



# Did the paycheck protection program hit the target?

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## 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. Second, we exploit regional heterogeneity in lending relationships and individual firm-loan matched data to study the role of banks in explaining the employment effects of the PPP. We find 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.

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## 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, often relying on banks to rapidly deploy funds to firms.<sup>1</sup>

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), and the role of banks in explaining the employment effects of the PPP. Part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, the PPP offered guaranteed, forgivable loans to pro-

<sup>1</sup> 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 et al. (2020b) 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.

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vide liquidity to small and mid-sized businesses and prevent job losses. The PPP 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. The program was administered by the Small Business Administration (SBA) with the loan application process operated by commercial banks. We document substantial heterogeneity across banks in disbursing PPP funds and find that this heterogeneity led to meaningful differences across firms and regions in terms of targeting and employment impacts.

We have three main findings. First, banks played an important role in mediating program targeting. The extent of bank participation in the initial phase of the program depends intuitively on ex ante bank characteristics, including relationships with the SBA, greater reliance on labor relative to automation, and active enforcement actions against banks. These differences in bank participation explain spatial differences in the initial distribution of funds and 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. Our analysis reveals how bank performance differences in loan deployment contribute to these employment effects over time. 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.<sup>2</sup> 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 borrower- and loan-level information. The data offer a clear look at which lenders are most active in disbursing loans, how program participation evolves over time, and the geographic distribution of PPP lending across the US 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-level unemployment insurance claims, the Census Small Business Pulse survey, small business revenue data from Womply, and employment rates from the COVID-19 economic tracker (Chetty et al., 2020).

We consider three dimensions of program targeting. First, did the funds flow to where the economic shock was greatest? 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? Third, why did some banks systematically under- or overperform in disbursing PPP loans relative to their share of the small business loan market?

Preventing unnecessary mass layoffs and firm bankruptcies by injecting liquidity into firms were central goals of the program and the benefits of PPP were likely greatest in areas with more pre-policy economic dislocation. However, 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, employee counts, 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 et al., 2020).

We find significant heterogeneity across banks in terms of disbursing PPP funds, which reflects more than mere differences in underlying loan demand and contributes to the weak correlation between economic declines and PPP lending. Ex ante bank characteristics, including greater labor capacity to process loans, pre-existing SBA relationships, and active enforcement actions against banks, predict banks' relative performance in disbursing PPP loans. The PPP program required lenders to collect and enter information into a custom application and submit it via the SBA portal. Thus, reliance on labor rather than automation, as well as pre-existing access and familiarity with the SBA portal facilitated disbursement of PPP loans, especially in the initial phase. Conversely, banks subject to formal enforcement actions were not automatically approved to make PPP loans, initially leading to lower PPP disbursement for these banks.

Our results on bank participation motivate two complementary research designs to evaluate the PPP using ZIP-level variation in banks' propensity to disburse loans. We use these research designs to study business shutdowns, employment levels, reductions in hours worked, initial unemployment insurance (UI) claims, and small business revenues. We construct measures of geographic exposure to bank performance in the PPP using (1) the distribution of bank branches across geographic regions and (2) geographic exposure to the ex ante bank characteristics that predict PPP disbursement. Both measures exploit the fact that most small business lending is local (Brevoort et al., 2010; Granja et al., 2018). Our first measure compares regions exposed to high-performance banks—whose share of PPP lending exceeded what would be expected from them given their national share of the small business lending market—to regions exposed to low-performance banks—whose PPP lending share underperformed relative to their national share of the small business lending market. This bank-driven 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 second measure further attempts to isolate specific elements of bank performance that we can trace back to bank-supply frictions prevailing prior to the pandemic.

<sup>2</sup> Almeida et al. (2004) and Riddick and Whited (2009) show that uncertainty increases firms' precautionary motives to hold cash, particularly when external financing is difficult to obtain.

We do not find evidence that the PPP had a substantial effect on local employment outcomes or business shutdowns during the first round of the program, and find modest effects on hours worked and employee counts during the second round. We confirm the firm-level evidence by documenting limited impacts on initial UI claims, small business revenues, and employment rates in small businesses 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. Results are qualitatively and quantitatively similar for both measures of exposure to bank performance. Aggregate impacts over the first five months of the program equal 4% of eligible employment, implying a cost-per-job-year of at least \$175,000. 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.

Both research designs are akin to Bartik instruments and rely on the assumption that pre-policy bank branch shares in a given region are not correlated with the outcomes we study. This assumption likely holds once we condition on key observables, including the relationship between PPP funding and the initial severity of the crisis. Our preferred specification conditions on firm- and state-by-time fixed effects, which remove many potential confounding factors from the analysis. We present further evidence supporting the Bartik assumption from pre-trends comparisons between high- and low-exposure groups and diagnostics for unpacking the Bartik instrument following Goldsmith-Pinkham et al. (2020).

We complement our aggregate regional designs with a timing design using matched firm-loan data. We match by name 10,694 firms in Homebase to PPP loans and then compare firms that received loans earlier versus later. We instrument for the date of PPP receipt using regional exposure to lenders that disbursed different amounts of PPP funding or predicted 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.

The fact that the program disbursed significant funds, yet had little effect on employment, raises 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 UI 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. Consistent with this notion, we find suggestive evidence

that exposure to higher-performing banks is associated with fewer permanent firm shutdowns, defined as the firm being closed for all weeks from the beginning of the program through the end of August. This result suggests that initial bank-driven distortions may have had persistent effects on the ability of firms to reopen after the initial shock.

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.

Our work complements several contemporaneous studies that also focus on the employment effects of the PPP, although with less emphasis on the role of financial institutions. Three studies (Autor et al., 2020; Chetty et al., 2020; Hubbard and Strain, 2020) 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. Approximately 0.4% of PPP loans were disbursed to firms with more than 250 employees, which account for only 13% of covered employment among all borrowers. Nevertheless, despite using different primary data sources and a different research design, these papers tend to find either modest or negligible effects on employment, consistent with our findings.<sup>3</sup>

Several other studies use differences in the timing of PPP receipt to examine the program's employment effects, while also exploiting differences in timing due to pre-existing variation in bank lending relationships. Li and Strahan (2020) use variation in the strength of the relationships between local banks and firms and, similar to us, find modest employment effects of the program. Faulkender et al. (2020) leverage the faster pace that community banks approved and disbursed funds relative to their counterparts and find large employment effects of the program. Bartik et al. (2020b) find significantly lower self-reported survival probabilities for firms whose primary lender was a top-four bank. Doniger and Kay (2021) find that areas with a greater fraction of businesses receiving PPP funds right before the end of the first round rather than at the start of the second round had higher employment rates, with magnitudes that align with our uninstrumented matched sample analysis. While the conceptual approach in these papers is similar to ours, a key source of difference is the extent to which the research design accounts for nonrandom program targeting. Given the role lenders played in allocating funds to areas that were initially less affected by the pandemic, accounting for targeting differences across areas is crucial for identifying the employment effects of the PPP. Our paper also contributes

<sup>3</sup> Hubbard and Strain (2020) present some specifications that yield larger estimates, but the overall takeaway from their analysis appears in line with these other threshold designs.

by identifying and exploiting ex ante bank characteristics that affected banks' ability to deploy funds quickly.<sup>4</sup>

More broadly, this paper joins a literature focusing on how government interventions following crises impact recovery and the broader economy (Agarwal et al., 2017; Mian and Sufi, 2012; Zwick and Mahon, 2017). Specifically, we offer a comprehensive evaluation of the role that banks played in allocating PPP funds, and the impact that this force had on program targeting and economic outcomes. We contribute to an understanding of government responses to crises, including subsidized lending, tax incentives, and loan guarantees, a widely-used form of government intervention in credit markets (Smith, 1983; Gale, 1990; 1991; Lucas, 2016; Kelly et al., 2016; Atkeson et al., 2019). A burgeoning empirical literature examines the transmission and effects of loan guarantees or tax-based stimulus on credit supply, employment, and small business outcomes (House and Shapiro, 2008; Lelarge et al., 2010; Bachas et al., 2020; Barrot et al., 2019; Mullins and Toro, 2017; Gonzalez-Urbe and Wang, 2019; Zwick, 2021). Our paper contributes directly to this literature by showing how policy transmission depends on the agents delegated to deploy it (e.g., banks). These results are consistent with those of studies that emphasize the importance of proximity (Granja et al., 2018), as well as emerging evidence from the pandemic that firms with pre-existing borrowing and lending relationships received access to PPP funds faster than their counterparts (Balyuk et al., 2020; Amiram and Rabetti, 2020; Li and Strahan, 2020).

The article 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 local labor market and economic outcomes using our bank exposure and timing research designs. Section 6 presents aggregate impact estimates. Section 7 explores mechanisms behind our results. Section 8 concludes.

## 2. The Paycheck Protection Program (PPP)

The Paycheck Protection Program (PPP) began on April 3rd, 2020 as part of the 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 applied for support through banks and the Small Business Administration (SBA) was responsible for overseeing the program and processing loan guarantees and forgiveness. A motivation for 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.<sup>5</sup>

The lending program was generally targeted toward small businesses of 500 or fewer employees.<sup>6</sup> Although the initial round of funding was exhausted on April 16th, a second round of \$320 billion in PPP funding was passed by Congress as part of the fourth COVID-19 aid bill. 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 30th, but this was eventually extended to August 8th. Our analysis of the program runs through the end of August.<sup>7</sup>

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.<sup>8</sup> The interest rate on all loans is 1% and their maturity is two years. Under SBA's interpretation of 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% 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.<sup>9</sup> 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.

Congress expanded PPP on June 3rd, allowing more flexible terms for loan forgiveness. The updates to the PPP

<sup>5</sup> Many of these relationships are limited to having transaction accounts. Using data from a large survey on Facebook, Alekseev et al. (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.

<sup>6</sup> 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 <https://www.sba.com/funding-a-business/government-small-business-loans/ppp/faq/small-business-concerns-eligibility/> for further information about the program.

<sup>7</sup> The Consolidated Appropriations Act of 2021 included \$284 billion in additional forgivable loans for a second draw of PPP loans for small businesses. This program began in January 2021 and its eligibility criteria were targeted to small businesses that experienced reductions in revenue. Our analyses do not encompass the effects of this second draw of PPP.

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

<sup>9</sup> Loan payments on the remainder of the loan can be deferred for six months and interest accrues at 1%.

<sup>4</sup> Other studies take a more theoretical approach (Elenev et al., 2020; Joaquim and Netto, 2020; Barrios et al., 2020), or study specific aspects of the PPP, such as the role of Fintechs, bank lending relationships, and firm size in the allocation of funds under the program, the impact of the program in supporting liquidity for firms of different size, and how the distribution of funds varied with businesses' ability to work remotely (Erel and Liebersohn, 2020; Cororaton and Rosen, 2020; Papanikolaou and Schmidt, 2020; Morse and Bartlett, 2020; Chodorow-Reich et al., 2020).

expanded the duration from eight weeks to twenty-four and extended the deadline to rehire workers until the end of the year. This change 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%.

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 poses 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.<sup>10</sup>

During the first weeks of April, demand for PPP loans outstripped supply, which was limited by statute. Between April 3rd and 16th 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 21st and 23rd, respectively, which was signed into law on the 24th. The PPP began accepting applications on April 27th for the second round of funding. While 60% of the second round funds were allocated within two weeks of initial disbursement, the remaining second round funds were disbursed slowly, 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 8th, culminating in \$525 billion in total disbursements.

### 3. Data

Our primary source for data on the PPP comes from microdata made available through the Small Business Administration (SBA) and the Department of Treasury. We are able to observe all loans approved under the program. For all loans, the data include borrower and lender name, the borrower’s self-reported industry, location, corporate form, and workers covered by the loan. 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 a matched sample of loans that we were able to match to the Homebase dataset.

We merge this data set with the Reports of Condition and Income (Call Reports) filed by all active commercial banks as of the first quarter of 2020. 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 match to the Call Reports account for 90.5% of all loans disbursed under the PPP.

We obtain information about the financial characteristics of each bank from the Call Reports. This data set includes information about the size, capital structure, and portfolio composition of all banks operating in the US. 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. We also use Call Report data to compute measures of average capitalization and liquidity of banks serving a region and to compute some ex ante characteristics that limited banks’ ability to quickly deploy funds under 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 US 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 et al., 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. We define performance using each bank’s national share of PPP lending relative to its national share of the small business lending market. We use County Business Patterns data 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 software to manage their scheduling and time clock.<sup>11</sup> Homebase processes exact hours worked by the employees of a large number of

<sup>10</sup> The traditional SBA program responding to disasters is the Economic Injury Disaster Loan (EIDL) program. Recipients of an EIDL loan can receive a \$10,000 loan advance that does not need to be paid back. EIDL loan advance amounts are deducted from PPP forgiveness. 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 Appendix E.

<sup>11</sup> See <https://joinhomebase.com/> for more information.

businesses in the US. We use information obtained from Homebase to track employment indicators at a weekly frequency at the establishment level. The Homebase dataset 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 obtain small business revenue 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 level, defined by the location where a transaction occurred. We complement these data sources with additional county-level employment data from Opportunity Insights, which are described in detail in Chetty et al. (2020).<sup>12</sup> The employment data come 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. In Appendix A, we provide a more detailed discussion of each data source and final dataset construction. Finally, we obtain data from the Bureau of Economic Analysis (BEA) on the median household income at the county level between 2017 and 2019 to control for differences in economic activity at the local level that could be related to the evolution of our outcomes of interest during the pandemic.

#### 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 US, as measured by total assets. Columns (2) and (3) report the share of total PPP volume in the first round and overall, respectively, while columns (7) and (8) report the share of the number of PPP loans of each bank in the first round and overall. Columns (4) and (9) show the share of the small business loan (SBL) market as of the fourth quarter of 2019 in terms of total volume and number of loans, respectively.

In columns (5)–(6) and (10)–(11), we compute a measure of relative bank performance in round one and for the

whole program, which is measured as

$$PPPE_b = \frac{\text{Share PPP} - \text{Share SBL}}{\text{Share PPP} + \text{Share SBL}} \times 0.5$$

where *Share PPP* is the share of PPP for bank *b*, and *Share SBL* is the bank's small business loan share. In our main analysis, we use the PPPE measure of relative bank performance that is based on the share of the number of PPP and SBL loans of each bank.<sup>13</sup> We prefer the number-based measure of relative bank performance because larger businesses had prompter access to PPP loans (Balyuk et al., 2020) and the volume-based measure of bank performance puts greater weight on large loans and less weight on smaller loans to businesses whose access to the program was more likely constrained by lack of local access to commercial banks that were quick to deploy loans.

Fig. 1 shows the cumulative share of PPP (blue triangles) and small business loans (red circles) by all banks at the end of the first (Panel A) and second funding rounds (Panel B), with banks ordered by number-based PPPE.<sup>14</sup> 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.

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. If there were no heterogeneity in PPP performance, the PPP and SBL shares would follow similar patterns. This is not the case, and the S-shaped pattern for PPP indicates that many banks disbursed relatively few PPP loans, while roughly a third of banks disbursed half of 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 half 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.

Fig. 1, 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.

<sup>13</sup> Small business loans include outstanding balances on credit cards issued to small businesses, but it is not possible to ascertain what fraction of these loans are credit card accounts. To the extent that these balances could represent the most important lending relationship of many small businesses, including these balances could be useful to capture the share of firms that consider each bank as a banking relationship. Nevertheless, our findings that large banks underperform their respective share of small business loans are not affected when we consider only small business loans with a principal amount between \$100,000 and \$1 million, which are less likely to include outstanding credit card balances.

<sup>14</sup> 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.

