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The Impact of Intervention: Insights from Fiscal and Monetary Policy

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by

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VITA

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ABSTRACT OF THE DISSERTATION

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Doctor of Philosophy in Economics

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Professor Guillaume Rocheteau, Chair

One key role as an economist is to analyze the impact of fiscal and monetary policy. In the following essays, I look at the impact of both types of policy through a theoretical and empirical lens. The first two essay looks at the role of the Federal Reserve as a lender of last resort. Theoretically, I find that the discount window can expand welfare by granting agents access to external liquidity, as well as serve as an outside option for borrowing banks in the interbank market. I then look at COVID, a time when the discount window was heavily, and find that usage of the liquidity facility expands bank lending to consumers and firms in the presence of an unexpected aggregate liquidity shock. Finally, my last essay explores the impact of fiscal policy intervention on counties affected by natural disasters. I find that disaster aid improves local recovery without negative impact to the financial sector. All three chapters shows that the benefits of government intervention outweighs the cost, especially in the event of unexpected shocks.

Chapter 1

The Role of Public Lending as an Outside Option in Private Markets

Abstract

This paper constructs a New Monetarist model with public and private lending to analyze whether the discount window operated by the Federal Reserve serves a role aside from its primary purpose of last resort lending and interest rate control. The model shows two main results. First, the discount window reduces the market power of lenders in the interbank market by serving as an outside option for borrowers during the bargaining. Second, if the discount window rate is sufficiently low, public lending can improve welfare by increasing aggregate consumption through the provision of external liquidity, which helps agents avoid the inflation tax.

1.1 Introduction

Banks hold reserves to meet reserve requirements, fund investment opportunities, and act as a means of payment to settle transactions.[1] They can acquire these reserves by holding deposits, borrowing from other banks through the Federal Funds (interbank) market, or borrowing from the central bank. To ensure that banks have sufficient liquidity, the discount window (DW) was opened in 1913 to serve as a lender of last resort and to place a ceiling on interest rates. While its usefulness during crises is unquestionable, issues with the operation of the DW during normal (absent of aggregate shocks) times have been extensively documented.[2] Furthermore, recent policy changes have made the primary roles of the DW obsolete.[3] If the two main purposes of the DW during normal times are obsolete, does the discount window still serve a role, or should the Federal Reserve entertain the possibility of discretionary operation of the public lending facility?

This paper uses a New Monetarist framework surveyed in Lagos, Rocheteau, and Wright [2017] and Rocheteau and Nosal [2017] to explore a mechanism of the DW that is overlooked in the literature. In this model, agents (which we view as banks) face idiosyncratic consumption shocks that cannot be paid for by credit due to anonymity and lack of commitment, therefore requiring the use of an immediate medium of exchange. We introduce a

[1] For most of recent history, reserve requirements in the US have been 10%. Reserve's role as a means of payment is demonstrated through the daily transaction of around USD 3.3 trillion dollars and involves around 10,000 banks made through the Federal Reserve Wire Network (Fedwire). For a model that has these features, see Bianchi and Bigio [2017].

[2] Schwartz [1992] shows that banks that do borrow from the window in the 1980s were mainly insolvent and used 'almost daily' to delay bankruptcy, putting the burden of repayment on taxpayers instead of the responsible institution. Ennis and Price [2015] examines a case study where BoNY had to borrow \$22.6 billion dollars from the window due to software failure, equivalent to paying a fine of $\tilde{5}$ million dollars, and asks whether intervention was justified given that it might lead to inadequate safeguards taken by banks against failures. Ennis and Klee [2021] finds that banks with access to the discount window hold lower reserves along with riskier asset portfolios than their counterparts after controlling for size and other salient characteristics, implying that the DW promotes moral hazard.

[3] In April of 2021, the Federal Reserve reduced its reserve requirements for banks from 10% to 0%, subsequently eliminating one of the banks' primary reason for holding reserves. Additionally, Figure A.1 in the Appendix shows that the interest rate difference between the discount rate and the interbank rate from 2003 to 2020 is positive, therefore the DW rate is non-binding as an interest rate control mechanism.

monitored lending market for private lending (similar to the interbank market) and a public lending facility (DW) operated by the government to be used as a last resort before random consumption opportunities. Agents in the model have bargaining power when conducting private trades, and the inclusion of the public lending facility will affect equilibrium allocations between agents even when the public rate is non-binding. We show that there exist three distinct monetary regimes in the equilibrium. When the discount rate is low, the cost of borrowing from the DW is lower than the cost of holding money, and the DW increases aggregate consumption through the provision of credit. When the discount rate is high, public borrowing is more costly than holding money, agents hold no public debt, and the DW only affects the distribution of surpluses between borrowers and lenders but not aggregate consumption. This result implies that changes in the discount rate could be ineffective if raised past a critical level, since the change would have no effect on aggregate consumption. Under the third, the cost of holding money and borrowing from the DW is equal, and agents are indifferent between using private and public money to pay for the consumption good.

The welfare impact of the DW differs across banks depending on their asset portfolio. When banks have access to high return investments, the optimal behavior is to borrow money from the DW rather than using reserves to settle transactions. On the other hand, the asset portfolio of banks only contain low-return securities, their cost of holding reserves is low, and they use held reserves to settle transactions instead of borrowing from the DW. This theoretical result confirms empirical findings by Drechsler et al. [2016] and Ennis and Klee [2021] that banks with access to the DW hold lower reserves along with riskier asset portfolios (higher real returns) relative to their counterparts after controlling for size and other salient characteristics. As for the question of interest rate control, access to the discount window reduces the capturable surplus of lenders during private negotiations by affecting the outside option of borrowers.[4] Therefore, even if the interbank rate is at a level where the constraint

[4] Choi and Rocheteau [2021] shows this more extensively under a continuous time New Monetarist model where bargaining power is taken endogenously through the arrival rate of matches for agents through the outside option channel. They find that as meeting speed becomes infinite, there exists a sequence of equilibria

is non-binding, a change in the DW rate still affects the equilibrium private rate through the outside option channel.[5]

Literature Review

The model builds on Berentsen, Camera, and Waller [2007] and Section 8.5 of Rocheteau and Nosal [2017]. In the former, liquidity is reallocated through competitive banks after the consumption shock, and the welfare impact arises from payments on interest for lenders. This model removes the banks and matches lenders and borrowers directly. By removing competitive banks, we can include bargaining power between the lender and borrower to see whether changes to bargaining power distorts equilibrium trade quantities. The inclusion of bargaining power in the model environment also allows us to study how the DW channel of monetary policy affects the private lending rate. In the latter, Nosal and Rocheteau shows that the existence of a lending market is welfare improving by allowing agents with preference shocks access to credit, relaxing the liquidity constraint and increasing trade quantities. This model builds off of their work by including the central bank as a lender of last resort during the lending stage.

We view agents as banks and interpret the market where they interact as the interbank market. In Berentsen and Monnet [2008], agents receive a noisy signal of their consumption shock, and lending is conducted based on the signal's reliability. When the signal is perfectly accurate, they find that agents can fully adjust their portfolio during the lending stage and do not need access to central bank lending. We find that banks may still find it optimal

along which sellers' market power vanishes.

[5] Historical evidence supports the argument that the central bank has a role in limiting the market power of surplus (lender) banks during both normal and unstable times. In 1907, before the formation of the Federal Reserve, collapse of copper stocks in the US caused depositors to run on Mercantile National Bank, while JP Morgan was unaffected due to its good reputation. To help bail out needy banks, JPM leveraged its market power to stage a takeover of MNB assets at a discount. This crisis paved the way for the establishment of a central bank that was welfare maximizing instead of profit maximizing, along with a discount window that would prevent similar crises and promote competition. Donaldson [1992] also show that private lending rates prior to the establishment of the Federal Reserve are substantially higher than after the formation, which can be seen as evidence for market power of surplus banks. See Acharya et al. [2012] Appendix B for more instances.

to access the public lending facility even with perfect signalling if the rates that agents can borrow from the CB are lower private rates. The result for central bank lending resembles Acharya et al. [2012] in the sense that government lending can reduce market power of surplus banks in the interbank market, but their finite horizon environment lacks the tools to examine general equilibrium effects and the interplay between government lending and money growth.

Recent works that examines the effects of monetary policy implementation on interbank lending are Rocheteau, Wright, and Zhang [2018] and Bianchi and Bigio [2017]. Rocheteau, Wright, and Zhang [2018] uses the New Monetarist framework that includes reserve and capital requirements to examine the pass-through from monetary policy to entrepreneurs through the interbank market. They find that reserve requirements increase the potency of monetary policy and that an Open Market Purchase by the Central Bank reduces cost of borrowing reserves and incentivize banks to extend loans. Bianchi and Bigio [2017] develops a model with the interbank market from Afonso and Lagos [2015] to examine how monetary policy affects banks by altering the trade-off between profiting from lending and incurring greater liquidity risk. We present a simplified version of the interbank market that still captures the most salient effect of standing facilities; the fact that it can relax liquidity constraints and influence private agreements.

The discount window lending literature is surveyed in Ennis [2017]. In this model, the welfare improvement mechanism of the DW comes from its ability to provide liquidity at a lower interest rate than money.

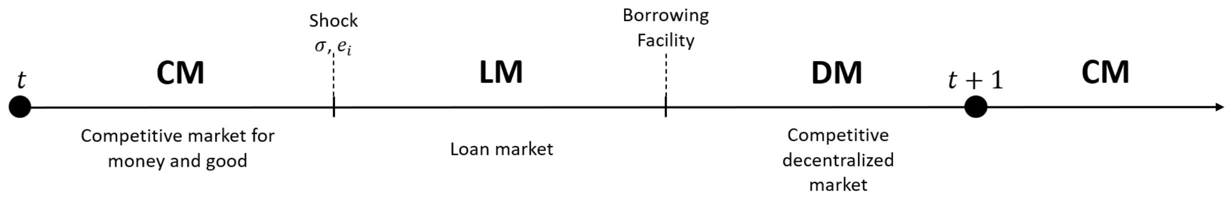


Figure 1.1: Events in period t

1.2 The Environment

Time is discrete indexed by $t \in N$. The economy is populated by a unit measure of agents. Each period is divided into 3 Stages (markets). The first stage is a frictionless centralized market (CM) and the third stage is a frictional decentralized market (DM). Between these two stages, we introduce an OTC lending market (LM) where agents can readjust their portfolio.[6] Figure 1.1 shows a graphical representation of the events in period t. There are two perishable goods: a good c produced in the CM and taken as the numéraire, and a good q produced in the DM.

The agent's lifetime utility is:

$$U = \sum_{t=0}^{\infty} \beta^t [c_t - h_t + u(q_t^b) - q_t^s] \tag{1.1}$$

where β is the discount factor. All agents have linear utility over c. At the beginning of Stage 2, agents get a preference shock such that they can consume or produce in Stage 3 with probability σ; we refer to these consumers as buyers and producers as sellers. A consumption of q gives the buyer utility $u(q^b)$, and a production of q incurs a cost q^s for the seller. Utility $u(q)$ is strictly concave, where $u'(q) > 0$, $u''(q) < 0$, $u'(0) = \infty$, and $u'(\infty) = 0$. This paper defines q^* as the level of the specialized good that satisfies $u'(q^*) = 1$.

[6]The OTC structure of the Federal Funds market that we adopt has been empirically highlighted in Ashcraft and Duffie [2007], where they find that in the aggregate, approximately 73% of all loans made through the federal funds market were traded bilaterally.

In Stage 3 trades, agents are anonymous so that trading partners cannot identify the counterparty. Trading histories are private information, therefore credit arrangements are not incentive feasible. There exists a central bank that controls the supply of money. The money stock evolves by $M_{t+1} = \gamma M_t$, where M_t denotes the money stock at time t, and $\gamma > 0$ denotes the gross growth rate of money. The central bank also operates a standing facility where agents can borrow money before Stage 3 and after Stage 2. An agent who borrows b units of money from the central bank at time t repays $(1 + i^b)b$ units of money in Stage 1 of the following period. In this baseline, we assume that there is a costless enforcement technology operated by the central bank that rules out default. The budget constraint of the government is therefore: $T_t - i^b B_t = (\gamma - 1)M_t$, where T_t is lump-sum transfers given to agents in Stage 1 at period t, and $i_t^b B_t$ is the aggregate interest payment that agents make to the central bank from borrowing.

We model credit as personal liabilities issued by borrowers to lenders that can be redeemed in the subsequent CM. While the implications to allocations are similar for selling an asset (such as a bond), For this process to function, there exists a costless technology only available in Stage 2 that allows record keeping of financial histories in Stage 2.

In the interbank market, random matches are formed bilaterally following a Leontief matching function; more specifically, if a mass b of borrowers and s lenders are searching, then $m(b, s) = \min\{b, s\}$. In these pairwise meetings, loan size and repayment amount (l, x) are determined according to the proportional solution of Kalai [1977], where the share of surplus received by lenders is $\theta \in [0, 1]$.[7] We define $i = x/l$ as the nominal interest on the loan.

[7]The Nash [1950] solution has been shown to be problematic when liquidity constraints are binding, see Hu and Rocheteau [2020]. In summary, the Nash solution replicates the case where the output is negotiated all at once in an Rubinstein alternating-offer game, while the proportional solution replicates the case where there is an infinite number of negotiations over infinitesimally small bundles. They furthermore show that the liquidity constraint for buyers bind for any $N < +\infty$.

1.3 Equilibrium

Let ϕ_t be the price of money in the CM in period t. This section characterizes a steady state stationary equilibra where aggregate real balances and allocations are constant; i.e., $\phi_t M_t = \phi_{t+1} M_{t+1}$. Under the assumption that γ is constant, $\phi_t/\phi_{t+1} = M_{t+1}/M_t = \gamma$.

Let $W(\omega)$ denote the expected value from entering Stage 1 with total wealth $\omega \equiv \omega(m, l, b)$ expressed in the numeraire. Let $X^b(m)$ denote the value of entering Stage 2 as a buyer holding m units of money, $X^l(m)$ when entering Stage 2 as a lender, and $V(m, l, b)$ the expected value of entering Stage 3 with m units of money, l units of privately borrowed money, and b units of publicly borrowed money. We examine the individual decision problems at each sub-period in t, then solve the equilibria.

1.3.1 Stage 1 - Central Market

Consider an agent who holds ω units of wealth. In Stage 1, their value function is:

$$W(\omega) = \max_{c,h,m'} \quad \left\{-h + c + \sigma[X^b(m') + X^l(m')] + (1 - 2\sigma)\beta W_{t+1}(m')\right\} \tag{1.2}$$

$$\text{s.t.} \quad h + \omega + T = \phi m' + c \tag{1.3}$$

where m' is the choice of money holdings brought forth into Stage 2. According to (1.3), agents must finance their consumption, c, and money holdings, m', with their current wealth, ω, production income, h, and lump-sum transfers from the government (expressed in CM good) T. There are equal measures of borrowers and lenders due to the Leontief matching function, and the remaining unmatched agents moved onto the next period's CM. Agents hit with the consumption shock always want to borrow, therefore the probability that agents become a borrower in the LM is σ.

Rewriting the budget constraint and substituting (1.3) into (1.2) yields:

$$W(\omega) = \omega + T$$
$$\max_{m'} \quad \left\{-\phi m' + \sigma X^b(m') + \sigma X^l(m') + (1-2\sigma)\beta W_{t+1}(m')\right\} \tag{1.4}$$

Due to the linearity of W_{t+1} with respect to m, $W_{t+1}(m') = \phi_{t+1}m' + W_{t+1}(0)$. The first-order condition is given by:

$$\phi_t = \sigma[X^{b\prime}(m') + X^{l\prime}(m')] + (1-2\sigma)\beta\phi_{t+1} \tag{1.5}$$

The left side of (1.5) is the marginal cost of holding an extra unit of money, and the right side is the expected marginal benefit from acquiring one extra unit of money. Note that the optimal choice of m' is independent of past history and independent of ω.

1.3.2 Stage 3 - Decentralized Market

In Stage 3, the terms of trade are determined in a bilateral match between a buyer with m units of money, l units of private loans, and b units of public loans. In these meetings, buyers make a take-it-or-leave-it (TIOLI) offer to the seller, which determines the quantity and payment (q, d). Because of linearity in wealth, the wealth of the seller is inconsequential to the trade because their marginal utility from consumption of the CM good is constant and independent of their wealth. The buyer and seller's individual value functions entering Stage 3 are given by:

$$V^b(m, l^b, b^b) = u[q(m, l^b, b^b)] + \beta W_{t+1}(m - d, l^b, b^b) \tag{1.6}$$

$$V^s(m, l^s, b^s) = -q + \beta W_{t+1}(m + d, l^s, b^s) \tag{1.7}$$

The buyer's wealth consists of their money holdings from the CM, as well as any private loan, l, and public debt, b, incurred in Stage 2. The buyer gains $u(q)$ from the consumption of the DM good and the seller incurs a cost $-q$ from production. Under TIOLI, the buyer makes an offer (q, d) that maximizes their consumption utility subject to the seller's participation constraint. The buyer ends the period with zero net asset holdings since all assets are traded for q, but the continuation value still contains the argument for personal liabilities since repayment is due in the subsequent period. Taking into account that one unit of money can be redeemed for ϕ_{t+1} units of CM good in the subsequent period, $\partial\omega/\partial m = \phi_{t+1}$, therefore $W_{t+1}(\omega) = \omega + W_{t+1}(0)$. Using the linearity of W_{t+1}, the offer solves:

$$\underset{q,d}{max} \quad \Big[u(q) - \beta\phi_{t+1}d\Big] \text{ s.t. } q \leq \beta\phi_{t+1}d \tag{1.8}$$

$$d \leq m + l + b \tag{1.9}$$

Where (1.8) is the offer subject to the seller's participation constraint and (1.9) is a feasibility constraint that says buyers cannot offer to transact more than their total money holdings; either held from the CM or borrowed from the LM. From (1.9), we can note that sellers are indifferent between the payment instruments, since their prices are equal in the following CM. Taking into account that the feasibility constraint of the seller holds at equality from the bargaining formulation, the solution to (1.8)-(1.9) is:

$$q = \begin{cases} q^* & \text{if} \quad \beta\phi_{t+1}d \geq q^* \\ \beta\phi_{t+1}d & \text{if} \quad \beta\phi_{t+1}d < q^* \end{cases} \tag{1.10}$$

$$d = \frac{q}{\beta\phi_{t+1}} \tag{1.11}$$

The buyer obtains the socially efficient level of trade if they bring enough money to compensate the seller for a production of q^*, otherwise the buyer is liquidity constrained. Given the bargaining solution, the seller receives no surplus in the DM, and the buyer receives a surplus of $\psi(\omega) \equiv u[q(\omega)] - q(\omega)$. Since this is the case, the value of being a seller in the DM is the same as non-participation; i.e, $V_t^s = \beta W_{t+1}$. Note that m, b, and l are interchangeable, since b and l are personal liabilities taken to obtain the medium of exchange, and the only factor that differentiates them is the repayment cost.

1.3.3 Stage 2 - Lending Market

Public Borrowing Decision: Consider an agent that has already conducted private trades. Subsequently, agents have an option to borrow b directly from the public lending facility at a posted interest rate i^b. Since non-buyers have no incentive to borrow, their public borrowing will be zero, and only buyers will need to borrow from the lending facility. We suppress arguments for ω when it is equal to zero. Taking Stage 2 loan size as given, the value function for a borrower in this scenario is given by:

$$\hat{X}^b(m,l) = \max_{b \geq 0} \quad \psi(m,l,b) + \beta W_{t+1}(m-d,l,b) \tag{1.12}$$

The restriction on b comes from the fact that agents can only borrow form the lending facility and cannot make deposits. Using the linearity W_{t+1} and removing terms orthogonal to b, the choice b maximizes:

$$b^* = \underset{b \geq 0}{\operatorname{argmax}} \quad \psi(m,l,b) - \beta\phi_{t+1}[b-(1+i^b)b] \tag{1.13}$$

Therefore, taking the derivative of (1.13) with respect to b and (1.11) with respect to q, the optimal value of b for the agent solves:

$$b = \begin{cases} u'[q(\omega)] - 1 = i^b & \text{if} \quad u'[q(\omega)] - 1 \geq i^b \\ 0 & \text{if} \quad u'[q(\omega)] - 1 < i^b \end{cases} \tag{1.14}$$

The borrower borrows from the public facility until the point where their marginal benefit equals their marginal cost. The marginal benefit is given by the liquidity premium of Stage 3 transactions, defined as $u'(q) - 1$, and the marginal cost is the repayment interest. While public borrowing relaxes the liquidity constraint in Stage 3 trades by increasing d, it also reduces total wealth ω due to the repayment of interest; therefore, $\partial\omega/\partial b = -\phi_{t+1}i^b$. If private lending can fully satisfy the optimal trade quantity, then it is not necessary for agents to borrow from the discount window and $b = 0$; otherwise buyers still have excess liquidity demand that is not satisfied by private lending and $b > 0$.

Private Borrowing Decision: After the realization of the consumption shock in the beginning of Stage 2, agents can bilaterally negotiate over a loan contract (l, x) depending on liquidity need. Since only buyers have a need for liquidity in Stage 3, they are the only ones who will borrow a positive amount in equilibrium. The value functions of buyers and lenders is given by:

$$X^b(m) = \max_{l,x} \quad \psi(m,l) + \beta \hat{X}^b(m,l) \tag{1.15}$$

$$X^l(m) = \max_{l,x} \quad \beta W_{t+1}(m,-l) \tag{1.16}$$

Under the Kalai [1977] bargaining solution, the disagreement point for the borrower is their best outside option, in this case, taking out a public loan of size $b^r = \min\{l, b^*\}$, where b^* fulfills (1.14). For the lender, the disagreement point is the cost of holding money into the next period's CM. Therefore, the loan contract (l, x) solves:

$$\max_{l,x} \quad S^b \equiv \Big[\psi(m,l) + \beta W_{t+1}(m,l)\Big] - \Big[\psi(m,b^r) + \beta W_{t+1}(m,b^r)\Big] \tag{1.17}$$

$$\text{s.t.} \quad \theta S^b = (1-\theta)\beta\Big[W_{t+1}(m-l,-l) - W_{t+1}(m)\Big] \tag{1.18}$$

$$l \leq m \tag{1.19}$$

The bargaining solution maximizes the excess surplus of the borrower from taking out a private loan net of the disagreement point, taking into account that the share of the excess surplus θ must go to the lender through transfer x. It is straightforward to show that for $x > 0$, the seller finds it optimal to lend all their money. From the the lending contract, borrowing l units of money privately relaxes the liquidity constraint in Stage 3 trade, but decreases total wealth of the borrower due to repayment x. Constraint (1.18) represent the share of the excess surplus that goes to each agent and constraint (1.19) is the maximal lending constraint for the lenders. Define $\Delta\psi \equiv \psi(m,l) - \psi(m,b^r)$ as the difference in surplus between borrowing privately and publicly. Using the linearity of W_{t+1}, (1.17) and

(1.18) can be reduced to:

$$\max_{l,x} \quad \Delta\psi + \beta\phi_{t+1}[(1+i^b)b^r - (l+x)] \tag{1.20}$$

$$\text{s.t.} \quad \theta\Big[\Delta\psi + \beta\phi_{t+1}[(1+i^b)b^r - (l+x)]\Big] = (1-\theta)\beta\phi_{t+1}x \tag{1.21}$$

If constraint (1.19) is slack, then the loan size is less than the total money holdings that agents bring into the Stage 2 market, and agents can face a smaller loss in utility due to the inflation tax by bringing a marginal unit less. Therefore, in the equilibrium, (1.19) holds at equality since $l < m$ is not individually rational. The solution to the bargaining game is:

$$x = \frac{\theta\Big[\Delta\psi + \beta\phi_{t+1}[(1+i^b)b^r - l]\Big]}{\beta\phi_{t+1}} \tag{1.22}$$

$$l = m \tag{1.23}$$

Figure 1.2 shows the corridor created by outside option and liquidity constraint. The blue area represents the share of *capturable* surplus captured by lenders, and the green area is the share of *capturable* surplus to borrowers. The public interest rate i^b determines the outside option of borrowers, and increasing i^b shifts the vertical line separating the outside option to the left since it lowers the outside option of the borrower. The vertical line representing the liquidity constraint is captured by the money growth rate γ, and an increase in γ shifts the liquidity constraint leftwards. Increasing the bargaining power of the lender θ increases the amount of *capturable* surplus they are entitled to.

