Did the Paycheck Protection Program Hit the Target?*

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November 2020

Abstract

This paper provides a comprehensive assessment of financial intermediation and the economic effects of the Paycheck Protection Program (PPP), a large and novel small business support program that was part of the initial policy response to the COVID-19 pandemic in the US. We use loan-level microdata for all PPP loans and high-frequency administrative employment data to present three main findings. First, banks played an important role in mediating program targeting, which helps explain why some funds initially flowed to regions that were less adversely affected by the pandemic. The top-4 banks alone account for 36% of total pre-policy small business loans, but disbursed less than 3% of all PPP loans in the first round of funding. Second, we exploit regional heterogeneity in lending relationships and individual firm-loan matched data to show that the short- and medium-term employment effects of the program were small compared to the program's size. Third, many firms used the loans to make non-payroll fixed payments and build up savings buffers, which can account for small employment effects and likely reflects precautionary motives in the face of heightened uncertainty. Limited targeting in terms of who was eligible likely also led to many inframarginal firms receiving funds and to a low correlation between regional PPP funding and shock severity. Our findings illustrate how business liquidity support programs affect firm behavior and local economic activity and how policy transmission depends on the agents delegated to deploy it.

*We thank seminar participants at the Princeton-Stanford Conference on Corporate Finance and Macro, University of Chicago Booth School of Business, the Stigler Center Economic Effects of COVID-19 Workshop, the University at Texas Austin McCombs School of Business, the Federal Reserve Bank of New York, the Federal Reserve Bank of Philadelphia, the Congressional Budget Office and the Bank of Portugal as well as Scott Baker, Jean-Noel Barrot (discussant), Sylvain Catherine, Jediphi Cabal, Raj Chetty, Gabe Chodorow-Reich (discussant), Mike Faulkender, Sam Hanson, Steve Kaplan, Anil Kashyap, Mike Minnis, Josh Rauh (discussant), Tiago Pinheiro, Larry Schmidt, Adi Sunderam, and Luigi Zingales for comments. Laurence O'Brien and Igor Kuznetsov provided excellent research assistance. João Granja gratefully acknowledges support from the Jane and Basil Vasiliou Faculty Scholarship and from the Booth School of Business at the University of Chicago. Yannelis and Zwick gratefully acknowledges financial support from the Booth School of Business at the University of Chicago. We are grateful to the Small Business Administration, Womply, and Homebase for providing data.

1 Introduction

The COVID-19 pandemic triggered an unprecedented economic freeze and a massive immediate policy response. Among the firms most affected by the freeze were millions of small businesses without access to public financial markets or other ways to manage short-term costs. Without an existing system of social insurance to support these firms, policymakers around the world rushed to develop new programs to contain the damage, including wage subsidies, small business grants, and guaranteed business loan schemes.[1]

This paper studies a large and novel business support program that was part of the crisis response in the US, the Paycheck Protection Program (PPP). Part of the CARES Act, the PPP offers guaranteed, forgivable loans to provide liquidity to small and mid-sized businesses and prevent job losses. The program is administered by the Small Business Administration (SBA) with the loan application process operated by commercial banks. The loans are forgivable—that is, they become grants—if firms do not permanently lay off workers or change their compensation, and if firms use the funds for eligible expenses. The PPP ultimately deployed more than $500 billion within just four months of passage, making it one of the largest firm-based fiscal policy programs in US history.

We have three main findings. First, banks played an important role in mediating program targeting, which helps explain why some funds initially flowed to regions that were less adversely affected by the pandemic. Second, the short- and medium-term employment effects of the program were small compared to the program's size. Third, many firms used the loans to make non-payroll fixed payments and build up savings buffers, which can account for small employment effects and likely reflects precautionary motives in the face of heightened uncertainty.[2] Limited targeting in terms of who was eligible likely also led to many inframarginal firms receiving funds and to a low correlation between regional PPP funding and shock severity.

We bring data from two sources to study the PPP. First, we use loan-level microdata from the SBA for all PPP loans, which includes lender, geography, and some borrower- and loan-

[1] For example, the UK, France, Germany, Spain, Italy, and Australia introduced or expanded loan guarantee and small business grant schemes in response to the pandemic. Hanson, Stein, Sunderam and Zwick (2020*b*) provide a theoretical discussion of business credit support programs in the pandemic and a review of key programs in Europe. Many of these countries also separately implemented temporary wage subsidy programs to provide incomes to unemployed workers directly through firms (see Hubbard and Strain (2020) for a comprehensive list). While the program we study combines these features, the larger source of wage support in the US came via the unemployment insurance system.

[2] Riddick and Whited (2009) and Gao, Whited and Zhang (2020) show that uncertainty increases firms' precautionary motives to hold cash, particularly when external financing is difficult to obtain.

level information.The data offer a clear look at which lenders are most active in disbursing loans, how program participation evolves over time, and at the geographic distribution of PPP lending across the U.S. economy. Additionally, we obtained high-frequency employment data from Homebase, a software company that provides free scheduling, payroll reporting and other services to small businesses, primarily in the retail and hospitality sectors. The granularity of the data, coupled with the focus on sectors most adversely affected by the pandemic, allows us to trace out the response of employment, wages, hours worked, and business closures in almost real-time and evaluate the effects of PPP support. We complement these primary data sources with a number of other sources, including county unemployment insurance claims, the Census Small Business Pulse survey, small business revenue data from Womply, and worker earnings in small businesses from the COVID-19 economic tracker (Chetty, Friedman, Hendren and Stepner, 2020).

In the first part of the paper, we consider two dimensions of program targeting. First, did the funds flow to where the economic shock was greatest? A central policy goal of the program was to prevent unnecessary mass layoffs and firm bankruptcies by injecting liquidity into firms. These potential benefits are likely greatest in areas with more pre-policy economic dislocation and disease spread. We find no evidence that funds flowed to areas that were more adversely affected by the economic effects of the pandemic, which we proxy using declines in hours worked, business shutdowns, and coronavirus infections and deaths. If anything, we find evidence that funds flowed to areas less hard hit. Over both rounds of funding, the correlation between pre-policy economic dislocation and program participation was approximately zero, which likely reflects the program's broad definition of eligibility (Barrios, Minnis, Minnis and Sijthoff, 2020).

Second, given that the PPP used the banking system as a conduit to access firms, what role did the banks play in mediating policy targeting? We find significant heterogeneity across banks in terms of disbursing PPP funds, which reflects more than mere differences in underlying loan demand and appears to contribute to the weak correlation between economic declines and PPP lending. We construct a measure of geographic exposure to bank performance in the PPP using the distribution of deposits across geographic regions. The measure exploits the fact that most small business lending is local (Brevoort, Holmes and Wolken, 2010; Granja, Leuz and Rajan, 2018), comparing lenders that did more PPP lending, relative to other small business lending, versus their counterparts who did less. We find that areas that were significantly more exposed to banks whose PPP lending shares exceeded their small business lending market shares received disproportionately larger allocations of PPP loans.

In the second part of the paper, we study the effect of the PPP on economic outcomes. Our results on bank participation motivate a research design to evaluate the PPP using bank-driven differences in regional exposure to the program. This variation across regions allows us to isolate the effect of the PPP from differences in loan demand or confounding correlations between PPP funding and local economic outcomes. Our research design relies on the assumption that pre-policy bank deposit shares in particular regions are not correlated with the various outcomes we study. This assumption holds once we condition on relevant observables, such as the relationship between PPP funding and the initial severity of the crisis. We use this research design to study business shutdowns, reductions in hours worked, initial unemployment insurance (UI) claims, and small business revenues at the county level.

We do not find evidence that the PPP had a substantial effect on local economic outcomes or business shutdowns during the first round of the program, and find modest effects on hours worked in May and June. Consistent with modest employment effects, we also find small effects of the program on small business revenues in May and June. We confirm the firm-level evidence by documenting limited impacts on initial unemployment insurance claims at the county level. Our confidence intervals on employment outcomes are wide enough to permit modest effects of the program, but precise enough to reject large effects. Our estimates suggest that more than 90% of jobs supported by the PPP were inframarginal. If wages for inframarginal workers did not adjust, then the bulk of the program's economic benefits appear to accrue to other stakeholders, including owners, landlords, lenders, suppliers, customers, and possibly future workers.

We complement our aggregate regional design with a timing design using matched firm-loan data. We are able to match 1,176 firms in Homebase to PPP loans and then compare firms that received loans earlier versus later.[3] We instrument for the date of PPP receipt using regional exposure to lenders that disbursed different amounts of PPP funding. This variation allows us to capture the effect of firms receiving loans during a crisis in earlier versus later weeks. Results from this research design also show modest effects that fall within the confidence interval of our bank exposure design.

[3]We focus on this limited matched sample, as we are only able to identify firms for a subset of PPP loans. The public Treasury PPP data includes firm identifiers only for loans above $150,000, or 13.5% of all loans disbursed.

The fact that the program disbursed significant funds, yet had little effect on employment, leads to the natural question of what firms did with the money. We draw on the Census Small Business Pulse Survey to show that PPP funds allowed firms to build up liquidity and to meet loan and other non-payroll spending commitments. For these firms, the PPP may have strengthened balance sheets at a time when shelter-in-place orders prevented workers from doing work and when unemployment insurance was more generous than wages for a large share of workers.

This finding is important because it implies that, while employment effects are small in the short run, they may well be positive in the longer run because firms are less likely to close permanently. The program also likely had important effects in terms of promoting financial stability by avoiding corporate loan defaults and business evictions. At the same time, because program eligibility was defined broadly, many less affected firms received PPP funding and may have continued as they would have in the absence of the funds, either by spending less out of retained earnings or by borrowing less from other sources. For these firms, while the statutory incidence of funding falls on labor and creditors, the economic incidence falls mainly on business owners.

This paper joins a literature focusing on how government interventions following crises impact recovery and the broader economy. Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski and Seru (2017) and Ganong and Noel (2018) study the impact of mortgage modifications following the Great Recession. House and Shapiro (2008) and Zwick and Mahon (2017) study the effect of fiscal stimulus in the form of temporary tax incentives for business investment, and Zwick (Forthcoming) documents the role of delegated agents in mediating take-up of tax-based liquidity support for small firms. Mian and Sufi (2012), Parker, Souleles, Johnson and McClelland (2013), Kaplan and Violante (2014), Biggs and Rauh (2020) and Baker, Farrokhnia, Meyer, Pagel and Yannelis (2020) study how stimulus payments following recessions affect household consumption. This paper evaluates a very large stimulus program aimed at providing liquidity and payroll support to small firms.

This paper also joins a rapidly growing literature studying the impact of the COVID-19 pandemic on the economy. While we offer a comprehensive evaluation of the PPP, a growing number of studies also explore various aspects of the PPP program. Barrios, Minnis, Minnis and Sijthoff (2020) study the relationship between payroll and allocation of funds. Elenev, Landvoigt and Van Nieuwerburgh (2020) theoretically assess the optimal targeting of PPP loans during the pandemic, focusing on the idea of providing support to small versus larger firms that differ in their liquidity constraints. Cororaton and Rosen (2020) examine the firm characteristics of public firms that received PPP loans, underscoring the importance of targeting loans to firms that need liquidity most. Consistent with our findings, Papanikolaou and Schmidt (2020) use sectoral variation in the ability to work remotely and find that the PPP provided smaller per-employee relief to the sectors most exposed to employment declines. Our targeting results contrast with the rollout of a similar lending program in Italy where smaller firms and those in more adversely affected areas were more likely to receive support (Core and De Marco, 2020).

Two studies use the size threshold of 500 employees to study the employment effects of the program. This research design estimates a different treatment effect, as it uses variation local to larger firms, while most PPP loans were disbursed to much smaller firms.[4] Consistent with our findings, these papers find either modest or negligible effects. Chetty, Friedman, Hendren and Stepner (2020) find no employment effects of the PPP and can rule out modest effects. Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar and Yildirmaz (2020) find that the PPP boosted employment at eligible firms by 2–4.5%.

Motivated in part by our targeting results, Bartik, Cullen, Glaeser, Luca, Stanton and Sunderam (2020) adopt a cross-sectional variant of our research strategy, which leverages the fact that larger banks provided relatively fewer PPP loans. They use variation in firms' exposure to larger banks to instrument for PPP receipt, under the assumption that firms banking with larger institutions are similar other than their exposure to underperforming PPP lenders. They find a 14-30 percentage point rise in firm survival probabilities and a positive, but statistically insignificant, effect on employment. Morse and Bartlett (2020) use survey data and find that PPP receipt increases survival probabilities for micro-businesses. Both findings are potentially consistent with our results that firms treated the program more as liquidity support than as an immediate incentive to recall unemployed workers. Consistent with this idea, Chodorow-Reich, Darmouni, Luck and Plosser (2020) show that small and medium sized recipients of PPP reduced their non-PPP borrowing and find that a significant portion of funds was used to strengthen balance sheets.

Our paper provides a comprehensive assessment of an important and large forgivable loan guarantee program. We join work studying subsidized lending and loan guarantees, a widely-

[4]Approximately 0.4% of PPP loans were disbursed to firms with more than 250 employees, which account for 13% of covered employment among all borrowers.

used form of government intervention in credit markets. Classic work such as Smith (1983), Gale (1990), and Gale (1991) focuses on modeling government credit interventions such as loan guarantees. Early empirical work focused on loan guarantee programs in France (Lelarge, Sraer and Thesmar, 2010). Recent theoretical work has studied government guarantees to banks (Atkeson, d'Avernas, Eisfeldt and Weill, 2018; Kelly, Lustig and Van Nieuwerburgh, 2016) as economic stimulus (Lucas, 2016), and a burgeoning empirical literature examines the effects of loan guarantees on credit supply, employment, and small business outcomes (Bachas, Kim and Yannelis, 2020; Barrot, Martin, Sauvagnat and Vallee, 2019; Mullins and Toro, 2017; Gonzalez-Uribe and Wang, 2019). Our results highlight how public policy interventions can interact with other policies (e.g., unemployment insurance, state-level lockdowns) and how fiscal policy transmission depends on the agents delegated to deploy it (e.g., banks). Our results also relate with a literature about relationship lending. Consistent with our results, Amiram and Rabetti (2020) find that firms with pre-existing lending relationships were able to get faster access to funds, but were more likely to incur costs associated with returning funds when the guidelines were revised. Furthermore, Li and Strahan (2020) find that lending early during the PPP expanded most among banks with close borrower relationships, particularly with those that had higher levels of small business loans prior to the pandemic.

The remainder of this draft is organized as follows. Section 2 describes the PPP. Section 3 discusses the main data sources used. Section 4 describes how the distribution of relative performance in the PPP is correlated with bank and other characteristics, documents how differences across banks in PPP activity imply geographic differences in PPP exposure, and explores the implications for PPP targeting to different geographic areas. Section 5 analyzes the effects of the PPP on labor market and local economic outcomes using our bank exposure and timing research designs. Section 6 explores mechanisms behind these effects. Section 7 concludes.

2 The Paycheck Protection Program (PPP)

The Paycheck Protection Program (PPP) began on April 3rd, 2020 as part of the 2020 CARES Act as a temporary source of liquidity for small businesses, authorizing $349 billion in forgivable loans to help small businesses pay their employees and additional fixed expenses during the COVID-19 pandemic. Firms apply for support through banks and the Small Business Administration (SBA) is responsible for overseeing the program and processing loan guarantees and forgiveness. An advantage of using the banking system (including FinTech) as a conduit for providing liquidity to firms is that, because nearly all small businesses have pre-existing relationships with banks, this connection could be used to ensure timely transmission of funds.[5]

The lending program was generally targeted toward small businesses of 500 or fewer employees.[6] Although the initial round of funding was exhausted on April 16th, a second round of $310 billion in PPP funding was passed by Congress as part of the fourth COVID-19 aid bill.[7] Small businesses were eligible as of April 3rd and independent contractors and self-employed workers were eligible as of April 10th. The initial deadline for firms to apply to the program was June 30, but this was eventually extended to August 8.

The terms of the loan were the same for all businesses. The maximum amount of a PPP loan is the lesser of 2.5 times the average monthly payroll costs or $10 million. The average monthly payroll is based on prior year's payroll after subtracting the portion of compensation to individual employees that exceeds $100,000.[8] The interest rate on all loans is 1% and their maturity is two years. Under the initial bill, the PPP loans can be forgiven if two conditions are met. First, proceeds must be used to cover payroll costs, mortgage interest, rent, and utility costs over the eight-week period following the provision of the loan, but not more than 25 percent of the loan forgiveness amount may be attributable to non-payroll costs. Second, employee counts and compensation levels must be maintained. If companies cut pay or employment levels, loans may not be forgiven.[9] However, if companies lay off workers or cut compensation between February 15th and April 26th, but subsequently restore their employment levels and employee compensation, their standing can be restored.

[5]Many of these relationships are limited to having transaction accounts. Using data from a large survey on Facebook, Alekseev, Amer, Gopal, Kuchler, Schneider, Stroebel and Wernerfelt (2020) find that half of firms report not having pre-existing relationships as borrowers with banks, which appears to have led to such firms initially struggling to access the program and eventually switching lenders in order to receive funds.

[6]A notable exception was made for firms operating in NAICS Code 72 (accommodations and food services), which are eligible to apply insofar as they employ under 500 employees per physical location. Firms whose maximum tangible net worth is not more than $15 million and average net income after Federal income taxes (excluding any carry-over losses) of the business for the two full fiscal years before the date of the application is not more than $5 million can also apply. See the SBA for further information about the program.

[7]The traditional SBA program responding to disasters is the Economic Injury Disaster Loan (EIDL) program. EIDL amounts are deducted from PPP forgiveness. Recipients of an EIDL loan can receive a $10,000 loan advance that does not need to be paid back. The EIDL loan itself is capped at a maximum of $2 million, is not forgivable, and the funds can be used flexibly for operating expenses. The EIDL and PPP programs functioned in tandem, and EIDL loans are further discussed in section 6 .

[8]Payroll costs include wages and salaries but also payments for vacation, family and medical leave, healthcare coverage, retirement benefits, and state and local taxes.

[9]Loan payments on the remainder of the loan can be deferred for six months and interest accrues at 1%.

Congress expanded PPP on June 3rd, allowing more flexible terms for loan forgiveness. The updates to the PPP expanded the duration from eight-weeks to twenty-four and extended the deadline to rehire workers until the end of the year. This effectively gave small businesses more time to use program funds and rehire workers. Additionally, the minimum amount of funds used for payroll while still qualifying for forgiveness was lowered from 75 to 60 percent.

An important feature of the program is that the SBA waived its standard "credit elsewhere" test used to grant regular SBA 7(a) loans. This test determines whether the borrower has the ability to obtain the requested loan funds from alternative sources and amounts to a significant barrier in the access to regular SBA loans. Instead, under PPP rules, applicants were only required to provide documentation of their payroll and other expenses, together with a simple two-page application process where they certify that the documents are true and that current economic uncertainty makes this loan request necessary to support ongoing operations. In sum, the PPP program was designed to be a "first-come-first-served" program with eligibility guidelines that allowed it to reach a broad spectrum of small businesses.

During the first weeks of April, demand for PPP loans outstripped supply, which was limited by statute. Between April 3 and 16 all of the initial $349 billion was disbursed, and the program stopped issuing loans for a period of time. The House and Senate passed a bill to add an additional $320 billion in funding on April 21 and 23 respectively, which was signed into law on the 24th. The PPP began accepting applications on April 27 for the second round of funding. While 60% of the second round funds were allocated within two weeks of initial disbursement, for the remainder of the second round funds were disbursed much less quickly, with unallocated PPP funds being available in late June. By early July, more than $130 billion remained available in PPP funds. Loan disbursement remained low throughout July and August, suggesting that the second round had sufficient funds to meet demand. The program stopped accepting applications on August 8, culminating in $525 billion in disbursement through the PPP.

3 Data

Our primary source for the on the PPP comes from microdata made available through the Small Business Administration (SBA) and the Department of Treasury containing all PPP loans. We are able to observe all loans approved under the program. For all loans, the data include lender name, the borrower's self-reported industry and corporate form, workers covered by the loan, and some demographic data on firm owners. Borrowers are only identifiable for loans above $150,000. Our targeting analysis and bank exposure research design use data for all loans aggregated to either the regional or local geography level, while our individual research design uses matched sample research design for loans above $150,000.

We merge this data set with the Reports of Condition and Income (Call Reports) filed by all active commercial banks as of 2020:Q1. We are able to match 4,370 bank participants in the PPP program to the Call Reports data set. We did not match 795 commercial and savings banks that filed a Call Report in the first quarter of 2020. We assume that these banks did not participate in the PPP program and made no PPP loans. Overall, lenders in the PPP sample that we matched to the Call Report account for 90.5% of all loans disbursed under the PPP.

We obtain financial characteristics of all banks from the Call Reports, which provide detailed data on the size, capital structure, and asset composition of each commercial and savings bank operating in the United States. Importantly, we obtain information on the number and amount of small business loans outstanding of each commercial and savings bank from the "Loans to Small Business and Small Farms Schedule" of the Call Reports. Using this information, we benchmark the participation of all commercial and savings banks in the PPP program relative to their share of the small business lending market prior to the program.

To compute measures of exposure of each state, county, and ZIP to PPP lenders, we combine the matched-PPP-Call-Reports data set with Summary of Deposits data containing the location of all branches and respective deposit amounts for all depository institutions operating in the United States as of June 30th, 2019.In our bank exposure research design, we take advantage of the idea that small business lending is mostly local (e.g., Granja, Leuz and Rajan (2018)) and use the distribution of deposits across geographic regions to create our Bartik-style measure of exposure of these regions to lenders that over- or underperformed in terms of their national share of PPP lending relative to their respective national share of the small business lending market. We use data from the County Business Patterns dataset to approximate the amount of PPP lending per establishment and the fraction of establishments receiving PPP loans in the region and to investigate whether the fraction of establishments receiving PPP loans in a region is affected by that region's exposure to the performance of its local banks in the PPP.

To evaluate whether PPP amounts were allocated to areas that were hardest-hit by the COVID-19 crisis and whether the program improved economic employment and other economic outcomes following its passage, we use data from multiple available sources on the employment, social distancing, and health impact of the crisis. We obtained detailed data on hours worked among employees of firms that use Homebase, a software company, to manage their scheduling and time clock.[10] Homebase processes exact hours worked by the employees of a large number of businesses in the United States. We use information obtained from Homebase to track employment indicators at a weekly frequency at the establishment level. The Homebase data set disproportionately covers small firms in food and beverage service and retail, therefore it is not representative of aggregate employment. At the same time, the Homebase data are quite useful for evaluating the employment impacts of the PPP specifically since many hard-hit firms are in the industries Homebase covers and much of the early employment losses came from these firms. We use the Homebase data in our bank exposure and matched sample analysis to measure the impact of PPP funding on employment and business shutdowns.

To broaden this analysis, we supplement the Homebase data with three additional data sources. First, we obtain county-by-week initial unemployment insurance claims from state web sites or by contacting state employment offices for data. Second, we supplement the Homebase data with data from Womply, a company that aggregates data from credit card processors. The Womply data includes aggregate card spending at small businesses at the county-industry level, defined by the location where a transaction occurred. We complement these data sources with additional county-level employment data from Opportunity Insights,[11] which are described in detail in Chetty, Friedman, Hendren and Stepner (2020).[12] The employment rates are based on the data from Paychex, Earnin, and Intuit. We additionally obtain counts of COVID-19 cases by county and state from the Center for Disease Control and use data on the effectiveness of social distancing from Unacast. To understand the mechanisms underlying our results, we draw on data from the Census Bureau's Small Business Pulse Survey (SBPS), a new representative survey that was launched to obtain real-time information tailored towards small businesses. Appendix A provides a more detailed discussion of each data source and final dataset construction.

[10]https://joinhomebase.com/

[11]https://tracktherecovery.org

[12]We also refer readers to Chetty, Friedman, Hendren and Stepner (2020) who provide comparisons between HomeBase and aggregate employment, showing that it provides an overall good glimpse of employment dynamics.

4 Program Targeting and Bank Performance

4.1 Paycheck Protection Program Exposure

Table 1 shows summary statistics for the 20 largest financial institutions in the United States, as measured by total assets. The second and third columns show the share of total PPP volume and the share of the small business loan (SBL) market, respectively, for each institution as of the fourth quarter of 2019. The fifth column presents relative bank performance in terms of loan volume, which is measured as

$$PPPE_{b,Vol} = \frac{\textit{Share Vol. PPP} - \textit{Share Vol. SBL}}{\textit{Share Vol. PPP} + \textit{Share Vol. SBL}} \times 0.5$$

where *Share Vol. PPP* is the share of PPP volume for bank b, and *Share Vol. SBL* is the bank's small business loan volume share. The next three columns present similar information to columns (2) through (4) using the share of the total number of PPP and SBL loans rather than their dollar volume, where

$$PPPE_{b,Nbr} = \frac{\textit{Share Nbr. PPP} - \textit{Share Nbr. SBL}}{\textit{Share Nbr. PPP} + \textit{Share Nbr. SBL}} \times 0.5.$$

Share Nbr. PPP is the share of the number of PPP loans made by bank b, and *Share Nbr. SBL* is the bank's total small business loan market share, based on the number of loans outstanding on each bank's balance sheet as of the fourth quarter of 2019.

Figure A.1 shows the distribution of $PPPE_{b,Nbr}$ (henceforth, PPPE) as of the end of the first round of the PPP (April 15th, 2020) and when the flow of second round funds approximately ends (June 30th, 2020).[13] These histograms provide a window into the evolution of bank performance in deploying PPP loans. Recall that values close to -0.5 indicate little to no participation in the program relative to a bank's initial small business lending share. Both histograms show a wide dispersion of relative performance, with some banks overperforming their expected share of PPP loans given their share of small business loans, while many other banks significantly underperform.

The second round histogram shows a shift in PPPE, with some banks that barely participated in the first round considerably improving their performance subsequently. This improvement

[13] Although the PPP application window continued into August, we refer to the end of June as the end of the second round because nearly all funds were disbursed by then.

allowed these banks to attain a share of the PPP program more commensurate with their share of the small business lending market. Yet, there remains a wide spread between banks. If most eligible borrowers ultimately received funding, this pattern suggests considerable reallocation of borrowers across lenders during the program.

Figure 1 shows the cumulative share of PPP and SBL lending by all banks at the end of the first (Panel A) and second funding rounds (Panel B). There are significant dislocations between the share of PPP lending of underperforming banks and the share of PPP that we would expect had these banks issued PPP loans in proportion to their share of the small business lending market. The blue hollow triangles and red hollow circles represent, respectively, the cumulative share of the PPP and small business lending of banks whose PPPE is below a certain threshold.

The plot provides visual evidence that many lenders underperformed in terms of PPP disbursement. The red hollow circles, representing all small business lending, generally follow a straight line. If there were no heterogeneity in PPP performance, the blue hollow triangles would follow a similar pattern. This is not the case, and the S-shaped pattern indicates that many banks disbursed relatively few PPP loans, while roughly a third disbursed half the PPP loans. Panel A shows that commercial and savings banks representing 20% of the small business lending market simply did not participate at all in the first round of the program ($PPPE = -0.5$). At the end of the first round, the group of banks whose share of the program was below their share of the small business lending market ($PPPE < 0$) made less than 20% of the PPP loans, but account for approximately two-thirds of the entire small business lending market. The top-4 banks are central to this fact, as Table 1 shows that these banks accounted for 36% of total pre-policy small business loans, but disbursed less than 3% of all PPP loans in the first round. Panel B shows that these dislocations became less pronounced during the second round, which accounted for 30% of total PPP lending. In the second round, the banks that underperformed in the first round were able to catch up and partly close the performance gap. Overall, the evidence is consistent with substantial heterogeneity across lenders in their responses to the program's rollout.

4.2 Bank Performance Over Time

Figure 2 traces the evolution of PPP lending over time and by bank size using different metrics. We plot cumulative average PPPE using a number-based approach (Panel A), average PPPE using a volume-based approach (Panel B), average loan size (Panel C), and the fraction of loans above $1 million (Panel D). Panels A and B show that banks with total assets below $50 billion deployed a greater share of PPP loans relative to their respective share of small business loans. Conversely, large banks underperformed relative to their share of small business lending. The differences in bank PPPE across categories of bank size were very large throughout the first round. These differences partly converged at the beginning of the second round.[14] In spite of this partial convergence, large banks still largely underperformed, which is consistent with a few press accounts suggesting that clients of large banks were frustrated by their banks' inability to process PPP in the first round and switched to smaller banks and non-banks that were able to accelerate their loan applications. As demand for PPP funds waned during May, the evolution of bank PPPE across size categories stabilized.

Figure 2, Panels C and D suggest that all banks made larger loans in the earliest weeks of the program. The average size of loans declines significantly over time and jumps down at the beginning of the second round. Nearly 50% of the loans disbursed by banks whose total assets ranged between $50 billion and $1 trillion were over $1 million as of April 3rd. That figure falls to roughly 30% by April 8th and 20% by April 13th. By April 18th, loan sizes across banks of different sizes begin to converge between $200,000 and $450,000. This fact may be consistent with higher awareness and sophistication by larger borrowers (Humphries, Neilson and Ulyssea, 2020), or with banks prioritizing certain customers, such as existing loan customers who tend to be larger.[15] Interestingly, the top-4 banks disbursed a relatively smaller fraction of large loans compared to other large banks, which likely reflects the large number of microbusinesses and small businesses connected to these banks, especially in urban regions.

Overall, these findings suggest that the banking system did not play a neutral role in mediating the allocation of PPP funds during the program. There were large differences in performance across banks, which likely reflect differences in the ability and willingness of banks

[14]The differences were noted in the the popular press. For example a April 6 Wall Street Journal Article "Big Banks Favor Certain Customers in $350 Billion Small-Business Loan Program" (https://www.wsj.com/articles/big-banks-favor-certain-customers-in-350-billion-small-business-loan-program-11586174401) and a July 31 Wall Street Journal Article "When Their PPP Loans Didn't Come Through, These Businesses Broke Up With Their Banks" (https://www.wsj.com/articles/when-their-ppp-loans-didnt-come-through-these-businesses-broke-up-with-their-banks-11596205736).

[15]See for example www.cbsnews.com/news/paycheck-protection-program-big-banks-loans-larger-clients-over-smaller-businesses/. It is also the case that sole proprietors, who represent approximately 15% of total PPP loans, were only allowed to apply with a delay that likely excluded many such firms from accessing funds until the second round.

to respond to the sudden influx of PPP applications.[16] In the second round, most underperforming banks were able to improve their bank PPPE and ultimately process a number of PPP applications more commensurate with their overall share of the small business lending market. Despite this improvement, differences in first round performance resulted in substantial differences in the timing of access to the program because of the first-come-first-served nature of the program and limited first round PPP budget. Appendix Figure A.3 shows that exposure to underperforming PPPE banks is associated with meaningful differences in when borrowers could access funds. Only 25% of all PPP borrowers located in ZIP codes whose banks underperformed obtained PPP approval prior to the end of the first round. By contrast, approximately 42% of all PPP borrowers in ZIP codes whose banks overperformed had access to funds in the first round.

4.3 Geographic Exposure to Bank PPP Performance

Significant heterogeneity across lenders in processing PPP loans would not necessarily result in aggregate differences in PPP lending across regions if small businesses can easily substitute to lenders that are willing to accept and expedite applications. If many lenders, however, prioritize their existing business relationships in the processing of PPP applications, firms' pre-existing relationships might determine whether and when they are able to access PPP funds. In this case, the exposure of geographic areas to banks that underperformed as PPP lenders might significantly determine the aggregate PPP amounts received by small businesses located in these areas.

To examine if geographic areas that were exposed to underperforming banks received less PPP lending overall, we construct regional measures of PPPE by distributing bank-level PPPE based on the share of deposits of each bank in a region. We first consider the distribution of PPPE during the first round of funding. Appendix Figure A.4 presents a national map of

[16]For example, Wells Fargo was severely constrained from expanding its balance sheet as a result of an asset cap imposed by the Fed in the aftermath of the fake accounts scandal. This asset cap was only lifted on April 10 (https://www.wsj.com/articles/fed-eases-wells-fargos-asset-cap-to-lend-to-small-businesses-harmed-by-coronavirus-11586360144), when the Fed excluded PPP loans from the formula it uses to restrict Wells Fargo's growth. The asset cap limited Wells Fargo's ability to lend under the PPP in the early days for the first phase of the program. Appendix Figure A.2 shows that counties with greater presence of Wells Fargo branches received significantly less PPP during the first round. Another piece of evidence suggesting that banks' abilities to process loans differed substantially across banks and that such differences are associated with the bank's PPP performance is presented in Appendix Table A.1. In that table, we show that the ability of banks to include information about the program in their websites and to receive online applications is positively associated with bank performance.

county-level PPPE using the first round distribution of PPP funds and a map of ZIP-level PPPE for the Chicago and New York metro areas. Exposure varies across the United States with some western areas containing a large Wells Fargo presence exhibiting much lower levels of PPPE. More rural areas in the Midwest and Northeast show higher PPPE. Within the NYC metro area, the distribution of PPPE tends toward less affluent and less densely populated ZIP codes but the same is not the case in the Chicago area, which probably reflects the heterogeneous role of exposure to bank PPP performance.