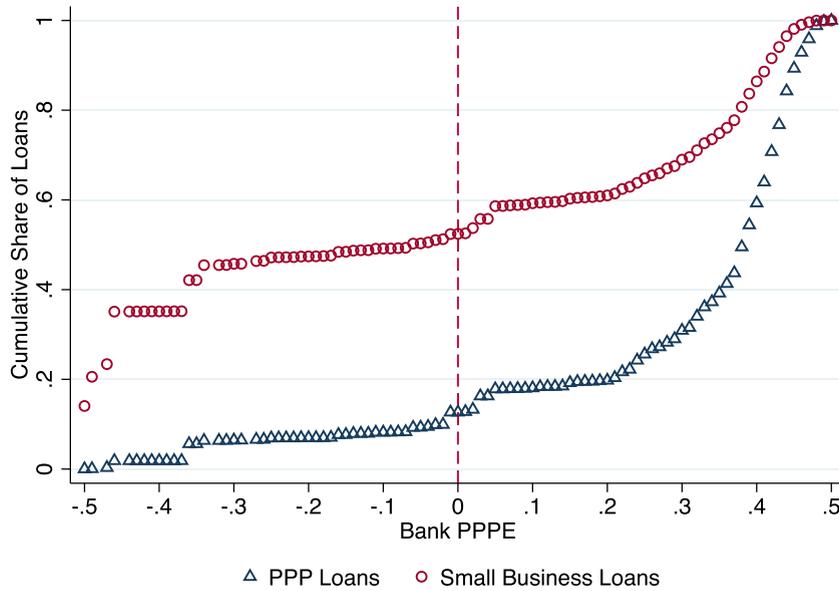
<sup>12</sup> We also refer readers to Chetty et al. (2020) who provide comparisons between Homebase and alternative high-frequency measures of aggregate employment.

**Table 1**

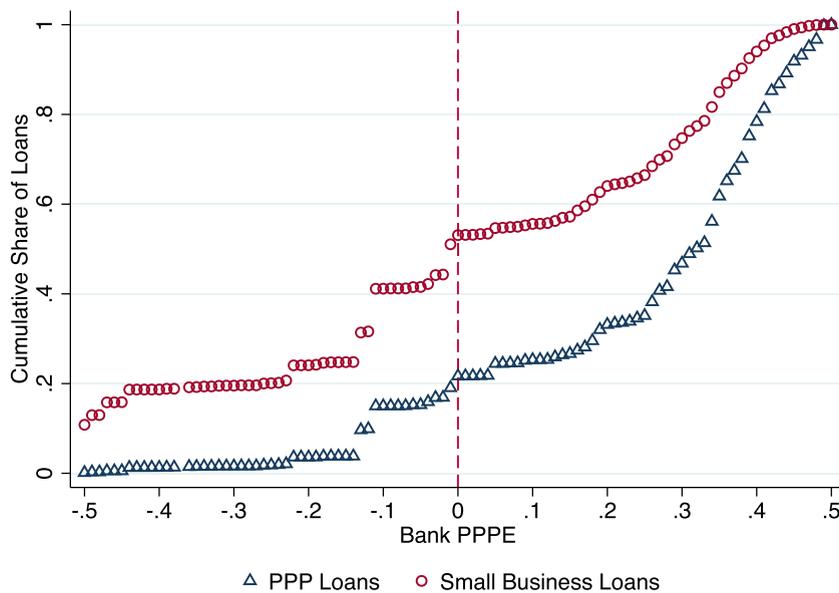
PPP Performance and PPPE for the Largest 20 Banks. Table 1 reports individual bank statistics and the PPPE index for the 20 largest financial institutions in the United States. Total Assets is computed using information from fourth quarter 2019 Call Reports. Share of PPP Volume is the total amount disbursed 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 PPPE 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 either the first round or in 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 PPPE 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%	4.49%	-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

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



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



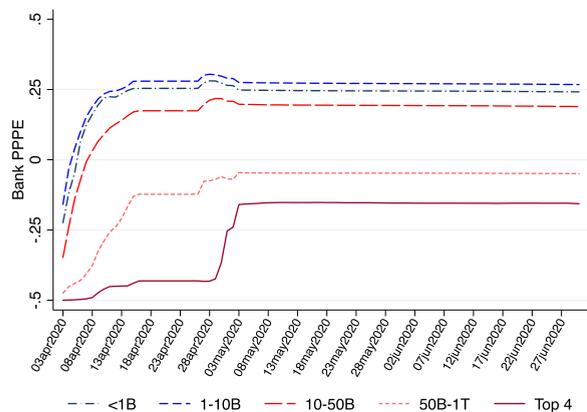
**Fig. 1.** PPPE and PPP Allocation. Fig. 1 plots the cumulative share of PPP and small business lending by all banks whose PPPE is below  $x$ , where  $x \in (-0.5, 0.5)$ . Panel A plots cumulative amounts using PPP data as of the end of the first round (April 16th, 2020), and Panel B reports cumulative 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.

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. Overall, the evidence is consistent with substantial heterogeneity across lenders in their responses to the program’s rollout.

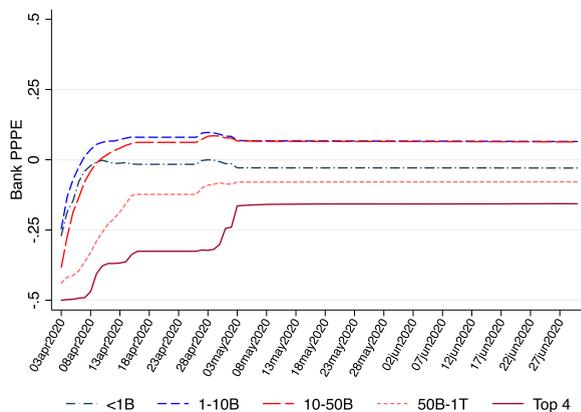
#### 4.2. Bank performance over time

Fig. 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), av-

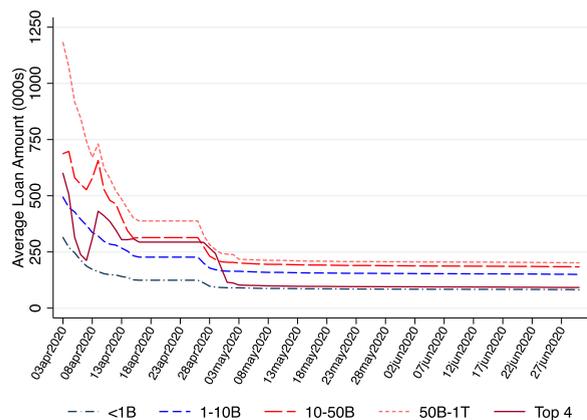
Panel A. Number-based PPPE by Size Bin



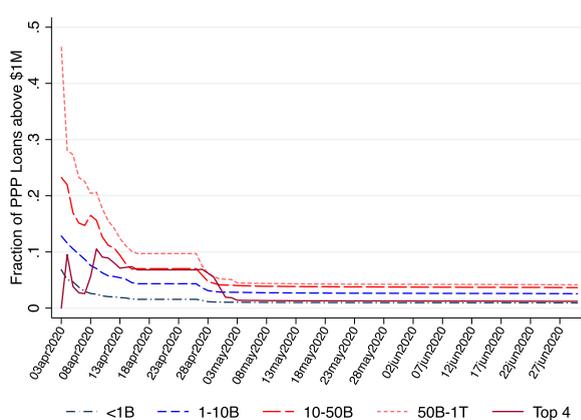
Panel B. Volume-based PPPE by Size Bin



Panel C. Average Loan Amount by Size Bin



Panel D. Share of Loans > \$1M by Size Bin



**Fig. 2.** Evolution of PPPE and Average Loan Size by Bank Size. Fig. 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 (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.

verage 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. In contrast, 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.<sup>15</sup>

<sup>15</sup> The differences were noted in the popular press. For example, see the April 6th 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 the July 31st 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>).

In spite of this partial convergence, large banks still underperformed overall, consistent with press accounts suggesting that clients were frustrated by large banks’ inability to process PPP loans and switched to smaller banks and non-banks. As demand for PPP funds waned during May, the evolution of bank PPPE across size categories stabilized.

Fig. 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 et al., 2020), or with banks prioritizing certain customers, such as existing loan customers who

tend to be larger (Balyuk et al., 2020).<sup>16</sup> 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 to respond to the sudden influx of PPP applications. In the second round, most underperforming banks were able to improve their performance and ultimately process many PPP applications. 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. In Appendix B, we plot the Kaplan-Meier curve of the fraction of small businesses receiving PPP approval. 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. Bank attributes and predicted PPPE

A potential concern with our PPPE measure of relative bank performance is that it might reflect differences in local demand for the program rather than differences in the ability or willingness to process applications. The broad eligibility criteria and generous terms of the program likely meant that demand for the program was high across most locations and industries. Nevertheless, we address this specific concern by attempting to isolate variation in relative bank performance that is explained by differences in banks' ability to process applications under the program. Specifically, we focus on three factors that capture differences in pre-existing conditions and capacity constraints at the bank-level, which led some banks to respond more quickly to the program's rollout.

The first factor is motivated by the fact that banks had to employ an unprecedented amount of labor hours in a short amount of time to process the unexpected and sudden influx of PPP loan applications. Bank staff had to interact with clients to collect and review their loan documentation and then submit the information in those applications through the SBA portal.<sup>17</sup> Moreover, Bank Secrecy Act and Anti-Money Laundering regulations meant that the staff had to perform customer due diligence for new clients. Thus, banks with greater labor capacity had a relative advantage in processing PPP loans more quickly.

<sup>16</sup> See for example, "Biggest banks 'prioritized' larger clients for small business loans, lawsuits claim," (<http://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.

<sup>17</sup> Sparks (2020) provides an account of the critical role of staffing limitations in the deployment of the first round of PPP.

We use Call Report data to measure how much a bank spends in wages relative to data processing expenses. This measure serves as a proxy for bank reliance on a lending model that depends relatively more on labor from loan officers and less on information technology.

Another critical factor in determining banks' ability to quickly deploy PPP loans during the first round of PPP was whether they had a pre-existing SBA lending relationship. Lenders needed valid SBA portal credentials (E-Tran accounts) and access to the SBA's Capital Access Financial System (CAFS) to submit PPP applications for their clients. Fintechs and other commercial banks with no previous SBA lending experience had to wait until almost the end of the first round of PPP to gain access to the SBA portal.<sup>18</sup> To measure the role of prior relationships with the SBA in explaining relative bank performance during the first round, we create an indicator variable that captures whether the bank had any prior experience working with the SBA in the three years prior to the program. To capture the intensity of the SBA relationship, we compute the fraction of the number of SBA-guaranteed loans that the bank originated relative to the average number of small business loans in the bank's balance sheet over the previous three years.

Finally, many banks were operating under active formal supervisory enforcement actions related to deficiencies in their commercial lending operations and in their compliance with the Bank Secrecy Act and Anti-Money Laundering requirements. Lenders subject to formal enforcement actions related to unsafe or unsound practices were not automatically approved to make PPP loans according to the April 2nd, Interim Final Rule of the SBA, which provided information for lenders interested in participating in the program. Accordingly, banks under a formal enforcement action could not submit PPP loan applications for their clients without first getting approval from the SBA, which likely delayed their ability to quickly submit those applications.<sup>19</sup>

The most important case of a bank whose ability to lend under the PPP was restricted by a formal enforcement action is that of Wells Fargo. Wells Fargo had been operating under an asset growth restriction imposed by its primary regulator since the aftermath of the 2016 fake accounts scandal. Because of this restriction, Wells Fargo could not make PPP loans because they would risk breach-

<sup>18</sup> For instance, according to Sparks (2020), Carter Bank & Trust of Martinsville, Virginia was forced to wait for its SBA portal credentials and only got access to the SBA portal for PPP applications 48 hours before round one funds were exhausted. Another account of these difficulties can be found in Wooten (2020). Furthermore, even banks that had previously worked with the SBA had difficulties submitting applications either because they needed additional authorizations or because they "forgot their credentials or their login expired."

<sup>19</sup> For instance, PeopleFirst Bank from Joliet, Illinois was issued a formal supervisory actions in 2019 due to weaknesses in its Bank Secrecy Act and Anti-Money Laundering controls. Another example is Home Bank of Arkansas from Portland, Arkansas which was under an active formal supervisory written agreement for deficiencies in management and in their internal audit control programs. Both banks did not disburse any PPP loan in the first round of PPP but processed a number of loans more commensurate with their small business lending share in the second round of the program suggesting that the enforcement actions limited these banks' ability to respond quickly to the program.

**Table 2**

Bank PPPE and Capacity Constraints. Table 2 examines the relation between bank performance in deploying PPP during the first round of the program and bank characteristics. The dependent variable, *Bank PPPE*, is the number-based bank PPPE index.  $\frac{Wages}{(DataExpenses+Wages)}$  is a measure of labor intensity at the bank that we compute as the ratio between bank wages (RIAD4135) and the sum of wages and data processing expenses (RIADC017).  $I(SBA Lender=1)$  is an indicator variable that takes the value of one if the bank originated at least one SBA 7(a) guaranteed loan between 2017 and 2019. *SBA Loans/SBL Loans* is the ratio between the number of SBA government-guaranteed loans that a bank originated between 2017 and 2019 and the number of all small business loans (SBA and non-SBA) that the bank held on its balance sheet at the end of 2019. *Active Enforcement Action* is an indicator variable that takes the value of one if the bank had an active enforcement action when the PPP was launched.  $I(Wells Fargo=1)$  is an indicator variable that takes the value of one for Wells Fargo Bank. Columns (5) – (8) include controls for size deciles to ensure that the results are not driven by differences in size. Standard errors are presented in parentheses, and are clustered at the state level. \*\*\*, \*\*, and \*, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LHS is Bank PPPE							
$\frac{Wages}{(DataExpenses+Wages)}$	0.330*** (0.072)			0.251*** (0.060)	0.295*** (0.060)			0.283*** (0.055)
$I(SBA Lender=1)$		0.172*** (0.012)		0.170*** (0.011)		0.149*** (0.016)		0.150*** (0.016)
<i>SBA Loans/SBL Loans</i>		0.158*** (0.036)		0.173*** (0.036)		0.162*** (0.038)		0.176*** (0.037)
<i>Active Enforcement Action</i>			-0.283*** (0.054)	-0.280*** (0.052)			-0.254*** (0.051)	-0.256*** (0.050)
$I(Wells Fargo=1)$			-0.418*** (0.053)	-0.554*** (0.053)			-0.470*** (0.056)	-0.522*** (0.057)
Observations	5204	5212	5212	5204	5204	5212	5212	5204
Adjusted R <sup>2</sup>	0.005	0.076	0.009	0.088	0.058	0.103	0.062	0.114
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Deciles	No	No	No	No	Yes	Yes	Yes	Yes

ing the asset cap. It was not until April 8th, 2020 that the Federal Reserve issued a press release modifying the growth restriction such that the bank could disburse PPP loans. This delay meant that Wells Fargo could not process PPP loans until the asset cap restriction was modified. As a result, its share of PPP lending in the first round was just a small fraction of its share of small business lending.<sup>20</sup>

We examine how pre-PPP variation in these characteristics across banks affects their relative performance in deploying the PPP during the first round. We estimate cross sectional regressions of the form:

$$PPPE_j = \alpha_j + \zeta B_j + \varepsilon_j$$

where  $PPPE_j$  is PPPE for bank  $j$  at the end of the first round of the program,  $\alpha_j$  are size deciles, and  $B_j$  are measures of the bank attributes: wages over wages plus data expenses, pre-existing SBA lending relationships, and enforcement actions.

The first three columns of Table 2 represent the three factors discussed above. Column (1) shows that a measure of labor capacity at the bank correlates positively with bank PPPE, consistent with our hypothesis that greater capacity to hand-process loan applications allowed banks to disburse PPP loans at a faster rate. Column (2) shows that the existence and strength of a prior relationship with the SBA are both positively associated with bank performance in rolling-out PPP funds.<sup>21</sup> Column (3) shows that banks with active enforcement actions as well as Wells

Fargo performed significantly worse, on average, during the first round. Column (4) shows that the explanatory power of each of these variables is not subsumed when we include them in a multivariate specification. In columns (5)–(8), we further show that these estimated coefficients are very similar when we include controls for bank size. Thus, these factors are not merely capturing differences in performance across banks of different sizes. We compute the predicted values of the empirical specification in column (8) of Table 2 as a measure of relative bank performance that is explained by these predetermined supply-side frictions and likely to be orthogonal to differences in local demand for PPP funds.

#### 4.4. 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 fewer PPP funds, we construct regional measures of PPPE by distributing bank-level

<sup>20</sup> We highlight the case of Wells Fargo due to the importance of Wells Fargo in the economy and to the fact that we can point to an external reason that was the subject of public discussion and directly explains the underperformance of Wells Fargo during the first round of the program. In Appendix C, we repeat our main results using the exposure of each local area to Wells Fargo branches as our main exposure measure.