The size of transfers that borrowers give to lenders depend on the lender's bargaining power, and the **total** surplus that can be gained in the Stage 3 trade net of what is guaranteed to the borrower by the outside option. Define $i^l = x/l$ as the interest of the loan paid to lenders (ie: the interbank rate); the following comparative statics table shows how the optimal interbank rate responds to a change in the exogenous variables:

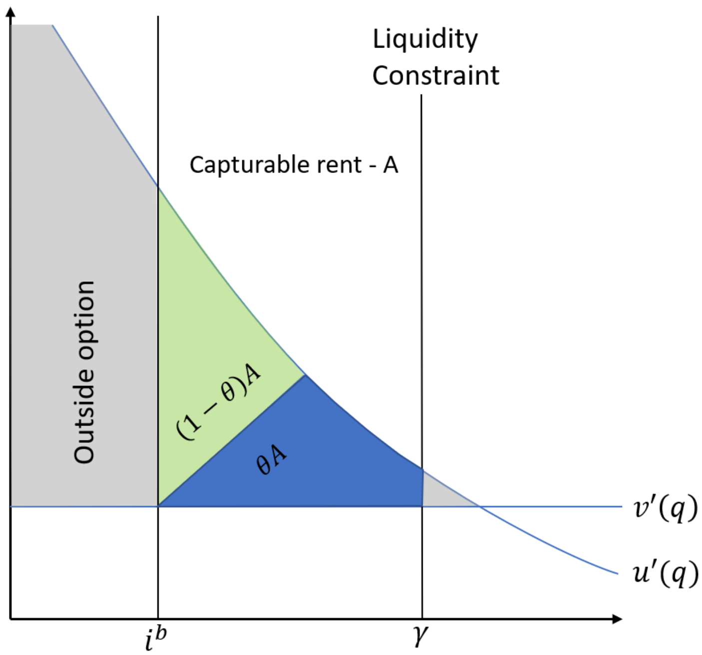


Figure 1.2: θ controls the size of the rent that can be captured by the lender.

	$\partial\theta$	∂i^b	$\partial\gamma$	$\partial\beta$
∂i^l	+	+	+	-

The rate that lenders can charge borrowers increase with their bargaining power. As the central bank makes it more difficult to acquire liquidity through public means by increasing i^b, lenders have more leeway to capture any trade surplus encountered by borrowers in Stage 3 since it lowers the outside option of the borrower. When money growth is high, the cost of holding money increases, the liquidity premium increases due to the lower money holdings, and lending is more valuable. As agents become more patient, the cost of holding money decreases, the liquidity premium decreases since q is closer to q^*, and lending becomes less valued. Given the solution to the bargaining game, we can rewrite 1.15 and 1.16 as:

$$X^b(m) = \psi(m, l, b) - \beta\phi_{t+1}(x + i^b b) + \beta W_{t+1}(m) \tag{1.24}$$

$$X^l(m) = \beta\phi_{t+1}x + \beta W_{t+1}(m) \tag{1.25}$$

The borrower gains the trade surplus in the Stage 3 using their whole portfolio net of the future period repayment. The lender, holding m units of money can lend it out for a repayment size of x. The transfer amount, x shows up in both the value function of the borrower and lender. Since an agent ex-ante has equal probabilities to be borrower or lender, in expectation, the transfer size cancels out and money holdings is independent of transfers, implying that money holdings is also independent of bargaining power. This result should be robust to specifications where matches are formed bilaterally.

1.3.4 Equilibrium Types

Substituting the FOC of 1.24 and 1.25 into (1.5) gives the following money demand:

$$\frac{\gamma - \beta}{2\sigma\beta} = u'[q(m, l, b)] - 1 \tag{1.26}$$

Definition 1.1. *An equilibrium is a tuple* $\{m, x, l, b, q\}$ *that satisfies money demand* (1.26)*, transfers* (1.22)*, private loan demand* (1.23)*, public loan demand* (1.14)*, and DM trade quantity* (1.10)*.*

The left side of (1.26) represents the expected cost of holding one extra unit of money, and the right side represents the liquidity premium of the extra unit. The intuition follows that of Berentsen et al. [2007], in which lending reduces the holding cost of money by allowing more agents to extract surplus from the DM trade through monetary transfers x. From (1.14) and (1.26), either the cost of borrowing from the public facility is lower than the cost of holding money, in which case $b > 0$ and $m = l = 0$; or the cost of holding money is lower, in which case $b = 0$ and $l = m > 0$. Given the parameter space, the asset portfolio of agents (m, l, b) depends on the values of i^b and γ:

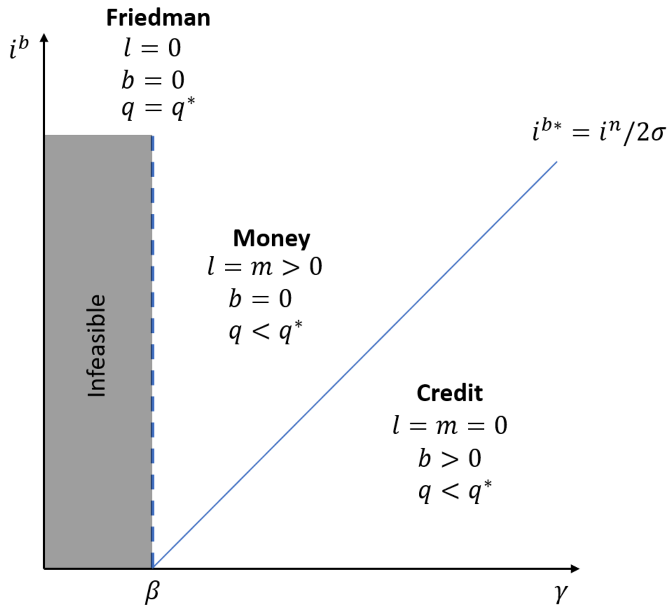


Figure 1.3: The possible equilibria on (γ, i^b) plane.

Equilibrium Type	Condition	Holdings
Friedman Rule	$\gamma = \beta$	$m = m(q^*)$, $l = b = 0$
Credit	$i^b < i^{b*} \equiv \frac{\gamma-\beta}{2\sigma\beta}$	$l = m = 0$, $b > 0$
Money	$i^b \geq i^{b*}$	$l = m > 0$, $b = 0$

For the intuition of the critical value, take the real rate of return to be equal to the rate of time preference, and use the definition of the Fisher equation $1 + i^n = \gamma(1 + r)$. Substituting this into the left side of (1.26) gives $1 + \frac{i^n}{2\sigma}$. A graphical representation of the set of equilibria is shown in Figure 1.3.

The space where $\gamma < \beta$ is infeasible since agents would have positive returns on money and choose to hold an infinite amount. Intuitively, all agents weigh the cost of holding money against what they can gain from trade. Since the price of borrowing and holding money are both fixed cost, agents will choose to only hold the asset that as the highest rate of return.[8] When money growth γ is high, holding fiat money is less valuable, and agents will find it

[8] See Williamson and Wright [2010] for a survey on the New Monetarist literature with two competing assets.

optimal to borrow publicly from the window. In this type of equilibria, agents only work in the CM to repay borrowed funds from the lending facility and hold no cash in their portfolio when exiting the CM. This implies that private lending is shut down and the Stage 2 market becomes obsolete.

When inflation is low, holding money is less costly than borrowing, and the lending facility becomes unused. If meeting probabilities σ are high, the marginal value of holding money increases, and agents do not need to borrow publicly. In terms of nominal interest rates, the nominal interest on money represents how costly it is to hold money instead of an alternative asset. Since the consumption opportunity of agents are determined by their meeting probability, agents are only willing to hold money if the equilibrium interest rate on money is lower than credit, with the risk wedge (defined as $\frac{i^n}{i^b}$) equal to 2σ. When borrowing from the discount window agents are willing to accept a higher premium since they are facing the consumption shock with certainty.

Under the money equilibrium, the Stage 2 lending contract is nonzero. There are two cases for the determination of the transfer depending on the reservation value b^r. From (1.14), if $y = m$ satisfy the second case, then $b^r = 0$ and the outside option is just money holdings from Stage 1. Because the outside option is independent of interest rates, a change in i^b has no effect on transfers x. Figure 1.4 shows this phenomenon; in reality, the required borrowing rate is too high to make this scenario feasible, although it does give insight on what would happen if the discount window was abolished.

There exists a critical level $\tilde{i^b} > i^{b*}$ such that if $i^b \in (i^{b*}, \tilde{i^b}]$, then the transfer amount $x = x(i^b)$. If $i^b > \tilde{i^b}$, then private lending is independent of the public lending rate.

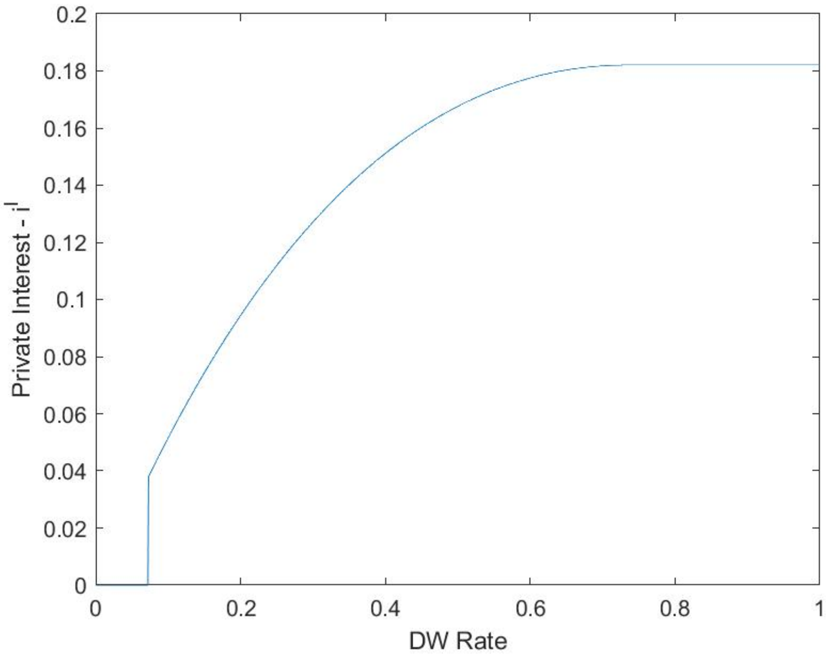


Figure 1.4: Private lending rate as a function of public rate.

1.4 Application

In this section, we compare the effect of monetary policy through the discount window channel under low and high inflation regimes. As a numerical example, we look at the period from July 2010-December 2017 to match the findings of Ennis and Klee [2021]. We assume that $c(q) = q$, and $u(q) = q^{\alpha}$. All calibrated values can be seen from Table 1.1. We let the discount factor $\beta = 1/(1+r)$, where r is the avereage real interest rate on the 1-year T-bill over the period. The matching probability, σ, is calibrated to the share of large domestic banks that have borrowed from the discount window at least five times within the period.

4.1 ***Effect of Monetary Policy***

Figures 1.5 and 1.6 show the response of key variables when the DW rate varies from 0% to 20% under low and high inflation following the calibrated parameters. In the first figure, the

	Parameter	Value	Target
Coefficient on q	α	.6	Fixed
Discount factor	β	.99	1Y T-Bill
Matching probability	σ	.21	Ennis and Klee [2021]
Lender market power	θ	.52	EFFR
Inflation	γ	1.02	Fed target inflation
Discount window rate	i^b		Variable

Table 1.1: Parameter Values

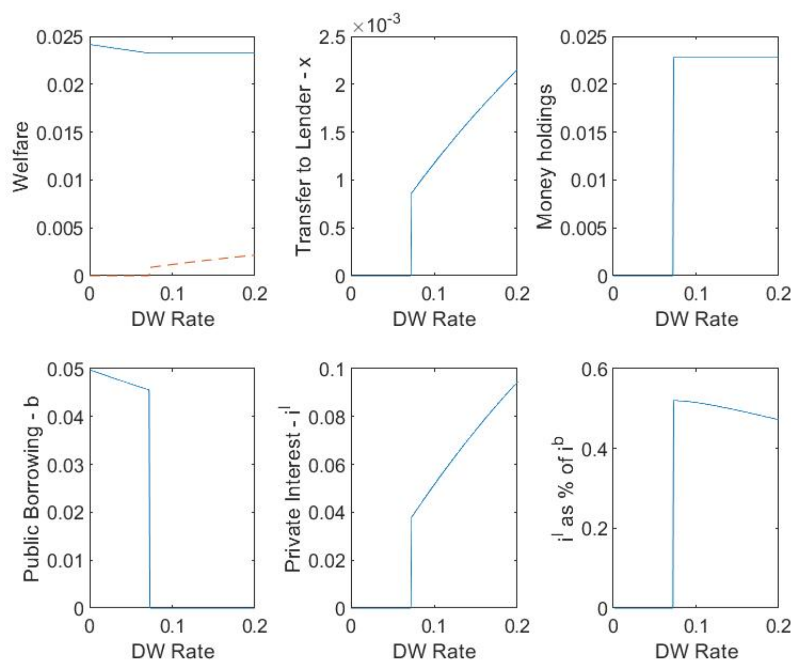


Figure 1.5: Low inflation - $\gamma = 1.02$

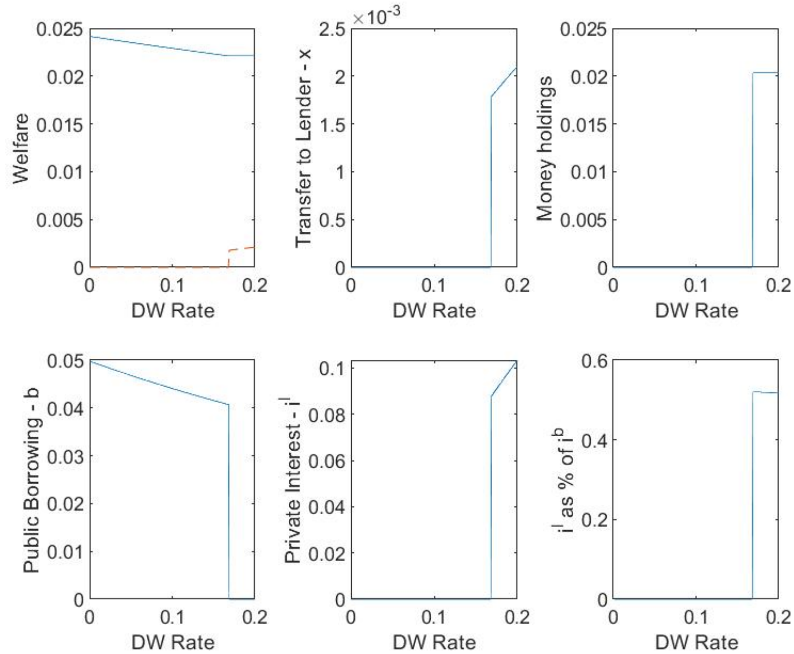


Figure 1.6: High inflation - $\gamma = 1.06$

welfare, measured by $W = \sigma(u(q) - q)$ is plotted in blue, along with the share of the surplus that goes to the lender x in orange. We can see that there is a jump at the critical value i^{b*}, which corresponds to the dual asset equilibrium, and the correspondence of money holdings at the critical value can be seen on the third sub-figure. Values to the left of i^{b*} represents the credit equilibrium and values to the right represents the money equilibrium.

In the credit economy, as the central bank rate increases, the optimal borrowing decreases to match the liquidity premium, with a critical value of $i^{b*} = 7.2\%$ for the low inflation regime, and $i^{b*} = 16.8\%$ for the high inflation regime. Under the low inflation regime, welfare is downward sloping up until the critical value, implying that the discount window is welfare improving (since removal of the DW would put us in the money equilibrium, lowering W), then lies flat when agents do not resort to public borrowing. As we move into the monetary equilibrium, the discount rate only plays a role of outside options for the private bargaining, increasing the surplus of lenders as the value of borrowing decreases. Removal of the outside option would be similar to taking the limit as $i^b \to \infty$, and the transfer x would approach

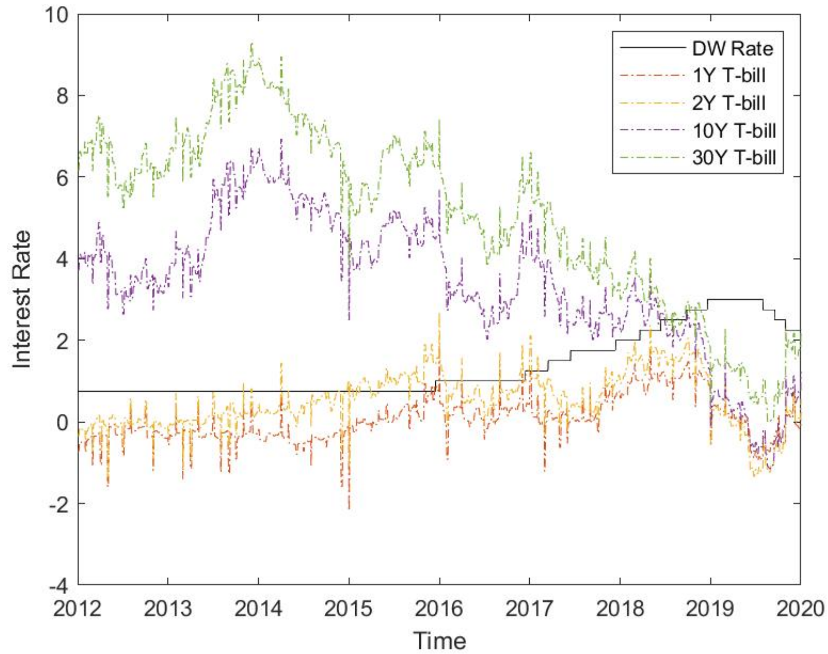


Figure 1.7: Critical and actual DW value.

$\theta(u(q) - q)$. Since $\partial x/\partial\theta$ is positive while lending size stays the same, we can conclude that the public rate is a factor in private agreements even if private rates are not at the upper bound.

The last two plots show the interest rate computed by $i = x/l$, as well as the private interest rate as a percentage of the discount window rate. We can observe that $i/i^b \leq \theta$, where the equality is at the critical value of i^{b*}. From this, we can see that the private interest rate does not need to reach the corridor ceiling i^b, but rather depends on the market power of lenders and borrowers, and can also explain the wedge between the FFR and the DW rate.

If $i^b < \frac{\gamma-\beta}{2\sigma\beta}$, public lending improves aggregate welfare by relaxing the liquidity constraint.

When public rates are low, borrowing from the lending facility is cheaper than holding onto cash due to the inflation tax. This expands the agent's total money holdings brought into Stage 3, and quantity traded is higher than in the money equilibrium. Can this explanation be seen empirically? Since the discount window for the US is always utilized, we expect that $i^b < i^n/2\sigma$ generally holds true. Figure 1.7 plots both the discount window rate and the nominal interest rate using different treasury securities for a period of 2012-2020. If the critical value is above the DW rate, then the DW is welfare improving and vice versa. From the theory, we find that for this period, access to the discount window is welfare improving if banks hold long term securities (higher nominal interest rate), and does not affect consumption if banks hold short term securities (lower nominal interest rate). When the central bank enacts contractionary monetary policy and raise the DW rate, then access to public credit does not increase aggregate consumption since the cost of borrowing is higher. When the central bank enacts expansionary monetary policy by lowering the DW rate, then the gains to the trade surplus depends on the asset portfolio of banks.

This theoretical result supports empirical findings by Drechsler et al. [2016], who looks at a panel of countries from 2007 to 2011, and Ennis and Klee [2021], who looks at US banks from 2012-2017. They find that controlling for salient characteristics between banks and across-country variations, banks who access the discount window hold a riskier (higher return) asset portfolio and lower reserves (lower m), which is a relationship that we also find in the model. From a finance perspective, access to a standing facility can relax liquidity constraints and affect the bank's maximization problem, which allows them to put more weight on their risky asset because the public funds can be treated as a risk-less asset.

1.5 Conclusion

This paper characterizes the steady state equilibrium in a Lagos-Wright economy where there exists one type of agent who can participate in public and private lending. The model finds that: (1) the DW improves aggregate consumption by relaxing the liquidity constraint if the lending rate is sufficiently low, (2) the choice to borrow from the DW is dependent on the asset portfolio of the bank, (3) bargaining power between borrower and lender banks only affect surplus allocations and does not affect trade quantities, (4) access to the DW ensures that buyers capture a larger share of the trade surplus by providing an outside option.

This paper contributes to the literature on monetary policy mechanisms, and shows that public lending can be welfare improving even in the absence of aggregate shocks by providing credit, which acts to relax the liquidity constraints faced by banks. This externality offered by the discount window should be considered for future policy debate.

Further directions that could be explored includes: introducing two types of agents so that bargaining power can also affects trade quantities, as well as heterogeneous access to public lending. Switching the matching technology in Stage 2 with a continuous time matching problem akin to Afonso and Lagos [2015] or adding a stochastic demand coefficient to the Stage 3 good would also generate a distribution of trade sizes even with degenerate distribution of money holdings.

Chapter 2

The Role of Public Lending as an Outside Option in Private Markets

Abstract

How do banks use external funding sources when faced with an unexpected liquidity shock? This paper uses loan-level transactions from the Paycheck Protection Program (PPP) to understand how a bank's decision to borrow reserves from the discount window (DW) affected its lending behavior during the COVID-19 crisis. Implementation of the PPP can be seen as an exogenous shock to the liquidity demand for banks, independent of their financial conditions. By exploiting this independence, I find a causal relationship between use of DW and the number of PPP loans extended by large banks but not small banks. While both types used the DW in the absence of a long-term funding source, usage of the DW almost doubled PPP lending for large banks. After the establishment of a long-term funding source, however, this effect was reduced to 69% due to substitution away from the DW. These findings suggest that in the presence of an unexpected liquidity shock, the DW plays a critical role in extending short-term liquidity to the banking sector.

2.1 Introduction

The discount window (DW), operated by the Federal Reserve, has always been central to financial stability. Banks that cannot obtain liquidity from other sources use the DW as a lender-of-last-resort. Until recently, there has not been enough data to show how important the window was in ensuring that the liquidity needs of banks are met.[1] The implementation of the Paycheck Protection Program (PPP) during the COVID-19 Epidemic gives us a natural experiment to observe the liquidity-provision services of the DW. PPP loans demanded can be seen as a conditionally exogenous shock to the liquidity needs of banks independent of their financial health. Therefore, by using loan-level data, we can estimate the impact of the DW on the banking sector by observing its effect on bank lending.

This paper finds evidence that banks used the DW as a temporary liquidity source and to expand the number of loans they can originate early in the program. Using a recently released set of DW data, I find a strong correlation between a bank's daily PPP lending and its propensity to use the DW. An event study approach finds that all banks use the DW as a temporary measure of liquidity while waiting for a long-term liquidity source. Furthermore, a cross-sectional analysis finds a positive causal effect of DW usage on the quantity of PPP lending done by large banks, defined as assets greater than $600 million. The point estimate for small banks was large but had no statistical significance. Large banks that borrowed from the DW during the early stages of the PPP program extended almost twice as many loans as their non-borrowing counterparts. This effect was strongest before the establishment of a long-term funding source but retained significance even after long-term funding was available. Conditional on usage, a higher quantity borrowed from the DW also increased the quantity of PPP loans extended.

Does the DW play an economically significant role? Prior to COVID, discount window

[1] Discount window data was only publicly released after the enactment of the Dodd-Frank Act in 2010. Since then, there has not been a major incident until the COVID-19 Epidemic.

borrowing averaged about one to two billion dollars every quarter. In the second quarter of 2020, overnight borrowing from the window increased by three orders of magnitude, reaching a level of around $927 billion (38% of aggregate reserves). On April 3, 2020, submissions for PPP loans officially began, allowing small businesses to request loans from eligible financial institutions. Phase 1 of the PPP program lasted from April 3 to April 16 and distributed $349 billion to small businesses. Phase 2 of the program began on April 27 after an additional $320 billion in funding was approved. During Phase 1, banks borrowed a total of $220 billion in overnight funding from the DW.[2]

Figure 2.1 plots the aggregate level of DW borrowing and PPP lending for the period of April and May when the majority of PPP loans were distributed. As the figure shows, large increases in PPP lending are strongly correlated with large increases in DW borrowing. The correlation is strong during Phase 1, before funds from the PPPLF were distributed, and weaker in Phase 2 when banks had access to long-term liquidity and loan demand was more stable. As Phase 1 of the PPP program ended unexpectedly due to an announcement from the SBA, the timing of the PPP program was not related to the choice of banks to use the DW.[3]

My first analysis explores whether banks increase their likelihood of borrowing from the DW when they lend more PPP loans. Using a linear probability model, I regress an indicator variable of DW usage on reserve-adjusted PPP lending at the bank-day level, with controls for bank characteristics and Fed district and date fixed effects. Since banks of different asset class hold different levels of reserves, the reserve-adjustment is necessary to not over-weigh the impact of large banks in the regression. I find that a 10 percentage point (pp) increase

[2] Since banks can borrow the same amount for multiple days, these values have been converted to overnight-equivalent rates. When looking at just the total quantity of borrowing (without the conversion), Phase 1 borrowing was only $22.4 billion (implying an average loan length of 10 days). Total borrowing for the second quarter of 2020 reached $74 billion.