Table 2 reports the results of bivariate regressions of PPPE exposure on ZIP-level observables. The variables are normalized so that coefficients can be interpreted as the effect of a one-standard-deviation change. The table confirms our earlier descriptive evidence–the top-4 banks disbursed significantly fewer PPP loans relative to their overall market share, while institutions with smaller deposits performed better relative to their small business market share. These patterns manifest in terms of regional PPPE. Perhaps surprisingly, ZIP codes with a greater fraction of SBA approved lenders that approved a regular SBA loan in the past three years and ZIP codes with more branches per capita have slightly lower PPPE.

The table suggests that early PPP disbursement may have been targeted towards areas less affected by the economic effects of the pandemic. More populous areas, areas with higher population density, as well as areas with higher COVID-19 cases and deaths also see lower PPPE. Areas where greater social distancing took place, as measured by declines in movement, see higher PPPE. While there is no statistically significant relationship between unemployment and PPPE, areas that saw a greater revenue drop prior to the start of the program also see higher PPPE.

Figure 3 explores the relationship between PPPE exposure and PPP lending at the state-level.[17] Figure 3, Panel A plots the fraction of all small businesses receiving PPP loans in each state during the first round of lending. There is a strong positive relationship between PPP lending and PPPE at the state level. States with the highest PPPE saw nearly 40% of small businesses receiving PPP funding in round one; states with the lowest PPPE saw just 10% of small businesses receiving funding.[18]

[17]Figure 3 plots the relationship between the percent of firms receiving funds and numbers-based PPPE. Appendix Figure A.5 shows the same relationship holds for PPP amounts per firm in round one and volume-based PPPE.

[18]A potential concern is that the fraction of establishments receiving PPP at a regional-level includes the total count of PPP loans in the area regardless of whether the PPP recipient is a self-employed individual or a sole proprietorship but the total number of establishments from County Business Patterns does not include these

A potential concern with these results is that the causality runs reverse. That is, banks do relatively better where demand for PPP loans is abundant. To address this concern, we compare survey measures on firm applications and PPP receipt. The Small Business Census survey includes questions on both PPP application and receipt. Figure 3, Panel B compares PPPE to the percentage of businesses in each state reporting having applied for but not yet received PPP funds as of the end of round one in each state. The difference between PPP application and receipt is much lower in states with higher exposure to banks that allocated more PPP funds. In other words, even conditional on applying, businesses were more likely to receive PPP funds in states where there were more banks allocating funds.

Figure 4 explores the relation between exposure to bank PPP performance during the first round and PPP lending at a finer geographic level. Specifically, we compute the PPPE of each county during the first round and we partition counties in bins based on their PPPE after demeaning using the average PPPE of their respective state to ensure that the empirical relations hold when we use only within-state variation. A strong positive relation between county PPPE and the fraction of businesses receiving PPP at the end of the first round (the week of April 12-18) further supports the idea that the allocation of funds following the first round was shaped by the exposure to the performance of local banks.

Figure 4 also shows that the strong positive relation between county PPPE and the fraction of businesses receiving PPP persists over the following weeks but becomes gradually weaker later in May and into June. We interpret this pattern as further suggesting that the relation between PPPE and the fraction of businesses receiving PPP is driven not by differences in demand for PPP loans across regions but rather by their exposure to banks that underperformed. Otherwise, this positive association would not necessarily disappear over time and areas with weak PPPE would continue to show a lower fraction of businesses receiving PPP. Instead, the positive relation between county PPPE in the first round and the fraction of businesses receiving PPP over time slowly weakens, suggesting either that underperforming local banks improved their performance in deploying PPP over time or that small businesses in areas where local banks underperformed were able to obtain funds from other non-local lenders.

We further probe the relation between local PPPE and the allocation of PPP funds in Table 3. We compute the local exposure to bank performance at the ZIP level by taking the weighted

establishments if they do not have wage workers on their payroll. Figure A.21 shows that when we exclude self-employed individuals and sole-proprietorships from the analysis we obtain PPPE measures that very highly strongly correlated with the ones we use in the main analysis.

average of bank PPPE for all branches within ten miles of the center of the respective ZIP code. We then assess the association between ZIP PPPE and the fraction of businesses receiving PPP in that ZIP-by-industry group after conditioning on county-by-industry fixed effects. Thus, we evaluate whether businesses within the same county and industry had different fortunes in obtaining PPP loans because they were located in ZIP codes whose nearest banks performed relatively well in deploying PPP funds compared to businesses in the same county and industry but in ZIP codes whose banks underperformed.

Columns (1) and (2) of Table 3 further support the idea that local exposure to banks that underperformed in the PPP had a negative impact on the ability of businesses to obtain PPP funds during the first round. Even within a given county and industry, being within 10 miles of banks that underperformed in the first round was associated with a significantly lower share of businesses receiving PPP during the first round. In columns (3) and (4), we assess whether this impact persisted through both rounds of the program. Consistent with the findings we discussed above, local ZIP exposure to banks that over- or underperformed in the first round is not significantly associated with the total fraction of businesses receiving PPP after both rounds of the program.

A potential explanation for these findings is that many banks that underperformed during the first round improved their performance in subsequent weeks. To evaluate this conjecture, we compute a PPPE measure based on the performance of each bank in both rounds. This measure therefore captures whether borrowers in a ZIP code are exposed to local banks that over- or underperformed during the entire program rather than only during the first round. Columns (5) and (6) of Table 3 show that local exposure to banks that underperformed over the entire program is associated with a lower fraction of businesses receiving PPP. We interpret the collection of findings in columns (3) through (6) as indicative that exposure to underperforming banks in the first round forced many borrowers to wait additional weeks to receive PPP funds but did not necessarily affect whether they ultimately received PPP funds. When program funds were replenished, many banks were able to process delayed applications. However, areas served by banks that were permanently unable or unwilling to offer PPP loans received a significantly lower fraction of loans per establishments.

Another possible reason for the gradual weakening of the relation between local PPPE and the fraction of businesses receiving PPP is that non-local banks and non-banks stepped in to substitute for underperforming local banks. To investigate this possibility, we decompose the total fraction of establishments receiving PPP in each ZIP and industry into the fraction of establishments receiving loans from local banks (defined as banks with a branch within 10 miles of zip), non-local banks (defined as all banks with branches that are farther than 10 miles from the zip), credit unions, Fintech companies, and all other non-banks participating in the PPP. Figure 5 shows the average fraction of establishments receiving PPP during round one, round two, and the entire program by source of PPP funding. The chart shows that only approximately 20% of all establishments were able to obtain funding during the first round, and local banks accounted for most of these loans. Fintech lenders and non-banks participated very little during the first round. During the second round, local banks still accounted for the majority of disbursed loans, but both Fintech lenders and especially non-local banks participated to a much larger extent, consistent with these non-bank institutions substituting for local banks in the area. Over the entire program, local banks accounted for more than two-thirds of all loans, while Fintechs and non-banks accounted for five percent of loans.

The collection of results in this section suggests that exposure to bank-specific heterogeneity in their willingness and ability to extend PPP loans was a significant determinant of the allocation of PPP loans in the economy. Next, we examine how the PPP allocation and exposure to bank performance correlated with the local magnitude of the epidemic.

4.4 Are PPP Allocations Targeted to the Hardest Hit Regions?

Were PPP funds disbursed to geographic areas that were initially most affected by the pandemic? Given that one of the policy goals of the program was to inject liquidity into small businesses and prevent unnecessary bankruptcies, we examine whether funds flowed to distressed areas with more pre-policy economic dislocation and disease spread. In addition, we ask whether the significant heterogeneity in bank performance and exposure to bank performance across regions played an important role in the targeting of the program.

Figure 6 partitions the distribution of ZIP codes according to the ratio of PPP loans in the first round to the number of establishments in the ZIP code (Panels A and C) and according to first round ZIP-level PPPE (Panels B and D). We then compare areas with high and low PPP allocations in terms of economic and health outcomes prior to any funds being distributed. In Panel A, we observe a negative relationship between the share of business shutdowns in the week of March 22nd–March 28th and the share of businesses receiving PPP in round one.[19]

[19]Following Bartik, Betrand, Lin, Rothstein and Unrath (2020), we define a business shutdown as businesses

In Panel C, we repeat the analysis using the decline in hours worked between January and the week of March 22nd–March 28th. An analogous relationship holds, with regions receiving more PPP funding during the first round displaying smaller shocks in terms of the initial decline in hours worked. Panels B and D display similar relationships using ZIP-level PPPE. The strong correspondence between the patterns we observe between both the PPP fraction (Panels A and C) and PPPE (Panels B and D) and the pre-PPP employment outcomes also gives us confidence that our measure of PPPE is detecting meaningful variation in the banking structure that mediated the disbursement of funds.[20]

The results suggest that PPP funds were not initially targeted towards geographic areas that were most affected by the economic shock, at least in terms of small business employment declines. If anything, PPP funds in the first round were disproportionately allocated to geographic areas that were less hard hit by the first wave of the crisis. Appendix Figure A.8 confirms our findings using the Homebase data with another public data source—we find no consistent relationship between PPP allocation and bank exposure with state UI claims.

We also explore whether funds initially flowed to areas with early pandemic outbreaks. Appendix Figures A.9 shows that there is a slight negative correlation between loans and PPPE with COVID-19 confirmed cases and deaths at the state level. The figure shows similar results to Figure 6 using COVID-19 cases and deaths. Figure A.10 indicates that states with earlier shelter-in-place orders—which were presumably harder hit by the pandemic—saw lower fund allocations during the first round. The figure additionally shows that there is little correlation between the magnitude of social distancing at the state level and PPP allocations. The totality of the evidence suggests that there was little targeting of funds in the first round to geographic areas that were harder bit by the pandemic and, if anything, areas hit harder by the virus and subsequent economic impacts initially received smaller allocations.

Our interpretation of these results is that between the first and second round of the program, non-local banks and non-banks stepped-in and partly responded to the residual demand of businesses in areas that were both exposed to underperforming banks during the first round

that report zero hours worked during a week using the data from HomeBase.

[20]Appendix Figures A.6 plots the relation between the share of business shutdowns and the decline in the ratio of hours worked prior to any funds being distributed and the measures of PPP allocation at the state level. Moreover, Appendix Figure A.7 repeats this analysis at the county level. Across exhibits, the PPP funds disbursed in the first round are not flowing to regions that are most hit by the economic dislocations of the pandemic. In Tables A.2 and A.3, we employ cross-sectional regression specifications at the ZIP level to further support the idea that the PPP funding did not flow to areas with largest pre-PPP declines in employment and ratios of shutdown businesses.

and were more severely hit initially by the pandemic. Thus, the targeting of the program improved during the second round despite the fact that areas with greater pre-PPP economic dislocations remained exposed to banks that performed worse in deploying PPP. These plots further support the conjecture that the use of the banking system to deploy PPP funds had a strong non-neutral impact in their allocation over time.

Our targeting results likely also reflect the pre-existing bank relationships across regions, rather than a problem with implementation: perhaps banks were caught off guard by the pandemic and the corresponding policies taken to social distance. A related factor that may influence these geographic patterns is differential loan demand in harder hit areas. Because PPP support is more generous for firms that maintain their payroll, the program likely appealed more to firms with smaller reductions in their business. To the extent these geographic patterns reflect such differences in loan demand for the first round, the evidence suggests the PPP functioned less as social insurance to support the hardest hit areas and more as liquidity support for less affected firms.

This interpretation remains true when considering both rounds of funding, as the relationship between shock severity and PPP funding turns less negative without turning positive. Our findings are also consistent with the broad eligibility criteria for PPP loans—most firms below the size threshold could apply for funding—and the absence of conditionality in program generosity—loan forgiveness did not depend on shock severity. The argument in Barrios, Minnis, Minnis and Sijthoff (2020) that firm payroll closely predicted PPP loan receipt accords with this view. Nevertheless, our bank-level results also point to an important loan supply factor distorting the distribution of PPP loans, especially during the program's initial rollout.

5 Employment Impacts and Local Economic Activity

5.1 Research Design

Our results on PPP performance differences across banks motivate a research design for evaluating the PPP. The basic idea is to use differences in local area PPP exposure (PPPE) to partition geographies and compare the evolution of local outcomes for high versus low PPPE regions. By exploiting differential exposure to banks that performed poorly in distributing PPP funding during the first round of the program, we can isolate the effect of the PPP from other differences across regions that may drive differences in PPP loan demand. As described above, we map bank level aggregates for PPP lending from the SBA data onto local geographies using measures of local bank branch presence. The research design is akin to a Bartik instrument and therefore relies on the assumption that pre-policy bank deposit shares in particular regions are not correlated with the various outcomes we study, conditional on observables.

We focus our analysis on the time period between the third week of January and the first week of July to study the short- and medium-term effects of the PPP in the immediate aftermath of the pandemic when the injection of liquidity was thought to matter the most for sustaining employment. Starting the sample period in January allows us to establish a baseline period prior to the pandemic, thereby controlling for time-invariant determinants of economic activity within the same location. The PPP began accepting loans on April 3rd and all of the initial funds were exhausted by April 16th. During this period, banks played a key role in allocating limited funds, creating the variation we use to identify the effects of the program. We exploit the fact that firms are located in regions that vary in their exposure to bank performance, which mediates both the level of PPP loan disbursement and its timing. With the second round of funds, which began on April 27th, PPP funding limits were no longer binding and the gap between high and low PPPE exposure regions mostly closed. Thus, as we move to study the program later in May and June, we will interpret the research design as assigning some firms funding with a lag, instead of as assigning some firms no funding at all.

In our main analysis, we present reduced form regressions of employment and local economic outcomes on PPPE while allowing for separate treatment effects by week or month. Given the rapid nature and size of the economic shock, we highlight two important considerations when analyzing data from this time period. First, our targeting analysis shows that regions receiving more PPP funding were less hard hit by the initial shock, in part due to the banking channel we emphasize. Thus, it is important to properly condition on this non-random assignment of PPP funding. For example, if one does not break out the data finely enough or condition properly for targeting differences—for example by treating all of March as a pre-period benchmark—then one might detect a spurious effect of the program. We show this issue is very clear in week-by-week outcomes around the policy window.[21]

To account for these targeting differences, we estimate the effects of the program by com-

[21] There are a few other ongoing analyses of the PPP that also recognize the presence of targeting. For example, motivated in part by our targeting results, Bartik, Cullen, Glaeser, Luca, Stanton and Sunderam (2020) exploit the fact that firms connected to bigger banks (especially the top-4) were less likely to receive PPP loans.

paring weeks in the post-PPP period to the two weeks in the post-lockdown, pre-PPP period. The pre-lockdown period serves as a baseline for constructing establishment-level employment shocks in our Homebase analysis. We also include time-varying controls and state-by-time-by-industry fixed effects to estimate treatment effects under weaker versions of the Bartik assumption. Once we adjust for targeting differences, including these more restrictive controls has little effect on our estimates.

A second consideration is that research designs that exploit differences in PPP receipt or application without an instrument for loan supply or eligibility will likely overstate the impact of the program. Demand for PPP loans is likely correlated with omitted firm-level factors, such as whether the firm anticipates being able to use the funds during the forgiveness window. Our PPPE instrument attempts to isolate loan supply drivers independent of loan demand. An alternative strategy pursued by Chetty, Friedman, Hendren and Stepner (2020) (henceforth, CFHS) and Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner, Villar and Yildirmaz (2020) (henceforth, ACCGLMPRVY) uses variation from above and below the PPP eligibility cutoff, with the latter study also controlling for industry-by-week and state-by-week effects. Relative to our approach, this strategy has the strength that it focuses on a fixed program characteristic as an instrument. However, the estimates from this approach are local to the eligibility cutoff, while nearly all loans and the majority of funds were received by much smaller firms. In addition, this approach cannot be used to evaluate the broader aggregate impact of the program on local labor markets.

5.2 Small Business Employment

A significant portion of the policy and media interest in the PPP concerned the program's potential employment effects. Previous work has shown that credit market disruptions can have large effects on employment Chodorow-Reich (2014), which may have in part motivated the quick policy response. We examine business shutdowns (i.e., hours worked reduced to zero during the entire week), declines in hours worked, and unemployment insurance claims and find no evidence of substantial effects of the PPP on these outcomes, and at most modest effects.

Figure 7 presents simple difference-in-difference graphs for each of our employment and local activity outcomes. We divide all firms in the sample based on whether they are located in regions with above- or below-median PPPE. We use PPPE measured at the ZIP level for

Homebase analyses and at the county level for other less granular outcomes. We use vertical markers to demarcate the post-lockdown, pre-PPP period; the post-PPP launch; when the first round of PPP funds are exhausted; and when the second round of PPP funding begins. The top left panel shows business shutdowns, the top right panel shows hours worked, the bottom left panel shows initial UI claims and the bottom right panel shows small business revenue.

For both measures of small business employment, we see a dramatic decline in employment starting in the week prior to the lockdowns. Consistent with our targeting results, this decline is modestly larger for regions with low PPPE. Importantly, during the first round of PPP, the gap between high and low PPPE areas does not widen further, indicating little incremental impact of PPP during this time. As time progresses, the gap for the ratio of hours worked widens gradually during May, which suggests intensive-margin employment effects, while the gap for shutdowns remains unchanged. Unemployment claims show a similar pattern, with an initial gap opening up the week before PPP funds are disbursed and then remaining relatively stable over time. Business revenue declines sharply in the week prior to the lockdowns, and levels are quite similar for regions with low and high levels of PPE through April but opening modestly in May.

Figure 8 plots coefficients and standard errors for regressions of differences in employment outcomes on exposure to PPPE. We estimate weekly regressions of the form:

$$\Delta y_{ijnt} = \alpha_{sn} + \beta PPPE_j + \Gamma X_{ijnt} + \epsilon_{ijnt}$$

where Δy_{isjn} is the difference between the outcomes y_{isjn} (shutdown, hours decline, UI claims, revenue) of firm or region i in each week relative to the average value in the two weeks prior to the PPP launch; $PPPE_j$ is the exposure of region j (county or ZIP) to bank PPPE; α_{sn} are state-by-industry fixed effects; and X_{ijnt} are additional control variables.

The top-left and top-right panels plot the estimates where the outcome variables are differences in business shutdown and decline in hours worked relative to January, respectively, both measured in Homebase at the establishment level. The bottom-left panel plots estimates where the outcome variable is the difference in the ratio of initial unemployment filings to total employment at the county level. In the bottom-right panel, the outcome is the difference in average small business revenues for small businesses within 3-digit NAICS industry and county.

The coefficients capture the effect of PPP exposure on the outcome of interest under the identifying assumption that the firms and areas differentially exposed would have trended similarly in the absence of the PPP. Given the fast-moving employment losses and differential state policies, the choice of baseline and fixed effects are particularly important. We account for differential targeting by using as a baseline the two weeks prior to PPP funds being disbursed, which is consistent with the aggregate time series in Figure 7. The state-by-industry-by-week fixed effects imply that we are comparing trajectories for firms within state-by-industry groups and allowing general time trends within these groups. Focusing on within-state estimates is particularly important because many lockdown and reopening policy decisions occur at the state level. There is some evidence that state shutdown orders impacted the decline in economic activity (Goolsbee and Syverson, 2020).

The results align with the raw differences across high and low PPPE regions in Figure 7. We see little effect on any outcome until the end of May, and on business shutdowns and UI filings until the end of the sample period. Beginning in May there are statistically significant positive effects on hours worked and local area small business revenues.

Table 4 presents our difference-in-difference estimates, in which we pool the weekly effects into months. We estimate the following specification:

$$\begin{aligned} E_{isjnt} = \alpha_i + \delta_{jnt} &+ \beta_1 \mathbb{1}[\text{April}] \times PPPE_j \\ &+ \beta_2 \mathbb{1}[\text{May}] \times PPPE_j \\ &+ \beta_3 \mathbb{1}[\text{June}] \times PPPE_j + \gamma X_j + \varepsilon_{isjnt}, \end{aligned}$$

where E_{isjnt} is an outcome (business shutdowns or the decline in hours worked) for firm i in state s, region j (county or zip), and industry n in week t. The term α_i captures firm fixed effects, δ_{snt} are state-by-industry-by-week fixed effects, $PPPE_j$ is regional PPP exposure based on round one fund disbursements, and ε_{isjnt} is an error term.

The coefficients β_1, β_2, and β_3 capture the differential effect of PPP exposure on the outcome of interest during each time period relative to the two weeks prior to the launch of PPP. The identifying assumption is that the firms differentially exposed would have trended similarly in the absence of the PPP, conditional on fixed effects and controls. Due to the inclusion of state-by-industry-by-week fixed effects, we are comparing trajectories for firms within state-by-industry groups and allowing general time trends within these groups. The coefficient β_1 captures differences between firms after the initial rollout of the PPP, when most regions remained under some form of shelter-in-place order. The coefficient β_2 captures effects in May, as many regions began to lift restrictions. The coefficient β_3 captures effects as PPP funding ended in June and state reopenings continued.

In the top panel, the outcome of interest is business shutdowns, while in the bottom panel it is the decline in hours worked. All columns include state-by-industry-by-week fixed effects. The first two columns and second two columns exclude and include firm fixed effects, respectively. The second and fourth columns add pandemic controls, which include social distancing and COVID-19 cases and deaths per capita measured prior to PPP interacted with month fixed effects.

The table confirms the finding of no statistically or economically significant relationship between PPP bank exposure and these employment outcomes in April, during the initial month that PPP loans were offered. Moreover, our least squares estimates are not simply statistically insignificant with large confidence intervals; rather, they are precise zeros. In May and June, we continue to find precise zero effects for business shutdowns. In contrast, the decline in hours worked measure increases for firms with higher PPP exposure in May and June. The effect sizes are small—approximately 2 percentage points in May and 3 percentage points in June for a unit increase in PPPE—but highly statistically significant.

As another way of interpreting our magnitudes, consider the following comparison. The difference between PPPE for top versus bottom quartile ZIP codes is 0.44. This difference implies an increase in the share of establishments receiving PPP funding of 8.4 percentage points, which is large relative to the mean level of 22%.[22] Using the reduced form estimates for April in Table 4, column (4), this change in funding implies a increase in the probability of firm shutdown of 0.1 percentage points ($= 0.44 \times 0.002$). The lower bound of the 95% confidence interval is well below a one percentage point effect. An analogous calculation for the decline in hours worked gives similarly small effect sizes. Thus, relative to the aggregate patterns in Figure 7—a 40 percentage point increase in the probability of firm shutdown and 60 percentage point reduction in the ratio of hours worked relative to January—we can reject modest effect sizes during this period.

As we move into May and June, the results for business shutdowns do not change. However, the effect sizes for the decline in hours worked increase. In May, the point estimate of 0.019 implies an increase in hours worked of 0.8 percentage points ($= 0.44 \times 0.019$) with a

[22]This calculation comes from 0.44×0.19, which is the coefficient of PPP per establishment as of the end of round one on PPPE in a ZIP-level regression with state fixed effects.

95% confidence upper bound of 1.3 percentage points ($= 0.44 \times (0.019 + 1.96 \times 0.005)$). The analogous estimates for June are 1.3 and 1.8 percentage points, respectively. Because the second round of funds did not reach firms until late in May, our research design can be interpreted as comparing firms that did receive funds to those that did not for April and May. In June, the research design is better interpreted as reflecting differences between early and late recipients. Thus, our estimates may be conservative regarding the overall employment effects of the program by this point in time. On the other hand, if many firms that did not receive funds early decided to close permanently, then our estimates for June can be more easily compared to those in April and May.

Our results are consistent with contemporaneous evidence from other researchers using different data sets and research designs. CFHS use high frequency employment data from Homebase and Earnin and study the evolution of employment outcomes for firms above and below the 500 employee PPP eligibility threshold. They also find insignificant effects of the PPP on employment. ACCGLMPRVY use payroll data from ADP, a large payroll processor, and also use the 500 employee threshold design to estimate employment effects. They find that the PPP boosted employment at eligible firms by 2–4.5%.

Relative to this approach, our research design has a few benefits. First, it is not local to firms around the 500 employee threshold; most PPP borrowers are considerably smaller. Second, that design requires smaller firms and larger firms to trend similarly around the reform, which is a strong assumption if smaller firms are more vulnerable to shocks and because the PPP coincided with other programs operated by the Federal Reserve to help larger firms. Third, we use our design in the next section to study impacts on aggregate local labor market and economic outcomes, which is not feasible with the threshold design. Nevertheless, it is informative that similar results emerge from different data sets and research designs.[23]

Aggregate Impacts. We consider two approaches to aggregation. Our first approach follows Mian and Sufi (2012) and Berger, Turner and Zwick (2020). We estimate the total employment gains caused by the program in its first three months, exploiting only differences in cross-

[23] Another reason we may find smaller effects than ACCGLMPRVY is that our data measure hours worked while their data measure payroll. If firms partly deploy PPP to compensate furloughed workers who remain functionally unemployed, then this difference in measurement could account for some of the gap between our estimates. Our results using the Census Household Pulse Survey data lean against this interpretation. In Figure A.12 we find that only 12% of households report receiving any payment for time not working in the previous week. Moreover, the share of households reporting receiving no pay is not associated with exposure to State PPPE.

sectional exposure and using the group receiving the smallest shock as a counterfactual. We choose the bottom 1% of ZIPs as the counterfactual group and compute the effect of the policy for other groups relative to this group. By construction, any time-series effect of the policy shown by the bottom group is set to zero and removed from the effect computed for other groups.[24]

Standardized exposure for the bottom group is −2.03 and increases to 2.49 for the highest group. Thus, for exposure group g, the aggregate increase in employment induced by the program is

$$\Delta Y_g = \beta_t \times (e_g - (-2.03)) \times Y_{g,pre}$$

where β_t is our preferred reduced form estimate, e_g is weighted-average program exposure where the weights are estimated eligible employment in each ZIP, and $Y_{g,pre}$ is within-sample pre-program employment. A less conservative approach aggregates estimates relative to a zero-exposure baseline, which equals −3.28 in standardized exposure. We can then average the more and less conservative approaches. We choose $\beta_t = 0.017$, the mean of the three monthly coefficients from Table 4, Panel B, column (4).

Following this approach, we estimate the PPP increased employment by 45,000 within sample during the first three months of the program, or 3.4% of pre-program employment of 1.33 million. Note this is a lower-bound estimate if the lowest exposure ZIP also responds to the program. When we aggregate relative to a zero-exposure baseline, we estimate an increase of 74,000 within sample, or 5.5% of pre-program employment. Averaging the more conservative and more aggressive estimates yields an estimate of 4.5%.

The second approach to aggregation follows ACCGLMPRVY. We convert the intention-to-treat estimate from our banking exposure design into an average treatment effect and then adjust for the mean take-up rate as of the end of the program's first round. Formally, consider

$$\text{Total Payroll Effect}_t = \underbrace{\delta_t \times \gamma}_{\Delta_t} \times N$$

where Δ_t is the intention-to-treat estimate for period t, δ_t is the treatment-on-the-treated estimate for period t, γ is the take-up rate of PPP, and N is the number of employees at PPP-

[24] As is the case for any aggregate estimates that rely on cross-sectional identification net of time fixed effects, we cannot observe a counterfactual that measures general equilibrium effects. This is another reason why producing estimates with different assumed counterfactuals can inform the range of plausible aggregate impacts, in addition to demonstrating the degree of sensitivity of results to different assumptions.

eligible firms. ACCGLMPRVY find $\Delta_t \in [2\%, 4.5\%]$ in their research design based on the firm size eligibility cutoff. We follow ACCGLMPRVY and use $N = 70M$ based on Census data on employment in small establishments.

We estimate an instrumental variables (IV) version of our bank exposure design at the ZIP-level, with the effect of ZIP-level PPPE on the fraction of eligible establishments receiving PPP funding as the first stage. The IV estimate for the effect of PPP receipt on the change in hours worked in May is 0.33 (s.e.=0.10) with a first-stage coefficient of 0.24 (s.e.=0.03). Mean take-up as of the end of round one in our data is 24%. Thus, our estimate of β is 7.8% (s.e.=2.5%) for the month of May. Analogous specifications for April and June yield estimates of -0.1% (s.e.=1.2%) and 12.7% (s.e.=3.0%), respectively. Pooling all three months yields an estimate of 6.8% (s.e.=2.0%).[25]

Taking both aggregation approaches into account, our preferred aggregate response ranges from 3.2 million to 4.8 million (either 4.5% or 6.8% of 70 million eligible workers). It is important to keep in mind that these estimates derive from the particular subset of eligible firms represented in Homebase, so extrapolating within-sample estimates to the broader economy does require an assumption about representativeness. Relative to ACCGLMPRVY, whose preferred estimates range from 2% to 4.5%, our estimates are slightly larger but quite similar. We also estimate an increasing treatment effect over the course of the program's first few months, whereas ACCGLMPRVY find an immediate response that appears stable over time. While we do not want to overstate these differences, they may reflect the fact that our estimates feature smaller firms who may be more responsive to stimulus policy (Zwick and Mahon, 2017) and who may have been less able to increase employment while shelter-in-place orders remained in force. These firms are more representative of the overall population of PPP recipients, so our results might be especially informative about the program's overall impact during this time.[26]

When considered relative to the scale of the PPP program, the employment effects we estimate are fairly modest. The program disbursed $525 billion in total loans, which implies a cost-per-job ranging from $109,000 to $164,000.[27] Incorporating the saved funds from lower

[25]Weighting regressions by pre-program employment results in effect sizes that are 2 to 3 percentage points larger. The sample comprises mainly small firms with median pre-program employment of 27 and mean pre-program employment of 37 and very few firms with more than 100 employees.

[26]CHFS also find a small and statistically insignificant response (approximately 2%) over the program's first few months using a research design similar to ACCGLMPRVY and data from the Opportunity Insights tracker. Hubbard and Strain (2020) also use the firm size threshold design find modest employment and firm closure effects using data from Dun & Bradstreet, as well as small effects on an index measure of financial vulnerability.

[27]These cost-per-job estimates might be misleading if the employment gains are especially short-lived. Given

unemployment insurance claims (roughly \$5–10K per worker) only modestly alters this calculation. Firms applying for PPP loans reported 51 million jobs in total supported by the program. When combined with our estimates, an implication is that more than 90% of these supported jobs were inframarginal. If wages for inframarginal workers did not adjust, then the bulk of the program's economic benefits appear to accrue to other stakeholders, including owners, landlords, lenders, suppliers, customers, and possibly future workers.

5.3 Local Labor Market and Economic Activity

The top panel of Table 5 presents results using a broader measure of employment outcomes: initial UI claims. The outcome variable is the difference between initial UI filings in a week and average initial UI filings two weeks prior to the launch of the PPP, measured at the county level and scaled by county-level employment. The results paint a similar picture to those in Table 4. Again, there is no statistically or economically significant relationship between PPP loans or PPP bank exposure and UI claims. While the results are slightly less precise than those using outcomes from the Homebase data, the coefficients imply very modest effects at best. Following the calculations above, the 95% lower bound estimated effect for the month of June is -3.1 basis points ($= 0.44 \times (0.071 - 1.96 \times 0.052)$), which is negligible relative to a mean in the weeks prior to PPP of 3.0 percentage points.

We complement our Homebase analysis with results using employment data from Earnin, an app targeting lower income workers but with broader employer coverage than Homebase. Appendix Figure A.11 repeats the earlier analysis, replacing the main outcomes with the difference in county level employment in a week and its average shutdown status in between March 22 and April 4. The Earnin data suggests a pattern similar to the hours worked estimates, albeit with significant effects appearing earlier in April and slightly smaller magnitudes. We also divide the sample by quartiles of earnings.

The middle panel of Table 5 presents regressions of employment growth in Earnin on county-level PPPE. The estimates suggest very small effects in April that increase modestly in May and June. The 95% upper bound estimate for June is 0.7 percentage points ($= 0.44 \times (0.007 + 1.96 \times 0.004)$). Appendix Table A.4 presents the results from the split-sample analysis, which reveals that the employment effect is insignificant for the lowest earning quartile

the research design and time frame we study, we are not able to provide a more informative cost-per-job-year estimate. See Chodorow-Reich (2019) for a discussion in the context of estimating fiscal spending multipliers.

of workers and larger in magnitude and significant for the top earnings quartile.[28] One reason firms may have selectively deployed PPP funds to higher earning workers is because these workers face lower replacement rates from expanded UI. Another reason is that these workers may be less likely to work in customer-facing roles that were restricted by public health measures, for example, they are less likely to be waiters and bartenders.