<sup>21</sup> In Appendix B, we provide a plot of the relation between bank PPPE and the labor intensity of the bank as well as the existence and strength of the pre-existing SBA relationship.

**Table 3**

Correlates of PPPE Exposure. Table 3 presents bivariate regressions of PPPE and Predicted PPPE on ZIP-level observables. Both PPPE, Predicted 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  $\beta$ -standard deviation change in PPPE exposure, where  $\beta$  is the reported coefficient. \*\*\*, \*\*, and \*, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	LHS is Resid PPPE as of R1			LHS is Pred Resid PPPE as of R1		
	Coefficient	R <sup>2</sup>	N	Coefficient	R <sup>2</sup>	N
<b>Exposure Correlates:</b>						
Share of Top 4 Banks	-0.703*** (0.006)	0.3619	35882	-0.603*** (0.013)	0.2584	35882
Number of Branches per Capita	-0.020*** (0.002)	0.0006	29545	-0.010*** (0.001)	0.0002	29545
Share of Small Banks Deposits	0.400*** (0.006)	0.1592	35830	0.193*** (0.006)	0.0364	35830
<b>Other Correlates:</b>						
Log(Population)	-0.203*** (0.005)	0.0476	29545	-0.065*** (0.006)	0.0046	29545
Log(Population Density)	-0.336*** (0.006)	0.1044	29545	-0.116*** (0.006)	0.0118	29545
Social Distancing	0.225*** (0.007)	0.0412	35549	0.099*** (0.008)	0.0078	35549
Covid Cases per Capita	-0.262*** (0.007)	0.0797	35870	-0.120*** (0.003)	0.0161	35870
Deaths per Capita	-0.160*** (0.006)	0.0344	35870	-0.059*** (0.004)	0.0045	35870
Unemployment Filing Ratios	0.012 (0.008)	0.0001	24576	0.056*** (0.008)	0.0019	24576
Employment Opportunity Insights	-0.075*** (0.007)	0.0071	19525	-0.012* (0.007)	0.0002	19525
Revenue Change of Small Business	0.159*** (0.006)	0.0333	29715	0.075*** (0.006)	0.0073	29715

PPPE and predicted PPPE based on the share of the number of branches of each bank in a region.<sup>22</sup> We first consider the spatial distribution of PPPE during the first round of funding. Exposure varies across the United States with western areas exhibiting much lower levels of PPPE and more rural areas in the Midwest and Northeast showing higher PPPE.<sup>23</sup>

To further understand the conditional distribution of PPPE and predicted PPPE, Table 3 reports the results of bivariate regressions of ZIP-level PPPE and ZIP-level Predicted PPPE on ZIP-level observables. The variables are normalized so that coefficients can be interpreted as the effect of a one-standard-deviation change. The results are quite similar using both PPPE and predicted PPPE. The table confirms our earlier descriptive evidence—the top-4 banks disbursed significantly fewer PPP loans relative to their overall market share, while regions served by smaller banks performed better and were served by banks with

fewer constraints in deploying PPPE. Perhaps surprisingly, ZIP codes with a greater branch density have slightly lower PPPE.

The table suggests that early PPP disbursement may have been targeted towards areas less affected by the pandemic. More populous areas, areas with higher population density, and areas with higher COVID-19 cases and deaths see lower PPPE. Greater social distancing—measured by a greater decline in the social distancing index—also see lower PPPE. There is no statistically significant relationship between unemployment and PPPE and areas that saw a smaller decline in revenue for small businesses also have higher PPPE. In contrast, in the OI data, areas with greater employment declines have higher PPPE, which suggests better targeting by this measure. However, the magnitude of this relation is small. The coefficients from regressions using predicted PPPE have similar signs but smaller magnitudes than those using PPPE. This pattern is consistent with the idea that predicted PPPE captures supply-side frictions that are less correlated with local economic, demographic, and health factors.

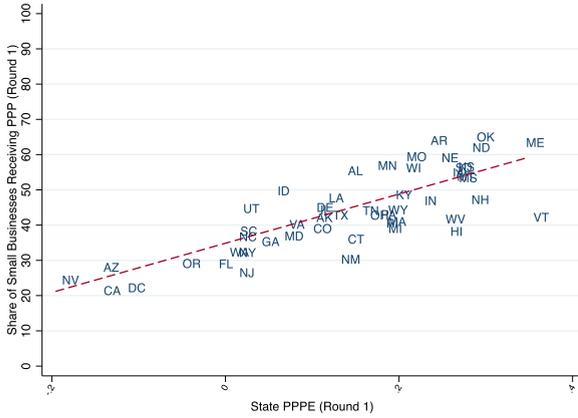
Fig. 3 explores the relation between PPPE and PPP lending at the state-level using data from the Census Small Business Pulse Survey at the end of the first round of the PPP.<sup>24</sup> We plot the relationship between the percent of firms receiving funds and state exposure to bank performance. Panel A plots the fraction of all small businesses

<sup>22</sup> By using the share of number of branches rather than the share of deposits of each bank in a region, we implicitly downweight branches with significant amounts of brokered and internet-deposit balances that do not necessarily represent a commensurate share of the local small business relationships. If data were available, each bank's respective pre-pandemic share of small business loans in each local area would be the ideal weighting scheme. However, the best-available data, the Community Reinvestment Act (CRA) small business lending dataset, only includes county-level data and only provides data for large banks whose total assets exceed \$1 billion.

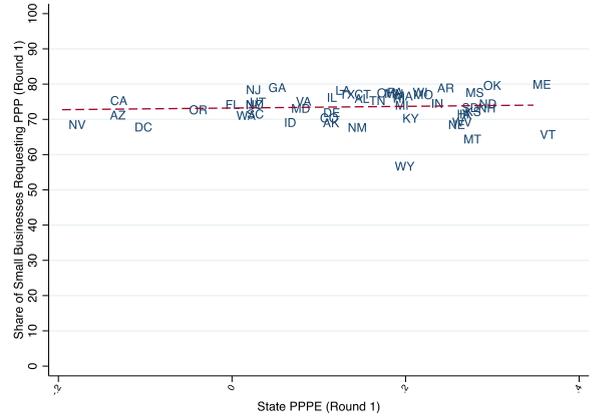
<sup>23</sup> Appendix B provides further information on the geography of targeting, a national map of county-level PPPE and the first round distribution of PPP funds, and a map of ZIP-level PPPE for the Chicago and New York metro areas.

<sup>24</sup> Most of our analyzes are at the ZIP-level but the Census survey is only available at the state-level.

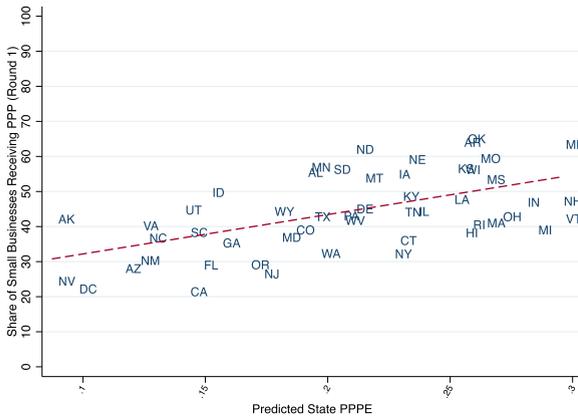
Panel A. PPPE and Share Receiving PPP



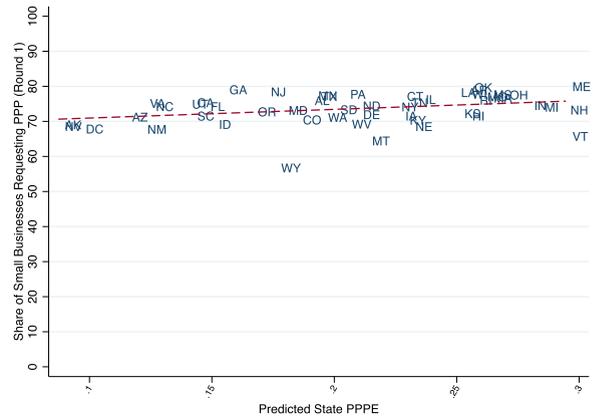
Panel B. PPPE and Share Requesting PPP



Panel C. Predicted PPPE and Share Receiving PPP



Panel D. Predicted PPPE and Share Requesting PPP



**Fig. 3.** State Exposure to PPPE and Share of Small Businesses Requesting and Receiving PPP. Fig. 3 presents scatter plots comparing state-level exposure to PPPE and Predicted PPPE and Census survey outcomes from after the first round of funding. Panels A and C plots the percentage of firms receiving PPP at the end of the first round, and Panels B and D plots the percentage of small businesses reporting having applied to PPPE funds at the end of the first round of the PPP program. Data come from the Census Bureau Small Business Pulse Survey, SBA, Call Reports and Summary of Deposits.

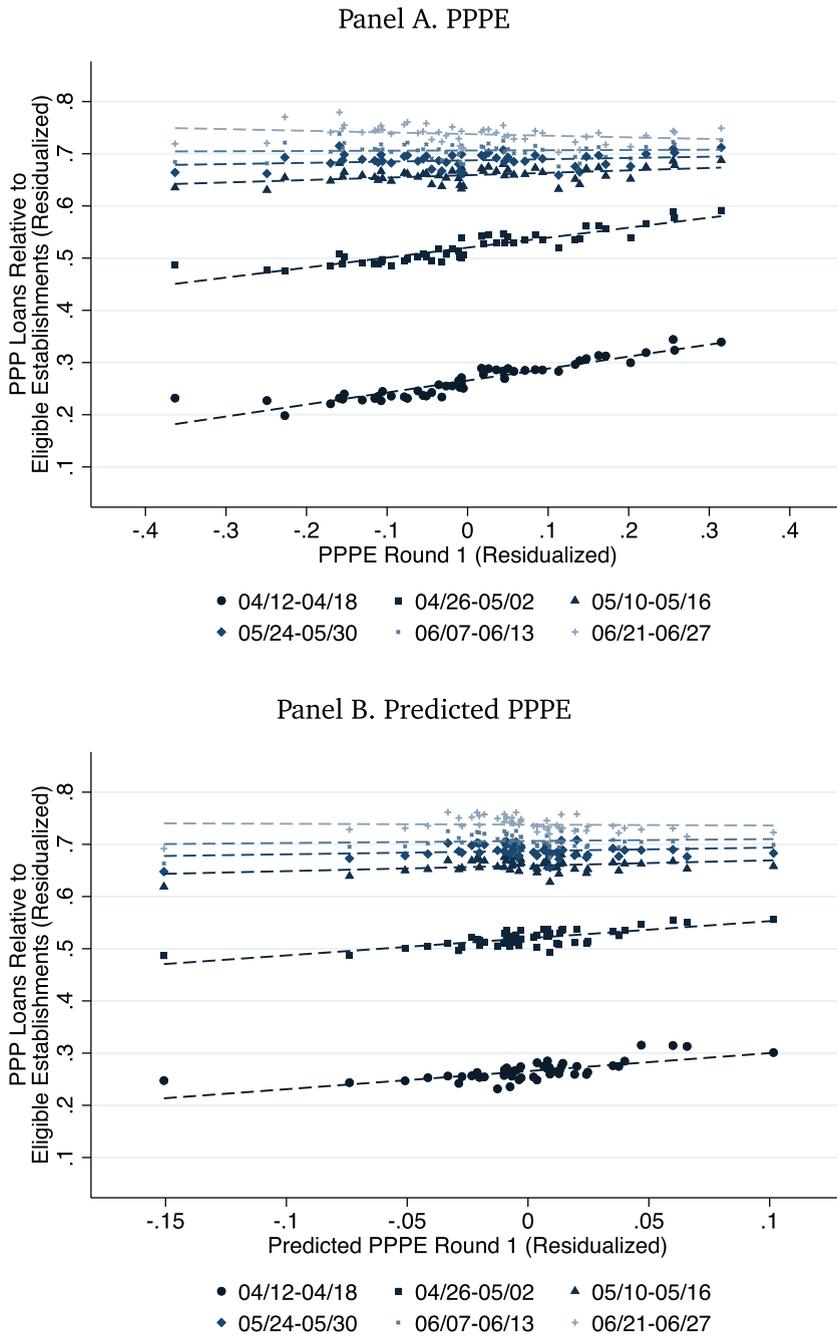
reporting receiving PPP loans in each state during the first round of lending. There is a strong positive relationship between PPP lending and PPPE at the state level. States with the highest PPPE saw nearly 50% of small businesses receiving PPP funding in round one; states with the lowest PPPE saw just 20% of small businesses receiving funding.

A potential concern with these results is that the causality runs in reverse. That is, banks do relatively better in deploying PPP in areas where demand for PPP loans is strong. 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. Fig. 3, Panel B compares PPPE to the percentage of businesses in each state that report having applied for PPP funds as of the end of round one in each state. Between 65% and 80% of small businesses in each state report having applied for PPP funds at the end of the first round. Importantly, the likelihood of PPP application is unrelated with state PPPE. In other words, demand for PPP

funds at the state level does not seem to correlate with our state-level PPPE measure of relative bank performance.

The bottom panels of Fig. 3 repeat the analysis using predicted PPPE measure at the state level. We see very similar patterns as with our regular PPPE measure. Fig. 3, Panel C shows that there is a strong positive relationship between state exposure to banks with supply-side constraints and the percentage of small businesses receiving PPP at the end of the first round. Panel D shows that there is little to no relationship between our predicted PPPE and PPP applications. This fact supports the idea that our predicted PPPE measure captures supply-side frictions that affected banks' ability to process PPP loans and not differences in exposure to local demand.

Fig. 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 local exposure to bank performance at the ZIP level by taking the weighted average of bank PPPE or predicted PPPE for



**Fig. 4.** ZIP Exposure to First Round PPPE and PPP Coverage over Time. Fig. 4 plots binned scatter plots of the average fraction of small business establishments that received a PPP loan versus ZIP-level PPPE (Panel A) and Zip-level Predicted PPPE (Panel B). 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.

all branches that are either in the ZIP or within ten miles of the center of the respective ZIP code. We then partition ZIPs 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. Panel A shows the relationship between zip-level PPPE and the fraction of businesses receiving PPP, while Panel B shows the same relationship replacing PPPE with predicted PPPE. Both panels show similar results. A strong positive relation between ZIP PPPE and ZIP predicted PPPE

and the fraction of businesses receiving PPP during the first round further supports the idea that the initial allocation of funds was shaped by exposure to the performance of local banks.<sup>25</sup>

The strong positive relation between ZIP PPPE or predicted PPPE and the fraction of businesses receiving PPP during the first round of the program persists over the following weeks but becomes gradually weaker later in May and into June. This pattern offers further evidence that the relation between PPPE and the fraction of businesses receiving PPP in the first round 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. The pattern suggests 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 4. There, we assess the association between ZIP PPPE or predicted PPPE and the fraction of businesses receiving PPP in each ZIP-by-industry group after conditioning on state-by-industry fixed effects. In each panel, the top row shows the relationship between PPPE and the fraction of businesses receiving PPP, while the bottom row shows the same relationship replacing PPPE with predicted PPPE. Again, both panels show broadly similar results. Thus, we evaluate whether businesses within the same state and industry had different access to PPP loans because they were located in ZIP codes whose nearest banks performed relatively well compared to businesses in the same state and industry but in ZIP codes whose banks underperformed.

Column (1) of Table 4, Panel A, further supports the idea that local exposure to banks that overperformed in the PPP had a positive impact on the ability of businesses to obtain PPP funds during the first round. Even within a given state and industry, being in the same ZIP or within 10 miles of banks that overperformed in the first round was associated with a significantly higher share of businesses receiving PPP during the first round. We find similar conclusions when we measure local ZIP exposure to banks that were constrained processing PPP applications using our predicted PPPE measure.<sup>26</sup> In column (1) of Panel B, we assess whether this impact persisted

through both rounds of the program. Consistent with the findings above, local ZIP exposure to banks that over- or underperformed in the first round is no longer positively associated with the fraction of businesses receiving PPP after both rounds of the program. If anything, there is a modest negative relationship between round one PPPE and total PPP loans per establishment. This relationship is significant using PPPE, and insignificant at conventional levels using predicted PPPE. This result further suggests that as supply-side frictions subsided during the second round of the program, the relation between PPPE and the fraction of businesses receiving PPP flattened, which indicates that differences in demand for PPP funds were unlikely to explain the positive relation during the first round.