[3] The SBA posted a statement on its website on April 16, 2020, saying that it is currently unable to accept new PPP applications based on currently available funding. A joint statement by Secretary of the Treasury, Steven Mnuchin, and Administrator of the SBA, Jovita Carranza, was made on April 15 to urge the Senate to appropriate additional funding for the program.

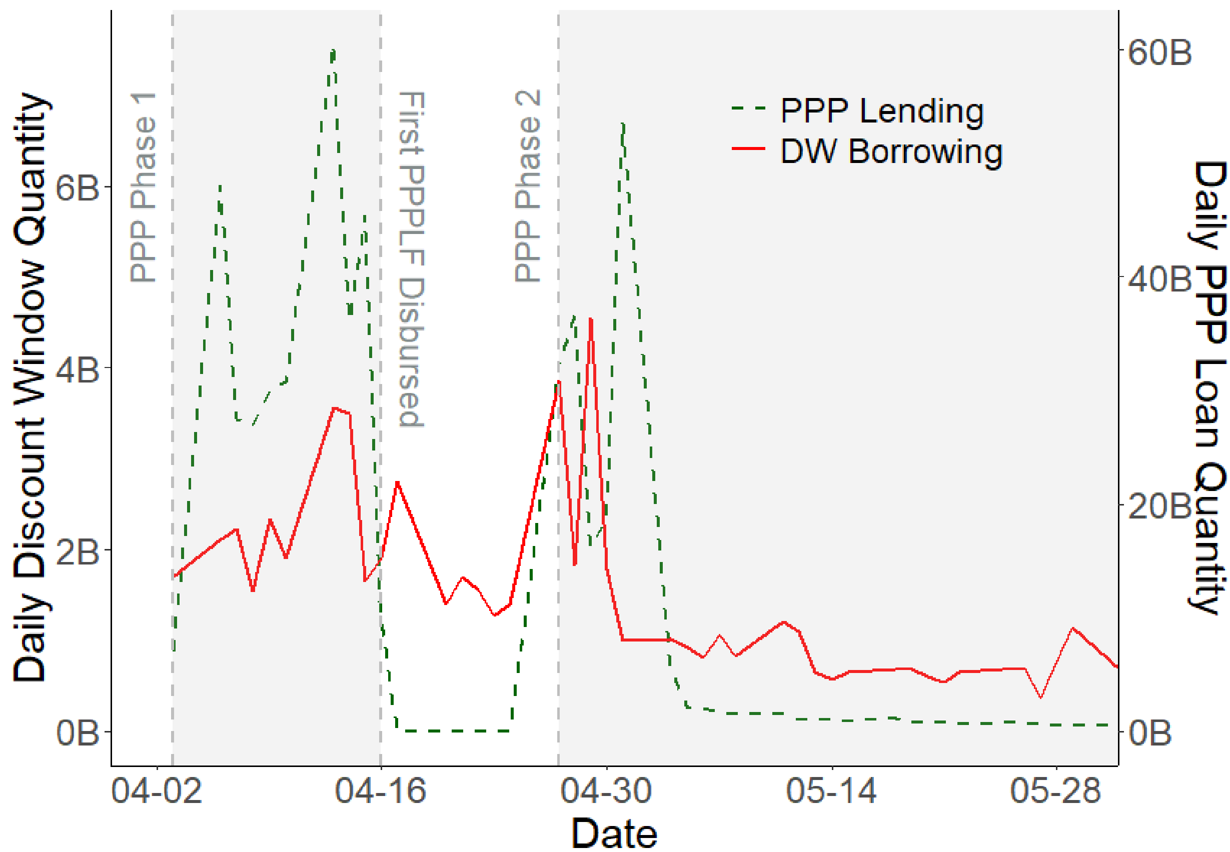


Figure 2.1: Plot of the DW Borrowing and PPP Lending at the aggregate level after the PPP program has begun until mid-May. The green dashed line represents the aggregate amount of PPP loans lent out on that day, with scaling on the right y-axis. The red line corresponds to the aggregate amount of DW borrowing on that day, with scaling on the left y-axis. Vertical dashed lines signify important events. PPP phases are shaded in gray. Values of the DW borrowing quantity have not been converted to overnight borrowing, so magnitudes are smaller. Weekends are dropped since DW is closed on weekends. An alternate version with the weekends included is shown in Figure B.1.

in reserve-adjusted PPP lending is correlated with an 18% higher chance of borrowing from the DW in the pooled sample. When looking at large versus small banks (~$600M cut-off), the results differ depending on how the regression is specified. The pooled sample with an interaction term between size and PPP lending shows a significant positive correlation for large banks. When looking at subgroups, however, the coefficient of interest is significant for small banks.[4] This probability is negatively correlated with measures of a bank's financial

[4]The main analysis is done using Federal District fixed effects to account for different scrutiny levels of each district to window usage. A robustness check replacing district by bank fixed effects finds that a 10 pp increase in PPP lending is correlated with a 4.8% higher chance of borrowing from the DW for large banks

stability and has no correlation with the impact of COVID.

I then look to the question of why banks are borrowing from the DW. If banks only used the DW as a temporary source of liquidity before they can get long-term funding, then they should use it while waiting for funds from the PPPLF to arrive. To test this hypothesis, I construct an event study design, taking the period while banks wait for PPPLF funds as a treatment. I then estimate a two-way fixed effects model and find that on average, large banks increase their DW borrowing probability by 2.6-3%, while small banks only increase their borrowing by 1.1-1.3%. This effect persists for up to three weeks in the case of large banks, and only about one and a half weeks for small banks. After receiving funds from the PPPLF, both sets of banks decrease their use of the DW, lending support to the conjecture that banks are using it as a stopgap measure of liquidity.

Given that there is evidence that banks used the DW for liquidity purposes, did DW borrowing expand the amount of PPP lending done by banks? In the tertiary analysis, I perform a cross-sectional regression using the aggregated lending by banks during April and May. There are two main endogeneity concerns that must be resolved to establish a causal relationship between DW usage and PPP lending. The first source of endogeneity comes from heterogeneous balance sheet cost and liquidity constraints for each bank. Banks that are highly constrained in their reserves could simultaneously tap into the DW for funds and decrease the amount of PPP loans that they extend, which would negatively bias the true relationship if left uncontrolled. Since bank characteristics can only be observed quarterly, daily fluctuations in liquidity constraints cannot be captured. The second source of endogeneity comes from the fact that banks can choose the number of loans they originate on a given day. Therefore, the decision of loan origination and DW borrowing is likely jointly decided, resulting in simultaneity bias.

To solve these endogeneity problems, I use the previous familiarity of each bank with the DW

and no effect for small banks. This result is robust to Poisson and Logistic regressions.

as an instrument, an approach similar to Anbil et al. [2021]. The instrument is constructed by aggregating all DW borrowing by a bank since 2010 and dividing it by the reserve quantity reported in the Call Reports. The instrument fulfills the relevancy condition due to a bank's propensity to use the DW again if they have already used it in the past and captures how familiar a bank is with posting collateral and withdrawing funds from the window. Banks that want to use the DW have to submit forms to determine their eligibility, as well as post collaterals to the DW before funds can be advanced. These logistical constraints can make it difficult for banks to borrow from the DW without having prior experience. Once the fixed cost is paid, however, borrowing from the DW only requires a call to the local Federal Reserve branch. This makes it so banks who have previously used the DW face a lower marginal cost of using the DW once again.

The primary assumption for this instrument to be valid is that familiarity with the DW only affects the amount of PPP lending done through its effect on a bank's current likelihood to use the window again. Although previous DW usage might affect a bank's propensity to use other sources of external funding, I control for these alternative sources. The exclusion restriction can also be violated from unobserved bank-specific risk tolerance, which could affect their decision to use the DW and their decision to extend risky loans. PPP loans are a special case, however, as they carry zero weight when calculating risk-weighted regulatory ratios. As a result, any unobserved risk factors should be orthogonal to the number of PPP loans a bank chooses to extend since PPP loans are riskless. Using a two-stage least squares approach, I find that DW usage increased PPP lending from large banks by 91% during Phase 1 of the PPP program, but had little to no effect on small banks. At the intensive margin, an increase in DW borrowing by one standard deviation during Phase 1 increased PPP lending by 43.6%. These effects hold when the sample is extended to the end of May, but are weaker due to substitution towards long-term funding provided by the PPPLF.

One explanation for the differences in small bank behavior could be that small banks face greater stigma than large banks when accessing the DW. From Berger et al. [2014], small banks that borrowed from the 2008 Term Auction Facility were generally weak as compared to their counterparts while large banks were not. This implies that when accessing central bank lending facilities, smaller banks give off a stronger negative signal of asset quality and are subsequently more averse to using them. Another possible reason why small banks aren't as affected by the DW is that the fixed costs for small banks are not worth the marginal benefit. Since banks only obtain 1-5% of the origination fee for PPP loans, banks that have high PPP demand from businesses obtain greater benefits from lending. If the fixed cost of borrowing from the DW is large, either from logistical or informational frictions, then small banks might not find it worth their resources. Additionally, smaller banks hold more liquid assets as a share of their portfolio and do not need as much external funding as large banks. This can be seen through the DW data, as only 23% of small banks have previously borrowed from the DW as compared to the 63% of large banks.

This paper shows the importance of the DW during liquidity crises. When banks face any liquidity crisis, either through exogenous demand shocks or an increase in interest rates, they should be aware of the options available. If banks are more willing to obtain external liquidity through the discount window, they can drastically reduce the risk of bankruptcy. Currently, due to the rapid interest rate increase, many banks are facing an unrealized loss due to the fall in bond prices. Borrowing from the DW during this time could alleviate the consequences of a possible bank run and ensure the stability of the banking sector.

2.1.1 Related Literature

This paper contributes to a growing literature on examining the effects of liquidity facilities on the PPP program. Lopez and Spiegel [2021] and Anbil et al. [2021] analyze the effect of the PPP Lending Facility (PPPLF) on the distribution of PPP loans using measures of prior relationship with the Small Business Administration and familiarity with the posting of loan collateral to the DW as exogenous instruments. Both articles find a strong causal effect of the PPPLF on the quantity of PPP lending, with larger effects for small banks. This paper examines the effect of an alternate source of central bank lending, the discount window, and finds that it primarily supports large banks in PPP lending, especially during Phase 1 of the program before funds from the PPPLF were available. Some banks used the DW to extend more loans and others used it for temporary liquidity before PPPLF funds were available. I find my work to be highly complementary to this literature by exploring how banks acquire liquidity during the early stages of the PPP program and how different types of banks use each funding source.

Another part of the PPP literature examines its role on employment. The articles in this field include Barraza et al. [2020], Chetty et al. [2020], Autor et al. [2022], and Faulkender et al. [2020], all of which find a positive effect of PPP lending on employment outcomes. Specific to my work, Granja et al. [2020] finds that firms that received PPP loans earlier in the program had better employment outcomes than those that received loans later. Li and Strahan [2020] also finds that PPP supply had a strong effect in preserving local employment, especially those received during Phase 1 of the program. I study how DW borrowing during Phase 1 increased the number of PPP loans lent out by large banks, which implies that if banks were more willing to use the DW to relax liquidity constraints, the employment effect of the PPP could have been amplified.

This work also relates to the prior literature that examines DW use for liquidity during financial crises. Armantier et al. [2015] show that banks are willing to pay a premium of 44 basis points across other funding sources (Term Auction Facility (TAF), repos, etc.) to avoid usage of the window due to stigma during 2008. Berger et al. [2014] looks at Federal Reserve lending through the DW and the TAF in 2008 and found that the liquidity injected through these two facilities increased aggregate lending to small and large businesses. Furthermore, they found that small banks that chose to use the DW were weaker than their counterparts, measured by lower capital ratios and higher portfolio risk. This does not hold true for large banks, which could imply that the information channel of stigma faced by small banks could be larger than for big banks. A more recent analysis done by Glancy et al. [2020] shows that deposits were the main source of funding for banks, as aggregate deposit inflows exceeded aggregate growth in commercial and industrial lending. Although this pattern holds in the aggregate, there exists heterogeneity in deposit growth within the banking sector, leading some banks to access external funding. I contribute to this literature by exploring how DW borrowing affected small and large banks heterogeneously during the COVID crisis, and find differences between the two classes of banks in terms of lending behavior.

The rest of the paper is organized as follows. Section 2 describes the institutional details. Section 3 describes the data construction process. Section 4 contains the descriptive statistics from the data. In section 5, I present my empirical methodology and results. Section 6 discusses the policy implications. Section 7 concludes.

2.2 Institutional Background

The Paycheck Protection Program (PPP) began on April 3, 2020, to help small businesses continue to pay their workers through the early phases of COVID. The program was administered by the Small Business Administration (SBA) but was directly distributed to consumers by eligible financial institutions. Banks that qualify to lend included all federally insured depository institutions, credit unions, and Farm Credit System institutions that were pre-qualified to lend through the SBA. Financial Technology (FinTech) companies were approved to offer PPP loans at a later date due to the high demand faced by traditional banks. Although FinTechs were introduced as an alternative source of loans, they did not compete with traditional banking for customers. Erel and Liebersohn [2020] shows that the decision to allow lending through FinTechs expanded overall access to financial services, playing a complementary role to traditional banking.

PPP loans were disbursed in two phases. The program's first phase distributed $349 billion to small businesses and lasted from April 3 to April 16, when government funding quickly depleted due to high demand. Phase 2 began on April 27 when President Trump extended another $320 billion that lasted until August 8, with most of the loans in Phase 2 distributed in April and May. As a borrower, PPP loans had a fixed interest rate of 1%, were deferred for the first six months, and were generally forgiven. The loans had a maturity of two years if originated before June and five years after June. Lenders can obtain 5% of the origination amount as a fee on loans smaller than $350,000, 3% on loans between $350,000 and $2,000,000, and 1% for loans greater than $2,000,000.

The PPP faced many problems during the early stages. First, the SBA was slow at publishing their regulatory forms, causing some banks to devote extra resources to helping customers. Second, SBA computers had limitations on how many PPP loans they can process at a given time during the early stages, causing banks to favor customers with pre-existing relationships. While this may have heterogeneous effects on which businesses succeeded and failed, this preferential treatment should not affect the demand shock on liquidity faced by the banks.

Since PPP loans were insured by the SBA, financial institutions faced no default risk for lending and were only constrained by their liquidity. PPP loans also carried zero weight when calculating the capital ratio for the bank, but are added to the total assets when calculating the leverage ratio unless pledged to the PPPLF as collateral. This zero-weighing made these loans very attractive to banks, as they were riskless and therefore not subject to risk-weighted regulatory capital requirements. PPP loans were generally provided within ten days of a small business applying, so the decision of a bank to approve a loan was likely jointly decided along with the decision to use external funding.

The Fed also established a PPP Lending Facility (PPPLF) to provide long-term funding to the financial sector beginning on April 9, six days after the PPP program began. To apply for funding, banks had to post their PPP loans as collateral, be approved by the Fed, and subsequently receive their funds. This period can take anywhere from one week to three months, with the median time being three weeks. Funds advanced by the PPPLF had an interest rate of 35 basis points and had the same maturity as the PPP loan used as collateral. Loans made under the PPPLF to banks were extended on a non-recourse basis, so banks did not face any liquidity risk from borrower defaults. PPP loans that were pledged to the PPPLF were also not included in the leverage ratio requirements, allowing banks to extend liquidity without regulatory restrictions. Anbil et al. [2021] finds a causal relationship between the choice of banks to access the PPPLF and the amount of PPP loans they originate, with banks that use the PPPLF extended over twice as many PPP loans as their counterparts. The first PPPLF distribution to a bank was made on April 16, the last day of Phase 1 of the PPP program, so lenders who faced liquidity issues during Phase 1 had to use alternative sources for liquidity.

A possible alternative source of funding for banks at this time was the discount window (DW). During times of crisis, the DW is meant to be a lender of last resort to manage liquidity risk and prevent credit rationing from banks. The DW extended overnight loans for up to 90 days at a rate of 25 basis points to all financially sound institutions through their Primary Credit program.[5] DW funds must be collateralized using eligible bank loans and securities before the date of borrowing, including but not limited to PPP loans.[6] Requesting a loan consists of calling the local Reserve Bank and providing verification information. However, before requesting a loan, the bank must file the corresponding Operating Circular No. 10

[5]On March 15, 2020, the Federal Reserve announced changes to primary credit, including changes to the length of DW loans from a period of 30 days to 90 days.

[6]Eligible loans include commercial, industrial, agricultural, consumer, and real estate loans. Eligible assets include corporate bonds, money market instruments, asset-backed securities, collateralized mortgage obligations, and Treasury bills. From the Fed Board: Generally, it is not operationally feasible to pledge collateral (other than book-entry securities issued by the U.S. Treasury, U.S. government agencies, or U.S. government-sponsored enterprises) on the day a loan is requested.

agreement with the lending Reserve Branch. Banks that have previously used the DW are more likely to use it again in case of an emergency, due to lower information/logistical costs or habit formation. Because the DW can provide instant liquidity with no questions asked, it could have played an integral role during Phase 1 of the PPP program before a long-term funding source was established.

Liquidity issues were most likely during Phase 1 of the program when loan demand outpaced loan supply.[7] After the first two weeks of Phase 2, demand for PPP loans slowed, and the remaining funds were slowly distributed until August 8. Without funding from the PPPLF in Phase 1 of the program, banks flooded to the DW and borrowed $40.8 billion over the two weeks. Although the PPPLF began on April 9, the first disbursement to banks did not occur until April 16, the last day of Phase 1. This meant that for two weeks, banks did not have the necessary long-term funding to extend loans, which could have driven them toward the DW.

2.3 Data

The primary data source for this paper comes from the PPP loan database obtained from the SBA. This data set contains the loan level data for all PPP loans that were distributed throughout the program, the quantity of the PPP loan, select borrower characteristics, ZIP level location, and the name of the originating financial institution. Data linking each financial institution to its unique Federal Reserve ID was compiled and provided by Erel and Liebersohn [2020]. Since one bank can have multiple branches, I aggregate the data to the bank-by-day level and match the resulting data to commercial banks that filed FFIEC Call Reports in Q1 of 2020.

[7] See Li and Strahan [2020] and 'PPP Money Abounded – But Some Got It Faster Than Others', Wall Street Journal. Although the PPP was eventually extended to all eligible businesses, there was a disparity in which businesses received it first.

I also used loan-level PPPLF data obtained from the Federal Reserve website. The data contain the borrowing institution, the date of advance, the loan size, and the maturity date. Because the maturity dates of the PPPLF are matched to the maturity dates of the PPP loans, I can calculate the time it takes for the PPPLF to process a request for an advance. Although banks can expect when they will receive PPPLF funds, the exact date is unknown to them. This implies that the date of PPPLF receipt can be taken as an exogenous shock to banks. There also exists a slight negative correlation between the processing time and the date of the PPP program, which suggests that the PPPLF process was more streamlined in the later stages of the PPP program compared to when the PPPLF began operation.

Then I merge information on the daily borrowing of banks from the DW available on the Federal Reserve website. The DW data gives information on the borrowing financial institution, the size and duration of the loan, the collateral posted to the Fed by that institution, and the type of credit (primary credit, secondary credit, and seasonal credit). I include only primary credit in the analysis since seasonal credit is meant for seasonal fluctuations in the credit demand of smaller banks and secondary credit constitutes less than 1% of DW observations in the sample period. This data set contains the universe of loans lent by the Federal Reserve from 2010 to 2020. For loans that are borrowed for multiple days, I calculate the overnight-equivalent amount.[8] I count any borrowings from the discount window less than $100,000 as a test loan and drop these values in the analysis.

I gather bank characteristics from quarterly Call Reports published by the FFIEC and filed by all commercial banks with US branches. Banks in this set are split into two groups based on size, with 'small' banks defined as those in the lower 75th percentile of assets measured in the Q1 2020 Call Reports. This cutoff corresponds to a bank with assets equal to $593 million, which was close to the cutoff level of $600 million made in Anbil et al. [2021]. For robustness, I also consider the 90th and 95th percentile cutoff values, corresponding to

[8]Following Ennis and Klee [2021], a loan of $10 million for three days is equivalent to three overnight loans of $10 million.

banks with \$1.73 and \$4.46 billion in assets, respectively. Since bank-level characteristics are only observed through Call Reports at the quarterly level, I construct the measure of DW borrowing and PPP shock as a share of reserves by dividing the daily size of PPP lending against the last known reserves of that bank.[9] I also consider a normalization based on the assets of banks instead of reserves, but this would over-weigh large banks due to the negative correlation between bank size and the reserve-to-asset ratio.

Finally, I combine information from the 2019 Summary of Deposits, which gives branch-level information about each bank and the amount of reserves held at the branch. I use this deposit share as weights to calculate a bank's COVID exposure at the county level (measured by new cases) and exposure to economic shocks at the week-state level using the time series data from Baumeister et al. [2021]. Although imperfect, these two exposure measures should be sufficient to eliminate most of the differential effects attributed to COVID exposure. Even if COVID exposure cannot be perfectly controlled, Granja et al. [2020] finds little to no evidence that funds flowed to areas more economically affected by COVID.

Since the question of interest is to look at how liquidity demand by banks affects their choice to borrow from the DW, I only include periods where the demand for PPP loans is greater than the supply. From Granja et al. [2020], banks were mostly restricted in liquidity during April and May, when the PPP program had the highest levels of demand from businesses and 97.1% of all PPP loans were issued. After May, demand for PPP loans fell off and the supply was not the constraining factor. Therefore, I include only data from Phase 1 and April and May of Phase 2.

Summary statistics for all variables used in the regressions can be found in Table B.1 for the panel data and Table B.2 for the cross-sectional data. Since the behaviors of the largest banks can drive the majority of the results (Chase, Bank of America, etc.), I winsorize the

[9] In practice, consider a bank with a reserve of \$40 million filed in Q1 of 2020 and \$42 million filed in Q2 of 2020. A \$20 million shock on May 5 would be considered 50% of the bank's reserves, while the same shock on July 3 would be considered 47.6% of the bank's reserves.