While much of the focus of the PPP was in countering a surge in unemployment and business closures, we now explore the potential effects of PPP on the evolution of small business revenues as a proxy for local consumption activity. The bottom right panel of Figure 8 shows a significant relationship arising between small business revenue and PPPE, beginning in April.

We find a slight positive correlation between county PPPE and consumer spending. We find a positive relationship between PPP exposure and small business revenue, which is statistically significant at the 10% level in April and at the 1% level in subsequent months. The relationship is only significant at the 10% level for June once we introduce additional control variables, such as the social distancing index and coronavirus cases and deaths per capita. Although our fixed effects already purge potentially endogenous time-varying shocks arising from state policymaking (e.g., stay-at-home orders and nonessential business closures), local enforcement of these rules, as well as time-invariant heterogeneity across locations, could be correlated with the change in consumer spending. For example, since areas that were more adversely affected by the pandemic have higher concentrations of retail employment, and these areas have lower quality banking relationships, then we are likely to overestimate the effects of consumer spending.

To disentangle these factors, column (3) omits the time-varying county controls and adds county-by-industry fixed effects, which again produces a positive correlation between PPPE in June. The results in column (3) are quite similar to those in column (1), suggesting a positive relationship between PPP exposure and small business revenue. Although the time-invariant heterogeneity is removed, differences in social distancing and coronavirus cases and deaths are likely still correlated with consumer spending. Column (4) validates this hypothesis by introducing the controls and fixed effects, producing a relationship between PPPE and the change in consumer spending, which is only significant at the 10% level.

[28]Following the calculation above, for the highest earnings quartile, the 95% upper bound effect size for the month of June is 1.0 percentage points ($= 0.44 \times (0.013 + 1.96 \times 0.005)$).

5.4 Matched Sample Analysis

We complement our regional estimates with a sample of 1,176 firms, for which we are able to match PPP loan information to payroll information from Homebase. In this analysis, we focus on the timing of the receipt of PPP loans and whether differences in timing materially affected short-term employment outcomes. The public PPP data only include firm identifiers for loans above $150,000, which account for 13.5% of all PPP loans. Thus, for the vast majority of firms we cannot determine whether or when they obtained a PPP loan. We focus on firms that we identify as obtaining a PPP loan, and compare outcomes for firms that received loans earlier versus later. Because this approach is subject to the critique that the timing of loan receipt may reflect differences in loan demand across firms, we also instrument for the timing of receipt using PPPE exposure.

Figure 9 shows the percent of businesses shut down and the ratio of hours worked over time split based on whether they received a loan in the week beginning on April 5, or earlier versus those that received a loan in the week beginning May 3 or later. Vertical lines indicate these two breakpoints, which effectively partition firms in the sample into top, middle, and bottom terciles in terms of when they received loans. In terms of business shutdowns, trends are parallel prior to the disbursement of PPP loans, and then diverge following loan disbursement. In terms of hours worked, we see a gap open up prior to PPP loan disbursement, which may reflect a combination of our targeting results and differences in loan demand. Following PPP disbursement, the gap grows over time. The raw data is suggestive of earlier PPP receipt leading to higher employment and business survival rates.

Table 6 presents results from the individual matched sample exploring the timing of PPP receipt, regressing outcomes on the week in which a firm received PPP. We focus on outcomes in the week of May 2 to May 9, the final week before the second round of PPP loans was disbursed. Therefore, the regression is measuring employment effects using first round recipients as a "treatment" group and second round recipients—who have not yet received their loans—as a "control" group. We estimate the following specification:

$$\Delta y_{isn} = \alpha_{sn} + \zeta WeekPPP_i + \Phi X_{ist} + \varepsilon_{is}$$

where Δy_{isj} is the difference between the outcomes y_{isj} (shutdown, hours) of firm i in the week beginning on May 3, and the average outcome for the same firm during the two weeks prior to the launch of PPP. $WeekPPP_i$ is the week in which a firm received a PPP loan, and thus ζ captures the effect of receiving a PPP loan one week later. The first two columns show OLS estimates, varying industry or state-by-industry fixed effects, while the second two columns show IV estimates, instrumenting the week in which a firm received PPP with PPPE measured at the ZIP level. The terms α_{sn} represent state-by-industry fixed effects and X_{ist} are additional control variables.

The results in Table 6 are largely consistent with our bank exposure results, suggesting modest short-term effects of the PPP. In Panel B, column (3), we find that obtaining a PPP loan one week earlier leads to an increase in hours worked of 4.3% (s.e.=1.5%) for a firm receiving a PPP loan a week earlier. We find no significant effects on business shutdowns in the IV specification. In both cases, when we include state-by-industry effects, the IV estimates become imprecise, which prevents us from drawing strong conclusions for this sample.

A potential concern is that firms that obtained PPP earlier or later may be on different trends. To address this, Figure 9 shows raw trends as well as weekly coefficients of regressions corresponding to Table 6. The top panels show means of business shutdowns and employment for firms that received PPP approval in the week beginning April 5 or earlier to those that received PPP approval in the week beginning May 3 or later, while the bottom panel presents OLS estimates corresponding to column (1) of Table 6. The coefficients closely align with the timing of PPP receipt. We see parallel trends initially, with early and late receipt firms diverging in mid-April after PPP loans were received. By late May, as all firms in the sample begin to receive PPP loans, we see the gap in hours worked close, consistent with the late-receipt firms receiving PPP loans.

6 Interpretation and Mechanisms

We find limited evidence that PPP funding has significant effects on employment or local economic activity during the first month of the program. In the subsequent months, we find more evidence of employment effects on the intensive margin, but can still rule out large employment effects of the program. If firms did not primarily maintain or increase employment, what did they do with these funds?

There are several non-mutually exclusive channels through which businesses may have absorbed the funds without immediate employment effects. First, program eligibility was defined broadly, so many less affected firms likely received funds and continued as they would have in the absence of the funds.[29] In these cases, the program's benefits accrue to the firm's owners. Second, firms retained significant flexibility in how they could use the funds over time and they may have used funds to strengthened balance sheets and on non-employment related expenses. Third, some firms may have increased employment or called back workers. Finally, and related to the first channel, banks may substitute more generous PPP loans for other lending that would have happened otherwise.

6.1 Fixed Payments

To explore the effects of PPP on non-employment financial outcomes, we use information from the Census Small Business Pulse Survey questions on PPP applications and financial conditions. The survey was conducted weekly from April to June, so these data cover the same sample period as our employment analysis. In the top panel of Table 7, we examine whether receipt of PPP allowed firms to avoid becoming delinquent on scheduled payments (either loan or non-loan). We estimate regressions of the relationship between PPP fund allocation and the percentage of firms reporting missing loan payments at the state-industry level. In Table 7, we show similar regressions using the percentage of firms missing other scheduled payments such as rent, utilities, supplier payments, and payroll.[30] In light of our targeting results, these regressions add controls for pre-PPP measures of crisis severity, including the pre-PPP decline in hours worked from Homebase, the pre-PPP counts of COVID cases and deaths per capita, and the pre-PPP social distancing index.

The top panel of Table 7, column (1), indicates that a percentage point increase in the share of firms reporting receiving PPP is not significantly associated with a decline in the percentage of firms missing loan payments. This result, however, could indicate that areas and industries with a lower percentage of businesses receiving PPP had a larger fraction of businesses that were uninterested or unable to apply for funds. To address this issue, we use state PPPE to

[29] Drawing on data from a large survey of business owners on Facebook, Alekseev, Amer, Gopal, Kuchler, Schneider, Stroebel and Wernerfelt (2020) find that 30% to 40% of small businesses did not experience sales declines in the first month of the crisis. Among the businesses that did experience declines, the severity of the decline varies widely from declines of 10% to 20% to nearly complete shutdowns. Moreover, only half of firms surveyed reported struggling to pay obligated expenses (though presumably this share increased over time).

[30] Unfortunately, the Pulse survey does not separate this category of other payments into payroll versus non-payroll components. However, it does focus on "required" payments, which firms may interpret as referring to payments for past labor rather than discretionary payments based on retaining workers going forward. Results for this measure should be interpreted with some uncertainty about respondents' interpretation of the question.

capture geographic differences in access to the supply of PPP funds resulting from differences across regions in their exposure to bank PPP performance. These differences are plausibly unrelated to demand factors and therefore less likely to be confounded by them. In column (2), we run a similar specification, but instead focus on the relation between the percentage of firms reporting missing payments and state PPPE. In this case, an increase in state PPPE is associated with a significantly lower percentage of firms reporting missing loan payments. In columns (3) and (4), we present results of an IV strategy whereby we instrument for the percentage of firms receiving PPP using state PPPE. Using this strategy, we find that a ten percentage point increase in firms receiving PPP is associated with 1.5 percentage point (s.e.=0.5) decline in missing loan payments.

In the top panel of Table 7, we find that a ten percentage point increase in the share of firms receiving PPP is associated with an even larger effect on missed non-loan payments. This result reflects the fact that many small businesses do not necessarily have loans. Instead, their primary fixed obligations are rent payments, utilities, supplier payments, and fixed employment-related expenses. Across all specifications, an increase in the percentage of firms receiving PPP or in the access to PPP funds is associated with a lower percentage of firms reporting missing these types of payments. Specifically, the results of the IV strategy in columns (3) and (4) suggest that a ten percentage point increase in firms receiving PPP is associated with a 4.4 percentage point (s.e.=0.7) decline in the number of firms reporting missing any type of scheduled payments.

Appendix Table A.6 shows that the share of firms reporting missing loan and non-loan payments is positively related to the fraction of small businesses reporting having applied, but not having received PPP funds. The percentage of firms that applied, but not did not receive, PPP funding is plausibly less related to differences in demand for PPP funds across geographies and industries and likely better captures timely access of small businesses to the supply of PPP funds. These results further suggest that PPP might have been crucial in allowing small businesses to make scheduled payments and survive the economic crisis without permanently closing.

6.2 Liquidity Support and Precautionary Savings

The Census survey data also reveal that the PPP funds increased firms' cash on hand. The middle panel of Table 7 shows regressions results examining the relation between access to PPP funds during the first round and the percentage of businesses reporting having three or more months of cash on hand. This exercise also offers a useful sanity check of the informativeness of the survey data. As above, these regressions include controls for pre-PPP measures of crisis severity.

Similar to results on missed payments, the coefficients reported in column (1) of Table 7 do not indicate an economically or statistically significant relation between cash-on-hand and the percentage of firms in that state-by-industry group that reported receiving PPP. However, when we examine the relation between state PPPE, which better isolates access to the supply of PPP funds, access to PPP is economically and significantly related to the share of firms reporting significant liquidity. In the reduced form specification presented in column (2), we find that a unit increase in state PPPE is associated with a ten percentage point increase in the share of firms reporting significant liquidity. In the IV regression, which uses state PPPE to instrument for the share of firms receiving PPP, a ten percentage point increase in the share of firms receiving PPP is associated with a 2.7 percentage point (s.e.=1.3) increase in the share of firms reporting at least three months of cash to cover business operations. Appendix Table A.6 reports similar results when we use the share of small businesses that applied for but did not receive PPP as our right-hand-side variable of interest.

Overall, these results are consistent with the idea that the PPP provided firms with an important liquidity cushion that they used to navigate the initial months of the pandemic. These results also align with our evidence that PPP did not immediately induce employment responses and only modest increased employment in the months following PPP receipt. Many businesses may have retained the PPP funds in bank accounts as precautionary savings until they were ready to resume activities, perhaps as demand for their goods and services return to normal or as relaxed shelter-in-place orders permit them to reopen for business. The fact that many firms increased savings could reflect the fact that precautionary savings are increasing in the costs of financing (Hennessy and Whited, 2007) and uncertainty (Riddick and Whited, 2009). Generally, the results are not consistent with the idea that the PPP served as a large-scale alternative to unemployment insurance for delivering funds directly to affected workers.

6.3 Crowd-Out

One potential mechanism explaining the small employment effects of the program is crowd-out. Government loan programs crowding out private lending has long been a concern for loan guarantee programs (e.g., Gale (1991)). In the counterfactual, PPP loans may have been made under standard commercial loan programs. In the presence of substantial crowd-out, the program would have little effect on employment and other firm outcomes. While we find some evidence of modest crowd-out, the results suggest that magnitudes are small and private lending would not have fully offset PPP lending. This finding is plausible because loans to replace lost revenue would be unlikely to pass a private loan underwriting test.

Appendix Figure A.16 shows suggestive evidence of crowd-out from California Uniform Commercial Code (UCC) filings.[31] The figure shows a significant spike in UCC filings in May, following the exhaustion of PPP funds, which is consistent with the program crowding-out private lending. On the other hand, the time series could simply reflect bureaucratic delays in filing or the recovery. Appendix Figure A.17 shows scatterplots of the ratio of UCC filings per establishment and the county exposure to the number-based PPPE under different time horizons. The relationship is relatively flat, suggesting little relationship between the availability of PPP loans and commercial lending.[32]

While suggestive of some crowd-out, we use the bank Call Report data to explore this pattern more broadly and formally. The Call Reports are quarterly at the bank level, and the first two annual quarters (January-March, April-Jun) almost perfectly align with the disbursement of PPP loans, which began on April 3. The number of loans in the second quarter is given by $C\&ILoans_{Q2} = C\&ILoans_{Q1} + PPP + NL - P$ where $C\&ILoans_{Qi}$ are commercial and industrial loans in quarter i, PPP refers to PPP loans and NL are other new non-PPP commercial loans, and P are loans that are paid or charged-off. We rearrange the equation in terms of quarterly loan growth and write:

$$\frac{C\&ILoans_{Q2}}{C\&ILoans_{Q1}} = 1 + \gamma \frac{PPPLoans}{C\&ILoans_{Q1}} + \zeta \tag{1}$$

[31] We obtained data from California on all UCC filings, which are required for all secured business loans to protect creditor claims. These UCC laws are set at the state-level, although the National Conference of Commissioners has sought to make them fairly uniform across states. We are able to observe the names and addresses of the debtor and the secured property, together with the description of the property that has a security interest. We subsequently match the UCC data with firm-level information from HomeBase. We refer readers to Edgerton (2012) for further details about the UCC data and its features.

[32] Note that PPP loans are unsecured and hence are not included in UCC filings.

The coefficient γ captures crowd-out. If there is no crowd-out, an additional PPP loan leads to one additional total loan and $\gamma = 1$. Under full crowd-out, an additional PPP loan if offset by a reduction in another commercial loan, and $\gamma = 0$. Appendix Figure A.20 plots the ratio of Commercial and Industrial Loans in Q2 2020 and Commercial and Industrial Loans in Q1 2020 and the ratio of PPP loans and Commercial and Industrial Loans in Q1 2020, while Table A.13 reports OLS and IV regressions examining the relation between the ratio of Commercial and Industrial Loans in Q2 2020 and Commercial and Industrial Loans in Q1 2020 ($\frac{C\&I Loans_{Q2}}{C\&I Loans_{Q1}}$) and the ratio of PPP loans and Commercial and Industrial Loans in Q1 2020 ($\frac{PPP Loans}{C\&I Loans_{Q1}}$). Column (2) instruments using lender PPPE. In column (1), the coefficient γ is 0.558 and statistically significant at the 1 percent level. This is suggestive of some crowd-out, but not full crowd-out. The OLS estimates may be biased if $\frac{PPP Loans}{C\&I Loans_{Q1}}$ and ζ are correlated, so in column (2) we instrument using lender PPPE. The IV estimate in column (2) provides an estimate of $\gamma = 0.991$, and we cannot reject no crowd out. The confidence interval of the IV estimate allows us to reject full crowd-out.

Though we find limited evidence of private sector crowd-out, the PPP may have crowded out other federal loan programs, namely, the Economic Injury Disaster Loan (EIDL) program. If firms could obtain other federally guaranteed loans in the absence of PPP, this would also lead to the program having muted effects. The COVID-19 EIDL program is a SBA program that provides economic relief to small businesses that experience a temporary loss of revenue due to the coronavirus. The program offers advantageous terms for regular businesses with interest rates set at 3.75% and maturity of 30 years with no prepayment penalties. Given these terms, it is possible that the second-best option of firms that did not obtain access to PPP was to apply for a loan under the EIDL program.

Appendix Figure A.18 shows cumulative PPP and EIDL lending over time. Similar to UCC filings, we see an uptick in EIDL loans after PPP funds level off in May. The SBA was slower to open up the expanded EIDL provisions of the CARES Act, which may also account for this lagged increase. The fact that EIDL loans only start rising in late May implies that crowd-out of EIDL is unlikely to explain the modest effects we estimate for April and May.

Appendix Figure A.19 shows scatterplots of the average fraction of small business establishments that received an EIDL loan in each percentile bin based on state exposure to the PPPE in the weeks of May 3-9 and June 28-July 4. The figure shows a weak relationship in the early period with very few firms receiving EIDL loans. In the later period, there is a strong negative relationship, consistent with crowd-out and indicating that higher PPP exposure is associated with fewer EIDL loans. If firms that were unable to access PPP were more likely to apply for and receive an EIDL loan, and if EIDL loans were sufficiently good substitutes for PPP loans, that could help account for modest estimated effects of PPP in June.

6.4 UI Expansion

One possible reason why the observed employment effects were so small is that historically high levels of UI made it difficult for firms to recall workers. Indeed, many workers saw UI replacement rates above their usual salaries due to an additional $600 a week in federal benefits (Ganong, Noel and Vavra, 2020). Some commentators and media reports suggested that this benefit led to difficulties for firms in recalling workers, which could have attenuated the employment effects of the PPP.[33] While recent work such as Altonji, Contractor, Finamor, Haygood, Lindenlaub, Meghir, O'Dea, Scott, Wang and Washington (2020) suggest muted effect of UI extensions on unemployment employment levels and the speed of returning to work, we consider this possibility exploiting state variation in UI replacement rates.

We explore whether UI generosity attenuated the employment effects of PPP lending by splitting our sample by the generosity of state UI benefits. Appendix Tables A.14, A.15 and A.16 repeat our employment, UI claims, and small business reveneue analyses, respectively, with the sample divided between states with above- or below-median UI replacement rates. The results do not support the hypothesis that the responses are greater in states with less generous UI. For employment, shutdowns, and revenues, effect sizes are *greater* in high benefit states. For UI filings, effects are qualitatively larger in low-benefit states, but the results are imprecise. It is important to note that, even in states with less generous UI systems, replacements rates were historically high for lower income workers and thus we may be unable to capture the effects of a counterfactual without elevated UI benefits.

[33] For example, the Wall Street Journal "Businesses Struggle to Lure Workers Away From Unemployment" on May 8 (https://www.wsj.com/articles/businesses-struggle-to-lure-workers-away-from-unemployment-11588930202?mod=flipboard) suggested that "Businesses looking for a quick return to normal are running into a big hitch: Workers on unemployment benefits are reluctant to give them up."

7 Concluding Remarks

This paper takes an early look at a large and novel small business support program that was part of the initial crisis response package, the Paycheck Protection Program (PPP). We explore both program targeting, including the role that banks played in intermediating PPP funds, and the overall short- and medium-term employment and local economic effects of the program.

We consider two dimensions of program targeting. First, did the funds flow to where the economic shock was greatest? Second, given the PPP used the banking system as a conduit to access firms, we ask what role did the banks play in mediating policy targeting? We find little evidence that funds were targeted towards geographic regions more severely affected by the pandemic. If anything, the opposite is true and funds were targeted towards areas less severely affected by the virus, at least initially. Bank heterogeneity played an important role in mediating funds, affecting who received funds and when their applications were ultimately processed. We construct a new measure of regional exposure to banks that underperformed in terms of PPP allocation relative to their pre-PPP share of small business lending. Regions with higher exposure to banks that performed well saw higher levels of PPP lending and received funds more quickly.[34] Limited targeting in terms of who was eligible likely also led to many inframarginal firms receiving funds and to a low correlation between regional PPP funding and shock severity.

Using a number of data sources and exploiting lender heterogeneity in disbursement of PPP funds, we find evidence that the PPP had only a small effect on employment in the months following the initial rollout. Our estimates are precise enough to rule out large employment effects in the short-term. At the same time, the program may have played an important role in promoting financial stability. Firms with greater exposure to the PPP hold more cash on hand, and are more likely to make loan and other scheduled payments. Measuring these responses is critical for evaluating the social insurance value of the PPP and similar policies and designing them effectively.

[34]The analysis here focuses on ex ante targeting of the PPP, that is, the distribution of funding provided at the start of the program. Ultimate targeting will depend on the extent of loan forgiveness and defaults, as well as subsequent changes to the PPP, including conditions for recoupment based on ex post economic hardship and changes to program eligibility criteria going forward. See Hanson, Stein, Sunderam and Zwick (2020*a*) for a discussion of these dynamic policy considerations in the design of business liquidity support during the pandemic.

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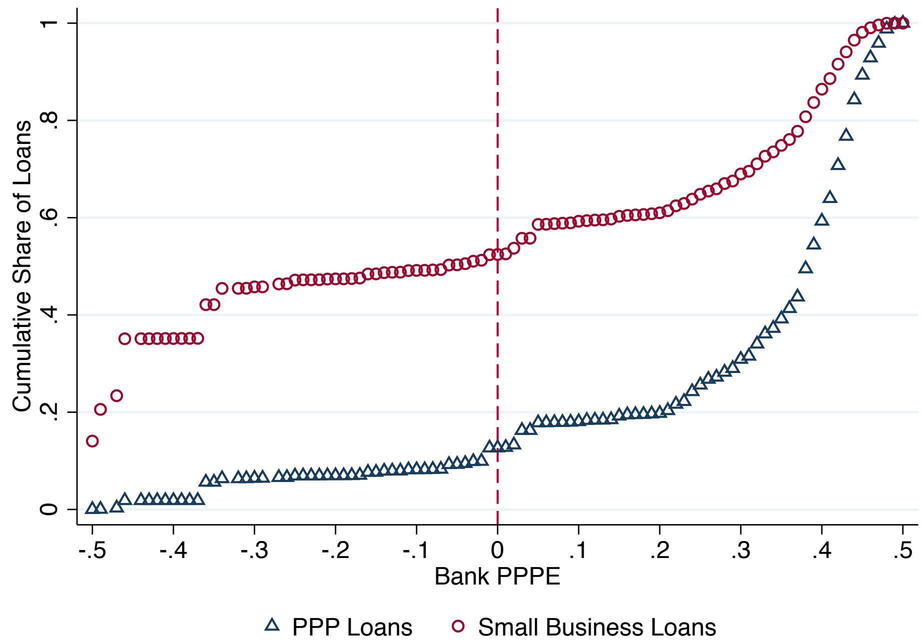
Figure 1: PPPE and PPP Allocation

Figure 1 plots the cumulative share of PPP and small business lending by all banks whose PPPE is below x, where $x \in (-0.5, 0.5)$. Panel A plots cumulative amounts using PPP data as of the end of the first round (April 15th, 2020), and Panel B reports cumulative amounts using PPP data as of when the flow of second round funds approximately ends (June 30th, 2020). Data is obtained from the SBA and commercial bank Call Reports.

Panel A. Cumulative PPP and SBL Lending on April 15th



Panel B. Cumulative PPP and SBL Lending on June 30th

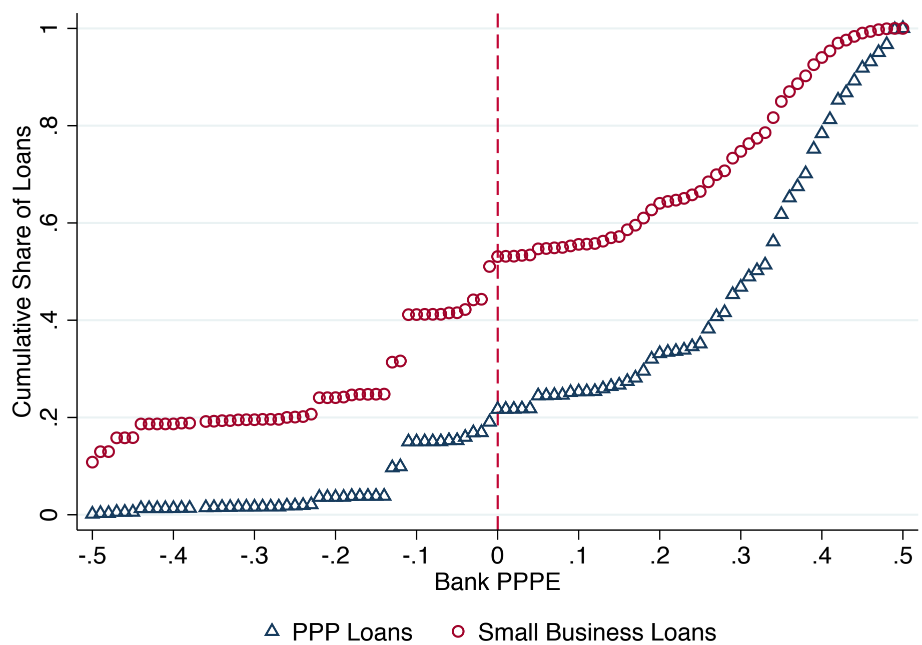


Figure 2: Evolution of PPPE and Average Loan Size by Bank Size

Figure 2 plots the evolution of average PPPE based on the number of PPP loans (Panel A), average PPPE based on the volume of PPP loans (Panel B), the average loan amount of loans (Panel C), and the fraction of loans above $1 million (Panel D) by bank size bin. The size bins stratify all commercial banks operating as of the fourth quarter of 2019 based on their total assets. Data is obtained from the SBA and commercial bank Call Reports.

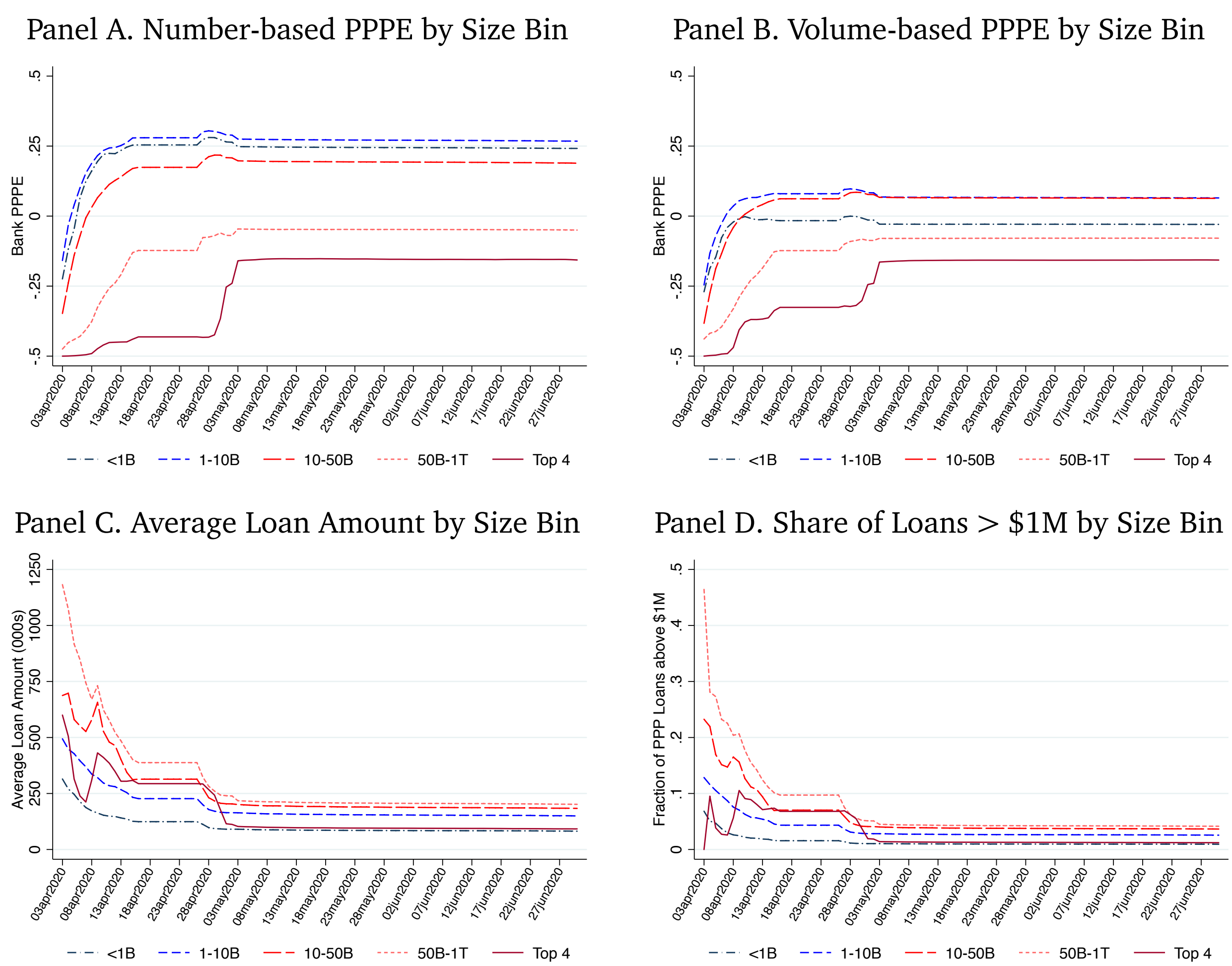
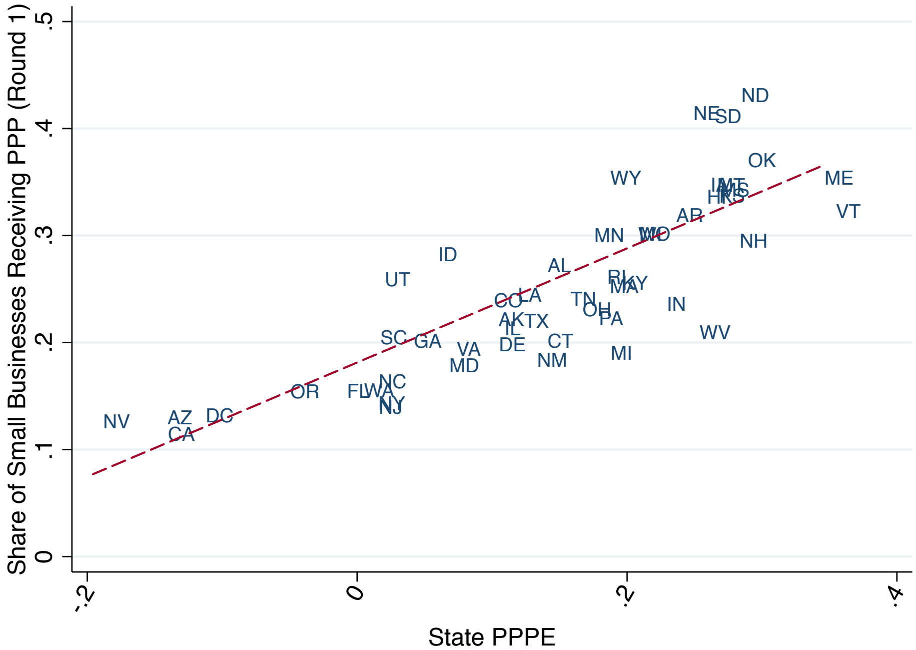


Figure 3: State Exposure to PPPE, PPP per Firm, and Unmet Demand

Figure 3 presents scatter plots comparing state-level exposure to PPPE and Census survey outcomes from after the first round of funding. Panel A plots the percentage of firms receiving PPP at the end of the first round, and Panel B plots unmet demand, which is defined as the difference between the percentage of businesses reporting having applied for PPP and those reporting having received PPP. Data come from the Census Bureau Small Business Pulse Survey, SBA, Call Reports, Summary of Deposits, and County Business Patterns.

Panel A. State PPPE and the Fraction of Small Businesses Receiving PPP



Panel B. State PPPE and Unmet Demand

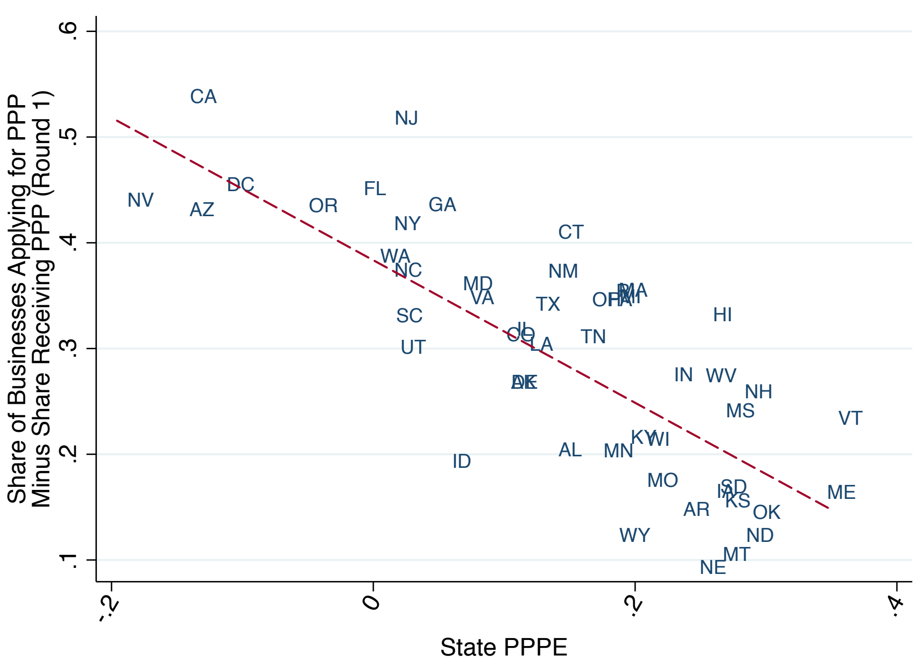


Figure 4: ZIP Exposure to First Round PPPE and PPP Coverage over Time

Figure 4 plots binned scatter plots of the average fraction of small business establishments that received a PPP loan versus ZIP-level PPPE. Eligible establishment counts equal all establishments in a ZIP less an estimate of the share of establishments with more than 500 employees (which are not eligible for PPP) plus an estimate of the number of proprietorships likely to apply for PPP. Both variables are demeaned at the state level to present the within-state relationship. Data come from SBA, Call Reports, Summary of Deposits, and County Business Patterns.