A potential explanation for the gradual weakening of the relation between local PPPE and the fraction of businesses receiving PPP is that non-local banks and nonbanks 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 a ZIP code centroid), non-local banks (defined as all banks with branches that are farther than 10 miles from the ZIP), credit unions, Fintechs, and all other nonbanks participants. Fig. 5 shows the average fraction of establishments receiving PPP during round one, round two, and the entire program by source of PPP funding. On average, approximately 20% of all establishments in a ZIP were able to obtain funding during the first round, and local banks accounted for most of these loans. Fintech lenders and nonbanks participated very little during the first round. During the second round, local banks still accounted for the majority of disbursed loans, but Fintech lenders and especially non-local banks participated to a much larger extent. This pattern is consistent with Fintech 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 other non-banks institutions accounted for five percent of loans.<sup>27</sup>

Next, we evaluate whether the presence of non-local banks and Fintechs mattered most in areas that were exposed to local banks that underperformed in the PPP. Unsurprisingly, in column (2) of Table 4, Panel A, we show that local PPPE is associated with a greater fraction of establishments receiving loans from local banks in the first round of the program. Columns (3), (4), and (6) show that ZIP PPPE is unrelated with the fraction of establishments receiving loans from non-local banks, credit unions, and

<sup>25</sup> In this figure, we measure PPP loans relative to eligible establishments, which equals 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.

<sup>26</sup> In Appendix B, we show that our results are robust to including county-by-industry fixed effects. Thus, we find a positive relationship between ZIP PPPE and the fraction of businesses receiving PPP even when we compare businesses that are located within the same county and industry and thus are even more likely to be exposed to similar external conditions. Having said that, we use state-by-industry fixed effects in our analysis of employment impacts because the Homebase data does not cover the full set of counties and industries. This limitation substantially reduces the amount of variation available for estimation within these narrow cells, preventing us from drawing strong inference on employment impacts when relying only on this variation.

<sup>27</sup> Lenders that were not classified as depository institutions were classified between community lenders, credit unions, and other businesses manually. We classified the following lenders as Fintech companies: Kabbage, BSD Capital, Lendistry, Flagship, Marketplace, Fund-Ex Solutions, Fundbox, Fountainhead, Intuit, Itria, MBE, Mountain Bizcapital, Readycap and Newtek. Some of these lenders, including Kabbage, associated with banks in the first round because they could not yet operate on a standalone basis due to program rules. Prior to the eligibility of Fintechs, we count these as non-local banks and thus some substitution between local and non-local banks could come from collaboration with Fintechs.

**Table 4**

ZIP PPPE in Round 1 and PPP Reallocation across Funding Sources. Table 4 shows the correlation between PPPE and the fraction of eligible 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 eligible 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 the number 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 and observations are weighted by the number of eligible establishments in each zip-industry pair. Predicted PPPE is the weighted average of predicted bank PPPE during the first round at the ZIP level. The predicted values of bank PPPE are obtained from estimating the empirical specification of column (8) of Table 2. The weights are defined by the share of the number of branches of each bank in the zip code or within 10 miles of the center of the respective ZIP. 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. All regressions include state-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/Est (%)	PPP Loans Relative to All Establishments by Lender Source				
		Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Zip PPPE (Round #1)	5.458*** (0.736)	5.401*** (0.581)	0.162 (0.117)	0.152 (0.109)	-0.204*** (0.021)	-0.007 (0.020)
Observations	250078	250678	251344	251468	251488	251435
Adjusted R <sup>2</sup>	0.408	0.390	0.126	0.156	0.294	0.045
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Predicted PPPE	2.837*** (0.981)	2.961*** (0.866)	-0.085 (0.149)	0.054 (0.093)	-0.073*** (0.024)	0.003 (0.017)
Observations	250078	250678	251344	251468	251488	251435
Adjusted R <sup>2</sup>	0.380	0.357	0.126	0.155	0.290	0.045
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Allocation in Round 1 and 2						
	(1)	(2)	(3)	(4)	(5)	(6)
	PPP/Est (%)	PPP Loans Relative to All Establishments by Lender Source				
		Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Zip PPPE (Round #1)	-1.580*** (0.345)	1.224*** (0.350)	-1.942*** (0.271)	0.310 (0.336)	-1.480*** (0.191)	-0.139* (0.080)
Observations	234128	244928	250263	251342	251377	251387
Adjusted R <sup>2</sup>	0.359	0.324	0.199	0.222	0.231	0.088
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Predicted PPPE	-0.660 (0.453)	0.932* (0.516)	-1.095*** (0.391)	0.031 (0.287)	-0.751*** (0.262)	-0.040 (0.041)
Observations	234128	244928	250263	251342	251377	251387
Adjusted R <sup>2</sup>	0.358	0.323	0.193	0.221	0.213	0.087
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

nonbank lenders in the first round. Column (5) shows a negative relation between local bank performance and the fraction of local establishments served by Fintechs, suggesting that these institutions played a greater role in PPP lending in areas with worse local bank performance. Despite the statistical significance of the effects of column (5), their economic magnitude is relatively small, indicating these substitute lenders were unable to offset the dislocations from underperforming local banks during the first round.

In columns (2) through (6) of Table 4, Panel B, we examine if these non-local sources of funding had an economically larger role in substituting for local banks dur-

ing the second round of the PPP program. In column (2), local PPPE remains an important determinant of the fraction of loans from local banks, though the relationship is somewhat weaker. This weaker relationship possibly results from improved performance of low-PPPE banks during the second round. Consistent with the findings in Erel and Liebersohn (2020), we find in columns (3), (4), (5), and (6) that other financial institutions such as non-local banks and Fintechs substitute for underperforming local banks. Non-local banks are the most important source of substitute funds. Fintechs are less important but still quite elastic to the effect of weak local bank performance. By the end of the program, the total effect of substitute lenders is

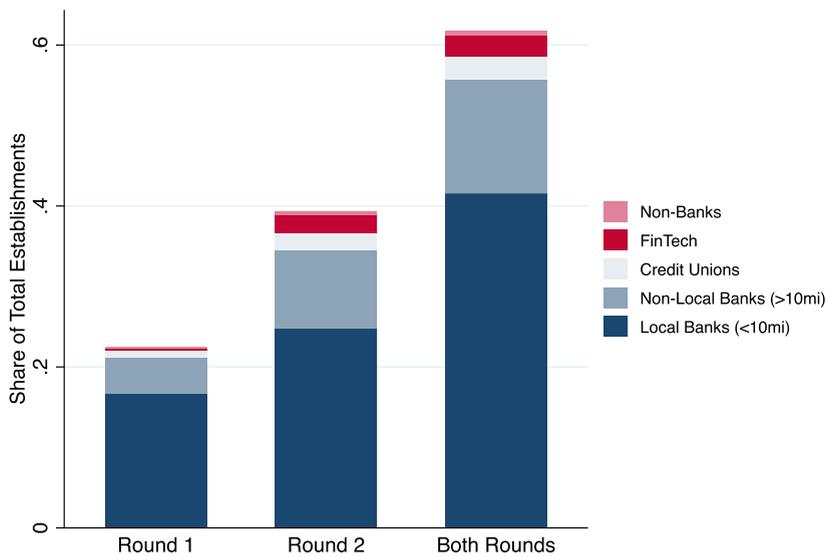


Fig. 5. Share of Establishments Receiving PPP by Lender Type. Figure 5 shows the number of PPP loans broken down by lender type and funding round, scaled by the total number of establishments. Data come from the SBA, FDIC Summary of Deposits, and Census.

large enough to fully offset the weak performance of local banks in low PPPE areas.<sup>28</sup>

#### 4.5. 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.

Fig. 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. We then compare areas with high and low PPP allocations in terms of employment 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.<sup>29</sup> Consistent with the broad definition of eligibility of the program and with a decline of the supply-side

distortions during the second round, we find that the relationship between the share of business shutdowns in the week of March 22nd–March 28th and the share of businesses receiving PPP weakens substantially when we consider the share of businesses receiving PPP during both rounds. In Panel B, 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 and with this relationship becoming weaker or even nonexistent when we consider PPP funding over the two rounds. In Panel C, we repeat the analysis using the decline in the number of employees. The results mirror those in Panels A and B; regions receiving more PPP funding during the first round see a smaller reduction in the number of employees prior to the PPP. There is little relationship in the second round.

In Appendix D, we further confirm our findings using the Homebase data with other levels of aggregation and using other data sources—we find no consistent relationship between PPP allocation and bank exposure with UI claims or small business revenues. We also explore whether funds initially flowed to areas with early pandemic outbreaks. There is a slight negative correlation between PPP receipt and COVID-19 confirmed cases and deaths at the state level. 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 hit by the pandemic and, if anything, areas hit harder by the virus and subsequent economic impacts initially received smaller allocations.

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

<sup>28</sup> In Appendix B, we use a proprietary dataset obtained through a member bank of the Community Development Bankers Association (CDBA) and we find that the PPP loans issued by that member bank to new clients are late relative to those from existing clients and these new clients come predominantly from regions served by banks with low-PPPE performance relative to the PPPE of the regions where the bank and its existing clients were located. These results further indicate that exposure to banks with low PPPE performance forced small businesses to seek PPP funding elsewhere.

<sup>29</sup> Following Bartik et al. (2020a), we define a business shutdown as businesses that report zero hours worked during a week using the data from HomeBase.



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 et al. \(2020\)](#) 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

Did banks' unequal ability and willingness to quickly process PPP applications have any impact in explaining the employment effects of the PPP? Our results on PPP performance differences across banks motivate two complementary research designs for evaluating the PPP. The basic idea is to use differences in local area PPP exposure (PPPE), as well as pre-determined supply-side variation in PPPE (predicted 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 branch shares are not correlated with the various outcomes we study, conditional on observables.<sup>30</sup>

We focus our analysis on the time period between the third week of January and the end of the program in the last week of August 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 through the end of August, we will interpret the research design as assigning some firms funding with a delay, 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 and predicted 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. If one does not break out the data finely enough or condition properly for targeting differences—for example, by treating the last weeks of March as a pre-period benchmark—then one might detect a spurious effect of the program. This issue is very clear when we examine week-by-week outcomes around the policy window.

To account for these targeting differences, we estimate the effects of the program by comparing weeks in the post-PPP period to the two weeks in the post-lockdown, pre-PPP period. 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. Controls include the social distance index, COVID cases per capita and deaths per capita measured as of week 9, all interacted with indicator variables for the months of April, May, June, July, and August. We also include bank controls for the average tier-1 capital and core deposit ratios of all banks within a 10-mile radius of the ZIP code, weighted by the number of banks' branches within a 10-mile radius of the ZIP code. 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 and predicted PPPE instruments attempt to isolate loan supply drivers independent of loan demand.

### 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 several employment outcomes, including business shutdowns (i.e., hours worked

<sup>30</sup> Appendix G evaluates the research design using diagnostic Bartik tests following [Goldsmith-Pinkham et al. \(2020\)](#). The diagnostics provide some intuition about the sources of identification. First, our estimates are not driven by just one or two banks, or even by the top-4 banks alone. Second, influential banks tend to be either large or mid-sized banks and those with PPPE pointing to substantial over- or underperformance. Third, more of the identifying variation comes from banks with positive Rotemberg weights, which enables the Bartik estimator to be interpreted more easily as a LATE. Finally, bank-branch shares are only weakly correlated with local observables, supporting the key identification assumption.

reduced to zero during the entire week), declines in hours worked, and declines in the number of employees.

Fig. 7 presents simple difference-in-difference graphs for each of our Homebase employment outcomes. We divide all firms in the sample based on whether they are located in regions with above- or below-median PPPE or predicted PPPE. We take advantage of the granularity in the Homebase data and conduct our analysis at the ZIP level. 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. Predicted PPPE is determined using the supply-side factors in Table 2: wages over wages plus data expenses, pre-existing SBA lending relationships, and enforcement actions. Panel A shows business shutdowns, Panel B shows hours worked, and Panel C shows the change in the number of employees. Results using both PPPE measures point to very similar patterns.

Prior to the initial lockdown orders, employment outcomes in high- and low-PPPE areas evolve very similarly, even in the absence of controls, suggesting that in normal times these areas were following similar trends. We then see a dramatic decline in each employment outcome starting in the week prior to the lockdowns. Consistent with our targeting results, this decline is modestly larger for regions with low PPPE. The difference in employment declines is somewhat smaller when we split the sample into high- and low-predicted PPPE ZIPs, which indicates that the predicted PPPE measure is less correlated with geographic differences in targeting of the program. 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. The gap for the ratio of hours worked and for the change in the number of employees widens gradually during May and June, which suggests intensive-margin employment effects, while the gap for shutdowns changes little.

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

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

where  $\Delta y_{ijnt}$  is the difference between the Homebase outcomes  $y_{ijnt}$  (business shutdown, hours decline, and employee counts) of a firm  $i$  in each week relative to the average value in the two weeks prior to the PPP launch;  $PPPE_j$  is either PPPE or predicted PPPE in ZIP  $j$ ;  $\alpha_{sn}$  are state-by-industry fixed effects; and  $X_{ijnt}$  are additional control variables. The plots in the top panels use our main PPPE measure as the variable of interest, while the bottom panels estimate the local projections using the predicted PPPE variable as the main variable of interest. Panel A, B, and C plot estimates where the outcome variable is the difference in business shutdowns, the decline in hours worked, and the change in the number of employees, respectively.

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 after conditioning on covariates. 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 Fig. 7. The weekly regressions combined with state-by-industry 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, and there is some evidence that state shutdown orders partly influenced the decline in economic activity (Goolsbee and Syverson, 2020).

For both PPPE measures, the results align with the raw differences across high and low PPPE regions in Fig. 7. When using predicted PPPE, we see weaker evidence of targeting as the gap opens following the launch of the PPP. We see little effect on business shutdowns until the end of the sample period. Beginning in May, there are statistically significant positive effects on hours worked and the number of employees, which remain stable through August.

Table 5 presents our regression estimates, in which we pool the weekly effects into months. We estimate the following specification:

$$E_{isjnt} = \alpha_i + \delta_{snt} + \beta_1 \mathbb{1}[\text{April}] \times PPPE_j + \beta_2 \mathbb{1}[\text{May}] \times PPPE_j + \beta_3 \mathbb{1}[\text{June}] \times PPPE_j + \beta_4 \mathbb{1}[\text{July}] \times PPPE_j + \beta_5 \mathbb{1}[\text{August}] \times PPPE_j + \gamma X_j + \epsilon_{isjnt}, \tag{1}$$

where  $E_{isjnt}$  is an outcome (business shutdowns, the decline in hours worked, or the number of employees) for firm  $i$  in state  $s$ , ZIP  $j$ , and industry  $n$  in week  $t$ . The outcome variable for each establishment is measured in that week relative to the hours worked in that same establishment during the two weeks prior to the PPP launch. The term  $\alpha_i$  captures firm fixed effects,  $\delta_{snt}$  are state-by-industry-by-week fixed effects,  $PPPE_j$  is ZIP PPPE or predicted PPPE, and  $\epsilon_{isjnt}$  is an error term. We also include interactions between the social distance index, COVID cases and deaths per capita measured as of week 9, all interacted with indicator variables for April, May, June, July, and August. These controls capture time-varying effects of the initial severity of the pandemic at the local level. We further include bank controls for the average tier-1 capital and core deposit ratios of all banks within a 10-mile radius of the ZIP code where the firm is located, weighted by the number of banks' branches within a 10-mile radius of the ZIP code.

