Should Central Banks Lend to Local Governments in Times of Crisis? Evidence from the Federal Reserve's Covid-19 Municipal Liquidity Facility

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Senior Essay
April 2021

Abstract

In the spring of 2020, the United States Federal Reserve responded to the emerging Covid-19 crisis by establishing the Municipal Liquidity Facility (MLF), an unprecedented program designed to support local governments through large-scale purchases of municipal securities. In this paper, I estimate the effects of this program on municipal borrowing costs and secondary market transaction costs. Using city population cutoffs for MLF eligibility as a treatment identification mechanism, I perform difference-in-differences analyses to estimate these causal effects. I find that the Federal Reserve's policy announcement modestly reduced municipal bond yields when considering a narrow bandwidth of cities just above and just below the population eligibility cutoff. Moreover, the policy significantly reduced effective spreads between buyers' prices and sellers' prices, suggesting that liquidity conditions improved in the municipal bond market. Subsequent analyses of heterogeneous treatment effects reveal that these effects were mixed, however. The policy reduced yields and effective spreads for revenue bonds, but not for general obligation bonds. I conclude by discussing the efficacy of the MLF and proposing avenues for future research.

Acknowledgments

First and foremost, I owe a debt of gratitude to my advisor, Professor Yair Listokin. His tireless support and candid feedback over the past year made this paper possible and strengthened my interest in economic policy analysis. Additionally, I thank my peers who provided assistance on technical aspects of this project, particularly Ayumi Sudo and Zara Contractor. I am also grateful for the Yale Economics Department, which provided financial support for my work through the Herb Scarf Summer Research Opportunities Program and the Tobin Research Assistant Program. Finally, I thank my parents, who have nurtured my curiosity for as long as I can remember.

Introduction

As the Covid-19 pandemic emerged in the United States in March 2020, financial markets began to spiral out of control. In the equity markets, widespread fear of a pandemic-induced recession led to selloffs that were historic in magnitude. On March 16th, the Dow Jones Industrial Average plummeted by a record-setting 12.9 percent, exceeding the infamous stock market plunge that occurred on "Black Monday" in 1929 (Quddus, 2020). The fixed income markets experienced a similar meltdown. In the corporate bond market, for example, liquidity dried up and spreads surged from a pre-pandemic average of 20 basis points to nearly 40 basis points by mid-March (Petruno, 2020). The rapid destabilization of financial markets necessitated intervention by the Federal Reserve.

Beginning in mid-March, the Fed unveiled a wide array of programs to restore liquidity and stability. Among these programs were several measures designed to calm the volatile corporate bond market, particularly the Primary and Secondary Corporate Credit Facilities. As part of these programs, the Fed pledged to directly purchase corporate bonds, thereby absorbing a substantial amount of credit risk on its balance sheet. The Fed also established large-scale asset purchase programs for U.S. Treasury bonds and mortgage-backed securities. Although the Fed initiated asset purchases in a variety of critical markets, the municipal bond market will serve as the focus of this paper.

The municipal bond market faced a unique set of challenges during the nascent stages of the Covid-19 crisis. Although municipal bonds are typically regarded as safe assets, they were perceived as riskier than usual in March 2020 as investors feared the possibility that local and state tax revenues would suffer in a pandemic-fueled recession. These fears likely contributed to the enormous selloffs that occurred in the municipal market. From March 11th to March 18th, investors sold $12 billion of municipal mutual fund assets, representing nearly 2.5 percent of this market (Schüle and Sheiner, 2020). During this mass flight from the municipal market, the Municipal Market Data Yield—a frequently cited index of municipal bond yields computed by Thomson Reuters—quintupled from 0.5 to 2.5 percent. With diminishing liquidity and soaring borrowing costs, state and local governments found it increasingly difficult to borrow at a time when they were under significant fiscal duress.

In order to support local and state governments, the Fed established the Municipal Liquidity Facility (MLF). On April 9th, the Fed announced the formation of the MLF, stating that pursuant to Section 13(3) of the Federal Reserve Act, it would purchase up to $500 billion of municipal securities from cities with populations of at least one million residents, counties with populations of at least two million residents, and all U.S. states ("Federal Reserve takes additional actions to provide up to $2.3 trillion in loans to support the economy"). On April 27th, the Fed expanded the scope of the MLF by committing to lend to cities with populations of at least 250,000 and counties with populations of at least 500,000 ("Federal Reserve Board announces an expansion of the scope and duration of the Municipal Liquidity Facility"). The April 27th update further specified that eligible securities included municipal bonds with investment grade credit ratings and maturity periods of no more than 36 months. Moreover, the April 27th announcement noted that the MLF would be in effect until December 31st, 2020. The goal of the MLF was to enable state and local governments to better manage their cash flow pressures by effectively providing a backstop to the municipal market, thereby reducing borrowing costs and improving liquidity conditions.

In this paper, I empirically evaluate the efficacy of the MLF by estimating the impact of this program on municipal borrowing costs and liquidity costs. In order to estimate these causal effects, I exploit the city population thresholds for MLF eligibility, as defined in the Fed's April 27th announcement, as an identification mechanism for treatment status. I then perform difference-in-difference analyses to measure changes in municipal bond yields and effective spreads during the four-week window around the April 27th policy announcement. I find that in the two weeks following the Fed's April 27th announcement, municipal borrowing costs and liquidity costs both declined, suggesting that the MLF produced the desired effects, at least in the short term. However, an analysis of heterogeneous treatment effects reveals that the success of the MLF was mixed. The decline in yields and effective spreads occurred for revenue bonds, which represent the majority of bonds traded during this period, but not for general obligation bonds, which are backed by the taxing authority of municipal issuers. Thus, while the MLF appears to have achieved its goals for a large class of municipal bonds, the desired effects did not occur for all categories of eligible bonds. These findings provide an initial scorecard for the Fed's unprecedented MLF program, while also contributing new insights to the broader literature on large-scale asset purchases by central banks.

The remainder of this paper is organized as follows. In Part 1, I survey the existing literature on the effects of quantitative easing on borrowing costs and liquidity. In Part 2, I describe my data sources and sampling methods. In Part 3, I discuss my empirical framework for estimating the causal effects. In Part 4, I present my findings for the effect of the Fed's announcement on municipal borrowing costs. In Part 5, I present my findings for the effect of the policy announcement on secondary market transaction costs. In Part 6, I discuss the efficacy of the MLF and integrate my findings with the emerging literature on the Fed's policy response to Covid-19. Finally, in Part 7, I provide concluding remarks.