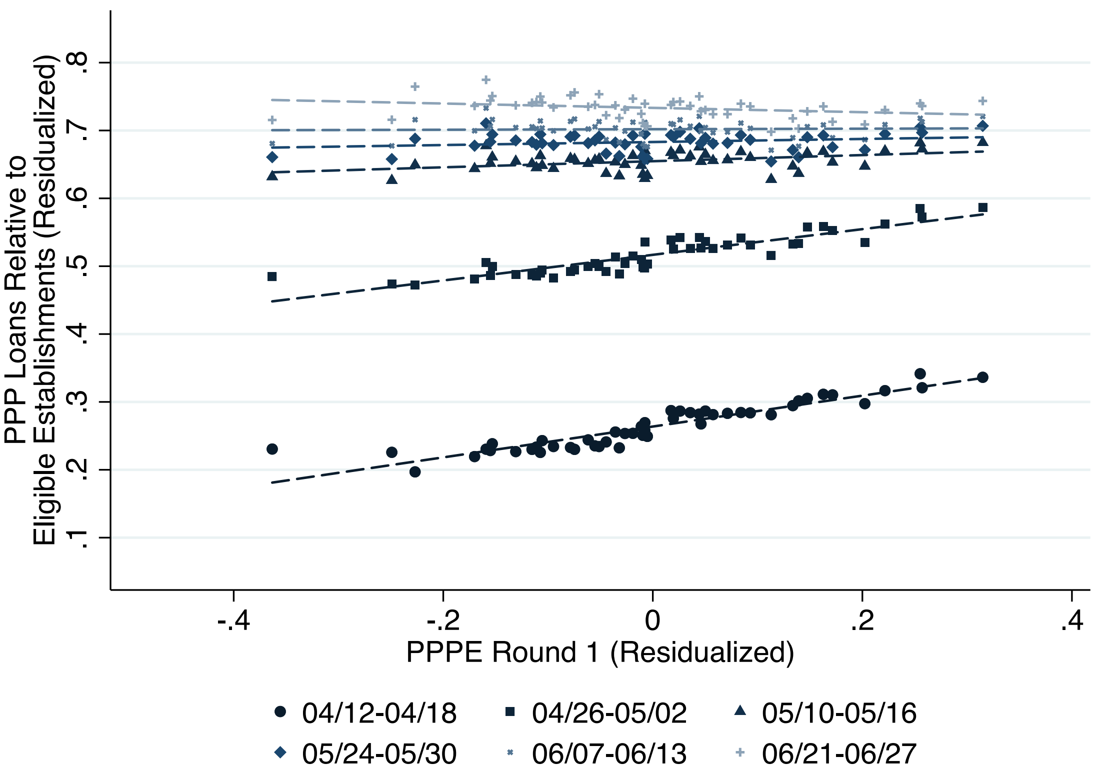


Figure 5: Share of Establishments Receiving PPP by Lender Type

Figure 5 shows the number of PPP loans broken down by lender type and funding round, scaled by the total number of establishments. Data come from the SBA, FDIC Summary of Deposits, and Census.

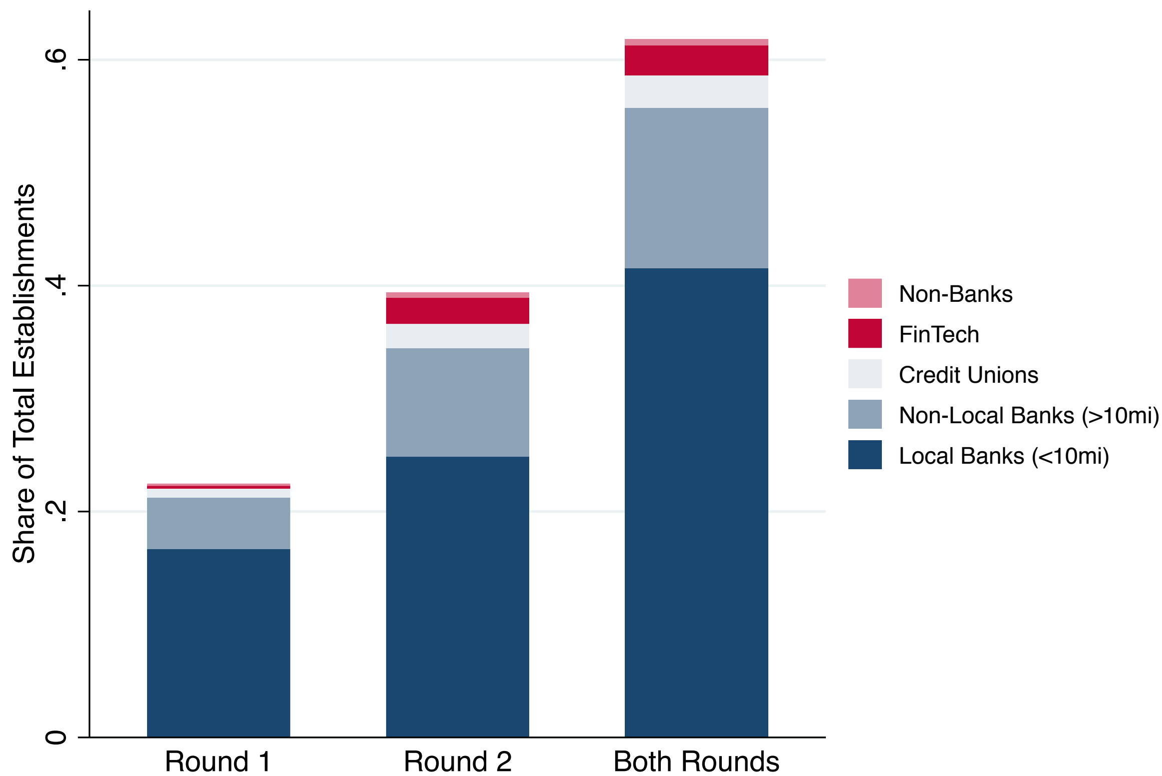
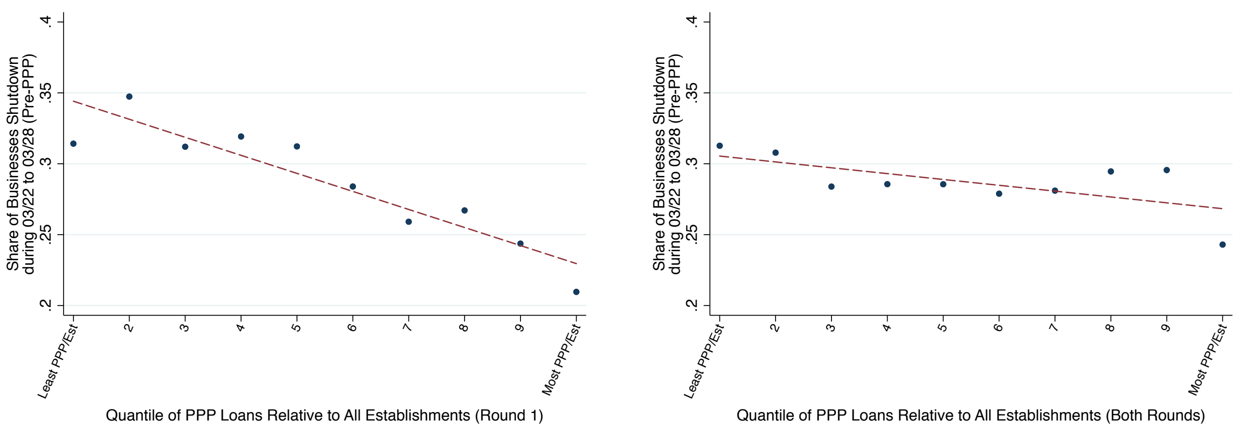


Figure 6: Targeting of PPP Allocation (First Round and Overall)

Figure 6 stratifies all businesses in Homebase in 10 bins based on the fraction of establishments in their ZIP code receiving PPP during the first round and during both rounds combined. Panel A plots for each bin the share of Homebase businesses that shut down in the week of March 22nd–March 28th. Panel B plots for each bin the average decline in hours worked in the week of March 22nd–March 28th relative to a baseline of the average weekly hours worked in the last two weeks of January. Data are from SBA, Homebase, and County Business Patterns.

Panel A. Business Shutdowns and Fraction of Establishments Receiving PPP

Round 1 | Rounds 1 and 2



Panel B. Decline in Hours Worked and Fraction of Establishments Receiving PPP

Round 1 | Rounds 1 and 2

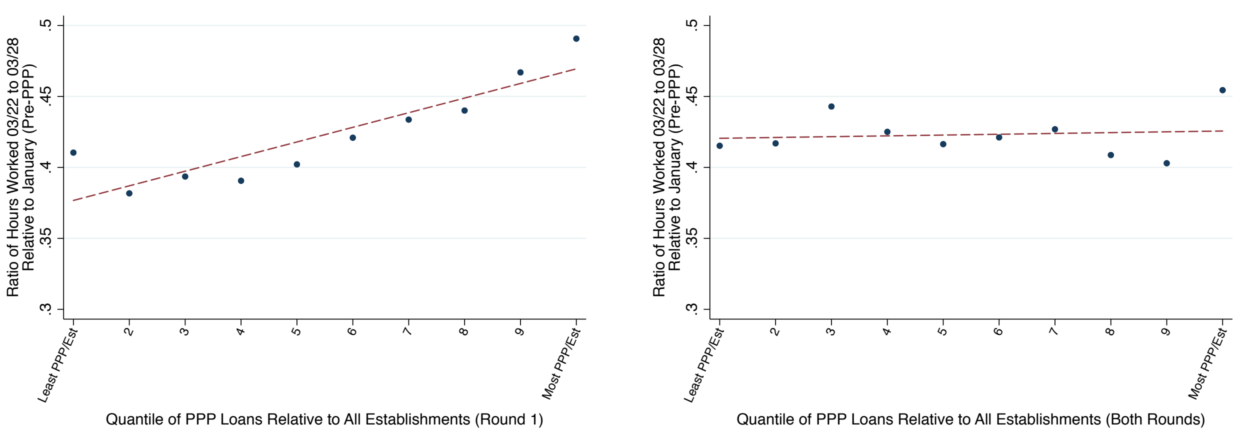
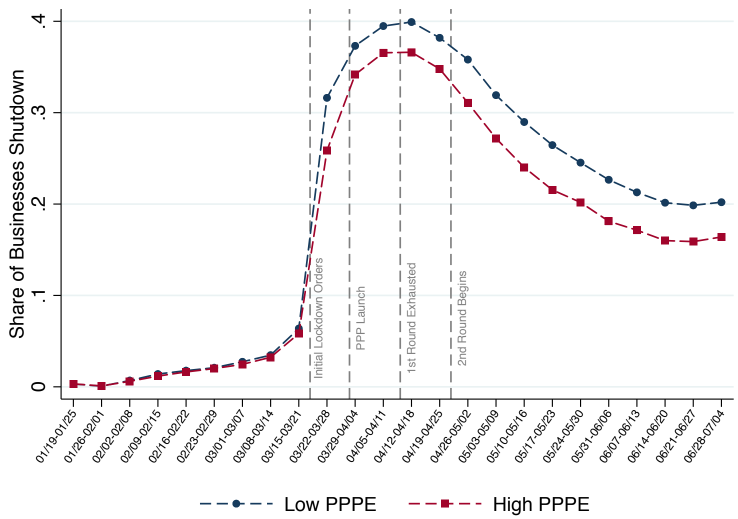


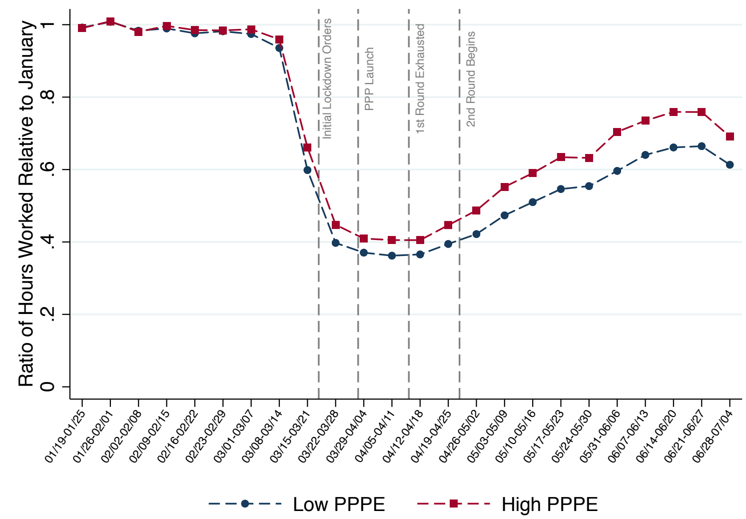
Figure 7: PPPE and Post-PPP Outcomes (Difference-in-Differences)

Figure 7 shows the ratio of hours worked over time, the percent of businesses shut down, initial unemployment filings, and small business revenue growth, splitting the sample into regions with above- versus below-median PPPE. Data are from SBA, Homebase, County Business Patterns, state labor departments, and Womply.

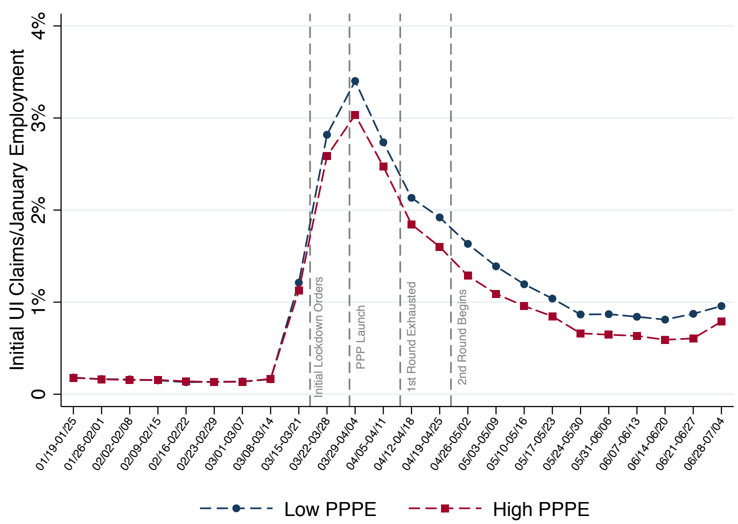
Panel A. Business Shutdowns



Panel B. Change in Hours Worked



Panel C. Initial UI Claims



Panel D. Small Business Revenue Growth

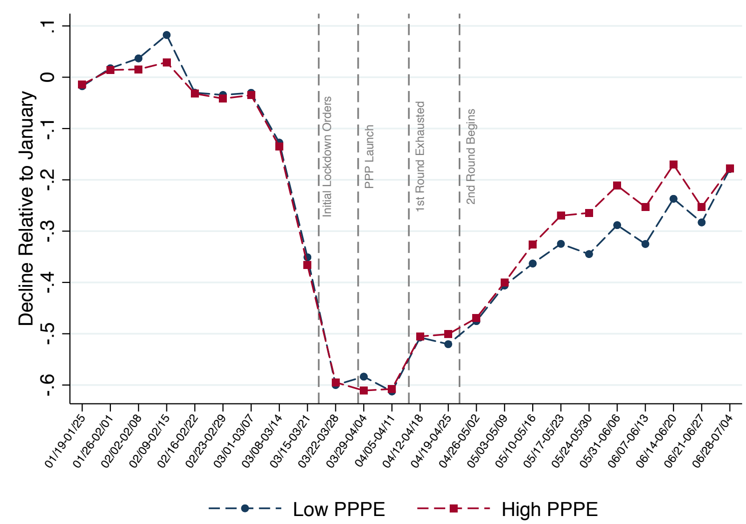
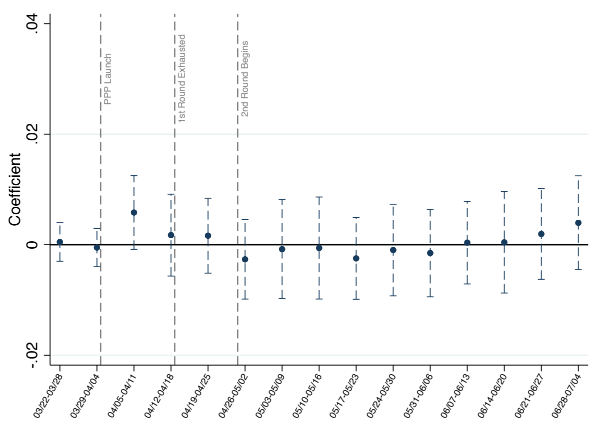


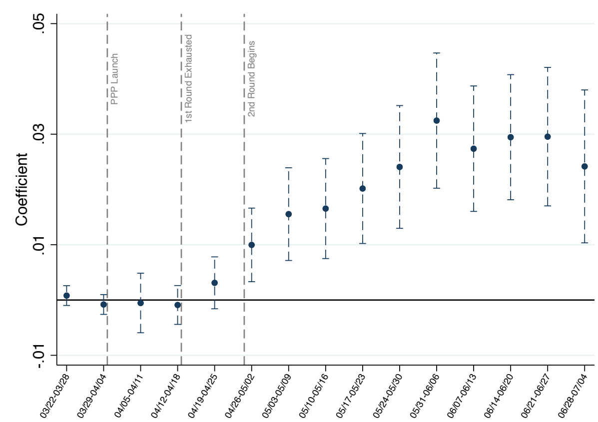
Figure 8: PPPE and Post-PPP Outcomes (Local Projections)

Figure 8 plots coefficients and standard errors of regressions investigating the impact of exposure to PPPE on employment and firm outcomes, defined as the difference between these outcomes in each week relative to their average in the two weeks prior to program launch (weeks 10 and 11). Panel A plots the coefficients β and standard errors of $\Delta Shutdown_{ijnt} = \alpha_{sn} + \beta PPPE_j + \Gamma X_{ijnt} + \epsilon_{ijnt}$, where $\Delta Shutdown_{ijnt}$ is the difference between the shutdown indicator of firm i in each week and the average shutdown indicator for that firm during the two weeks prior to program launch, $PPPE_j$ is the average exposure of the ZIP j to bank PPPE, α_{sn} are state-by-industry fixed effects and X_{ijnt} are additional control variables. Panel B plots estimates from similar week-by-week regressions that use the change in the decline in hours worked relative to January as the dependent variable. Panel C plots estimates from similar week-by-week regressions that use changes in initial unemployment filings scaled by total employment at the county level, and Panel D plots estimates from a similar empirical specification that uses the changes in average small business revenues for small businesses within a 3-digit NAICS industry and county. Data are from SBA, Homebase, County Business Patterns, state labor departments, and Womply.

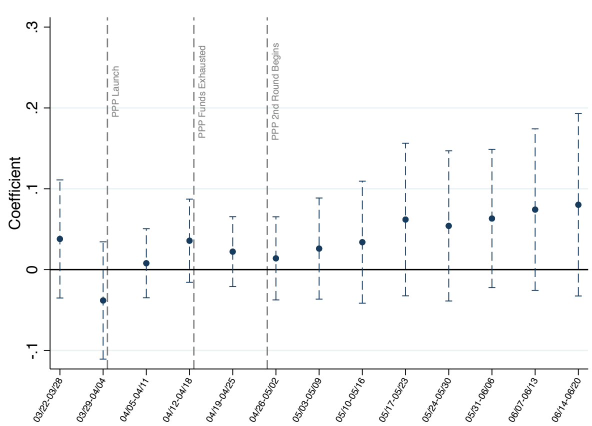
Panel A. Business Shutdowns



Panel B. Change in Hours Worked



Panel C. Initial UI Claims



Panel D. Small Business Revenue Growth

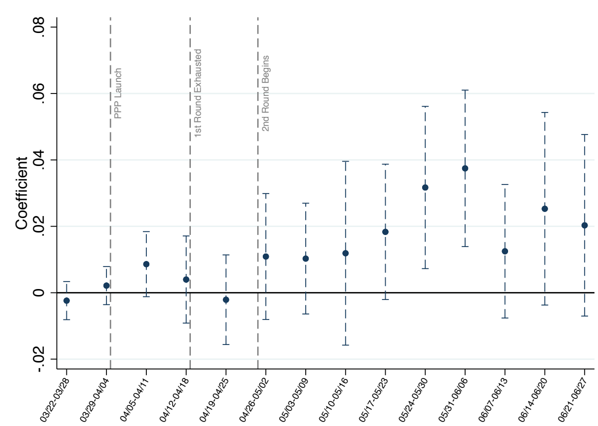
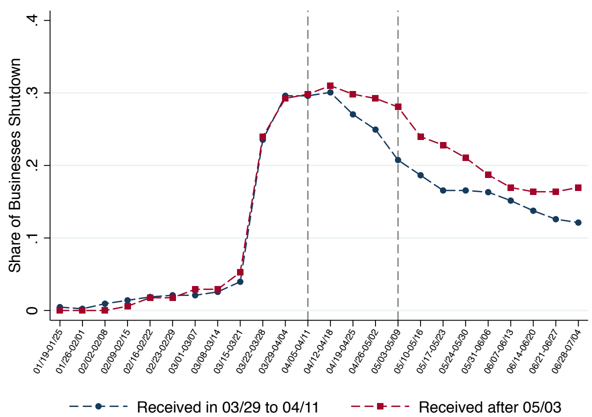


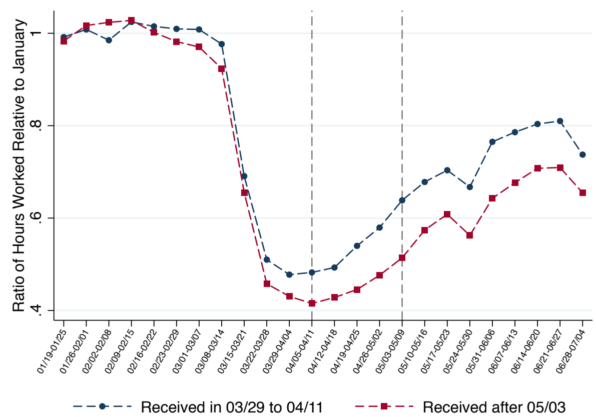
Figure 9: PPPE and Post-PPP Outcomes (Matched Sample Analysis)

Figure 9 investigates business shutdowns and changes in the ratio of hours worked for firms in the Homebase sample that are name-matched to the PPP data set from SBA. In Panels A and B, we compare firms that received PPP approval in week 12 or earlier to those that received PPP approval in week 16 or later. In Panels C and D, we plot estimates from regressions using this sample of firms that reveal the impact of delays in receiving PPP on differences in employment and firm outcomes. Outcome variables are defined as the difference between measured outcomes in each week and their average in the two weeks preceding program launch. The right-hand-side variable is the week of PPP receipt. The regressions repeat the specifications of column (1) of Table 6 for every week in the sample. Data is from SBA and Homebase.

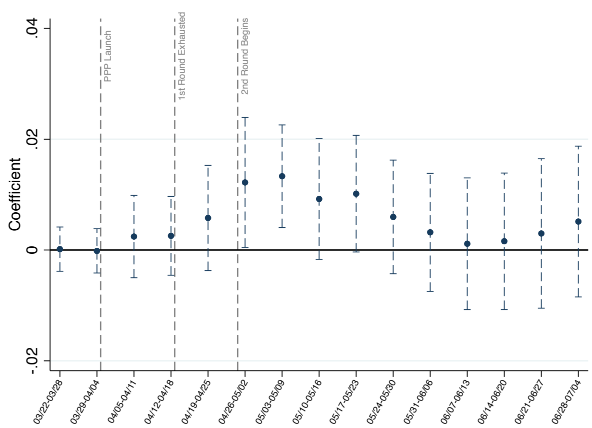
Panel A. Business Shutdowns
Early vs. Late Recipients



Panel B. Change in Hours Worked
Early vs. Late Recipients



Panel C. Business Shutdowns
Week of PPP Projections



Panel D. Change in Hours Worked
Week of PPP Projections

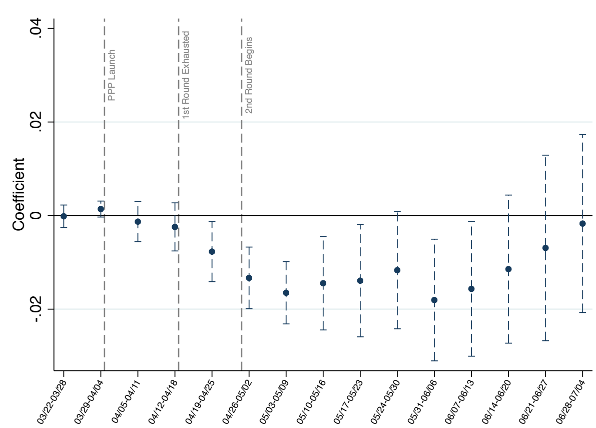


Table 1: PPP Performance and PPPE for the Largest 20 Banks

Table 1 reports individual bank statistics and the PPPE index for the 20 largest financial institutions in the United States. Total Assets is computed using information from fourth quarter 2019 Call Reports. Share of PPP Volume is the total amount disbursed by each financial institution relative to the total amount disbursed under either the first round or both rounds of the program. Share of SBL Market is the share of the total outstanding amount of small business loans held by each financial institution relative to the total outstanding amount of small business loans as of the fourth quarter of 2019. PPPE (Vol.) is the volume-based bank PPP index. Total assets are in millions of USD. Share of PPP Loans is the total number of loans processed by each financial institution relative to the total number of loans processed in the either the first round or both rounds of the program. Share of SBL Loans is the share of the total number of outstanding small business loans held by each financial institution relative to the total outstanding number of small business loans as of the fourth quarter of 2019. PPPE (Nbr.) is the number-based bank PPP index.

Financial Institution Name	(1) Total Assets	(2) Share of PPP Volume R1	(3) Share of PPP Volume R1&2	(4) Share of SBL Market	(5) PPPE R1 (Vol.)	(6) PPPE R1&2 (Vol.)	(7) Share of PPP Loans R1	(8) Share of PPP Loans R1&2	(9) Share of SBL Loans	(10) PPPE R1 (Nbr.)	(11) PPPE R1&2 (Nbr.)
JPMORGAN CHASE BANK, NATIONAL ASSOCIATION	2337707	3.74%	5.84%	6.54%	-0.136	-0.028	1.71%	6.16%	10.4%	-0.360	-0.130
BANK OF AMERICA, NATIONAL ASSOCIATION	1866841	1.13%	5.10%	9.51%	-0.393	-0.151	.595%	7.79%	11.8%	-0.452	-0.103
WELLS FARGO BANK, NATIONAL ASSOCIATION	1736928	.038%	2.08%	6.50%	-0.494	-0.257	.066%	4.14%	4.30%	-0.485	-0.009
CITIBANK, N.A.	1453998	.394%	.702%	2.12%	-0.343	-0.251	.456%	.693%	9.72%	-0.455	-0.433
U.S. BANK NATIONAL ASSOCIATION	486004	.723%	1.48%	3.32%	-0.321	-0.192	1.15%	2.25%	5.64%	-0.331	-0.215
TRUIST BANK	461256	2.97%	2.62%	2.01%	0.096	0.066	2.02%	1.77%	1.73%	0.040	0.006
CAPITAL ONE, NATIONAL ASSOCIATION	453626	.022%	.243%	2.82%	-0.492	-0.421	.012%	.335%	10.3%	-0.499	-0.469
PNC BANK, NATIONAL ASSOCIATION	397703	2.75%	2.60%	1.12%	0.210	0.199	1.35%	1.70%	1.37%	-0.004	0.054
BANK OF NEW YORK MELLON, THE	342225	0%	0%	.002%	-0.500	-0.500	0%	0%	.000%	-0.500	-0.500
TD BANK, N.A.	338272	1.83%	1.69%	.687%	0.228	0.212	1.70%	1.88%	.569%	0.249	0.268
STATE STREET BANK AND TRUST COMPANY	242148	0%	.000%	0%	0.000	0.500	0%	.000%	.000%	-0.500	0.413
CHARLES SCHWAB BANK	236995	0%	0%	.074%	-0.500	-0.500	0%	0%	.003%	-0.500	-0.500
MORGAN STANLEY BANK, N.A.	229681	0%	0%	.144%	-0.500	-0.500	0%	0%	.008%	-0.500	-0.500
GOLDMAN SACHS BANK USA	228836	0%	0%	.003%	-0.500	-0.500	0%	0%	.000%	-0.500	-0.500
HSBC BANK USA, NATIONAL ASSOCIATION	172888	.129%	.240%	.084%	0.105	0.240	.067%	.093%	.014%	0.328	0.369
FIFTH THIRD BANK, NATIONAL ASSOCIATION	167845	1.01%	1.06%	.458%	0.188	0.200	.625%	.861%	.192%	0.265	0.318
ALLY BANK	167492	.213%	.145%	2.11%	-0.408	-0.436	.055%	.021%	1.38%	-0.461	-0.485
CITIZENS BANK, NATIONAL ASSOCIATION	165742	1.14%	.992%	.807%	0.086	0.051	1.60%	1.15%	.527%	0.253	0.187
KEYBANK NATIONAL ASSOCIATION	143390	2.19%	1.59%	.729%	0.251	0.186	2.14%	.932%	.274%	0.387	0.273
BMO HARRIS BANK NATIONAL ASSOCIATION	137588	1.20%	.919%	1.95%	-0.120	-0.181	.683%	.489%	.541%	0.058	-0.025
ALL OTHER BANKS	6889908	80.4%	72.6%	58.9%	-0.042	-0.048	85.7%	69.6%	40.9%	0.215	0.212

Table 2: Correlates of PPPE Exposure

Table 2 presents bivariate regressions of PPPE Exposure on ZIP-level observables. Both PPPE and observables are residualized with respect to state dummies. Variables have been normalized, so the coefficients can be interpreted as a one-standard deviation change in x produces a β-standard deviation change in PPPE exposure, where β is the reported coefficient. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	LHS is Residual PPPE as of Round 1		
	Coefficient	R^2	N
Exposure Correlates:			
Share of Top 4 Banks	-0.703***	0.3619	35882
	(0.006)		
Number of Branches per Capita	-0.020***	0.0006	29545
	(0.002)		
Share of Small Banks Deposits	0.400***	0.1592	35830
	(0.006)		
Share of Approved SBA Lenders	-0.060***	0.0044	35882
	(0.007)		
Other Correlates:			
Log(Population)	-0.203***	0.0476	29545
	(0.005)		
Log(Population Density)	-0.336***	0.1044	29545
	(0.006)		
Social Distancing	0.225***	0.0412	35549
	(0.007)		
Covid Cases per Capita	-0.162***	0.0375	35870
	(0.004)		
Deaths per Capita	-0.102***	0.0163	35870
	(0.002)		
Unemployment Filing Ratios	0.012	0.0001	24576
	(0.008)		
Revenue Change of Small Business	-0.317***	0.0717	35706
	(0.006)		

Table 3: ZIP PPPE in Round 1 and PPP Reallocation across Funding Sources

Table 3 shows the correlation between PPPE and the fraction of establishments receiving PPP loans from different sources in the first and second rounds of the program. The left-hand-side variable in column (1) is the fraction of establishments within a ZIP and 2-digit NAICS industry that received PPP in the first round in Panel A and in both rounds in Panel B. Left-hand-side variables in other columns represent a decomposition of the dependent variable in column (1) into the fraction of establishments within a ZIP and 2-digit NAICS industry that received PPP from local banks, non-local banks, credit unions, FinTech companies, and other nonbanks. *ZIP PPPE (Round 1)* is the weighted average of bank PPPE during the first round at the ZIP level. The weights are defined by the share of branches of each bank within 10 miles of the center of the respective ZIP. ZIP PPPE is standardized to permit coefficients to be interpreted as the effect of a one-standard-deviation increase in ZIP PPPE All regressions include county-by-NAICS fixed effects. Standard errors are presented in parentheses, and are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