The coefficients  $\beta_1$ ,  $\beta_2$ ,  $\beta_3$ ,  $\beta_4$ , and  $\beta_5$  capture the differential effect of PPP exposure on the outcome of interest in each month relative to the two weeks prior to the launch of PPP. The coefficient  $\beta_1$  captures the average effect of exposure to better bank PPP performance after the initial rollout of the PPP, when most regions remained under some form of shelter-in-place order. The coefficient  $\beta_2$  captures effects in May, as many regions began to lift restrictions. The coefficients  $\beta_3$ ,  $\beta_4$ , and  $\beta_5$  capture medium-

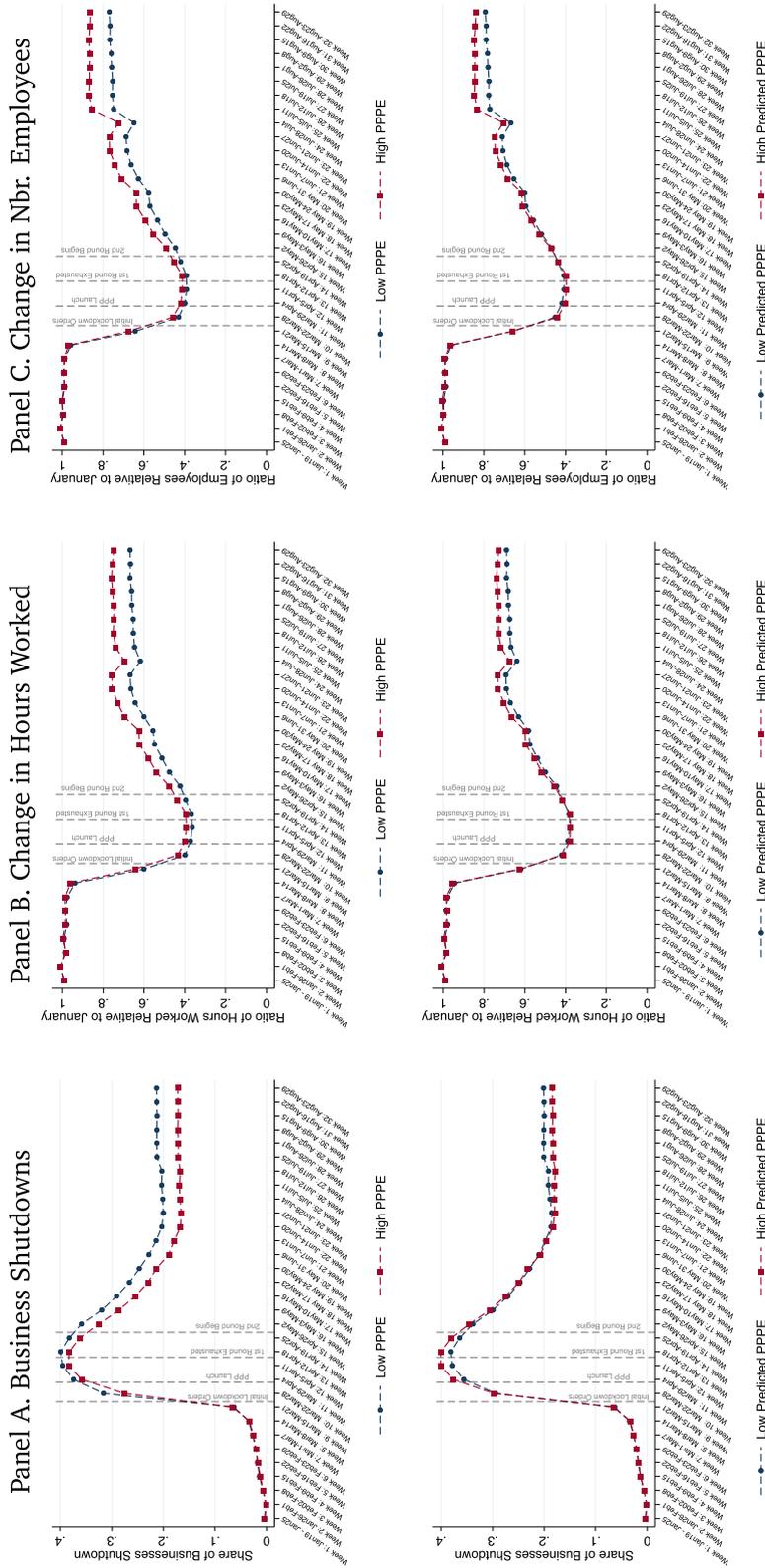
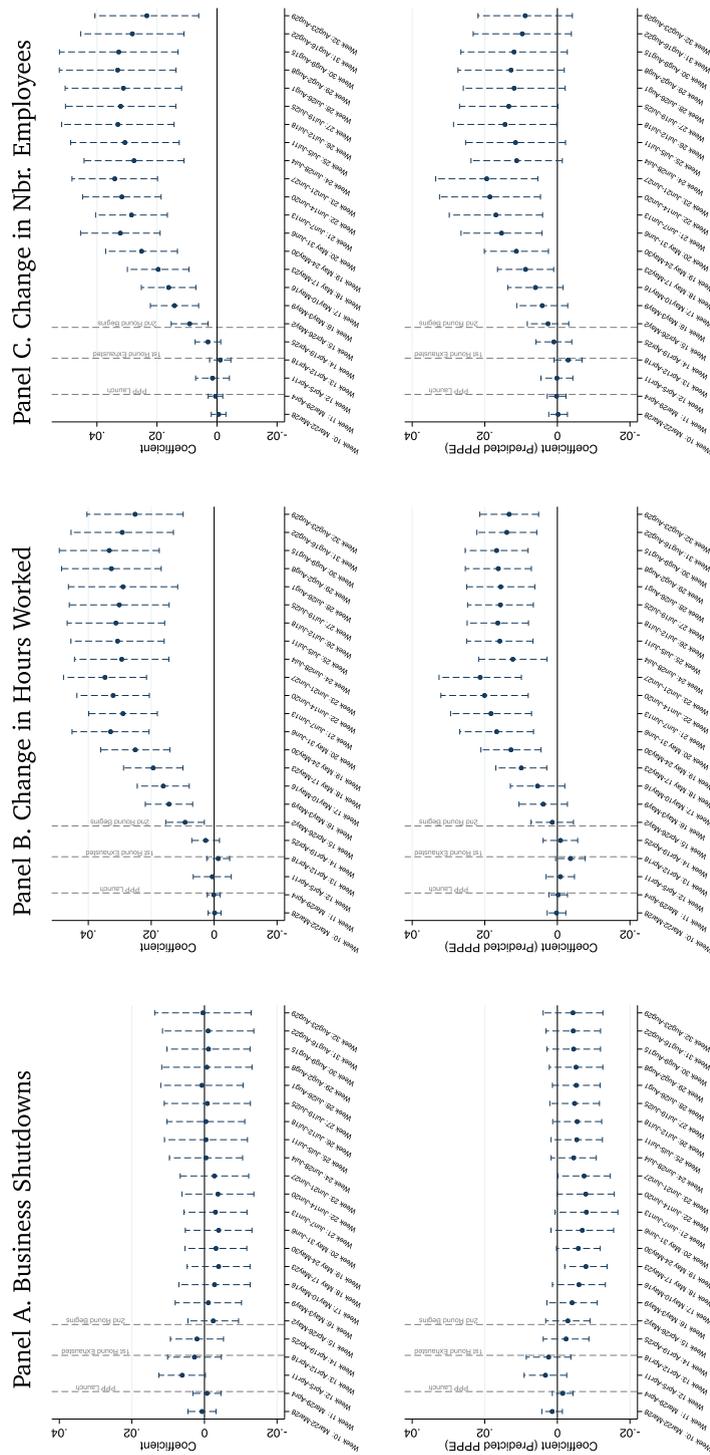


Fig. 7. PPPE and Homebase Employment Outcomes. Fig. 7 shows the ratio of hours worked over time, the percent of businesses shut down, and the ratio of number of employees splitting the sample into regions with above- versus below-median PPPE and above versus below-median predicted PPPE. Data are from SBA, Homebase, County Business Patterns.



**Fig. 8.** PPPE and Homebase Post-PPP Outcomes (Local Projections). Fig. 8 plots coefficients and standard errors of regressions investigating the impact of exposure to PPPE (top row) and predicted PPPE (bottom row) 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  $\beta$  and standard errors of week-by-week regressions of  $\Delta Shutdown_{ijt} = \alpha_{sn} + \beta PPPE_j + \Gamma X_{ijt} + \epsilon_{ijt}$ , where  $\Delta Shutdown_{ijt}$  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,  $\alpha_{sn}$  are state-by-industry fixed effects and  $X_{ijt}$  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 the change in the decline in number of firm employees relative to January as the dependent variable. Data are from Call Reports, SBA, Homebase, and County Business Patterns.

**Table 5**

PPP Exposure and Homebase Employment Outcomes. Table 5 reports the results of OLS regressions examining the relation between exposure to PPP 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.  $\Delta$  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.  $\Delta$  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.  $\Delta$  Nbr. Employees, is the difference in the ratio of the number of employees in each establishment in a week and the average ratio of number of employees in that establishment in weeks 10 and 11. The ratio of number of employees in each establishment is measured as the number of distinct employees that worked in the establishment in that week relative to the number of distinct employees working 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 the number of branches of each bank in the zip code or within 10 miles of the center of the respective ZIP. Predicted PPPE is the weighted average of predicted bank PPPE during the first round at the ZIP level. The predicted values of bank PPPE are obtained from estimating the empirical specification of column (8) of Table 2. The weights are defined by the share of the number of branches of each bank in the zip code or within 10 miles of the center of the respective ZIP.  $I(\text{Month}=\textit{M})$ , where  $M = \{\textit{April, May, June, July, August}\}$  are indicator variables for the weeks that span those respective months. Other control variables include interactions between the median household income, social distance index, COVID cases per capita and deaths per capita measured as of week 9 interacted with the indicator variables for April, May, June, July, and August and controls for the average tier 1 capital and core deposit ratios of all banks within the zip code or within a 10 miles radius of the zip code also interacted with the indicator variables for April, May, June, July, and August. Standard errors are clustered at the state level. \*\*\*, \*\*, and \*, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Bus. Shutdown		$\Delta$ Hours Worked		$\Delta$ Nbr. Employees	
Zip PPPE (Round #1) $\times$ $I(\text{Month}=\textit{April})$	0.002 (0.004)	0.002 (0.003)	0.002 (0.003)	0.003 (0.002)	0.001 (0.003)	0.003 (0.002)
Zip PPPE (Round #1) $\times$ $I(\text{Month}=\textit{May})$	-0.000 (0.004)	-0.003 (0.004)	0.022*** (0.004)	0.020*** (0.004)	0.019*** (0.004)	0.020*** (0.005)
Zip PPPE (Round #1) $\times$ $I(\text{Month}=\textit{June})$	0.005 (0.005)	-0.003 (0.005)	0.034*** (0.006)	0.033*** (0.006)	0.030*** (0.006)	0.032*** (0.007)
Zip PPPE (Round #1) $\times$ $I(\text{Month}=\textit{July})$	0.011** (0.005)	-0.001 (0.006)	0.029*** (0.008)	0.032*** (0.008)	0.026*** (0.008)	0.033*** (0.009)
Zip PPPE (Round #1) $\times$ $I(\text{Month}=\textit{August})$	0.011* (0.005)	-0.001 (0.006)	0.029*** (0.008)	0.031*** (0.008)	0.024** (0.009)	0.031*** (0.009)
Observations	819834	819834	819834	819834	819834	819834
Adjusted R <sup>2</sup>	0.058	0.602	0.134	0.629	0.110	0.571
State $\times$ Industry $\times$ Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes	No	Yes
Firm Fixed Effects	No	Yes	No	Yes	No	Yes
	$\Delta$ Bus. Shutdown		$\Delta$ Hours Worked		$\Delta$ Nbr. Employees	
Predicted PPPE $\times$ $I(\text{Month}=\textit{April})$	-0.000 (0.003)	0.000 (0.003)	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.002)	0.000 (0.002)
Predicted PPPE $\times$ $I(\text{Month}=\textit{May})$	-0.005 (0.003)	-0.006** (0.003)	0.012*** (0.004)	0.009*** (0.003)	0.010*** (0.004)	0.008** (0.003)
Predicted PPPE $\times$ $I(\text{Month}=\textit{June})$	-0.003 (0.004)	-0.007* (0.004)	0.022*** (0.005)	0.019*** (0.005)	0.019*** (0.006)	0.017*** (0.006)
Predicted PPPE $\times$ $I(\text{Month}=\textit{July})$	0.000 (0.003)	-0.005 (0.004)	0.019*** (0.005)	0.017*** (0.004)	0.013* (0.007)	0.014** (0.007)
Predicted PPPE $\times$ $I(\text{Month}=\textit{August})$	0.001 (0.004)	-0.005 (0.004)	0.017*** (0.005)	0.016*** (0.004)	0.011 (0.007)	0.012* (0.007)
Observations	819834	819834	819834	819834	819834	819834
Adjusted R <sup>2</sup>	0.058	0.602	0.133	0.629	0.110	0.571
State $\times$ Industry $\times$ Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes	No	Yes
Firm Fixed Effects	No	Yes	No	Yes	No	Yes

term effects in June, July, and August after state reopenings continued.

In the first two columns of Table 5, the outcome of interest is business shutdowns, in the following two columns it is the decline in hours worked, and in the final pair of columns it is the number of employees. For each pair of columns, the first column includes state-by-industry-by-week fixed effects, while the second column adds firm fixed effects and additional control variables. The top panel shows estimates of Eq. (1) using PPPE as the treatment, while the bottom panel uses predicted PPPE as the treatment.

The table confirms the finding of no statistically or economically significant relationship between PPP bank exposure and these employment outcomes in April, the initial month of the PPP. 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, and either no or marginally significant positive effects in later months when using PPPE. Using predicted PPPE, there is a very small relationship with shutdowns in May and June which fades out by July. For intensive margin employment, the decline in hours worked measure in-

creases for firms with higher PPP exposure in May and June, and this effect remains significant through August. The effect sizes are small—approximately two percentage points in May and three percentage points thereafter for a standard-deviation increase in PPPE—but highly statistically significant. Effects on the number of employees are quite similar to those for the number of hours worked. The coefficient patterns are also quite similar between the top and the bottom panel suggesting that both PPPE and predicted PPPE capture similar variation. In other words, PPPE does not appear to be driven by demand to a great extent relative to predicted PPPE.

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%.<sup>31</sup> Using the reduced form estimates for April in Table 5, column (2), this change in funding implies an increase in the probability of firm shutdown of 0.6 percentage points ( $= 0.44 \times 0.002 \times (1/0.16)$ ), where 0.16 is one standard deviation of PPPE. The lower bound of the 95% confidence interval is well below a one percentage point effect. Analogous calculations for the other outcomes give similarly small effect sizes. The effect sizes are marginally smaller when using predicted PPPE as an instrument rather than PPPE, though the confidence intervals overlap. Thus, relative to the aggregate patterns in Fig. 7—a 40 percentage point increase in the probability of firm shutdown and 60 percentage point reductions in the ratio of hours worked and the number of employees 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 0.020 implies an increase in hours worked of 5.5 percentage points ( $= 0.44 \times 0.020 \times (1/0.16)$ ) with a 95% confidence upper bound of 7.7 percentage points ( $= 0.44 \times (0.020 + 1.96 \times 0.004) \times (1/0.16)$ ). The analogous estimates for June are 9.4 and 12.5 percentage points, respectively, which stabilize through August. Estimates for the number of employees are nearly identical to those for the hours worked outcome.

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.<sup>32</sup>

<sup>31</sup> This calculation comes from  $0.44 \times 0.19$ , 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.

<sup>32</sup> Appendix F uses Homebase data to study the relationship between PPPE and a measure of “permanent” shutdowns, defined as the establish-

ment being closed for all weeks from the beginning of the PPP through the end of August. The results suggest a non-trivial impact of PPP on firms over the medium run, consistent with the idea that some firms that did not receive funds early enough decided to close permanently.

Our results are largely consistent with some contemporaneous evidence from other researchers using different data sets and research designs. Autor et al. (2020) (henceforth 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%. Chetty et al. (2020) use high frequency employment data from several payroll processors for small businesses and study the evolution of employment outcomes for firms above and below the 500 employee PPP eligibility threshold. They find statistically insignificant effects on employment with confidence intervals that permit modest effect sizes. Hubbard and Strain (2020) use Dun & Bradstreet data to implement the threshold design. They present some specifications that yield larger estimates, but the overall takeaway from their analysis appears in line with these other threshold designs.

