebo Plots of MLF Access Effect on Public Sector Employment

(a) Δ Overall Employment, Pre-MLF (Placebo) (b) Δ Services Employment, Pre-MLF (Placebo)

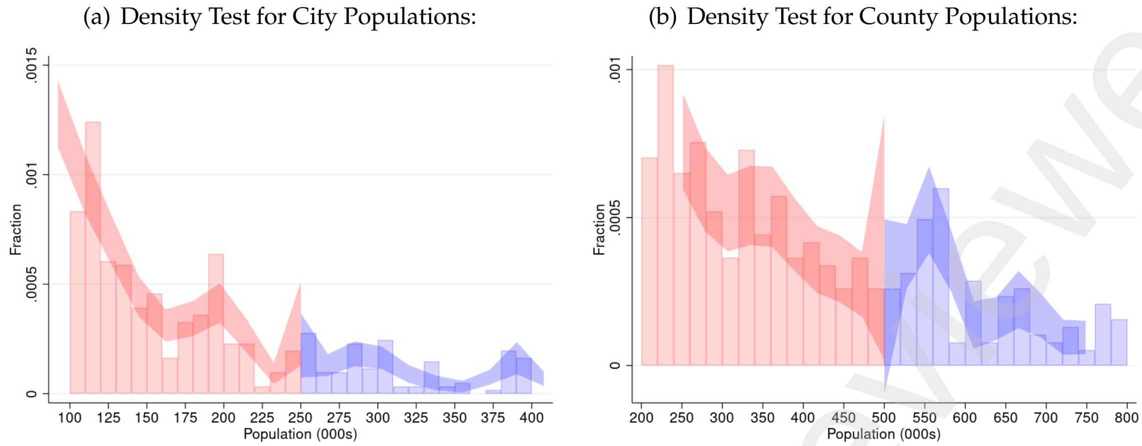


NOTES—Panels (a) and (b) show year-on-year differences in public employment for the months of January and February, 2020, relative to January and February, 2019. Results are shown for the differenced RD strategy discussed in the text, pooling all issuers in one sample. This RD does not yet implement an IMSE-optimal bandwidth. The baseline number of local employees in a given locality in 2019 was about 750 employees. Source: QCEW, May 26, 2021, Q4 release (through December)

6.2 Manipulation Tests

Below we show formal McCrary (2008) running variable manipulation tests, using the Cattaneo et al. (2018) method of local polynomial density estimation with robust standard errors, where bandwidths for density tests are data-driven and thus do not have to span the whole support of the histogram.

Figure 18: Manipulation Test for City and County Population Running Variables



NOTES—Figure separately plots histograms of unique cities and counties on either side of their respective MLF population cutoffs, and estimates 95% confidence intervals from local polynomial fits on each side of cutoff using the Cattaneo et al. (2018) method. Significant overlap in confidence intervals signifies a passing test. City manipulation density test is overlaid on top of a histogram with binwidths of 10,000 while county manipulation density test is overlaid on top of histograms with binwidths of 25,000 (due to there being fewer counties than cities). Fractions are small because majority of cities and counties fall below 100,000 and 200,000 in population respectively.

Consistent with cutoffs being chosen using round-number heuristics and the MLF's regulatory population eligibility being lagged by 1 to 2 years, we find no evidence of cutoff manipulation by policymakers, nor cutoff-targeting based on the underlying distribution of of issuers. We also provide p-values from a density manipulation test associated with a discrete running variable (Frandsen (2017)), should lumpy population observations be interpreted as discrete rather than continuous. The Frandsen (2017) test of equality of projected intercepts at the cutoff produces to a p-value of 0.967 for counties, and 0.388 for cities, significantly away from conventional p-values that would reject equality.

6.3 Sensitivity to Controls

Here we report sensitivity of our main RD yields estimates to controls and sample restrictions, where column (6) is our preferred specification.

Table 7: RD Effects on Yields, Sensitivity to Controls and Restrictions

	(1)	(2)	(3)	(4)	(5)	**(6)**	(7)	(8)	(9)	(10)	(11)	(12)
a. Pooled Post:												
Current Yield (Overall)	-9.88	-11.13	-11.24	-18.51	-16.17	**-19.26**	-20.60	-19.53	-19.97	-20.46	-21.15	-20.18
City Only	-13.26	-13.79	-13.85	-25.46	-24.38	**-27.97**	-30.63	-28.14	-27.13	-26.72	-25.74	-23.18
County Only	-19.81	-21.99	-21.95	-12.73	-11.11	**-15.06**	-12.79	-15.58	-13.69	-13.43	-15.46	-20.23
High-Rated (AAA & AA)	4.36	4.44	4.50	-1.06	0.43	**-1.54**	-2.97	-1.73	-1.37	-1.96	-7.77	-6.75
Low-Rated (A & BBB)	-121.90***	-121.27***	-121.06***	-69.87**	-69.78**	**-72.28****	-73.81**	-72.91**	-62.92*	-64.86**	-57.04*	-53.21*
b. Pooled Pre (Placebo):												
Current Yield (Overall)	-6.13	-6.81	-6.95	-14.59	-12.99	**-13.25**	-13.52	-14.64	-13.50	-13.61	-11.73	-11.23
City Only	-1.20	-1.31	-1.30	-8.12	-7.89	**-9.13**	-9.34	-10.92	-8.05	-7.89	-5.37	-4.91
County Only	-24.49	-26.48	-26.81	-24.05	-22.29	**-21.61**	-19.84	-19.46	-20.23	-20.42	-22.40	-24.29
High-Rated (AAA & AA)	1.68	1.85	2.16	-5.94	-5.10	**-6.15**	-6.66	-5.70	-5.76	-5.93	-7.11	-5.68
Low-Rated (A & BBB)	-56.58**	-54.88**	-54.90**	-21.62	-23.20	**-24.87**	-25.87	-31.43	-16.19	-18.88	-21.73	-20.54
Fed. Tax Adjust		X	X	X								
St. & Fed. Tax Adjust			X	X								
State FE				X	X	**X**	X	X	X	X	X	X
Month FE						**X**	X	X	X	X	X	X
Revenue/GO Bond							X					
Day/Week of Trade								X				
Maturity Size									X			
Amount Outstanding										X		
Tenor Length											X	
Duration												X
Sample Restrictions	X	X	X	X	X	**X**	X	X	X	X	X	X

NOTES—Sample restrictions include 1% winsorizing and outlier filtering. See Table 2 notes and text for further details. *** p≤0.01, ** p≤0.05, * p≤0.1.

Column (2) applies the NBER-TAXSIM deduction-adjusted top federal tax rate for a wealthy household to all exempt bonds, and assumes the marginal investor buys bonds *outside* of their resident state. Since local coupon payments are exempt from state tax liabilities, it is optimal in most states for individual investors to invest in within-state issuers. Columns (3) and (4) therefore apply both federal and state effective tax rates to double-exempt bonds, and federal rates to federal-exempt only bonds, assuming the marginal investor resides in the state. That is, for this specification, we apply $1/(1-\tau_f^{deduct}-\tau_s^{deduct})$ to tax-exempt yields, which accounts for both local and state tax deductions.[66]

[66]This is similar to using a state-deducted formula such as $1/(1-\tau_f)(1-\tau s)$, which does not adjust for local deductions. See, for example, Schwert (2017).