1. Literature Review

1.1 Quantitative Easing and Borrowing Costs

The literature on quantitative easing and borrowing costs, as measured by bond yields, is relatively well established. The dominant finding in this literature is that large scale asset purchases (LSAPs) by central banks reduce yields on government-issued bonds. This finding has been demonstrated in a variety of contexts, including the Fed's quantitative easing programs during the Great Recession (Krishnamurthy and Vissing-Jorgensen, 2011; D'Amico and King, 2013), the Bank of England's asset purchases from 2009-2010 (Joyce et al., 2012), the European Central Bank's asset purchase program in 2015 (DeSantis, 2020), and the Bank of Japan's quantitative easing policy from 2001-2006 (Ugai, 2007). This well-established effect of LSAPs on government-issued bond yields can be understood through a simple supply and demand framework. In the market for government-issued bonds, the entrance of a large committed buyer, such as a central bank, leads to an outward shift of the demand curve. As a result, the equilibrium bond price increases. Since bonds and yields are inversely related, the increase in bond prices corresponds to a decrease in bond yields. In light of this simple framework, economists have unsurprisingly found that a central bank's commitment to purchase bonds consistently leads to a reduction in yields.

In order to quantify the effects of LSAPs on yields, most studies in this domain employ an event study methodology. Using this approach, studies estimate the impact of central bank LSAPs on yields by identifying the time at which the central bank first announced the LSAP, constructing a short temporal window around the policy announcement, and then quantifying the change in yields during this window, relative to another window when no such LSAP announcement was made. A key limitation of this method, however, is that during any given event study window, yields might be affected not only by the announcement of interest, but also by other announcements or developments in the market. In order to address this limitation, existing analyses have used relatively short temporal windows to isolate the effect of the LSAP from other concurrent events that could affect asset prices. For example, Jakl (2017) uses two-day event windows to quantify the impacts of several Fed chairman speeches and quantitative easing announcements on yields. Krishnamurthy and Vissing-Jorgensen (2011) use an even more restrictive window; they measure changes in yields over the span of hours rather than days. In this paper, I overcome this challenge by exploiting MLF lending eligibility thresholds as an identification strategy for treatment status, thereby allowing me to measure changes in yields over a longer event study window without the same level of concern for confounding effects.

Since the MLF is the first time that the Fed has engaged in large scale purchases of municipal securities, the literature on LSAPs and yields in the context of municipal bonds is virtually nonexistent. To the best of my knowledge, as of April 2021, there is only one working paper that has estimated the causal effects of the MLF on municipal borrowing costs. Haughwout et al. (2021) exploit MLF population eligibility cutoffs and perform a regression discontinuity to estimate the effects of the Fed's April 27th policy announcement on yields. They report that the policy announcement reduced yields for a subset of municipal bonds with low credit ratings, but the announcement had no effect on yields in their overall sample.

The current paper differs from the analysis conducted by Haughwout et al. in several ways. First, although both of our analyses exploit the same population eligibility thresholds, I pursue a difference-in-differences approach rather than a regression discontinuity approach. Second, I augment my main analysis by exploring heterogeneous treatment effects for categories of bonds that previously have not been considered (e.g., general obligation versus revenue bonds). Third, my analysis provides a cleaner estimate of the MLF's short-term effects because I use a four-week window around April 27th, during which there were no other Fed announcements related to the MLF. By contrast, Haughwout and colleagues' analysis provides a better estimate of the long-term effects because they use a window that extends to November 2020, but this longer window also exposes their analysis to potential confounding effects that might arise from subsequent modifications to MLF eligibility that occurred during the summer of 2020. The current paper therefore complements the analysis conducted by Haughwout et al., while also contributing to the broader literature on quantitative easing and borrowing costs.

1.2 Quantitative Easing and Liquidity

Compared to the literature on quantitative easing and borrowing costs, the body of research on quantitative easing and liquidity costs is less firmly established. In a widely cited study on LSAPs and liquidity, Christensen and Gillan (2018) analyze the Fed's large-scale purchases of Treasury inflation-protected securities (TIPS) from November 2010 to June 2011. They report that the Fed's intervention reduced liquidity premiums for TIPS by ten basis points. Underlying this effect is the theory that when a large buyer (such as the Fed) enters the market, bargaining power shifts from buyers to sellers because sellers can simply bypass private market participants who demand high liquidity premiums and instead submit bids to the Fed. As a result, buyers are willing to accept lower liquidity premiums and price frictions between buyers and sellers diminish.

Empirical evidence for Christensen and Gillan's theory, however, is mixed. Kandrac and Schlusche (2013) find no significant effects of LSAP transactions on Treasury bond liquidity between 2009 and 2012. Moreover, some academics and policymakers have cautioned that

LSAPs may actually deteriorate market liquidity. In his 2012 speech at Jackson Hole, former Fed Chairman Ben Bernanke remarked: “The Federal Reserve is limited by law mainly to the purchase of Treasury and agency securities; the supply of these securities is large but finite, and not all of the supply is actively traded. Conceivably, if the Federal Reserve became too dominant a buyer in certain segments of these markets, trading among private agents could dry up, degrading liquidity and price discovery” (Bernanke, 2012). Essentially, the overbearing presence of a large, powerful buyer (such as the Fed) can crowd out private market participants, thereby reducing trading activity and straining market liquidity. Bernanke raises a valid point, but his concerns are unlikely to apply to the MLF because the Fed’s involvement in municipal markets was very modest during 2020.

Since the Fed has never implemented an asset purchase program in the municipal market, there is no existing literature on quantitative easing and liquidity in the context of municipal bonds. This paper is therefore the first to explore the causal effects of LSAPs on municipal market liquidity. There is, however, an emerging literature on the effects of the Fed’s Secondary Market Corporate Credit Facility (SMCFF) during the early stages of the Covid-19 crisis. Gilchrist et al. (2020) perform difference-in-difference analyses that exploit SMCFF eligibility requirements, reporting that bid-ask spreads declined by ten basis points within ten days after the Fed’s first announcement of the new corporate credit facilities. Kargar et al. (2020) employ a similar empirical strategy and report that when the Fed later expanded the scope of the SMCFF, bid-ask spreads fell for both eligible and ineligible corporate bonds. These findings suggest that the Fed’s pledge to absorb corporate debt on its balance sheet significantly improved liquidity conditions in the corporate bond market. Building upon these findings, I hypothesize that the MLF had a similar effect on municipal market liquidity during the weeks following the Fed’s

April 27th announcement. My analysis provides the first estimates of the causal effects of the MLF on liquidity, while also contributing new insights to the growing literature on LSAPs and liquidity.