	No Borrowing	Borrowed from DW	Borrowed from PPPLF	Borrowed from both	Total
Small Community Banks	2242	75	239	23	2579
Large Banks	757	91	166	34	1048
Pooled	2999	166	405	57	3627

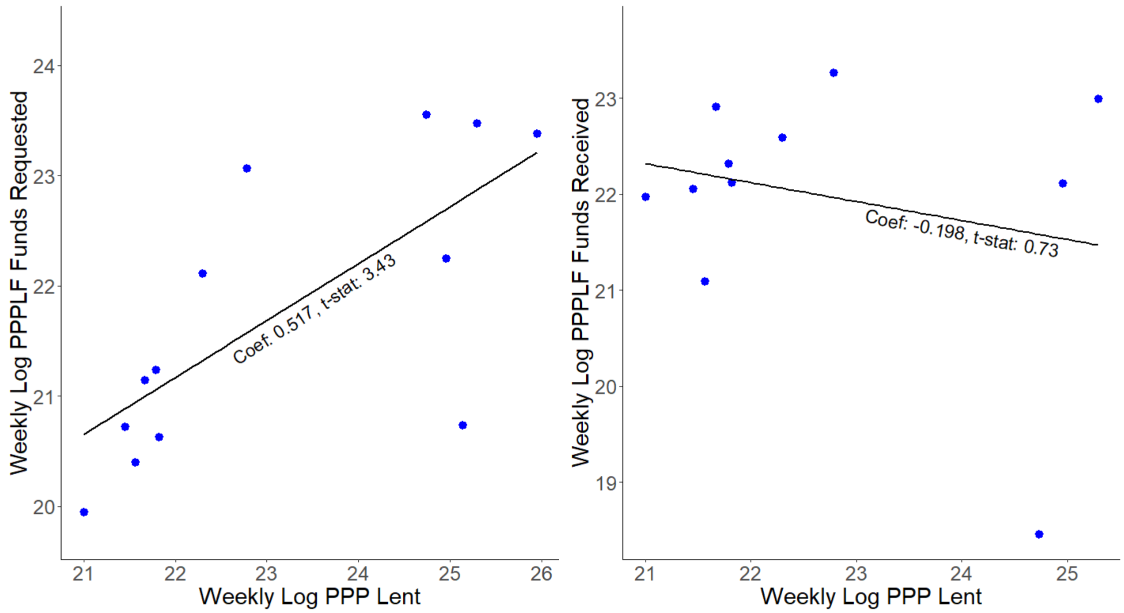
Table 2.1: Breakdown of banks observed lending PPP loans. The sample contains all banks observed during Phase 1 (April 3 to April 16) and the first two months of Phase 2 (April 27 to May 31).

data to remove the effects of these outliers. All variables, except bank size, are winsorized at the .1 percentile and 99.9 percentile.[10]

2.4 Descriptive Statistics

Table 2.1 shows the breakdown of small and large banks in the sample. I include only banks that lent out at least one PPP loan during April and May, since banks that choose not to lend out PPP loans may have other financial constraints at play, which would affect their decision to borrow from the window independent of PPP shocks. The table shows that 28% of large banks borrowed from the DW or the PPPLF, while only 13% of small banks did. This could be because 92% of the PPP loans during the first two phases were lent out by these large banks, which contributed to them needing the most liquidity. Large banks also borrowed 89% of the DW funds observed in this period and 83% of the PPPLF funds based on volume. These statistics imply that there is a differential effect of the two lending facilities across the two classes of banks. Larger banks might have the expertise necessary to withdraw funds from the window, whereas smaller banks find it easier to use the PPPLF due to fewer restrictions and greater accessibility. Consistent with the hypothesis, Lopez and Spiegel [2021] also shows evidence that participation in the PPPLF was an important driver for small bank lending during this period.

[10]The standard winsorizing method is to use the 1st and 99th percentile as cutoffs, but since only 2% of the observations have DW quantity greater than zero in the time series data, a 99th percentile cutoff would change half of the DW data. I take a more conservative winsorizing approach to keep the majority of the DW borrowing information.



(a) Weekly PPPLF funds **requested**. **(b)** Weekly PPPLF funds **received**.

Figure 2.2: X-axis is the log quantity of PPP loans granted in a given week, Panel A plots the total quantity of PPPLF funds requested in the same week, and Panel B plots the total quantity of PPPLF funds received in a given week. The data is aggregated at the country level. Each data point represents one week of the PPP program.

A possible question that might arise is why do banks not borrow from the PPPLF if they face liquidity constraints. If banks borrow from the DW to fund their PPP loans, they face the issue of maturity mismatch, where the maturity of the liability (DW borrowing) is years shorter than the maturity of the asset (PPP loan). The PPPLF resolves the mismatch issue by extending liquidity advances to banks matching the maturity of the PPP loan posted as collateral, therefore banks should use the PPPLF instead of the DW to fund PPP loans. One reason why banks cannot do this is that there are logistical issues that banks face when requesting a PPPLF advance. For a bank to receive an advance, they must post the PPP loan as collateral and submit the application materials to the SBA. This application process can take anywhere from one day to up to four months before the SBA approves the PPPLF advance.[11] Therefore, banks cannot receive PPPLF funds before extending PPP loans, forcing them to use either internal funding or alternative funding sources. Figure

[11]Mean: 31.7 days, median: 18 days.

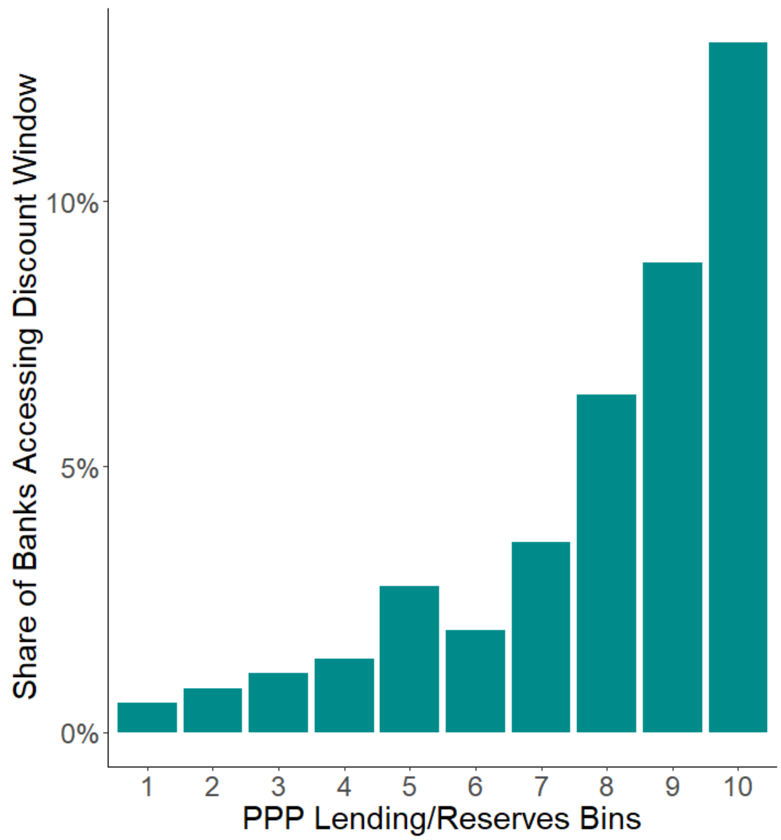


Figure 2.3: Binned bar chart of PPP shock as a share of reserves on the x-axis and the share of banks in that bin that went to the discount window on the y-axis. The bars are an aggregation of each 10% quintile and are aggregated by using means.

2.2 shows the aggregate amount of PPPLF funds requested in panel **(a)** by banks at the weekly level, and when banks actually received the requested funds in panel **(b)**. There is a strong correlation between PPP lending and PPPLF requests, but no correlation between PPP lending and PPPLF funds received. Therefore, a possible reason why banks used the DW during this period was to fill the liquidity shortage from the PPPLF caused by the processing delay.

Was DW borrowing economically significant? During April and May of the PPP program, the aggregate level of reserves of the financial sector was \$2.4 trillion, \$721 billion in PPP loans disbursed (30% of aggregate reserves), \$42.3 billion in long-term funding through the PPPLF, and \$139 billion lent by the Fed through the DW. Pre-pandemic, quarterly bor-

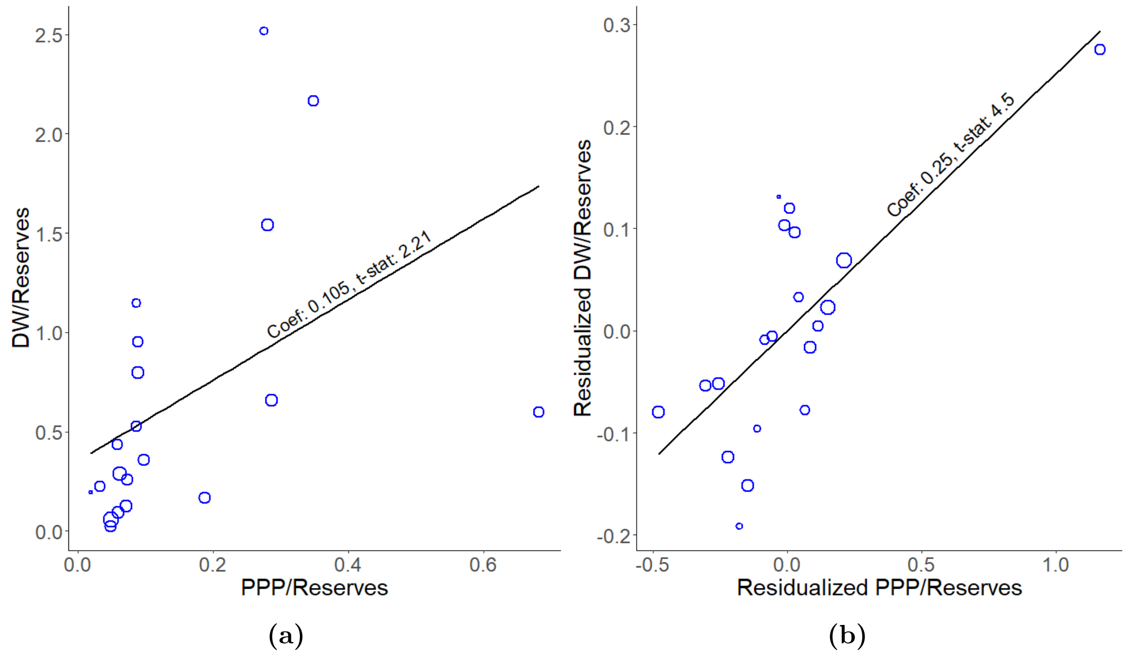


Figure 2.4: Binned scatter plot showing the relationship between PPP Lending/Reserves and DW Borrowing/Reserves using the time-series data. Panel **(a)** is the non-residualized data while panel **(b)** uses the residualized data after controlling for bank characteristics. The banks included in this plot are banks that simultaneously borrow from the DW and lend out PPP loans on a given day. Any test loans (defined as DW borrowing < \$100,000) are dropped from the sample. The size of the points represents the average bank asset within the bin.

rowing quantity from the DW averaged around one to two billion dollars from the financial sector as a whole, with the majority of borrowing coming from small banks using seasonal credit. Not only was there a 100-fold increase in the quantity borrowed, but there also exists a high correlation between banks that lent out PPP loans and DW borrowing. Figure 2.3 uses the cross-sectional data and divides banks into deciles based on the reserve-adjusted amount of PPP lending. 13% of the highest decile banks used the DW at least once during April and May, while only 0.5% of the lowest decile banks did. This increasing relationship also holds when splitting the banks into groups based on the size cutoff criteria, which shows that bank size is not the main driver of the relationship. When looking at the relationship between the binned reserve-to-asset ratio and the share of banks that access the DW, there is a strong negative relationship between the two series, implying that liquidity constraints are a primary factor that influences banks to borrow from the window.

Given that liquidity-constrained banks might not extend as many loans as their counterparts, would DW borrowing alleviate those restrictions? Figure 3.3 shows the binned relationship between the reserve-scaled DW borrowing and PPP lending using the time series data. A positive slope means that a larger amount of PPP lending is observed jointly with a larger quantity of DW borrowing. The size of the points represents the average bank size in that bin. We can see from the figure that PPP shocks are positively correlated with DW borrowing, and the effect is stronger after controlling for bank characteristics such as small business relationships, liquidity measures, and COVID exposure. One possible explanation from this graph is that when liquidity-constrained banks are faced with the choice to extend loans, they approach the DW to relax these constraints instead of rationing credit. Since the DW and PPP values are normalized by bank reserves, the distribution of bank sizes along the axes are relatively evenly distributed.

2.5 Results

In this section, I first show the correlation between DW usage and PPP shocks using the time-series data for the panel of banks. I then look at the cross-section of banks and answer the question of whether usage of the DW expands the amount of PPP lending.

2.5.1 Relationship between PPP lending and DW borrowing

Using the panel data, I first explore whether banks increase their probability of borrowing from the DW when they lend more PPP loans. I estimate the linear probability model:

$$1[\mathrm{DW}_{it}] = \beta \mathrm{PPP}_{it} + \gamma \mathbf{X}_{it} + \delta_{F(i)} + \delta_t + e_{it} \tag{2.1}$$

where DW_{it} is an indicator that equals one if the bank has borrowed from the DW during that day. PPP is the PPP lending quantity for that day, scaled by the bank's first quarter reserves.

β estimates the response of DW borrowing probability to a change in the PPP shock. $\mathbf{X}_{it}$ is a vector of time-invariant bank-specific control variables and exposure variables that are time-variant. δ are both Federal Reserve District and time fixed effects.

This specification allows us to look at the variations between banks within a Fed district within a particular day. I include district fixed effects, since DW policy may differ across Federal Reserve Districts and potentially confound the estimation. I also include time fixed effects to account for changes in the aggregate demand of PPP loans and conditions that affect all banks equally. For example, the first disbursement of PPPLF funds to banks began after phase 1 and before phase 2, therefore the response behavior of banks could change between the phases due to less liquidity need after the first phase. The DW was also closed on weekends, which would affect the borrowing behavior of all banks equally.

In terms of controls, I control for three relationship measures between banks and small businesses: unused CI commitments, small CI loans, and core deposits scaled by bank assets.[12] I include other relevant bank characteristics that could affect a bank's decision to use the DW or lend PPP loans, such as bank size, liquid assets, commercial and industrial (CI) lending, Tier 1 leverage ratio, reserve-to-asset ratio, and deposit-to-asset ratio. I include a proxy for the sophistication of the bank, its branch-weighted bank age, exposure to new cases of COVID at the county level (daily), and exposure to economic conditions at the state level (weekly) using deposits as weights. Most of these controls have been used by Li and Strahan [2020] and Anbil et al. [2021], which explores the relevant characteristics of banks that lent out PPP loans and whether borrowing from the PPPLF affected their lending behavior. Lastly, I include an indicator variable that takes a value of 1 if the bank has borrowed from the DW before COVID, since that could influence their decision to borrow again.

I include specifications with and without controls for the pooled sample in columns (1) and

[12]These controls have been used in Berger and Udell [1995], Berlin and Mester [1999], Norden and Weber [2010].

Table 2.2: LPM of DW borrowing probability. Column (1-2) is the pooled sample with and without controls, column (3) is the pooled sample with the treatment variable interacted with bank size, column (4-5) is for large banks in the upper 25th percentile (assets greater than \$600M), and column (6-7) is for small community banks with assets in the lower 75th percentile. The sample contains observations from Phase 1 (April 3 to 16) and the early stages of Phase 2 (April 27 to May 31).

Dependent Variable:	DW Indicator						
	Pooled		Interacted	Large Banks		Small Banks	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
PPP Lending/Reserves	0.035***	0.026**		0.038*	0.036	0.023***	0.016**
	(0.011)	(0.011)		(0.022)	(0.023)	(0.009)	(0.008)
PPP Lending/Reserves × Size=0			0.013				
			(0.008)				
PPP Lending/Reserves × Size=1			0.041*				
			(0.022)				
Previous DW Use Indicator		0.022***	0.022***		0.010		0.029***
		(0.006)	(0.006)		(0.010)		(0.008)
Reserve to Asset		-0.060***	-0.061***		-0.274***		-0.043***
		(0.013)	(0.013)		(0.070)		(0.012)
Deposit to Asset		-0.215***	-0.216***		-0.327***		-0.161***
		(0.041)	(0.041)		(0.087)		(0.046)
Equity Cap Ratio		-0.513***	-0.516***		-1.16***		-0.307**
		(0.140)	(0.140)		(0.366)		(0.128)
Tier 1 leverage ratio		0.315**	0.318**		1.16**		0.138
		(0.129)	(0.129)		(0.478)		(0.098)
Economic Exposure		-0.0005	-0.0005		-0.002		0.0001
		(0.0007)	(0.0007)		(0.002)		(0.0006)
Deposit-weighted new COVID rate		-0.0010*	-0.0010*		-0.002		-0.0007
		(0.0006)	(0.0006)		(0.002)		(0.0006)
Bank Controls:		Yes	Yes		Yes		Yes
Fixed-effects							
Fed District	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	250,782	242,648	242,648	62,622	58,016	188,160	184,632
Dependent variable mean	0.01459	0.01475	0.01475	0.03320	0.03449	0.00840	0.00855

Clustered (Bank) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

(2) of Table 2.2 and also split the sample into large banks that have assets within the upper 25th percentile (assets > \$573M) and small banks. When looking at the pooled sample in columns 1 and 2, the quantity of PPP lending has a strong and positively correlated relationship with whether the bank chooses to borrow from the DW or not irrespective of bank-level controls. The results in column (2) imply that a 10 percentage point increase

in the quantity of PPP loaned as a share of the reserves is associated with an increase of .25 pp in DW borrowing probability. Given that the dependent variable mean is 1.47%, a .25 pp increase can be interpreted as a 18% increase in DW borrowing probability. When looking at the interacted term in column (3), both coefficients for large and small banks are positive, but only large banks have statistical significance. This flips when we look at the subgroups since column (5) shows no significance for large banks after controlling for bank level characteristics, while the coefficient for small banks is still significant.

When looking at the control variables, the reserve-to-asset ratio, the deposit-to-asset ratio, and the equity capital ratio have strong negative relationships with the probability that a bank borrows from the DW since those are the main indicators of liquidity and stability. The coefficients for the tier 1 leverage ratio are strong for large banks but not small banks, implying that leveraged large banks are more likely to use the DW. Previous usage of the DW is a strong indicator of repeat usage for small banks, but not for large banks. Finally, there is no significant correlation when looking at both measures of COVID exposure, suggesting that exposure to COVID was not a strong factor that affected a bank's choice to tap into the DW.

If we believe that banks are very idiosyncratic in their responses to liquidity shocks, then we should look at the within-bank variation over time. For robustness, Table B.3 reports the result of the same regression using bank instead of district fixed effect. Under this specification, we drop all banks that do not access the DW during the period, since those are the least liquidity constrained. Panel 1 shows the linear probability model with the observations dropped and the same column specifications. When looking solely at liquidity-constrained banks, large banks are the ones most likely to increase their chance to borrow from the DW when they lend more PPP loans. This result is similar in both the interacted column (3) and when looking at each subgroup (4-5). Panel 2 runs the same regression without dropping observations of banks that do not borrow. In this specification, banks that do not borrow from the DW still lend out PPP loans, which drives coefficients toward zero. To account for non-linearity, I also estimate the same model under the Poisson (in panel 3) and logistic (in panel 4) specification, which gives similar results to the linear probability model.

2.5.2 DW as a temporary source of liquidity

My final analysis of the panel data uncovers whether banks use the DW when waiting for a PPPLF advance. Since the DW loan has a shorter maturity than PPP loans, banks might substitute away from the DW loans when PPPLF funds become available. It is possible to extract from the data when a bank applies for PPPLF funding and when they received the advance. If banks do in fact use the DW as a stopgap measure of liquidity, we should find an increase in the overall DW borrowing probability after the bank has requested funding from the PPPLF. Furthermore, when banks received funds from the PPPLF, they should stop borrowing from the DW concurrently since their long-term source of funding has been secured.

To estimate whether this behavior holds in the data, I construct an indicator (WAITING) that takes on a value of one if a bank has applied for funds from the PPPLF but has not received the funds yet. Additionally, I create another indicator (POST) that takes on a value of one after the bank has received PPPLF funding. Following intuition, the expected treatment effect for WAITING should be positive, since banks increase their usage of the DW while waiting for long-term liquidity. Inversely, the expected treatment effect of POST should be negative following the same logic. I set up the canonical two-way fixed effects (TWFE) estimation equation:

$$DW_{it} = \beta \mathbf{X}_{it} + \delta_i + \delta_t + e_{it} \tag{2.2}$$

Where $\mathbf{X}_{it}$ is the WAITING or POST variable, DW_{it} is the indicator of whether the bank

Table 2.3: Regression of DW borrowing indicator on the waiting indicator is displayed in the first panel. In the second panel, the waiting indicator is replaced with an indicator for whether the bank has received the PPPLF advance. All banks are included. The time period is from April to May 2020. Column (1) is the pooled sample, column (2) is the pooled sample with the treatment interacted with size, and columns (3) and (4) are the sub-sample analysis for large and small banks.

Dependent Variable:	DW Indicator			
Model:	Pooled	Interacted	Large	Small
After PPPLF Requested				
WAITING	0.019***		0.026**	0.013***
	(0.005)		(0.011)	(0.005)
WAITING × Small Banks		0.011**		
		(0.005)		
WAITING × Large Banks		0.030***		
		(0.010)		
Observations	296,593	296,593	73,809	222,784
After PPPLF Received				
POST	-0.019***		-0.024*	-0.015**
	(0.007)		(0.014)	(0.007)
POST × Small Banks		-0.013*		
		(0.007)		
POST × Large Banks		-0.028**		
		(0.014)		
Fixed-effects				
Bank	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes

Clustered (Bank) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

was observed to have used the DW, and δ are individual and time fixed effects. β is the difference-in-differences estimator and estimates the average treatment effect of banks that are waiting for PPPLF funds if using the WAITING indicator and after PPPLF funds are received if using the POST indicator.

Table 2.3 displays the result of the TWFE estimation. The first panel displays results using WAITING as the regressor, and the second panel uses POST as the regressor. Column (1) displays results for the pooled sample, (2) for the interacted sample, and (3) and (4) splits the sample into large and small banks at the 75th percentile cutoff. As expected, when looking at the first column, banks that are in the process of receiving funding from the PPPLF increase their DW usage by 2% compared to the counterfactual bank. Once advances from the PPPLF arrived, those banks drop their DW usage down to baseline levels. This effect holds for both large and small banks, with large banks increasing their DW usage by two to three times the amount of small banks depending on the specification. This could imply that either small banks are more averse to using the DW as compared to large banks, or the relative liquidity needs of small banks are not as large.

I then decompose the average treatment effect of banks to individual time periods to look at the treatment effect over time following an event study approach. To achieve this, I replace $\beta\mathbf{X}_{it}$ in Equation (2.2) with $\sum_{-t}^{t} \beta_t X_{it}$. Where t represents the relative time period, with the treatment period normalized to zero for each bank. β_t would then estimate the pre-treatment trend if $t < 0$ and the post-treatment effect for each period following the treatment when $t \geq 0$. If WAITING is the treatment, we should expect to see no pre-treatment trend, since the inclusion of bank fixed effects should net out the variations in liquidity constraints across banks. Any pre-treatment trend would be driven by the differential exposure of banks to PPP lending. If banks that lend out more PPP loans are more financially stable, then they should be more likely to use internal funding to source PPP loans and there would be a negative pre-treatment trend. When using POST as a treatment instead, we should expect to see a positive pre-trend if banks are using the DW before receiving PPPLF funding, with a negative treatment effect due to substitution away from DW funding and into PPPLF funding. The treatment effect should slowly increase in magnitude as banks repay their DW loans.

Since the decision of banks to apply for PPPLF funding is staggered, it is subject to Goodman-Bacon [2018] bias. This bias exists in all staggered treatment designs since units that are treated early are used as a control for units that are later treated. Estimates are more contaminated by this bias if the treatment group is large relative to the control and if treatment effects are heterogenous amongst different treated cohorts. Therefore, we should expect the bias to be larger for large banks since 24% of them accessed the PPPLF during this time as compared to the 11% for small banks. To account for this treatment staggering, I apply the Sun and Abraham [2020] bias correction algorithm, which should give unbiased estimates of the average treatment effect.

Figure 2.5 displays the result of the event study with the Sun and Abraham [2020] algorithm applied.[13] The series in green shows the estimates using the WAITING variable as treatment, and the series in red uses the POST variable as treatment. It can be seen from the figure that in the pooled sample, banks increase their use of the DW after the PPPLF fund has been requested and decrease their use of the DW after receipt of the funds. This effect is persistent for up to three weeks, close to the median time of PPPLF processing time of 18 days. Because the treatment time is normalized to zero, the effect in $t-1$ and $t-2$ are the two days before a bank apply to the PPPLF. From the pre-treatment trend, we can see that there is a slight anticipation effect, since banks increase their usage of the DW for up to three days before requesting funds from the PPPLF. This effect could be driven by the fact that to request funding from the PPPLF, the bank must post their PPP loan as collateral. Since use of the DW is correlated with PPP borrowing, as seen in Table 2.2, the negative pre-treatment trend could be driven by banks accessing the DW on days where they also originate a large number of PPP loans. The only large difference between large and small banks is the post-treatment effect after requesting PPPLF funds. For large banks, estimates hover around a 5% increase in DW usage, while for small banks the same estimate is only around 2%.

[13] The result of the baseline estimates can be found in Figure B.2. There is not a huge difference in the post-treatment estimates, but the pre-treatment trends for large banks are slightly contaminated. This effect also appears for small banks at a smaller magnitude.