From columns (4) onward, we include state fixed effects that absorb 2020 state-tax adjustments (the coefficient difference between (4) and (5) is thus coming entirely from Federal-exempt only bonds that are eventually absorbed by month fixed effects). While perfect balance among the low-rated A and BBB group requires conditioning on these state fixed effects (to partial out volatility from the smaller sample), we interpret this as forcing crisper comparisons due to divergence in early 2020 (when low-rated issuer states were less balanced and trading volume low). These concerns however, should be minimal given that unconditional raw mean trajectories in the symmetric 100,000 population bandwidth (Figure 6) demonstrated that pretrends were relatively parallel (especially in the run up to the first Federal announcement). All other ratings groups are otherwise unconditionally balanced, as indicated by the lack of point estimate statistical significance in the placebo period.

6.4 Yields Sensitivity to CARES Act Notch in CRF Aid Formula

While the above manipulation test confirms that cutoffs were not chosen based on the underlying variation in issuers, it does not rule out the possibility that these heuristic population cutoffs could be binding for other government policies, confounding our interpretation of the intervention. As discussed in the context of employment effects, we identify one potential confounding policy: the March 27th CARES act included a Coronavirus Relief Fund (CRF) provision that "local governments serving a population of at least 500,000, as measured in the most recent census data, may elect to receive assistance directly from Treasury. Such direct local assistance allocations reduce the allocation that is made to the state government (keeping the state allocation constant)."[67] In other words, localities over 500,000 in population may have had quicker access to CRF aid, relative to localities just under that cutoff that had to rely on downstreaming from their underlying state allocation cap.[68] Since allocation caps were fixed to the right of this cutoff, but unrestricted to the left, the amounts received may have been differentially higher or lower to the right of the cutoff.

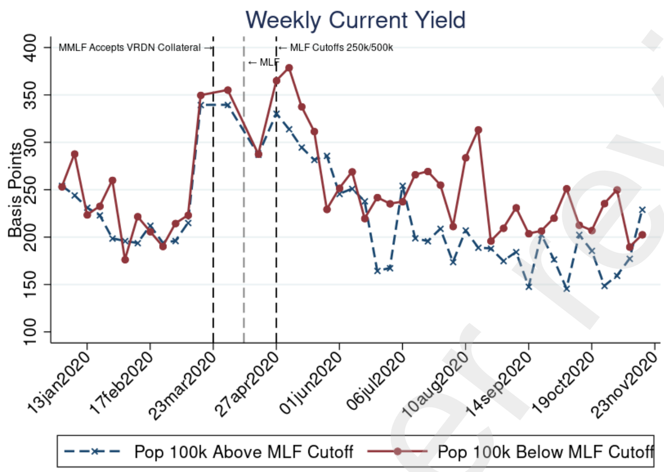
As discussed, this poses a potential problem for counties in our analysis, which are subject to the April 27 MLF population cutoff (also using 500,000 as an eligibility threshold). In the context of yields, we are served by two additional tests to help us tease apart CARES from MLF effects, which were not available in our employment analysis. We first note that visually, we have more variation in the month that elapsed between the CARES and MLF revision announcement. During this inter-announcement interval, yields appeared to trend almost identically (shown in Figure 6), whereas they separated earlier among employment outcomes. Second, only one of the two cutoffs (counties) were aligned exactly at

[67] https://crsreports.congress.gov/product/pdf/R/R46298.

[68] CRF aid allocation caps for localities greater than 500,000 in population are calculated as the product of the total state allocation (based on population) weighted by the locality population share.

500,000, whereas the other cutoff (cities) was not. We thus show in Figure 19 the variation underlying our main results for low-rated (A & BBB) yields, excluding counties from the analysis and thus only focusing on the 250,000 population cutoff which is unimpaired by CARES aid—or at least, only affected far away from the cutoff in a way in which the RD polynomial can fully control for.

Figure 19: Mean Yields within 100k of MLF Population Cutoff, Low-Rated (A & BBB) Cities Only



NOTES—Figure shows mean weekly yields (pooled over buyer and seller prices) for low-rated city eligible issuers with cutoff-relative populations between 0 and 100,000 inclusively (blue dashed series), and issuers with cutoff-relative populations greater than or equal to -100,000 and less than 0 (red solid series). Trades between announcements are pooled into a single period beginning on the announcement day. See text for sample restrictions and definitions. *Source:* MSRB, S&P, Moody's, Fitch

This additional placebo test shows a very similar pattern to our main results including counties, and our yield RD estimates for low-rated cities are similarly robust and statistically significant. While the results for employment are less conclusive, we can definitively claim that investors seem not to have responded to CARES act aid, whereas they did respond to an MLF emergency liquidity option.

7 Discussion and Remaining Puzzles

We estimate the option value of municipal liquidity by studying bond market behavior and public sector hiring decisions when government budgets are severely distressed. The option is made available due to a number of Federal sector facilities that were announced after the onset of the COVID-19 pandemic, which simultaneously shocked incomes and resulted in a liquidity crisis in the municipal bond market. We first show that overall secondary market yields and primary issuance for the most part returned to normal market functioning as a result of the totality of Federal interventions that were introduced between mid-March and the end of April 2020, while low-rated investment grade bonds remained relatively distressed. To estimate the option value, we use a regression discontinuity (RD) design that exploits lending eligibility cutoffs introduced by the Federal sector's Municipal Liquidity Facility (MLF) on April 27, 2020. This granted direct Federal Reserve lending eligibility via a Special Purpose Vehicle to city issuers with populations greater than 250,000, and county issuers with populations greater than 500,000. We first estimate the value of this differential access to emergency liquidity using municipal bond yields as an investor perceived measure of access. We find that low-rated (A and BBB) government issuers' bonds traded at higher prices with facility access. Low-rated issuers that were narrowly eligible for emergency lending immediately exhibited a yields decline of roughly 72 basis points relative to issuers that narrowly missed eligibility—a magnitude that closely corresponds to the overall municipal market spread between BBB yields and higher-rated issuers. We find modest evidence that eligible issuers experienced lower credit downgrade probabilities overall, and stronger evidence that MLF optionality translated to differential primary issuance on private markets. Using a complementary asset price decomposition approach, we quantify the credit-risk channel over a broader set of issuers, which implies a non-trivial role for credit risk in determining yield spreads during crises.

We then show that local governments, in contrast to investors, responded to the totality of MLF optionality and direct CARES aid by retaining 25% to 30% more service-providing public sector employees (reflecting educational institutions) in light of mass furloughs and separations that were prevalent during this period. These employment smoothing improvements are detected predominantly among high-rated issuers (the majority of governments) and were strong even when schools remained shut down, consistent with a viewpoint that state and local governments may have over-weighted the worst possible outcomes (based on past experience) in furloughing education sector employees given realized revenue shortfalls were far lower than originally anticipated (Sheiner (2021)). The timing of the effects and ancillary decomposition tests suggest that most of the return in employment is attributable to direct CARES aid rather than MLF lending (consistent with CARES employment effects estimated in Green and Loualiche (2020)). When the additional liquidity through both interventions became available as potential buffer for the locality's revenue position, this may have counteracted some of the uncertainty that induced the initial furloughs. Taken together, our results imply municipal debt market and employment outcomes would likely have been more distressed absent the MLF facility's operation and CARES aid respectively.

Interestingly, these results also suggest the presence of a potential credit-risk sharing channel on top of the Fed's role as lender of last resort. This brings up a number of questions and puzzles that future research will need to address. Is emergency liquidity only valuable at the low end of the ratings distribution because in response to adverse shocks, extra liquidity provides implicit insurance against credit risk? If so valuable, why did only two issuers take up direct facility lending?[69] We provide two candidate explanations here: the first relates to the design of the MLF penalty pricing grid (shown in Appendix A.2). While part of the facility's rationale was that issuers simply could not find markets to match their target issuance due to a general liquidity freeze, a more nuanced view is that issuers could in fact find matches, but only used the MLF when its prices were more favorable than those offered through a competitive bidding process. It would, for example, be in the issuer's interest to access the MLF if the issuer's rating was substantially lower than the bond being issued, as the facility penalty prices are at the *issuer* rather than *issuance* level. A second explanation relates to stigma in borrowing, which may have deterred issuers as suggested by Moore (2017) who studied the Fed's Term Auction Facility.