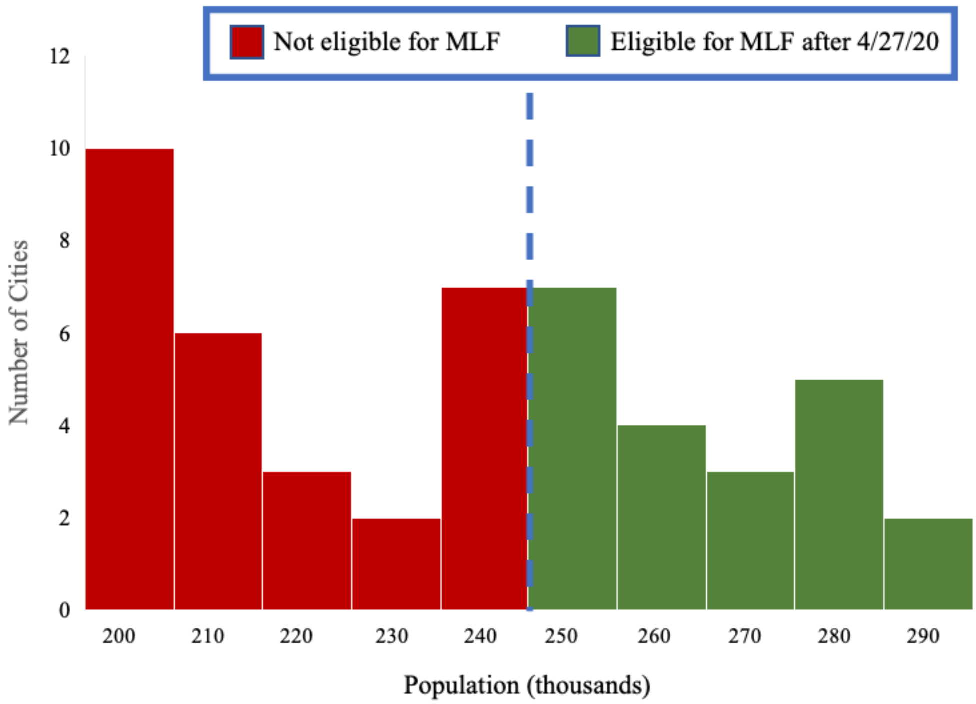
2. Data Sources and Sample Construction

2.1 Municipal Issuers

In this paper, I exploit city population thresholds for MLF eligibility to estimate the causal effects of the MLF on municipal borrowing costs and transaction costs. On April 9th, the Fed announced the formation of the MLF, stating that cities with more than one million residents would be eligible for lending. On April 27th, the Fed expanded the scope of the MLF by making the program available to all cities with populations of at least 250,000. In order to identify eligible cities, I use the same US Census Bureau population estimates used by the Fed ("City and Town Population Totals: 2010-2019"). According to these population estimates, only ten cities were eligible for the MLF after the initial April 9th announcement, but the April 27th announcement expanded eligibility to a total of 87 cities. Given that my empirical strategy entails comparing municipal bond trades in cities just above the eligibility threshold versus in cities just below the threshold, this paper exclusively analyzes the effects of the April 27th announcement due to the greater density of cities situated around the 250,000 threshold. For my regression analyses, I consider all cities with populations between 200,000 and 300,000 residents, thereby restricting the sample to a symmetric window of cities with populations within 50,000 of the eligibility cutoff.

The resulting sample includes 49 cities, 21 of which were eligible for MLF lending. Figure 1 maps these 49 cities in relation to the population eligibility threshold specified in the April 27th announcement.

Figure 1. Distribution of City Issuers by Population



For most of my regression analyses, I use the full sample of 49 cities. In some cases, however, I also consider how the effects vary when restricting the sample to a narrower bandwidth of cities around the eligibility cutoff.

2.2 Municipal Bond Trades

After defining the sample of municipal issuers, I compiled a dataset of all municipal bond trades executed in the sample of 49 cities between April 13th and May 11th. This four-week symmetric window around the April 27th announcement allows me to estimate the short-term effects of the policy announcement during the two weeks after the announcement, relative to the two weeks prior. I chose this four-week period as my sample window because the Fed made zero modifications to MLF eligibility during this period, thereby isolating the effect of the April 27th announcement from the potential effects of the April 9th announcement.

For each of the 49 cities, I obtained all municipal bond trading data from the Municipal Securities Rulemaking Board's Electronic Municipal Market Access (EMMA) service. EMMA publishes data on transactions in the secondary market for municipal securities and allows users to search for transactions by city ("Municipal Securities Rulemaking Board"). For each municipal trade, EMMA provides information on each of the following attributes: the bond description, the date and time of the trade, the coupon rate, the maturity date, the price and yield of the trade, the par value traded, the trade type (customer sold, customer bought, or interdealer trade), and the bond type (general obligation, revenue, or double barrel). Since EMMA does not readily provide CUSIP codes for trades, I use unique combinations of bond descriptions and maturity dates to identify each unique bond and essentially construct my own equivalent of a CUSIP identifier for each unique bond.

The resulting dataset consists of 2,752 unique municipal bonds traded and 13,320 trades executed during the four-week period from April 13th to May 11th. Table 1 provides descriptive statistics for several of the variables that are used in subsequent analyses.

Table 1. Descriptive Characteristics of Municipal Bond Dataset

	Trade Characteristics		
	25th percentile	Median	75th percentile
Price	102.051	106.46	113.249
Yield	1.48	2.00	2.594
Coupon	4	5	5
Remaining Time to Maturity	3.34 years	7.28 years	13.33 years
	Distribution by Trade Type		
	Customer Bought	Customer Sold	Interdealer Trade
	36.8%	24.8%	38.4%
	Distribution by Bond Type		
	General Obligation	Revenue	Double Barrel
	43.3%	52.5%	4.2%

2.3 Municipal Borrowing Costs: Operationalization and Sample Restrictions

In subsequent analyses, I operationalize municipal borrowing costs in terms of yields on municipal bond trades, where higher yields correspond to higher borrowing costs for municipalities. In order to mitigate the effects of outliers, I winsorized all trades with yields below the 1st percentile and above the 99th percentile, consistent with the sample restrictions used by Haughwout et al. (2021). After removing outliers, the resulting sample consists of 13,047 trades across 2,733 unique municipal bonds. Additionally, in order to analyze bond-fixed effects—that is, the change in yields for each unique bond from before the announcement to after the announcement—I removed all bonds for which there were trades only before or only after the announcement. In other words, I only include bonds for which there were trades both before *and* after the April 27th policy announcement. The final dataset consists of 5,589 trades and 692 unique municipal bonds. In order to compute the bond-level change in yields over time, for each bond, I subtracted the average yield of all trades executed before April 27th from the average yield of all trades executed after April 27th. In Section 3.2, I incorporate this measure for change in yields into my empirical framework.

2.4 Transaction Costs: Operationalization and Sample Restrictions

In order to study the effects of the MLF on liquidity conditions in the municipal market, I estimate changes in transaction costs during the four-week window around the Fed's April 27th announcement. Transaction costs have been widely used to estimate municipal market liquidity, or the ease with which securities can be traded, with lower transaction costs reflecting greater liquidity (Harris et al., 2006; Green et al., 2007; Cuny, 2016). Prior research on the municipal market has typically operationalized transaction costs in terms of effective spreads, defined as the difference between the price that the buyer pays to the dealer and the price that the seller receives from the dealer (Wu, 2018). In illiquid market conditions, dealers demand higher liquidity premiums, resulting in higher effective spreads between buyer and seller prices.

In order to calculate effective spreads, I first identified unique transactions by matching pairs of "customer bought" and "customer sold" trades executed within one hour of each other with the same bond description, maturity date, and trade volume. Each unique transaction therefore consists of a customer purchase and customer sale of the same municipal security and the same trade volume, executed within one hour of each other. Using this matching algorithm, I identified 702 unique transactions over the four-week window around April 27th. For each unique transaction, I calculated the effective spread using the formula below, consistent with prior literature:

$$\text{Effective Spread} = \frac{\text{Buyer's Price - Seller's Price}}{\text{Seller's Price}} \text{ x } 100$$

I then multiplied the effective spread for each transaction by 100 in order to express spreads in terms of basis points, thereby simplifying the interpretation of subsequent regression results. After calculating the effective spreads for each transaction, I winsorized all transactions with effective spreads below the 1st percentile and above the 99th percentile, consistent with the sample restrictions used by Schwert (2017). The final dataset consisted of 698 unique transactions. In Section 3.3, I incorporate effective spreads into my empirical framework for analyzing the effects of the MLF on transaction costs.

3. Overview of Empirical Framework

3.1 Exploiting MLF Eligibility Cutoffs as a Treatment Identification Mechanism

The city population cutoffs for MLF eligibility provide a natural experiment setting to study the causal effects of the MLF policy announcement on municipal bond yields and spreads. In this paper, I use the eligibility cutoffs announced on April 27th (population > 250,000) as an identification strategy for treatment status. The rationale for this empirical strategy is that municipal bonds in cities just above the eligibility cutoff should be similar to bonds in cities just below the cutoff in virtually all respects; they should only differ vis-à-vis their eligibility for the MLF. I use this treatment identification strategy for analyses pertaining to both yields and effective spreads. In Sections 3.2 and 3.3, I describe how this treatment identification strategy fits into my regression models for yields and effective spreads.

3.2 Regression Design for Estimating the MLF's Effects on Yields

In order to estimate the effects of the MLF on yields, I consider bond-fixed effects. That is, for each bond, I calculate the change in average yield from the two-week period before the

Fed’s announcement to the two-week period after the announcement. The formula below illustrates how I calculated the change in yield for each bond:

$$\Delta \text{ Yield} = \frac{\text{Average Yield After Announcement} - \text{Average Yield Before Announcement}}{\text{Average Yield Before Announcement}} \text{ x } 100$$

The formula above produces the change in yield in percentage terms for each bond. I then multiplied the resulting values by 100 in order to express the change in yield in terms of basis points, thereby simplifying the interpretation of subsequent regression results. Using this bond-level change in yields as the dependent measure, I defined the following the regression model to estimate the fixed effects of the MLF on yields, where the subscript *i* corresponds to each unique bond:

$$\Delta \text{Yield}_i = \alpha + \beta^* \text{Treated}_i + \varepsilon_i$$

“Treated” is a dummy variable that takes on a value of 0 for cities below the population eligibility cutoff and 1 for cities above the cutoff. In this model, the coefficient β quantifies the change in yields in eligible cities, relative to ineligible cities. Consistent with the regression specifications used by Haughwout et al. (2021), I did not include any control variables for bond characteristics (e.g., time to maturity) because my analysis measures fixed effects for each bond, thereby minimizing the influence of such bond characteristics on changes in yields. I do, however, investigate heterogeneous treatment effects on the basis of such bond characteristics.

In particular, I test for heterogeneous treatment effects on the basis of on bond type (general obligation versus revenue bonds) by creating a dummy variable for bond type and defining the following regression model, where the subscript *i* corresponds to each unique bond:

$$\Delta \text{Yield}_i = \alpha + \beta\text{*Treated}_i + \gamma\text{*Bond Type}_i + \delta\text{*Treated}_i\text{*Bond Type}_i + \varepsilon_i$$

Here, I define "Bond Type" as a dummy variable that equals 0 for general obligation bonds and 1 for revenue bonds. The coefficient on the interaction term, δ, estimates the difference in the treatment effects for revenue bonds versus general obligation bonds. Taken together, the regression models defined in this section allow me to both estimate the main effect of the MLF on yields and examine whether this treatment effect exhibits heterogeneity on the basis of bond type.

3.3 Regression Design for Estimating the MLF's Effects on Spreads

Given the small sample size of unique transactions consisting of customer purchase customer sale pairs matched by trade volume and trade time (n=698), I do not consider bond-fixed effects in my analysis of spreads. Instead of using each bond as the unit of observation, as I do in my analysis of yields, I now use each unique transaction as the unit of observation. In order to estimate the main effect of the MLF on spreads, I define the following model using a difference-in-differences approach, where the subscript *i* corresponds to each unique transaction:

$$\text{Effective Spread}_i = \alpha + \beta\text{*Time}_i + \gamma\text{*Treated}_i + \delta\text{*Time}_i\text{*Treated}_i + \chi\text{*Par Value}_i + \lambda\text{*Time to Maturity}_i + \theta\text{*Coupon}_i + \varphi\text{*Trade Date}_i + \varepsilon_i$$

Since this analysis does not consider bond-fixed effects, I include control variables for individual bond characteristics that could affect spreads (par value, time to maturity, and coupon), consistent with the control variables used by Wu (2018). Moreover, I include a "Trade Date" variable in order to estimate day-fixed effects. I define "Time" as a dummy variable that equals 0 for transactions that occurred before the Fed's April 27th announcement and 1 for transactions that occurred after the announcement. The coefficient on the interaction term, δ, quantifies the main effect of the MLF on spreads in eligible cities, relative to ineligible cities.

Once again, I supplement my estimates of main effects with an analysis of heterogeneous treatment effects on the basis of bond type. In order to compare the magnitudes of treatment effects for general obligation versus revenue bonds, I create a dummy variable for bond type and define the following regression model, where the subscript *i* corresponds to each unique transaction:

$$\text{Effective Spread}_i = \alpha + \beta*\text{Time}_i + \gamma*\text{Treated}_i + \delta*\text{Bond Type}_i + \chi*\text{Time}_i*\text{Treated}_i + \lambda*\text{Treated}_i*\text{Bond Type}_i + \varphi*\text{Time}_i*\text{Bond Type}_i + \theta*\text{Time}_i*\text{Treated}_i*\text{Bond Type}_i + \kappa*\text{Trade Date}_i + \varepsilon_i$$

Here, I define "Bond Type" as a dummy variable that equals 0 for general obligation bonds and 1 for revenue bonds. The coefficient on the three-way interaction term, θ, quantifies the difference in the treatment effects for revenue bonds versus general obligation bonds. Taken together, the regression models defined in this section allow me to both estimate the main effect of the MLF on spreads and examine whether this treatment effect exhibits heterogeneity on the basis of bond type.

4. Results: Effects of MLF on Yields (Municipal Borrowing Costs)

4.1 Main Effects

Table 2 presents the main effects of the MLF announcement on municipal bond yields, stratified by city population bandwidths of varying sizes.

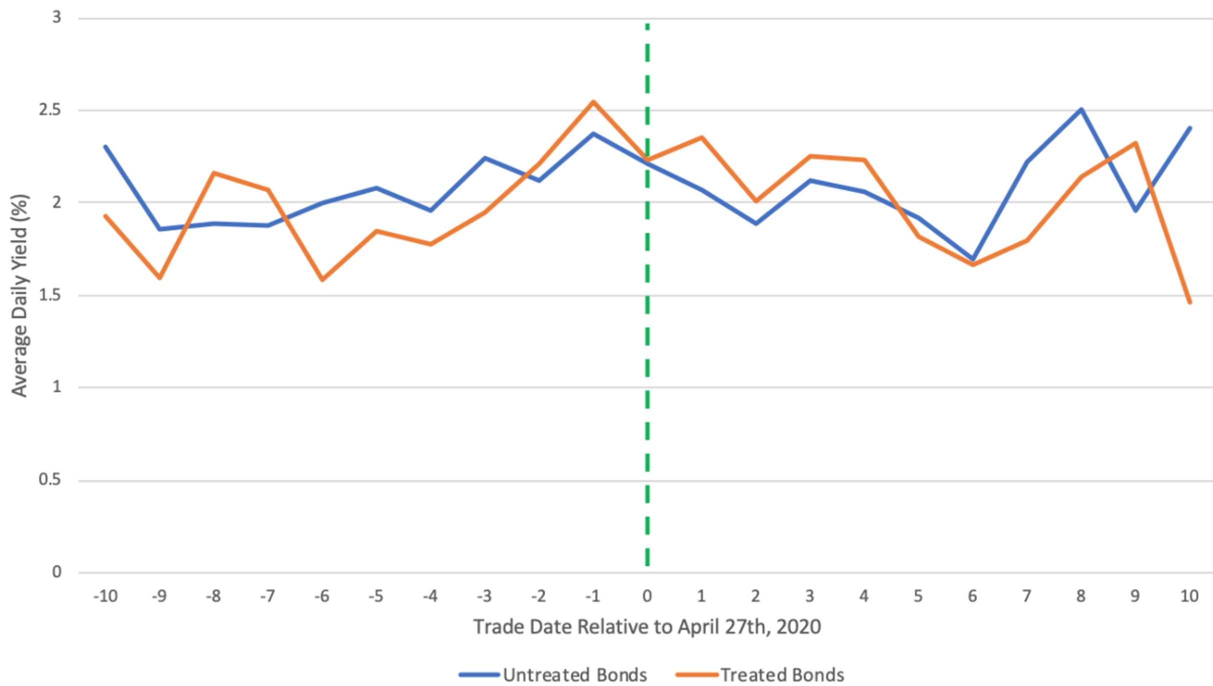
Table 2. Main Effect of MLF on Yields

	Δ Yield (basis points)				
Independent Variable	*Population: 200-300k (n=5,589)*	*Population: 210-290k (n=4,376)*	*Population: 220-280k (n=2,766)*	*Population: 230-270k (n=2,153)*	*Population: 240-260k (n=1,558)*
Treated	-0.91 (3.60)	-0.67 (4.09)	-2.04 (5.25)	-5.62 (5.22)	-10.44*** (2.98)

NOTE: Regression coefficients for change in yield are expressed in terms of basis points. For example, a coefficient of -10.44 indicates that municipal bond yields decreased by 10.44 basis points in cities eligible for the MLF, relative to cities not eligible for the MLF. Each column represents a separate regression with different sample restrictions based on city population. Treated is a dummy variable that takes on a value of 0 for bonds that were never eligible for the MLF and 1 for bonds that were eligible for the MLF after April 27th, 2020. Robust standard errors are in parentheses and are clustered at the city level. Statistical significance was determined using t-tests in STATA.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

As seen in Table 2, the MLF policy announcement had no significant effect on yields in our overall sample of cities with populations ranging from 200,000 to 300,000. The graph below further illustrates that among cities with populations ranging from 200,000 to 300,000 residents, although eligible and ineligible bonds exhibited roughly parallel trends in the two weeks leading up to the April 27th announcement, there is no discernible effect of the MLF announcement on yields for eligible versus ineligible bonds during the following two weeks.

Figure 2. Trends in Daily Average Yields for Treated and Untreated Municipal Bonds



NOTE: In the figure above, each data point represents the average daily yield (%) for the sample of treated bonds (population > 250,000) or untreated bonds (population < 250,000). The vertical dashed line represents April 27th, 2020, the date when the Fed announced the expansion of MLF eligibility to all cities with populations of 250,000 or more residents. For this graph, I used the full sample of bonds from cities with populations ranging from 200,000 to 300,000 residents.

However, when considering narrower bandwidths of cities immediately above and below the population eligibility cutoff, the policy does appear to have had a significant effect on yields. In the most restrictive regression specification, when looking at bonds in cities with populations ranging from 240,000 to 260,000, yields decreased by 10 basis points in eligible cities, relative to ineligible cities. In this narrow sample, municipal bonds had an average yield of 2.19% (219 basis points) during the two weeks before the April 27th MLF announcement. The 10-basis-point decrease in yields therefore represents a 4.6% decline in municipal borrowing costs for cities that narrowly qualified for the MLF, relative to cities with populations narrowly missed qualifying for the MLF.

4.2 Comparing Treatment Effects by Bond Type

Having estimated the main effects of the MLF on yields, I subsequently evaluated whether the effect on yields was driven by certain types of bonds. In particular, I was interested in determining whether the MLF's effect on yields differed for general obligation versus revenue bonds. In order to address this question, I first restricted the sample to municipal bonds in cities with populations from 240,000 to 260,000—the bandwidth where the MLF had a significant main effect on yields, as seen in Table 2—and I ran the regressions separately for sub-samples consisting of only general obligation bonds and only revenue bonds. Table 3 presents the results of this analysis.

Table 3. MLF Effects on Yields Separated by Bond Type

	Δ Yield (basis points)		
Independent Variable	*All bonds (n=1,558)*	*General obligation (n=576)*	*Revenue (n=792)*
Treated	-10.44***	-1.41	-18.49**
	(2.98)	(6.69)	(6.16)

NOTE: Regression coefficients for change in yield are expressed in terms of basis points. For example, a coefficient of -10.44 indicates that municipal bond yields decreased by 10.44 basis points in cities eligible for the MLF, relative to cities not eligible for the MLF. Each column represents a separate regression with different sample restrictions based on bond type. The first column considers all bonds, the second column considers general obligation bonds only, and the third column considers revenue bonds only. I do not perform a separate regression for double barrel bonds because of the small sample size of such bonds (n=190). In all regressions presented here, I only include municipal bonds from cities with populations ranging from 240,000 to 260,000, since this was the bandwidth where significant main effects were previously found (See Table 2). Robust standard errors are in parentheses and are clustered at the city level. Statistical significance was determined using t-tests in STATA.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Although the MLF reduced yields by 10 basis points in cities with populations ranging from 240,000 to 260,000, the results in Table 3 reveal that this effect was not uniform across different bond types. Notably, the MLF significantly reduced yields for revenue bonds, but had no significant effect on yields for general obligation bonds. In light of this finding, I proceeded by conducting a more rigorous test of heterogeneous treatment effects. As introduced in Section 3.2, I performed a regression analysis with an interaction term for treatment status and bond type in order to determine whether the magnitudes of the treatment effects on yields differed significantly for general obligation versus revenue bonds. Table 4 presents the results of this analysis.

Table 4. Heterogeneous Treatment Effects for Yields

Independent Variable	Δ Yield (basis points) n=1,368
Treated	-1.42
	(6.65)
Bond Type	8.20
	(8.80)
Treated*Bond Type	-17.08
	(11.07)

NOTE: All regression coefficients express the dependent measure (change in yield from the pre to post period) in terms of basis points. For example, a coefficient of -1.42 indicates that municipal bond yields decreased by 1.42 basis points in cities eligible for the MLF, relative to cities not eligible for the MLF. In the regression presented here, I only include municipal bonds from cities with populations ranging from 240,000 to 260,000, since this was the bandwidth where significant main effects were previously found (See Table 2). Bond Type is a dummy variable that takes on a value of 0 for general obligation bonds and 1 for revenue bonds. The coefficient on Treated*Bond represents the interaction effect of interest. Robust standard errors are in parentheses and are clustered at the city level. Statistical significance was determined using t-tests in STATA.
* $p < 0.1$ **$p < 0.05$ *** $p < 0.01$

At first glance, the negative coefficient on the Treated*Bond Type interaction variable suggests that, as expected, the MLF's effect on yields was stronger for revenue bonds than for general obligation bonds. However, this interaction effect was not significant (p=0.15). Thus, although I can reasonably conclude from Table 3 that the MLF reduced yields for revenue bonds, but not for general obligation bonds, the results in Table 4 do not allow me to conclude that the *magnitudes* of the MLF's effects were significantly different for revenue bonds versus general obligation bonds. I discuss the importance of these findings at greater length in Section 6.

5. Results: Effects of MLF on Spreads (Transaction Costs)

5.1 Main Effects

In order to estimate the main effect of the MLF on spreads in the secondary municipal market, I perform a difference-in-differences analysis, as introduced in Section 3.3. Table 5 presents the results of this analysis.

Table 5. Main Effects of MLF on Spreads

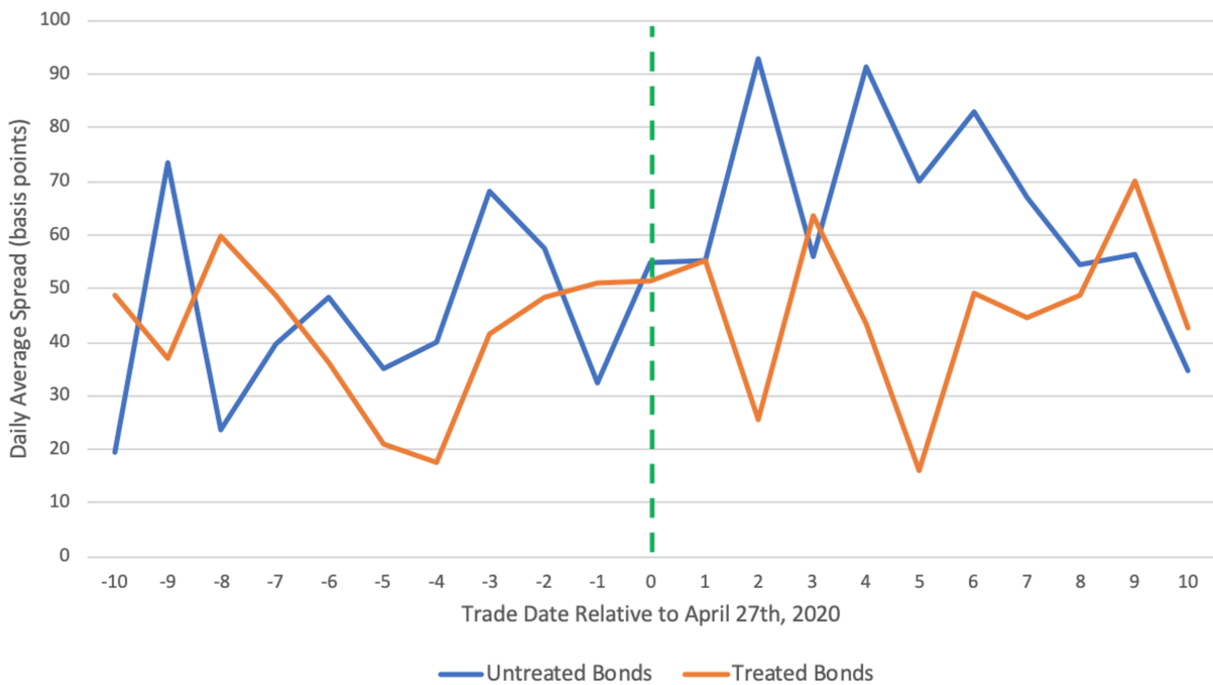
Independent Variable	Spread (basis points) (n=698)
Treated*Time	-18.11** (8.28)
Treated	2.74 (5.73)
Time	19.99* (10.15)
Trade Date	-0.29 (0.50)
Par Value Traded	-0.000015*** (0.000002)
Time to Maturity	0.0087*** (0.00103)
Coupon	-8.10** (2.94)

NOTE: All regression coefficients express the dependent measure (effective spreads) in terms of basis points. In the regression presented here, I include municipal bonds from the full sample of cities with populations ranging from 200,000 to 300,000. The coefficient on Treated*Time represents the diff-in-diff estimator of interest. Robust standard errors are in parentheses and are clustered at the city level. Statistical significance was determined using t-tests in STATA.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

As seen in the table above, the MLF policy announcement reduced spreads by 18 basis points in eligible cities, relative to ineligible cities. In the sample used for this analysis, municipal bonds had an average spread of 44 basis points during the two weeks leading up to the April 27th MLF announcement, indicating that the 18-basis point reduction was sizable. Moreover, the Time variable has a coefficient that is positive and statistically significant, suggesting that the MLF essentially helped eligible cities avoid the surge in transaction costs experienced by ineligible cities during this four-week period.

The graph below illustrates that although spreads were highly volatile during this period, spreads for eligible and ineligible bonds exhibited roughly similar trends during the two weeks leading up to the April 27th announcement.

Figure 3. Trends in Daily Average Spreads for Treated and Untreated Municipal Bonds



NOTE: In the figure above, each data point represents the average daily effective spread (in basis points) for the sample of treated bonds (population > 250,000) or untreated bonds (population < 250,000). The vertical dashed line represents April 27th, 2020, the date when the Fed announced the expansion of MLF eligibility to all cities with populations of 250,000 or more residents. For this graph, I used the full sample of bonds from cities with populations ranging from 200,000 to 300,000 residents.

Moreover, there is a discernible effect of the MLF announcement on spreads for eligible versus ineligible bonds during the following two weeks. After the April 27th announcement, spreads for eligible bonds remained at levels roughly consistent with pre-announcement levels, whereas spreads for ineligible bonds continued to soar for approximately one week.

5.2 Comparing Treatment Effects by Bond Type

Having identified a significant main effect of the MLF on municipal bond spreads, I was subsequently interested in evaluating whether this treatment effect was uniform across bond types (general obligation versus revenue). To that end, I replicated the regression in Table 5 for subsets of the sample consisting of only general obligation bonds and only revenue bonds. Table 6 presents the findings of this analysis.

Table 6. MLF Effects on Spreads Separated by Bond Type

	Spread (basis points)		
Independent Variable	*All bonds (n=698)*	*General obligation (n=271)*	*Revenue bonds (n=392)*
Treated*Time	-18.11**	-2.92	-25.22*
	(8.28)	(8.94)	(13.45)
Treated	2.74	-2.93	4.07
	(5.73)	(8.74)	(8.83)
Time	19.99*	3.69	27.73*
	(10.15)	(12.37)	(14.74)
Trade Date	-0.29	-0.59	0.073
	(0.50)	(0.64)	(0.78)
Par Value Traded	-0.000015***	-0.000017***	-0.000014***
	(0.000002)	(0.0000048)	(0.0000025)
Time to Maturity	0.0087***	0.00697***	0.0084***
	(0.00103)	(0.0023)	(0.0015)
Coupon	-8.10**	-6.04*	-10.03**
	(2.94)	(3.29)	(3.91)

NOTE: All regression coefficients express the dependent measure (effective spreads) in terms of basis points. Each column represents a separate regression with different sample restrictions based on bond type. The first column considers all bonds, the second column considers general obligation bonds only, and the third column considers revenue bonds only. I do not perform a separate regression for double barrel bonds because of the small sample size of such bonds (n=35). In all three regressions presented here, I include municipal bonds from the full sample of cities with populations ranging from 200,000 to 300,000. The coefficient on Treated*Time represents the diff-in-diff estimator of interest. Robust standard errors are in parentheses and are clustered at the city level. Statistical significance was determined using t-tests in STATA.

* $p < 0.1$ **$p < 0.05$ *** $p < 0.01$

As seen in Table 6, the MLF significantly reduced spreads for revenue bonds, but not for general obligation bonds. These findings suggest that the MLF's main effects on spreads may have been driven by revenue bonds. In order to determine whether the magnitudes of the treatment effects differed significantly for general obligation versus revenue bonds, I proceeded by conducting a more rigorous test of heterogeneous treatment effects. Specifically, I performed a regression analysis with a three-way interaction term for time, treatment status, and bond type. Table 7 presents the results of this analysis.

Table 7. Heterogeneous Treatment Effects for Spreads

Independent Variable	Δ Spread (basis points) (n=693)
Time	2.69 (9.99)
Treated	-6.49 (10.64)
Bond Type	6.85 (8.97)
Time*Treated	-5.34 (9.78)
Treated*Bond Type	6.10 (15.43)
Time*Bond Type	28.20 (19.84)
Time*Treated*Bond Type	-21.39 (22.45)
Trade Date	0.011 (0.43)

NOTE: All regression coefficients express the dependent measure (effective spreads) in terms of basis points. In the regression presented here, I only include all municipal bonds from cities with populations ranging from 200,000 to 300,000. Bond Type is a dummy variable that takes on a value of 0 for general obligation bonds and 1 for revenue bonds. The coefficient on Time*Treated*Bond Type represents the estimated interaction effect of interest in this analysis. Robust standard errors are in parentheses and are clustered at the city level. Statistical significance was determined using t-tests in STATA.
* $p < 0.1$ **$p < 0.05$ *** $p < 0.01$

At first glance, the negative coefficient on the Time*Treated*Bond Type interaction variable suggests that, as expected, the MLF's negative effect on spreads was stronger in magnitude for revenue bonds than for general obligation bonds. However, this interaction effect was not significant (p=0.35). Thus, although I can reasonably conclude from Table 6 that the MLF reduced spreads for revenue bonds, but not for general obligation bonds, the results in Table 7 do not allow me to conclude that the *magnitudes* of the MLF's effects were significantly different for revenue bonds versus general obligation bonds. I discuss the importance of these findings at greater length in Section 6.

6. Discussion

The findings presented here estimate the causal effects of the MLF on municipal borrowing costs and secondary transaction costs. For the remainder of this paper, I discuss the efficacy of the MLF with respect to yields and spreads, consider the broader question of whether central banks should lend to local governments in times of crisis, and present ideas for future directions that build upon the limitations of the current research.

With respect to municipal borrowing costs, the MLF appears to have had only a modest effect on yields. In the full sample of cities with populations ranging from 200,000 to 300,000 residents, the MLF had no statistically significant effect on yields. However, when considering the narrowest bandwidth of cities just above and just below the eligibility cutoff—cities with populations ranging from 240,000 to 260,000—I find that municipal yields declined by 10 basis points, representing a 4.6% reduction in municipal borrowing costs for cities in this narrow sample. These results suggest that although the MLF might have reduced municipal borrowing costs, these effects were modest in size. Moreover, given that a significant effect was detected for the narrowest bandwidth of cities but not for the overall sample, it is possible that the magnitude of the MLF's effect is related to each city's distance from the population eligibility cutoff. In future analyses, it might therefore be fruitful to use a regression discontinuity approach to estimate the bond-fixed effects of the MLF on yields, using city population as the running variable.

My estimated effects of the MLF on yields complement the recent analysis conducted by Haughwout et al. (2021). Similar to my analysis, Haughwout and colleagues report that the MLF had a minimal effect on yields. They find that yields declined for a subset of bonds with low credit ratings, but they find no significant effect of the MLF on yields in their overall sample. A key difference between the current research and the work by Haughwout et al. is that I estimate the short-term effects of the MLF over a four-week window, during which there were no other MLF-related announcements from the Fed, whereas Haughwout et al. use a substantially longer sample period that extends to November 2020. Importantly, the Fed announced additional modifications to MLF eligibility in June 2020, which could potentially influence the estimates obtained by Haughwout et al. over this longer sampling period. The current research therefore complements the work by Haughwout et al. (2021) by providing a clean estimate of the short-term effects.

The current research also contributes to the broader literature on large scale asset purchases (LSAPs) and borrowing costs. The existing research in this domain has primarily focused on Treasury bonds and mortgage-backed securities, which were eligible for the Fed's quantitative easing programs after the financial crisis of 2007-2008 (Krishnamurthy and Vissing-Jorgensen, 2011; D'Amico and King, 2013). The dominant finding in this literature is that LSAPs reduce borrowing costs. Here, I extend this line of research to a domain that has not previously been considered: the municipal bond market.

In addition to yields, my analysis also estimates the effects of the MLF on transaction costs. I find that the MLF caused effective spreads to decline by 18 basis points in eligible cities, relative to ineligible cities. In order to properly interpret this finding, it is important to specify the trajectory of spreads in both eligible and ineligible cities during the four-week window. In cities that were not eligible for the MLF, spreads soared by 40 percent from an average of 47 basis points during the two weeks prior to the Fed's announcement to an average of 66 basis points in the two weeks following the Fed's announcement. Meanwhile, in cities that were eligible for the MLF, spreads only rose from 45 basis points to 47 basis points over the same period. Therefore, the MLF did not decrease spreads in absolute terms. Rather, the MLF helped eligible cities avoid the enormous surge in spreads that likely would have occurred in the absence of the MLF. These findings suggest that the Fed provided a remarkably effective liquidity backstop to the municipal market at a time when transaction costs were soaring.

These results advance the broader literature on LSAPs and liquidity. Prior work in this domain has focused on Treasury securities that have been eligible for asset purchase programs (Christensen and Gillan, 2018). To the best of my knowledge, the current paper is the first to examine the impact of LSAPs on liquidity in the municipal bond market. My findings also complement an emerging body of research on the effects of other LSAPs implemented during the early stages of the Covid-19 crisis—particularly the Secondary Market Corporate Credit Facility (SMCCF), whereby the Fed made an unprecedented pledge to directly purchase corporate bonds. Gilchrist et al. (2020) perform difference-in-difference analyses that exploit (SMCCF) eligibility requirements, reporting that bid-ask spreads declined by ten basis points within ten days after the Fed first announced that it would purchase corporate debt. Kargar et al. (2020) employ a similar empirical strategy and report that when the Fed later expanded the scope of the SMCFF, bid-ask spreads fell for both eligible and ineligible corporate bonds. Taken together, when combining these two papers with the current research, the emerging body of work suggests that the Fed's asset purchases provided an effective liquidity backstop in both the corporate and municipal bond markets.

One noteworthy finding is that the MLF's effects on yields appear to be driven by revenue bonds rather than general obligation bonds—a distinction that has not been explored in the existing literature on municipal bonds. In this paper, I find that the MLF had a significant main effect of reducing yields (when considering a narrow bandwidth of cities) and spreads. However, as reported in Tables 3 and 6, these significant effects occurred only for revenue bonds—the MLF had no significant effect on yields or spreads when evaluating general obligation bonds in isolation. Revenue bonds must be repaid with specific revenue sources (e.g., airports, highways, stadiums), many of which were jeopardized by the lockdowns and restrictions implemented at the onset of the pandemic. By contrast, general obligation bonds are fully backed by the taxing authority of municipalities, and therefore may have carried a lower perceived risk of default than revenue bonds during this period. If this is the case, then the Fed's announcement of the MLF may have been more important for reassuring investors about revenue bonds than general obligation bonds, thereby explaining why market conditions subsequently improved for revenue bonds, but not for general obligation bonds. Additional research is needed to confirm this theory, however.

In a broader sense, the present findings begin to address the question as to whether central banks should directly lend to local governments in times of crisis. Prior to the onset of Covid-19, economists had speculated that central bank intervention in municipal debt markets could pose a serious risk of moral hazard. For example, Nobel laureate Thomas Sargent has argued that the prospect of the federal government's assumption of state and local debt may lead states and localities to develop less sustainable budgeting habits, thereby jeopardizing their creditworthiness (Sargent, 2012). Given that the Fed's pledge to directly purchase municipal bonds in the spring of 2020 was a historically unprecedented move, the MLF serves as a natural experiment that can help policymakers more concretely weigh the costs and benefits of central bank intervention. Although my research does not directly investigate the potential downsides of the Fed's intervention, I do provide preliminary evidence of the potential benefits. In particular, my analysis of spreads suggests that the Fed's pledge to purchase municipal securities can restore liquidity by significantly reducing transaction costs. It is worth noting that over the course of the Covid-19 pandemic, the uptake of MLF lending support by state and local governments was extremely low. As of October 2020, only two issuers—the state of Illinois and the New York Metropolitan Transit Authority—had taken advantage of MLF lending (Scaggs, 2020). Thus, it appears that the mere commitment to purchase municipal securities can provide an effective liquidity backstop in times of crisis, even without any significant uptake of the pledged lending support.

Although the present research contributes to policy debates and the academic literature in meaningful ways, it is important to highlight two key limitations of this body of work. In this paper, I primarily perform difference-in-difference analyses rather than regression discontinuities. However, as observed in Table 2, the effects of the MLF appear to be stronger when considering narrower bandwidths of cities around the population eligibility cutoff. In light of this finding, although a difference-in-differences approach provides a useful first approximation of the MLF's effects, a regression discontinuity approach might be better suited for detecting the MLF's treatment effects. Future research should therefore replicate the current body of work with a regression discontinuity approach that uses city population as the running variable. Another key limitation is that in my analysis of spreads, the sample size is quite small. Drawing from the sample of municipal trades in 49 cities from April 13th to May 11th, I identified 698 unique transactions (each consisting of a customer purchase and a customer sale) that served as the basis for my regressions. Due to this small sample size, I was unable to measure bond-fixed effects, as I did for my analysis of yields. In order to remedy this issue, future research should expand the sample of transactions by widening the sample of cities beyond the current restrictions (population of 200,000 to 300,000 residents) and considering a longer temporal window around the April 27th announcement. These limitations provide an initial platform for future research directions.

In addition to directly addressing these limitations, there are several other fruitful ways to build upon the current body of work. In my analysis of yields, I measure the change in yields from the two weeks prior to the April 27th announcement to the two weeks following the announcement. I selected this four-week window because the Fed made no other MLF-related announcements during this period, thereby isolating the effect of the April 27th announcement. However, it is possible that the effects of the MLF on yields were mostly concentrated over a window shorter than the one that I used. Future research should therefore replicate the current analysis with shorter temporal windows around the April 27th analysis. Moreover, in my analysis of liquidity conditions, I focus exclusively on measuring changes in effective spreads. Although the significant reduction in effective spreads suggests that the MLF restored municipal market liquidity, future research should examine trends for other metrics of liquidity (e.g., trade volume) in order to provide a more complete portrait of the MLF's effects on liquidity conditions. Furthermore, in my analysis of heterogeneous treatment effects, I exclusively consider differences between revenue and general obligation bonds. Future research should broaden our understanding of heterogeneity by also examining whether the MLF's effects differ based on credit ratings. In particular, this avenue of research will be useful for testing the robustness of Haughwout and colleagues' finding that the MLF was most effective at reducing yields for bonds with low credit ratings. These future directions, among others, will strengthen our understanding of the MLF and its economic consequences.

7. Conclusion

In this paper, I exploit population cutoffs for MLF eligibility to estimate the causal effects of the MLF on municipal borrowing costs and secondary market transaction costs. I find that although the MLF only had a modest impact on municipal borrowing costs, the policy announcement significantly reduced transaction costs, suggesting that the MLF served as an effective liquidity backstop during the early stages of the Covid-19 crisis. These findings contribute to the emerging literature on Fed's multifaceted policy response to Covid-19 by indicating that the Fed's intervention successfully restored some degree of liquidity in not only the corporate bond market, but also the municipal bond market. Furthermore, given the unprecedented nature of the MLF, this paper is one of the first attempts to examine the causal effects of large-scale asset purchases in municipal bond markets, thereby addressing the broader question of whether central banks ought to lend to local governments in times of crisis. Although this paper makes important strides toward answering questions of interest to academics and policymakers, there is still much to be learned about the MLF. In particular, future research should examine the effects of the MLF on other measures of liquidity in order to develop a more thorough assessment of the MLF's efficacy. Until then, the present research suggests that the Fed successfully provided a liquidity backstop for municipalities at a time of immense uncertainty and volatility.

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