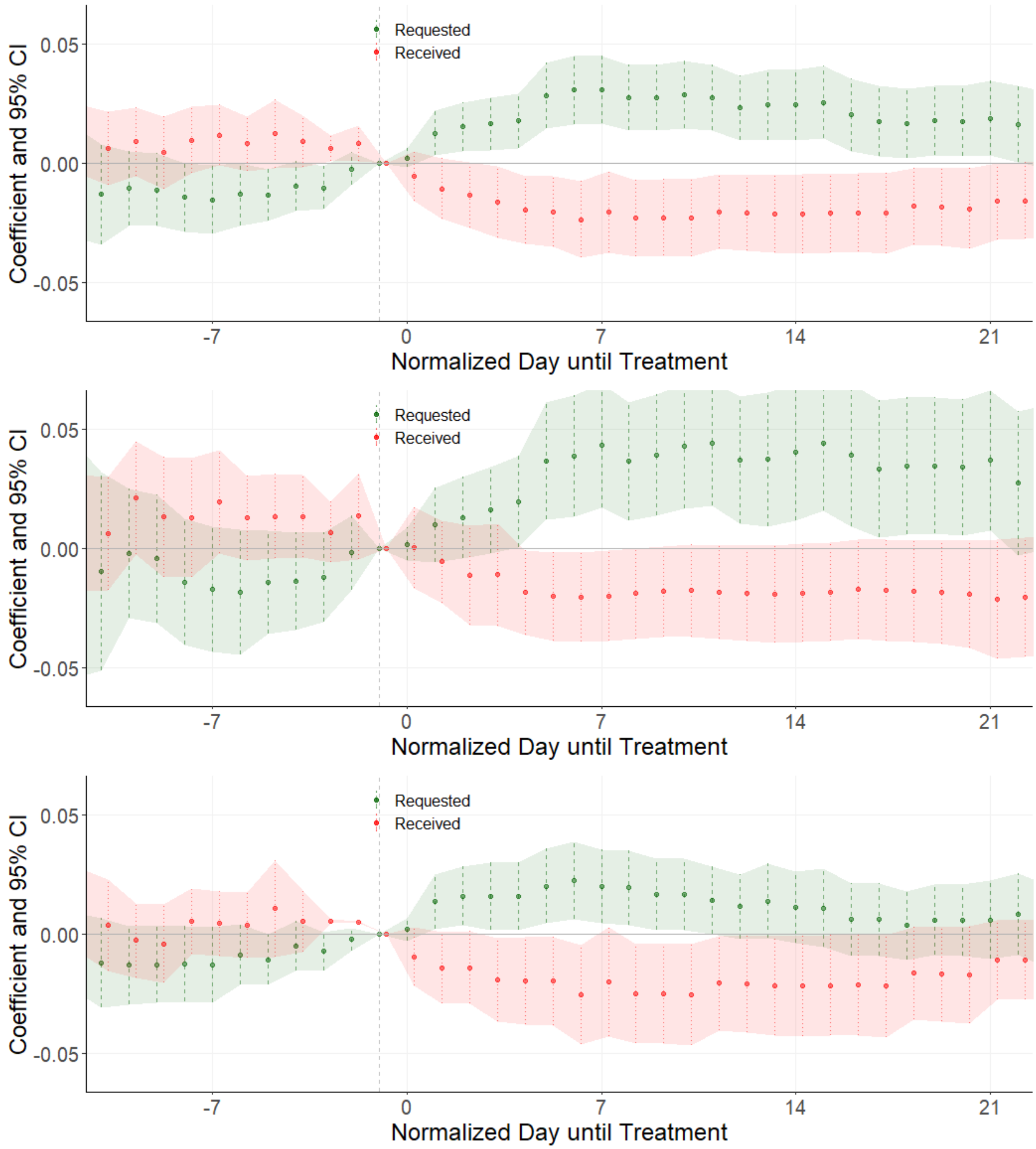


Figure 2.5: Bias-corrected event study results for the pooled sample in the top figure, large banks in the second figure, and small banks in the third figure. The timeline is normalized to when treatment has started for each individual bank. The series in green represents using the date of when banks **requested** the PPPLF money as treatment. The series in red represents using the date of when banks **received** the PPPLF advance as treatment. Displayed is the Sun and Abraham [2020] correction for staggered treatment.

2.5.3 Did DW borrowing expand PPP lending?

Using the panel data, this paper finds a strong correlation between PPP lending and DW borrowing for large liquidity-constrained banks. Additionally, both large and small banks use the DW as a stopgap measure for funds before advances from the PPPLF was received. To answer whether DW borrowing had any impact on PPP originations, I aggregate the data from April and May to look at the cross-sectional variations across banks. The bank characteristics in this data set are taken from Call Reports filed in the first quarter of 2020. All lending facility borrowing and PPP lending are the aggregates from April and May.

I consider two alternative avenues in my empirical analysis. One, on the extensive margin, does borrowing from the DW increase the number of PPP loans that a lender can originate? Two, on the intensive margin, when looking at banks that do access the DW at least once during the period, what is the relationship between the quantity borrowed at the DW and the number of PPP loans lent out? To answer these questions, I set up the following regression equation:

$$\log(\text{Number of PPP loans}) = \beta 1[\text{DW}_i] + \gamma \mathbf{X}_i + \delta_{S(i)} + \delta_{F(i)} + e_i \tag{2.3}$$

where DW is an indicator variable of whether the bank has been observed to borrow from the DW during the sample. X is a vector of bank-specific control variables from the Q1 2020 Call Report, and δ are both size decile and Fed District fixed effects. I control for size decile since there might be confounding policies that affect subgroups of banks depending on asset size, and district fixed effects for differential DW policies across districts, such as scrutiny. Furthermore, I include controls that measure a bank's alternate sources of short-term external funding, such as FHLB loans with a maturity of less than one year and total borrowing from the Federal Funds and Reverse Repo markets extracted from the Call Report data. I also control for sources of long-term funding, such as the deposit level and deposit growth from Q1 to Q2 of 2020 as well as money received from the PPPLF.

An issue that might arise in this specification is whether the correlation between DW borrowing and PPP lending still exists. From Figure 2.1, we see a high correlation in the short horizon, since DW borrowing is correlated with PPP lending at the daily level, but it might not exist in the aggregated cross-section. There are also differences in the correlation between phases 1 and 2 of the PPP program due to institutional changes. Since the first advance from the PPPLF was disbursed on April 16, banks that lent PPP loans during Phase 1 of the program did not have a ready source of long-term liquidity, which could drive them toward the DW. When looking at Phase 2 after PPPLF funds were distributed, banks may not have needed DW funds as much since there was an easier alternative without stigma. Therefore, I split the regression into three parts, looking at the aggregate of only Phase 1 quantity from April 3 to April 16, only Phase 2 from April 27 to May 31, and a pooled sample including all observations during April and May.

Table 2.4 presents the results of the naive cross-sectional regression. Column 1 regresses the log number of PPP loans on the DW indicator with only fixed effects, column 2 includes all controls for bank characteristics, and column 3 includes alternate sources of short-term funding for the pooled sample. Columns 4 and 5 report the results for large and small banks with the cutoff interval being the 75th percentile of asset size. We see a significant correlation in the pooled sample, as well as when we split the sample between large and small banks. Small banks seem to have a slightly stronger correlation with the DW usage (.217), but not significantly different than the coefficient for large banks (.178). All sources of external funding are significantly correlated in the pooled sample, signifying that external funding was an important factor in the extension of PPP loans.

Table 2.4: This table reports the OLS regression between DW borrowing and the number of PPP loans lent using data aggregated from April and May of the PPP program. Reserves are measured from Call Report data in Q1 of 2020 and are the sum of RCON0071 and RCON0081. The period used for this aggregation was April and May of 2020. Column (1) includes no controls, column (2) includes all bank-level characteristics and relevant covariates, and column (3) includes alternate sources of short-term funding, in this case, funding from FLHB and the Fed Funds/Overnight Repo Repurchase market. Column (4) uses the pooled sample with an interaction term between the bank size and the DW indicator. Column (5) is for large banks in the upper 25th percentile (∼600M Assets), and column (6) is for small banks in the bottom 75th percentile.

Dependent Variable:	Log Number of PPP Loans					
		Pooled		Interacted	Large Banks	Small Banks
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
DW Borrowing Indicator	0.261***	0.250***	0.171**		0.177*	0.218***
	(0.098)	(0.084)	(0.076)		(0.107)	(0.084)
DW Borrowing Indicator × Small Banks				0.238**		
				(0.093)		
DW Borrowing Indicator × Large Banks				0.132		
				(0.107)		
Fed Funds+ONRRP/Reserves			0.0007***	0.0007***	0.0007*	0.0005
			(0.0002)	(0.0002)	(0.0004)	(0.0003)
FLHB/Reserves			0.266***	0.262***	0.332	0.209**
			(0.082)	(0.082)	(0.605)	(0.083)
PPP LF/Reserves			0.264***	0.263***	0.481***	0.184***
			(0.034)	(0.034)	(0.059)	(0.030)
Deposit Growth		0.019***	0.017***	0.017***	0.015***	0.015***
		(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Deposit to Asset		1.91***	2.30***	2.30***	3.22***	1.87***
		(0.392)	(0.393)	(0.393)	(0.649)	(0.500)
Bank Characteristic Controls:		Yes	Yes	Yes	Yes	Yes
Fixed-effects						
Size Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Fed District	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	3,627	3,558	3,558	3,558	997	2,561
R^2	0.61964	0.70215	0.72113	0.72117	0.55989	0.58623

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

2.5.4 Instrumental Variables Analysis

A source of concern in the cross-sectional analysis is that there can exist multiple sources of omitted variables. Balance sheet costs and liquidity constraints for each bank cannot be perfectly extracted from publicly available data, so changes to those factors while the PPP program is ongoing could influence both their decision to access the DW as well as their decision to extend PPP loans. If a bank faced liquidity constraints, they could choose to simultaneously decrease the quantity of PPP lending as well as tap into the DW, biasing the estimates towards zero. Although banks cannot control the quantity of PPP loan applications they receive, they can control the quantity of PPP loans they originate. Therefore, the decision on how many loans to originate is likely positively correlated with the bank's decision to use external funding, resulting in simultaneity bias.

To resolve issues of omitted variable bias, I instrument the DW usage during the PPP program with the bank's previous exposure to the DW. To create this measure, I look at the total borrowing of each bank from the DW from the period of Q1 2010-Q1 2020 normalized by the bank's reserves from their Q1 2020 Call Report as a measure of familiarity of each bank with the Fed's DW program.[14] Since familiarity with the DW was measured before COVID, biases due to simultaneity are effectively eliminated due to the time difference. This approach shares a similarity to Anbil et al. [2021], which uses familiarity with pledging loan collaterals to the DW as an instrument for the bank's probability of using the PPPLF.

The relevancy condition for the instruments comes from the high propensity of banks to use the DW again if they have used it previously. Banks who have previously used the DW have a 9 pp greater chance of using the DW again during Q2 of 2020. This relationship is 10.2% for large banks and 5.7% for small banks. Although current DW usage is driven more by current liquidity shocks than previous usage, the strong correlation between familiarity and current usage makes the instrument strong and relevant.[15] There are two possible channels for why this relationship exists: (1) logistical friction and (2) bank-specific risk preferences. Since the DW requires Operating Circular No. 10 to be filed and collateral to be posted, banks that have previously used the window already paid the fixed cost of setting up operations

[14] An alternate version where I only use only DW data since 2018 shows a similar result of a slightly smaller magnitude. This is possibly due to a smaller number of observations and ignoring all bank familiarity with the DW before 2018. If we use data since 2018, then 24.4% of the banks have previously used the DW compared to 32.7% if we use data from 2010.

[15] The F-statistic for all specifications is larger than ten, and in most cases is in the hundreds.

and are more willing to use it again in the future. For banks that have never used the DW, there is a processing time between filing the required paperwork and borrowing, which makes it difficult for them to acquire DW liquidity quickly. Alternatively, some individual banks can prefer to use institutions rather than borrowing from the interbank market for liquidity. If this was the case, those specific banks would frequently borrow from the DW, in normal periods and during times of crisis.

The primary assumption for this instrument is that prior usage of the DW usage only affects current DW usage and not any other factors that can influence PPP lending. Although previous DW usage might affect a bank's propensity to use other sources of external funding besides the DW, I control for possible sources of short- and long-term funding, including: FHLB loans, Federal Funds (FF), Overnight Reverse Repo Agreements (ON-RRP), and PPP Lending Facility borrowing. Other sources of external funding are second order when compared to those that have been included. Another possible source of endogeneity still exists through unobserved bank-specific risk tolerance. If some banks are inherently willing to take more risks than others, then they could extend more loans and borrow from the DW to fund this extension. This would imply that the estimates we find are driven more by risky banks than usage of the DW itself, since I cannot control for bank fixed effects in the cross-sectional regression. Although this analysis holds for normal loans, PPP loans are a special case, since they carry zero weight when it comes to risk. Due to zero risk weighing on PPP loans, unobserved risk factors should be orthogonal to the amount of PPP loans that a bank chooses to extend.

Table 2.5 reports the results for the instrumented regression with heteroskedasticity-robust standard errors. The first panel runs the regression only using aggregated data from Phase 1 of the PPP program from April 3 to April 16, where the demand for PPP loans by firms greatly exceeded the supply. The second panel runs the regression on data from Phase 2 of the PPP program, from April 27 to May 31. The third panel runs the regression using the

Table 2.5: This table reports the results of how accessing the DW affects the number of PPP loans originated using a TSLS approach. The columns have the same specification as in Table 2.4. The instrument used is a measure of familiarity with the DW, measured by the total quantity that the bank has borrowed from the DW since 2010 divided by bank reserves (RCON0071 + RCON0081) from the Q1 2020 Call Report. The first panel uses only data from Phase 1 of the PPP program from April 3 to April 16. The second panel runs the regression on data from Phase 2 of the PPP program, from April 27 to May 31. The third panel runs the regression using the pooled data for Phases 1 and 2, aggregating all the borrowing and lending done in April and May of 2020.

Dependent Variable:	Log Number of PPP Loans					
		Pooled		Interacted	Large Banks	Small Banks
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Phase 1 Only						
DW Indicator	0.831***	0.934***	0.908***		0.928***	1.26
	(0.263)	(0.292)	(0.291)		(0.309)	(0.855)
DW Indicator × Small Banks				1.48*		
				(0.807)		
DW Indicator × Large Banks				0.734**		
				(0.308)		
Observations	3,486	3,419	3,419	3,419	977	2,442
Phase 2 Only						
DW Indicator	0.096	0.392	0.440*		0.691**	0.069
	(0.267)	(0.267)	(0.261)		(0.343)	(0.489)
DW Indicator × Small Banks				0.448		
				(0.466)		
DW Indicator × Large Banks				0.437		
				(0.298)		
PPP LF/Reserves			0.297***	0.297***	0.520***	0.221***
			(0.036)	(0.036)	(0.061)	(0.032)
Observations	3,621	3,552	3,552	3,552	996	2,556
Phase 1 and 2						
DW Indicator	0.345	0.535**	0.548**		0.688**	0.380
	(0.240)	(0.255)	(0.249)		(0.282)	(0.598)
DW Indicator × Small Banks				0.624		
				(0.556)		
DW Indicator × Large Banks				0.521**		
				(0.258)		
PPP LF/Reserves			0.258***	0.257***	0.475***	0.181***
			(0.034)	(0.035)	(0.059)	(0.032)
Observations	3,627	3,558	3,558	3,558	997	2,561
Bank Characteristic Controls:		Yes	Yes	Yes	Yes	Yes
Fixed-effects						
Size Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Fed District	Yes	Yes	Yes	Yes	Yes	Yes

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

pooled data for Phases 1 and 2, aggregating all borrowing and lending done in April and May of 2020. Column 1 includes only fixed effects for size deciles and districts, column 2 includes controls for bank characteristics, and column 3 includes controls for alternate sources of external funding for the pooled sample of banks. Column 4 shows the interacted term in the pooled sample, column 5 displays the results for large banks in the upper 25th percentile, and column 6 has results for small banks. Since the interacted specification has two endogenous variables, I also interact the instrument with the size class to be just-identified. While this method should still satisfy the exclusion restriction, the preferred specifications are the results from the subgroup analysis.

Looking at the first panel, we can see that the effect of DW borrowing is strong and significant for the pooled sample and large banks, but not for small banks due to large standard errors. Standard errors are better in the pooled interacted sample due to a higher number of observations, in which case the effects for small banks are significant at the 10% level. If we compare differences between columns 2 and 3 in both Table 2.4 and Table 2.5, we do not see as significant changes in the coefficients of the pooled sample in the instrumented regression. One explanation for this behavior is that variations in the treatment variable induced by the instrument have very little correlation with other sources of funding, showing that levels of previous DW usage do not significantly affect a bank's choice to tap into other sources of funding. This should be true because previous familiarity with the DW does not imply familiarity with interbank transactions.

Regarding the interpretation of the coefficients, large banks that accessed the DW in Phase 1 of the PPP program extended 92.8% more PPP loans than their counterparts, 69.1% when looking only at the early stages of Phase 2, and 68.8% overall during April and May. This effect is economically significant given that the median number of PPP loans lent out by large banks was $\sim$820 during April and May, so a 69% increase corresponds to 565 loans per bank. The effect is stronger in Phase 1, before a dedicated source of long-term funding from the PPPLF was available, but is still significant during Phase 2. This could have happened because banks substituted away from the DW and borrowed more extensively from the PPPLF. Since the PPPLF provided long-term funding that matches the maturity of the PPP loans, banks would prefer that over overnight loans from the DW. When looking at the interacted column, the results for large banks are weaker than when performing a subgroup analysis and stronger for small banks. One possible reason is that subgroup regression implicitly forces the controls to also interact with bank size, allowing for more fine-tuned controls of bank characteristics. Since the effect of bank liquidity is most likely different amongst bank sizes, the subgroup regression is the preferred specification.

This result is robust to alternate levels of the cutoff criteria for large banks, shown in Table B.4. The regression includes only lending in Phase 1, and columns 1-4 represent the 75th, 80th, 90th, and 95th percentile cutoff criteria for large banks. Although column 4 has only 158 observations, we still see strong and significant effects of DW access on the quantity of PPP lending, giving reliability to the estimation.

I also consider using an alternative instrument: the number of times the bank has borrowed from the DW pre-COVID. Since I measure the information channel of DW familiarity for the instrument, a bank that borrows a large amount once might have less information than another bank that borrows small quantities frequently, even if the total amount is the same. Table B.5 displays results from using the alternative instrument. I find that estimates for large banks are slightly more conservative, and estimates for small banks are twice as large and gain significance at the 10% level. In Panel 3, I include both instruments to perform the Sargan over-identification test, which fails to reject the null hypothesis that both instruments are exogenous.

Finally, I examine the intensive margin between the reserve-adjusted quantity borrowed from the DW and the log quantity of PPP loans. I use Equation 2.3, substituting the DW indicator variable for a reserve-adjusted quantity. In this sample, I include only banks that have used

Table 2.6: This table reports the results of how DW borrowing as a share of reserves affects the number of PPP loans originated using a TSLS approach. The columns are the same specifications as in Table 2.5, but only include the banks that have used the DW at least once. The first panel runs the regression only using aggregated data from Phase 1 of the PPP program. The second panel runs the regression using data from Phase 2 of the program, from April 27 to May 31. The third panel runs the regression using the aggregated data for April and May of 2020.

Dependent Variable:	Log Number of PPP Loans					
		Pooled		Interacted	Large Banks	Small Banks
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Phase 1 Only						
DW Borr/Res	0.022	0.060**	0.057**		0.078***	-0.003
	(0.025)	(0.026)	(0.026)		(0.026)	(0.036)
DW Borr/Res × Small Banks				0.011		
				(0.033)		
DW Borr/Res × Large Banks				0.068**		
				(0.030)		
Observations	149	145	145	145	89	56
Phase 2 Only						
DW Borr/Res	0.005	0.011	0.057		0.570	-0.037
	(0.017)	(0.036)	(0.057)		(4.63)	(0.056)
DW Borr/Res × Small Banks				0.043		
				(0.071)		
DW Borr/Res × Large Banks				0.194		
				(0.426)		
PPP LF/Reserves			0.402***	0.438**	0.410	0.191
			(0.094)	(0.208)	(1.45)	(0.155)
Observations	140	137	137	137	72	65
Phase 1 and 2						
DW Borr/Res	0.005	0.014	0.021*		0.037**	0.002
	(0.008)	(0.011)	(0.011)		(0.017)	(0.006)
DW Borr/Res × Small Banks				0.013**		
				(0.006)		
DW Borr/Res × Large Banks				0.027*		
				(0.016)		
PPP LF/Reserves			0.309***	0.313***	0.576***	0.126**
			(0.094)	(0.095)	(0.110)	(0.054)
Observations	223	218	218	218	121	97
Bank Characteristic Controls:		Yes	Yes	Yes	Yes	Yes
Fixed-effects						
Size Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Fed District	Yes	Yes	Yes	Yes	Yes	Yes

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

the DW at least once during the sample period, which drops ∼90-95% of the observations since most banks did not access the DW. Table 2.6 reports the results of the instrumented regression, with the first row of each panel being the most important. The specifications of the columns are the same as in Table 2.5, and the panels refer to the same phase aggregation method. The results of columns 4 and 5 of the first panel show that the quantity of DW borrowing only affected large banks. An increase in DW borrowing as a share of reserves by 10 percentage points increased the quantity of PPP loans extended by .78%. Although this effect may seem small, the reserve-adjusted DW borrowing series is an aggregation of all borrowing done in Phase 1, with a mean of 3.23 and a standard deviation of 5.58 when looking at banks that have used the DW at least once. Therefore, a one standard deviation increase in DW borrowing increased the quantity of PPP loans lent by 43.6%. This effect is not large or significant for small banks and has no effect at the intensive margin during the early stages of Phase 2. It is important to note that the first panel of column 4 only has 89 observations, but the effect still holds and is significant at the 0.01 level, further giving reliability to the estimation.

2.6 Discussion

What do these results imply when it comes to the implementation of fiscal policy? For one, the implementation of monetary and fiscal policy through the financial sector requires enough liquidity to facilitate a smooth transfer of credit to businesses and households. In that sense, the fiscal authority should have coordinated with the Fed and ensured that the PPPLF was set up before the roll-out of the PPP program, instead of implementing a liquidity facility ex-post. Due to this delay, banks were forced to go to the DW for their liquidity needs, which could have negatively impacted lending for banks that were DW-averse. There could have also existed a source of positive externality, since if more banks were forced to borrow from the DW, the 'stigma' of DW borrowing could be reduced due to observations of borrowing being less informative about the asset quality of the borrower. This asset quality mechanism has been explored in Ennis and Weinberg [2013], where observation of borrowing from the window is observed as a negative signal, therefore agents would pay a higher price in the interbank market to avoid revealing information.

Another way the PPP program could have been implemented better is if the processing time of the PPPLF was reduced. On average, the delay between when banks requested an advance from the PPPLF and when they received the funds was around three weeks, which could have negatively impacted the speed of loan distribution. Banks that are inherently averse to borrowing from the DW might have waited for funds from the PPPLF to come in before making any lending decision, subsequently taking longer than necessary to extend credit. From Granja et al. [2020], we know that firms that receive PPP funds earlier have better employment outcomes than firms that receive funds later in the program.[16] A faster processing time from the PPPLF could have improved employment outcomes and reduced the banking sector's dependence on DW lending.

The PPPLF was established to extend funding to banks for a period of two to five years, while loans made through the DW had a maximum maturity of 90 days. The length of funding, as well as the implicit cost of the DW, made these institutions naturally serve different purposes for banks. Since banks prefer to match maturities, the PPPLF seems like the optimal lending facility while the DW remains a stopgap measure of temporary liquidity. The Fed could have folded the functions of the PPPLF into the DW, extending DW credit to longer maturities and allowing PPP collateral to be posted to the window at face value. This would have further reduced the cost of accessing the DW and encouraged banks to further borrow from the window in future crises.

[16] Employment outcomes are measured by the number of hours worked and the number of employees employed. This effect also persisted until August, so it was relatively persistent.

The empirical analysis also points to drastic differences between the behaviors of large and small banks. On both the extensive and intensive margins, DW borrowing did not affect small bank lending, while the PPPLF has been shown to have a greater effect on small banks compared to large banks by Anbil et al. [2021] and Lopez and Spiegel [2021]. One potential reason is that since the PPPLF was an emergency measure, regulations were less strict on the type of banks that could access them. Another possibility could be that small banks face greater stigma when borrowing from the DW and are more reluctant to use it as a source of liquidity. If the Fed wants to encourage usage of the DW amongst small banks, it might be a good idea to make it more accessible for those sets of customers by reducing either the explicit or implicit cost.

Finally, consider the current banking liquidity crisis due to high interest rates. The fall in bond prices count as an unrealized loss in the bank's balance sheet, and deposit withdrawals cannot be sufficiently covered by liquidating bonds. If banks were more willing to access the DW during this time, there would be a decreased probability of bank failures. Therefore, expanding access to the DW either by reducing the associated stigma or targeted outreach could improve the financial stability of the banking sector.

2.7 Conclusion

The discount window is an important tool in the Federal Reserve's arsenal to ensure the continued stability of the financial sector. To that extent, this paper analyzes the relationship between a bank's choice to use the DW and the impact of an exogenous liquidity shock, proxied by the quantity of PPP loans. Primarily, both large and small banks used the DW as a measure of temporary liquidity before a long-term source of funding was available. Using prior familiarity with the DW as an exogenous instrument, I find a causal relationship between DW usage and PPP lending at both the intensive and extensive margins. Large banks were the main users of the DW, and those that used the window during Phase 1 of the PPP program extended 92.8% more PPP loans than their counterparts. This effect decreases to 68.9% after extending the sample to the end of May 2020, when the demand for PPP loans from firms decreased and PPPLF funding was available. At the intensive margin, a one standard deviation increase in reserve-adjusted DW borrowing increased PPP lending by 43.6% during Phase 1 of the program for large banks, but has no significant effect for small banks.

Results from this study suggest that multiple sources of central bank lending might play a complementary role in supporting fiscal policy when implemented through the financial sector. The DW provided medium-term liquidity for banks for up to three months, while the PPPLF provided long-term liquidity for banks from two to five years. The DW and the PPPLF also appeared to serve different subsets of the financial sector, with the DW having a greater effect on larger banks, while the PPPLF has a greater effect on small banks.

Chapter 3

The Role of Public Lending as an Outside Option in Private Markets

Abstract

Does federal aid improve local recovery efforts after a natural disaster? We estimate the impact of FEMA aid on economic recovery at the county level in the United States for all disasters from 2000-2021. Using an instrumental variables approach, we find that a 1% increase in aid results in a 0.008% increase in local GDP, corresponding to a fiscal multiplier of 1.45. Furthermore, we also examine whether external aid could change any behaviors in the financial markets. We find that the occurrence of disasters impacts the market concentration and deposit distribution in affected regions, but the provision of aid has no significant effects on financial market metrics. These findings suggest that federal disaster aid significantly contributes to economic recovery without distorting local financial markets.

3.1 Introduction

Natural disasters cause $\tilde{1}2$ billion dollars worth of damage to the United States every year. To aid impacted parties in recovering from these disaster, the Federal Emergency Management Agency (FEMA) provides access to grants as well as low-interest rate loans allocated by the Small Business Administration (SBA). These infusions are integral to the revival of the affected economies, since 23.4% of the recorded yearly damages are compensated for.[1] Notably, although the economic toll of natural disasters constitutes less than 0.1% of the United States' annual Gross Domestic Product (GDP), one in twenty events has a local GDP impact of 2% or more within affected jurisdictions.[2] Climate change has also exacerbated the effects of disasters, with damages increasing at around 5.6% per year since 2000 and real disaster costs growing faster than GDP (Deryugina [2017]).

As the impact of natural disasters rises, it is important to examine the efficacy of current federal programs in aiding post-disaster recovery efforts. This paper explores to what extent federal aid could help counties recover after the occurrence of a natural disaster. In particular, we explore how Individual Assistance (IA) grants by FEMA and low-interest rate loans by the SBA changes local output one year post-disaster. Using the results of the estimations, we provide the first county-level estimates of the fiscal multiplier through the aid channel. We supplement previous research on the efficacy of FEMA intervention by showing that the program improves county-level outcomes without negatively impacting financial institutions that already conducts private lending.

There are reasons to believe that external intervention could help recovery as well as influence local private markets. Davlasheridze and Geylani [2017] shows that small businesses have a greater chance of survival when a local region is provided with disaster loans. Since the return behaviors of individuals and businesses are mutually reinforcing, it is important to supplement private industries so that funds can be optimally distributed to maximize economic growth. On the other hand, intervention could also cause many problems. Under the expectation that an external player could intervene after the occurrence of a negative shock, businesses might not want to operate lending programs in the region. Evidence from Kousky et al. [2018] shows that individuals decrease insurance purchases if they were given a

[1]SBA gives on average 372 million per year from 2000 to 2021. FEMA gives an additional $61.7 million through grants to households. The average yearly requested amount through applications averages $996 million dollars. All data is obtained publicly from the SBA Disaster Loan Data found here: SBA Disaster Loan Data.

[2]2% is chosen as the cutoff point since it is the benchmark for a recession set out by the IMF. When looking at severe recession, defined as a 5% impact to local GDP, this figure accounts for 1.8% of all counties.

disaster relief loan. This brings up the question if the banking sector is any way affected by governmental intervention since the provision of external low-interest rate loans could reduce loan demand for local community banks. Given that Duqi et al. [2021] provides evidence of faster recovery in less competitive banking markets through increased real estate credit supply, we are also interested in whether federal aid impacts the banking landscape in the affected area.

To answer these questions, we compile a robust dataset using data from FEMA and SHELDUS to construct the universe of disaster occurrences in the United States from 2000-2021. Our dataset includes 1524 disasters affecting 870 counties across the United States aggregated at the county-year level. We compile county-level descriptors such as population, GDP, unemployment, bank concentration from the Bureau of Economic Analysis (BEA), disaster loan data from the SBA, and IA grants from FEMA. This lets us examine the quantity of grant applications and approvals, as well as total approved amounts, damages, and outcomes for each county within the time period. We merge the disaster and county data with banking data obtained from the Summary of Deposits and Community Bank Data obtained from the Federal Deposit Insurance Committee (FDIC) which allows us to look at the changes in distribution of deposit shares over time.

Our analysis also needs to account for potential confounding factors that could affect both GDP and loan approvals. Since the decision made by FEMA to grant aid is accomplished through an internal selection criteria, this leads to a selection issue where individuals affected by a natural disaster might not qualify for aid (non-compliance). Evidence of this comes from Garrett and Sobel [2003], who finds that the decision to extend disaster relief is politically motivated. Swing states and those with representation on the FEMA committee receive more aid than their counterparts. Furthermore, endogeneity could arise if there are variations in insurance coverage across counties, which could potentially affect the quantity of aid that FEMA chooses to grant.

Due to these issues, we estimate both a fixed effects model as well as using an instrumental variables (IV) approach. Under both models, we control for county and time fixed effects as is standard in TWFE models, as well as disaster and state-by-year fixed effects. The inclusion of disaster fixed effect directly compares counties affected by the same natural disaster in the same region, and state-by-year fixed effects accounts for changes in state-level aid policy that could affect all counties. In our IV analysis, we borrow from the medical literature and use an intent-to-treat type instrument. While the quantity of aid granted to a county is subject to selection bias, the quantity of applications sent in by individuals and firms within a county can be seen as perfectly exogenous. Since disasters, in a localized area, cause damage at random, the number of individuals requesting support only affects the aggregate GDP through its effect on how much aid is sent in by FEMA. Using these insights, our first stage regresses the quantity of aid received by a county with how many aid applications are sent in through individuals living in that county for a particular disaster. We use the same methodology to determine the effect of an aid shock on the changes in deposit shares between community banks and larger national banks.

Under the fixed-effects model, aid quantity and GDP recovery appear to be statistically insignificant. This appears since the selection criteria by FEMA allocates the most aid towards the counties most damaged by natural disasters and therefore has the slowest recovery. When looking at the IV specifications however, federal aid has a significant effect on GDP, translating to a fiscal multiplier of 1.45. While this is on the lower end of previous estimates, one limitation of a panel estimation is that long-run effects cannot be accounted for due to carry-over bias, making our coefficients lower-bound estimates. Robustness checks separating applications from the public and private sector as instruments and using public and private output as the dependent variable confirms the significance, with multiplier values ranging from 1.11-3.00. Different measures of county GDP show varying effects, with aid having a larger effect on economic recovery when given to households and firms. Overall, federal aid significantly improves local county recovery, with multiplier values varying depending on the specification.

When looking at the financial sector, natural disasters tend to increase concentration in the banking market and lead to an increase in the share of deposits held at community banks. This shift in HHI is primarily due to deposits moving towards larger banks, possibly explained by an increased risk in defaults for smaller banks more exposed to the disaster. However, when considering the intensive margin, there are no statistically significant changes to deposit behaviors, indicating that shifts in deposit shares are driven by the occurrence of the disaster rather than its intensity. Aggregate deposits on average increases in metro areas and experience a decrease in rural areas. This is likely due to aid provisions favoring larger counties even when controlling for population and damages. In rural areas, withdrawals from large banks are often reinvested into community banks, increasing the composition of deposits held by community banks. This is due to the fact that community banks are more responsive to a disaster from higher exposure, and therefore change deposit and loan rates more than their national counterparts (Barth et al. [2022]). Conversely, metro areas show insignificant changes in deposit composition, indicating a relatively even increase in deposits across both community and large banks. Throughout all specifications, the provision of aid does not influence any financial market markers both at the internal and external margin.

Projecting from these estimates, every dollar put towards increasing FEMA aid could increase tax revenue by at least 40 cents. Policy-makers should consider both the multiplier effect and the effect that aid has on future tax revenue when designing future aid packages. While the provision of aid might increase the rate of under-insured parties in the affected regions due to moral hazard, we find no evidence that aid has any significant effect on the banking sector. This lack of significance should alleviate concerns of federal aid distorting local financial markets.

This paper contributes to three major avenues of research. First, we estimate the impact of policy interventions on the recovery of local provinces following a natural disaster. Recently, Watson [2021] explores the role of SBA loans on small business recovery after Hurricane Ike. They find that businesses approved for a loan had 2.8-3.9 times higher likelihood of survival than businesses that do not receive disaster relief loans. Davlasheridze and Geylani [2017] finds that for every additional dollar per business spent on disaster loans, four small businesses survive. Generally, literature in this field only looks at firm and household-level (Billings et al. [2019]) outcomes without considering the effect on the aggregate economy of the affected county. Our work expands on these results by generalizing this finding to whole economies and considers loans not only to firms but also to individuals. To this extent, we improve the external validity of previous papers and show that results are still beneficial even at a more aggregated scale.

Second, we determine whether there are any negative externalities when implementing fiscal aid policy. Kousky et al. [2018] finds that the provision of FEMA grants and low-interest SBA loans decreases the quantity of flood insurance purchased by individuals in the following year by around $5,000. This shows that intervention by governmental parties could significantly crowd out private markets. There is also evidence that banks heterogeneously change their behavior after the occurrence of a natural disaster as shown by Barth et al. [2022]. They find that after exposure to a natural disaster, community banks drive up interest rates on loan more than deposit rates to take advantage of the increased demand for loans. Since these results are mainly driven by community banks due to their higher geographical exposure, provision of low-interest rate SBA loans could reduce loan demand and change the competitive scene of the banking market. Research by Davlasheridze et al. [2017] and Kousky et al. [2018] focuses mainly on the impact of disaster aid on property damage and insurance markets, this study adds to that field by showing that there are no negative impacts to the financial market.

Third, we use a novel way to estimate the fiscal multiplier of the federal government for its domestic programs. Papers in this field uses military shocks as an exogenous shock applied to a VAR model to estimate the effect of changes in government spending on aggregate output. Using a VAR model, Giordano et al. [2007] finds a cumulative multiplier of 1.7-2.4 using Italian monetary data. Panel IVs have also been used by Almunia et al. [2009] to look at the impact of defense spending of 27 countries in the period 1925-1939 and finds a multiplier of 1.1-2.2. Nakamura and Steinsson [2014] finds a multiplier of 1.5 for US states using the panel IV approach. Yang et al. [2012] is the first attempt to use natural disaster data to estimate fiscal multiplier at the state level and suggests a multiplier of 1.4-2.5. We contribute to this literature by further localizing the area of impact and considering multipliers at the county-level (small economy). While this analysis cannot recover the long-term impact as compared to an impulse response function, most studies agree that fiscal policy generally stimulates output only in the short run. It is also important to only look at short-run results of natural disaster, since affected areas are subject to repeated shocks, which could contaminate the long-run analysis.

3.2 Data

The main data for this paper comes from two sources, SHELDUS and FEMA. SHELDUS is a county-level hazard data set for the U.S. and covers natural hazards such thunderstorms, hurricanes, floods, wildfires, and tornadoes. The database contains information on the date of an event, affected location (county and state) and the direct losses caused by the event (property and crop losses, injuries, and fatalities) from 1960 to present.[3] OpenFEMA provides information on which natural disaster was approved or denied for government aid, as well as the total approved amount of aid. FEMA identifies damages based on the total verified loss, which goes through an application process initiated by the affected party and must be approved by a FEMA employee. These datasets contains unique identifiers to link to the county, such as the county-level FIPS code as well as the disaster identifier. This allows us to link each county to a particular disaster that happened in a given year and observe the associated outcomes. One limitation of this dataset is that both sources of reporting might not be fully accurate.

[3]SHELDUS obtains its data from the "Storm Data and Unusual Weather Phenomena" by the National Climatic Data Center (NCDC) and the National Centers for Environmental Information (NCEI). The NCDC and NCEI obtains their data from local NWS offices, the media, law enforcement, other government agencies, private companies, and individuals.

It is necessary to merge both the FEMA and SHELDUS databases to achieve the universe of United States disaster data. FEMA only includes declared disaster zones, which is acquired through an approval process and is not given out to all applicants. One benefit of being a declared disaster is that people and businesses within the affected area are eligible for grants as well as access to low-interest rate loans through the SBA outside of what insurance might cover. The data on SBA loans can be found on their website, and usually compensates for a further 11% of the total damages caused by the disaster to that area.[4] Aside from the provision of low-interest rate loans, individuals that reside in disaster areas also receive mortgage and housing assistance programs, which might not be available in non-declared areas. This differential in treatments conditional on a county being a declared disaster area allows us to examine the economic outcomes between two counties affected by the same natural disaster. Currently, our dataset only includes low-interest SBA loans along with FEMA grants given to individuals, as well as the number of applications requested and approved.

Data on county outcomes are obtained from the BEA website and includes data on households, businesses, population, unemployment, and production GDP. I merge these three datasets at the county-year level to conduct the main analysis for this paper. Provision of low-interest rate loans could also have an adverse effect on commercial lending. The introduction of a large 'firm' entering the market offering a product at low prices could crowd out already established financial firms in the local region. Therefore, I combine information from the yearly Summary of Deposits, which gives branch-level information about each bank and the amount of reserves held at the branch. I use this measure to calculate the Herfindahl–Hirschman index (HHI) at the county-level using deposit shares, which is a generally used measure of market concentration. One limitation of this way of construction is that customers may use banks outside of their county, so HHI measured at the county-level might not fully capture the competition scene in the local region. To supplement the banking data, we also obtain banking data published by the FDIC that establishes which banks are considered community banks at the yearly level.

One of the main issues with analyzing disaster data is the repeated occurrence of disasters. In the United States, there are certain zones that are more susceptible to encountering hurricanes and tornadoes than other. For example, areas along the Gulf Coast, the Atlantic

[4]SBA does not approve all damages. Generally, the individual requesting the loan needs to send in an application to request compensation for damages, which could be smaller than the actual loss recorded on SHELDUS. This could happen either because their insurance has covered a portion of the damages already, or if they do not need any further debt assistance. On average, the SBA provide loans amount that reaches 49% of their inspected loss, with a median of 44%.

Coast, and the Mississippi River are particularly prone to flooding. The West Coast and the central United States along the New Madrid fault line are at higher risk of earthquakes. Finally, Florida and the Carolinas, along with other counties near the Atlantic coast are often affected by hurricanes. This makes it important to compare counties with neighboring counties within the same zone, since the policy response to a natural disaster occurring could be zone-dependent. Since natural disasters occur repeatedly within these regions, estimating long-run effects of federal aid could be difficult since it is subject to contamination and carry-over bias. To resolve this issue, we restrict our analysis to only looking at the one-year-ahead GDP measure from the date of the disaster.

Figure 3.1 shows the severity of the disasters over the years, with 2012 being the hardest hitting at $51 billion dollars worth of damages, mostly made up of Hurricane Sandy. As we can see from the figure, only a small proportion of all disaster damages were reported to and covered by the SBA.[5] Insurance did not pick up the rest of the coverage either; a recent report by Dixon et al. [2020] shows that local governments are under-insured on average and only receives 28% of repair costs. This allows ample room for external funding, through low-interest loans provided by the SBA, to help with local recovery efforts.

In total, our dataset spans from 2000-2021 and includes 1524 disasters being considered spanning 870 counties. Out of the 1524 disasters in our dataset, 315 of them had at least one county that was given aid and one county not given aid to serve as the control group. On average, the mean number of counties affected by each natural disaster is 4.3 (median = 2), with an mean of 1.1 counties given aid by the SBA (median = 0) and 3.3 counties not given any form of aid (median = 1). The total distribution of natural disasters and the amount of counties that it affected can be seen on Figure 3.2.

In Figure 3.3, we plot the distribution of natural disaster damages at the time of the disaster for our aided and unaided counties. In Panel A, we can see that on average, the impact of the disaster across counties that receive federal aid are generally larger than those that do not receive aid. However, after normalizing it to the county's private production that year, the counties that were not given aid were more impacted by the disaster relative to their counterparts. This implies that there is a selection mechanism by the FEMA organization that might prefer larger counties than smaller ones.[6] Since FEMA gives aid based on their internal criteria of which counties are most in need, these large counties could also be less

[5] As a share of the total recorded damages, only 8.2% was reported to the SBA and only 3% were covered.

[6] On their website, FEMA notes that part of the selection criteria depends on the citizenship status, household income and dependents, other insurance payouts, as well as occupancy and identity verification.

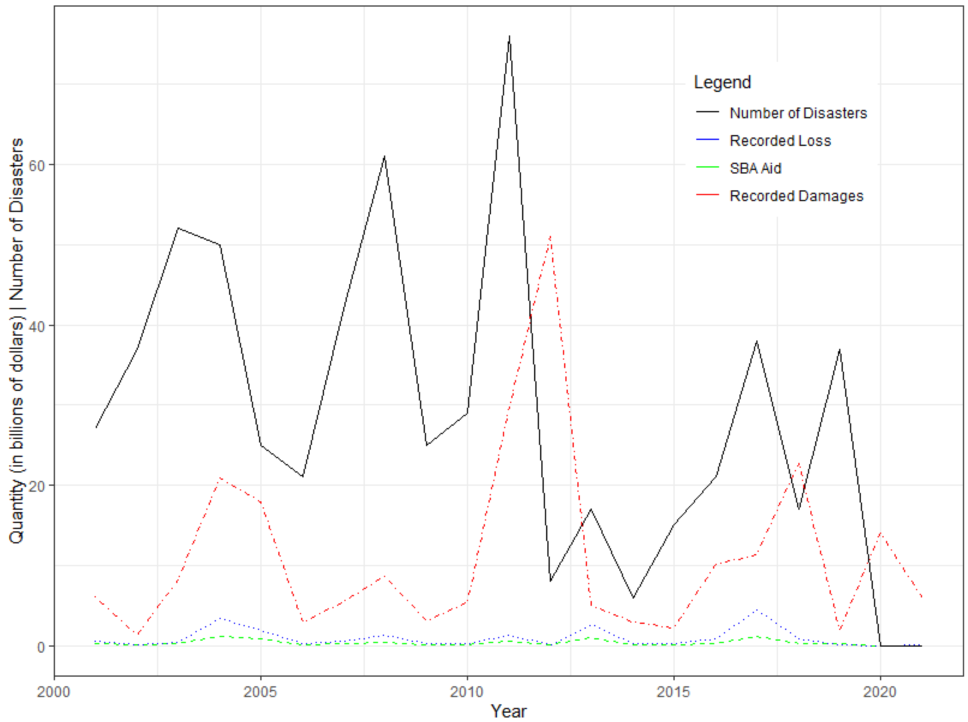


Figure 3.1: Graph of the number of natural disasters each year (black, with bar on the right) along with aggregate information such as recorded damages (red, mean=\$11.2 billion), SBA measured loss (blue, mean = \$929 million), and loans given by the SBA (green, \$336 million). The major spike in damages is due to the 2012 Hurricane Sandy, which caused the most property damage at \$21.6 billion dollars.

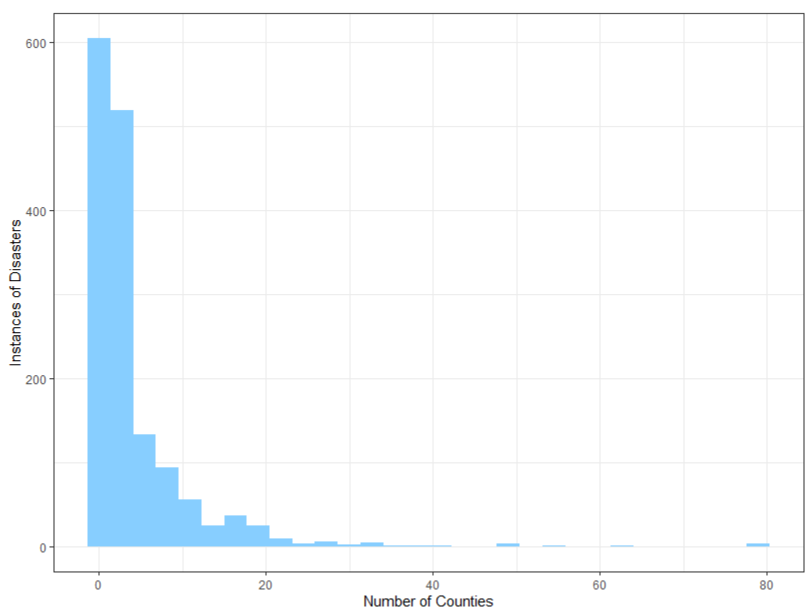
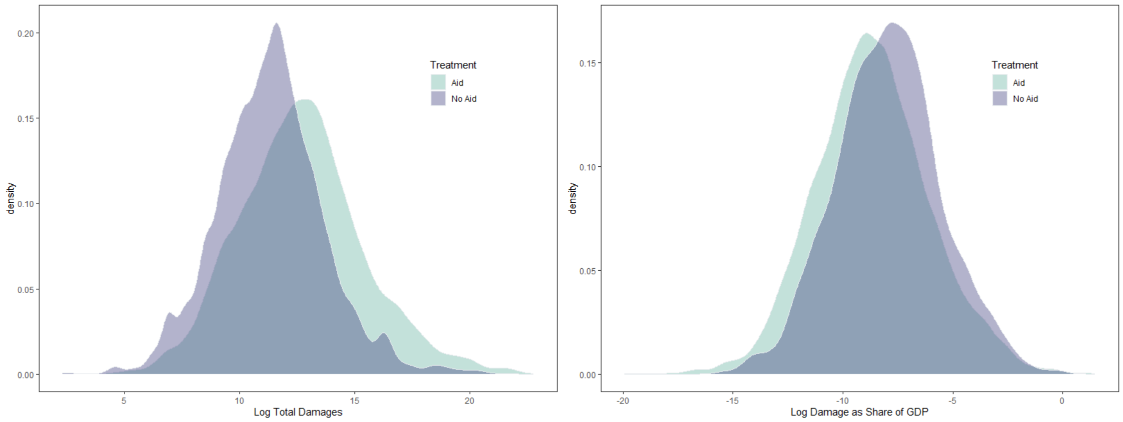


Figure 3.2: Histogram of number of counties affected by each natural disaster. Each observation is one natural disaster, with the x-axis denoting the number of counties that were affected by that natural disaster.



(a) **(b)**

Figure 3.3: Distribution of natural disaster damages at the time of the disaster occurring. Panel **(a)** plots the log-transformed total damages reported by SHELDUS to a county affected by the natural disaster. The light blue distribution represents all the counties that were given aid, and the light purple represents counties that were not given aid. Panel **(b)** denotes the distributions after they have been normalized to the particular county's GDP at the time of the disaster.

prepared to face the consequences of a large natural disaster than their small counterparts.