A second question is if the facility provided short-term assistance to new primary issuance, how did this support pass through to long term secondary market yields? We begin the discussion by noting that sufficient liquidity is necessary for municipalities to meet spending obligations like payroll, debt service, and other fixed payments, as well as unforeseen circumstances.[70] Municipality access to short-term borrowing can allow such payments, including those related to long term debt, to be made.[71] Fiscal rules or bond covenants may prohibit the proceeds of new debt issuance to be used for debt repayment, but because cash is fungible, access to liquidity can be used to fund non-debt expenses, like payroll, thereby freeing up resources to be used for debt service. Finally, by avoiding unnecessary public employee layoffs or tax increases, short-term financing may help to preserve the local tax base from which the funds for long term bond repayment are drawn. Each, or all, of these mechanisms is a potential connection between the kind of short-term assistance offered by MLF and the performance of long term debt.[72]

Given our new estimate of the willingness to credit-risk share, a parameter which enters a broader welfare calculus regarding the efficiency of macroeconomics stabilization policies, and sizable employment effects, one natural question is when does municipal credit-risk sharing represent a socially efficient policy improvement? From a financial frictions viewpoint, several influential papers have suggested that sub-optimal risk sharing could result from institutional investors only benefiting from local exemptions, and a home market bias that distorts the efficient spread of risk (Poterba (1989), Pirinsky and Wang (2011), Schwert (2017), Babina et al. (2021)). Other market structure constraints may also result in inefficient muni market pricing (Garrett (2020)). If financial frictions pass through to labor market frictions or revenue sources that have positive fiscal multipliers or high public service

[69] Illinois has issued via the MLF twice, for a total $3.2 billion in loans (of its $9.67b MLF state borrowing cap), while the New York Metropolitan Transit Authority (MTA) has also issued twice, totaling roughly $3.35 billion.

[70] Indeed, cash on hand is a major factor in determining bond ratings. See Moody's July 2020, pages 9-12 (Moody's Investors Service, 2020).

[71] In addition to making payments directly to bondholders, municipalities may be required by bond covenants to maintain a liquid sinking fund sufficient to cover several months of debt service.

[72] In ongoing work, we are also exploring whether we can leverage differences in fiscal rules and features of outstanding long term bonds (such as revenue versus GO), to try to address this apparent short term to long term mechanism.

externalities (high social value), these too, enter the welfare calculus. When such frictions are also heterogeneously correlated with the mean income of underlying geographies, inequality weights must also be taken into consideration. Due to these complexities and the many models that could rationalize such trade-offs, we leave to future work analysis of the social efficiency of such a policy with so many diverse potential mechanisms, as well as questions related to what issuance would have looked like were the penalty pricing schedule steeper down the ratings distribution, or less punitive overall.

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A Appendices

A.1 Forecast Evolution of State and Local Government Revenue and Employment

As the pandemic took hold in the US during spring and summer of 2020, many analysts predicted extremely dire consequences for the revenues of state and local governments. In addition to expected delays in the receipt of final settlements of 2019 personal income tax liabilities associated with the delay of the federal filing deadline (described in more detail in the text), disruptions to economic activity threatened virtually all forms of the sector's revenues.

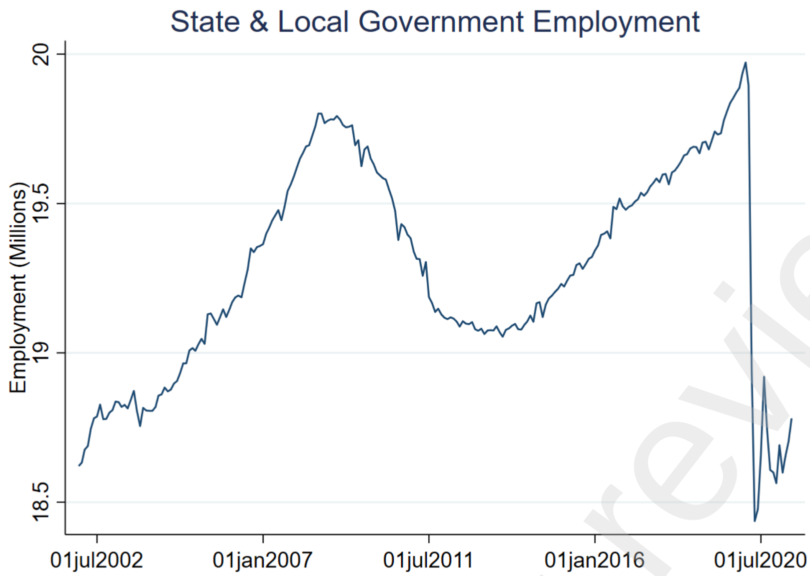
One analysis (Fiedler and Powell (2020)), using data from previous downturns, suggested that each year over year percentage point increase in the unemployment rate had historically been associated with a $45 billion deterioration in the fiscal situation of state and local governments, the vast majority of which consists of revenue declines associated with reduced economic activity. In April 2020, the unemployment rate stood at 14.7%, a stunning 11.1 percentage points higher than its year-earlier level (and 11.2 percentage points above its level of two months earlier, in February 2020). This suggested an annual fiscal shock of around $500 billion to the sector. Fiedler and Powell (2020) describe several sources of uncertainty in this estimate, including reasons to expect the shock to be larger (for example, business closures may mean that the sales tax elasticity to unemployment may turn out to be larger than usual) or smaller (the increase in unemployment was unusually concentrated among low wage workers, reducing the income tax elasticity to below normal levels).

By mid-summer, it was becoming clear that at least some of the sharp increase in the unemployment was transitory. By July, the rate stood at 10.2%, still far above its year-ago level, but already down 450 basis points from its April peak. Estimates from this period suggested state fiscal impacts in the neighborhood of $75-100 billion for fiscal 2020, and $100-300 for 2022 (Dadayan (2020) and McNichol and Leachman (2020)); Auerbach et al. (2020) estimated a FY 2021 effect of $167 billion for state and local governments combined).

By mid-autumn, unemployment was down to 6.7%, and it had become clear that the most dire concerns for the sector were off the table. In addition to the sharp rebound in unemployment, federal fiscal support in the form of enhanced unemployment benefits, frequently taxable at the state level, had passed through to state income tax revenues, and indirectly to sales taxes. Consumption was still below its pre-Covid levels, but had fallen far more sharply for services rather than more heavily-taxed goods. These subtle variations in the changes in tax bases generated significant heterogeneity in the revenue experience of states. One bright spot in the outlook was property taxes, which held firm for the first three quarters of calendar year 2020, and given high house price growth seemed likely to remain strong into 2021. Nonetheless, the sector as a whole had shed over 1.3 million jobs by November, primarily in local education, and concerns for state and local governments remained heightened.

By early 2021, as it became more clear that realized revenue losses were lower than anticipated, some analysts began to note that government employment had declined at a far steeper rate than revenue, and that budget cuts were both faster and deeper in scale than during the Great Recession, which was far more gradual. (Bloomberg, Jan 2021). For example, Sheiner (2021) attributed this to preemptive and perhaps over-cautious cuts in the education sector due to high uncertainty: "...there is also evidence that tight fiscal conditions have led to employment declines in local education, which accounts for roughly 5% of the overall U.S. workforce. Furthermore, state and local governments, having been scarred by the Great Recession, are likely being very cautious about spending given the tremendous uncertainty about the economic outlook...". Both the long-term stress from the Great Recession, and the sharp pattern following the COVID pandemic, can be seen clearly in Figure A.1.