Panel A. Allocation in Round 1

	(1)	(2)	(3)	(4)	(5)	(6)
		PPP Loans Relative to All Establishments by Lender Source				
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
ZIP PPPE (Round 1)	1.86***	2.47***	-0.38**	-0.11***	-0.09***	-0.03
	(0.17)	(0.24)	(0.15)	(0.04)	(0.01)	(0.02)
Observations	239872	239872	239872	239872	239872	239872
Adjusted R^2	0.35	0.31	0.22	0.24	0.17	0.16
County×Indy FX	Yes	Yes	Yes	Yes	Yes	Yes

Panel B. Allocation in Rounds 1 and 2

	(1)	(2)	(3)	(4)	(5)	(6)
		PPP Loans Relative to All Establishments by Lender Source				
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
ZIP PPPE (Round 1)	0.22	2.00***	-0.84**	-0.31***	-0.48***	-0.15**
	(0.34)	(0.52)	(0.33)	(0.07)	(0.09)	(0.07)
Observations	239872	239872	239872	239872	239872	239872
Adjusted R^2	0.51	0.52	0.26	0.32	0.18	0.21
County×Indy FX	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: PPP Exposure and Homebase Employment Outcomes

Table 4 reports the results of OLS regressions examining the relation between exposure to PPPE during the first round and the difference between a firm's average employment outcomes in the two weeks prior to the launch of PPP and the firm's outcomes in each of the following weeks. The left-hand-side variable in Panel A, Δ *Bus. Shutdown*, is the difference between the firm's shutdown status in a week and its average shutdown status in weeks 10 and 11, where shutdown status takes a value of one if the business reported zero hours worked over the entire week. The left-hand-side variable in Panel B, Δ *Hours Worked*, is the difference in the ratio of hours worked in each establishment in a week and the average ratio of hours worked in that establishment in weeks 10 and 11. The ratio of hours worked in each establishment is measured as the hours worked in that week relative to the hours worked in that same establishment during the last two weeks of January. *Zip PPPE (Round 1)* is the weighted average of bank PPPE during the first round at the ZIP level. The weights are defined by the share of branches of each bank within 10 miles of the center of the respective ZIP. *I(Month=April)* is an indicator variable for the weeks that span the month of April starting with the week of April 5th to April 12th and ending in April 26th to May 2nd (inclusive), *I(Month=May)* is an indicator variable for the weeks that span the month of May starting with the week of May 3rd to May 9th and ending in the week of May 24th to May 30th (inclusive), and *I(Month=June)* is an indicator variable for the weeks that span the month of June starting with the week of May 31st to June 6th and ending in the week of June 28th (inclusive). Other control variables include interactions between the social distance index, and COVID cases per capita and deaths per capita measured as of week 9 interacted with the indicator variables for April, May, and June. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

Panel A. Business Shutdowns

	(1)	(2)	(3)	(4)
	Δ Bus. Shutdown			
Zip PPPE (Round 1) × I(Month=April)	0.001	0.002	0.001	0.002
	(0.004)	(0.003)	(0.004)	(0.003)
Zip PPPE (Round 1) × I(Month=May)	-0.000	-0.001	-0.000	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Zip PPPE (Round 1) × I(Month=June)	0.005	0.001	0.005	0.001
	(0.005)	(0.004)	(0.005)	(0.004)
Observations	534690	534690	534690	534690
Adjusted R^2	0.062	0.062	0.513	0.513
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes
Firm Fixed Effects	No	No	Yes	Yes

Panel B. Ratio Hours Worked

	(1)	(2)	(3)	(4)
	Δ Hours Worked			
Zip PPPE (Round 1) × I(Month=April)	0.002	0.003	0.002	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Zip PPPE (Round 1) × I(Month=May)	0.021***	0.019***	0.021***	0.019***
	(0.004)	(0.005)	(0.004)	(0.005)
Zip PPPE (Round 1) × I(Month=June)	0.032***	0.029***	0.032***	0.029***
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	534690	534690	534690	534690
Adjusted R^2	0.158	0.159	0.569	0.570
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes
Firm Fixed Effects	No	No	Yes	Yes

Table 5: PPP Exposure and Local Labor Market and Economic Effects

Table 5 reports the results of OLS regressions examining the relation between exposure to PPPE during the first round and county-level unemployment filings, small business revenue at the county-by-industry level, and county-level employment growth. The left-hand-side variable in the top panel, Δ *County Initial UI Filings Ratio*, is the difference between the initial county unemployment filings during a week and the average initial unemployment filings in the county in weeks 10 and 11. The left-hand-side variable in the middle panel is Δ *Employment*, is the difference between county employment growth in a week and employment growth in weeks 10 and 11. The county-level employment data come from Opportunity Insights. The left-hand-side variable in the bottom panel, Δ *Y/Y Change in Total Consumer Spending*, is the difference between the average change in small business revenue of all establishments of a county operating within a 6-digit NAICS industry and the average change in small business revenue in weeks 10 and 11. *County PPPE (Round 1)* is the weighted average of the PPPE based on the number of outstanding loans, weighted by the share of branches of each bank in each county. *I(Month=April)*, *I(Month=May)* and *I(Month=June)* and other control variables are defined as in Table 4. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Δ County Initial UI Filings Ratio			
County PPPE (Round 1) × I(Month=April)	0.015	0.020	0.013	0.020
	(0.023)	(0.021)	(0.023)	(0.021)
County PPPE (Round 1) × I(Month=May)	0.041	0.044	0.039	0.042
	(0.040)	(0.040)	(0.039)	(0.040)
County PPPE (Round 1) × I(Month=June)	0.083	0.085	0.070	0.071
	(0.056)	(0.057)	(0.050)	(0.052)
Observations	29567	28397	29566	28396
Adjusted R^2	0.655	0.663	0.876	0.879
Other Control Variables	No	Yes	No	Yes
State×Week Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	No	No	Yes	Yes
	Δ Employment in Opportunity Insights Data			
County PPPE (Round 1) × I(Month=April)	0.012***	0.007**	0.006***	0.005**
	(0.003)	(0.004)	(0.002)	(0.002)
County PPPE (Round 1) × I(Month=May)	0.013***	0.010***	0.008***	0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
County PPPE (Round 1) × I(Month=June)	0.011**	0.010**	0.006	0.007*
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	17584	17584	17584	17584
Adjusted R^2	0.248	0.255	0.834	0.835
Other Control Variables	No	Yes	No	Yes
State×Week Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	No	No	Yes	Yes
	Δ Y/Y Change in Total Consumer Spending			
County PPPE (Round 1) × I(Month=April)	0.008**	0.005	0.007*	0.005
	(0.003)	(0.004)	(0.004)	(0.004)
County PPPE (Round 1) × I(Month=May)	0.031***	0.018*	0.030***	0.017
	(0.009)	(0.011)	(0.009)	(0.011)
County PPPE (Round 1) × I(Month=June)	0.037***	0.024*	0.036***	0.023*
	(0.008)	(0.012)	(0.008)	(0.012)
Observations	191310	179019	191310	179019
Adjusted R^2	0.162	0.171	0.472	0.462
Other Control Variables	No	Yes	No	Yes
State×Week×Industry Fixed Effects	Yes	Yes	Yes	Yes
County×Industry Fixed Effects	No	No	Yes	Yes

Table 6: Homebase Employment and PPP Loan Timing (Matched Sample)

Table 6 presents the results from the individual matched sample. The left-hand-side variable in Panel A, Δ *Bus. Shutdown*, is the difference between the firm's shutdown status in week 16 (May 3rd to May 9th) and its average shutdown status in weeks 10 and 11. Bus. Shutdown is an indicator variable that takes the value of one if the business reported zero hours worked over the entire week. The left-hand-side variable in Panel B, Δ *Hours Worked*, is the change in hours worked in each establishment between the average of the last two weeks prior to the launch of PPP and the hours worked in week 16 (May 3rd to May 9th). *Week of PPP Loan* is a variable representing the week in which the firm received PPP loan approval. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

Panel A. Business Shutdowns

	(1)	(2)	(3)	(4)
	Δ Shutdown			
	OLS		IV	
Week of PPP Loan	0.013***	0.009	0.024	0.107
	(0.005)	(0.006)	(0.027)	(0.088)
Observations	1176	840	1176	840
F-Statistic			89.286	8.554
Industry Fixed Effects	Yes	No	Yes	No
State×Industry Fixed Effects	No	Yes	No	Yes

Panel B. Ratio Hours Worked

	(1)	(2)	(3)	(4)
	Δ Hours Worked			
	OLS		IV	
Week of PPP Loan	-0.014***	-0.012***	-0.043***	-0.102*
	(0.003)	(0.004)	(0.015)	(0.054)
Observations	1176	840	1176	840
F-Statistic			89.286	8.554
Industry Fixed Effects	Yes	No	Yes	No
State×Industry Fixed Effects	No	Yes	No	Yes

Table 7: PPP Receipt, Missed Payments, and Cash-on-Hand (Census Pulse Survey)

Table 7 reports the results of OLS and IV regressions examining the relation between the geographic allocation of PPP funds during the first round and outcomes from the Census Small Business Pulse Survey. Survey outcomes cover the six weeks from April 26th through June 6th. The left-hand-side variable in the top panel is the percentage of firms reporting a missed scheduled loan payment. The left-hand-side variable in the middle panel is the percentage of firms reporting a missed other scheduled payment such as rent, utilities, and payroll. The left-hand-side variable in the bottom panel is the fraction of businesses with cash on hand to sustain operations for two months or more. *% PPP Received* is the percentage of businesses reporting having received PPP funds in a state-by-industry group. *State PPPE* is the state average of the PPPE based on the number of outstanding loans. Regressions include controls for: *Pre-PPP Decline Hours Worked*, which equals the average decline in hours worked in each state between January and the last week of March; *Pre-PPP State Covid-19 Cases (per capita)* and *Pre-PPP State Covid-19 Deaths (per capita)* at the state level; and *Pre-PPP State Social Distancing Index*, which is the change in average distance travelled in the state until the end of March using individuals' GPS signals. All specifications include industry×week fixed effects. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
LHS Variable	% Missing Loan Payments	% Missing Loan Payments	% PPP Received	% Missing Loan Payments
% PPP Received	-0.005			-0.149***
	(0.014)			(0.037)
State PPPE (Round 1)		-4.401***	34.402***	
		(1.380)	(3.055)	
Observations	2218	2487	2218	2218
F-Statistic				126.801
LHS Variable	% Missing Schd. Payments	% Missing Schd. Payments	% PPP Received	% Missing Schd. Payments
% PPP Received	-0.091***			-0.435***
	(0.022)			(0.067)
State PPPE (Round 1)		-14.807***	34.662***	
		(2.651)	(3.053)	
Observations	2206	2448	2206	2206
F-Statistic				128.871
LHS Variable	% Cash 3 months	% Cash 3 months	% PPP Received	% Cash 3 months
% PPP Received	-0.000			0.270**
	(0.035)			(0.121)
State PPPE (Round 1)		8.920**	32.502***	
		(3.768)	(3.116)	
Observations	903	918	903	903
F-Statistic				108.766
Controls	Yes	Yes	Yes	Yes
Industry×Week Fixed Effects	Yes	Yes	Yes	Yes

A Data Appendix

We draw upon microdata made available through the Small Business Administration (SBA) and the Department of Treasury containing all PPP loans, allowing us to observe all loans approved under the program.[35] For all loans, the data include lender name, the borrower's self-reported industry and corporate form, workers covered by the loan, and some demographic data on firm owners. For loans below \$150,000, the data include precise loan amounts but do not report the name and address of the borrower. For loans above \$150,000, the data include only a range rather than a precise loan amount but include borrower name and address. Our targeting analysis and bank exposure research design use data for all loans aggregated to either the regional or local geography level. We use the additional borrower detail for larger loans in our matched sample research design.

We merge this data set with the Reports of Condition and Income (Call Reports) filed by all active commercial banks as of 2020:Q1. Specifically, we use a bigram string comparator for the lender name to match the lender names in the PPP data set to commercial and savings banks in the Call Reports data set. The main challenge in this process is that many lender names are matched to multiple distinct banks with the same legal name. For instance, there are fifteen distinct banks whose legal name is "Community State Bank" filing a call report in the first quarter of 2020. We address this issue by assigning each loan made by these distinct banks with similar legal names to the similarly-named bank with the branch that is closest to the zip code where the loan was made.[36] We are able to match 4,370 bank participants in the PPP program to the Call Reports data set. We did not match 795 commercial and savings banks that filed a Call Report in the first quarter of 2020. We assume that these banks did not participate in the PPP program and made no PPP loans. Overall, lenders in the PPP sample that we matched to the Call Report account for 90.5% of all loans disbursed under the PPP.

We classified 926 PPP program participants as credit unions and 45 participants as agricultural credit associations. We also classified the remaining 123 participants as non-bank PPP lenders. This group is very heterogenous and comprises small community development funds (e.g. Montana Community Development Corporation), as well as finance companies and Fintech lenders. After careful investigation of companies websites, we classified thirteen non-bank lenders as Fintech lenders. Interestingly, Fintech lenders account for 4.2% of the total number

[35] An earlier version of this paper used data from a Freedom of Information Act request on the number of approved PPP loans and approved PPP amounts during the first round of the program.

[36] Most of these banks with similar legal are small and operate in different states. Given the proximity between lenders and PPP borrowers across the entire sample, we are confident that our allocation process assigns most loans to their correct lender.

of loans in the program and a single Fintech lender, Kabbage Inc., accounts for more than half of the loans made by Fintech lenders.

We obtain financial characteristics of all banks from the Call Reports, which provide detailed data on the size, capital structure, and asset composition of each commercial and savings bank operating in the United States. Importantly, we obtain information on the number and amount of small business loans outstanding of each commercial and savings bank from the "Loans to Small Business and Small Farms Schedule" of the Call Reports. Using this information, we benchmark the participation of all commercial and savings banks in the PPP program relative to their share of the small business lending market prior to the program.

As noted in the main text, we use the the matched-PPP-Call-Reports data and Summary of Deposits data containing the location of all branches and respective deposit amounts for all depository institutions operating in the United States as of June 30th, 2019. A significant number of depository institutions merged in the second half of 2019, which means that some branches are assigned to commercial and savings banks that no longer exist as stand-alone institutions. Notably, SunTrust Banks, Inc. merged with Branch Banking and Trust Company (BB&T) to create the sixth largest financial institutions in the United States. We use the bank mergers file from the National Information Center to adjust the branch network of merged institutions and account for these mergers. We use data from the County Business Patterns dataset to approximate the amount of PPP lending per establishment and the fraction of establishments receiving PPP loans in the region. It is important to note that the County Business Patterns data include establishments for all firms, including those too large to qualify for the PPP. We use these data to examine how the use of the banking system to deploy the PPP funds affected their distribution. The maintained assumption is that the unobservable share of establishments that are not eligible to receive PPP funds is not systematically associated with the exposure to the PPP performance of local banks in the region.

To evaluate whether PPP amounts were allocated to areas that were hardest-hit by the COVID-19 crisis and whether the program improved economic employment and other economic outcomes following its passage, we use data from multiple available sources on the employment, social distancing, and health impact of the crisis. We obtained detailed data on hours worked among employees of firms that use Homebase to manage their scheduling and time clock. Homebase processes exact hours worked by the employees of a large number of businesses in the United States. We use information obtained from Homebase to track employment indicators at a weekly frequency at the establishment level. The Homebase data set disproportionately covers small firms in food and beverage service and retail, therefore it is not representative of aggregate employment. At the same time, the Homebase data are quite useful for evaluating the employment impacts of the PPP specifically since many hard-hit firms are in the industries Homebase covers and much of the early employment losses came from these firms. To broaden our targeting analysis, we complement the Homebase data set with official weekly state unemployment insurance filings from the Department of Labor.

We use the Homebase data in our bank exposure and matched sample analysis to measure the impact of PPP funding on employment and business shutdowns. To broaden this analysis, we supplement the Homebase data with three additional data sources. First, we obtain county-by-week initial unemployment insurance claims from state web sites or by contacting state employment offices for data. We use initial unemployment insurance claims as a measure of flows into unemployment. Second, we supplement the Homebase data with data from Womply, a company that aggregates data from credit card processors. The Womply data includes aggregate card spending at small businesses at the county-industry level, defined by the location where a transaction occurred. Small businesses are defined as businesses with revenues below SBA thresholds. We complement these data sources with additional county-level employment data from Opportunity Insights, which are described in detail in Chetty, Friedman, Hendren and Stepner (2020).[37]. The employment rates are based on the data from Paychex, Earnin, and Intuit. Like Homebase, the Earnin sample consists of predominantly lower income workers who sign up using a cell phone app. The original data is on the county/day level and it spans the period from Jan 14th 2020 to Jun 17th 2020. In order to match our research design in other parts of the analysis, we aggregate the data on the county/week level and limit it to the period from Mar 22nd 2020 (beginning of week 10) to Jun 23rd 2020 (end of week 23).

We obtain counts of COVID-19 cases by county and state from the Center for Disease Control and use data on the effectiveness of social distancing from Unacast. Unacast provides a social distancing scoreboard that describes daily changes in average physical mobility. Unacast measures the change in average distance travelled using individuals' GPS signals. The data is available on a daily basis at the county level. We obtain information on the effective dates of statewide shelter-in-place orders from the New York Times.[38]

To understand the mechanisms underlying our results, we draw on data from the Census Bureau's Small Business Pulse Survey (SBPS), launched within seven weeks of the national emergency declaration in March (Buffington, Dennis, Dinlersoz, Foster and Klimek, 2020). To

[37] We also refer readers to Chetty, Friedman, Hendren and Stepner (2020) who provide comparisons between HomeBase and aggregate employment, showing that it provides an overall good glimpse of employment dynamics.

[38] See `https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html`.

obtain real-time information tailored towards small businesses, the SBPS was run weekly from April 26th to June 27th with businesses contacted via email based on the Census Bureau's Business Register, which is populated using responses to the Economic Census across the 50 states (and D.C. and Puerto Rico). Furthermore, the SBPS focuses on businesses with receipts that are greater than or equal to $1,000 but retain 500 employees or fewer. This sampling frame closely fits the target population for the PPP. Each week, the sample weights are adjusted to maintain representativeness.

Our goal in assembling these diverse data is to conduct a comprehensive assessment of how business liquidity support affects firm behavior. We observe both intensive and extensive margin employment and operating responses by targeted firms and in their local labor markets. We link this behavior to data on the performance and geographic footprint of banks, the agents used to transmit funds to eligible firms as quickly as possible. With the Census data, we draw upon responses to questions about small business liquidity, loans, defaults, and applications for various forms of private and public government assistance, including specific questions about the PPP and EIDL programs.

Figure A.1: Histogram of Bank Paycheck Protection Program Exposure (PPPE)

Figure A.1 plots the distribution of bank PPPE measured at the end of the first round (April 15th, 2020) and when the flow of second round funds approximately ends (June 30th, 2020). We compute this measure as: $PPPE_{b,Nbr} = \frac{\text{Share Nbr. PPP}-\text{Share Nbr. SBL}}{\text{Share Nbr. PPP}+\text{Share Nbr. SBL}} \times 0.5$. We weigh each bank observation by its size measured as total assets as of the end of 2019. Data are from the SBA and commercial bank Call Reports.

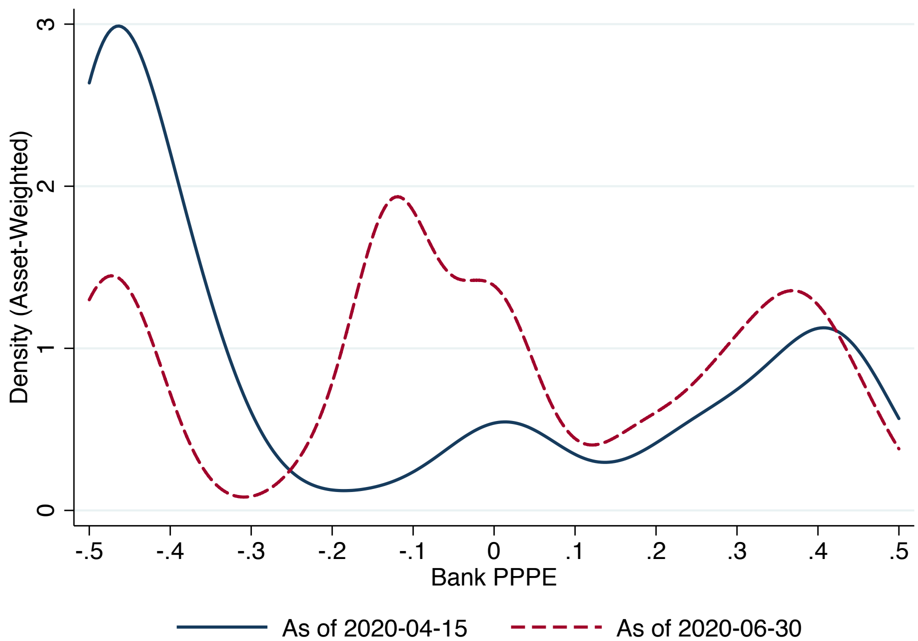


Figure A.2: Wells Fargo Exposure and PPP per Establishment

Figure A.2 is a scatterplot of the number of PPP loans per establishment and the share of Branches of Wells Fargo in a county. Data comes from the SBA, Summary of Deposits, and County Business Patterns. Some counties show a ratio of PPP loans per establishment greater than one. It means that there are more PPP loans than businesses in these counties. Receiving more than one PPP loan is against PPP rules. In these cases we recode the ratio to one.

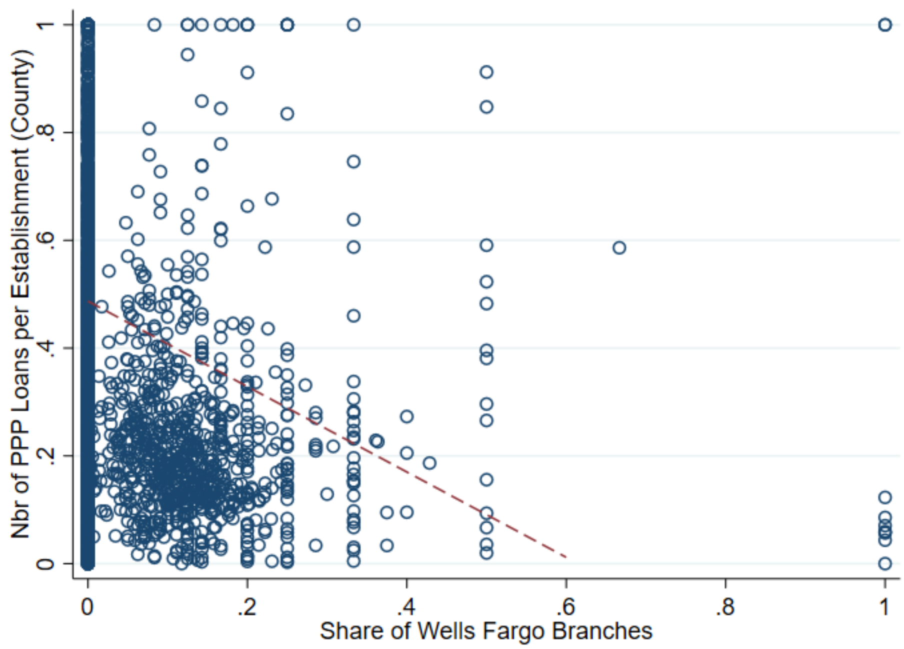


Figure A.3: Kaplan-Meier Survival Functions

Figure A.3 plots Kaplan-Meier survival functions. The blue line represents the survival function for the group of firms located in zip codes exposed to banks with low PPPE and the red line plots the survival function for the group of firms located in zip codes exposed to banks with high PPPE. Data is obtained from the SBA and call reports.

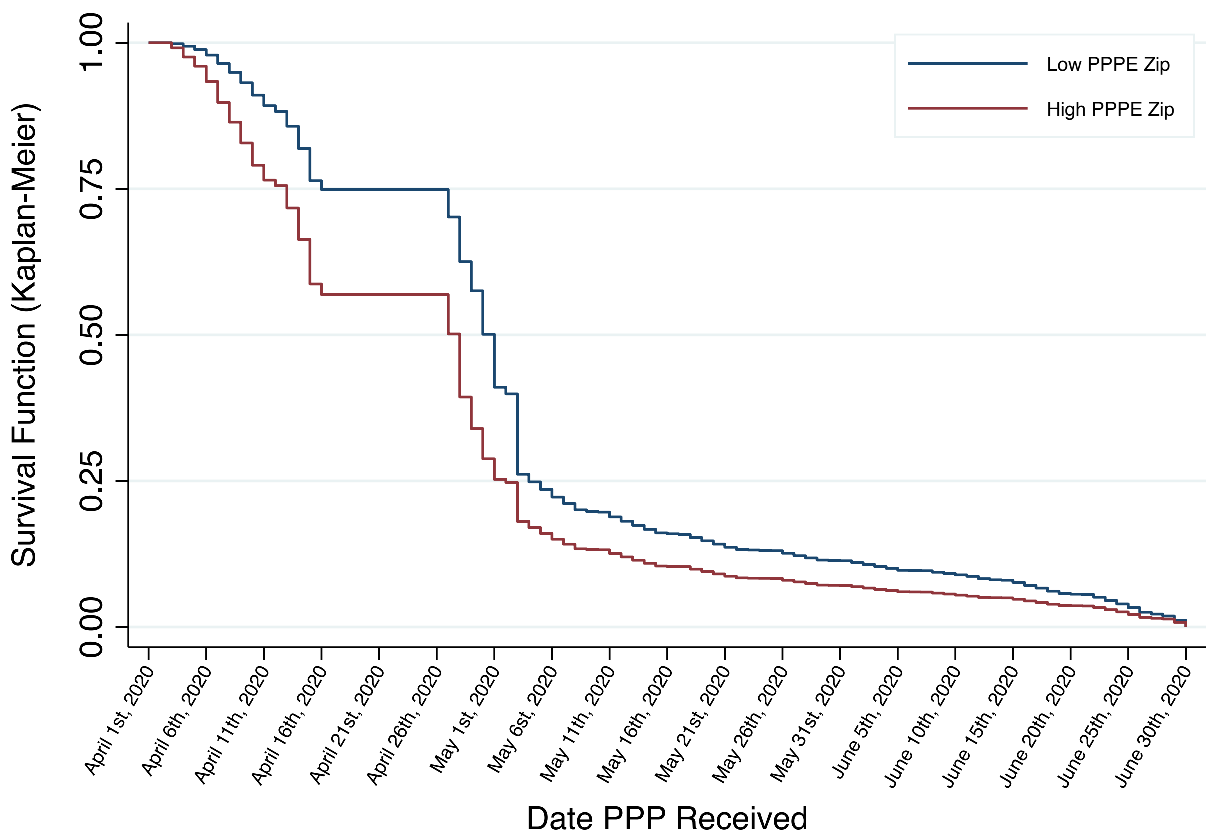
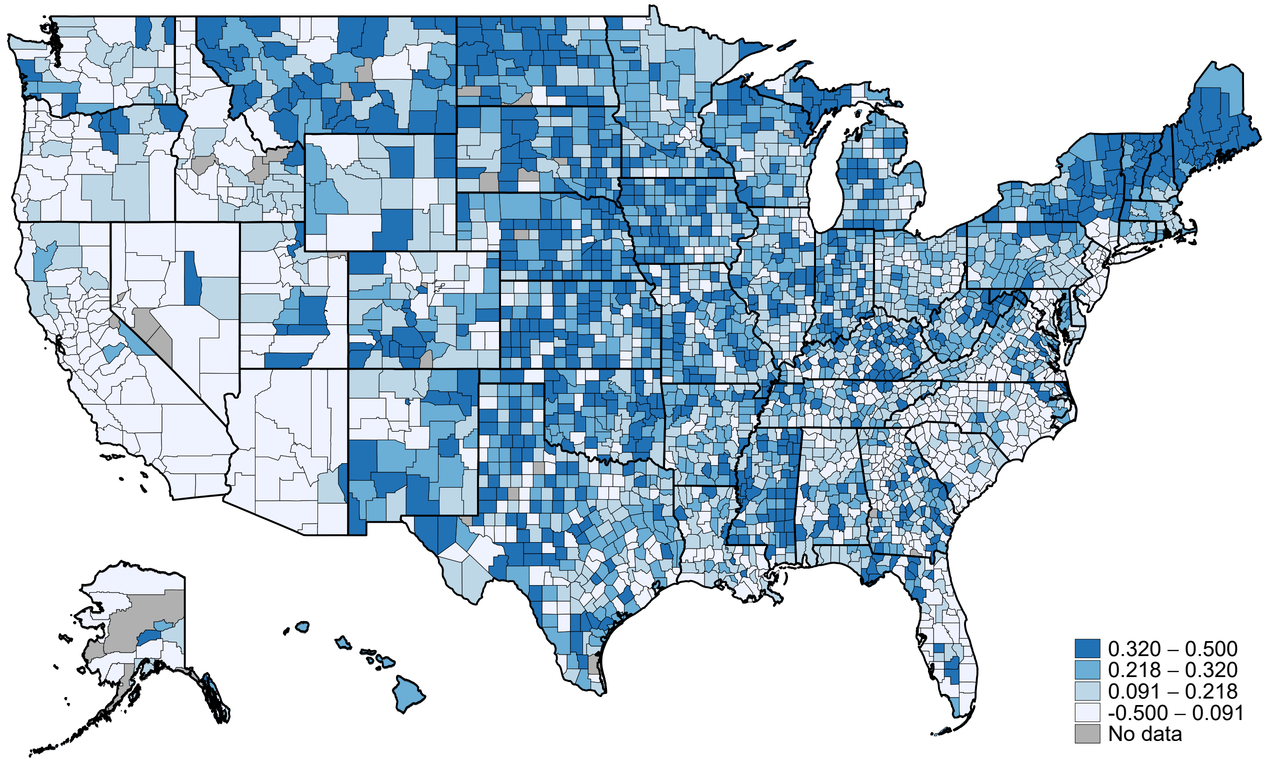


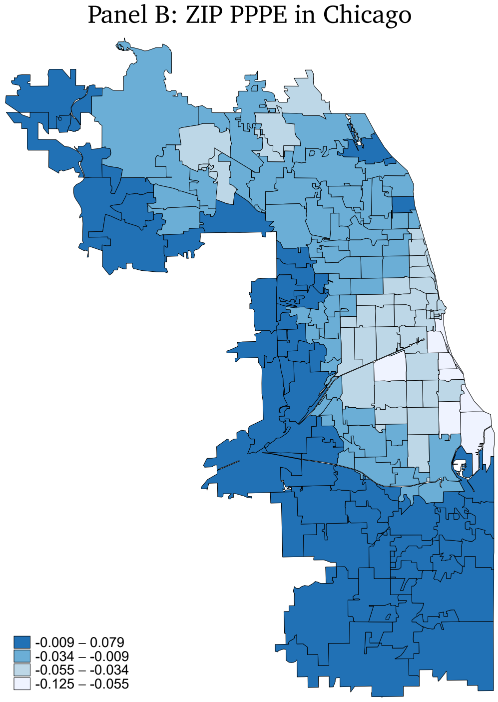
Figure A.4: Map of Exposure to PPPE

Figure A.4 plots the average exposure of each county to the number-based PPPE. County exposure to PPPE is computed as the average of the PPPE of each bank with a branch presence in the county. The PPPE of each bank is weighted by the share of deposits of the bank in the county as of June 30th, 2019. Data is from the SBA, Call Reports, and FDIC's Summary of Deposits.