3.3 Experimental Design

Our first specification is the following fixed effects model, with c indexing county, t indexing years, s indexing state, and d indexing the disaster identifier:

$$\log{(GDP)}_{c,t+1} = \beta \log Aid_{c,t} + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \sigma_{s,t} + \rho_{d,t} + \epsilon_{c,s,d,t} \tag{3.1}$$

Where $\log{(Aid)}_{c,t}$ represents the log transformed amount of aid the county received at time t, $\mathbf{X}_{c,t}$ is a vector of time varying county-level control variables including population, number of jobs, unemployment, disaster damages, and other factors of interest. We control for the three most important aspects that could determine I include fixed effects each for the county, year, and disaster number to make a comparison between counties affected by each natural disaster. The inclusion of time-varying disaster fixed-effect allows us to compare the outcomes of counties affected by the same natural disaster in the same year, with the only variation being that one group was given grants and low-interest loans. I also include fixed effect for state-by-year to account for changes in state-level policies that could affect disaster recovery. From the 130 disaster-groups, only 11 of them affected counties across multiple states. Therefore, the estimated coefficient β represents the average effect of an infusion of aid as a share of the county's GDP in comparison to counties affected by the same natural disaster but did not receive any aid.[7]

Under this specification, a positive sign on β implies that counties that received a fiscal stimulus on average increase their GDP faster than counties that did not receive the same assistance. Using this estimation, we can also calculate the fiscal multiplier to see how effective a dollar of federal spending is in helping individual counties. Due to the occurrence of repeated disasters, we restrict our estimation to the effect on one-year-ahead GDP instead of anything greater than one. Repeated disasters can be be seen as unexpected shocks that would compound the estimation the further the dependent variable is away from the current time, leading to biased results. We cluster our standard errors at the state level, to account for correlated errors across counties within the same state. To account for zero values in our dataset, we also add one to all variables as a standard transformation following MaCurdy and Pencavel [1986].

Under the fixed effect model, selection bias might exists if there are confounding factors that affect FEMA's decision to lend aid to particular counties. Since there exists internal criteria that FEMA uses to decide who should receive aid, this unobserved confounding variable would bias our estimates in the FE model. FEMA might also give preference to the counties that are most under-insured against disasters, which could potentially lead the fixed effect model to show a negative relationship between GDP and the amount of aid given. To account for this source of correlated omitted variables, we also estimate the impact of federal aid using an instrumental variables approach.

Our identification strategy utilizes the intent-to-treat (ITT) framework usually found in the medical literature.[8] Under the ITT framework, patients in the treatment group can be non-compliant, leading to selection issues when only examining outcomes of patients taking the medicine. One way to resolve this selection issue is by using a instrumental variables approach where the assignment of treatment is used as the instrument to predict compliance. Since the assignment of treatment is perfectly random and is highly correlated with the compliance probability, it satisfies the exogeneity and relevancy condition. The estimated parameter in the second stage would then correspond to the local average treatment effect comparing the differences in outcome between compliers.

[7] In all specifications, we use GDP measured by private industries instead of the total GDP of the county since we are estimating private aid instead of public aid. The estimations using both private and public GDP can be seen in the Appendix.

[8] A primer on ITT analyses is authored by McCoy [2017].

Under our model, we assume that the localized areas damaged by a natural disaster is perfectly randomized. The incident when a disaster affects the firm or household could be seen as treatment assignment, causing them to apply for federal relief. Non-compliance (selection) arises if FEMA denies aid to applications since their actions would then be non-compliant with the assignment. By using the number of applications made by individuals within a county in a given year, we can recover an unbiased estimate of the effect of federal relief on GDP recovery. The first stage equation would then take on the form of:

$$\log(Aid)_{c,t} = \log(Applications)_{c,t} + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \sigma_{s,t} + \rho_{d,t} + \epsilon_{c,s,d,t} \tag{3.2}$$

Where $\log(Applications)_{c,t}$ is the exogenous instrument measuring the number of aid applications sent by both public and private entities from county c in year t. The ITT methodology is employed to address potential selection bias in the distribution of FEMA aid. By using the number of aid applications as an instrument, we ensure that our estimates are not biased by non-random aid allocation, as discussed by Garrett and Sobel [2003].

Our key identification assumption is that, conditional on the other control variables, the number of applications only effect the recovery of GDP through its effect on how much it can increase federal funding. Since we control for the damage to the county caused by the disaster, this identification assumption should be valid. To have the number of applications be truly exogenous, we also assume that uptake on disaster insurance is truly random across counties. Under our IV specification, the estimate β recovers the average treatment effect for counties that had a positive number of aid applications.[9]

Our baseline specification is the IV analysis with standard errors clustered by disasters. Since FEMA takes into account each individual disaster when they make their relief decision, it is likely that errors are correlated across counties affected by the same disaster. Our analysis are also robust to cases where we use public as well as private GDP measures, as well as using either public or private aid applications as instrument instead of the pooled applications.[10]

We also use the same empirical methodology to determine the effects of government intervention on bank behaviors. For this analysis, we replace the dependent variable with a couple of financial markets. The first measure we consider is the HHI for that county, measured by deposit shares obtained from the Summary of Deposits. Since the occurrence of a natural disaster can be seen as an exogenous shock to money demand, individuals must pull money out of their banking institutions to pay for repairs. If there are differential rates of deposit

[9] This constitutes 2,377 observations out of the 18,185 observations we use in the sample.

[10] Appendix A contains estimates and can be found in Table C.1 and Table C.2.

withdrawals, this would cause a change in the HHI of the county in the following year. The provision of low-interest rate loans by the SBA as well as FEMA grants could change the money demand of individuals, therefore having an effect on the HHI of the county. Furthermore, we also examine the changes in deposit shares between community banks and large national banks. Barth et al. [2022] finds that since community banks are more exposed to disaster shocks, their deposit and loan rates are more elastic in response to a localized shock. This could increase the share of deposits customers hold at community banks compared to large banks due to the differences in relative elasticity.

There have been many ways that researchers define exactly what a community bank is. The standard industry practice is that the bank must not hold more than \$1 billion dollars of assets, scaled to a particular year's value of the dollar. Other authors has a more extensive definition of community banks, such as DeYoung et al. [2004], which defines six different criteria including size, location restrictions, ownership, product mix, etc. For the scope of this analysis, we define a community bank using the FDIC guidelines.[11] Under the FDIC rules, a bank is considered a community bank if it satisfies the following six criteria:

1. The organization's loan to asset ratio is greater than 33 percent.
2. The organization's core deposits to assets ratio is greater than 50 percent.
3. The organization has no office with deposits in excess of the quarter's indexed maximum office deposit size.
4. The organization has at least one office, and no more than that quarter's indexed maximum number of offices.
5. The organization has offices in less than 3 large metro areas.
6. The organization has offices in less than 4 states.

Therefore we use this criteria when looking at the change in deposit distribution between community banks and large banks.

For our analysis on banking, we focus on three main aspects. First, under the same experimental design, we determine whether or not extending federal aid could potentially change the distribution of deposits of pre-existing financial institutions. We do this by estimating:

$$Y_{c,t+1} = \beta_1 I(Aid)_{c,t} + \beta_2 I(Disaster)_{c,t} + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \sigma_{s,t} + \rho_{d,t} + \epsilon_{c,s,d,t} \tag{3.3}$$

[11] The FDIC guidelines can be found here: Community Bank Definitions.

Where $Y_{c,t+1}$ is the one-year-ahead measure of both HHI and the deposit shares held at community banks within the county. The HHI measure is created by using deposit shares of all FDIC insured branches that exists within the county. If community banks are more responsive to local shocks and raise deposit rates more than national banks, we should see a shift of deposits towards community banks, implying that $\beta_2 > 0$. Since natural disasters occur at a random rate, we can take the coefficient β_2 to be perfectly exogenous and interpret it as a causal estimate. For β_1, we estimate the IV specification to account for any selection bias that persist in the aid approval process. One assumption that has to be made in this analysis is that individuals and firms within the county only uses financial institutions within the county and not outside of the county. Another assumption that must be made is that the deposit shares calculated is a good representation of the competition structure for the deposit market. Furthermore, we compute the effect of aid and disasters on the one-year-ahead values of the total deposits for a county, total deposits held by large banks, and total deposits held by community banks to confirm previous findings by (Barth et al. [2022], Dlugosz et al. [2024]).

It is also possible that individual behavior differ on a region-by-region basis. If the way that consumers use the banking sector differ based on the current composition of banks, we could see differential response behaviors between areas that predominantly consists of large banks versus areas that consists of small banks. As seen in Figure 3.4, when we split the observations up into metro versus rural regions, we can see a stark difference in the ways that these communities use local banks.[12] For the x-axis, we plot the share of deposits held at community banks. We can see that rural regions rely much more on local banks, whereas large metro region rely mainly on large national banks. If we used pooled estimates without including the interaction term, we might see statistically insignificant effect if national banks are less responsive to local shocks and do not change their lending behavior as much as community banks do.

Therefore, for the analysis of deposits, we use the following specification:

$$Y_{c,t+1} = \beta_1 I(Aid)_{c,t} + \beta_2 I(Disaster)_{c,t} + \beta_{23} I(Disaster)_{c,t} * I(Metro)_c + \gamma \mathbf{X}_{c,t} + \delta_c + \tau_t + \sigma_{s,t} + \rho_{d,t} + \epsilon_{c,s,d,t} \tag{3.4}$$

Where $I(Metro)_c$ is a time invariant indicator of whether that county is a metro region. Since

[12]In practice, a region is defined as metro if the largest number of population that is recorded in the BEA over the period of 2000-2021 is 500 thousand. We use this cut-off since it is the cut-off that the FDIC uses to determine the criteria for a community bank. A county is defined as rural if the largest population it has does not exceed 500 thousand.

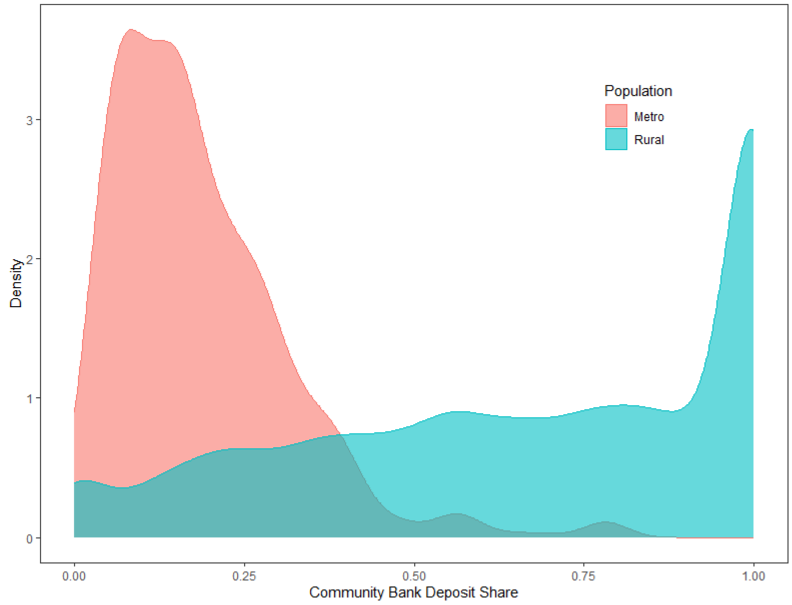


Figure 3.4: Density plot of the share of deposits held at community banks. The graph is split into two groups, one by large metropolitan areas with a population of more than 500 thousand, and one with rural areas with a smaller population. This cutoff for a metro area is defined by the FDIC.

the indicator is time invariant, the loading on the metro indicator is perfectly collinear with the county-fixed effects and does not have an estimate. Under this equation, β_2 measures the effect of a disaster on the dependent variable Y irrespective of the metro status of the county and β_{23} measures the difference in outcomes between metro and rural counties due to the affect of a disaster. Metro counties affected by a disaster has an effect of $\beta_2 + \beta_{23}$, while non-metro counties only change their behavior by β_2.

3.4 Results

Effect on County GDP

We present the baseline results in Table 3.1, with all standard errors clustered at the disaster level. Columns 1-4 displays the effect of increasing aid given, both through grants as well as low-interest rate loans, on the county's GDP in the following year. Column (2) corresponds to the fixed effect estimation described by Equation 3.1 that includes the vector of control variables. We can see that under the fixed-effects specification, the quantity of aid and the GDP recovery is actually negatively correlated and statistically insignificant. The most likely explanation for this observation is that FEMA gives aid to counties that are most affected by the disaster, even after accounting for the disaster damages. The control variables all move in the expected direction, with one-year-ahead GDP increasing in number of jobs and decreasing in unemployment payouts and disaster damages.

Moving onto our first stage shown in column (4), we see that the number of applications is a strong instrument to predict the size of the aid package, with an F-statistic of 1500. We present our baseline IV specifications in column (3). This clustering accounts for correlations in the error term of counties affected by the same disaster, which is likely given that FEMA takes into account each individual disaster when they make their relief decision. In the first stage analysis in column (4), we see that the two main factors that affect the quantity of aid a county receives are the number of applications sent by private parties and the damage caused by the disaster. Under our baseline, we find that the effect of federal aid is significant at the 95% confidence level. Using this model, a 1% increase in the size of the aid package increases GDP in one year by .0077% for the affected county. From the calculations, this corresponds to a fiscal multiplier of 1.45.[13] This fiscal multiplier of federal aid is on the lower end of the estimated multiplier by Yang et al. [2012]. The results are surprising since we should expect the fiscal multiplier to be higher, given that there are less bureaucratic procedure associated with direct monetary transfusions.

We consider multiple robustness checks for our baseline regression. First. we separate public and private application as instruments in Table C.2 and find that the significance still holds, with multiplier estimates ranging from 1.11-3.00. It is possible that including public grant applications could bias our results if FEMA aid is politically motivated, as highlighted in Garrett and Sobel [2003]. They find that both the rate of disaster declarations as well as allocation of disaster expenditures are influenced by the state's political affiliation, with larger impact during election years. This might not be a large issue however, since our within comparison looks at differences in aid given to counties within the same state. Out of the 1524 disasters in our dataset, only 70 disasters affected counties in multiple states. Therefore, the majority of the bias from political motivations is eliminated when taking into account our state-by-year fixed effect.

We also conducted robustness using different measures of county GDP instead of just private industry output in Table C.1. In the table, we consider the case where the total combined

[13] We calculate the fiscal multiplier by predicting what would have happened to the county's GDP if all aid packages were increased by 1%. We then subtract the current fitted values from the predicted one, then divide by 1% of the average aid package.

Table 3.1: Regression Table: N = 18,185. The panel units are counties, and the time unit are years. All specifications include county, year, state-by-year, and disaster fixed effects. Standard errors are clustered at the disaster level. Column (1) corresponds to a FE regression with no control variables. Column (2) corresponds to the fixed effect estimation described by Equation 3.1. Column (3) corresponds to the instrumental variables estimation described in 3.2. Column (4) reports the first-stage results corresponding to Column (3).

Dependent Variables:		Log GDP t+1		Log Aid
	FE	FE with Controls	IV	IV First Stage
Model:	(1)	(2)	(3)	(4)
Variables				
Log Aid	-0.0050	-0.0048	0.0077**	
	(0.0045)	(0.0045)	(0.0034)	
Log Unemployment Insurance		-0.0900***	-0.0912***	0.0945
		(0.0120)	(0.0122)	(0.0883)
Log Number of Jobs		1.029***	1.028***	0.0626
		(0.0730)	(0.0767)	(0.4568)
Log Population		-1.066***	-1.078***	0.9842
		(0.2773)	(0.2919)	(0.9293)
Log Disaster Damages		-0.0006	-0.0019***	0.0849***
		(0.0008)	(0.0006)	(0.0149)
Log Total Applications				1.737***
				(0.0965)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes
Disaster Identifier	Yes	Yes	Yes	Yes
Fit statistics				
F-test (1st stage)				1,468.3
F-test (1st stage), Log Aid			1,468.3	

Clustered (Disaster Identifier) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

GDP is used as the dependent variable (Column (1)) along with only using the public sector GDP as the dependent variable (Column (2)). On average, public sector GDP makes up around 16% of total county GDP, which is a significant share when estimating fiscal policy pass-through. Estimates from these regression would only be different from the baseline if there exist differential aid pass-through rates of local government spending versus private spending. We find that the effect of aid on the public sector is almost three times as high as funds allocated to the private sector. When converting to multiplier values, however, due to the public sector having a much smaller share of GDP, the multiplier for public aid is only .47 compared to the 1.1 for private aid. To conclude, we find that federal aid has a statistically significant effect on local county recovery, with multiplier values ranging from 1.11-3.00 depending on the specification.

Effect on Banks

Table 3.2 shows our results for the effect of aid and disasters on banking outcomes following Equation 3.3. Columns (1) and (2) shows the fixed effect and IV estimates for the effect of aid and disaster on HHI, while columns (3) and (4) shows estimates for the effect on the share of deposits held at community banks. Under our main specification of (2) and (4), we see that the occurence of a natural disaster both increases concentration in the banking market as well as increase the share of deposits held at community banks. This increase in HHI is driven by deposits shifting towards (larger banks?) We also look at the intensive margin in C.3. Under this specification, we only include observations where disaster damages are greater than zero, and re-run the regression using the log-transformed dependent variables and the log-transformed aid and disaster damages. Under the IV specification in columns (2) and (4), we see no statistically significant changes to deposit behaviors in the banking market, suggesting that the shifts in deposit shares are driven by the occurrence of the disaster and not by the intensity.

For our analysis on deposits, shown on Table C.4, we find that deposits in metro areas increase across the board. Total deposits increase on average by 17% in metro areas but decreases around 6% in rural areas. This could be driven by the fact that more aid is given to larger counties even though they are relatively less impacted by disasters than their smaller counterparts, as shown in Figure 3.3. In rural areas, most of these withdrawals are from large banks and reinvested into community banks, increasing the composition of deposits held by community banks in these regions by 1.2%. While the coefficient for the

Table 3.2: Regression Table: This table estimates the extensive margin of the effect of a natural disaster and the provision of aid on HHI and deposit shares held at community banks. we estimate Equation 3.3 where the panel units are counties and the time unit are years. All specifications include county, year, state-by-year, and disaster fixed effects. Standard errors are clustered at the disaster level. Columns (1) and (2) estimates the FE and IV specification with the dependent variable being one-year-ahead HHI. Columns (3) and (4) estimates the FE and IV specification with the dependent variable being one-year-ahead community bank deposit shares.

Dependent Variables:	HHI t+1		CB Dep. Share t+1	
	FE HHI	IV HHI	FE CB Share	IV CB Share
Model:	(1)	(2)	(3)	(4)
Variables				
Aid Indicator	60.70	1.572	0.0132^{**}	-0.0194
	(49.68)	(142.5)	(0.0066)	(0.0222)
Disaster Indicator	99.68^{***}	92.69^{**}	0.0161^{***}	0.0123^{*}
	(35.78)	(36.06)	(0.0062)	(0.0064)
Controls:	Yes	Yes	Yes	Yes
Fixed-effects				
Year	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes
Disaster Identifier	Yes	Yes	Yes	Yes
Fit statistics				
F-test (1st stage), Aid Indicator		689.93		689.93
Observations	17,827	17,827	17,827	17,827

Clustered (Disaster Identifier) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

disaster indicator is statistically insignificant when looking at columns (2) and (3), the sign still suggests the changes in composition is driven by withdrawals from large banks and increase in deposits to community banks. This supports the hypothesis that community banks are more responsive in changing their deposit rates after a natural disaster due to higher exposure, which increases the market share that they can capture. In metro areas however, we see insignificant changes in the composition of deposits, which implies that the increase in deposits in both community banks and large banks are relatively even.

3.5 Conclusion

This paper examines the fiscal impact of disaster aid on local economies, analyzing how FEMA and SBA interventions aid recovery post-natural disasters. The results indicate that a 1% increase in aid leads to a modest 0.008% increase in the GDP of the affected county the following year. This suggests a fiscal multiplier of 1.45, a value on the lower end of existing literature. Alternative specifications finds multiplier estimates ranging from 1.11-3.0, with a stronger effect when giving aid to individualks and firms instead of local governments. The occurrence of a natural disaster also has significant impact on the local financial sector. We find that disasters increase banking concentration in the local region by $\tilde{3}$% and shifts deposit shares towards community banks due to their higher exposure to local markets. Aggregate deposits also increase in metropolitan areas after a disaster but falls in more rural regions.

From a policy perspective, the findings reinforce the necessity of federal disaster aid in fostering economic recovery. The demonstrated fiscal multiplier provides evidence that federal aid has a positive effect, though modest, on local economies. Every dollar put towards increasing FEMA aid could increase tax revenue by at least 40 cents and help local counties recover from localized recessions.[14] Policymakers might consider this multiplier effect when designing future aid packages, ensuring that the aid is not only sufficient but also efficiently distributed to maximize economic recovery. Furthermore, the lack of significant changes in local banking behaviors suggests that fears of federal aid distorting local financial markets may be overstated, which could reassure policymakers concerned about potential negative externalities of aid.

Looking forward, the research opens several avenues for deeper investigation. First, assess-

[14] This is calculated from our estimated fiscal multiplier of 1.45, taken as the lower bound since long-run effects are not considered. We then use data from Centre for Tax Policy and Administration [2023] that shows average US tax revenue is 27.7%.

ing the long-term impacts of disaster aid on local economies would provide valuable insights beyond the immediate post-disaster year. Such studies could help in understanding the persistence of aid impacts and the potential for significant long-term benefits. Additionally, exploring more granular aspects of how aid affects different sectors within local economies might offer tailored insights for sector-specific recovery strategies. Finally, given the complexity of disaster impacts and recovery dynamics, integrating more sophisticated economic models could enhance the accuracy of fiscal multiplier estimations to provide a stronger basis for policy decisions.

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Appendix A

Chapter 1

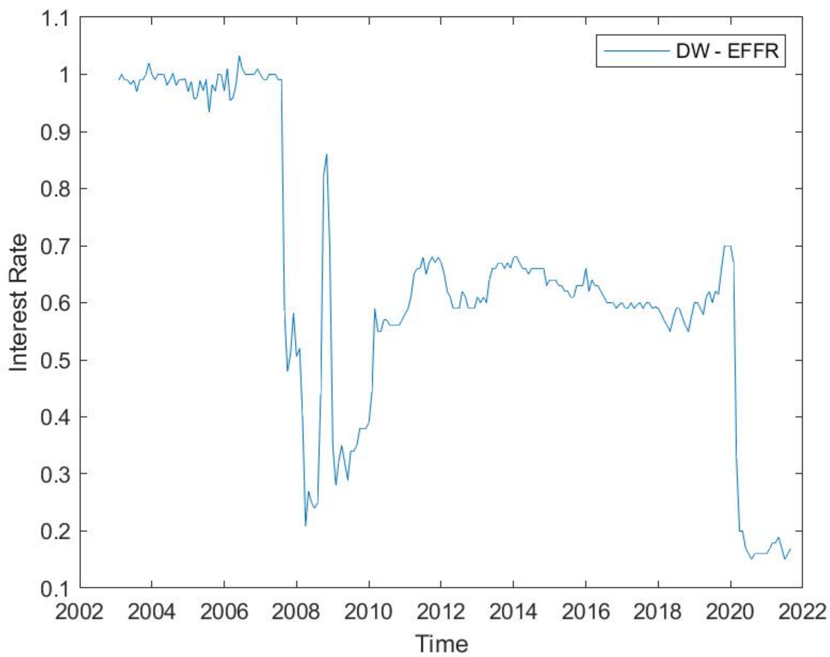


Figure A.1: Difference in percentage points between DW rate and fed funds rate (FRED).

Appendix B

Chapter 2

Appendix A: Figures

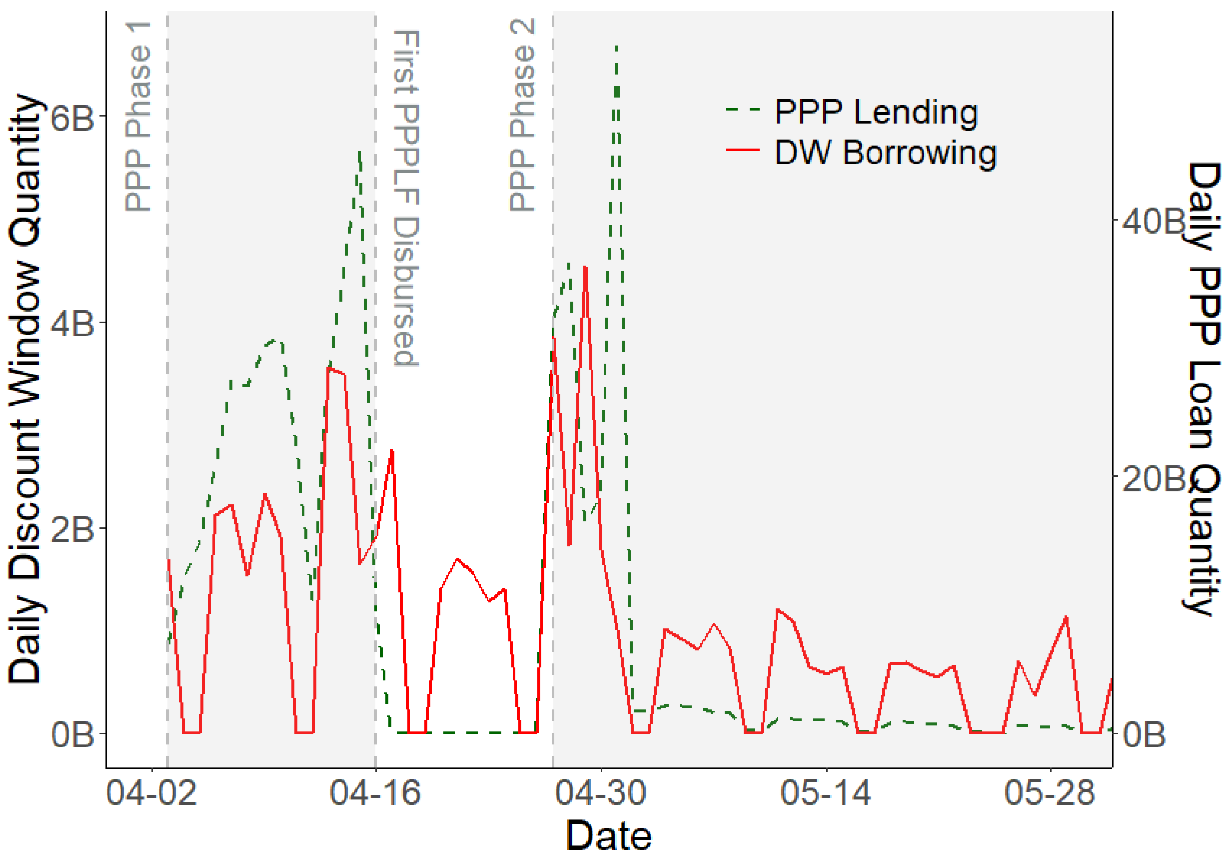
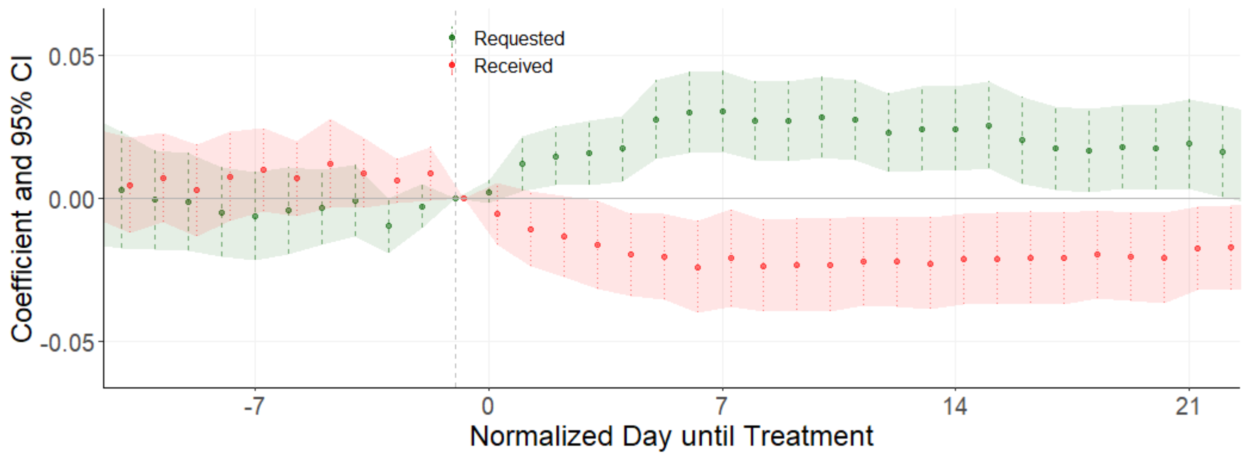
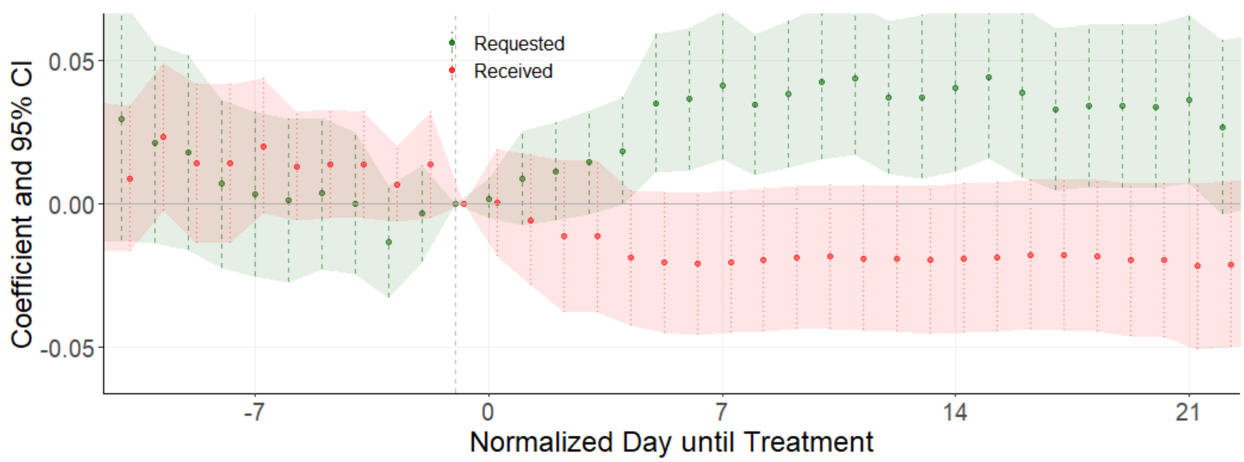


Figure B.1: Version of Figure 2.1 with weekends included.

(a) Pooled Sample



(b) Large Banks



(c) Small Banks

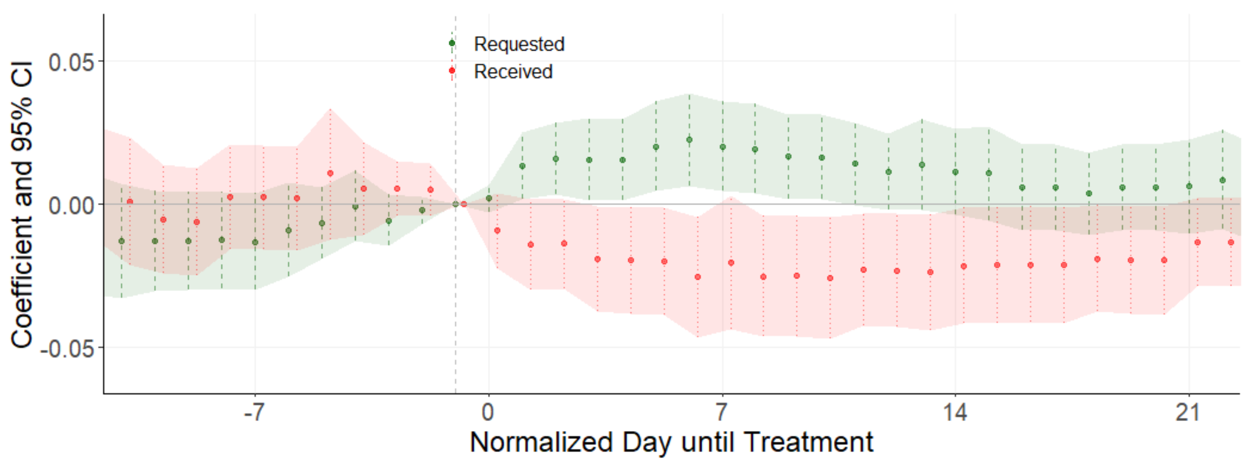


Figure B.2: Baseline result for the event studies in Figure 2.5.

Appendix B: Heckman Selection Instrument

The Heckman correction is designed to correct for selection bias in situations where the sample used in a study is not randomly selected from the population of interest. This is particularly relevant when there is a subset of data that is censored. In this scenario, we have a large proportion of banks showing zero borrowing. This zero borrowing could be due to two reasons: either the bank is genuinely not borrowing (true zero), or the bank is familiar with the discount window but chooses not to borrow (censored zero).

Appendix C: Tables

white

B.1

Table B.1: Summary Statistics Table - Panel Series

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
DW/Reserves	301,962	0.011	0.121	0	0	0	3
PPP/Reserves	301,962	0.021	0.129	0	0	0.000	3
Borrowing from LF in last 30 Days/Reserves	301,962	0.072	0.487	0	0	0	9
DW since 2015 Indicator	301,962	0.121	0.326	0	0	0	1
Number of Offices	301,490	15.910	125	1	2	9	4,850
Reserve/Asset	301,962	0.106	0.105	0.001	0.042	0.135	0.978
Equity Capital Ratio	301,962	0.128	0.076	0.048	0.100	0.134	0.970
Branch Economic Exposure	299,720	−10.779	2.877	−23.874	−12.612	−8.879	−2.869
Unused CI Commitments	301,962	0.031	0.035	0.000	0.007	0.042	0.255
Small Business share of CI	301,962	0.011	0.028	0	0	0	0
Core Deposits	301,962	0.432	0.248	0.000	0.216	0.596	1.531
C&I Loans/Assets	297,183	0.082	0.069	0.000	0.038	0.108	0.645
Liquid Assets	301,962	0.290	0.168	0.010	0.171	0.370	0.996
T1 Leverage Ratio	301,962	0.124	0.076	0.044	0.096	0.128	0.974
Log Assets	301,962	12.573	1.457	9.513	11.611	13.294	18.110
Branch weighted bank age	300,546	65.603	34.514	2.475	37.125	91.962	156.351
COVID Exposure	296,947	0.471	2.320	0.000	0.003	0.196	42.347

Table B.2: Summary Statistics Table - Cross Section

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Number of PPP Loans	3,627	1,411.938	8,061.874	1	80	540	134,051
DW/Reserves during PPP	3,627	0.562	5.318	0	0	0	99
DW/Reserve since 2010	3,627	0.212	1.373	0	0	0.001	37
PPPLF/Reserves	3,627	0.156	0.852	0	0	0	13
Deposit/Asset	3,627	0.832	0.065	0.065	0.805	0.875	0.936
Deposit Growth	3,623	12.750	41.355	−31.525	6.299	14.328	2,004.353
Number of Offices	3,624	17.346	80.882	0.109	2.000	10.000	1,298.479
Reserve/Asset	3,627	0.096	0.086	0.004	0.040	0.123	0.950
Equity Capital Ratio	3,627	0.120	0.043	0.022	0.099	0.130	0.915
Branch Economic Exposure	3,627	−520.065	115.379	−719.985	−602.696	−460.184	0.000
Unused CI Commitments	3,627	0.034	0.036	0.000	0.010	0.046	0.254
Small Business share of CI	3,627	0.013	0.028	0	0	0.01	0
Core Deposits	3,627	0.435	0.241	0.000	0.224	0.590	1.530
C&I Loans/Assets	3,579	0.088	0.068	0.000	0.043	0.114	0.643
Liquid Assets	3,627	0.275	0.149	0.010	0.168	0.352	0.951
T1 Leverage Ratio	3,627	0.116	0.044	0.045	0.096	0.125	0.964
Log Assets	3,627	12.762	1.465	9.040	11.794	13.473	21.714
Branch weighted bank age	3,611	63.872	34.066	1.797	36.272	88.720	188.677
COVID Exposure	3,627	19.161	88.036	0.000	0.537	9.882	1,783.398

Table B.3: Robustness results for Table 2.2. Columns are made under the same specifications as Table 2.2. Panel A displays the results for the linear probability model, only including banks that have borrowed at least once from the DW during April and May. Panel 2 runs an LPM including all banks. Panel 3 ruins a Poisson regression on the same dataset. Panel 4 runs a binomial logistic regression. The sample includes all observations from April 3 to April 16 and April 27 to May 31. In all the nonlinear regression, banks without variations in the dependent variables are dropped due to perfect collinearity with the bank fixed effects.

Dependent Variable:	DW Indicator						
	Pooled		Interacted	Large Banks		Small Banks	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LPM - Dropping Banks							
PPP Lending/Reserves	0.101*	0.097*		0.188***	0.191***	0.022	-0.011
	(0.053)	(0.055)		(0.065)	(0.063)	(0.039)	(0.041)
PPP Lending/Reserves × Small Banks			-0.015				
			(0.039)				
PPP Lending/Reserves × Large Banks			0.216***				
			(0.061)				
Observations	8,771	8,428	8,428	5,292	4,998	3,479	3,430
LPM - Without Dropping							
PPP Lending/Reserves	0.009	0.008		0.021	0.023	-0.002	-0.004
	(0.007)	(0.007)		(0.014)	(0.015)	(0.004)	(0.004)
PPP Lending/Reserves × Small Banks			-0.003				
			(0.004)				
PPP Lending/Reserves × Large Banks			0.022				
			(0.014)				
Observations	250,782	242,648	242,648	62,622	58,016	188,160	184,632
Poisson							
PPP Lending/Reserves	0.219*	0.208*		0.333***	0.336***	0.064	-0.007
	(0.118)	(0.120)		(0.127)	(0.125)	(0.130)	(0.130)
PPP Lending/Reserves × Small Banks			-0.038				
			(0.124)				
PPP Lending/Reserves × Large Banks			0.376***				
			(0.122)				
Observations	8,771	8,428	8,428	5,292	4,998	3,479	3,430
Logistic							
PPP Lending/Reserves	1.09***	1.10***		1.63***	1.67***	0.067	-0.241
	(0.416)	(0.427)		(0.484)	(0.485)	(0.472)	(0.439)
PPP Lending/Reserves × Small Banks			-0.215				
			(0.453)				
PPP Lending/Reserves × Large Banks			1.91***				
			(0.525)				
Observations	8,428	8,085	8,085	5,145	4,851	3,283	3,234
Bank Controls:		Yes	Yes		Yes		Yes
Fixed-effects							
Bank	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Clustered (Bank) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.4: This table reports the results of how DW borrowing as a share of reserves affects the number of PPP loans originated using a TSLS approach with each column denoting a different cutoff size for large banks. The table reports the extensive margin of the effect of DW access on PPP lending for the large bank subset in Phase 1. Column (1) represents the cutoff for large banks at the 75th percentile, corresponding to a bank that has $593 million in assets in Q1 of 2020. Column (2) uses the 80th percentile ($783 million) as a cutoff. Column (3) uses the 90th percentile ($1.73 billion) as a cutoff. Column (4) uses the 95th percentile ($4.46 billion).

Dependent Variable:	Log Number of PPP Loans			
Model:	(1)	(2)	(3)	(4)
Variables				
DW Indicator	0.928***	0.873***	0.877**	0.906**
	(0.309)	(0.318)	(0.352)	(0.380)
Deposit Growth	0.021***	0.027***	0.023***	0.018*
	(0.006)	(0.006)	(0.008)	(0.009)
Fed Funds+ONRRP/Reserves	0.001**	0.0009*	0.001	-0.0003
	(0.0004)	(0.0005)	(0.0008)	(0.001)
FLHB/Reserves	0.808	1.93	3.61	16.7*
	(0.606)	(1.33)	(2.56)	(8.80)
Bank Characteristic Controls:	Yes	Yes	Yes	Yes
Fixed-effects				
Size Deciles	Yes	Yes	Yes	Yes
Fed District	Yes	Yes	Yes	Yes
Fit statistics				
Observations	977	777	368	158
R^2	0.40955	0.37375	0.42776	0.47337
F-test (1st stage), DW Indicator	186.49	160.43	63.194	25.632

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.5: This table reports the results of how DW borrowing as a share of reserves affects the number of PPP loans originated using a TSLS approach. Columns are made under the same specification as 2.5. The sample is made using the aggregation of only Phase 1 lending. Panel 1 shows the baseline instrument, panel 2 replaces the baseline instrument with the number of times a bank has previously borrowed from the DW. Panel 3 uses both instruments to perform a Sargan test for over-identification. I fail to reject the null hypothesis (both instruments are exogenous) for all specifications.

Dependent Variable:	Log Number of PPP Loans					
		Pooled		Interacted	Large Banks	Small Banks
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Size of previous borrowing from DW						
DW Indicator	0.831***	0.934***	0.909***		0.928***	1.26
	(0.263)	(0.292)	(0.291)		(0.309)	(0.855)
DW Indicator × Small Banks				1.48*		
				(0.807)		
DW Indicator × Large Banks				0.734**		
				(0.308)		
# of times previously borrowed from DW						
DW Indicator	1.15**	1.11**	1.09**		0.779*	2.44*
	(0.473)	(0.444)	(0.434)		(0.457)	(1.37)
DW Indicator × Small Banks				2.70**		
				(1.29)		
DW Indicator × Large Banks				0.452		
				(0.419)		
Both Instruments						
DW Indicator	0.873***	0.958***	0.934***		0.914***	1.57*
	(0.259)	(0.284)	(0.282)		(0.300)	(0.863)
DW Indicator × Small Banks				1.81**		
				(0.821)		
DW Indicator × Large Banks				0.708**		
				(0.298)		
Bank Characteristic Controls:		Yes	Yes	Yes	Yes	Yes
Fixed-effects						
Size Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Fed District	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	3,486	3,419	3,419	3,419	977	2,442
Sargan Over-Identification Test	0.45376	0.15413	0.17737	1.8910	0.08931	1.2939

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix C

Chapter 3

Table C.1: Regression Table: N = 18,185. The panel units are counties, and the time unit are years. All specifications include county, year, state-by-year, and disaster fixed effects. Standard errors are clustered at the disaster level. Column (1) uses total applications as the instrument to estimate the effect on total GDP of the county. Column (2) uses applications by the local government as the instrument to estimate the effect on the public sector GDP. Column (3) uses applications by individuals and firms as the instrument to estimate the effect on private sector GDP.

Dependent Variables:	Log All GDP t+1 Total Applications	Log Public GDP t+1 Public Applications	Log Private GDP t+1 Private Applications
Model:	(1)	(2)	(3)
Variables			
Log Aid	0.0076**	0.0148**	0.0059**
	(0.0032)	(0.0075)	(0.0029)
Log Unemployment Insurance	-0.0813***	-0.0250***	-0.0911***
	(0.0115)	(0.0096)	(0.0122)
Log Number of Jobs	0.9910***	0.4191***	1.028***
	(0.0776)	(0.0693)	(0.0762)
Log Population	-1.045***	-0.6186*	-1.076***
	(0.2953)	(0.3441)	(0.2903)
Log Disaster Damages	-0.0016***	-0.0014	-0.0017***
	(0.0006)	(0.0009)	(0.0006)
Fixed-effects			
Year	Yes	Yes	Yes
County	Yes	Yes	Yes
State-Year	Yes	Yes	Yes
Disaster Identifier	Yes	Yes	Yes
Fit statistics			
F-test (1st stage), Log Aid	1,468.3	379.03	1,691.2

Clustered (Disaster Identifier) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

[!htbp]

Table C.2: Robustness Table looking at different types of applications but separately using public and private aid applications as instrument instead of the combined total. Columns (1) and (3) are the second stage results corresponding to public and private applications respectively. Columns (2) and (4) shows the first stage results.

Dependent Variables:	Log GDP t+1	Log Aid	Log GDP t+1	Log Aid
	Public Applications		Private Applications	
Model:	(1)	(2)	(3)	(4)
Variables				
Log Aid	0.0141*		0.0044*	
	(0.0073)		(0.0025)	
Log Unemployment Insurance	-0.1030***	0.1233	-0.1016***	0.1503
	(0.0359)	(0.1182)	(0.0357)	(0.1018)
Log Number of Jobs	0.2730***	0.5649	0.2785***	0.5572
	(0.0996)	(0.4518)	(0.0969)	(0.4323)
Log Disaster Damages	-0.0025	0.0934***	-0.0015	0.0856***
	(0.0015)	(0.0108)	(0.0013)	(0.0099)
Log Income per Capita	1.020***	0.0956	1.021***	-0.1300
	(0.0820)	(0.4009)	(0.0831)	(0.3539)
Log Public Applications		1.525***		
		(0.1758)		
Log Private Applications				1.810***
				(0.0946)
Fixed-effects				
Year				
County				
State-Year				
Disaster Identifier				
Fit statistics				
F-test (1st stage), Log Aid	387.22		1,727.2	
F-test (1st stage)		387.22		1,727.2

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table C.3: Regression Table: The panel units are counties, and the time unit are years. All specifications include county, year, state-by-year, and disaster fixed effects. Standard errors are clustered at the disaster level. For the intensive margin, we only filter out observations where disaster damages are greater than zero, leaving us with N = 13,685. Column (1) corresponds to a FE regression where we regress logged HHI on logged aid and damages. Column (2) uses the number of applications to instrument for the amount of aid. Since disasters are randomized, we find it unnecessary to instrument for it. Column (3) is a FE regression of the logged deposit share held at community banks on logged aid and damages. Column (4) reports the IV results for Column (3).

Dependent Variables:	Log HHI t+1		Log CB Dep. Share t+1	
	FE HHI	IV HHI	FE CB Share	IV CB Share
Model:	(1)	(2)	(3)	(4)
Variables				
Log Aid	0.0016*	-0.0023	0.0024**	-0.0047
	(0.0010)	(0.0024)	(0.0011)	(0.0035)
Log Disaster Damages	-0.0026***	-0.0014	-4.06×10^{-5}	0.0020
	(0.0008)	(0.0010)	(0.0017)	(0.0022)
Log Unemployment Insurance	0.0029	0.0035	0.0217	0.0226
	(0.0129)	(0.0132)	(0.0150)	(0.0152)
Log Number of Jobs	0.1870***	0.1865***	-0.0234	-0.0237
	(0.0520)	(0.0533)	(0.1119)	(0.1140)
Log Population	0.0946	0.0958	0.0653	0.0679
	(0.0961)	(0.0977)	(0.1220)	(0.1219)
Log Income per Capita	-0.1669***	-0.1647***	-0.2336***	-0.2296***
	(0.0361)	(0.0350)	(0.0372)	(0.0378)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes
Disaster Identifier	Yes	Yes	Yes	Yes
Fit statistics				
F-test (1st stage), Log Aid		956.09		923.94
Observations	13,685	13,685	13,378	13,378

Clustered (Disaster Identifier) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table C.4: Regression Table: The panel units are counties, and the time unit are years. All specifications include county, year, state-by-year, and disaster fixed effects and follows Equation 3.4. Standard errors are clustered at the disaster level. Column (1)-(3) estimates the log changes in the total deposits, deposits held by large banks, and deposits held by small banks respectively. Column (4) reports the change in the deposit share held by small banks.

Dependent Variables:	Log Total Depo. t+1	Log Large Bank Dep. t+1	Log CB Dep. t+1	CB Dep. Share t+1
Model:	(1)	(2)	(3)	(4)
Variables				
Aid Ind	0.0880	0.3124	0.0711	-0.0195
	(0.0856)	(0.4220)	(0.1968)	(0.0222)
Disaster Ind	-0.0741***	-0.1842	0.0805	0.0123*
	(0.0193)	(0.1722)	(0.0569)	(0.0064)
Metro	-0.1395	-0.3353	-0.1654	0.0139
	(0.1234)	(0.3935)	(0.1649)	(0.0171)
Disaster Ind × Metro	0.2133**	0.5724***	0.2659**	-0.0112
	(0.0842)	(0.1922)	(0.1063)	(0.0158)
Controls:	Yes	Yes	Yes	Yes
Fixed-effects				
Year	Yes	Yes	Yes	Yes
County	Yes	Yes	Yes	Yes
State-Year	Yes	Yes	Yes	Yes
Disaster Identifier	Yes	Yes	Yes	Yes
Fit statistics				
F-test (1st stage), Aid Ind	689.53	689.53	689.53	689.41
Observations	17,840	17,840	17,840	17,827

Clustered (Disaster Identifier) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*