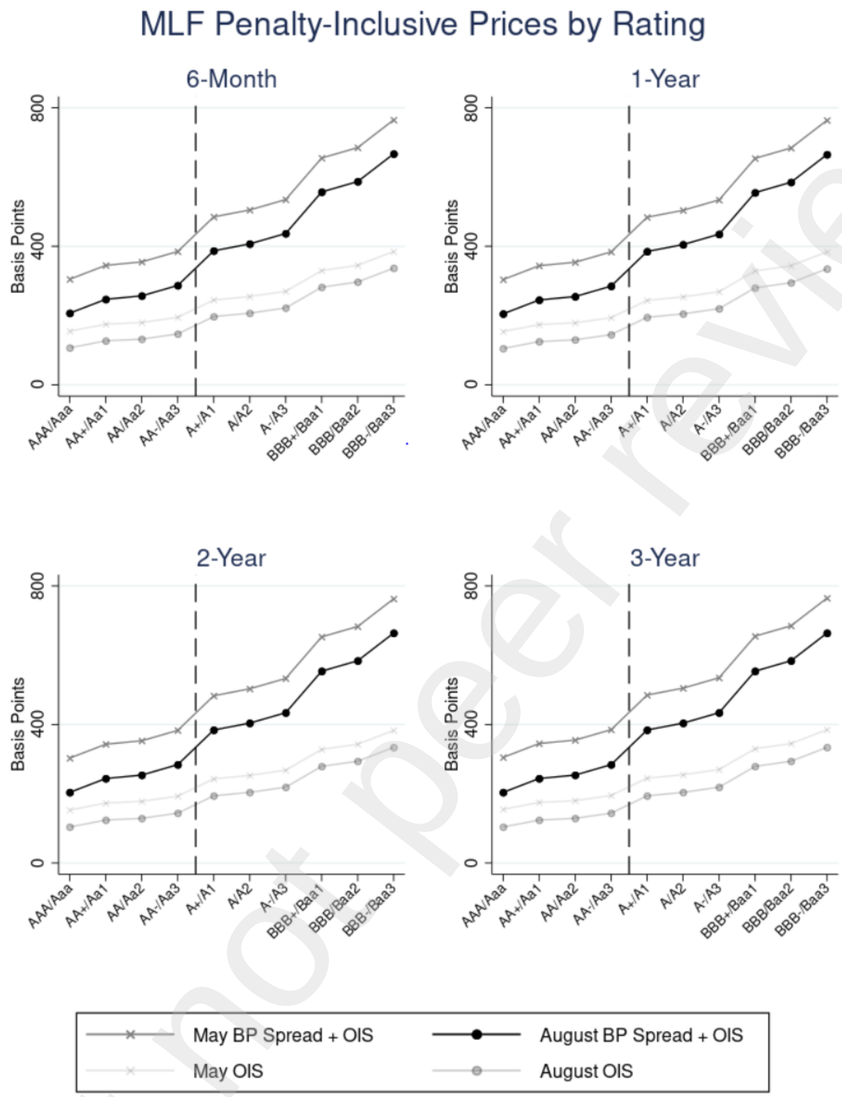
Figure A.1: State & Local Government Employment over the Great Recession and COVID Pandemic



NOTES—Calculations from Current Employment Statistics (CES) acquired from FRED, Economic Research Division, Federal Reserve Bank of St. Louis. Series: CES9092000001+CES9093000001, Thous. of Persons+Thous. of Persons, Monthly, Seasonally Adjusted. Data through May, 2021.

A.2 MLF Pricing Grid

Figure A.2: MLF Pricing Grid



NOTES—Figure shows penalty pricing grid by issuer ratings when it was announced on May 11, 2020, and revised on August 11, 2020, for different loan term lengths. OIS = overnight index swap rate, meant to represent prevailing private market interest rates, here pulled on May 27, 2020, and August 12, 2020, respectively. Dotted line indicates distinction between "high" and "low" rated issuers as defined in the draft. Pricing schedules are similar with slight variations for different tenors. See MLF Pricing Term Sheet for further details.

A.3 Matching MSRB Issuer Names to Census Populations

Our goal is to assign a single Census population to any city, county, or state municipal bond issuer that has traded on the secondary market at any point since Jan. 1, 2019, including all MLF eligible and ineligible issuers. We start with the term-sheet referenced U.S. Census Bureau datasets that include 2018 city populations greater than 50,000, and 2019 county populations greater than 50,000.[73] This provides populations for larger issuers, however we also desire to match populations to smaller issuers

[73] U.S. Census Bureau, "Annual Estimates of the Resident Population: April 1, 2010 to July 1, 2018" for cities, as of April 6, 2020; and U.S. Census Bureau, "Population, Population Change, and Estimated Components of Population Change: April 1, 2010 to July 1, 2019" for counties, as of April 6, 2020.

so that they may contribute to the RD running variable further away from the cutoff. Toward this end, we complement our analysis with the full population Census file "sub-est2019_all.csv", a place-level dataset with both 2019 and 2018 populations for localities of *all* populations.[74] This file contains a place description such as the city or county's name (which is always followed by the type suffix, such as "CITY" or "COUNTY"), state, census place code, and geographic code level (*sumlev*), allowing us to isolate the administrative name and its geographic level.

We desire the dataset be uniquely identified by city (county) name and city (county) state, so reshape wide the 2018 (2019) populations by *sumlev*, which produces a maximum of 6 potential population measures for each geographic code, corresponding to the number of unique values *sumlev* can take:

1. Minor Civil Divisions (MCDs)[75]
2. MCD "parts"
3. County place parts
4. Incorporated places (cities, towns, boroughs, villages)
5. Consolidated cities
6. Consolidated city parts

The data separately document localities whose "part" or "balance" spills over into another locality, which we subsequently drop (i.e. we drop (2) and (6)).[76] We then designate a rule to choose among these candidate populations. Most places have one or two population measures, but they will not have all six. If all non-missing populations are the same, or if there is only one population, we use that population. If any of the populations are different (0.1% of localities (27 observations; 3 of which ultimately do not merge to MSRB-Bloomberg)), we flag and omit them the analysis.

Finally, we go through a multi-step process to merge MSRB issuer names (from their security descriptions) to Census place names. This involves flagging duplicate MSRB place names within state which arise erroneously from our cleaning algorithm, and remapping them based on their original security description to their appropriate population. For example, "SPRINGFIELD TOWNSHIP, NJ" in the Census may correspond to "SPRINGFIELD, NJ" in MSRB trade data; by removing the township suffix, we can match these. But doing this universally risks false positives in other cases: for example, the Census might contain "HEMPSTEAD VILLAGE, NY", "HEMPSTEAD TOWN, NY", and "HEMPSTEAD CITY, NY" (all separate localities), while MSRB may only contain one HEMPSTEAD, NY. Removing suffixes here would lead to duplicate matches. We thus only remove such suffixes when the resulting merge is one-to-one, causing a loss rate of 4% of unmatched trades.

We also consider special treatment for matching fully consolidated cities (e.g. City and County of San Francisco) which have only one set of issuers at either the city or county level, and partially consolidated city-counties (e.g. Miami City vs. Dade County) which may have different revenue sources. We follow the MLF term sheet in assigning these cases to city and/or county eligibility lists based on the revenue sources of underlying issuers. Lastly, we manually flag potential counties that issue on behalf of cities ("downtreamers") based on their detailed issuer and security description. Among issuers with a reliable MSRB issuer name and actively trading post-2019, our final cleaning procedure results in 6,143 city (1,880

[74]The sub-est2019_all.csv file can be found here: https://www2.census.gov/programs-surveys/popest/datasets/2010-2019/cities/totals/.