Panel A: County PPPE



Panel B: ZIP PPPE in Chicago



Panel C: Zip PPPE in New York City

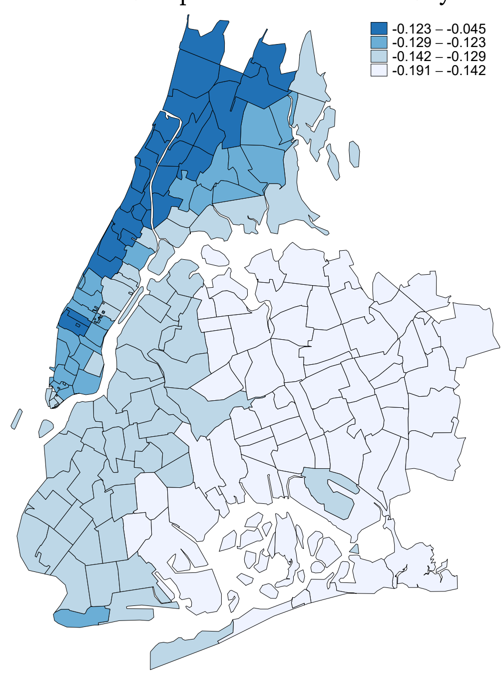
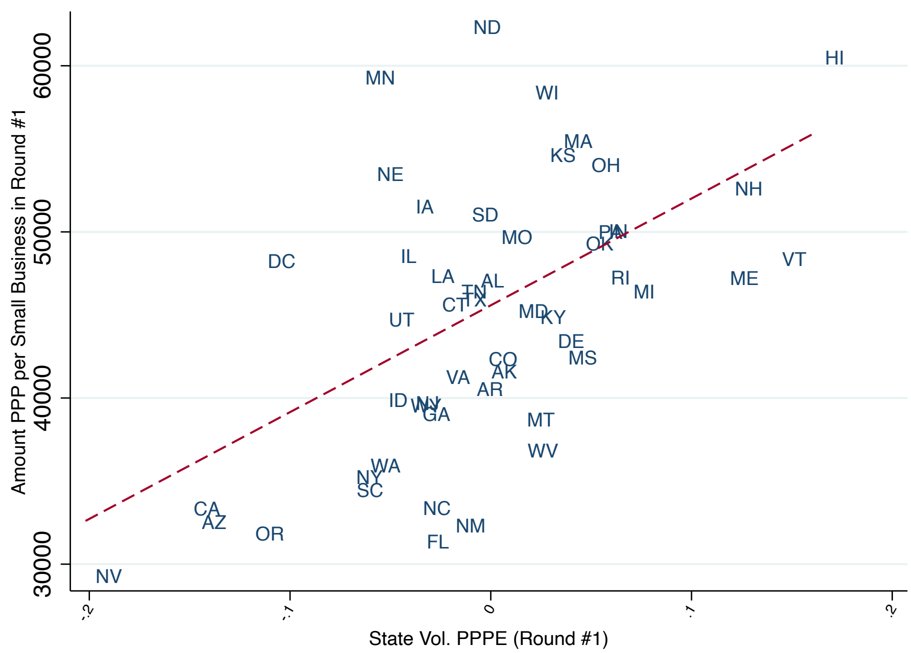


Figure A.5: State Exposure to PPPE and PPP per Establishment

Figure A.5 are scatterplots of state exposure to the volume-based PPPE at the end of Round #1 of the PPP and the amount of PPP per small business at the end of the first round of PPP (Panel A) and of state exposure to the volume-based PPPE at the end of Round #1 of the PPP and the difference between the percentage of businesses reporting having applied to PPP and the percentage of business that received PPP in each state (Panel B). Data comes from the Census Bureau Small Business Pulse Survey, SBA, Call Reports, Summary of Deposits, and County Business Patterns.

Panel A: State Exposure to Volume-based PPPE and Amount of PPP per Small Business during Round#1



Panel B: State Exposure to Volume-Based PPPE and Difference between % Businesses Applying and Receiving PPP

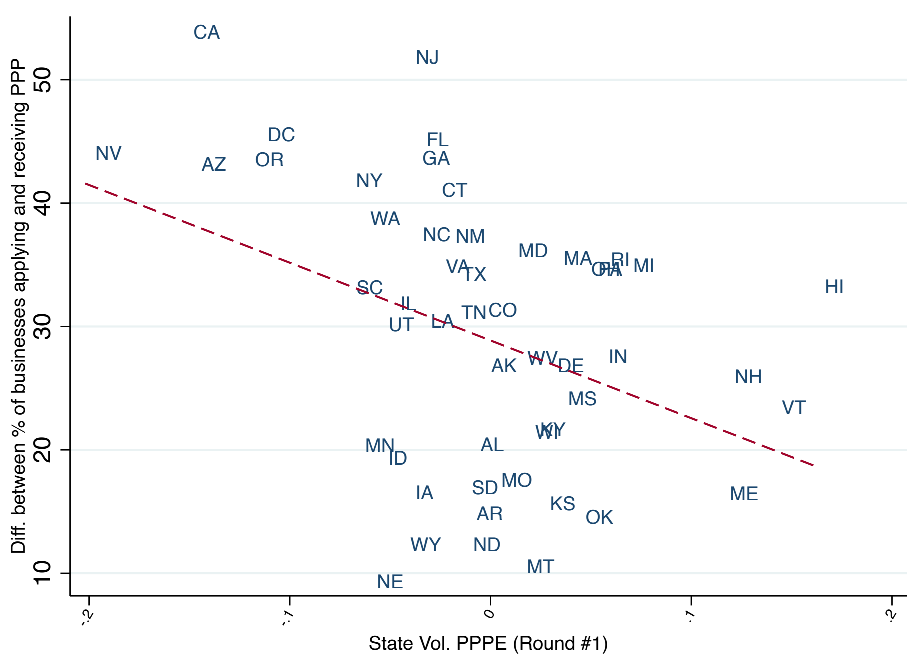


Figure A.6: Pre-PPP Homebase Employment Outcomes and PPP Allocation by State

Figure A.6 presents scatterplots of the share of businesses in each state that shutdown in the week of March 22nd to March 28th and of the decline in hours worked in each state relative to a January Baseline. The figures on the top plot the pre-PPP state-level employment outcomes from Homebase against the number of PPP loans received by small businesses in each state during the first round of the program divided by the total number of small businesses in the state. The figures on the bottom plot the same pre-PPP employment outcomes and the state-level PPPE measure.

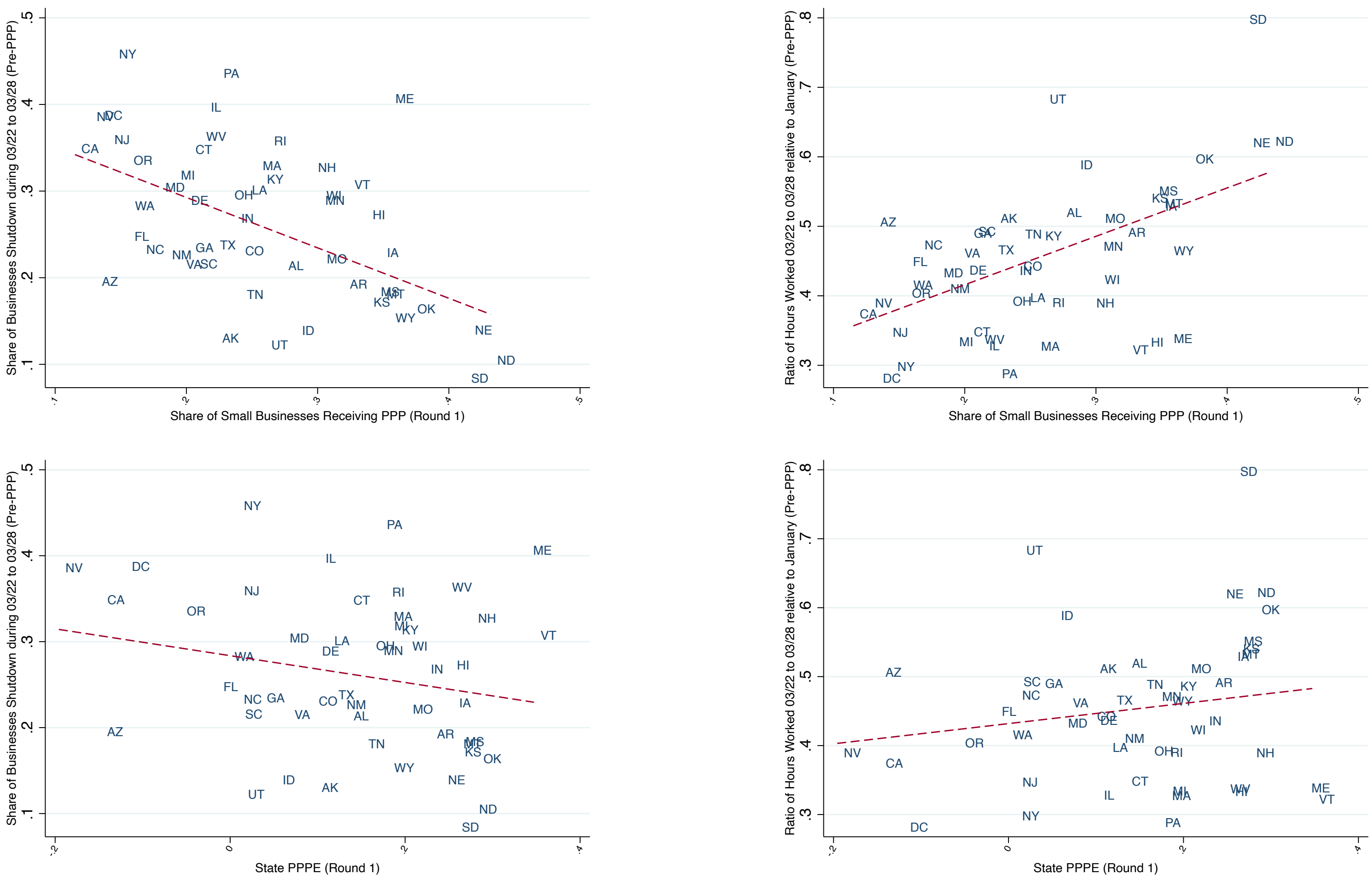


Figure A.7: Pre-PPP Homebase Employment Outcomes and PPP Allocation by County

Figure A.7 presents scatterplots of the share of businesses in each county that shutdown in the week of March 22nd to March 28th and of the decline in hours worked in each county relative to a January Baseline. The figures on the top plot the pre-PPP county-level employment outcomes from Homebase against the number of PPP loans received by small businesses in each county during the first round of the program divided by the total number of small businesses in the state. The figures on the bottom plot the same pre-PPP employment outcomes and the county-level PPPE measure.

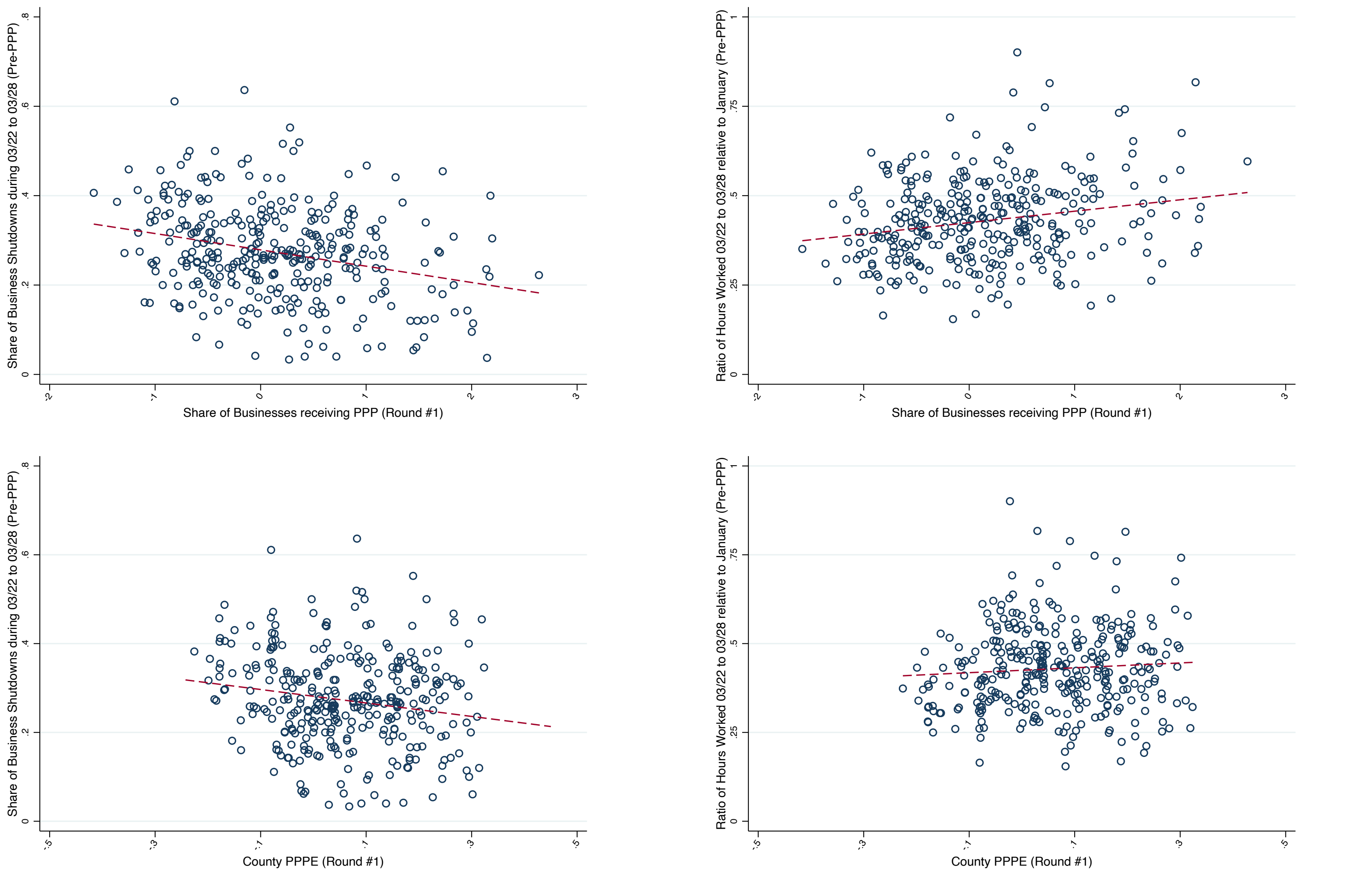


Figure A.8: Pre-PPP Initial UI Claims, Small Business Revenue and PPP Allocation by State

Figure A.8 presents scatterplots of the ratio of initial unemployment claims to employment and measures of PPP allocation at the state-level and of the change in total small business consumer spending at the state level and measures of PPP allocation at the state-level. State unemployment insurance claims are the sum of filed claims in the weeks ended March 21st, March 28th, and April 4th, 2020. Data comes from the Department of Labor, Womply, SBA, Call Reports, and FDIC Summary of Deposits.

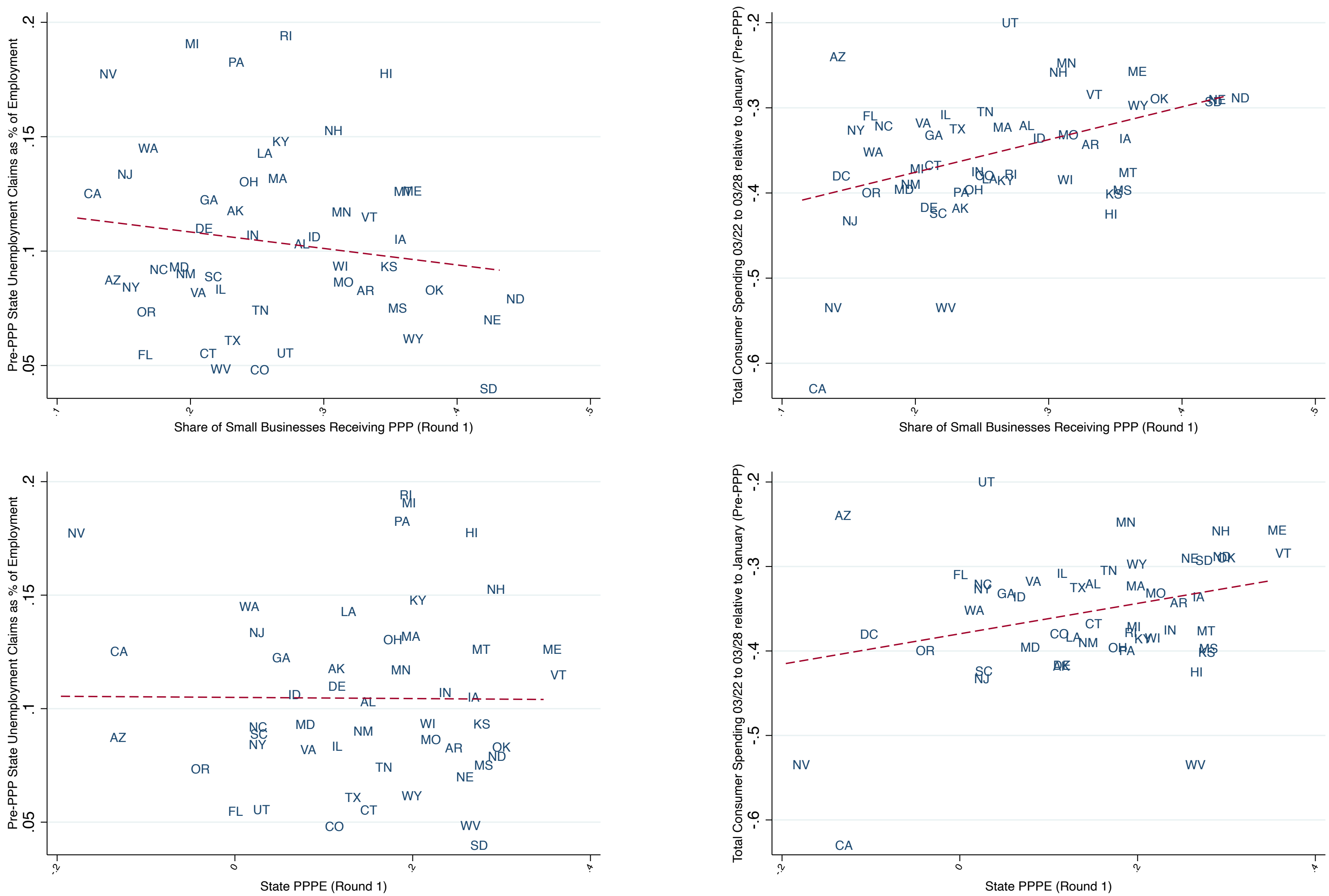


Figure A.9: Pre-PPP Health Outcomes and PPP Allocation by State

Figure A.9 presents scatterplots of the relation between the cumulative number of pre-PPP COVID-19 cases and deaths per thousand in each state as of April, 3rd 2020 and state-level measures of PPP allocation. The figures on the top panel plot the state-level health outcomes against the amount of PPP loans received by small businesses in each state divided by the total number of small businesses in the state. The figures on the bottom plot the state-level health outcomes and the state-level PPPE measure. Data comes from the Center for Disease Control, SBA, Call Reports, and FDIC Summary of Deposits.

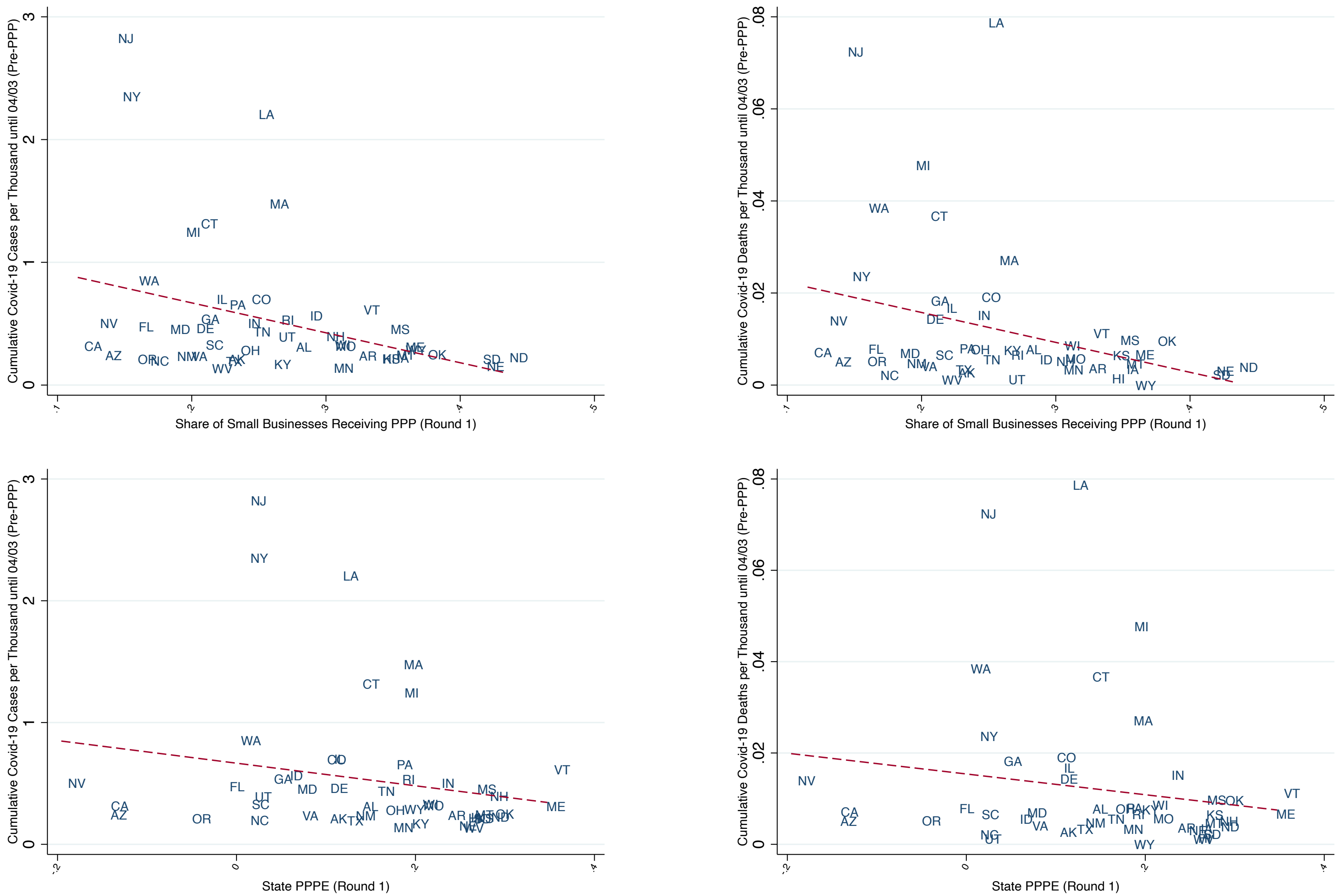


Figure A.10: Pre-PPP Social Distancing and Public Health Interventions and PPP Allocation by State

Figure A.10 presents two scatterplots of the timing of statewide shelter-in-place orders and measures of PPP allocation at the state level and two other scatterplots with measures of pre-PPP social distancing and measures of PPP allocation across states. The figure on the top left panel plots the amount of PPP loans received by small businesses in each state divided by the total number of small businesses in the state. The figure on the top right corner plots the fraction of small businesses in each state that received a PPP loan. The figure on the bottom left corner plots volume based PPPE and the state exposure to the PPPE measured in terms of the total volume of loans. The figure on the bottom right corner plots number of loan based PPPE and the state exposure to the PPPE measured in terms of the total number of loans. Data comes from the New York Times, SBA, Call Reports, and FDIC Summary of Deposits.

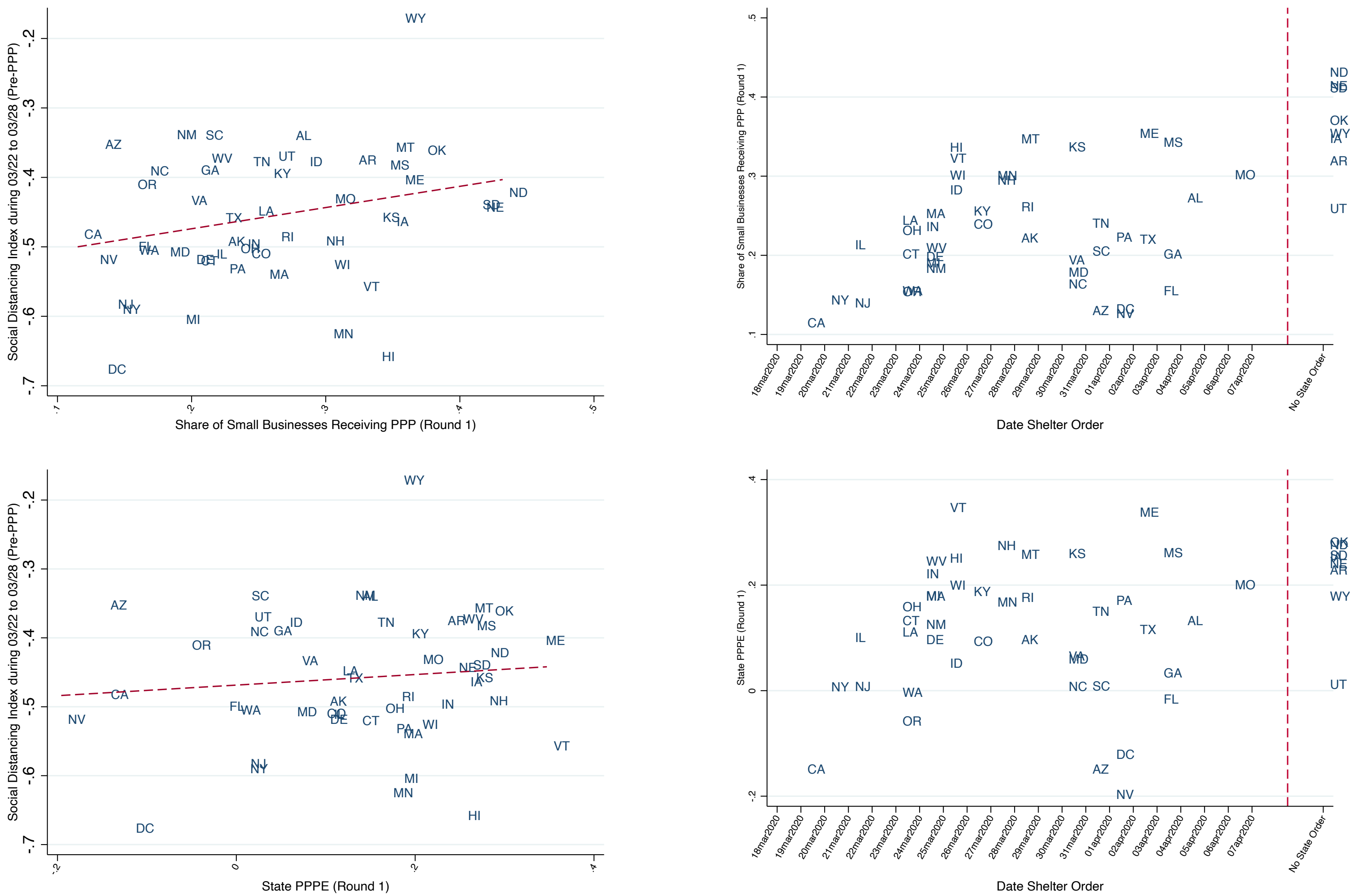


Figure A.11: Weekly Elasticities of PPPE and Employment Outcomes in the Opportunity Insights Employment Dataset

Figure A.11 plots coefficients and respective standard errors of regression analyses investigating the impact of exposure to PPPE on differences on employment and county-week outcomes defined as the difference between these outcomes in each week relative to the average of these variables in the two weeks that preceded the launch of PPP. The figures plot the estimated coefficients, β, and respective standard errors of $\Delta Emplohment_{isj} = \alpha_{sj} + \beta County\ PPPE_i + \Gamma X_{ist} + \epsilon_{ist}$, where $\Delta Shutdown_{isj}$ is the difference between the shutdown indicator of firm i in each week and the average shutdown indicator of the same county during the two weeks prior to the launch of PPP, $County\ PPPE_i$ is the average exposure of the county to bank PPPE, α_{sj} are state-by-industry fixed effects and X_{isj} are additional control variables. The top left panel shows results for all workers, the top right panel shows results for works in the middle two earnings quartiles, the bottom left panel shows results for the lower earnings quartile of workers while the bottom right panel shows results for the top earnings quartile of workers. Data is from SBA and Earnin.

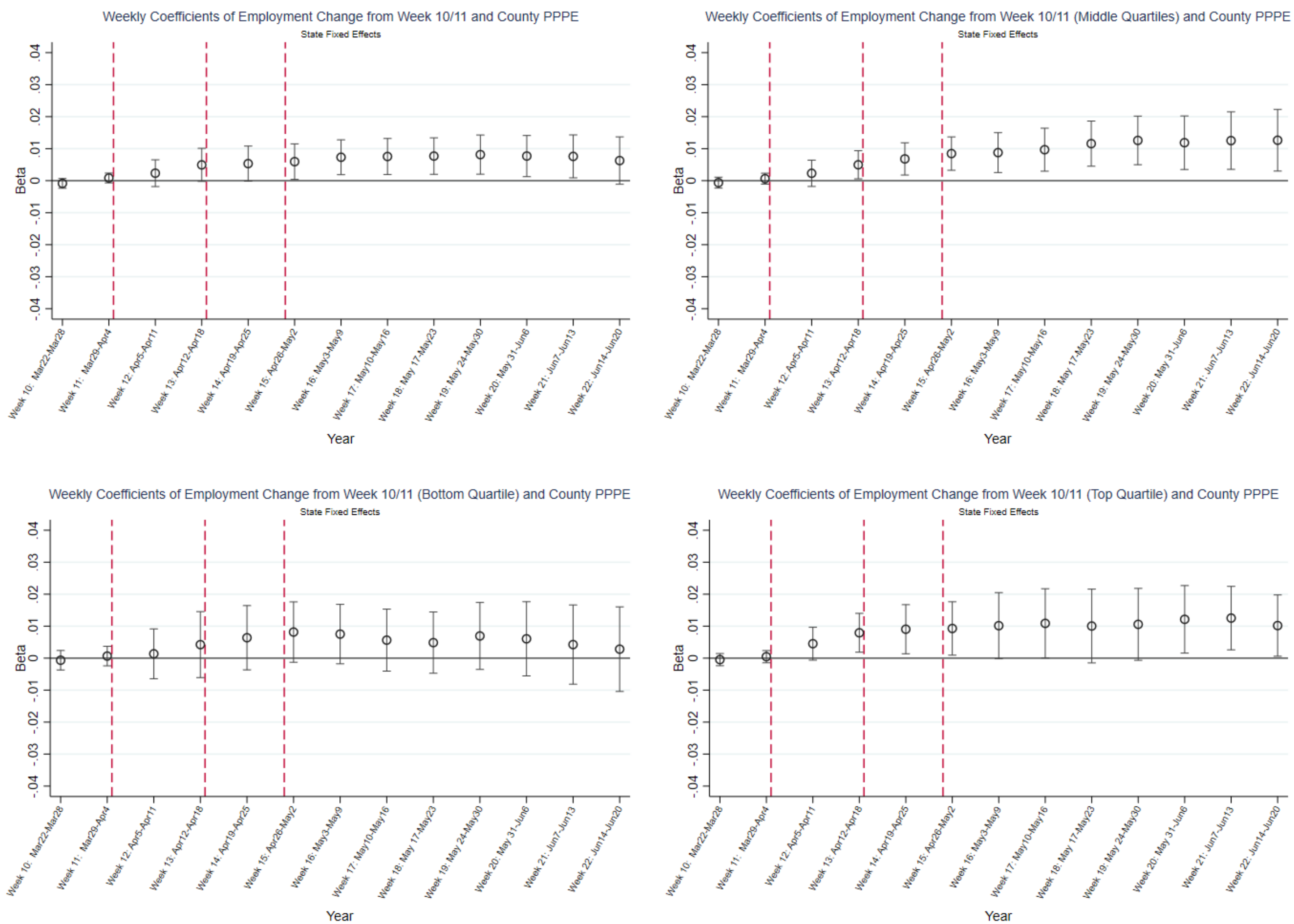


Figure A.12: Share of Households reporting receiving no pay and Exposure to State PPPE

Figure A.12 are scatterplots of state exposure to the number-based PPPE in Round 1 and the percentage of households and the share of households in the Census Household Pulse Survey that report not receiving any payment for time not working in the previous week. The plots represent the evolution of the relation between Round 1 State PPPE and the share of respondent reporting receiving no pay over the first six weeks of the survey. Data comes from the Census Bureau and SBA.