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, the threshold 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.<sup>33</sup>

Several other studies use differences in the timing of PPP receipt to examine the program’s employment effects, while also exploiting differences in timing due to pre-existing variation in bank lending relationships. Li and Strahan (2020) find modest employment effects of the program, as we do, while Faulkender et al. (2020) and Doniger and Kay (2021) find substantially larger employment effects. While the conceptual approach in these papers is similar to ours, a key source of difference is the extent to which the research design accounts for nonrandom program targeting, which we show is quantitatively important. Bartik et al. (2020b) also find significantly lower self-reported survival probabilities for firms whose primary banks were in the top four, though the outcome and sample of very small firms in this study make it difficult to compare their results to ours.

ment being closed for all weeks from the beginning of the PPP through the end of August. The results suggest a non-trivial impact of PPP on firms over the medium run, consistent with the idea that some firms that did not receive funds early enough decided to close permanently.

<sup>33</sup> 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. Appendix F presents results using the Census Household Pulse Survey data that lean against this interpretation. A relatively small share of households report receiving any payment for time not working in the previous week. Importantly, the share of households reporting receiving no pay is not associated with state PPPE.

**Table 6**

PPP Exposure and Local Labor Market and Economic Effects. Table 6 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 from Womply, and employment growth from Opportunity Insights.  $\Delta$  UI Claims is the difference between the county unemployment filings during a week and the average unemployment filings in the county in weeks 10 and 11.  $\Delta$  Small Business Revenue is the difference between the county aggregate change in small business revenue relative to January and the average change in small business revenue in weeks 10 and 11 relative to January. Aggregate change in small business revenue is from Womply.  $\Delta$  OI Emp. is the difference between county employment growth relative to January in a week and the average county employment growth relative to January in weeks 10 and 11. The county-level employment data come from Opportunity Insights. County PPPE is the weighted county average of the bank PPPE at the end of the first round, weighted by the share of the number of branches of each bank in each county. County Predicted PPPE is the weighted county average of predicted bank PPPE at the end of the first round. The predicted values of PPPE are obtained from estimating the empirical specification of column (8) of Table 2. The weights are defined by the share of the number of branches of each bank in the county.  $I(\text{Month}='M')$ , where  $M = \{\text{April, May, June, July, August}\}$  are indicator variables for the weeks that span those respective months. Other control variables include interactions between the median household income, social distance index, COVID cases per capita and deaths per capita measured as of week 9 interacted with the indicator variables for April, May, June, July, and August and controls for the average tier 1 capital and core deposit ratios of all banks within the county also interacted with the indicator variables for April, May, June, July, and August. Appendix Table B.1 shows summary statistics. Standard errors are clustered at the state level. \*\*\*, \*\*, and \*, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ UI claims		$\Delta$ Small Bus. Rev.		$\Delta$ OI Emp.	
County PPPE $\times$ I(Month=April)	-0.111 (0.071)	-0.114** (0.056)	0.013*** (0.004)	0.001 (0.004)	-0.001 (0.002)	-0.000 (0.003)
County PPPE $\times$ I(Month=May)	-0.151* (0.086)	-0.158** (0.073)	0.028*** (0.005)	0.007 (0.007)	0.009*** (0.003)	0.007*** (0.003)
County PPPE $\times$ I(Month=June)	-0.104 (0.082)	-0.119 (0.074)	0.003 (0.006)	-0.011 (0.008)	0.016** (0.006)	0.011*** (0.003)
County PPPE $\times$ I(Month=July)	-0.073 (0.099)	-0.116 (0.075)	-0.012** (0.005)	-0.022** (0.009)	0.020** (0.008)	0.014*** (0.003)
County PPPE $\times$ I(Month=August)	-0.066 (0.106)	-0.084 (0.073)	-0.014*** (0.005)	-0.021*** (0.007)	0.020** (0.009)	0.013*** (0.004)
Observations	46092	45533	43930	43863	17112	17112
Adjusted R <sup>2</sup>	0.745	0.950	0.491	0.735	0.697	0.879
State $\times$ Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes	No	Yes
County Fixed Effects	No	Yes	No	Yes	No	Yes
	$\Delta$ UI claims		$\Delta$ Small Bus. Rev.		$\Delta$ OI Emp.	
County Predicted PPPE $\times$ I(Month=April)	-0.084 (0.084)	-0.059 (0.057)	0.011*** (0.003)	0.002 (0.003)	-0.001 (0.003)	-0.000 (0.003)
County Predicted PPPE $\times$ I(Month=May)	-0.124 (0.100)	-0.098 (0.073)	0.025*** (0.008)	0.008 (0.007)	0.007 (0.006)	0.002 (0.004)
County Predicted PPPE $\times$ I(Month=June)	-0.103 (0.094)	-0.090 (0.075)	0.013 (0.009)	0.004 (0.009)	0.015 (0.010)	0.006 (0.004)
County Predicted PPPE $\times$ I(Month=July)	-0.064 (0.125)	-0.097 (0.080)	-0.002 (0.006)	-0.004 (0.006)	0.018 (0.014)	0.008 (0.005)
County Predicted PPPE $\times$ I(Month=August)	-0.016 (0.142)	-0.037 (0.080)	-0.001 (0.006)	-0.002 (0.005)	0.019 (0.015)	0.008 (0.006)
Observations	46092	45533	43886	43863	17112	17112
Adjusted R <sup>2</sup>	0.744	0.950	0.489	0.734	0.688	0.878
State $\times$ Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes	No	Yes
County Fixed Effects	No	Yes	No	Yes	No	Yes

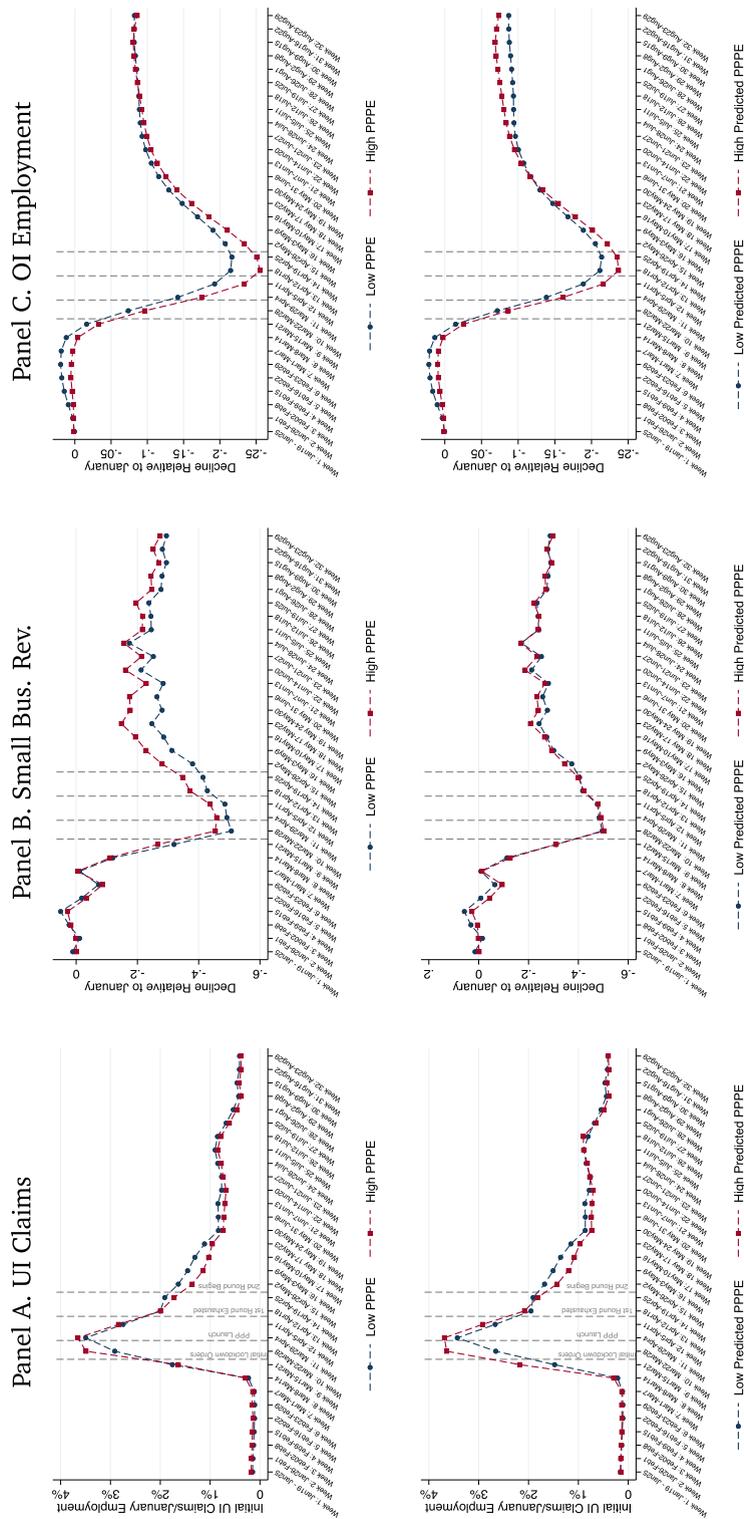
### 5.3. Local labor market and economic activity

Figs. 9 and 10 and Table 6 present results using broader measures of employment outcomes: initial unemployment insurance (UI) claims, small business revenue, and employment data from Opportunity Insights (OI). We focus on county-level outcomes because that is the finest level of aggregation for which these data are available.

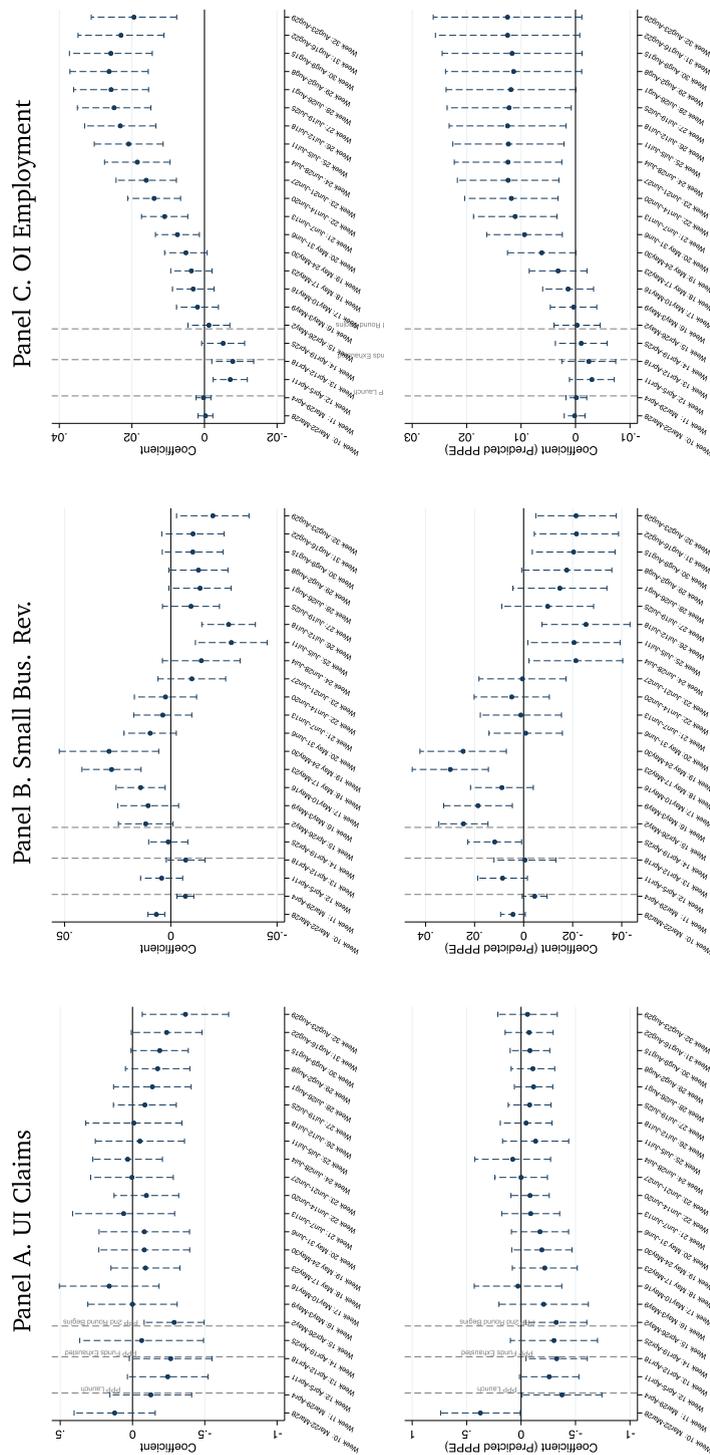
Fig. 9 splits the counties in the sample in two groups based on their PPPE and predicted PPPE measures and plots the evolution over time of average employment outcomes. The plots suggest that, prior to lockdown orders, average UI claims are relatively low, and UI claims, small business revenues, and OI employment rates all trend similarly across both groups during the period. After the ini-

tial lockdown orders, UI claims surge and small business revenues and OI employment rates decline in both groups. The high-PPPE group sees somewhat lower UI claims in May, and small business revenues and OI employment rates recover faster for this group relative to the low-PPPE group from mid-April until the end of May. These differences subsequently fade. The graphs also point to the importance of targeting differences across groups, as high-PPPE areas appear to be differentially hit prior to the PPP's rollout.

Fig. 10 provides further graphical evidence of the impact of PPP on these outcomes. The figure repeats the local projection analysis, replacing the main outcomes with the difference in UI claims, decline in small business revenue, and change in OI employment rates. We observe lit-



**Fig. 9.** PPPE and Alternative Outcome Variables. Fig. 9 shows the evolution of the ratio of weekly initial unemployment filing claims at the county level and total county employment (Panel A), the change in aggregate small business revenue at the county level relative to January (Panel B), and the ratio of county employment relative to January (Panel C). We exclude California counties from the time series of Panel A due to a large outlier in UI claims that is likely due to a backlog in UI claims processing in that state. In the appendix, we include the plot that includes Californian counties. Data are from Call Reports, SBA, County Business Patterns, State Labor Departments, Opportunity Insights website, and Womply.



**Fig. 10.** PPPE and Alternative Outcome Variables (Local Projections). Fig. 10 plots coefficients and standard errors of regressions investigating the impact of exposure to PPPE (top row) and predicted PPPE (bottom row) 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  $\beta$  and standard errors of week-by-week regressions of  $\Delta UI_c = \alpha_{st} + \beta PPPPE_c + \Gamma X_c + \epsilon_c$ , where  $\Delta UI_c$  is the difference between the UI claims of county  $c$  in each week of the sample and the average UI claims for that county during the two weeks prior to program launch,  $PPPPE_c$  is the average exposure of the county to bank PPPE,  $\alpha_s$  are state fixed effects and  $X_c$  are additional control variables. Panel B plots estimates from similar week-by-week regressions that use the change in weekly small business revenue relative to January as the dependent variable. Panel C plots estimates from similar week-by-week regressions that use the change in weekly employment outcomes relative to January as the dependent variable. Data are from Call Reports, SBA, Womply, and Opportunity Insights.

tle discernible impact on UI claims. The small business revenues analysis suggests a positive impact of PPP initially, which levels off in the subsequent months. The OI data suggest a pattern similar to our Homebase analysis, albeit with slightly smaller magnitudes.

Table 6 repeats the analysis of Table 5 for these outcomes. We find a small statistically significant effect on UI claims in April and May when using PPPE, which dissipates by June. Effects are insignificant when using predicted PPPE. Following the calculations above, the 95% lower bound estimated effect for the month of May is -13.2 basis points ( $= 0.44 \times (-0.158 - 1.96 \times 0.073)$ ). The middle columns show the relationship between small business revenue and PPPE or predicted PPPE. We find a positive relationship between PPP exposure and small business revenue, which is statistically significant in a few specifications in April and May, but levels off and even becomes negative in subsequent months. One possible explanation is that access to liquidity through the PPP allowed firms to avoid engaging in high-risk practices to generate revenue. For example, restaurants may have been able to close and focus on food delivery as opposed to resuming in-person dining when allowed. Early PPP recipients may also have felt less urgency to make investments to reopen immediately, instead electing to defer operations until more uncertainty resolved. The last two columns present regressions of employment growth in OI on county-level PPPE and predicted PPPE. The estimates suggest small effects in April that rise modestly in May and June.

The results using predicted PPPE, shown in the bottom panel, point to statistically insignificant effects of the PPP on UI or employment, and small effects on small business revenues in early months that dissipate by June. However, the point estimates are similar and the confidence intervals are large enough that we cannot rule out the reduced form effects from the PPPE instrument.

Table 7 presents a formal analysis that reconciles estimated employment effects across the Homebase and OI samples and when comparing the PPPE to predicted PPPE estimates. We estimate IV regressions for each sample and instrument, focusing in Panel A on the local projection estimate in week 23 (June 21–27), the week with the largest coefficient in the Homebase sample. Panel B presents estimates for all weeks pooled together. The Homebase data is not representative of the broader PPP-eligible economy, especially in terms of industry composition. We therefore also present a reweighted Homebase analysis.<sup>34</sup> Following DiNardo et al. (1996), we reweight observations to match the less-than-500-worker-establishment-count distribution across industries in the Census SUSB data. Concretely, this reweighting downweights bars and restaurants relative to other industries.

<sup>34</sup> The Homebase industry categories are coarsely defined and do not have a one-to-one mapping with NAICS industry categories. Thus, each Homebase category can potentially span multiple two-digit NAICS industries. We use the self-reported industry category of each PPP applicant recorded in their PPP applications to create a mapping between the Homebase industry for our matched sample and two-digit NAICS industries and apply this mapping to the full Homebase sample.

We emphasize two takeaways. First, the first stage coefficient for PPPE is marginally stronger than for predicted PPPE (see also Fig. 4, discussed in Section 4.4). Accounting for this difference brings estimated impacts of PPP loans on employment to statistical equivalence within each sample. If anything, the IV coefficients for predicted PPPE are slightly larger than for PPPE; we interpret this fact as suggesting that omitted upward-biasing demand factors are unlikely to confound our PPPE-based estimates.

The second takeaway is that reweighting the Homebase sample can reconcile the seemingly disparate estimates between OI and Homebase. Whereas the coefficient for the unweighted Homebase sample is statistically and economically greater than the OI coefficient at the 95% level, the reweighted Homebase coefficient falls by approximately one-third.<sup>35</sup> In this specification, bootstrapped confidence intervals for the difference between the OI and Homebase estimates are statistically insignificant in Panel A and barely reject zero in Panel B. As discussed in Section 6, given that Homebase includes smaller firms that are more likely to show larger responses, allowing for heterogeneous impacts by firm size would further narrow the modest remaining gap between these estimates.

Overall, no specification or outcome variable suggests large changes in employment across ZIPs or counties based on either PPPE measure, despite the large differences in program access predicted by these measures. Once we account for differences in first stage strength across instruments and industry representation across data samples, all specifications paint a consistent picture of modest, positive employment impacts.

#### 5.4. Matched sample analysis

We complement our regional estimates with a sample of 10,694 firms, for which we are able to match PPP loan information to payroll information from Homebase. In this analysis, we have a smaller sample of firms, but we can also directly measure if and when each individual firm obtained a PPP loan. We use the individual matched data and variation in the timing of when firms received PPP loans to examine the impact of PPP receipt on firms' employment outcomes. We ask whether differences in timing materially affected short-term employment outcomes of firms that received loans earlier versus later. Because the timing of loan receipt may reflect differences in loan demand across firms, we also instrument for the timing of receipt using PPPE and predicted PPPE. This alternative strategy provides a useful way to assess the robustness of our main results.

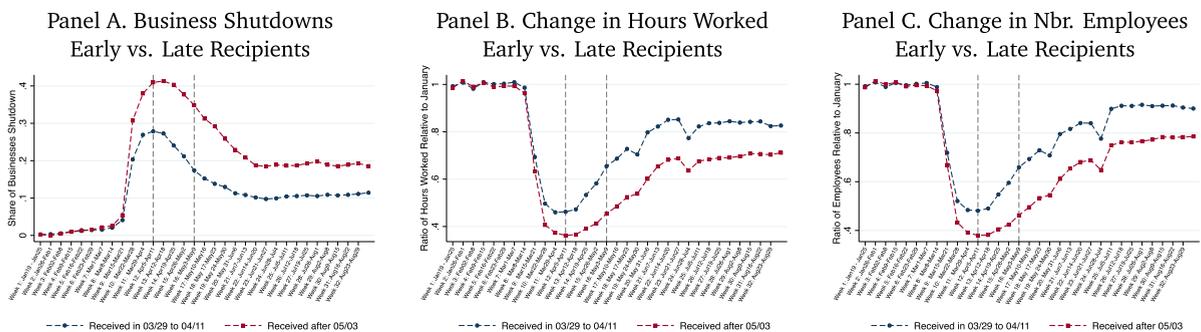
Fig. 11 shows the evolution of business shutdowns (Panel A), change in hours worked (Panel B), and the change in employee counts over time (Panel C) for early and late recipients of PPP funds. Early recipients are defined as those firms that receive a loan in the week ending on April 11th or earlier and late recipients are firms that

<sup>35</sup> Appendix F shows that the estimated effects of the program in the Homebase sample are generally stronger for the Food & Drink and Retail industries, which are disproportionately represented in Homebase relative to the Census or OI.

**Table 7**

Reconciling Estimates across Samples and Exposure Measures. Table 7 presents employment estimates across the Homebase and OI samples and compares estimates using the PPPE and predicted PPPE instruments. We estimate IV regressions for each sample and instrument, focusing on the local projection estimates in week 23 (June 21–27), the week with the largest coefficient in the Homebase sample, and pooled across all weeks. The endogenous variable is the fraction of establishments in an area that received PPP as of the end of the first round. We also present an industry-reweighted Homebase analysis. The Homebase industry categories are coarsely defined and do not have a one-to-one mapping with NAICS industry categories. Thus, each Homebase category can potentially span multiple two-industry NAICS industries. We use the self-reported industry category of each PPP applicant recorded in their PPP applications to create a mapping between the Homebase industry and the two-digit NAICS industries. Following DiNardo et al. (1996), we then reweight observations to match the less-than-500-worker-establishment-count distribution across industries in the Census SUSB data. Concretely, this reweighting downweights bars and restaurants relative to other industries. All specifications include state (OI) or state-by-industry (Homebase) fixed effects and pre-policy controls. Statistical tests within a sample are computed via simultaneous GMM with standard errors clustered at the state level. Confidence intervals report the difference in coefficients across samples, computed using bootstrapped coefficient distributions. \*\*\*, \*\*, and \*, represent statistical significance at 1%, 5%, and 10% levels, respectively.

Panel A. Peak Employment Effect (June 21–27)						
	(1) Homebase, No Wt		(4) Opportunity Insights		(6) Homebase, Indy Wt	
2SLS Coefficient	0.834*** (0.180)	1.354*** (0.511)	0.324*** (0.081)	0.594** (0.254)	0.550** (0.240)	1.565*** (0.568)
First Stage Coefficient	0.261*** (0.021)	0.229*** (0.047)	0.310*** (0.028)	0.259*** (0.095)	0.269*** (0.022)	0.228*** (0.047)
Instrument	PPPE	Predicted PPPE	PPPE	Predicted PPPE	PPPE	Predicted PPPE
Geography	ZIP	ZIP	County	County	ZIP	ZIP
N	35645	35645	742	742	35645	35645
F-Statistic	146.8	23.3	120.6	7.5	144.8	24.1
P-value vs PPPE	-	0.297	-	0.595	-	0.080
95% CI $HB_{NoWt} - OI$	-	-	[0.280, 0.849]	[-3.553, 3.720]	-	-
95% CI $HB_{Wt} - OI$	-	-	-	-	[-0.087, 0.640]	[-3.052, 3.880]
Panel B. Average Employment Effect (April–August)						
	(1) Homebase, No Wt		(4) Opportunity Insights		(6) Homebase, Indy Wt	
2SLS Coefficient	0.524*** (0.134)	0.653*** (0.244)	0.229*** (0.053)	0.353** (0.145)	0.308* (0.162)	0.655** (0.286)
First Stage Coefficient	0.261*** (0.022)	0.229*** (0.047)	0.301*** (0.027)	0.237** (0.093)	0.268*** (0.022)	0.228*** (0.047)
Instrument	PPPE	Predicted PPPE	PPPE	Predicted PPPE	PPPE	Predicted PPPE
Geography	ZIP	ZIP	County	County	ZIP	ZIP
N	820616	820616	17135	17135	820616	820616
F-Statistic	146.8	23.4	124.9	6.4	144.3	24.0
P-value vs PPPE	-	0.615	-	0.616	-	0.223
95% CI $HB_{NoWt} - OI$	-	-	[0.251, 0.417]	[-0.242, 0.522]	-	-
95% CI $HB_{Wt} - OI$	-	-	-	-	[0.024, 0.204]	[-0.282, 0.534]



**Fig. 11.** PPPE and Post-PPP Outcomes (Matched Sample Analysis). Fig. 11 investigates business shutdowns, changes in the ratio of hours worked, and changes in the number of employees for firms in the Homebase sample that are name-matched to the PPP data set from SBA. We compare firms that received PPP approval in week 12 or earlier to those that received PPP approval in week 16 or later. Data is from Call Reports, SBA, and Homebase.

**Table 8**

Homebase Employment and PPP Loan Timing (Matched Sample). **Table 8** presents the results from the individual matched sample. The left-hand-side variable in Panel A,  $\Delta$  *Shutdown*, is the difference between each matched firm’s shutdown status in week 16 (May 3rd to May 9th) and its average shutdown status in weeks 10 and 11. Shutdown is an indicator variable that takes the value of one if the business reported zero hours worked over the entire week.  $\Delta$  *Hours Worked* is the change between the average number of hours worked at each establishment in the last two weeks prior to the launch of PPP and the number of worked at each establishment in week 16 (May 3rd to May 9th).  $\Delta$  *Nbr. Employees* is the change between the average number of employees working for each establishment in the last two weeks prior to the launch of PPP and the number of employees working for each establishment 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)	(5)	(6)
	$\Delta$ Shutdown					
	OLS		IV		IV	
Week of PPP loan	0.007*** (0.002)	0.007*** (0.002)	0.012 (0.009)	0.014 (0.019)	0.026** (0.012)	0.061* (0.032)
Observations	10694	10024	10556	9891	10556	9891
F-Stat			186.060	51.073	28.033	4.982
Industry Fixed Effects	Yes	No	Yes	No	Yes	No
State×Ind Fixed Effects	No	Yes	No	Yes	No	Yes
Instrument	-	-	PPPE Zip		Predicted PPPE	
Panel B. Ratio Hours Worked						
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Hours Worked					
	OLS		IV		IV	
Week of PPP loan	-0.012*** (0.001)	-0.011*** (0.001)	-0.057*** (0.006)	-0.059*** (0.018)	-0.046*** (0.013)	-0.076 (0.046)
Observations	10694	10024	10556	9891	10556	9891
F-Stat			186.060	51.073	28.033	4.982
Industry Fixed Effects	Yes	No	Yes	No	Yes	No
State×Ind Fixed Effects	No	Yes	No	Yes	No	Yes
Instrument	-	-	PPPE Zip		Predicted PPPE	
Panel C. Ratio Nbr. Employees						
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta$ Nbr. Employees					
	OLS		IV		IV	
Week of PPP loan	-0.011*** (0.001)	-0.010*** (0.001)	-0.047*** (0.006)	-0.047*** (0.017)	-0.040*** (0.011)	-0.053 (0.039)
Observations	10694	10024	10556	9891	10556	9891
F-Stat			186.060	51.073	28.033	4.982
Industry Fixed Effects	Yes	No	Yes	No	Yes	No
State×Ind Fixed Effects	No	Yes	No	Yes	No	Yes
Instrument	-	-	PPPE Zip		Predicted PPPE	

receive a loan in the week beginning May 3rd or later. For all three outcomes, 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, consistent with prior results suggesting that early recipients were bigger and less constrained firms (Balyuk et al., 2020; Doniger and Kay, 2021). 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.

To address the concern that difference in timing may be driven by demand, we instrument using PPPE and predicted PPPE. **Table 8** 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 3rd to May

9th, 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:

$$\Delta y_{isj} = \alpha_{sn} + \zeta \text{WeekPPP}_i + \varepsilon_{is}$$

where  $\Delta y_{isj}$  is the difference between the outcomes  $y_{isj}$  (shutdown, hours worked, number of employees) of firm  $i$  in the week beginning on May 3rd, and the average outcome for the same firm during the two weeks prior to the launch of PPP. Using the two weeks prior to the launch of PPP as a benchmark is particularly important in this analysis given the substantial gap that opens between the average employment outcomes for these groups prior to the

PPP launch.  $WeekPPP_i$  is the week in which a firm received a PPP loan, and thus  $\zeta$  captures the effect of receiving a PPP loan one week later. The term  $\alpha_{sn}$  represents industry- or state-by-industry fixed effects. The first two columns show OLS estimates, with columns (1) and (2) including industry and state-by-industry fixed effects, respectively. Columns (3) and (4) show IV estimates, instrumenting the week in which a firm received PPP with PPPE measured at the ZIP level. The final two columns show IV estimates, instrumenting the week in which a firm received PPP with predicted PPPE, again measured at the ZIP level.

The results in Table 8 are largely consistent with our bank exposure results, suggesting modest short-term effects of the PPP. In the top panel the outcome is business shutdowns, in the middle panel it is hours worked, and in the bottom panel it is the number of employees. In all regressions, the magnitudes of the OLS coefficients are smaller than magnitudes of the IV estimates. One interpretation of this fact is that larger firms that were less credit-constrained had earlier access to the program, which would underscore the importance of instrumenting for the timing of PPP receipt. In the case of column (6) in all panels, the inclusion of state-by-industry fixed effects weakens the first stage of the IV considerably, which might explain the relatively larger magnitudes of the coefficients and imprecision of those estimates. As a result, we do not draw strong conclusions from this specification, though the results remain broadly consistent if noisier.

Panel A suggests marginally significant effects on business shutdowns in the IV specification, and that obtaining a PPP loan one week earlier leads to a decrease in shutdowns of between 1.4 and 2.6 percentage points. In Panel B, we find that obtaining a PPP loan one week earlier leads to an increase in hours worked of between 4.6 and 5.9 percentage points for a firm receiving a PPP loan a week earlier. Panel C points to similar effects on the number of employees, with obtaining a PPP loan one week earlier leading to an increase in the number of employees of between 4.0 and 4.7 percentage points for a firm receiving a PPP loan a week earlier.<sup>36</sup>

## 6. Aggregate impacts

Our approach to aggregation follows Mian and Sufi (2012) and Berger et al. (2020). We estimate the total employment gains caused by the program in its first five months, exploiting only differences in cross-sectional exposure and using the group receiving the smallest shock as a counterfactual. We choose the bottom 1% of geographies (ZIP or county) 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.<sup>37</sup>

PPPE for the bottom group is  $-0.30$  and increases to  $0.41$  for the highest group. Thus, for exposure group  $g$ , the aggregate increase in employment induced by the program is:

$$\Delta Y_g = \beta_t \times (e_g - (-0.30)) \times Y_{g,pre}, \quad (2)$$

where  $\beta_t$  is our reduced form estimate on PPPE,  $e_g$  is the weighted-average program exposure where the weights are estimated eligible employment in each geography, and  $Y_{g,pre}$  is within-sample pre-program employment. A less conservative approach aggregates estimates relative to a no-exposure baseline, which equals  $-0.5$ . We also report estimates based on the predicted PPPE exposure measure and from either the Homebase or OI samples, which represent different levels of local labor market aggregation.

We estimate reduced form regressions for each week from the beginning of the program through the end of August. For local labor market data, we focus on the OI employment measure. For Homebase data, we focus on the number of employees measure and the reweighted sample from Section 5.3 that balances the industry composition of Homebase. The reduced form regressions correspond to the IV specifications in Table 7, Panel A, columns (3) and (5), respectively. We report week-specific estimates from different phases of the program and a cumulative estimate that averages weekly estimates from the beginning of the program through August.

Table 9 presents the estimates. Using the PPPE design and reweighted Homebase sample, we estimate the PPP increased employment during the lockdown period by 1.57 million in week 15, or 2.2% of pre-program employment. This effect rises during the reopening period to 3.55 million (5.1%) in week 22 and then falls later in the program to 2.71 million (3.9%) in week 29. The cumulative impact over the program's first five months is 2.02 million (2.9%). Estimates based on predicted PPPE give nearly identical results.

Note this is a lower-bound estimate if the lowest exposure ZIP also responds to the program. When we aggregate relative to a no-exposure baseline, we estimate an increase that ranges between 3.28 and 5.43 million (4.7–7.8%). Averaging the more conservative and more aggressive estimates yields an estimated range of 3.8–5.4%.

Estimates based on the OI sample are quite similar in terms of cumulative magnitudes, ranging between 1.77 and 4.52 million. However, the dynamics in the OI data suggest a smaller effect during the lockdown period that rises during the reopening period before leveling off.<sup>38</sup> Averaging

<sup>36</sup> Appendix F shows the coefficients of week-by-week regressions that repeat the OLS and IV specifications of columns (1) and (3) of Table 8 for every week in the sample. Similar to our regional analyzes, the dynamics indicate that gaps in the number of employees and hours work persist until August. Again, we caution that the OLS estimator will be biased if firms that obtained loans earlier are fundamentally different from firms that received PPP loans later.

<sup>37</sup> 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.

<sup>38</sup> Table 9 also reports a specification that accounts for firm size heterogeneity by assuming treatment effects for larger firms that are half the size of the firms in Homebase. We assume these firms account for one-third of eligible employment. The Homebase sample comprises mainly small firms with median pre-program employment of 27 and mean pre-

**Table 9**

Aggregate Effects under Alternative Estimation Methods. Table 9 summarizes our estimates of the aggregate effect of the PPP program. The aggregate effect is presented based on different analysis samples and aggregation assumptions. We present estimates based on the ZIP-level estimates from Homebase employment data, reweighted to balance the industry composition of the Homebase data, and based on the county-level estimates from OI employment data. For Homebase, we include a specification that accounts for firm size heterogeneity by assuming treatment effects for larger firms that are half the size of the firms in Homebase. We assume these firms account for one-third of eligible employment. We report week-specific estimates from different phases of the program (lockdown, reopening, reopened) and a cumulative estimate that averages weekly estimates from the beginning of the program through August. Percent impacts in parentheses are computed relative to the eligible population of 70 million.

Estimation Window	(1) Week 15, Millions (%)	(2) Week 22, Millions (%)	(3) Week 29, Millions (%)	(4) Cumulative through August, Millions (%)	(5)
<b>Homebase, Industry Weights</b>					
A. Relative to Bottom 1%	1.57 (2.2%)	3.55 (5.1%)	2.71 (3.9%)	2.02 (2.9%)	2.11 (3.0%)
B. Relative to Zero	2.55 (3.6%)	5.76 (8.2%)	4.40 (6.3%)	3.28 (4.7%)	5.43 (7.8%)
C. Average of A and B	2.06 (2.9%)	4.66 (6.7%)	3.56 (5.1%)	2.65 (3.8%)	3.77 (5.4%)
D. C with Firm Size	1.72 (2.5%)	3.88 (5.5%)	2.96 (4.2%)	2.21 (3.2%)	3.14 (4.5%)
Heterogeneity					
<b>Opportunity Insights</b>					
A. Relative to Bottom 1%	0.72 (1.0%)	1.80 (2.6%)	2.16 (3.1%)	1.97 (2.8%)	1.77 (2.5%)
B. Relative to Zero	0.99 (1.4%)	2.49 (3.6%)	2.98 (4.3%)	4.52 (6.5%)	4.18 (6.0%)
C. Average of A and B	0.86 (1.2%)	2.15 (3.1%)	2.57 (3.7%)	3.25 (4.6%)	2.98 (4.3%)
<b>Combined Estimates</b>					
A. Average of HB.D and OI.C	1.29 (1.8%)	3.01 (4.3%)	2.77 (4.0%)	2.73 (3.9%)	3.06 (4.4%)
Exposure Measure	PPPE	PPPE	PPPE	PPPE	Predicted PPPE

estimates from Homebase and OI yields a combined employment impact of approximately 3 million workers, or 4% of pre-program employment, with peak employment impacts perhaps 1 to 2 percentage points higher.

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-year of \$175,000 under the assumption that all the induced jobs persist for a year.<sup>39</sup> Incorporating the saved funds from lower 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.

Relative to ACCGLMPRVY, our estimates are quite similar. Our aggregate estimates are remarkably consistent with a recent paper by Autor et al. (2022) (henceforth ACCGLMPRVY22) who, building on ACCGLMPRVY, find the PPP increased employment by about 3 million jobs per week in the second quarter of 2020. Their overall estimates through

June 2021 are 1.98 million worker years of employment at a cost of \$258,000 per worker-year retained.

We do find an increasing treatment effect over the course of the program’s first few months, whereas ACCGLMPRVY find an immediate response that appears more 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; ACCGLMPRVY22) 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.

## 7. Interpretation and mechanisms

### 7.1. Potential channels

The primary focus of our paper is to evaluate the PPP and the role of banks in driving the policy response we identify. 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. Thus, while differences in bank performance lead to distortions in access to the program, these differences appear relatively unimportant for employment, given the small overall employment effects we estimate. If firms did not primarily maintain or increase employment, what did they do with PPP funds and how might the funds ultimately affect employment?

There are several non-mutually exclusive channels through which businesses may have absorbed the funds

program employment of 37 and very few firms with more than 100 employees. This approach follows logic in Autor et al. (2022), who consider aggregation based on evidence that firms away from the 500 worker discontinuity display larger treatment effects. Even after accounting for firm size heterogeneity, the Homebase and OI estimates remain close.

<sup>39</sup> If half of the induced jobs persisted for 12 months, the estimate would be \$246,000; if they all ended in August, the estimate would be \$414,000. Finally, some of the PPP loans will be reimbursed, which reduces the overall cost of the program. Given the research design limits our ability to explore longer run impacts, we leave to future work to provide more comprehensive cost-per-job-year estimates.

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. In these cases, the program's benefits accrue to the firm's owners.<sup>40</sup>

Second, firms retained significant flexibility in how they could use the funds over time, and they may have used funds initially to strengthen balance sheets and for non-employment related expenses. Financial frictions can amplify precautionary savings motives, which imply ambiguous impacts on employment. While funds may have gone to distressed firms, they may still choose to downsize and cut employees in the face of uncertainty. For example, firms were uncertain about the duration of the pandemic and future revenue streams, and likely wanted to hold cash to survive a longer duration crisis. Such motives are consistent with Almeida et al. (2004) and Riddick and Whited (2009) who find that uncertainty increases firms' precautionary motives to hold cash, particularly when external financing is difficult to obtain.

Third, some firms may have increased employment or called back workers, though they account for a relatively small share of total recipients. The primary channel through which the PPP could affect employment is through financial frictions. Firms may temporarily need liquidity during the downturn to cover cash shortfalls, either due to a loss in demand or lockdown policies. These firms may be unable to access credit, for example, due to classic asymmetric information effects where lenders are unable to separate firms that will survive from those that fail (Stiglitz and Weiss, 1981). In this case, credit supply can be inefficiently low. PPP guarantees would make lenders willing to extend credit, enabling liquidity-constrained firms to survive, raising employment, and potentially increasing aggregate welfare by shifting credit supply toward efficient levels.<sup>41</sup>

Finally, related to the first channel, banks may substitute more generous PPP loans for other lending that would have happened otherwise (Gale, 1991). Such crowd-out of private financing is also consistent with small employment effects. Relatedly, business stealing spillovers between eligible and ineligible employers could account for low employment effects at the labor market level.

<sup>40</sup> Drawing on data from a large survey of business owners on Facebook, Alekseev et al. (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). Additionally Griffin et al. (2021) find evidence of significant fraud, with many loans going to ineligible or even non-existent firms. These loans are unlikely to generate large employment effects.

<sup>41</sup> Programs like the PPP can also increase employment through a subsidy channel, by reducing the cost of capital for firms and possibly attracting excessively risky borrowers. This channel can affect employment even in the absence of financial frictions. In these cases, the welfare benefits of subsidized credit are less clear.

## 7.2. Fixed payments and precautionary savings

To explore the effects of PPP on non-employment financial outcomes, we use information from the first phase of the Census Small Business Pulse Survey measuring the effect of changing business conditions during the Coronavirus pandemic on US small businesses. The first phase of the survey was conducted weekly from April to June 2020.<sup>42</sup> In the top two panels of Table 10, 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 payments at the state-industry level.<sup>43</sup> 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.

In the top panel of Table 10, column (1) indicates that an 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 and predicted PPPE to 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 columns (2) and (4), we focus on the relation between the percentage of firms receiving PPPE and state PPPE or predicted PPPE. Both IVs generate similar results. The relationship is strong, with F-statistics of 115 and 67 when using state PPPE or predicted PPPE as instruments, respectively. In columns (3) and (5), we present results of an IV strategy whereby we instrument for the percentage of firms receiving PPP using state PPPE and predicted PPPE. Using this strategy, we find that a ten percentage point increase in firms receiving PPP is associated with a 1.7 to 1.8 percentage point decline in missing loan payments.

In the middle panel of Table 10, 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

<sup>42</sup> We do not use the second phase of the survey, which began in August 2020 and ended in October 2020, because it falls outside the sample period of our analysis. Note that the sample size changes across variables, as the Census does not report for some state-industry observations, likely due to censoring.

<sup>43</sup> Unfortunately, the Pulse survey does not separate non-loan scheduled 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.

**Table 10**

PPP Receipt, Missed Payments, and Cash-on-Hand (Census Pulse Survey). Table 10 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 nine weeks from April 26th through June 27th. 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 three 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 weighted state average of bank PPPE at the end of the first round, where the weights are given by the share of the number of branches of each bank in each state. State Predicted PPPE is the weighted state average of predicted bank PPPE at the end of the first round. The predicted values of bank PPPE are obtained from the empirical specification of column (8) of Table 2. The weights are defined by the share of the number of branches of each bank in the state. 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 traveled 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) IV 1st Stage	(3) IV 2nd Stage	(4) IV 1st Stage	(5) IV 2nd Stage
LHS Variable	% Miss Loan Pmt	% PPP Rec.	% Miss Loan Pmt	% PPP Rec.	% Miss Loan Pmt
% PPP Received	-0.013 (0.011)		-0.166*** (0.035)		-0.184*** (0.039)
State PPPE		31.238*** (2.910)			
State Predicted PPPE				62.610*** (7.630)	
Observations	3659	3659	3659	3659	3659
Adjusted R <sup>2</sup>	0.518	0.614	-0.119	0.601	-0.147
FStat			115.265		67.343
LHS Variable	% Miss Schd Pmt	% PPP Rec.	% Miss Schd Pmt	% PPP Rec.	% Miss Schd Pmt
% PPP Received	-0.082*** (0.018)		-0.492*** (0.066)		-0.492*** (0.063)
State PPPE		31.014*** (2.880)			
State Predicted PPPE				62.154*** (7.447)	
Observations	3612	3612	3612	3612	3612
Adjusted R <sup>2</sup>	0.646	0.619	-0.128	0.606	-0.128
FStat			115.934		69.656
LHS Variable	% Cash 3 mths	% PPP Rec.	% Cash 3 mths	% PPP Rec.	% Cash 3 mths
% PPP Received	0.009 (0.030)		0.380** (0.143)		0.316** (0.149)
State PPPE		26.992*** (2.961)			
State Predicted PPPE				58.403*** (6.815)	
Observations	1445	1445	1445	1445	1445
Adjusted R <sup>2</sup>	0.603	0.774	-0.270	0.767	-0.200
FStat			83.103		73.435
Controls	Yes	Yes	Yes	Yes	Yes
Industry×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes

payments, and fixed employment-related expenses. Again the two IVs generate very similar results. The results of the IV strategy in columns (3) and (5) suggest that a ten percentage point increase in firms receiving PPP is associated with a 4.9 (s.e. = 0.7) percentage point decline in the number of firms reporting missing any type of scheduled payments.

The Census survey data also reveal that the PPP funds increased firms' cash on hand. This exercise also offers a useful sanity check of the informativeness of the survey data. Similar to results on missed loan payments, the coefficients reported in column (1) of the bottom panel of Table 10 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 same relation using instead the state PPPE or predicted PPPE variables, which better isolate 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 IV regression, which uses state PPPE and predicted 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 3.2 to 3.8 percentage point increase in the share of firms reporting at least three months of cash to cover business operations.

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 the PPP did not immediately induce employment responses and only modestly increased employment in the months following PPP receipt.<sup>44</sup> Many businesses may have retained the PPP funds in bank accounts as precautionary savings until they were ready to resume activities, perhaps when demand for their goods and services return to normal or when relaxed shelter-in-place orders permit them to reopen for business. 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.

### 7.3. Crowd-out and business stealing

One potential mechanism explaining the small employment effects of the program is crowd-out. The risk of 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 crowd-out, the results suggest that magnitudes are small and private lending would not have fully offset PPP lending. The results are presented in Appendix E. This finding is plausible because loans to replace lost revenue would be unlikely to pass a private loan underwriting test.

Another possibility is that eligible firms might expand at the expense of local competitors. Such business stealing spillovers could account for low employment effects at the labor market level. Alternatively, the program might have positive local demand effects, for instance, on the suppliers of treated firms. Given the scale and severity of the labor market disruption due to the pandemic, traditional measures of labor market tightness are unlikely to be useful. However, we can ask whether regions with a larger share of employment in PPP-eligible establishments exhibit different effects relative to those with fewer eligible establishments. Appendix F presents split sample analyses estimating employment effects for regions based on the share of establishments that would be eligible for funds. Employment effects are generally similar or greater in regions where a larger share of establishments are eligible for funds, inconsistent with a business stealing effect and possibly consistent with the presence of some local demand effects.

### 7.4. 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 et al., 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.<sup>45</sup> While recent work such as Altonji et al. (2020) suggest a muted effect of UI extensions on unemployment levels and the speed of returning to work, we consider this possibility by 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. In Appendix F, we repeat our analyses of Tables 5 and 6 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, UI filings, and small business revenues, effect sizes are either similar or *greater* in high benefit states. It is important to note that, even in states with less generous UI systems, replacement 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.

## 8. Conclusion

This paper studies a large and novel small business support program that was part of the initial crisis response package, the Paycheck Protection Program (PPP). We focus on the role that banks played in intermediating PPP funds, the impact of bank performance on program targeting, and the overall short- and medium-term employment and local economic effects of the program.

We consider three 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? Third, why did some banks systematically under- or overperform in disbursing PPP loans relative to their share of the small business loan market? We find little evidence that funds were targeted toward geographic regions more severely affected by the pandemic. If anything, the opposite is true and funds were targeted toward 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. Ex ante bank characteristics, including greater labor capacity to process loans, pre-existing SBA relationships, and active enforcement actions against banks, predict banks' relative performance in disbursing PPP loans. Regions with higher exposure to banks that performed well saw higher levels of PPP lending and received funds

<sup>44</sup> Appendix F provides additional evidence that exposure to PPPE is associated with fewer permanent shutdowns in the Homebase sample. This evidence is consistent with the idea that despite modest employment effects, the program may have prevented firms from closing and this effect could manifest in stronger employment outcomes in the long-run.

<sup>45</sup> For example, the Wall Street Journal article "Businesses Struggle to Lure Workers Away From Unemployment" on May 8th (<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."

more quickly.<sup>46</sup> 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. It appears likely that many relatively healthy firms received funds and continued with their business as usual. 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 the relative importance of these responses is critical for evaluating the social insurance value of the PPP and similar policies, and designing them effectively. Because policymakers often rely on banks to deploy credit subsidies, it is important to understand what distortions in policy targeting are caused by the pre-existing structure of banking markets, as well as whether and how these distortions can undermine policy priorities. These issues are not just important for the COVID-19 pandemic and PPP, rather they are likely to re-emerge in the policy response to the next crisis or recession.

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### Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:[10.1016/j.jfineco.2022.05.006](https://doi.org/10.1016/j.jfineco.2022.05.006).

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