[75]States in New England, New York, and Wisconsin, all classify towns as MCDs.

[76]A "balance" is a consolidated city minus the semi-independent incorporated places located within the consolidated city (overlapping service populations which would be double counted if we did not drop them). Incorporated places can cross both county and Minor Civil Division (MCD) boundaries. In such cases a separate record indicates the population estimates for the part of a place in each of its parent counties or MCDs. For such records, the place name is sometimes followed by the designation "pt" (which stands for part), allowing us to isolate these cases.

county) BaseCUSIPs (unique at the issuer level) that match a population to MSRB data, and 361 city (41 county) issuers that do not retrieve a match.

A.4 Data Cleaning

A.4.1 MSRB

Since January 1998, MSRB has required registered dealers to report all municipal bond transactions. The trade record includes information about CUSIP, date and time of the trade, price and yield, maturity, coupon, and a flag whether the dealer bought from a customer, sold to a customer, or whether it is an interdealer trade. In January 2015, MSRB started to publicly disseminate those transactions with up to a 15 minutes delay.

We first clean keep unique observations at the CUSIP—trade_date—RTRS_Control_Number level, using the RTRS_PUBLISH_DATE and RTRS_PUBLISH_TIME variables to ensure that duplicates are not arising from missing data. Then, in addition to the sampling restrictions applied to our RD analysis, for the yield decomposition sample, we further apply the following conditions:

1. Delete CUSIPs with:
   (a) missing coupon and maturity information for all trades
   (b) a listed coupon greater than 20%
   (c) a listed maturity over 100 years
   (d) fewer than 10 trades in the entire sample
2. Delete transactions where:
   (a) the price is less than 50 (i.e., 50% of face value)
   (b) the price is greater than 150 with a short time to maturity
   (c) trade date is after the maturity date of the bond

A.4.2 Mergent

The key characteristics from Mergent that we use include CUSIP, issuer name, offering amount, source of funds and use of proceeds, bond credit rating as rated by S&P, Moody's, and/or Fitch, coupon type (fixed, variable, or zero), the tax status of the coupon payments, callability and first call date, insurance status and the identity of the insurer, and pre-refunding status and timing. The Mergent data ratings provide a longer time series of ratings data relative to the Bloomberg ratings data used in the RD analysis. A few of the variables require some adjustments. Specifically:

Coupon Type: Mergent's variable *coupon_code* indicates the coupon type. However, for fixed-rate bonds issued at a discount or at a premium, it only indicates OID and OIP respectively. Most of these bonds are fixed-rate, nevertheless for these bonds we also use the VARRATE table to determine the coupon type.

Ratings: There are also duplicate observations at the CUSIP—rating_type—rating_date level, sometimes with different ratings values. Rating agencies submit revisions of ratings that are then posted in the Mergent database as a new rating instead of a revision to a current rating. The data provider is in the process of correcting that in its database, so for such observations we use ratings data from Bloomberg data.

A.5 NRSRO Ratings Concordance and Plurality Ratings

Each of S&P, Moody's, and Fitch, maintain separate ratings systems for long-term and short-term bonds. S&P and Moody's also use separate systems for short-term municipal note ratings. We map of each these ratings to 8 aggregate ratings bins that are guided by S&P's convention: AAA, AA, A, BBB, BB, B, C, D. To do so, we use the following concordances, which were developed manually in consultation with a number of sources.[77] The resulting columns ending in *_agg* form the basis of our plurality ratings, which are calculated as the plurality across long-term, short-term, and muni-note bonds based on their "aggregate" ratings.

[77] These include S&P reference, Moody's reference, and Fitch reference. Some disaggregated ratings are identified from MSRB trades rather than the NRSRO's themselves, however have natural mappings. We provide the full details of this concordance in the online appendix.

Figure A.3: Ratings Concordances to Aggregated Credit Rating Bins

Fitch, Long-Term Ratings

rank	fitch_rating_lt	fitch_rating_lt_agg	fitch_rating_desc
1	AAA+	AAA	Prime
2	AAAe	AAA	Prime
3	AAApre	AAA	Prime
4	AAA	AAA	Prime
5	AAA-	AAA	Prime
6	AA+e	AA	High Grade
7	AA+	AA	High Grade
7	AAe	AA	High Grade
8	AA	AA	High Grade
9	AA-e	AA	High Grade
10	AA-	AA	High Grade
11	A+e	A	Upper Medium Grade
12	A+	A	Upper Medium Grade
12	Ae	A	Upper Medium Grade
13	A	A	Upper Medium Grade
14	A-	A	Upper Medium Grade
15	BBB+	BBB	Lower Medium Grade
16	BBB	BBB	Lower Medium Grade
17	BBB-	BBB	Lower Medium Grade
18	BB+	BB	Non-Investment Grade Speculative
19	BB	BB	Non-Investment Grade Speculative
20	BB-	BB	Non-Investment Grade Speculative
21	B+	B	Highly Speculative
22	B	B	Highly Speculative
23	B-	B	Highly Speculative
24	CCC	C	Extermeley Speculative
25	DDD	D	In Default
26	DD	D	In Default
27	D	D	In Default

Fitch, Short-Term Ratings

rank	fitch_rating_st	fitch_rating_st_agg
1	F1+e	AA
2	F1+	AA
3	F1	A
4	F2	BBB
5	F3	BBB
6	B	B
7	C	C
8	D	D
9	/	D

Moody's, Long-Term Ratings

rank	moody_rating	moody_rating_lt_agg	moody_rating_desc
1	#Aaa	AAA	Prime
2	Aaa	AAA	High grade
3	Aa1	AA	High grade
4	Aa2	AA	High grade
5	Aa3e	AA	High grade
6	Aa3	AA	High grade
7	A1	A	Upper medium grade
8	A2	A	Upper medium grade
9	A3	A	Upper medium grade
10	Baa1	BBB	Lower medium grade
11	Baa2	BBB	Lower medium grade
12	Baa3	BBB	Lower medium grade
13	Ba1	BB	Non-investment grade speculative
14	Ba2	BB	Non-investment grade speculative
15	Ba3	BB	Non-investment grade speculative
16	B1	B	Highly speculative
17	B2	B	Highly speculative
18	B3	B	Highly speculative
19	Caa1	CCC	Substantial risks
20	Caa2	CCC	Extremely speculative
21	Caa3	CCC	Default imminent with little prospect for recovery
22	Ca	C	Default imminent with little prospect for recovery
23	C	D	In default
24	/	D	In default

Moody's, Short-Term Ratings

rank	moody_rating_st	moody_rating_st_agg
1	P-1	A
2	P-2	BBB
3	P-3	BBB

Moody's, Muni-Note Ratings

rank	moody_rating_muninotes	moody_rating_muninotes_agg
1	MIG1	A
2	VMIG1	A
3	MIG2	A
4	VMIG2	A
5	VMIG3	BBB
6	MIG3	BBB
7	SG	D

S&P, Long-Term Ratings

rank	sp_rating_lt	sp_rating_lt_agg	sp_rating_desc
1	AAA+	AAA	Prime
2	AAA	AAA	Prime
3	AAA-	AAA	Prime
4	AA+	AA	High Grade
5	AA	AA	High Grade
6	AA-	AA	High Grade
7	A+	A	Upper Medium Grade
8	A	A	Upper Medium Grade
9	A-	A	Upper Medium Grade
10	BBB+	BBB	Lower Medium Grade
11	BBB	BBB	Lower Medium Grade
12	BBB-	BBB	Lower Medium Grade
13	BB+	BB	Non-Investment Grade Speculative
14	BB	BB	Non-Investment Grade Speculative
15	BB-	BB	Non-Investment Grade Speculative
16	B+	B	Highly Speculative
17	B	B	Highly Speculative
18	B-	B	Highly Speculative
19	CCC+	CCC	Substantial Risks
20	CCC	CCC	Extermeley Speculative
21	CCC-	CCC	Default Imminent with little prospect for recovery
22	CC+	CC	Default Imminent with little prospect for recovery
23	CC	CC	Default Imminent with little prospect for recovery
24	C-	C	Default Imminent with little prospect for recovery
25	SD	D	In Default
26	D	D	In Default