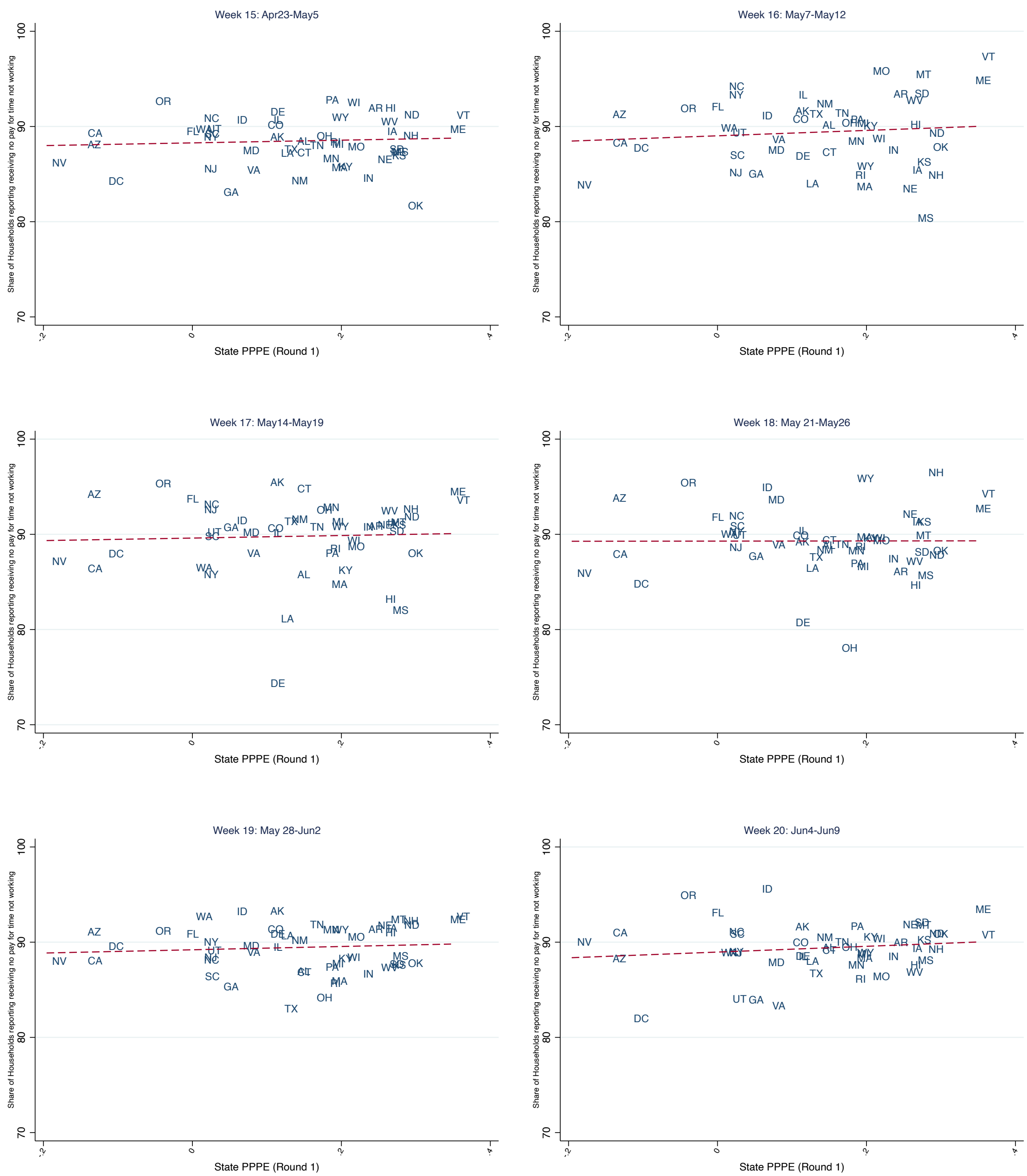


Figure A.13: Unmet PPP Demand and Exposure to State PPPE (Industry×State)

Figure A.13 are scatterplots of state exposure to the number-based PPPE and the percentage of businesses in each industry within a state that applied but did not receive PPP. The plots represent the evolution of the two variables for each survey week. Data comes from the Census Bureau, SBA, Call Reports, Summary of Deposits, and County Business Patterns.

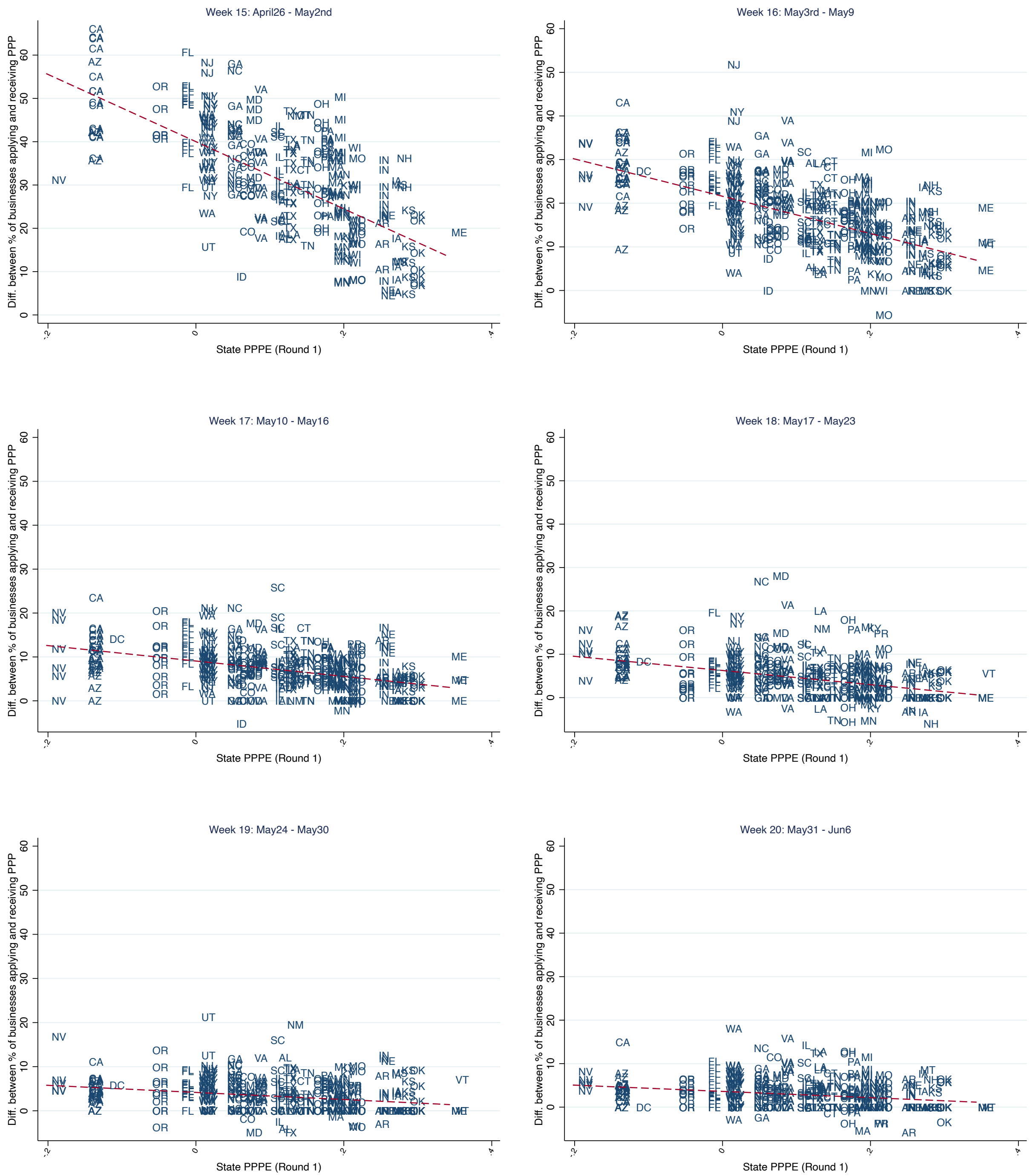


Figure A.14: % Receiving PPP and Exposure to MSA PPPE

Figure A.14 are scatterplots of MSA exposure to the number-based PPPE and the percentage of businesses in each MSA that applied but did not receive PPP in each week. The plots represent the evolution of the two variables for each survey week. Data comes from the Census Bureau, SBA, Call Reports, Summary of Deposits, and County Business Patterns.

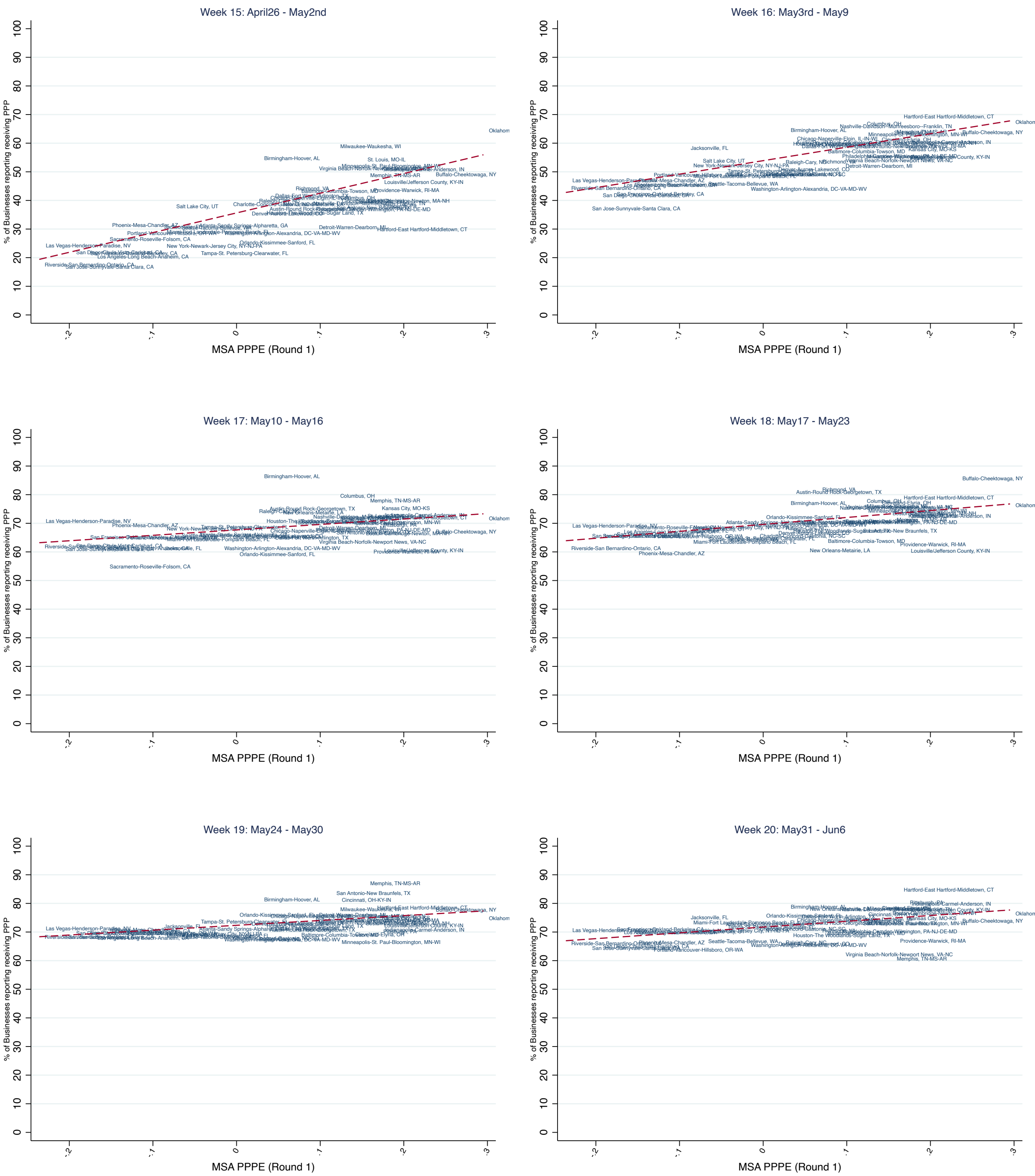


Figure A.15: Unmet PPP Demand and Exposure to MSA PPPE

Figure A.15 are scatterplots of MSA exposure to the number-based PPPE and the percentage of businesses in each MSA that applied but did not receive PPP in each week. The plots represent the evolution of the two variables for each survey week. Data comes from the Census Bureau, SBA, Call Reports, Summary of Deposits, and County Business Patterns.

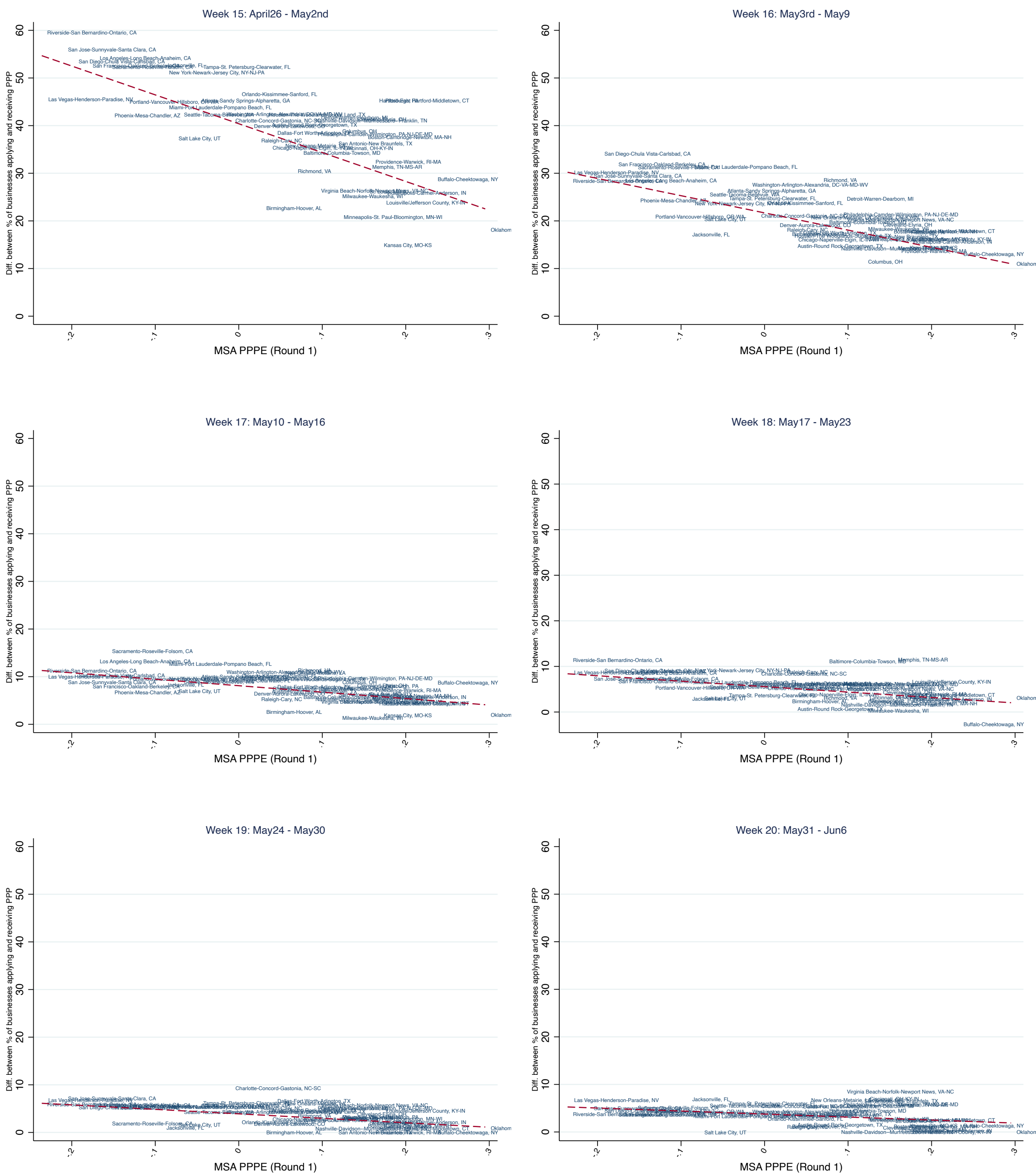


Figure A.16: UCC Filings Over Time

Figure A.16 shows the number of UCC filings between January and July 2020. Data comes from California UCC filings. These UCC laws are set at the state-level, although the National Conference of Commissioners has sought to make them fairly uniform across states. We are able to observe the names and addresses of the debtor and the secured property, together with the description of the property that has a security interest. We subsequently match the UCC data with firm-level information from HomeBase. We refer readers to Edgerton (2012) for further details about the UCC data and its features.

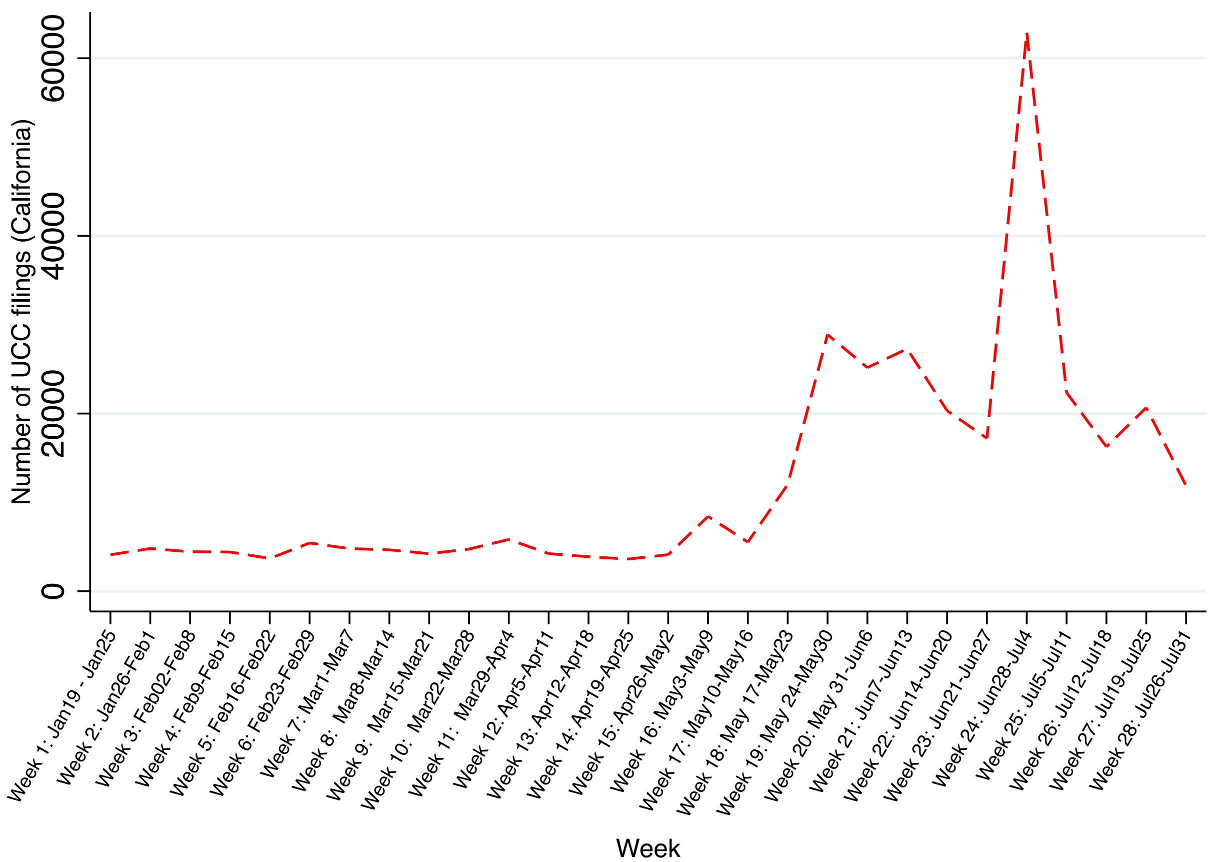


Figure A.17: UCC Filings and PPPE

Figure A.17 are scatterplots of the the ratio of UCC filings per establishment and the county exposure to the number-based PPPE. Data comes from SBA, Call Reports, Summary of Deposits, County Business Patterns and California UCC filings.

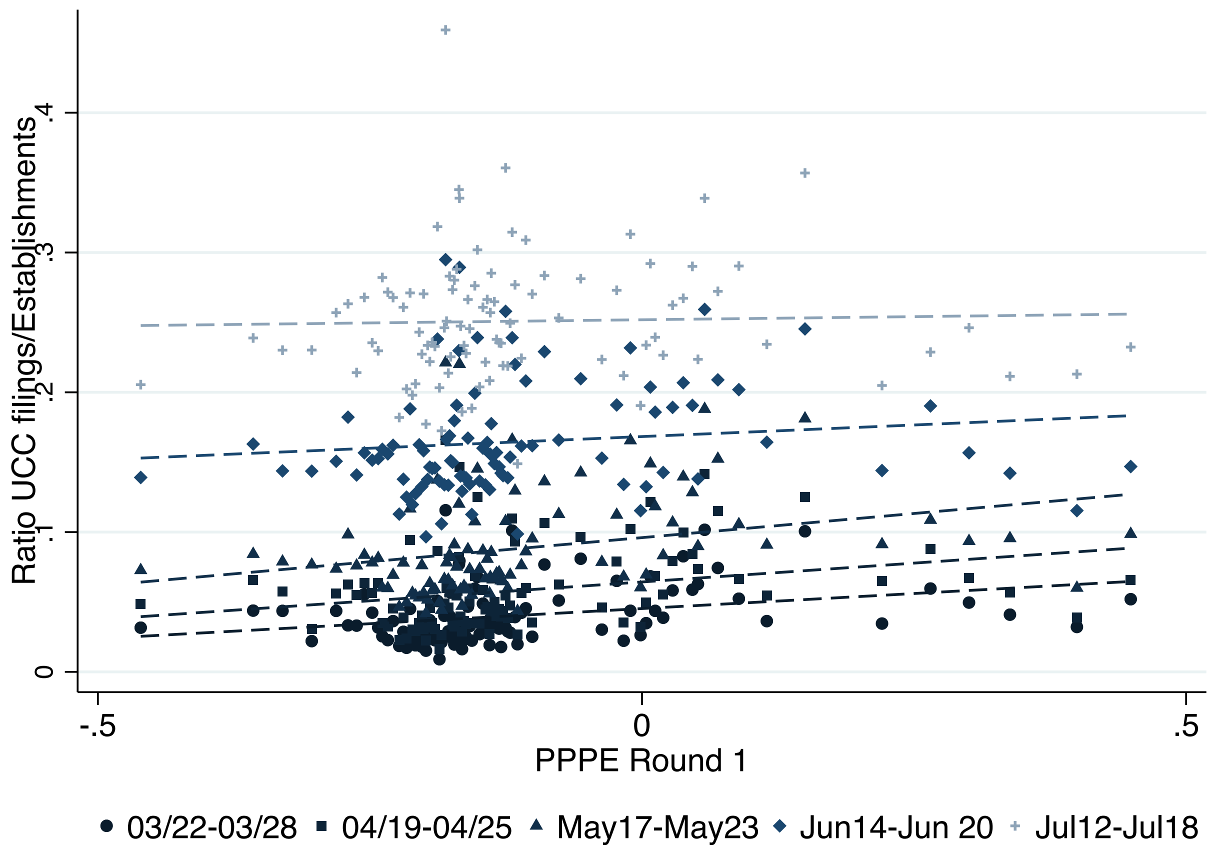


Figure A.18: EIDL and PPP

Figure A.18 shows cumulative PPP and EIDL lending between April and July, 2020. Data comes from the SBA.

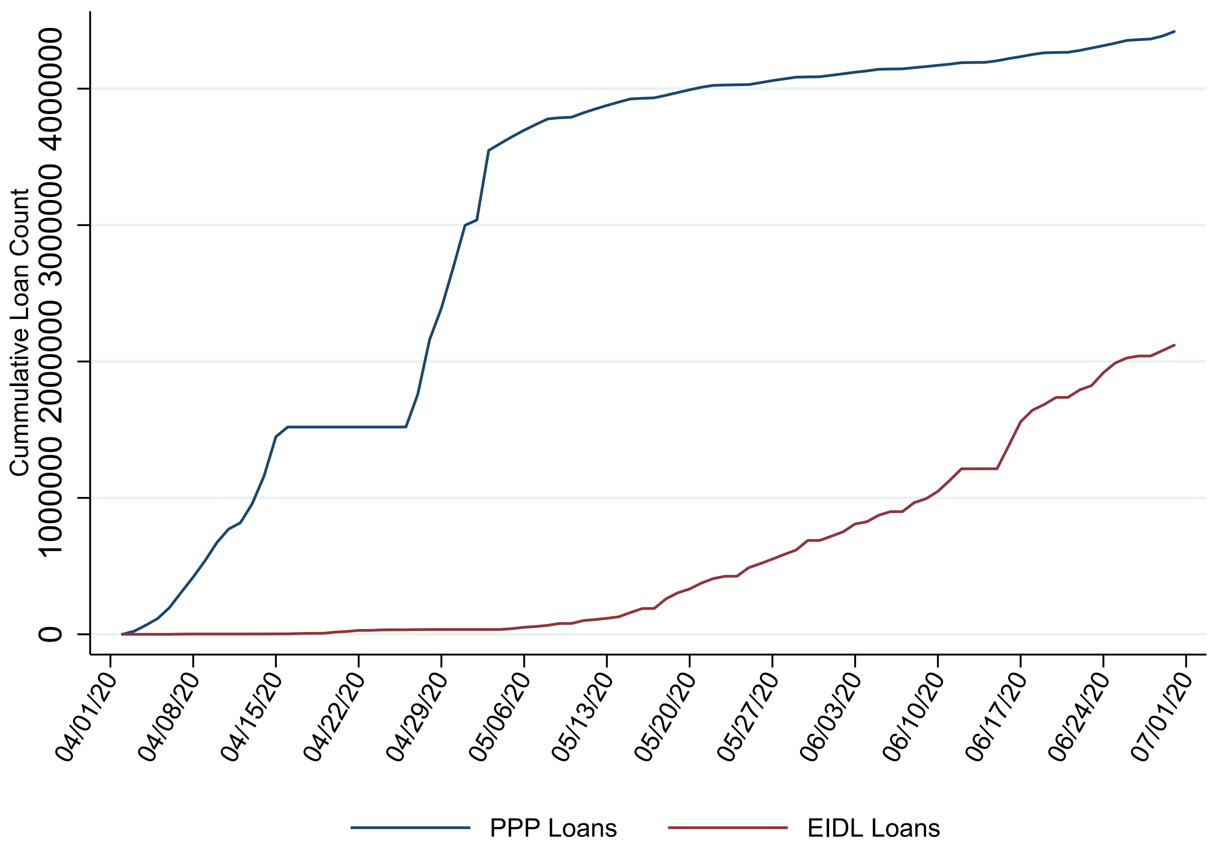
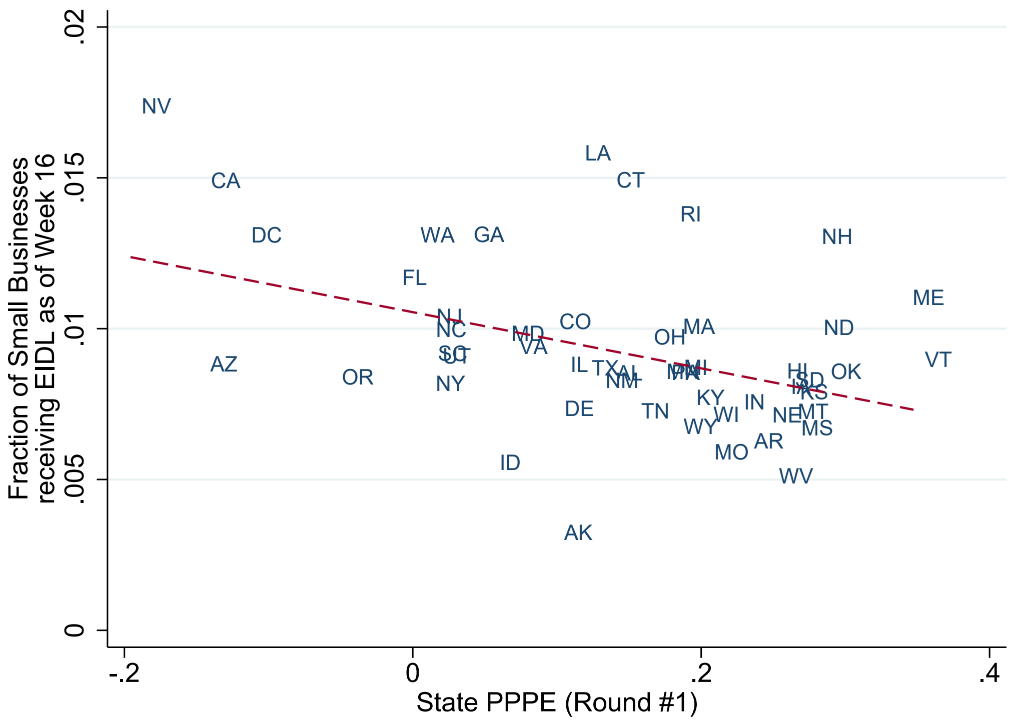


Figure A.19: EIDL and PPPE

Figure A.19 are scatterplots of the average fraction of small business establishment that received an EIDL loan in each percentile bin based on the county exposure to the number-based PPPE. PPP fraction and county PPPE are demeaned using their respective state averages. Data comes from SBA, Call Reports, Summary of Deposits, and County Business Patterns.



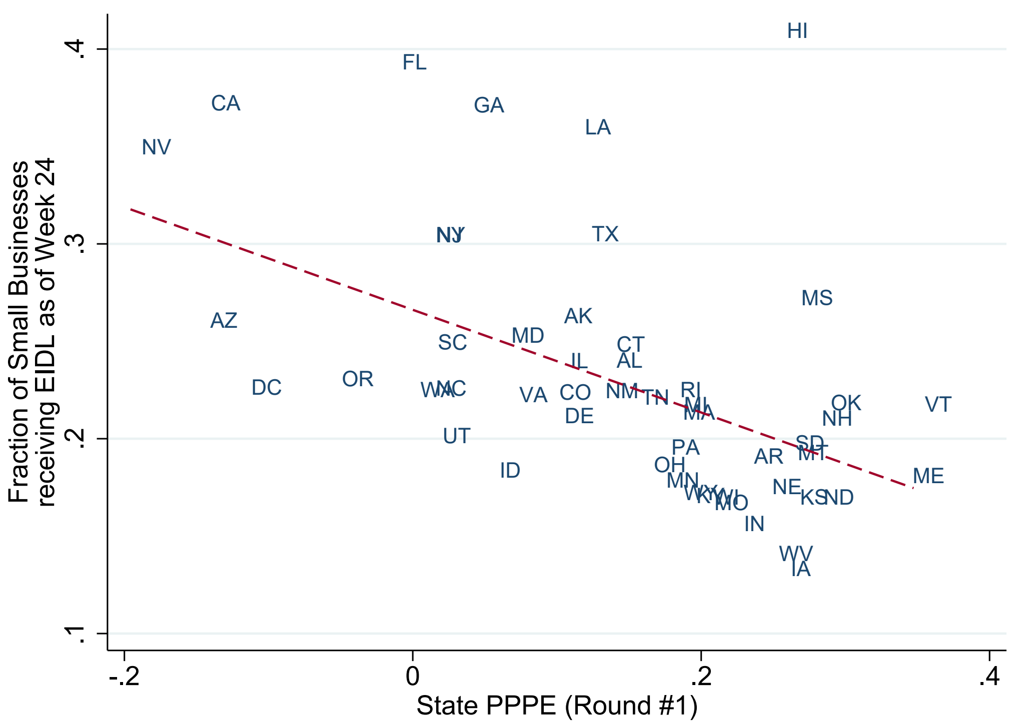


Figure A.20: PPP Lending and Commercial & Industrial Loans

Figure A.20 are scatterplots of the ratio of Commercial and Industrial Loans in Q2 2020 and Commercial and Industrial Loans in Q1 2020 ($\frac{C\&I Loans_{Q2}}{C\&I Loans_{Q1}}$) and the ratio of PPP loans and Commercial and Industrial Loans in Q1 2020 $\frac{PPP Loans}{C\&I Loans_{Q1}}$. Data comes from Federal Reserve Call Reports.