S&P, Short-Term Ratings

rank	sp_rating_st	sp_rating_st_agg
1	A-1+	A
2	A-1	A
3	A-2	BBB
4	A-3	BBB
5	B	B
6	C	C
7	/	D
8	D	D

S&P, Muni-Note Ratings

rank	sp_rating_muninotes	sp_rating_muninotes_agg
1	SP-1+	A
2	SP-1	A
3	SP-2	BBB
4	SP-3	BB
5	D	D

A.6 List of A and BBB Issuers within 100k of Cutoff

Table A.1: Low-Rated (A and BBB) Issuers within 100k of MLF Cutoff

BaseCUSIP	Issuer Name	State	Issuer Type	Number of Cusips	Above100
650367	Newark	NJ	Local Government	89	1
119677	Buffalo	NY	Local Government	81	1
889278	Toledo	OH	Local Government	73	1
86607C	Summit County	OH	County Government	58	1
46360R	Irvine	CA	Local Government	37	1
46360T	Irvine	CA	Local Government	35	1
660393	North Las Vegas	NV	Local Government	34	0
825434	Shreveport	LA	Local Government	31	0
549310	Lucas County	OH	County Government	29	0
463805	Irving	TX	Local Government	28	0
759861	Reno	NV	Local Government	27	1
79307T	St Paul	MN	Local Government	25	1
70643U	Pembroke Pines	FL	Local Government	24	0
10047	Akron	OH	Local Government	22	0
85233S	St Louis	MO	Local Government	22	1
555542	Macon Bibb County	GA	Local Government	21	0
270764	East Baton Rouge Parish	LA	County Government	20	0
79488C	Salinas	CA	Local Government	19	0
759829	Reno	NV	Local Government	16	1
743787	Providence	RI	Local Government	15	0
133402	Cameron County	TX	County Government	14	0
86607D	Summit County	OH	County Government	13	1
537374	Little Rock	AR	Local Government	10	0
534310	Lincoln	NE	Local Government	9	1
43615F	Hollywood	FL	Local Government	9	0
613549	Montgomery County	OH	County Government	8	1
650366	Newark	NJ	Local Government	8	1
55553N	Macon Bibb County	GA	Local Government	8	0
690278	Overland Park	KS	Local Government	7	0
66041H	North Las Vegas	NV	Local Government	7	0
79164T	St Louis	MO	Local Government	6	1
743940	Providence	RI	Local Government	5	0
702521	Pasco County	FL	County Government	4	1
29634D	Escondido	CA	Local Government	4	0
344610	Fontana	CA	Local Government	2	0
607715	Modesto	CA	Local Government	2	0
928844	Volusia County	FL	County Government	2	1
873477	Tacoma	WA	Local Government	2	0
696712	Palmdale	CA	Local Government	1	0
759830	Reno	NV	Local Government	1	1
51672	Aurora	IL	Local Government	1	0
768861	Riverside	CA	Local Government	1	1
35895	Anne Arundel County	MD	County Government	1	1

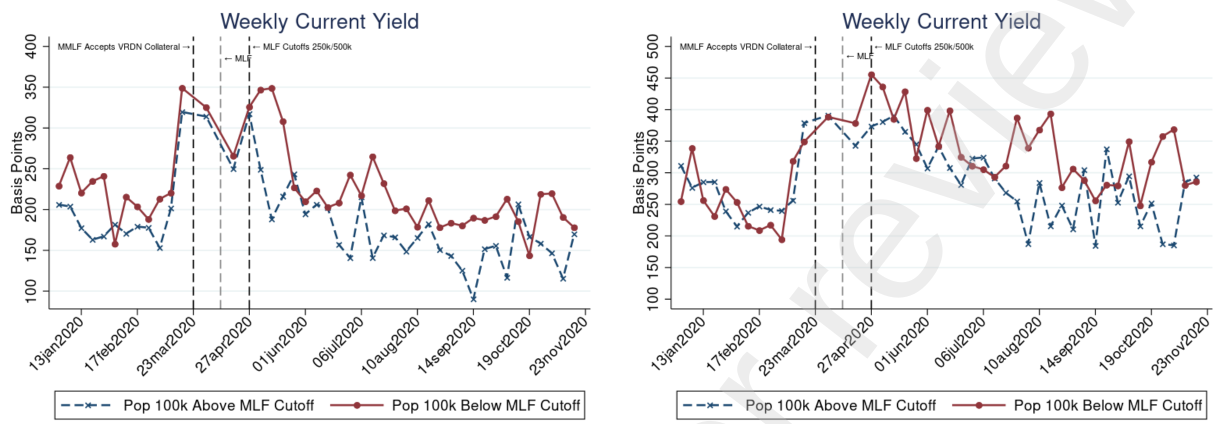
NOTES—Data contain 43 unique issuers with 831 unique outstanding bonds or notes. Each bond contributes additional variation to RD estimates, however inference only leverages differences between issuers (standard errors clustered by relative population). 20 issuers are above the MLF cutoff, while 23 are below. Irvine, Newark, North Las Vegas, Reno, and St. Louis are listed twice, but these reflect different issuers within those cities (for example, economic development corporations may issue their own independent debt).

A.7 Additional Results

In Figure A.4 below, we show more detailed variation behind our aggregated low-rated yields results (from Figure 6 of the main draft), disaggregated by A and BBB issuers separately.

Figure A.4: Mean Yields within 100k Population of MLF Cutoffs, Separately for A and BBB

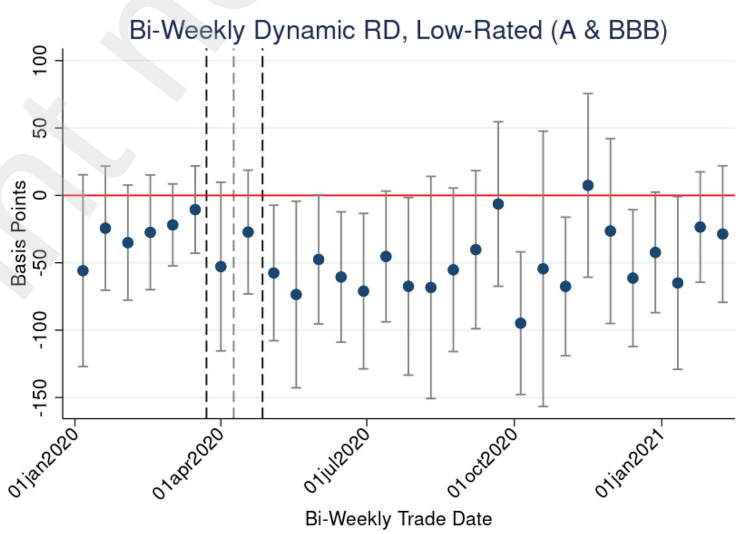
(a) A-Rated Issuers in Jan 2020: (b) BBB-Rated Issuers in Jan 2020:



NOTES—Figure shows mean yields for two subgroups: eligible issuers with cutoff-relative populations between 0 and 100,000 inclusively (blue dashed series), and issuers with cutoff-relative populations greater than or equal to -100,000 and less than 0 (red solid series). See text for sample restrictions and subgroup definitions. Source: MSRB, S&P, Moody's, Fitch

In Figure A.5, we report coefficients from looping through our main RD specification for low-rated issuers, estimated bi-weekly across the support of our sample frame. The specification is identical to Table 2, but omits calendar month fixed effects which are absorbed by the sampling restriction of keeping each 2-week period respectively, and extends the sample period reflecting the latest data.

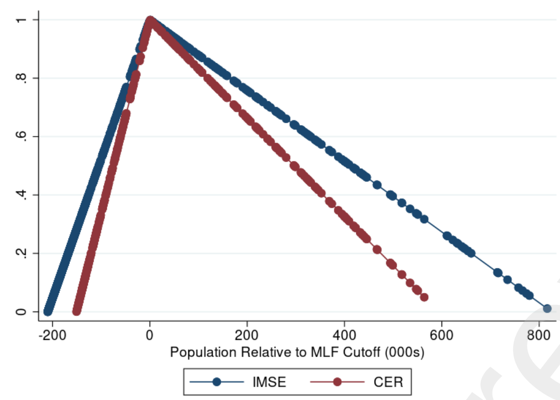
Figure A.5: Dynamic RD Estimates of MLF Access on Secondary Market Yields



NOTES—Estimates and 90% confidence intervals shown looping over bi-weekly intervals of the support, and collapsing all observations between announcement dates into one estimate (consistent with main results). See sample restrictions and notes associated with Table 2 for further details.

In Figure A.6, we present kernel weights generated through our optimal RD procedure, shown for exposition for our main yields estimates results for low-rated issuers.

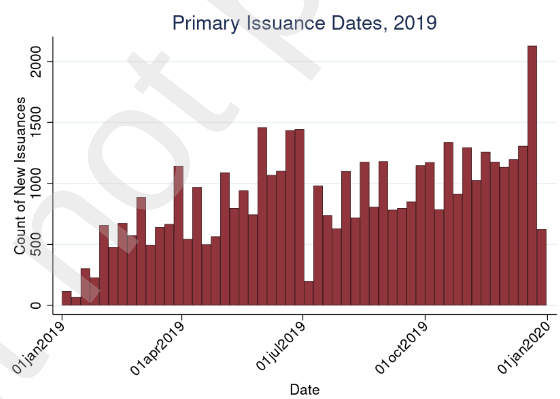
Figure A.6: Triangular Kernel Weights for Asymmetric Optimal Bandwidth, Effects on Yields



NOTES—Figure shows calculated triangular weights using asymmetric IMSE-optimal bandwidth on each side of the cutoff for main yields estimate, as well as an alternative weighting scheme based on the regression coverage error (CER), used for robustness. Weights and bandwidth are associated with the yields dependent variable in the pooled city and county sample in the post-period.

In Figure A.7, we present the seasonality of new issuance for a standard expansion year like 2019, which demonstrates that effects on primary issuance are unlikely due to seasonal mean reversion in issuance.

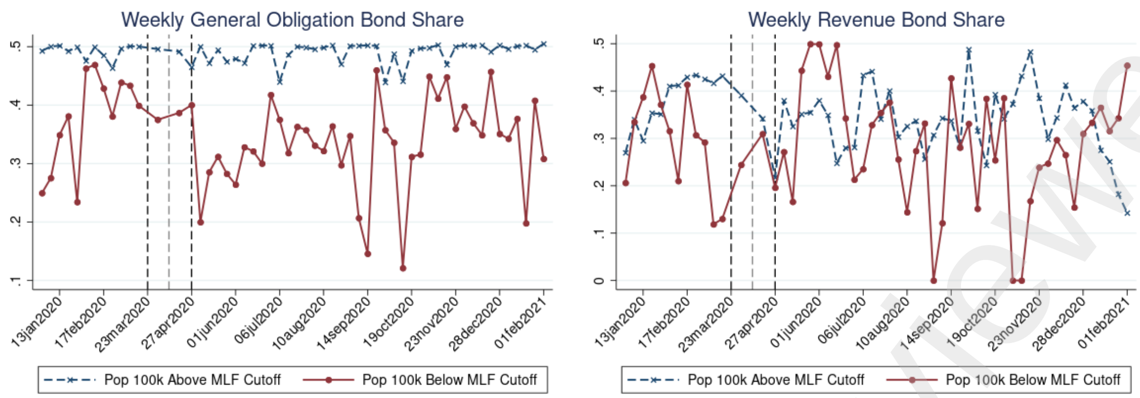
Figure A.7: 2019 Seasonality in Primary Issuance



NOTES—Figure shows all CUSIPs tagged as new primary issuance based on initial offering date by calendar week in 2019. Source: Mergent

In Figure A.8, we present the share of A/BBB trading bonds 100,000 in population above and below the pooled cutoff that are general obligation (left) and revenue (right) bonds respectively. This plot shows that while initial trading patterns support a spike in revenue bond trends when issuers were revealed to be ineligible, this trend does not sustain. In conjunction with earlier work demonstrating balance, composition differences across the cutoff immediately following the MLF announcement and unlikely to be the main factor driving these results.

Figure A.8: Composition Sensitivity: GO and RB A/BBB Trends



NOTES—Figure shows shares of all trading CUSIPs for issuers 100,000 above and below the cutoffs.

In Table A.2 below, we provide power tests for various effect sizes to asses whether we can detect reliable effect sizes (using the common convention of high power exceeding 0.80) for the 21 cities that are above the 250,000 cutoff.

Table A.2: Power Tests on City Running Variable (Cutoff = 250,000)

Level of Employment	$\tau = 1,000$	$\tau = 2,000$	$\tau = 3,000$	$\tau = 4,000$
Employment, City Only Treated	0.10	0.27	0.53	0.78
– Services Employment, City Only Treated	0.11	0.29	0.56	0.81
Employment, City & County Treated	0.30	0.81	0.99	1.00
– Services Employment, City & County Treated	0.22	0.67	0.95	1.00
YoY Level Change in Employment	$\tau = 100$	$\tau = 200$	$\tau = 300$	$\tau = 400$
Employment, City Only Treated	0.09	0.22	0.42	0.65
– Services Employment, City Only Treated	0.07	0.14	0.25	0.40
Employment, City & County Treated	0.16	0.47	0.81	0.97
– Services Employment, City & County Treated	0.12	0.33	0.63	0.87
YoY Percent Change in Employment	$\tau = 1\%$	$\tau = 2\%$	$\tau = 3\%$	$\tau = 4\%$
Employment, City Only Treated	0.10	0.25	0.48	0.72
– Goods Employment, City Only Treated	0.05	0.05	0.06	0.06
– Services Employment, City Only Treated	0.06	0.11	0.18	0.28
Employment, City & County Treated	0.20	0.61	0.92	0.99
– Goods Employment, City & County Treated	0.05	0.07	0.09	0.12
– Services Employment, City & County Treated	0.11	0.28	0.55	0.79

NOTES—We run power tests of our RD estimator for our city sample with a symmetric bandwidth of 250,000 in population. We omit the goods employment rows in the first two panels because our detected RD effects are lower in magnitude than for services and overall employment. Please see Cattaneo et al. (2019b) for more details. To perform the conventional power calculations above, we assume there is ignorable "smoothing bias" on either side of the cutoff. We can therefore interpret these calculations as upper bounds.

We conclude from this that only the largest effect sizes can be reliably estimated with full power, and in Table A.3, provide the estimation results with this caution in mind.

Table A.3: City-Level RD Estimates of MLF Access on Public Sector Employment

	Emp. (1)	Emp. (2)	Δ Emp. (3)	Δ Emp. (4)	% Δ Emp. (5)	% Δ Emp. (6)	N (fixed-bwdth)
a. Pooled Post:							
Overall Employment	-948	-1,045	-52	-49	-1.21	-0.56	29,609
	(694)	(781)	(94)	(94)	(0.89)	(0.72)	
– Goods Employment	-87**	-113***	-1	0	-4.55	-2.29	5,370
	(35)	(41)	(3)	(3)	(5.00)	(4.35)	
– Services Employment	1	-597	-98	-62	-1.01	0.37	19,669
	(826)	(943)	(124)	(120)	(1.03)	(0.74)	
b. Pooled Pre (Placebo):							
Overall Employment	-740	-867	54**	52**	1.04***	1.14***	29,643
	(725)	(819)	(24)	(23)	(0.40)	(0.33)	
– Goods Employment	-83**	-110***	-1	-1	0.22	-3.06	5,260
	(35)	(42)	(2)	(2)	(3.12)	(2.56)	
– Services Employment	281	-229	69**	65**	0.86**	1.20***	19,606
	(880)	(966)	(28)	(28)	(0.42)	(0.39)	
Month FEs		X					
State FEs		X		X		X	
Control Mean (post): Employment	10,067	9,487	-779	-730	-6.64	-7.09	
Control Mean (post): Goods Employment	154	134	-5	-4	-5.28	-4.92	
Control Mean (post): Services Employment	8,555	8,022	-701	-653	-6.94	-7.75	

NOTES—Table presents RD estimates of employment measure in column header and sample period. Each row corresponds to a separate regression with that characteristic as the dependent variable. The discontinuity measures the jump in the regression function at population eligibility 0 (relative to cutoff). The Control Mean denotes the mean of that characteristic immediately to the left of relative population 0. Each regression uses a fixed bandwidth with a triangular kernel to weight observations (see Section 4 for details). Percents in columns (5) and (6) range from 0 to 100. Standard errors are conservatively clustered by population relative to the cutoff (the running variable); as such, inference is drawn across unique issuers. *** p≤0.01, ** p≤0.05, * p≤0.1.

In the above table, we generate city employment as the county-weighted employment across counties spanned by both eligible and ineligible cities. If one were to ignore power concerns just discussed and take these results at face value, we are unable to detect any positive effects using this sample.

In Table A.5, we provide a full list of baseline summary statistics prior to March 23, including the entire calendar year of 2019. These do not restrict to issuers within 100,000 of the cutoff, and instead are meant to report endogenous differences between the samples of eligible and non-eligible issuers were they to be compared naively, for example in a difference-in-differences analysis.

Table A.4: Sensitivity of Main Yields Results to RD Kernel and Bandwidth Choice

	(1)	(2)	(3)	(4)	(5)	(6)
a. Pooled Post:						
Overall Yield	-19.26	-10.48	-11.67	-4.94	-15.64	-9.04
City Only	-27.97	-7.17	-17.85	2.34	-25.90	-4.28
County Only	-15.06	-20.44	-17.07	-23.54*	-14.45	-21.94
High-Rated	-1.54	-0.41	1.36	1.88	-1.28	-0.09
Low-Rated	-72.28**	-70.47*	-60.79*	-60.09*	-72.84**	-70.73*
b. Pooled Pre (Placebo):						
Overall Yield	-13.25	-5.31	-8.81	-2.17	-11.92	-4.94
City Only	-9.13	0.06	-4.79	3.71	-8.70	0.57
County Only	-21.61	-15.94	-18.90	-12.77	-21.16	-15.46
High-Rated	-6.15	-3.40	-6.01	-0.94	-6.07	-3.13
Low-Rated	-24.87	-20.34	-28.87	-3.49	-25.25	-19.51
Bandwidth	IMSE	Fixed	IMSE	Fixed	IMSE	Fixed
Kernel	TRI	TRI	UNI	UNI	EPN	EPN

NOTES—Table presents RD estimate sensitivity of current yields (in basis points) for MSRB-active trades during each sample period to different RD kernel and bandwidth choices. Each row corresponds to a separate regression with that characteristic as the dependent variable. The discontinuity measures the jump in the regression function at population eligibility 0 (relative to cutoff). IMSE: Integrated Mean-Squared Error; TRI: Triangular; UNI: Uniform; EPN: Epanechnikov. State and calendar month fixed effects are included in all regressions. Standard errors are clustered by population relative to the cutoff (the running variable), suppressed here fr exposition. See text for further sample restrictions. *** p≤0.01, ** p≤0.05, * p≤0.1.

Table A.5: Full Baseline Summary Statistics, January 1, 2019 to March 23, 2020

	MLF Eligible		MLF Ineligible		MLF Eligible - Ineligible
	Mean/SD (1)	# Observations (2)	Mean/SD (3)	# Observations (4)	Δ/SE (5)
A. MSRB-Bloomberg Trade-Level Data					
Coupon Rate (b.p.)	430.3	669,311	384.2	844,395	46.2***
	[135.1]		[116.6]		(0.21)
Security Price (per 100 par)	108.0	671,659	105.8	844,500	2.23***
	[8.95]		[7.45]		(0.014)
Current Yield (b.p.)	203.0	620,560	209.6	825,352	-6.68***
	[83.2]		[80.3]		(0.14)
Δ Yield (Feb20-Jan20)	-0.066	281,596	-0.14	153,899	0.078***
	[1.21]		[0.95]		(0.0033)
Δ Yield YoY (Jan20-Jan19)	-0.89	262,725	-1.11	124,523	0.22***
	[4.28]		[1.17]		(0.0090)
Δ Yield YoY (Feb20-Feb19)	-1.00	242,935	-1.07	110,336	0.074***
	[1.56]		[1.07]		(0.0045)
Amount Outstanding (MM)	2328.8	671,659	144.5	844,500	2184.3***
	[2982.3]		[224.1]		(3.65)
Maturity Size (MM)	3542.0	671,659	215.7	844,500	3326.2***
	[4209.3]		[305.5]		(5.15)
Tenor of Bond (Years)	14.8	671,464	13.2	844,500	1.55***
	[7.64]		[6.98]		(0.012)
Remaining Duration of Bond (Years)	9.22	671,464	8.72	844,500	0.51***
	[7.02]		[6.74]		(0.011)
Market Share of Issuer	0.95	671,659	0.064	844,500	0.89***
	[1.06]		[0.11]		(0.0013)
Number of Securities by Issuer	306.4	671,659	123.0	844,500	183.3***
	[210.6]		[107.5]		(0.28)
Par Traded (1000s)	290.9	671,659	95.4	844,500	195.5***
	[1915.2]		[579.9]		(2.42)
S&P Ratings (1-7 scale)	5.67	575,172	5.83	624,205	-0.16***
	[0.86]		[0.65]		(0.0014)
Moody's Ratings (1-7 scale)	5.78	557,662	5.88	474,590	-0.099***
	[0.90]		[0.92]		(0.0018)
Fitch Ratings (1-7 scale)	5.65	424,063	5.63	149,058	0.018***
	[1.03]		[0.79]		(0.0026)
Time of Day of Trade (minute)	770.6	671,659	776.1	844,500	-5.53***
	[131.7]		[132.2]		(0.22)
B. QCEW Month-County Loc. Gov. Emp. Data					
Δ Employment	627.8	228	19.9	5,250	607.9***
	[861.7]		[99.5]		(57.1)
Δ Goods Employment	9.77	130	0.20	2,402	9.57***
	[35.4]		[8.89]		(3.11)
Δ Service Employment	1509.8	228	23.1	5,250	1486.7**
	[10905.4]		[414.9]		(722.2)

NOTES—Table presents summary statistics for key variables of the analysis using a longer period prior to the first Federal intervention in the municipal market, along with a two-sided t-test with heteroskedastic-robust standard errors). See Appendix A.4 for further details on variable definitions and construction.