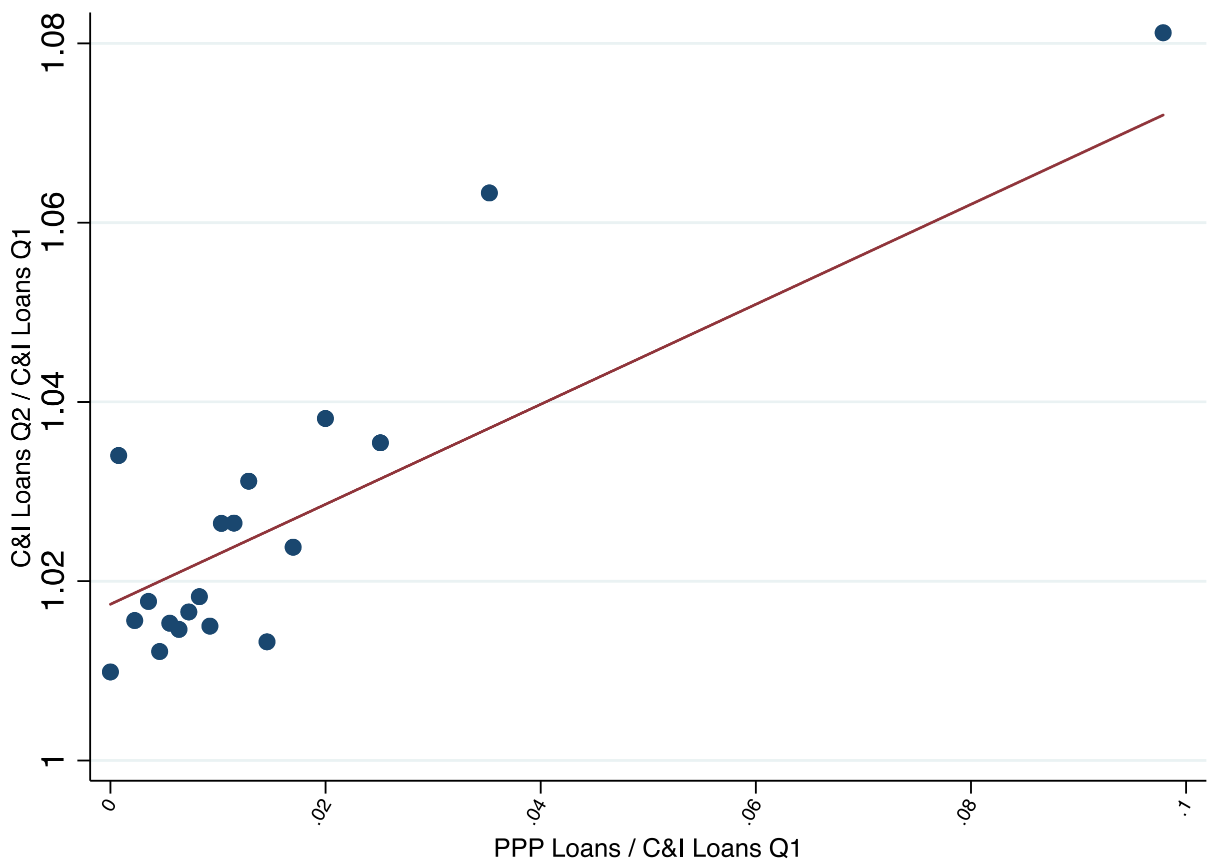
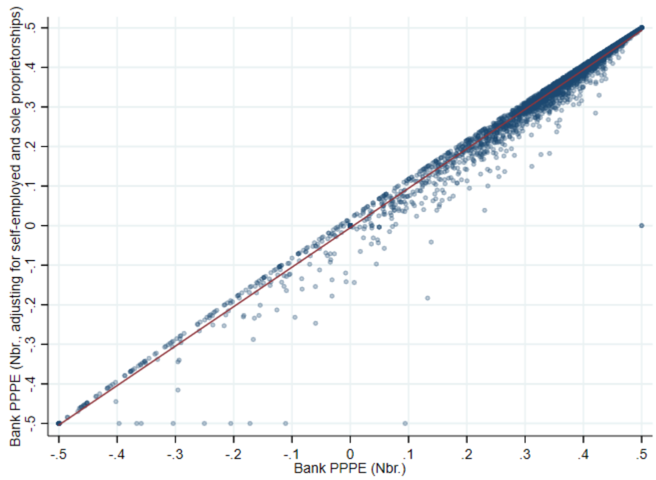


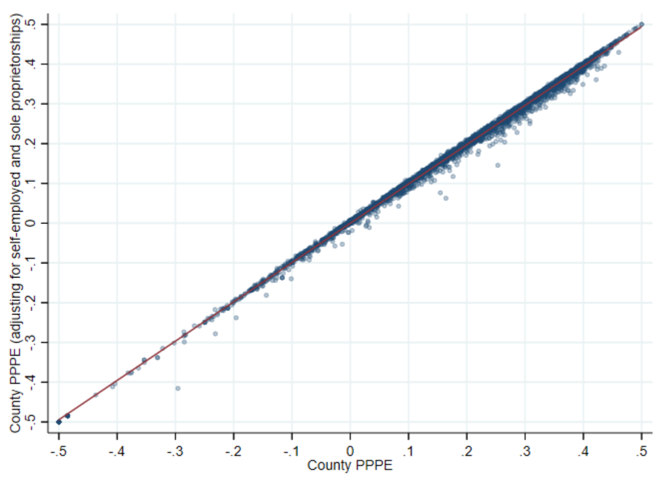
Figure A.21: PPPE in Round #1 with and without Self-Employed and Sole-Proprietorships

Figure A.21 plots the PPPE at the bank-, county-, and zip- level computed with and without counts of PPP loans to self-employed individuals and sole-proprietorships. Data comes from SBA and Summary of Deposits.

Panel A: The Comparison of Number-Based Bank PPPE Round #1 Measures with and without Self-Employed Individuals



Panel B: The Comparison of Number-Based County PPPE Round #1 Measures with and without Self-Employed Individuals



Panel C: The Comparison of Number-Based Zip PPPE Round #1 Measures with and without Self-Employed Individuals

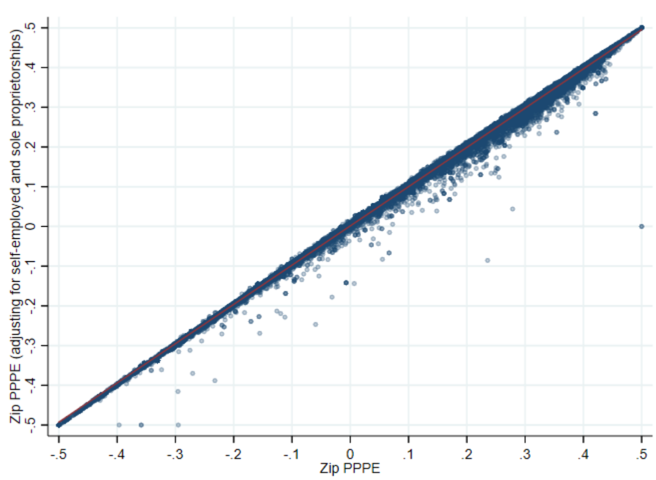


Table A.1: PPP in Bank Websites and PPP Performance

Table A.1 reports results of OLS regressions examining the relation between bank PPPE and the availability of information about applications to the PPP program in each bank's internet websites as of April 10th, 2020. The dependent variables are *PPP info*, *Receiving PPP applications*, and *Online Application*. *PPP info* is an indicator variables that takes the value of one if the bank provides any information about the PPP program in its internet website. *Receiving PPP applications* is an indicator variables that takes the value of one if the bank states in its website that is receiving applications to the PPP program as of April 10th. *Online Application* is an indicator variables that takes the value of one if the bank receives online applications through its internet website. *Bank PPPE (Round #1)* is the bank PPPE measured as of the end of round one of the PPP. Data about the PPP offerings in bank's websites was hand-collected from banks' websites during April 9th and April 10th, 2020. Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	PPP info		Receiving PPP applications		Online Application	
Bank PPPE (Round #1)	0.186***	0.170***	0.177***	0.162***	0.047**	0.036*
	(0.028)	(0.029)	(0.027)	(0.027)	(0.020)	(0.020)
ln(Assets)	0.121***	0.118***	0.103***	0.101***	0.056***	0.056***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.004)	(0.005)
Observations	4857	4856	4857	4856	4857	4856
Adjusted R^2	0.167	0.182	0.137	0.147	0.059	0.066
State Fixed Effects	No	Yes	No	Yes	No	Yes

Table A.2: Business Shutdowns and PPP Targeting

Table A.2 reports the results of ordinary least squares (OLS) regressions examining the relation between the allocation of PPP funds and the share of businesses that shutdown operations in the last week of March. The dependent variable, *Share of Firms Shutdown during Week 10*, is the share of businesses within a zip code and industry group that did not operate in the week of March 22nd to March 28th. *Fraction Receiving PPP (Round #1)* is the percentage of establishments in the zip code that received PPP funding during the first round. *Amount PPP per establishment (Round #1)* is the amount of PPP lending per establishment in the zip code during the first round. *ZIP PPPE (Round #1)* is the weighted average of the PPPE based on the number of outstanding loans, weighted by the share of branches of each bank in within 10 miles of the center of each zip. *ZIP Vol. PPPE (Round #1)* is the weighted average of the PPPE based on the total amount of lending, weighted by the share of branches of each bank in within 10 miles of the center of each zip. Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of Firms Shutdown during Week 10							
Fraction Receiving PPP (Round #1)	-0.023***	-0.009						
	(0.004)	(0.006)						
Amount PPP per establishment (Round #1)			0.004	0.002				
			(0.003)	(0.003)				
ZIP PPPE (Round #1)					-0.182***	-0.068		
					(0.027)	(0.069)		
ZIP Vol. PPPE (Round #1)							-0.133***	-0.042
							(0.038)	(0.094)
Observations	20821	20270	20821	20270	20608	20056	20608	20056
Adjusted R^2	0.062	0.067	0.060	0.067	0.062	0.067	0.061	0.067
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Table A.3: Decline in Hours Worked and PPP Targeting

Table A.3 reports the results of ordinary least squares (OLS) regressions examining the relation between the allocation of PPP funds and the share of businesses that shutdown operations in the last week of March. The dependent variable, *Decline in Hours Worked in Week 10 relative to Jan.*, is the average decline in hours worked within a zip code and industry group between the last two weeks of January and the week of March 22nd to March 28th. *Fraction Receiving PPP (Round #1)* is the percentage of establishments in the zip code that received PPP funding during the first round. *Amount PPP per establishment (Round #1)* is the amount of PPP lending per establishment in the zip code during the first round. *ZIP PPPE (Round #1)* is the weighted average of the PPPE based on the number of outstanding loans, weighted by the share of branches of each bank in within 10 miles of the center of each zip. *ZIP Vol. PPPE (Round #1)* is the weighted average of the PPPE based on the total amount of lending, weighted by the share of branches of each bank in within 10 miles of the center of each zip. Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Decline in Hours Worked in Week 10 relative to Jan.							
Fraction Receiving PPP (Round #1)	0.016***	-0.008						
	(0.005)	(0.006)						
Amount PPP per establishment (Round #1)			-0.010**	-0.006				
			(0.004)	(0.004)				
ZIP PPPE (Round #1)					0.197***	0.085		
					(0.029)	(0.065)		
ZIP Vol. PPPE (Round #1)							0.128**	0.095
							(0.051)	(0.109)
Observations	20821	20270	20821	20270	20608	20056	20608	20056
Adjusted R^2	0.090	0.116	0.090	0.116	0.091	0.115	0.089	0.115
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
County Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes

Table A.4: Evolution of Other Employment Outcomes in the Opportunity Insights Employment Dataset

Table A.4 reports the results of ordinary least squares (OLS) regressions examining the relation between the geographic allocation of PPP funds during the first round and county-level employment. The dependent variable Δ *Empl* is the difference in employment in a week and its average shutdown status in weeks 10 and 11. The first column shows results for all workers, the second column shows results for the first earnings quartile of workers, the third column shows results for workers in the middle two earnings quartiles while the last column shows results for the top earnings quartile of workers. *County PPPE (Round #1)* is the weighted average of the PPPE based on the number of outstanding loans, weighted by the share of branches of each bank in each county. *I(Month=April)* is an indicator variable that takes the value of one in the weeks that span the month of April starting with the week of April 5th to April 12th and ending in April 26th – May 2nd (inclusive), *I(Month=May)* is an indicator variable that takes the value of one in the weeks that span the month of May starting with the week of May 3rd to May 9th and ending in the week of May 24th to May 30th (inclusive), *I(Month=June)* is an indicator variable that takes the value of one in the weeks that span the month of June starting with the week of May 31st to June 6th and ending in the week of June 28th (inclusive); Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1) Δ Empl	(2) Δ Empl (Q1)	(3) Δ Empl (Q2&3)	(4) Δ Empl (Q4)
County PPPE (Round #1) × I(Month=April)	0.006***	0.009**	0.010***	0.008**
	(0.002)	(0.004)	(0.002)	(0.003)
County PPPE (Round #1) × I(Month=May)	0.008***	0.011**	0.014***	0.012**
	(0.003)	(0.005)	(0.004)	(0.005)
County PPPE (Round #1) × I(Month=June)	0.006	0.007	0.014**	0.013**
	(0.004)	(0.008)	(0.006)	(0.005)
Observations	17584	11886	13524	8176
Adjusted R^2	0.703	0.744	0.710	0.741
Other Control Variables	No	No	No	No
State×Week Fixed Effects	Yes	Yes	Yes	Yes
County×Industry Fixed Effects	No	No	No	No

Table A.5: Unmet Demand for Loans and Exposure to PPP

Table A.5 reports the results of ordinary least squares (OLS) regressions examining the relation between the geographic allocation of PPP funds during the first round and the percentage of businesses reporting applying but not receiving PPP funds at the state-by-industry level. The dependent variable is the difference between the percentage of businesses that applied for PPP funds and the percentage of businesses that received PPP funds at the state-by-industry level. *State PPPE (Nbr.)* is the state average of the PPPE based on the number of outstanding loans. All specifications include industry×week fixed-effects. Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	% PPP Requested - % PPP Received						
State PPPE (Nbr.)	-24.564***	-78.463***	-42.087***	-16.478***	-15.676***	-8.184***	-6.107***
	(2.525)	(7.743)	(3.943)	(2.851)	(1.975)	(1.789)	(1.333)
Observations	2229	277	386	390	390	393	393
Adjusted R^2	0.757	0.562	0.421	0.247	0.170	0.074	0.096
Industry Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Week Fixed Effects	Yes	No	No	No	No	No	No
Week:	Full Sample	Apr26–May2	May3–May9	May10–May16	May17–May23	May24–May30	May31–Jun6

Table A.6: Unmet Demand for PPP and Missed Loan Payments

Table A.6 reports the results of ordinary least squares (OLS) and instrumental variables (IV) regressions examining the relation between the geographic allocation of PPP funds during the first round and the percentage of businesses reporting missing scheduled payments at the state-by-industry level. The dependent variable is the percentage of firms reporting a missed scheduled loan payment. *% PPP Requested - % PPP Received* is the difference between the percentage of businesses that applied for PPP funds and the percentage of businesses that received PPP funds at a state-by-industry level. *State PPPE (Nbr.)* is the state average of the PPPE based on the number of outstanding loans. *Pre-PPP Decline Hours Worked* is the average decline in hours worked in each state between January and the last week of March. *Pre-PPP State Covid-19 Cases (per capita)* are per capita number of reported Covid-19 cases in the state. *Pre-PPP State Covid-19 Deaths (per capita)* are the weekly per capita number of reported Covid-19 deaths in the state. *Pre-PPP State Social Distancing Index* is the change in average distance travelled in the state until the end of March using individuals' GPS signals. All specifications include industry×week fixed-effects. Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
LHS Variable	% Missing Loan Payments	% Missing Loan Payments	% PPP Req. - % PPP Rec.	% Missing Loan Payments
% PPP Requested - % PPP Received	0.056***			0.219***
	(0.020)			(0.043)
State PPPE (Round 1)		-4.401***	-23.458***	
		(1.380)	(2.264)	
Observations	2217	2487	2217	2217
F-Statistic				107.388
LHS Variable	% Missing Schd. Payments	% Missing Schd. Payments	% PPP Req. - % PPP Rec.	% Missing Schd. Payments
% PPP Requested - % PPP Received	0.229***			0.639***
	(0.029)			(0.068)
State PPPE (Round 1)		-14.807***	-23.577***	
		(2.651)	(2.211)	
Observations	2205	2448	2205	2205
F-Statistic				113.656
LHS Variable	% Cash 3 months	% Cash 3 months	% PPP Req. - % PPP Rec.	% Cash 3 months
% PPP Requested - % PPP Received	-0.172***			-0.411**
	(0.047)			(0.152)
State PPPE (Round 1)		8.920**	-21.380***	
		(3.768)	(2.019)	
Observations	903	918	903	903
F-Statistic				112.191
Controls	Yes	Yes	Yes	Yes
Industry×Week Fixed Effects	Yes	Yes	Yes	Yes

Table A.7: % Receiving PPP and Exposure to State PPPE

Table A.7 reports the results of ordinary least squares (OLS) regressions examining the relation between the geographic allocation of PPP funds during the first round and the percentage of firms reporting having received PPP funds at the state-by-industry level in each week of the survey collected from the Census Small Business Pulse Survey. The dependent variable is the percentage of businesses that received PPP funds in a state-by-industry group during each week of the survey. *State PPPE (Nbr.)* is the state average of the PPPE based on the number of outstanding loans. Standard errors are presented in parentheses, and are double-clustered at the level of the state and week. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				% PPP Received			
State PPPE (Nbr.)	35.865***	89.549***	51.603***	23.899***	28.381***	17.061***	24.315***
	(3.201)	(8.169)	(4.298)	(3.594)	(5.024)	(3.610)	(3.879)
Observations	2230	277	386	390	390	393	394
Adjusted R^2	0.653	0.629	0.529	0.397	0.416	0.387	0.516
Industry Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Week Fixed Effects	Yes	No	No	No	No	No	No
Week:	Full Sample	Apr26–May2	May3–May9	May10–May16	May17–May23	May24–May30	May31–Jun6

Table A.8: % Receiving PPP and % Missing Payments: Week-by-Week

Table A.8 reports the results of instrumental variables (IV) regressions split week by week examining the relation between the geographic allocation of PPP funds during the first round and the percentage of businesses reporting missing scheduled payments at the state-by-industry level. In the top panel, the dependent variable is the percentage of firms reporting a missed scheduled loan payment. In the bottom panel, the dependent variable is the percentage of firms reporting a missed other scheduled payment such as rent, utilities, and payroll. *% PPP Received* is the percentage of businesses reporting having received PPP funds in a state-by-industry group. *State PPPE (Nbr.)* is the state average of the PPPE based on the number of outstanding loans. All specifications include industry×week fixed-effects. Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				% Missing Loan Payments			
% PPP Received	-0.173***	-0.068*	-0.148***	-0.278***	-0.210***	-0.349***	-0.208**
	(0.041)	(0.034)	(0.048)	(0.097)	(0.075)	(0.122)	(0.091)
Observations	2229	277	386	390	389	393	394
F-Statistic	126.358	120.162	144.125	44.222	30.384	22.335	39.285
				% Missing Schd. Payments			
% PPP Received	-0.555***	-0.282***	-0.468***	-0.880***	-0.651***	-0.998***	-0.630***
	(0.110)	(0.060)	(0.092)	(0.187)	(0.226)	(0.291)	(0.218)
Observations	2216	277	385	387	388	389	390
F-Statistic	131.427	120.162	144.126	44.423	29.598	25.182	39.759
				% Cash 3 months			
% PPP Received	0.283**	0.091	0.153	0.327*	0.285	0.619**	0.555
	(0.121)	(0.079)	(0.105)	(0.188)	(0.310)	(0.279)	(0.348)
Observations	903	95	141	136	177	180	174
F-Statistic	144.087	50.909	84.021	49.197	16.342	32.496	
Controls	No	No	No	No	No	No	No
Industry Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Week Fixed Effects	Yes	No	No	No	No	No	No
Week:	Full Sample	Apr26–May2	May3–May9	May10–May16	May17–May23	May24–May30	May31–Jun6

Table A.9: % Receiving PPP and Exposure to PPP: MSA Level

Table A.9 reports the results of ordinary least squares (OLS) regressions examining the relation between the geographic allocation of PPP funds during the first round and the percentage of businesses reporting applying but not receiving PPP funds at the MSA level. The dependent variable is the difference between the percentage of businesses that applied for PPP funds and the percentage of businesses that received PPP funds at the MSA level. *MSA PPPE (Nbr.)* is the MSA average of the PPPE based on the number of outstanding loans. Standard errors are presented in parentheses, and are clustered at the level of the MSA. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	% PPP Received						
MSA PPPE (Nbr.)	32.714***	68.793***	47.107***	19.004***	24.207***	16.938***	20.232***
	(6.579)	(6.202)	(4.106)	(5.071)	(4.977)	(3.356)	(4.363)
Observations	300	50	50	50	50	50	50
Adjusted R^2	0.083	0.656	0.657	0.171	0.284	0.242	0.241
Week Fixed Effects	Yes	No	No	No	No	No	No
Week:	Full Sample	Apr26–May2	May3–May9	May10–May16	May17–May23	May24–May30	May31–Jun6

Table A.10: % Receiving PPP and Missed Payments: MSA-Level

Table A.10 reports the results of ordinary least squares (OLS) and instrumental variables (IV) regressions examining the relation between the geographic allocation of PPP funds during the first round and the percentage of firms reporting missing payments at the MSA level collected from the Census Small Business Pulse Survey. In the top panel, the dependent variable is the percentage of firms at the MSA reporting a missed scheduled loan payment. In the bottom panel, the dependent variable is the percentage of firms reporting a missed other scheduled payment such as rent, utilities, and payroll. *% PPP Received* is the percentage of businesses reporting having received PPP funds in a state-by-industry group. *MSA PPPE (Nbr.)* is the MSA average of the PPPE based on the number of outstanding loans. All specifications include week fixed-effects. Robust standard errors are presented in parentheses. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
LHS Variable	% Missing Loan Payments	% Missing Loan Payments	% PPP Received	% Missing Loan Payments
% PPP Received	-0.154***			-0.181***
	(0.013)			(0.042)
MSA PPPE (Nbr.)		-5.902***	32.652***	
		(1.583)	(6.579)	
Observations	298	298	298	298
F-Statistic				24.635
LHS Variable	% Missing Schd. Payments	% Missing Schd. Payments	% PPP Received	% Missing Schd. Payments
% PPP Received	-0.235***			-0.451***
	(0.019)			(0.085)
MSA PPPE (Nbr.)		-14.753***	32.714***	
		(2.438)	(6.579)	
Observations	300	300	300	300
F-Statistic				24.728
LHS Variable	% Cash 3 months	% Cash 3 months	% PPP Received	% Cash 3 months
% PPP Received	0.172***			-0.172*
	(0.018)			(0.097)
MSA PPPE (Nbr.)		-5.628**	32.714***	
		(2.468)	(6.579)	
Observations	300	300	300	300
F-Statistic				24.728
Controls	No	No	No	No
Week Fixed Effects	Yes	Yes	Yes	Yes

Table A.11: Unmet Demand and Exposure to PPP: MSA Level

Table A.11 reports the results of ordinary least squares (OLS) regressions examining the relation between the geographic allocation of PPP funds during the first round and the percentage of businesses reporting applying but not receiving PPP funds at the MSA level. The dependent variable is the difference between the percentage of businesses that applied for PPP funds and the percentage of businesses that received PPP funds at the MSA level. *MSA PPPE (Nbr.)* is the MSA average of the PPPE based on the number of outstanding loans. Robust standard errors are presented in parentheses. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	% PPP Requested - % PPP Received						
MSA PPPE (Nbr.)	-22.987***	-60.564***	-36.165***	-13.531***	-11.946***	-9.384***	-6.331***
	(6.526)	(7.053)	(3.150)	(2.527)	(2.570)	(1.500)	(1.845)
Observations	300	50	50	50	50	50	50
Adjusted R^2	0.043	0.585	0.643	0.360	0.270	0.298	0.121
Week Fixed Effects	Yes	No	No	No	No	No	No
Week:	Full Sample	Apr26–May2	May3–May9	May10–May16	May17–May23	May24–May30	May31–Jun6

Table A.12: Unmet Demand and Missed Payments:MSA Level

Table A.12 reports the results of ordinary least squares (OLS) and instrumental variables (IV) regressions examining the relation between the geographic allocation of PPP funds during the first round and the percentage of businesses reporting missing scheduled payments at the MSA level. In the top panel, the dependent variable is the percentage of firms reporting a missed scheduled loan payment. In the bottom panel, the dependent variable is the percentage of firms reporting a missed other scheduled payment such as rent, utilities, and payroll. *% PPP Requested - % PPP Received* is the difference between the percentage of businesses that applied for PPP funds and the percentage of businesses that received PPP funds at the MSA level. *MSA PPPE (Nbr.)* is the MSA average of the PPPE based on the number of outstanding loans. All specifications include week fixed-effects. Robust standard errors are presented in parentheses. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
LHS Variable	% Missing Loan Payments	% Missing Loan Payments	% PPP Request - % PPP Received	% Missing Loan Payments
% PPP Requested - % PPP Received	0.166***			0.258***
	(0.013)			(0.067)
MSA PPPE (Nbr.)		-5.902***	-22.911***	
		(1.583)	(6.526)	
Observations	298	298	298	298
F-Statistic				12.325
LHS Variable	% Missing Schd. Payments	% Missing Schd. Payments	% PPP Request - % PPP Received	% Missing Schd. Payments
% PPP Requested - % PPP Received	0.258***			0.642***
	(0.020)			(0.155)
MSA PPPE (Nbr.)		-14.753***	-22.987***	
		(2.438)	(6.526)	
Observations	300	300	300	300
F-Statistic				12.406
LHS Variable	% Cash 3 months	% Cash 3 months	% PPP Request - % PPP Received	% Cash 3 months
% PPP Requested - % PPP Received	-0.203***			0.245
	(0.019)			(0.156)
MSA PPPE (Nbr.)		-5.628**	-22.987***	
		(2.468)	(6.526)	
Observations	300	300	300	300
F-Statistic				12.406
Controls	No	No	No	No
Week Fixed Effects	Yes	Yes	Yes	Yes

Table A.13: PPP Lending and Crowd-Out

Table A.13 reports the results of ordinary least squares (OLS) and instrumental variables (IV) regressions examining the relation between the ratio of Commercial and Industrial Loans in Q2 2020 and Commercial and Industrial Loans in Q1 2020 ($\frac{C\&ILoans_{Q2}}{C\&ILoans_{Q1}}$) and the ratio of PPP loans and Commercial and Industrial Loans in Q1 2020 $\frac{PPPLoans}{C\&ILoans_{Q1}}$. Column (2) instruments using lender PPPE. Data comes from Federal Reserve Call Reports. Robust standard errors are presented in parentheses. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	OLS	IV
Dep. Variable:	$\frac{C\&ILoans_{Q2}}{C\&ILoans_{Q1}}$	$\frac{C\&ILoans_{Q2}}{C\&ILoans_{Q1}}$
$\frac{PPPLoans}{C\&ILoans_{Q1}}$	0.558^{***}	0.991^{***}
	(0.162)	(0.278)
Constant	1.017^{***}	1.011^{***}
	(0.00255)	(0.00457)
Observations	4845	4845
Adjusted R^2	0.010	0.004

Table A.14: Evolution of Homebase Employment Outcomes and Exposure to PPP: Partition by State UI Replacement Rates

Table A.14 reports the results of ordinary least squares (OLS) regressions examining how differences in the generosity of the state unemployment insurance shapes the relation between the geographic allocation of PPP funds during the first round and the difference between a firm's average employment outcomes in the two weeks prior to the launch of PPP and the firm's outcomes in each of the following weeks. The dependent variable in Panel A, Δ *Bus. Shutdown*, is the difference between the firm's shutdown status in a week and its average shutdown status in weeks 10 and 11. Bus. Shutdown is an indicator variable that takes the value of one if the business reported zero hours worked over the entire week. The dependent variable in Panel B, Δ *Hours Worked*, is the difference in the ratio of hours worked in each establishment in a week and the average ratio of hours worked in that establishment in weeks 10 and 11. The ratio of hours worked in each establishment is measured as the hours worked in that week relative to the hours worked in that same establishment during the last two weeks of January. *Zip PPPE (Round #1)* is the weighted average of the PPPE based on the number of outstanding loans, weighted by the share of branches of each bank in within 10 miles of the center of each zip. *I(Month=April)* is an indicator variable that takes the value of one in the weeks that span the month of April starting with the week of April 5th to April 12th and ending in April 26th – May 2nd (inclusive), *I(Month=May)* is an indicator variable that takes the value of one in the weeks that span the month of May starting with the week of May 3rd to May 9th and ending in the week of May 24th to May 30th (inclusive), *I(Month=June)* is an indicator variable that takes the value of one in the weeks that span the month of June starting with the week of May 31st to June 6th and ending in the week of June 28th (inclusive); Other control variables include interactions between the social distance index, Covid cases per capita and Deaths per capita measured as of week 9 interacted with the indicator variables for the months of April, May, and June, Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Δ Bus. Shutdown		Δ Hours Worked	
Zip PPPE (Round #1) × I(Month=April)	0.006	-0.003	0.001	0.005*
	(0.004)	(0.004)	(0.003)	(0.002)
Zip PPPE (Round #1) × I(Month=May)	0.002	-0.004	0.021***	0.017**
	(0.007)	(0.003)	(0.006)	(0.007)
Zip PPPE (Round #1) × I(Month=June)	0.006	-0.004	0.033***	0.024***
	(0.006)	(0.003)	(0.009)	(0.007)
Observations	291195	243495	291195	243495
Adjusted R^2	0.510	0.516	0.573	0.563
Sample	Hi. Rep. Rate	Low Rep. Rate	Hi. Rep. Rate	Low Rep. Rate
Other Control Variables	Yes	Yes	Yes	Yes
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes

Table A.15: Evolution of County Unemployment Outcomes and Exposure to PPP: Partition by State UI Replacement Rates

Table A.15 reports the results of ordinary least squares (OLS) regressions examining how differences in the generosity of the state unemployment insurance shapes the relation between the geographic allocation of PPP funds during the first round and the difference between a county's initial unemployment filings in the two weeks prior to the launch of PPP and the county's initial unemployment filings in each of the following weeks. The dependent variable, Δ *County Initial UI Filings Ratio*, is the difference between the initial county unemployment filings during a week and the average initial unemployment filings in the county in weeks 10 and 11. *County PPPE (Round #1)* is the weighted average of the PPPE based on the number of outstanding loans, weighted by the share of branches of each bank in each county. *I(Month=April)* is an indicator variable that takes the value of one in the weeks that span the month of April starting with the week of April 5th to April 12th and ending in April 26th – May 2nd (inclusive), *I(Month=May)* is an indicator variable that takes the value of one in the weeks that span the month of May starting with the week of May 3rd to May 9th and ending in the week of May 24th to May 30th (inclusive), *I(Month=June)* is an indicator variable that takes the value of one in the weeks that span the month of June starting with the week of May 31st to June 6th and ending in the week of June 28th (inclusive); Other control variables include interactions between the social distance index, Covid cases per capita and Deaths per capita measured as of week 9 interacted with the indicator variables for the months of April, May, and June, Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Δ County Initial UI Filings Ratio			
County PPPE (Round #1) × I(Month=April)	0.016	0.011	0.014	0.021
	(0.025)	(0.038)	(0.025)	(0.037)
County PPPE (Round #1) × I(Month=May)	0.021	0.055	0.018	0.056
	(0.038)	(0.062)	(0.039)	(0.067)
County PPPE (Round #1) × I(Month=June)	0.032	0.103	0.033	0.097
	(0.038)	(0.082)	(0.039)	(0.090)
Observations	15350	14216	15061	13335
Adjusted R^2	0.893	0.832	0.894	0.836
Sample	Hi. Rep. Rate	Low Rep. Rate	Hi. Rep. Rate	Low Rep. Rate
Other Control Variables	No	No	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State×Week Fixed Effects	Yes	Yes	Yes	Yes

Table A.16: Evolution of Small Business Revenues and Exposure to PPP: Partition by State UI Replacement Rates

Table A.16 reports the results of ordinary least squares (OLS) regressions examining how differences in the generosity of the state unemployment insurance shapes the relation between the geographic allocation of PPPE funds during the first round and weekly small business revenues at the county×industry level between the second week of March until the last week of April collected from Womply, a company specializing in processing revenue for small businesses. The dependent variable, Δ *Y/Y Change in Total Consumer Spending*, is the difference between the year-on-year average change in small business revenue of all establishments of a county operating in a 3-digit NAICS industry and the average year-on-year average change in small business revenue of the same county and industry in weeks 10 and 11. *County PPPE (Round #1)* is the weighted average of the PPPE based on the number of outstanding loans, weighted by the share of branches of each bank in each county. *I(Month=April)* is an indicator variable that takes the value of one in the weeks that span the month of April starting with the week of April 5th to April 12th and ending in April 26th – May 2nd (inclusive), *I(Month=May)* is an indicator variable that takes the value of one in the weeks that span the month of May starting with the week of May 3rd to May 9th and ending in the week of May 24th to May 30th (inclusive), *I(Month=June)* is an indicator variable that takes the value of one in the weeks that span the month of June starting with the week of May 31st to June 6th and ending in the week of June 28th (inclusive); Other control variables include interactions between the social distance index, Covid cases per capita and Deaths per capita measured as of week 9 interacted with the indicator variables for the months of April, May, and June, Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Δ Y/Y Change in Total Consumer Spending			
County PPPE (Round #1) × I(Month=April)	0.013*	0.003	0.012*	0.001
	(0.007)	(0.004)	(0.006)	(0.005)
County PPPE (Round #1) × I(Month=May)	0.028**	0.031**	0.026*	0.009
	(0.013)	(0.013)	(0.014)	(0.013)
County PPPE (Round #1) × I(Month=June)	0.041***	0.033***	0.040***	0.009
	(0.012)	(0.010)	(0.014)	(0.013)
Observations	99421	91848	92656	86322
Adjusted R^2	0.481	0.460	0.471	0.451
Sample	Hi. Rep. Rate	Low Rep. Rate	Hi. Rep. Rate	Low Rep. Rate
Other Control Variables	No	No	Yes	Yes
County×Industry Fixed Effects	Yes	Yes	Yes	Yes
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes