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Bank Incentives and the Effect of the Paycheck Protection Program

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Abstract:
We assess the role of banks in the Paycheck Protection Program (PPP), a large and unprecedented small-business support program instituted as a response to the COVID-19 crisis in the United States. In 2020, the PPP administered more than $525 billion in loans and grants to small businesses through the banking system. First, we provide empirical evidence of heterogeneity in the allocation of PPP loans. Firms that were larger and less affected by the COVID-19 crisis received loans earlier, even in a within-bank analysis. Second, we develop a model of PPP allocation through banks that is consistent with the data. We show that research designs based on bank or regional shocks in PPP disbursement, common in the empirical literature, cannot directly identify the overall effect of the program. Bank targeting implies that these designs can, at best, recover the effect of the PPP on a set of firms that is endogenous, changes over time, and is systematically different from the overall set of firms that ultimately receive PPP loans. We propose and implement a model-based method to estimate the overall effect of the program and find that the PPP saved 7.5 million jobs.

JEL Classifications: H81, G28, J21, E24
Keywords: Paycheck Protection Program, COVID-19, small business lending, financial frictions

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I. Introduction

The COVID-19 pandemic led to an unprecedented decrease in economic activity affecting small businesses in particular. In April 2020, revenues of small businesses decreased by more than 40 percent compared with January of the same year, and they were still down by 20 percent in August 2020. As a response, Congress created the novel Paycheck Protection Program (PPP) as part of the larger Coronavirus Aid, Relief, and Economic Security (CARES) Act. The program provided loans, which could turn into grants, with the goal of preserving jobs of small and medium businesses that were substantially affected by COVID-19. More than $525 billion was allocated through the program in 2020, which corresponds to approximately 60 percent of *all* interventions by the federal government in the 2008–2009 financial crisis. To speed up the delivery of loans to businesses, the government used financial institutions to make decisions on applications, but the loans were ultimately guaranteed by the government. Throughout the paper, we refer to these intermediaries as banks. In this paper, we estimate the overall effect of PPP on employment. To do so, we first analyze which firms and regions received loans at each moment in time during the program. We develop a model of PPP allocation that is consistent with our empirical results and discuss the implications of heterogeneous allocation of PPP loans for the estimation of the PPP effect on employment. Finally, we estimate the overall effect of the program.

We provide robust evidence of heterogeneity in the allocation of PPP loans. Firms in counties that had a smaller decrease in revenue, spending, or mobility, or a lower case rate of COVID-19 at the baseline received loans earlier. From a firm perspective, we show that larger firms or those in sectors less affected by the pandemic received loans earlier, even in a within-county-bank comparison. Our results reflect constraints in loan supply and not differential demand for PPP loans. We find that firms in sectors that were more affected by the pandemic were more likely to apply but significantly less likely to have a PPP loan approved in the beginning of the program. As more loans were made in the program, this heterogeneity in allocation was reversed. For instance, we find that firms in counties more affected by COVID-19 were more likely to have received a loan in 2020. Overall, our evidence shows that the set of firms and regions that received PPP loans is not comparable to the set of firms that did not and that these two sets of firms (and their difference) changed over the course of the program.

Next, we develop a model of the allocation of PPP loans that is consistent with our allocation results. Our theoretical framework has two agents: firms and banks. Each firm faces a random cost shock that must be paid for with their current cash-on-hand and potential borrowing from the PPP (as in Guerrieri et al. (2020)). Firms choose to apply for the PPP, and the bank chooses which applications to approve to maximize its profit. Banks are constrained in the volume of PPP loans they can make per period. Banks have outstanding loans with firms that will default if they do not survive the pandemic. As a result, banks allocate PPP loans earlier to firms that are larger and with which it has outstanding loans (outside of the program). Moreover, banks potentially lose clients if they reject their PPP applications and face uncertainty in the forgiveness process. Consequently, banks allocate loans earlier to firms with a higher likelihood of survival in the absence of PPP loans, that is, those that are larger and less affected by the crisis.

We explore the consequences of banks' incentives in the empirical estimation of the effect of the PPP. For that, we extend our model to allow for heterogeneous banks and regions. We evaluate both a naive estimation of the effect of the PPP that uses PPP disbursement as an independent variable (and is thus subject to the textbook form of selection bias) and one that uses an instrument correlated with PPP disbursement (for example, the bank-level disbursement shock of Granja et al. (2020)). We show that the coefficient of interest can be decomposed into three terms: the causal effect of the PPP and two terms we denote by *selection* and *targeting*. The selection term captures the correlation between the instrument and the likelihood of firm survival in the absence of PPP loans. The targeting term refers to the correlation between the instrument and the treatment effect in a region.

Our empirical analysis suggests that both selection and targeting are present in the data. First, we find that instruments typically used in the literature for the disbursement of the PPP loans are correlated with baseline county characteristics, such as the change in revenue, spending, or mobility after the pandemic started but before the PPP was implemented. Second, we find that, within a bank, firms with different treatment effects are targeted at different moments, which implies that an instrument correlated with PPP allocation will also mechanically be correlated with the heterogeneity in treatment effect.

From a broader perspective, our decomposition result is relevant for empirical work in the area for three reasons. First, conditioning on PPP applications is not sufficient to control for selection bias of the PPP program, and both the firm and regional regressions using PPP disbursements as an independent variable deliver biased results. Second, the biases using firm-level and regional-level variation are different, and it is even possible that firm-level results overestimate while regional-level variation underestimates the true effect of the program. Third, bank targeting of PPP loans implies that there is a relation between any instrumental variable that satisfies the inclusion restriction and the set of firms that receive PPP loans. As a consequence, the coefficient of interest does not capture the overall effect of the PPP, and its interpretation changes throughout the course of the program.

To provide a tighter analytical characterization of the effect of bank targeting, we explore theoretically a case involving an exogenous technological shock by which some banks disburse more PPP loans than others. Even in this case where we have a perfect instrument, using a research design based on firm or regional exposure to banks will not identify the *policy relevant treatment effect* (Heckman and Vytlacil, 2001), which in our setting is the treatment on the treated. Although this research design can estimate the causal effect of the PPP on some firms, the set of firms on which the effect is estimated is systematically different from the set of firms that receive PPP loans. We show that this bias is not fixed over time, and it is likely the case that the effect is overestimated at the beginning of the program and underestimated later on. In the applied micro lingo, the set of compliers is endogenous and changes over time. We argue that the apparently conflicting empirical results found in the literature can be broadly rationalized (and consistent with each other) within our framework.

We use our decomposition result to estimate the effect of the PPP on employment using county-level data. We set up a two-step M estimator (Wooldridge, 2010) where we allow treatment effects in a county to be heterogeneous based on PPP penetration in that county. We find that the PPP program was much more effective during the second round, where banks did not play a significant role in the allocation of scarce funds. We find that the effect of the PPP was to increase employment by approximately 12.5 percentage points for firms that received PPP loans. This corresponds to 7.5 million jobs at a cost of approximately \$70,000 per job.

Related Literature. This paper joins the growing literature exploring the economic impact of the policy response to the COVID-19 pandemic, in particular the impact of the PPP. Neilson, Humphries and Ulyssea (2020) focus on the informational differences among small and large firms in terms of PPP application and approval rates. Erel and Liebersohn (2020) show that there is a significant level of substitutability between traditional banks and fintechs in the PPP. Chodorow-Reich et al. (2020) study differences in liquidity provision to small and larger firms, showing how the PPP ameliorated liquidity shortfalls experienced by small and medium-sized enterprises (SMEs) that have reduced access to credit lines relative to larger firms. Bartlett and Morse (2020) focus on firm resiliency and labor flexibility. Autor et al. (2020) and Chetty et al. (2020*b*) use the 500-employee eligibility cutoff to run a difference-in-differences analysis at the firm level.

Closer to our analysis, Granja et al. (2020) and Doniger and Kay (2020) provide suggestive evidence of the regional targeting of PPP loans. Bartik et al. (2020) explore targeting of PPP loans at the firm level using a survey of small businesses. We provide comprehensive evidence of PPP targeting across regions, and, more importantly, we focus on within-county-bank targeting. We find similar significant targeting of PPP loans even when controlling for bank-county fixed effects, that is, comparing the allocation, across time, of the same bank-county pair.

Granja et al. (2020), Faulkender, Jackman and Miran (2021) and Doniger and Kay (2020) take a regional approach to estimate the effect of the PPP. Granja et al. (2020) and Faulkender, Jackman and Miran (2021) use regional exposure to banks as instruments for timing and allocation of PPP loans. Doniger and Kay (2020) use allocation of PPP loans around the 10-day window between the first and second rounds of the program as exogenous variation in PPP timing. We discuss the empirical findings of these papers in detail and compare them with our results at the end of Section VII.3. Our key contributions are to clarify what it is that each of these papers estimates, show that their results can be consistent with each other's (and that it is likely that none of the results estimates the overall effect of the PPP), and estimate the effect of the program on employment.

Our paper also contributes to the literature on loan guarantee programs, which are a common form of intervention in credit markets (Beck, Klapper and Mendoza, 2010). These programs are studied from a theoretical perspective (Gale (1990); Gale (1991)) and an empirical perspective (Lelarge, Sraer and Thesmar (2010); Mullins and Toro (2016); Brown and Earle (2017); de Blasio et al. (2018); Bachas, Kim and Yannelis (2020); Gonzalez-Uribe and Wang

(2019); Julien and Vallée (2020)). We contribute to this literature by assessing the role of financial intermediaries and their incentives in a large and novel loan guarantee program. More broadly, our analysis also contributes to the empirical debate on the effectiveness of public policies aiming to protect employment in downturns. We show that the PPP was successful is preserving 7.5 million jobs in small and medium-sized businesses despite the unprecedented shock these businesses faced in the COVID-19 crisis.

Finally, our paper contributes to the empirical analysis of policies involving selection into treatment, even in the presence of random or quasi-random variation. This issue is extensively explored in the applied micro literature (e.g., Cornelissen et al. (2016); Abadie and Cattaneo (2018); and Brinch, Mogstad and Wiswall (2017)), in particular in the estimation of returns to education. We build on this literature to focus on a setting where intermediaries choose which units will receive treatment and are capacity constrained, with this constraint shifting over time. Although we focus here on the PPP, various other settings share similar characteristics (as in the loan guarantee literature discussed above) and thus similar concerns in the interpretation of their reduced-form evidence.

II. The Paycheck Protection Program

Created on March 27, 2020, as part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, the Paycheck Protection Program (PPP) was designed to address liquidity shortages that could lead to employment losses from small businesses. The Small Business Administration (SBA) oversaw the program. To guarantee a timely disbursement of funds, firms applied for a loan through qualified financial intermediaries.

In 2020 and 2021, the PPP disbursed loans in two separate draws. The first draw ran from April 3 through August 8, 2020, and it is the one we consider in this paper.[1] Given the PPP's small-business focus, only firms with fewer than 500 employees were eligible to apply,[2] and each firm could apply for no more than one loan in the first draw of the program. The maximum loan amount was 2.5 times the firm's average monthly payroll costs in the

[1] In December 2020, Congress authorized an additional $284 billion in funding for the program as part of the $900 billion Coronavirus stimulus package. The PPP started making loans again in 2021, including second-draw loans for some of the firms that had received a PPP loan in the first draw.

[2] The exceptions were firms in the restaurant and hospitality sectors (NAICS code 72), which were allowed to apply as long as they had no more than 500 employees in each location.

preceding year, up to $10 million. PPP loans have an interest rate of 1 percent, deferred payments for six months, and maturity of two years for loans issued before June 5 and five years for loans issued after June 5, 2020. Moreover, PPP loans do not require collateral or personal guarantees.

A PPP loan is fully forgiven if funds are used for the specific purpose of payroll maintenance. Originally, to obtain full loan forgiveness, businesses were required to use at least 75 percent of the loan amount on payroll expenses and to maintain pre-crisis employment headcount and wage levels. This percentage was retroactively reduced to 60 percent after the Paycheck Protection Program Flexibility Act was passed in June 2020. The amount forgiven is reduced if wages or full-time headcount decreases. Initially, funds had be used to pay for these costs over the eight-week period following the disbursement of the loan. This period was eventually extended to 24 weeks in June 2020.

Each application was processed by financial intermediaries, for example, federally insured depository institutions and credit unions, which were responsible for checking documentation submitted by applicants. For simplicity, we refer to these intermediaries as banks. Banks were paid a fee by the government to cover these processing costs. Importantly, loans from the PPP are fully guaranteed by the government and carry zero risk weight for the calculation of risk-weighted assets, with the purpose of minimizing the impact on banks' capital requirements. Additionally, Federal Reserve Banks were authorized to provide liquidity to banks through the Paycheck Protection Program Lending Facility (PPPLF). This allowed Federal Reserve Banks to extend loans to institutions that were eligible to make PPP loans using such loans as collateral. Overall, the program was designed to allow a large number of institutions to process loan requests while minimizing impacts on their balance sheet structure.

Figure 1 shows the volume of PPP loans approved by date through August 8, 2020, the application deadline for the first draw of PPP loans. The first PPP loan was approved on April 3, 2020. The first draw of the PPP, which is the focus of this paper, was composed of two separate rounds. The first round of the program ran from April 3 to April 16, 2020. During the first round, PPP loan demand vastly exceeded supply. We see in Figure 2 that 72 percent of firms reported applying for the program, but only 36 percent reported receiving a PPP loan at the end of the first round. This excess loan demand gave banks a significant role in the allocation of PPP funds. As a consequence of the enormous demand for PPP loans, the program ran out of money on April 16, 2020, and there was a 10-day hiatus when no PPP loans were made. On April 24, Congress enacted the Paycheck Protection Program and Health Care Enhancement Act, which appropriated an additional $321 billion (for a total of $670 billion) for PPP loans. Banks resumed approving PPP applications on April 27. The second round of the program ran from April 27 to August 8, 2020. From April 27 to May 1, 2020, there was still a backlog of applications, and loans were being made at a fast pace. At this stage, demand for PPP loans still outpaced supply, and banks played some role in the overall allocation of PPP loans. After May 1, 2020, demand for PPP loans was more subdued, and we see excess supply of PPP loans, which reduced the role of banks in the allocation of PPP loans. This change in the role of banks from the beginning to the end of our sample is key to our empirical and theoretical analysis. The first draw of the program stopped accepting applications on August 8, 2020, with $144 billion remaining from the Paycheck Protection Program and Health Care Enhancement Act appropriation. In 2020, more than 5 million loans were granted for a total amount of approximately $526 billion.

III. Data

In this section, we briefly describe our main data sources and present some summary statistics of the PPP. For more details on our data sources and data set construction, see Appendix B.

Our main data source is the SBA/Treasury data on PPP loans (February 2021 version), which includes all loans made in the first draw of the program. The data set includes information self-reported by the borrower (name, address, Zip code, NAICS code, and jobs supported) as well as loan amount, approval date, and lender name. Throughout the paper, we use the PPP data at the loan level or aggregated at the bank-, county-, or county-bank level. To aggregate the data to the county level, we use the HUD Zip crosswalk to match each loan to a county (HUD (2020)).

For our analysis at the bank- and county-bank levels, we merge the lenders in the PPP release by name with those institutions that were active in 2020 and registered in the National Information Center database (which includes, among others, commercial banks and credit unions). We are able to match 94 percent of the number and 95 percent of the volume of

PPP loans (Table B.1). From the Call Reports, we obtain financial characteristics of all banks, the outstanding amount of small-business loans (overall), and the amount outstanding in the PPP program. Within the set of banks that file Call Reports, 846 out of 4,970 had no outstanding PPP loans in 2020Q2. To check the quality of our merge procedure, we compare the PPP volume from the SBA/Treasury release on June 30, 2020, with that from the Call Report in 2020Q2. We find that the two alternative measures of PPP disbursement by bank are very close to each other. The correlation between them is 0.99. Additionally, for banks that have a zero amount of PPP loans outstanding in the Call Reports, our procedure does not match any loans from the PPP loan-level data.

We use data from the FDIC's Summary of Deposits (2019) to construct the exposure of counties to community banks and to banks that under/overperform in the PPP. The Summary of Deposits (SoD) data contain the location of all branches (and deposit amounts) for all depository institutions that were operating in the United States in June 2019. We aggregate the data from the SoD at the county- and county-bank levels by summing the individual branch data. We follow the FDIC's institution directory to construct an indicator variable of community banks. To construct the instruments used in Granja et al. (2020) for PPP disbursement, we combine the matched version of the PPP release with the Call Reports and with the SoD. These strategies leverage the idea that small-business lending is local (e.g., Granja, Leuz and Rajan (2018), Li and Strahan (2020)), and thus lending at a given county can be instrumented by exposure to bank-level shocks.

We use a combination of the County Business Patterns (CBP 2020) and the Survey of U.S. Business (SUSB 2017) to compute the total number of employees at the county level (by firm size and per NAICS-2 Digits), the total number of eligible firms, and the total annual payroll of these firms. Together with the PPP release data at the county level, we use the CBP/SUSB data to approximate the amount of PPP lending relative to payroll and fraction of eligible firms receiving PPP loans in a region. With this approximation, we find that 73.77 percent of eligible firms received a PPP loan by the end of the second round of the program. This number is consistent with the Small Business Pulse Survey (SBPS, discussed in more details below), where 72.5 percent report receiving a loan through the program.

We use the high-frequency (daily) data from Chetty et al. (2020*a*) to obtain county-level measures of employment, revenue, spending, COVID-19 cases and deaths, mobility, and unemployment insurance claims. For details on the data collection, see Chetty et al. (2020*a*). Although Chetty et al. (2020*a*) argue that their employment data are representative for all firms in the United States, it is available for only 770 counties. Therefore, to increase our sample of counties with employment data, we use the monthly measure of employment and labor force participation at the county level from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS). Although they are not in our main sample, we also use data from other sources throughout the paper. From the Small Business Pulse Survey (SBPS), we obtain firms' self-reported effects of the pandemic and their expectations during the disbursement of the PPP program at the state-industry (NAICS 2 Digits) level.

In Table 1, we report aggregate statistics from the PPP microdata. The program funded 5.147 million loans at a total amount of $526 billion. At the end of the second round of the program, the (cumulative) average loan size was about $100,000 for firms that self-reported on average 11.8 jobs. Overall, more than 61.1 million jobs were reported by the firms that receive PPP loans (in a universe of 70 million jobs at firms eligible for the program—see Autor et al. (2020)). Most of these loans were not made by the top-four banks (in terms of 2019 assets). Together, these banks are responsible for around 36 percent of pre-pandemic small-business loans, but even at the end of the first two rounds of the program (which are covered by our sample) they made only 13 percent of the PPP loans. In the average county in the United States, about 72 percent of eligible firms received PPP loans and received eight weeks' worth of the preceding year's payroll. In Table A.1, we present the summary statistics of our county data at the baseline, that is, post COVID-19 but before businesses could apply for the PPP program.

IV. Heterogeneity in PPP Allocation

In this section, we explore the heterogeneity in the allocation of PPP loans. We have three main results. First, we show that loans were allocated initially to firms in counties that were less affected by the pandemic at the baseline. This trend reverts over time, and by the end of the second round of the program it was the opposite: Firms in the more affected counties at the baseline received more PPP loans. Second, we show that larger firms in sectors less affected by the pandemic received loans earlier within a given county-bank pair. Third, we argue that this heterogeneity in PPP allocation was not driven by differential demand for PPP loans.

IV.1. County

To understand why firms in certain counties were more likely to receive PPP loans, we run the following regression at the county level

$$Y_{r,t} = \gamma_{s,t} + \beta_t X_{r,0} + \varepsilon_{r,t}, \tag{1}$$

where $Y_{r,t}$ is the dependent variable at the county r, time t; $\gamma_{s,t}$ are state fixed effects; and $X_{0,r}$ represents a county's characteristics at the baseline (April 2, 2020). The outcome variables are the number of PPP loans per eligible firm and the volume of PPP loans relative to a week's worth of payroll of eligible firms, both cumulative. The baseline county characteristics are divided into two categories. First, we have the revenue, spending, and employment of small businesses (change from January 2020). Second, we have exposure to COVID-19 (cases) and the mobility index of time spent away from home (change from January 2020). We run this regression at the end of the first round (April 16, 2020), on the inflection day in PPP disbursement in the second round (May 1, 2020), and at the original deadline for applications (June 30, 2020), but our results are robust to any choice of date.

Our results of the estimation of Eq. (1) are in Table 2-Panel A. We weight our regressions by the number of employees in eligible firms in 2019 such that the results can be interpreted as the effect of the PPP on workers (as opposed to employment in the average county). Standard errors are clustered at the state level. Our results suggest that, within a given state, counties that receive more PPP loans early in the program are less affected by the pandemic. Workers in counties with a lower drop in revenue, spending, or mobility, or a lower case rate are more likely to work in firms that receive PPP loans in the first round. However, as more loans are disbursed in the program, we find the correlations between the first-round allocation and county characteristics either become insignificant (as in the case of COVID-19 cases) or change directions (as in the case of revenue, spending, mobility, etc.). Relative to their allocation at the end of the first round, firms in more affected counties are more likely to have received a PPP loan by June 30, 2020 (Column 3). We find somewhat similar patterns for the relative allocation of the volume of loans (Columns 4 through 6). In terms of magnitudes, we find, for instance, that an increase of one standard deviation in county revenue (s.d. = 0.12) is associated with a 0.1 standard deviation increase in the share of eligible firms that received a PPP loan by April 16 (s.d. = 0.1). Note, however, that since our independent variables are measured at the county level, these regressions likely underestimate the amount of PPP targeting at the firm level.

Since by mid-May there is no longer an excessive demand for PPP loans, our results in Table 2-Panel A can be driven by differences in application rates. To test this channel, we focus now on the *timing* of PPP disbursement (that is, controlling for the ultimate allocation of PPP in a given county). We run Eq. (1) with the share of loans (or share of funds) allocated up to a certain date. That is, given the total amount of PPP demand in a county at the end of the second round of the program, what share of it was disbursed by a given date? The results are in Table 2-Panel B. On April 16, we find the results are similar to those in Table 2, namely that firms in less affected counties are more likely to receive their loans sooner. For instance, we find that a one standard deviation decrease in revenue (s.d. = 0.12) at the baseline is associated with a 0.18 standard deviation reduction in the share of loans by April 16 (s.d. = 0.12). On May 15, however, firms in less affected counties still receive a higher share of their total amount of PPP loans relative to other counties in the same state. This indicates that our results in Columns 2 and 3 of Table 2-Panel A likely come from differences in demand. We will discuss the issue of loan supply versus demand in more detail at the end of this section.

IV.2. Within-County-Bank Allocation

As shown consistently in the PPP literature (and here in Section V), PPP disbursements are strongly related to community bank status and bank size. Thus, if banks of different sizes have different physical footprints across counties, the results in Table 2 can be due simply to county exposure to banks. Therefore, we evaluate whether firms that receive PPP loans earlier are similar to those that receive loans later within a given bank-county pair. To test for within-county-bank heterogeneous allocation of PPP loans, we first run the following regression

$$Y_{b,r,t} = \zeta_{b,r} + \sum_{t \neq \text{Apr–16}} \beta_t + \sum_{t \neq \text{Apr–16}} \beta_t^{CB} \times CB_b + \varepsilon_{b,t} \tag{2}$$

where $Y_{r,b,t}$ is the average number of employees and average loan size (cumulative) at the county r, bank b, and time t level; $\zeta_{b,r}$ are bank-county fixed effects, and CB_b is a community bank indicator variable. With bank-county fixed effects, our variation comes from the different timing of PPP loan allocation for firms in different moments in time but for the same county-bank pair. We include the community bank status interacted with the time dummies to understand if the within-bank-county changes are heterogeneous across different banks. Standard errors are two-way clustered at the county and bank levels.

We report the results of the estimation of Eq.(2) in Figure 3. Loans made in the first two days of the program are, on average, almost \$200,000 larger than loans made at the end of the second round of the program for non-community banks and about \$125,000 for community banks. Note that since our dependent variables are cumulative, loans made in the first week enter into the calculation at every week of our analysis. We opt for cumulative to show that not only is the average loan size is falling, but that the total volume of smaller loans is still relevant to bringing the cumulative average down.

In Table 3, we show that this result, where the size of the loan and number of employees in firms receiving PPP loans are decreasing as more loans are made by each bank, is not only present over time but also within a given moment in time across counties; that is, as a bank makes more loans in a county, the size of these loans and number of employees in the firms receiving the loans decrease. For that, we first compute the share of loans made by bank b at county r by time t relative to the number of loans this bank makes in this county over the course of the program, $share_{b,r,t}$. We then estimate Eq. (2) with bank-time and county-time fixed effects; that is,

$$Y_{b,r,t} = \zeta_{b,r} + \gamma_{b,t} + \delta_{r,t} + \beta share_{b,r,t} + \varepsilon_{b,t} \tag{3}$$

In Eq. (2), the only variations we use are within-bank-time and county-time. We find that in counties where a 10 percentage points higher share of loans is made by a given moment in time, the average firm receiving this loan will have approximately 1.2 fewer employees. This indicates that banks provide PPP loans for the largest firms in a county and, as they make more loans, the average size of the firms receiving these loans decreases significantly.

Our final set of results of heterogeneous allocation leverages the loan-level data from the

SBA/PPP release. We run the following probability model at different t's

$$L_{b,r,t} = \zeta_{b,r} + \beta D_s + \gamma \log(\text{employees} + 1) + \varepsilon_{b,r,t} \quad (4)$$

where $L_{b,r,t}$ is an indicator if a loan was made up to time t, $\zeta_{b,r}$ are county-bank fixed effects, D_s is the share of firms in sector s (NAICS 2-Digits) that report a decrease in their revenues in the Small Business Pulse Survey, and $\log(\text{employees} + 1)$ is the log of employees reported by the firm plus one to account for the owner in firms with zero employees. Our parameter of interest is β, that is, the conditional relation of the sectoral effect of COVID-19 and the likelihood of obtaining a PPP loan. At each time t we run this model, we include two types of loans: those made up to time t (April 16, May 1, May 15) and those not made until after May 15. The question we want to answer is, compared with firms that received loans after May 15, how different are the firms that received loans up to a date t (and, in particular, how differently affected are their sectors)?

Our results are in Table 4. We show that firms in sectors where a larger set of firms reports a revenue decrease are less likely to receive PPP loans. For instance, firms in the utilities sector (where 45 percent report a decrease in revenue) are 5.8 percentage points more likely to receive a PPP loan compared with firms in the food and accommodation sector (where 74.6 percent report a decrease in revenue) in the first round of the program relative to loans made after May 15. When comparing only loans made after May, however, firms in the utilities sector are only 2.5 percentage points more likely to receive a PPP loan by May 15 compared with firms in the food and accommodation sector.

Causality. Our results in this section are not necessarily causal. For instance, our regressions do not imply that more PPP loans went earlier to counties *because* their revenue was relatively higher at the baseline. What we show is that there is a significant heterogeneity in the allocation of PPP loans across counties and firms, and that this heterogeneity is related to deep economic factors. These factors are very likely linked to both (i) employment in the absence of PPP and (ii) the effect of the PPP on employment. We will show in our model that these two correlations are indispensable in correctly interpreting the reduced-form evidence of the effect of the PPP and estimating the overall effect of the program.

Supply vs Demand of PPP Loans. One potential concern with our results is that they are all driven by differential demand of PPP loans by firms, rather than by banks' choices. A closer look at the evidence reveals that this is not the case. First, we see from Figure 2 that the application rate for the PPP is the same throughout the duration of the program (that is, most firms that applied for the program did so early on). This is consistent with the evidence of Neilson, Humphries and Ulyssea (2020), which shows that differences in application rates among firms dissipate a few days after the program starts disbursing loans. Second, we show that, if anything, the most affected firms apply earlier and in larger numbers. Using the Small Business Pulse Survey, we show that industries in which a larger share of firms report having negative revenue effects from COVID-19 are associated with a higher application rate but a lower receipt rate in the PPP program (Table A.2). Moreover, in our county-level analysis, we find that the share of eligible firms with a PPP loan on May 15 is larger in more affected counties. However, once we control for the total demand for PPP loans in that county by focusing on PPP timing, we find that this result disappears; that is, demand for PPP loans is higher overall in the most affected counties. Third, we can look at the survey data from Bartik et al. (2020). Bartik et al. (2020) uses a survey conducted by Alignable, a network of small business with more than 5 million members. The survey was conducted from April 25 to 27, 2020 (that is, between the first and second rounds of the program). The data show that firms more affected by the pandemic and smaller firms are more likely to apply but less likely to receive PPP loans. Moreover, they show that the firms with a preexisting loan and those with high cash on hand are also more likely to receive PPP loans conditional on application. The survey-based results are consistent with ours derived from observational data.

V. Bank Heterogeneity in PPP Disbursement and Potential Instruments for PPP Allocation

Our evidence in Section IV shows that PPP loan approvals are endogenous at the firm and regional levels. To deal with this endogeneity, the literature proposes several instruments, which we discuss empirically in this section. Most of the instruments used in the literature are related to firm and county heterogeneous exposure to different banks. Bartik et al. (2020) use a firm's primary bank size as an instrument. Faulkender, Jackman and Miran (2021) use the market share of community banks in a given county as an instrument for the timing of

PPP allocations across different counties. Granja et al. (2020) use the gap between the market share of a bank in PPP lending in the first round and its pre-pandemic small-business lending as a measure of a bank-level PPP shock, as in Eq.(5)

$$PPPE_b = \frac{\text{Share PPP}_b - \text{Share SBL}_b}{\text{Share PPP}_b + \text{Share SBL}_b} \times 0.5 \tag{5}$$

where Share PPP_b is the share of PPP lending from bank b at the end of the first round of the program, and Share SBL_b is the share of bank b lending in small-business loans in 2019Q4. Similar to Faulkender, Jackman and Miran (2021), Granja et al. (2020) construct their regional instrument as a Bartik instrument using county exposure to banks' disbursement shocks in the PPP. In this paper, we follow Granja et al. (2020) and construct these instruments based on the share of branches of banks in a given county.[3] We plot community bank status and bank $PPPE_b$ as a function of assets in Figure 4 for both the number and volume of loans for PPP lenders that we can match to the Call Reports.

The idea behind these instruments is that some banks faced an exogenous PPP disbursement shock, and thus firms where these banks are relatively over-represented received PPP loans more quickly. The research design is akin to a Bartik instrument. The identification assumption is that pre-PPP county exposure to banks is not correlated with employment changes, conditional on observables. To evaluate the plausibility of the identifying assumptions of this design, we follow Goldsmith-Pinkham, Sorkin and Swift (2020).[4] We explore the relation between regional bank exposure and location characteristics that may be correlated with employment shocks during the pandemic. This relation sheds light on the mechanisms that may be problematic for the exclusion restriction.

Our results are in Table 5. We normalize both the dependent and independent variables such that the coefficients can be interpreted as a one standard deviation effect of one variable in terms of the standard deviation of the other. We find that counties with higher exposure to community banks or banks with a high $PPPE_b$ are those that are less affected by the pandemic and have a relatively larger share of larger firms. This indicates that there are

[3]Our results are nearly identical if we instead use the deposit shares.

[4]Note that one also cannot use the shocks view of Borusyak, Hull and Jaravel (2018) for identification in our settings. From Figure 4, it is clear that the shocks at the bank level are not random, as they are essentially a function of bank size.

other channels through which an early disbursement of PPP loans is related to PPP survival. Although the empirical literature is, in general, concerned with selection bias, that is, that employment shocks can be correlated with community bank exposure of $PPPE$, we show in our model that bank targeting introduces a change in the interpretation of the coefficient of interest in the case of treatment effect heterogeneity. For instance, take the case of firm-size heterogeneity and the share of branches from community banks. Counties with a higher share of community banks also have a higher share of firms with 100 to 500 employees. In our setting, if larger firms are more likely to receive PPP loans early and *either* (1) are more likely to survive without a PPP loan (and this is not controlled for) [5] or (2) have a lower treatment effect upon receiving a PPP loan, a bank shock research design analysis won't identify the effect of the PPP on firms that receive PPP loans.

A notable exception is the instrument for PPP timing used in Doniger and Kay (2020). Doniger and Kay (2020) use the share of loans made right after the beginning of the second round (April 26 to 28) over those made from April 14 to 16 and April 26 to 28. The idea behind using this variation comes from the idea that counties receiving loans right at the end of the first round are comparable to those that receive loans at the beginning of the second round, and thus their difference in employment can be attributed to the 10-day delay in PPP disbursement. Although the share of loans delayed is correlated with some county characteristics (such as firm size), the main challenge of using this identification strategy is the interpretation of the results as the overall effect of the PPP. Intuitively, due to bank targeting, the set of compliers in this window is not random and likely corresponds to firms with the highest treatment effects. We discuss this issue of endogenous compliers in detail when we present our model in Section VI.

[5] It is hard to argue one can control for the probability of survival convincingly. If one could estimate this probability at the firm level, then the estimation of the effect of the PPP would be a trivial endeavor.

VI. A Model of Banks' Incentives and the Implications for the Estimation of the Effect of the PPP

In this section, we construct a model to understand how banks' incentives affect the estimation of the effect of the PPP. [6] Our model features two main ingredients: firms and banks. Firms are heterogeneous in their size, financial conditions, amount of outstanding debt, and firm-level shocks (in the case of the PPP, the effect of the pandemic on this firm). Banks observe the size, financial conditions, amount of outstanding debt of these firms, and the parameters of the distribution of the firm-level shock, but not the actual realization of the shock. Firms choose to apply for the PPP, and banks choose which applications to approve subject to a constraint on the volume of credit that can be approved each period. Our model features the rich bank and regional heterogeneity we observe in the data.

We show that there are two main channels that can affect the estimation of the effect of the PPP, both consistent with the empirical evidence in the previous sections. First, we show that there is still the potential for a selection bias to be present using bank or regional shocks in PPP disbursement. Second, in our model, as in the data, banks target firms (i) with a higher probability of survival without a PPP loan and (ii) with more outstanding debt. As a result, there is an endogenous correlation between the treatment effect, the probability of survival without PPP, and the allocation of PPP loans. We show that this correlation implies that reduced-form estimates from a research design leveraging bank or regional shocks cannot be directly interpreted as the effect of the program. We show that this is true even under the assumption that there is an exogenous technological shock at the bank level that affects PPP disbursement (for example, the ability to process loans through the program).

Finally, our model provides a simple decomposition of the reduced-form parameters in terms of the causal effect of the program, selection and targeting. We take this decomposition to the data in Section VII.

[6] The basic set-up of the model is the same as in our companion paper on the optimal allocation of PPP funds from the perspective of the government (Joaquim and Netto (2021)).

VI.1. Firms and Banks

Firms. We consider a continuum of mass one of firms indexed by j. Each firm has N_j workers. We will define our model in terms of *per-worker* variables. Firm j's cash on hand per worker before the pandemic and the lending program is given by Eq. (6)

$$c_j \equiv \rho_j - b_j \tag{6}$$

where b_j is its debt payments per worker and ρ_j is the remainder of the cash on hand. Without loss of generality, we normalize N_j such that $\int_j N_j dj = 1$. We assume that applying for the PPP has a fixed cost of F, and firms either choose to apply ($a_j = 1$) or not ($a_j = 0$) for the program. When applying for the PPP, each firm chooses to apply for the maximum amount subject to a program limit based on the firm's current employment level of φN_j. This loan has a cost of $r_G < 0$ (that is, it is at least partially a grant from the firm's point of view). [7]

We model the pandemic shock following Guerrieri et al. (2020). Each firm faces a reduction $\nu_j N_j$ in cash flows (revenue shortfalls, extra costs to remain open). The per-worker magnitude of the shock is ν_j, with c.d.f. denoted by Φ and p.d.f. ϕ parametrized by η_j (we define the specific functional form for the distribution below). A firm that borrows ω_j from the lending program *can* survive the pandemic if

$$\nu_j < c_j + \omega_j \equiv \Gamma_j$$

where Γ_j corresponds to the available funds per employee to guarantee firm survival. A firm that borrows ω_j from the lending program *wants* to survive the pandemic if

$$\nu_j < c_j - r_G \omega_j + \pi_j^{LR} \equiv \Pi_j$$

where π_j^{LR} is the perpetuity value of long-run profits of the firm and Π_j is the total profit of the firm (both per employee). We assume that all firms that *can* survive *want* to survive—that

[7] The decision to apply for the maximum allowed can be microfounded as an optimal decision from the point of view of the firm given that the implicit rate on PPP loans is negative. For details, see Joaquim and Netto (2021).

is, $\Gamma_j < \Pi_j$, $\forall j$.[8]

For tractability, we follow Guerrieri et al. (2020) and assume that the c.d.f. of the fixed-cost shock distribution is given by Eq. (7)

$$\Phi(\nu\,;\,\eta) = \begin{cases} 0, \text{ if } \nu < 0 \\ \left(\frac{\nu}{c_0}\right)^{\eta}, \text{ if } \nu \leq c_0 \\ 1, \text{ if } \nu > c_0 \end{cases}, \text{ with } \eta > 0 \tag{7}$$

The distribution in Eq. (7) is such that the shock to a firm with a higher η first-order stochastically dominates a distribution with lower η, making it easier to compare more affected (higher η) and least affected (lower η) firms.

Let $\Phi_j(\omega) \equiv \Phi\left(\Gamma_j(\omega)\,;\,\eta_j\right)$, which is the probability that a firm survives the pandemic if it receives ω from the program. The objective function of the firm can be written as Eq. (8). The expected profit is given by the probability of survival multiplied by the expected profit, conditional on survival, minus the application cost:

$$\max_{a\in\{0,1\}} \underbrace{\Phi_j(a\varphi)}_{\text{Prob. Survival}} \cdot \underbrace{\left[\Pi_j(a\varphi) - \mathbb{E}\left(\nu_j \mid \nu_j \leq \Gamma_j(a\varphi)\right)\right]}_{\text{Expected Profit}} - aF \tag{8}$$

To simplify the notation and speak directly to our empirical analysis, we define θ_j as the probability of survival of firm j without assistance and T_j as the treatment effect for firms of type j receiving φ in the PPP

$$\theta_j \equiv \Phi_j(0) \tag{9}$$

$$T_j \equiv \Phi_j(\varphi) - \Phi_j(0) \tag{10}$$

A firm applies for the program if the benefits of applying are larger than the fixed cost F. Therefore, firms with more workers (N_j) apply more often, since they have a smaller per-worker cost of applying. All else being equal, a firm j is more likely to apply for the program if $(c_j + \pi_j^{LR})T_j$ is higher, that is, if the *increase* in expected profits is higher. However,

[8] Lending programs are designed as a short-term source of finance for these firms, such that it is expected that $\pi_j^{LR} > (1 + r_G)\varphi$.

applying for the PPP increases the expected cost to be paid in terms of survival, that is, $\mathbb{E}\left(\nu_j \mid \nu_j \leq \Gamma_j(\varphi)\right) > \mathbb{E}\left(\nu_j \mid \nu_j \leq \Gamma_j(0)\right)$ and the loan will have to be repaid at r_G. Putting this all together, firms that apply for the PPP are those that satisfy Eq. (11), which, using our specific distribution, can be written as Eq. (12).

Firm's Choice in the PPP. *Firms apply for the PPP ($a_j^* = 1$) if:*

$$T_j \Pi_j(0) - T_j \mathbb{E}\left[\nu_j \mid \nu_j \in [\Gamma_j(0), \Gamma_j(\varphi)]\right] - (T_j + \theta_j) r_G \varphi > \frac{F}{N_j} \tag{11}$$

For the distribution in Eq. (12)

$$\left[\frac{1}{\eta_j + 1} c_j + \pi_j^{LR}\right] T_j - (T_j + \theta_j)\left(\frac{\eta_j}{\eta_j + 1} + r_G\right)\varphi > \frac{F}{N_j} \tag{12}$$

Firms, Workers, and Aggregation. For simplicity, when aggregating a variable x_j across firms, even those of different sizes, we will use the notation $\int x_j dj$. However, it is useful to spell out what is behind this aggregation and how we consider firms of different sizes in our model. Let $G(\rho, b, \eta, N)$ be the joint distribution of ρ, b, η, N in the population of firms. For any variable at the firm level that is not a function of the number of employees, $x(\rho, b, \eta)$, we can write:

$$\int N x(\rho, b, \eta) dG(\rho, b, \eta, N) = \int x(\rho, b, \eta) \overline{N}(\rho, b, \eta) dG(\rho, b, \eta) \tag{13}$$

where $\overline{N}(\rho, b, \eta) \equiv \int_N N dG(N \mid \rho, b, \eta)$. The term $\overline{N}$ is the average number of employees of firms of a given type $\{\rho, b, \eta\}$, and it acts in our model as a shifter in the distribution of firms of type $\{\rho, b, \eta\}$. What matters in our model is not the marginal distribution of firms, but rather the marginal distribution of the firm variables at the *job* level. Thus, our model also speaks to empirical results based on jobs (and not firms).[9] In our model, neither the treatment effect T_j nor the probability of survival without PPP, θ_j, is a function of N_j conditional on c_j and η_j; that is, large firms have different probabilities of survival because they are affected differentially by the pandemic or exhibit different liquidity conditions, and not simply because they are large.

Changes in the treatment effect and probability of survival over time. Given the massive

[9]For instance, see the evidence in Bartlett and Morse (2020) of how firm survival varies by firm size.

size of the PPP program, our empirical strategy and theoretical analysis leverages not only *who* received PPP loans, but also *when* those loans were received. To account for fluctuations over time of the probability of survival without PPP loans and the treatment effect of the PPP (due to, for instance, the re-opening of the economy, other assistance programs, etc.), we assume that there are functions α_θ, α_T such that

$$\theta_{j,t} = \theta_j + \alpha_\theta(t) \quad \text{and} \quad T_{j,r} = T_j + \alpha_T(t) \tag{14}$$

with $\alpha_T(t)$ decreasing over time. For simplicity, we consider that t is discrete (days or weeks in our particular setting). As most firms apply very early in the program (Figure 2), we suppose that an application is based on the baseline θ_j and T_j.

Banks. Banks can choose to accept or reject applications from firms to maximize their profits. For now, we focus on the problem of a single bank. We develop a model with multiple banks and regions in Section VI.2.

Banks receive positive profits from making more loans and thus will make as many loans as possible in the program. If the bank accepts a PPP application, there are two possible scenarios. If the firm survives, the bank recovers b_j of the current loan payments and a present value of $\psi_F b_j$ of potential future loans to this firm. If the firm does not survive, the bank receives a share $\delta \in (0,1)$ of the current payments and no potential future loans to this firm. The same two scenarios are possible when the bank rejects the PPP application. However, we additionally assume that if the firm survives after having its application denied, there is a probability $\psi_C < 1$ that the firm switches bank providers.[10] Additionally, to incorporate potential uncertainty regarding loan guarantees, we assume that with probability q the bank has to face the costs of the PPP loan, a concern for some banks in the pandemic.[11]

Let $l^B_{j,t} \in [0,1]$ be the choice of a bank to approve the application of a firm at time t. At a

[10]See, for instance, Peter Rudegeair, "When Their PPP Loans Didn't Come Through these Businesses Broke Up with Their Banks," *The Wall Street Journal*, July 31, 2020. https://www.wsj.com/articles/when-their-ppp-loans-didnt-come-through-these-businesses-broke-up-with-their-banks-11596205736.

[11]For instance, on March 31, 2020, the Treasury and the SBA released guidelines for lenders, including one that said banks would need to verify some of the borrower's information for the loan to be eligible for forgiveness. See Zachary Warmbrodt, "Banks Warn of Chaotic Launch of Small Business Lending Program," *Politico*, April 2, 2020. https://www.politico.com/news/2020/04/02/banks-small-business-lending-program-launch-161106. These guidelines were eventually reviewed several times, inducing even more uncertainty for lenders.

given period t, the profit Π_j^B per worker of firm j a bank receives is

$$\begin{aligned}\Pi_{j,t}^B \equiv & \left\{\Phi_{j,t}^\Gamma(\varphi)[1+\psi_F] + \left[1-\Phi_{j,t}^\Gamma(\varphi)\right]\left(\delta - q\frac{\varphi}{b_j}\right)\right\} b_j l_j^B + \\ & \left\{\Phi_{j,t}^\Gamma(0)[1+(1-\psi_C)\psi_F] + \left[1-\Phi_{j,t}^\Gamma(0)\right]\delta(1+\psi_F)\right\} b_j(1-l_j^B) \\ = & \ \Omega_{j,t} + \text{ constant}\end{aligned}$$

where

$$\Omega_{j,t} \equiv T_{j,t}\left[(1-\delta)b_j + \psi_F b_j + q\varphi\right] + \theta_{j,t}\left[\psi_C\psi_F b_j + q\varphi\right] \tag{15}$$

Let $\mathcal{A} \equiv \left\{j \mid a_j^* = 1\right\}$ be the set of firms that apply for the program at a given bank, M_t be the amount a bank can lend up to time t, and B_t be the set of firms that did not get a PPP loan until time t, that is

$$B_t = \left\{ j \,\middle|\, j \in \mathcal{A} \text{ and } \sum_t^{t-1} l_{j,t}^B = 0 \right\} \tag{16}$$

The problem of the bank at time t is given by Eq. (17)[12]

$$\max_{\{l_{j,t}^B \in [0,1]\}} \int_{B_t} N_j \Omega_{j,t} l_j^B \, dj \quad \text{s.t.} \quad \int_{B_t} l_j^B \, dj = \varphi^{-1}(M_t - M_{t-1}) \tag{17}$$

where dj in this case represents the integration of all firms of types $\{b_j, \rho_j, \eta_j\}_j$, with the cumulative distribution of type j given by $G(\rho, b, \eta) \times \overline{N}(\rho, b, \eta)$, that is, the job weighted distribution, as in Eq. (13). As in our model, the treatment effect T_j is not a function of N_j (conditional on c and η), and the optimal choice of loan size in the program is linear in N_j. The problem of the bank will also not be an explicit function of N_j.

Given the bank problem in Eq. (17), we describe the within-bank implications of our model for early versus late recipients of PPP loans. It is clear that banks approve loans for firms with a higher probability of survival without a PPP loan, θ_j, a higher treatment effect T_j, and more debt payments b_j to be made. The question is which firms are those in our model, as η_j affects θ_j and T_j simultaneously. We show in Lemma 1 that, all else being equal,

[12] Note that here we assume implicitly that the bank does a period-by-period optimization and does not take into account at time t the expected future dynamics of $T_{j,t}$, $\theta_{j,t}$, and M_t. We believe this is a reasonable assumption in our case, given the uncertainty at the time that most of the loans were disbursed (April and May 2020).

banks prefer to allocate loans to more indebted firms and to firms intermediately exposed to the pandemic (that is, the bank profit function is hump shaped in η).

Intuitively, banks already have heterogeneous exposure to firms with which they have outstanding loans, which is captured by $(1-\delta+\psi_F)b_j$, and want to keep their most profitable clients from moving to a competitor, which is captured by $\psi_C\psi_F b_j$. Everything else being equal, this implies that banks allocate loans to firms with higher levels of outstanding debt earlier. Moreover, banks are also concerned about the probability of survival of the firm θ_j, since those are firms that are more likely to become clients and pay back the PPP loan in case of regulatory uncertainty. This induces banks to allocate loans toward firms with a lower η. However, banks are also interested in the extent to which firm j is more likely to survive if given a PPP loan, T_j. Firms with a high treatment effect are those for which a bank can increase the chance of survival at the margin and thus reap the benefits of the relationship in the future. In our model, T_j is hump shaped in η: Firms that are too affected by the pandemic will likely not survive anyway, while firms that are not affected at all don't have as much use for PPP funds. We show that these channels together imply that Ω_j is also hump shaped in η_j, and thus, *a priori*, banks prefer to allocate loans to intermediately affected firms. Note that this statement depends on the set of firms that do apply. For instance, if the firms that apply are already those with a higher η (Eq. (11)), then it is possible the empirically relevant support of η is such that banks prefer to allocate loans to firms less affected by the pandemic.

Lemma 1. ***Within-Bank PPP Targeting. Early versus Late Recipients*** *Let t_j denote the date a firm receives a PPP loan, with $t_j=\infty$ if this firm doesn't receive one.*

- ***Debt heterogeneity.*** *Suppose all firms in the economy have the same $c_j=c, \eta_j=\eta$. Let $j,\tilde{j}$ be such that $b_j > b_{\tilde{j}}$, but $c_j=c_{\tilde{j}}$ and $\eta_j=\eta_{\tilde{j}}$. The solution to Eq. (17) implies that $t_j \leq t_{\tilde{j}}$.*

- ***Shock Exposure Heterogeneity.*** *Suppose all firms in the economy have the same $c_j=c$. The solution to Eq. (17) implies that $\exists!\ \underline{\eta}(t),\overline{\eta}(t)$, such that the bank chooses $t_j \leq t$ iff $\eta_j \in [\underline{\eta}(t),\overline{\eta}(t)]$, where $\underline{\eta}(t)$ is decreasing, and $\overline{\eta}(t)$ is increasing in t.*

The two results in Lemma 1 are consistent with the evidence in Bartik et al. (2020) and the evidence from Section IV.2. In terms of pre-pandemic debt, Bartik et al. (2020), using survey data, find that conditional on the set of firms with banking relationships (at the extensive margin), banks approved more loans from firms with higher pre-existing debt at what the authors call a "striking magnitude." In terms of shock exposure, we show in Table 2 (county) and Table 4 (county-bank) that PPP loans are allocated to those less affected by the pandemic. This is consistent with our result in Lemma 1, assuming that most firms have a high value of η; that is, they were significantly affected by the pandemic. In the SBPS data, for instance, 78.5 percent of firms report a large/moderate negative effect of the pandemic on August 15, 2020, with only 15.5 percent reporting a small effect and 5.8 percent a positive effect.

Finally, we see in Figure 3 that banks allocate loans earlier to larger firms. Although our model is written in per-employee terms, such that firm size does not enter directly in b_j, T_j, or θ_j, it still is consistent with heterogeneous allocation for firms of different sizes.[13] For instance, according to the Small Business Credit Survey from the Federal Reserve, firms with 50 to 499 employees are twice as likely to have used banks in the past five years compared with firms with one to four employees, such that targeting of firms with more debt will also induce targeting of larger firms. Similarly, Bartlett and Morse (2020) show that both the ability to survive without the PPP and the effect of the program are heterogeneous across firm size.

VI.2. Bank and Regional Heterogeneity

In this section, we extend our previous model to account for bank and regional heterogeneity. For that, we need to introduce some additional notation. We discuss broadly the potential sources of bias in the reduced-form estimation of the effect of the PPP. We then discuss in detail the case where the only difference across banks is a technological shock in their capacity to process PPP loans and show that even in this case, we can't recover the overall effect of the PPP.

VI.2.1 Setting

To extend our model to a more general setting with bank and regional heterogeneity, we need to introduce additional notation. Suppose that we have banks $b = 1, ..., B$. A bank b

[13] An alternative model in which banks face a constraint based on the number of loans, and not volume, as we have here, would deliver the prediction that banks would choose larger firms first ceteris paribus. This alternative model is also consistent with the data, and, more importantly, it carries all of the same implications in the following sections.

has a market share $\mu_{b,r}$ in region $r = 1, ..., R$. Each region has a weight W_r, $\sum_r W_r = 1$. Let the market share of bank b be given by $\mu_b \equiv \sum_r W_r \mu_{b,r}$. We model the bank-level shock as a shifter $s_{b,t} > 0$ of the expected volume of small-business loans this bank makes. That is, if the market share of bank b in small-business loans is given by μ_b, we assume that bank b makes a share $s_{b,t}\mu_b$ of PPP loans in time t, and we scale $s_{b,t}$ to be such that $\sum_b s_{b,t}\mu_b = 1$, $\forall t$.

We assume for simplicity that each firm has only one bank. Let B_j denote this bank for firm j, and r_j the region of this firm. The regional instrument we construct for PPP is given by the shift-share instrument s_r and is the baseline market-share weighted average of $s_{b,t}$ until time $\bar{t}$; that is

$$s_r \equiv \sum_t^{\bar{t}} \sum_b \mu_{b,r} s_{b,t} \tag{18}$$

In our setting, s_r thus plays the role of the potential instruments we discussed in Section V—the share of deposits held at community banks, the share of loans delayed, PPPE, etc. Let the expected probability of survival of firms that use bank b in region r be given by $\theta_{b,r} \equiv \mathbb{E}\left[\theta_{j,t}|b,r\right]$ and the average treatment effect of these firms by $T_{b,r} \equiv \mathbb{E}[T_{j,t}|PPP_j = 1, b, r]$. For all variables $X_{b,r}$ denoted at the bank-regional level, let $X_r \equiv \sum_b \mu_{b,r} X_{b,r}$ and $X_b \equiv \sum_r W_r \frac{\mu_{b,r}}{\mu_b} X_{b,r}$ be these variables aggregated at, respectively, the regional and bank levels using the appropriate weights.

We generalize the problem of the bank in Eq. (17) and simply assume that when bank b makes a PPP loan to firm j in region r, it has an expected profit given by $\Omega(T_j, \theta_j, b_j, \zeta_b, \zeta_r)$. This encompasses the problem of the bank we considered before, but with two key generalizations. First, it adds the possibility that all of the parameters (ψ_C, ψ_F, δ) vary at both the bank level and the regional level. Therefore, this takes into account banks that have different abilities to recover collateral (different δ's), capitalize clients in the future (different ψ_F's), etc. Second, it clarifies which ingredients are needed for our results of this section to be valid, namely the dependence of Ω_j on T_j and θ_j, rather than the micro-foundations of these terms.

Let $G_{b,r}$ be the distribution of the types of firms bank b faces in region r. The general version of the problem of bank b we consider is given by maximizing Eq. (19) s.t. Eq. (20) by choosing a probability of accepting a PPP application $l_{j,r,t} \in [0,1]$ from firm j in region r at time t.

$$\max_{\{l^b_{j,r,t}\}_{j,r}} \sum_r \mu_{b,r} W_r \int_j l^b_{j,r,t} \Omega(T_{j,t}, \theta_{j,t}, b_j, \zeta_b, \zeta_r)\, dG_{b,r}(j) \tag{19}$$

$$\sum_r \mu_{b,r} W_r \int_{j \in B_{b,r,t}} l^b_{j,r,t} dG_{b,r}(j) = s_{b,t} \mu_b \frac{M_t - M_{t-1}}{\varphi} \tag{20}$$

where M_t is the total amount dispersed in the program up to time t and $B_{b,r,t}$ is the bank-region-specific analog of B_t in Eq. (16), that is, the set of firms that bank with b in region r at time t that applied but did not receive a PPP loan yet.

Finally, consistent with our empirical analysis, we assume that our true model of the effect of the PPP at either the firm level or job level can be written as Eq. (21)

$$y_{j,t} = \alpha_j + \sum_t \gamma_t D_t \Xi_r + \mathbb{1}_{t \geq 0} \left[\theta_{j,t} + T_{j,t} PPP_{j,t} \right] + \varepsilon_{j,t} \tag{21}$$

where $y_{j,t}$ is a dummy that is equal to 1 if the firm/job survives up to time t, α_j is a firm fixed effect, D_t is a dummy for time t, Ξ_r are regional and time controls (state-time fixed effects, industry composition, etc.), $t = 0$ is the beginning of the pandemic (March 2020), $PPP_{j,t}$ is a dummy equal to 1 if firm j receives PPP loans by time t (including those firms that do not apply), and ε_j is a true idiosyncratic shock.

At the regional level, we assume that our model can be written as an aggregation of individual firm-level effects, that is,

$$y_{r,t} = \alpha_r + \gamma_{s,t} + \xi_t \Xi_r + \mathbb{1}_{t \geq 0} \left[\theta_{r,t} + T_{r,t} PPP_{r,t} \right] + \varepsilon_{r,t} \tag{22}$$

where $y_{r,t}$ is the share of firms/jobs that survive in region r, α_r are regional fixed effects, $\gamma_{s,t}$ are state-time fixed effects, Ξ_r are our controls, $\theta_{r,t}$ is the average probability of survival without intervention, $T_{r,t}$ is the regional ATT, $PPP_{r,t}$ is the share of eligible firms that receive PPP loans, and $\varepsilon_{r,t}$ is the regional-level shock. From a modeling standpoint, the restriction we impose on Eqs. (21) and (22) is that the controls $\Xi_{r,t}$ enter the equation in an additive fashion and that there is this firm/regional-level idiosyncratic shock affecting survival. The $\theta_{j,t} + T_{j,t} PPP_{j,t}$ term comes from our definitions of θ and T.

Counterfactual of Interest—Policy Relevant Treatment Effect. Our ultimate goal is to estimate the overall effect of the PPP. The counterfactual we want to be able to compute is how many jobs would have been lost in the absence of the program (or if it were significantly smaller). This is the policy relevant treatment effect (PRTE) defined in Heckman and Vytlacil (2001). In our setting, the PRTE is equal to the ATT. In our economy, we have that the ATT at time t is defined as

$$ATT_t \equiv \mathbb{E}[T_j|PPP_{j,t} = 1] = \frac{\mathbb{E}_w\left[T_{r,t}PPP_{r,t}\right]}{\mathbb{E}_w\left[PPP_{r,t}\right]} \tag{23}$$

where $\mathbb{E}_w(.)$ is the expectation using the size W_r of each region as weights.

VI.2.2 Reduced-Form Estimation and Potential Sources of Bias

Our focus in this section is to understand the mechanisms through which banks' incentives affect the estimation of the effect of the PPP. We analyze the differences between the overall effect of the program (measured by the ATT) and the asymptotic mean of a standard reduced-form estimator. For that, we consider the estimation of Eq. (22) either through a naive regression with PPP disbursement on the right-hand side or by using a potential instrumental variable. We focus on the estimation directly including the potential instrument in the second stage to relate directly to the empirical literature (e.g., Granja et al. (2020) and Doniger and Kay (2020)), but the results can be easily extended to an IV estimation.[14] For analytical tractability, we assume that except for their PPP exposure, there is no time-invariant heterogeneity across firms ($\alpha_j = 0$ in Eq. 21) or regions ($\alpha_r = 0$ in Eq. 22).[15]

Firm Level. We first consider the estimation of the effect of the PPP at the firm level. Consider the estimation of Eq. (24) by OLS

$$y_{j,r,t} = \gamma_{s,t} + \xi_t \Xi_r + \sum_t \beta_{F,t} D_t PPP_{j,t} + \epsilon_{j,r,t} \tag{24}$$

where $\gamma_{s,t}$ are state-time fixed effects, Ξ_r are regional controls, D_t is a dummy that is 1 at time t, and $PPP_{j,t}$ is a dummy equal to 1 if this firm received a PPP loan up to time t. Our true underlying model of the effect at the firm level is given by Eq. (21) with the additional

[14] See Appendix C.5 for a particular case.

[15] We show in Section VII that this assumption does not affect the equations we present here for the regression using the potential instruments on the right-hand side, but it simplifies the analysis of the naive regression.

assumption that $\alpha_j = 0$ for tractability. The OLS estimator $\hat{\beta}^{Naive}_{F,t}$ in Eq. (24) converges to[16]

$$\hat{\beta}^{Naive}_{F,t} \xrightarrow[p]{} ATT_t + \frac{\mathbb{E}\left[\theta_j \dot{PPP}_{j,t}\right]}{\mathbb{V}\left[\dot{PPP}_{j,t}\right]} \tag{25}$$

where $\dot{PPP}_{j,t}$ represents the residuals of a regression of $PPP_{j,t}$ on our controls $\gamma_t \Xi_r$. The bias arising from the naive regression is the well-known selection bias: Without a PPP loan (θ_j), firms that receive PPP would have a probability of survival that is different from that of firms that don't receive PPP (conditional on our controls), and thus the correlation between θ_j and $PPP_{j,t}$ biases the estimation of the ATT. Note that it is not sufficient in our setting to condition the sample on firms that apply for the PPP to eliminate the selection bias. Even conditional on applying, banks target firms based on θ_j for early receipt of PPP loans. As a consequence, PPP and θ_j are correlated within the subset of firms that apply for the program. This targeting channel is consistent with the empirical evidence in Bartik et al. (2020), who show that banks allocate PPP loans to firms less affected by the pandemic and with more cash on hand (even when those firms apply at lower rates). Similarly, we show in Table 4 that firms receiving PPP loans are less likely to be from a sector negatively affected by the pandemic.

Given the potential bias we highlight in Eq. (25), the literature proposes a myriad of instruments to bypass the selection problem (Section V) that correlates with the early allocation of PPP loans. Let $\mathcal{I}_j$ be one of these instruments (for instance, a dummy if this firm has a relationship with a community bank or if the firm is in a county with a large community bank share, as in Faulkender, Jackman and Miran (2021)). Consider the estimation of Eq. (26) at the firm-level by OLS

$$y_{j,r,t} = \gamma_{s,t} + \xi_t \Xi_r + \sum_t \beta_{F,t} D_t \mathcal{I}_j + \epsilon_{j,r,t} \tag{26}$$

where $\gamma_{s,t}$ are state–time fixed effects, Ξ_r are regional controls, and D_t is a dummy for time t.

We show in Lemma 2 that using a potential instrument $\mathcal{I}_j$ changes the nature of the selection problem and introduces a new potential source of bias in the estimation of the ATT.

Lemma 2. *Consider the OLS estimation of Eq. (26) when our true model is given by Eq. (22). Let*

[16]This result is a direct application of the FWL theorem. See Appendix C.3 for details.

$\dot{x}_{j,t}$ represent the residual of a regression of $x_{j,t}$ on $\gamma_{s,t} + \xi_t \Xi_r$. Then, for $t \geq 0$

$$\hat{\beta}_{F,t} \xrightarrow[p]{} ATT_t \times \beta_{1,t} + \underbrace{\frac{\mathbb{E}\left[\theta_j \dot{\mathcal{I}}_j\right]}{\mathbb{V}(\dot{\mathcal{I}}_j)}}_{\textit{Selection}} + \underbrace{\frac{\mathbb{E}\left[\left(T_{j,t} - ATT_t\right) PPP_{j,t} \dot{\mathcal{I}}_j\right]}{\mathbb{V}_w(\dot{\mathcal{I}}_j)}}_{\textit{Targeting}} \tag{27}$$

where $\beta_{1,t}$ is the coefficient in the first stage; that is,

$$PPP_{j,t} = \beta_{1,t} \dot{\mathcal{I}}_j + \epsilon_{j,t}^{1st} \tag{28}$$

For concreteness, let $\mathcal{I}_j$ be the share of branches from community banks in the county that firm j is in. In our naive regression, the selection bias came from the allocation of PPP loans (beyond what was expected) to firms with a different probability of survival without PPP loans, θ_j. In Lemma 2, the potential selection bias comes from the correlation between θ_j and the share of branches from community banks in a given county. For instance, we show in Table 5 that the share of community banks in a county is positively related to revenue, spending, and mobility (before the PPP program), and it is negatively correlated with COVID-19 cases, deaths, and firm size. These correlations suggest that the share of branches as an instrument does not satisfy the exclusion restriction: Counties with a large share of branches from community banks would have had a different trajectory for employment beyond the effect on PPP disbursement. Alternatively, if large banks, which empirically have lower s_b's (Figure 4), also have a higher application cost F_b, these banks will face a consistently different pool of firms—those with higher treatment effects and lower probabilities of survival without PPP loans (Eq. 11).

The targeting term of Lemma 2 comes from the correlation of T_j and the PPP allocation. These correlations can come from two main channels. First, since banks target firms based on their treatment effects, firms that do receive PPP loans are different from those that don't (even within bank or within county). This correlation can still exist if banks are heterogeneous only in their PPP disbursement s_b, and s_b is a perfect instrument—which we explore in detail in the next section. Intuitively, as a bank makes more loans, it eventually lends to a different set of firms. Therefore, the set of firms that receive loans and that would have received loans were they clients of other banks (that is, the compliers) changes over time and is always different from the overall set of firms that receive PPP loans. Second, another source of bias is bank heterogeneity in PPP targeting. For instance, if non-community banks are also those that invest less in banking relationships and risk losing more clients (higher ψ_C), they will systematically choose firms with even lower treatment effects than community banks, but with a higher probability of survival without a PPP loan. This channel is consistent with the evidence that community banks engage in less targeting based on firm size (Table 3), but community banks are also those with the highest values of s_b (Figure 4). Heterogeneous PPP targeting induces a systematic correlation between the share of branches from community banks and the outcome y_j that is not through PPP allocation, which leads to the targeting term in Lemma 2 being different from zero.

Regional Level. At the regional level, we have a result analogous to that of Lemma 2. The selection channels come from the fact that places with more firms that are likely to survive will be targeted by banks and thus will receive more PPP loans. The targeting channel comes from the relationship between the ATT in a region r, $T_{r,t}$ and the share of firms in that region that receive PPP loans, $PPP_{r,t}$. This relationship comes from two distinct components: Places with a given distribution of treatment effects will endogenously receive more $PPP_{r,t}$ loans from multi-market banks, and as banks expand lending in a given region, T_r also changes (as banks select different types of firms). Intuitively, the difference between the firm bias and regional bias comes from the within- and across-market targeting and selection. The regional estimation has both components (for example, banks target firms in different markets and, within a market, different firms), while the firm-level estimation with region-time fixed effects suffers only from the within-region biases.

Lemma 2 at the Regional Level. *Consider the OLS estimation of Eq. (26), weighted by region size W_r, when our true model is given by Eq. (22). Let $\dot{x}_{r,t}$ represent the residual of a regression of $x_{r,t}$ on $\gamma_{s,t} + \xi_t \Xi_r$. Then, for $t \geq 0$*

$$\hat{\beta}_{R,t} \underset{p}{\longrightarrow} ATT_t \times \beta_{1,t} + \underbrace{\frac{\mathbb{E}_w\left[\theta_r \dot{\mathcal{I}}_r\right]}{\mathbb{V}_w(\dot{\mathcal{I}}_r)}}_{\text{Selection}} + \underbrace{\frac{\mathbb{E}_w\left[(T_{r,t} - ATT_t) PPP_{r,t} \dot{\mathcal{I}}_r\right]}{\mathbb{V}_w(\dot{\mathcal{I}}_r)}}_{\text{Targeting}} \tag{29}$$

where $\mathbb{E}_w$ is the expectation using W_r as weights, and $\beta_{1,t}$ is the coefficient in the weighted first *stage; that is,*

$$PPP_{r,t} = \beta_{1,t}\dot{\mathcal{I}}_r + \epsilon_{r,t}^{1st} \tag{30}$$

VI.3. Exogenous Bank PPP Capacity

In this section, we explore the role of bank disbursement heterogeneity as the only source of variation in PPP allocation and firm survival. We abstract from the evidence that disbursement is correlated with other bank characteristics, such as size, liquidity, and community bank status. We assume in this section that the bank-level shock s_b is strictly exogenous, that is, a perfect instrument. The bank-level shock is not correlated with any bank, region, or firm observable or unobservable and does not vary over time. Apart from this heterogeneity in s_b, all banks have the same market shares and face the same distribution of firms, and all regions are identical. We also abstract from changes in treatment effects or the probability of survival over time.

Our goal is to characterize the role of bank capacity for disbursing PPP loans on our empirical estimates. Our main result is that an empirical strategy that leverages this exogenous variation at the bank level can estimate a causal effect of the PPP, but it will not estimate the overall effect of the program. The intuition behind this result is that banks endogenously choose the set of compliers (those firms for which we can estimate the causal effect of the PPP), which implies that there is a systematic difference between the LATE and the ATT. Under some conditions, we show theoretically that the effect of the program across firms that received PPP loans is overestimated in the beginning of the program and underestimated at the end of it.

Simplified Setting. Given the simplifying assumptions in this section, we depart from our general formulation and assume that the true effect of the PPP is given by

$$y_j = \theta_j + T_j PPP_j + \varepsilon_j \tag{31}$$

We run the following regression using bank s_{b_j} as an instrument for the PPP loan allocation[17]

$$y_j = \beta_0 + \beta_{F,IV} PPP_j + \epsilon_j \tag{32}$$

using the approval rate of the bank that firm j uses, s_{b_j}, as an instrument for PPP_j. In this section, the problem of the bank with heterogeneous disbursement can be simplified to

$$\max_{\{l_{j,t}^b\}} \int_j l_{j,t}^b \, \Omega(T_j, \theta_j, b_j) dG(j) \tag{33}$$

subject to

$$\int_{j \in B_{b,t}} l_{j,t}^b dG(j) = s_b \mu_b \frac{M_t - M_{t-1}}{\varphi} \tag{34}$$

where we simplify the generalized problem in Eq. (19) to leave s_b as the source of bank heterogeneity. As in Lemma 1, the problem of the bank is linear in $\Omega(.)$, and thus the allocation will be given such that firm j receives a loan at $t_j \le t$ if, and only if, $\Omega(T_j, \theta_j, b_j) > \underline{\Omega}_{b,t}$, where $\underline{\Omega}_{b,t}$ solves

$$\int_j \mathbb{1}_{\Omega_t(T_j, \theta_j, b_j) > \underline{\Omega}_{b,t}} dG(j) = \mathbb{P}[\Omega(\theta_j, T_j, b_j) > \underline{\Omega}_{b,t}] = s_b \mu_b \frac{M_t}{\varphi} \tag{35}$$

Therefore, a higher s_b implies a lower $\underline{\Omega}_{b,t}$, $\forall t$, which expands the set of firms j such that $\Omega(\theta_j, T_j, b_j) > \underline{\Omega}_{b,t}$. A heterogeneous PPP capacity s_b will thus endogenously create a relationship between s_b and the treatment effect of firms that receive PPP from this bank. Thus, an empirical strategy that uses s_b as an instrument will not deliver the effect of the PPP on the firms that did receive it. We state this result formally in Lemma 3.

Lemma 3. ***Heterogenous PPP capacity and bank IV.*** *Even when the only source of heterogeneity across banks is the technological PPP capacity shock s_b, estimating Eq. (32) with s_{b_j} as an instrument for PPP_j will almost surely lead to an incorrect estimation of the effect of the program; that is, $\beta_{F,IV} \underset{p}{\not\longrightarrow} ATT$.*[18]

If the shock s_b is in fact purely technological, as we assume in this section, why are we not able to estimate the effect of the program using a bank-IV strategy? As shown in Angrist,

[17] Although we present here our results from the firm-level regressions, all of the results are unchanged at the regional level using s_r as an instrument.

[18] Similarly, for regions that are identical except for their exposure to each bank (heterogeneous $\mu_{b,r}$, which is what guarantees a strong first stage, as empirically observed), we have $\beta_{R,IV} \underset{p}{\not\longrightarrow} ATT$.

Graddy and Imbens (2000) for the continuous IV case, this bank-IV strategy at the firm level can recover an average causal response when s_b is strictly exogenous. This causal response is a weighted average of the treatment effects of firms that would potentially have a different PPP allocation if they were clients of different banks. In our setting, as banks choose which firms they lend to based on their treatment effects, this set of compliers is systematically different from the set of always and never takers in terms of the treatment effect, such that the average treatment effect is always different from the average causal response. In the following section, we characterize the direction of the bias when firms are heterogeneous only in their shock exposure. In Appendix D, we focus on the two–bank case to make the comparison between ATT and LATE explicit.

VI.3.1 Heterogeneous Shock Exposure

In this section, we provide a tighter characterization of the difference between the average causal response and the ATT when banks are heterogeneous only in s_b under additional assumptions. Our additional assumptions are (i) that firms are heterogeneous only in their shock exposure η and, consistent with the empirical evidence, (ii) that the support of η is such that:

1. Banks allocate loans first to the firms less affected by the shock.

2. Banks do not allocate loans first to the firms with the highest treatment effects. For instance, banks allocate loans first to larger firms (Figure 3), which have the lowest treatment effects (e.g., Bartlett and Morse (2020), Neilson, Humphries and Ulyssea (2020)).

In the proof of Lemma 1, we show that both T_j and Ω_j are hump shaped in η_j, with the max of Ω_j being at a lower η compared with the max of T_j, as in Figure 5. For conditions 1 and 2 to be valid, we have that the support of η is such that Ω_j is decreasing—banks want to allocate loans to firms less affected by the pandemic—but T_j is increasing—banks do not allocate loans first to the firms with the highest treatment effects. Let $\underline{\eta}_{\mathcal{E}}$ be this empirically founded minimum of the support for η, where

$$\eta_T^* \equiv \arg\max_{\eta_j} T_j > \underline{\eta}_{\mathcal{E}} > \arg\max_{\eta_j} \Omega_j \tag{36}$$

as shown in Figure 5.

Lemma 4 shows that if firms are heterogeneous only in their shock exposure and $\eta_j > \underline{\eta}_{\mathcal{E}}$, an identification strategy that leverages the exogenous bank variation will initially overestimate and eventually underestimate the effect of the program as more loans are disbursed. At any moment in time, the exogenous variation at the bank level can recover an average treatment effect for firms that would have a different PPP allocation if they were clients of different banks. At a time t_0 at the early stages of the program, a bank that makes more loans selects firms with higher treatment effects on average compared with banks that make fewer loans (where $t_j \leq t_0$). On the other hand, when a sufficiently large amount of loans has been made (at $t_1 > t_0$), the bank that makes more loans selects firms with lower treatment effects on average (where $t_j \leq t_1$). In Appendix D, we provide a version of Lemma 4 with two banks that allows for an explicit characterization of the LATE. We show that $\beta_{IV,t} = LATE_t$ and that $LATE_t$ is higher or lower than the ATT_t based on the difference in treatment effects between banks.

Lemma 4. ***Heterogeneous Shock Exposure and PPP Effect Estimation.*** *Suppose that time is continuous; $t > 0$ and that firms are all equal except for their shock exposure η_j; banks are heterogeneous only in their capacity to disburse PPP loans, s_b; and that $\eta_j > \underline{\eta}_{\mathcal{E}}$. Let $\beta_{IV,t}$ be the parameter in a period-by-period version of Eq. (32) with s_{b_j} as an instrument for $PPP_{j,t}$. Then, $\exists! t^*$ such that*[19]

$$t < t^* \Rightarrow \beta_{IV,t} > ATT_t \text{ and } t > t^* \Rightarrow \beta_{IV,t} < ATT_t$$

The result in Lemma 4 is relevant for the interpretation of the PPP evidence. First, it shows that empirical studies with different time windows are bound to find different coefficients *even if the underlying effect of the PPP did not change*. Second, it also shows that the bias can change its sign as the program progresses, and we don't know *a priori* if researchers are finding a lower or upper bound for the effects of the program. The interpretation of the empirical evidence becomes particularly challenging when the overall effect of the program is itself changing over time. In Section VII, we use the structure of our model and our decomposition in Lemma 2 to estimate both the ATT_t and the targeting and selection biases we

[19] It is possible that $t^* = \infty$; that is, the $\beta_{IV,t}$ is always higher than the ATT in case firms above η_T^* never receive PPP loans. However, this is unlikely given that the program reached almost 75 percent of all small businesses in the United States.

discussed in this section.

VII. The Effect of PPP on Employment

In this section, we estimate the effect of PPP on employment. First, we show the results of a naive OLS regression with PPP or early allocation of PPP loans as the dependent variable. Next, we discuss our method for uncovering the treatment effect of the policy in the presence of bank targeting. Finally, we present the results of our estimation procedure and discuss how they fit in the literature.

VII.1. Reduced-Form Estimation

Our naive approach to estimating the effect of the PPP on employment in county r in state s at time t is to run

$$\text{Nonemployment}_{r,t} = \alpha_r + \gamma_{s,t} + \xi_t \Xi_r + \sum_t \beta_t P_{r,t} + \epsilon_{r,t} \tag{37}$$

where $\text{Nonemployment}_{r,t}$ is our measure of nonemployment constructed from the BLS Local Area Unemployment Statistics (monthly,) the rest of the terms are defined as before, and $P_{r,t}$ is some measure of the PPP at county t, either the simultaneous PPP allocation, $PPP^n_{r,t}$, or the share of eligible firms in a county with PPP loans by the end of the first round, $PPP^n_{r,Apr--16}$.

Our results are in Figure 6.[20] The version of our results that uses $PPP_{r,t}$ on the right-hand side shows that there is an insignificant correlation between PPP allocation and employment (Panel A). This correlation is negative at first and positive later on, which is consistent with the dynamic selection bias we highlight in the PPP allocation in Section IV. Using $PPP^n_{r,Apr--16}$ on the right-hand side (Panel B), we find a significant negative association between nonemployment post-PPP and a high share of PPP loans in the first round. In terms of magnitude, we find that a one standard deviation change in $PPP^n_{r,Apr--16}$ (approximately 0.1) is associated with nonemployment that is 1.7 percentage points lower in August and 1.2 percentage points lower in December.

[20] The results are similar when basing the independent variables on the volume, rather than the number, of PPP loans. They are available upon request.

We also estimate Eq. (37) using the instruments we discussed in Section V. The results are consistent with our simple measure of early allocation of PPP loans. The results of the first stage are in Figure A.1, and the effect on employment is shown in Figure A.2. In what follows, we use the estimation of Eq. (37) with $s_r = PPP^n_{r,Apr--16}$ as the independent variable as our benchmark case. In our estimation method, we explicitly take into account that early allocation of PPP loans (as the other potential instruments) is endogenous.

VII.2. Model Implied Estimation Method

Our estimation method is based on our theoretical model and our decomposition of Lemma 2 at the regional level. According to our decomposition in Lemma 2, there are two terms that prevent us from interpreting the reduced-form evidence as the average treatment effect. First, there is a correlation that comes from the endogeneity of s_r and the probability of survival of firms absent the PPP. We show empirically that this correlation is present using either early PPP allocation or the other instruments used in the literature. Second, there is the targeting effect. This term does not come from the instrument not satisfying the exclusion restriction, but rather from the structural relationship underlying PPP disbursement and treatment effects due to bank targeting. The idea of our estimation method is to parameterize the non-ATT terms on the right-hand side of the decomposition in terms of observables in a way that is consistent with our model.

First, we assume that θ_r is correlated with early PPP allocation, in particular, let $\alpha_\theta \equiv \frac{\mathrm{Cov}_w(\theta_r, PPP_{r,s_r})}{\mathbb{V}_w(s_r)}$. Second, our model shows that due to within- and across-market targeting, the treatment effect in a region depends on the amount of PPP in that region. We assume that the treatment effect in a given region is given by

$$T_{r,t} = f_t(PPP^n_{r,t}) + \varepsilon_{r,T} \tag{38}$$

for some function $f(.)$, where $\mathbb{E}[\varepsilon_{r,T} \mid PPP_{r,t}] = 0$ and $\mathbb{E}[\varepsilon_{r,T}\dot{s}_r \mid PPP_{r,t}] = 0$. Our identifying assumption is that conditional on $PPP_{r,t}$, s_r is not correlated with the treatment effect in region r. This assumption is different from the identifying assumption used to estimate the ATT from Eq. (37) with s_r as an independent variable. In Eq. (37), the identifying assumption for the ATT is that s_r is an instrument for PPP on employment and thus is not correlated with the error term or the heterogeneity in treatment effects. Here, we can still have s_r correlated with the heterogeneity in treatment effects, however, only through its effects on $PPP^n_{r,t}$. This identifying assumption is valid in our simplified setting of Section VI.3, where we assume that banks are heterogeneous only in their PPP shock, s_b. To see that, note that conditional on $PPP_{r,t}$, s_r is no longer correlated with T_r. More broadly, this assumption is satisfied in our regional analysis whenever banks within a region have similar levels of s_b.

Let $\zeta \equiv \{\alpha_\theta, \alpha_T, \beta_T, \gamma_T\}$ be the vector of our structural parameters. We parameterize the function $f_t(PPP^n_{r,t} \mid \zeta) = \alpha_T + \beta_T \times t + \gamma_T PPP^n_{r,t}$. We estimate ζ through a two-stage M-estimator. Let

$$\boldsymbol{\delta}_t \equiv \left\{1, \frac{\text{Cov}(PPP_{r,t}, \dot{s}_r)}{\mathbb{V}_w(\dot{s}_r)}, t \times \frac{\text{Cov}(PPP_{r,t}, \dot{s}_r)}{\mathbb{V}_w(\dot{s}_r)}, \frac{\text{Cov}(PPP^2_{r,t}, \dot{s}_r)}{\mathbb{V}_w(\dot{s}_r)}\right\}$$

and $\hat{\boldsymbol{\delta}}$ the sample analogue for $\boldsymbol{\delta}$ (first stage). Our estimator for $\hat{\zeta}$ is given by

$$\hat{\zeta} \equiv \arg\min_{\zeta} \sum_r W_r \left[y_{r,t} - \beta(\zeta, \hat{\delta})\dot{s}_r\right]^2 \tag{39}$$

where $y_{r,t}$ is nonemployment in county r at time t, $\dot{s}_r$ is the residual of early allocation of PPP loans on state-time fixed effects and industry-exposure-time controls, and $\beta(\zeta, \boldsymbol{\delta}_t) \equiv \zeta \boldsymbol{\delta}_t'$. From an estimate $\hat{\zeta}$, our ATT estimator is given by

$$\widehat{ATT}_t = \frac{\sum_r W_r f(PPP_{r,t} \mid \hat{\zeta}) PPP_{r,t}}{\sum_r W_r PPP_{r,t}} \tag{40}$$

We compute the standard errors for $\widehat{ATT}_t$ through 1,000 bootstrap repetitions.

Results. Our results are in Figures 7 and 8. Panel A of Figure 7 reports the reduced-form estimate of $\hat{\beta}$ from Eq. (37) versus the model implied $\beta(\hat{\zeta}, \hat{\boldsymbol{\delta}}_t)$. Although our functional form for the dependence of $T_{r,t}$ on $PPP_{r,t}$ is parsimonious, our implied $\beta(\hat{\zeta}, \hat{\boldsymbol{\delta}}_t)$ is similar to (and within the confidence bands of) our reduced-form $\hat{\beta}_t$. This indicates that a setting with a simple structure between θ_r and s_r, and $T_{r,t}$ and $PPP_{r,t}$ can generate the empirical patterns we actually observe in the data.

Panel B of Figure 7 reports our decomposition of $\beta(\hat{\zeta}, \hat{\boldsymbol{\delta}}_t)$ in terms of selection, targeting, and ATT (times the first-stage coefficient) from Lemma 2 at the regional level. First, note that, consistent with our empirical evidence, we find there is a positive relation between the probability of survival without PPP and early allocation of PPP loans (relative to our controls). This positive relation, however, is quantitatively small. Second, we see that the targeting term behaves exactly as predicted by our theory. At the start of the program, banks target firms with relatively low treatment effects, such that the correlation between s_r and T_r is negative at this point.[21] After the first round, however, we find that this correlation between s_r and T_r increases, which indicates that the new loans being made after the first round feature banks targeting firms with relatively high treatment effects. This result is consistent with our theory of non-monotone targeting on treatment effects (Lemma 1 and Lemma 4).

Figure 8 plots the implied ATT from Eq. (40). Note that in our estimation method, we do not constrain $ATT \leq 0$, but we do not find any ATT's significantly different from zero. We find a small value of the ATT in the first round of the program. Our targeting results indicate that banks are selecting firms with low treatment effects, so this result is not surprising. Concurrently, at this stage we have that the LATE is likely larger than the ATT, and therefore, it is consistent with our setting that other papers find some effect of the PPP. After the start of the second round, we find a large increase in the overall effectiveness of the program. At this stage, banks target firms with the highest treatment effect. This indicates that the second round of the program was much more effective than the first in increasing overall employment. We provide a welfare analysis based on this ATT estimate in Joaquim and Netto (2021).

VII.3. Cost per Job and Comparison with the Literature

We compute the overall effect from the PPP following the procedure in Autor et al. (2020). The total effect is given by

$$\text{Total Employment Effect}_t = ATT_t \times \gamma \times N$$

where ATT_t is our estimate of the ATT, γ is the fraction of the eligible population that receives a PPP loan (the take-up rate), and N is the number of employees at PPP-eligible firms. We

[21] Note here that as our dependent variable is nonemployment, all of the T_r's are negative, and a more negative T_r implies a larger treatment effect.

use N = 70 million and $\gamma = 61/70 = .87$ as the share of employees in eligible firms with a PPP loan by August 8, 2020. Our ATT estimate implies that the overall effect of the PPP was to reduce nonemployment by 7.5 million by the end of the second round of the program on August 8, 2020. This translates into a cost per job saved of approximately $70,000. The advantage of our estimate compared with the empirical literature's estimates is that ours considers who the set of compliers is at each moment in time. This allows us to translate the reduced-form estimates into the policy-relevant treatment effect.

Overall, our estimate of the cost per loan of the PPP is lower than most of those in the empirical literature. Chetty et al. (2020*a*) and Autor et al. (2020) find a cost per job saved of about $280,000. The caveat in the interpretation of their results is that their identification strategy leverages the 500-employee eligibility cutoff, and thus it applies only to the largest firms eligible for the program.

Granja et al. (2020), employing an approach similar to ours, use regional exposure to banks to estimate the effect of the program and estimate a cost per job of approximately $136,000. The authors use data from Homebase, a software company that provides free scheduling, payroll reporting, and other services to small businesses. There are three main problems in the interpretation and aggregation of their results. First, the dependent variables they use are business shutdowns (measured by businesses with zero hours worked in a given week) and hours worked. However, one of the objectives of the program is exactly to allow firms to temporarily close their businesses and reduce hours. Second, the sample from Homebase is far from representative. Homebase's customers are primarily small firms in food and drink, retail, and other sectors that employ hourly workers. Finally, Granja et al. (2020) do not take into account the potential selection and targeting mechanisms we highlight in this paper. Our framework predicts that in the presence of an unobserved characteristic that determines firm treatment, firms with relatively low treatment effects are likely to receive PPP loans early (Figure 5), suggesting that the LATE in Granja et al. (2020) underestimates the overall effect of the PPP.

Doniger and Kay (2020) leverage the fact that the PPP program did not approve any loans from April 16 through 26 to identify the effects of the PPP. The idea is that around this 10-day window the timing variation on PPP disbursement is as good as random. As our analysis shows that even if this is a valid strategy to identify the LATE of the program, it is likely to be very far from the overall effect of the program for all firms. Firms that receive loans around the 10-day window are not similar to those that do not. For instance, we show that these firms are smaller than firms that receive PPP loans earlier (Figure 3) but larger than those that receive PPP loans later. Second, our framework predicts that in the presence of an unobserved characteristic that determines firm treatment, firms with the highest treatment effects are likely to receive PPP loans exactly in the middle of the program (Figure 5), suggesting that the LATE in Doniger and Kay (2020) overestimates the overall effect of the PPP. Consistent with this view, the authors find a cost per job of approximately $43,000.

One important caveat from all of these numbers, including ours, is that they do not account for general equilibrium effects. Given the size of the program, this is expected. Methods that combine detailed firm data (such as those of Autor et al. (2020)) and our regional approach have the potential to disentangle these GE effects (for instance, using the techniques in Mian, Sarto and Sufi (2019)). We leave this topic to be explored in future research.

VIII. Conclusion

As a response to the COVID-19 crisis, the US government created the PPP to preserve jobs in small and medium-sized businesses. In 2020, the program disbursed more than $525 billion in loans and grants. To guarantee a timely delivery of loans to businesses, the program was intermediated by banks. In this paper, we explore how banks' incentives affected the disbursement of PPP loans and the impact of the PPP on employment.

We provide robust evidence of targeting of PPP loans across counties and within a given county-bank pair. Overall, PPP loans flowed earlier to larger firms and to firms and regions less affected by the pandemic. Leveraging a survey of small firms, we show that our results reflect constraints in supply and not differential demand for PPP loans.

We develop a model of the allocation of the PPP that is consistent with our targeting results and explore the consequences of banks' incentives on the empirical estimation of the effect of the PPP. We show that the coefficient from a region- or firm-level strategy that leverages variation in the disbursement of PPP (either due to bank or regional shocks) can be decomposed into three terms, which are the causal effect of the PPP and two terms we denote by selection and targeting. The selection term captures the correlation between the instrument and the likelihood of survival in the absence of PPP loans. The targeting term refers to the correlation between the instrument and the treatment effect in a region. Our empirical analysis suggests that both the selection and targeting terms are different from zero. We show that the targeting term is present even in a case with an ideal instrument, since the set of compliers is endogenous and changes over the course of the program.

We use our decomposition to estimate the effect of the PPP on employment using county-level data. We find that the effect of the PPP was to increase employment by approximately 12.5 percentage points, which corresponds to approximately 7.5 million jobs. From a broader perspective, our paper provides a theoretical framework to estimate the effect of programs that are implemented through the use of intermediaries, in particular those for which selection into treatment is dynamic. These include various types of programs, such as other lending facilities, credit subsidies, and loan guarantee programs.

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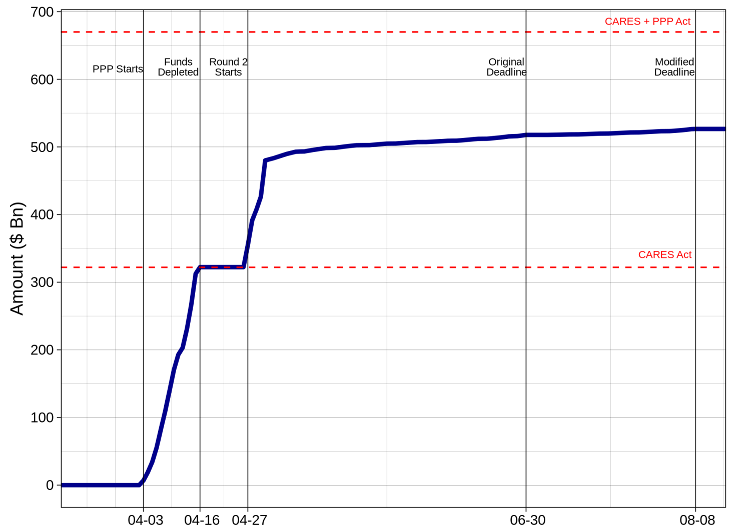
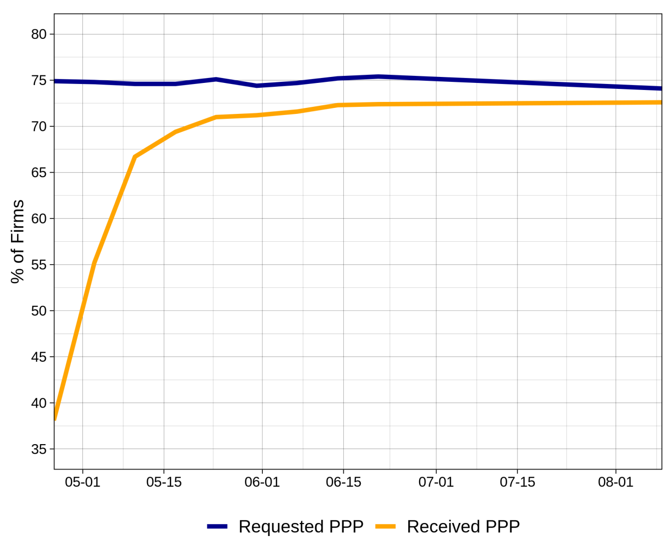
IX. Figures and Tables

Figure 1: Cumulative PPP Disbursement over Time ($, Billions)



Note: Aggregation of loan-level data from SBA/Treasury February 2021 PPP Release. Billions of dollars of PPP loans approved by day, from April 3 (CARES Act) through August 8, 2020 (modified deadline for second-round applications). Dashed horizontal lines represent the cumulative capacity of the program.

Figure 2: Small Business Pulse Survey: PPP Application vs. PPP Receipt (% of Firms)



Note: US-level data from the Small Business Pulse Survey (SBPS) collected weekly from April 26 through August 9, 2020). Blue line denotes the percentage of firms that report applying for a PPP loan. Yellow line denotes the firms that report receiving a PPP loan. For details on data collection, see Section III and Appendix B.

Table 1: Summary Statistics of the Paycheck Protection Program

	Apr-16	May-1	Jun-30	Aug-08
Loan Amount ($, Billions)	322.28	480.0	517.8	526.6
# Loans (,000)	1619.7	3700.02	4820.45	5,147.6
Jobs Supported (Millions)	33.2	54.62	59.96	61.1
Average Loan Size ($,000)	198.96	129.74	107.42	102.30
Average Jobs Supported	20.5	14.76	12.44	11.8
Top-4 Share – # Loans	0.03	0.16	0.17	0.17
Top-4 Share – Volume	0.05	0.12	0.13	0.13

Note: Aggregation of loan-level data from the SBA/Treasury February 2021 Release. Loan amount (in billions of dollars) and number of loans (in thousands) accumulated since the start of the program (April 3, 2020). Average loan size is the ratio of cumulative loan amount over the number of loans. Jobs supported are reported by the firms during the PPP application. The top-4 banks (by assets in 2019Q4) are (i) J.P. Morgan Chase Bank, (ii) Bank of America, (iii) Wells Fargo Bank, and (iv) Citibank, N.A.

Table 2: County Characteristics and PPP Allocation

Panel A. Relative to Eligible Firms

	$PPP^{n}_{r,t}$			$PPP^{vol}_{r,t}$		
	Apr–16	May–1	Jun-30	Apr–16	May–15	Jun-30
			Daily Economic Indicators			
Revenue	0.075**	-0.054**	-0.153***	2.126**	2.01*	2.001*
	(0.031)	(0.024)	(0.033)	(0.811)	(1.061)	(1.157)
Spending	0.184***	-0.052	-0.256***	4.054***	3.351**	3.075*
	(0.045)	(0.035)	(0.068)	(1.171)	(1.52)	(1.817)
			COVID Indicators			
Case Rate	-0.024***	-0.007***	0.008	-0.412***	-0.116***	-0.043
	(0.001)	(0.003)	(0.005)	(0.034)	(0.041)	(0.043)
Mobility	0.501***	-0.208*	-0.773***	11.937***	9.325***	8.773**
	(0.185)	(0.107)	(0.152)	(2.66)	(3.009)	(3.963)

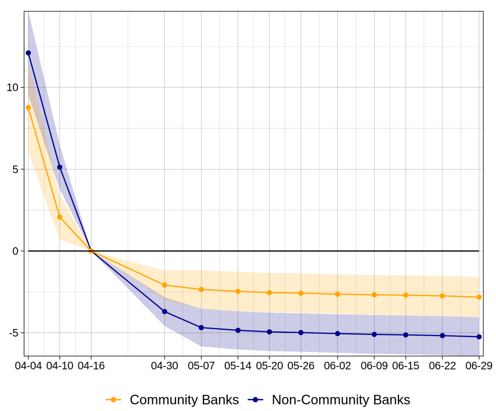
Panel B. Relative to Final PPP Allocation

	Share of Loans by t			Share of Volume by t		
	Apr–16	May–1	Jun-30	Apr–16	May–15	Jun-30
			Daily Economic Indicators			
Revenue	0.187***	0.116***	0.039***	0.121***	0.023**	0.007**
	(0.042)	(0.019)	(0.008)	(0.043)	(0.01)	(0.003)
Spending	0.367***	0.243***	0.088***	0.254***	0.063	0.016
	(0.066)	(0.069)	(0.03)	(0.069)	(0.039)	(0.011)
			COVID Indicators			
Case Rate	-0.04***	-0.024***	-0.008***	-0.039***	-0.009***	-0.002***
	(0.003)	(0.003)	(0.001)	(0.002)	(0.001)	(0)
Mobility	1.087***	0.663***	0.205***	0.762**	0.147	0.029
	(0.289)	(0.167)	(0.06)	(0.303)	(0.095)	(0.026)

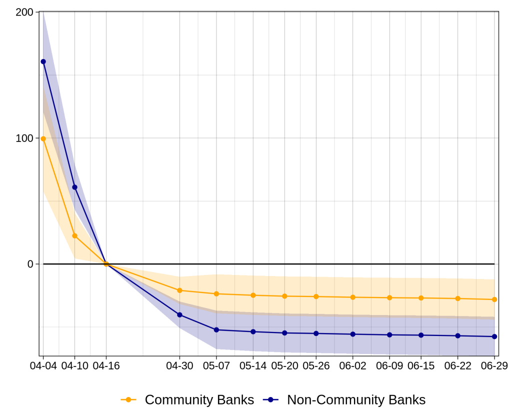
Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Standard Errors clustered at the state level in parentheses. In this table we test what explains the heterogeneity of PPP allocation across counties. Each data point in this table corresponds to a regression using county- level data. **Panel A.** The dependent variables are $PPP^{n}_{r,t}$—the ratio of PPP loans over firms in county r with fewer than 500 employees at the day in the columns (cumulative) and $PPP^{n}_{r,t}$—the ratio of PPP volume over one week's worth of payroll in county r at the same firms (cumulative). **Panel B.** The dependent variables are the share of loans/loan volume in county r that was allocated until time t (in the columns) relative to the end of the sample (August 8th) in a given bank-county pair. **Independent Variables.** The independent variables are in the rows. For a description of each, see Table A.1. Mathematically we estimate, for each day t, $Y_{r,t} = \gamma_{s,t} + \beta_t X_{0,r} + \varepsilon_{r,t}$, where $\gamma_{s,t}$ are state fixed effects and $X_{0,r}$ are the county characteristics at the baseline of the sample. Regressions are weighted by the number of employees at eligible firms at each county.

Figure 3: Within-County-Bank Allocation: Jobs Reported and Loan Size

(a) Jobs Reported



(b) Loan Size



Note: Data from the SBA/Treasury PPP release of February 2021 and Call Reports. We run the following regression at the bank b-day t level: $Y_{b,r,t} = \zeta_{b,r} + \sum_{t \neq \text{Apr-16}} \beta_t + \sum_{t \neq \text{Apr-16}} \beta_t^{CB} \times CB_b + \varepsilon_{b,t}$, where $\zeta_{b,r}$ are bank-county fixed effects, and CB_b is a community bank indicator variable. We plot the coefficients $\beta_t, \beta_t^{cb} \pm 1.96$ s.e. (clustered at the county-bank level). The dependent variables are, respectively, average number of jobs reported (Panel A) and average loan size (Panel B), in thousands of dollars, both cumulative at the county-bank-week level.

Table 3: Within-County-Bank Allocation: Share of Loans Made and Jobs Reported

	Avg. Jobs Supported (Cumulative)			
	(1)	(2)	(3)	(4)
Share of Loans Made	-14.29***	-11.62***	-12.49***	-11.41***
	(0.9426)	(0.5899)	(1.024)	(0.5636)
Bank × County	Y	Y	Y	Y
Bank × Week	N	Y	N	Y
County × Week	N	N	Y	Y
Observations	2,068,650	2,068,650	2,068,650	2,068,650
R^2	0.963	0.967	0.965	0.968

Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Data from the SBA/Treasury PPP release of February 2021 and Call Reports. We run the following regression at the bank b, county r-week t level: $Y_{b,r,t} = FEs + s_{b,r,t} + \varepsilon_{b,r,t}$, where the FEs are those described in the table and $s_{b,r,t}$ is the share of loans of bank b, in county r, made until week t (relative to the loans made by this bank, in this county, until August 8, 2020, the end of the second round of the program). The dependent variable is the average number of jobs reported at the county-bank-week level (cumulative). Errors are two-way clustered at the county and bank levels.

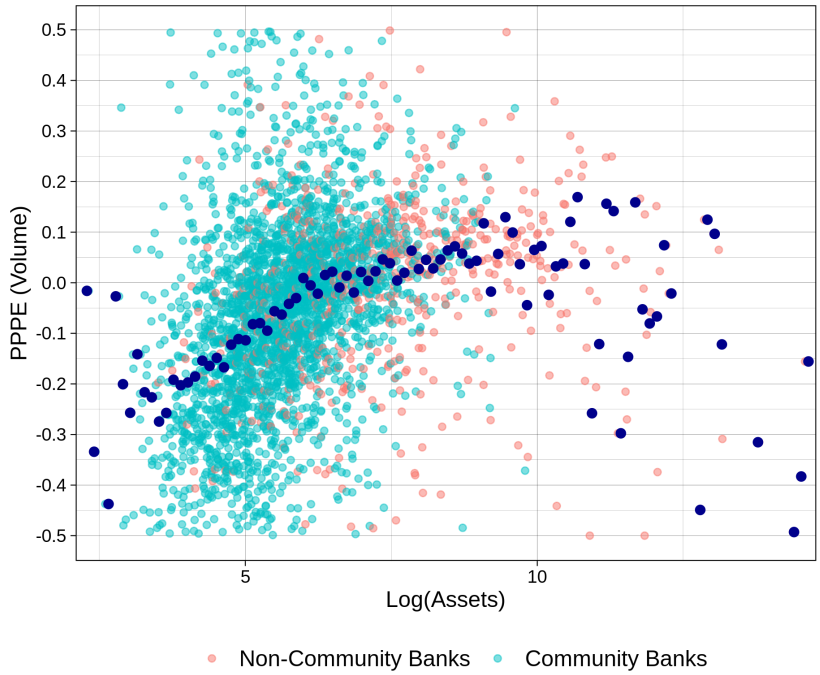
Table 4: Industry Revenue Decrease and Within-Bank PPP Allocation

	Apr- 1 (1)	May-1 (2)	May-15 (3)
Revenue Decrease	0.2060***	-0.1367***	-0.0858***
(Share of Firms in Sector)	(0.0255)	(0.0277)	(0.0314)
log(# employees +1)	0.0471***	0.0353***	0.0165***
	(0.0048)	(0.0041)	(0.0053)
Bank × County FE	Y	Y	Y
Observations	1,683,139	1,944,488	999,015
R^2	0.52	0.33	0.25

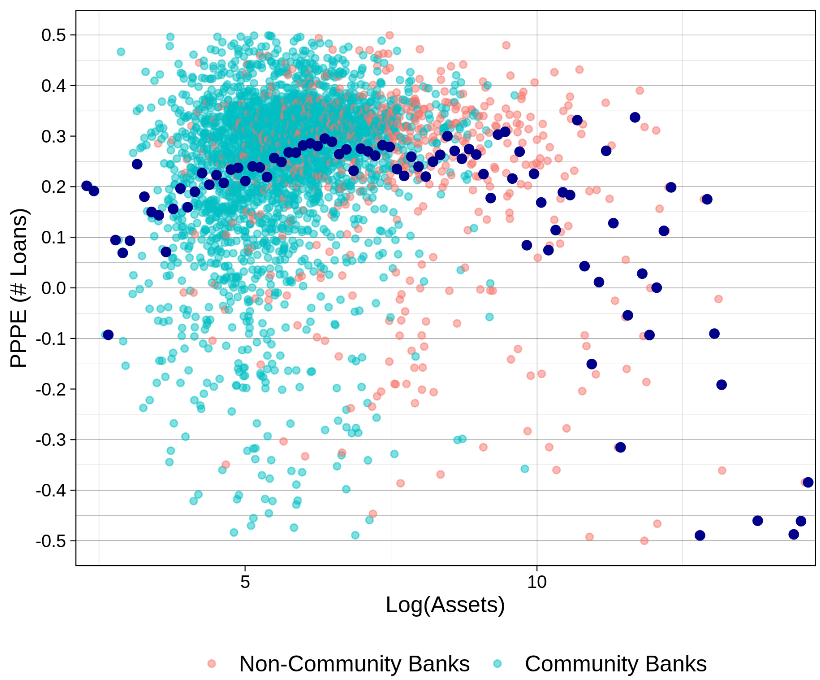
Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Data from the SBA/Treasury PPP release of February 2021. For each column, we run a linear probability model where the dependent variable is an indicator if a loan was made up to a given date (given in the columns) and the independent variables are log(# employees +1) from the PPP release and the share of firms in a sector that report a decrease in revenue in the SBPS collected between April 26 and May 2, 2020 (the first available). To keep our comparison group constant, the group of loans not made included in each column are loans not made until May 15, 2020. The loans made included are those that were made in that specific period (for instance, for column 2, those made between April 17 and May 1, 2020). Errors are two-way clustered at the county and bank levels.

Figure 4: PPPE and Bank Size: Volume and Number of Loans

(a) Volume



(b) # Loans



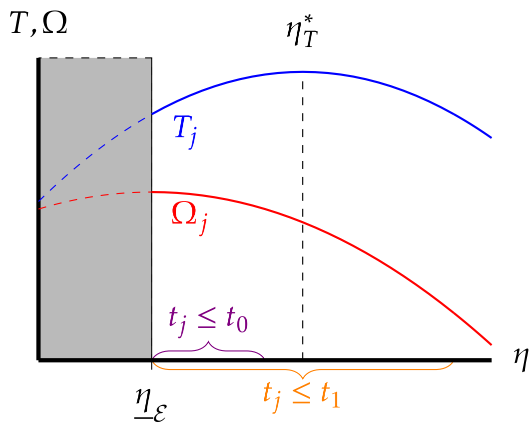
Note: Data from the SBA/Treasury February 2021 Release and Call Reports. *PPPE* is computed as in Granja et al. (2020). It is the symmetric difference of PPP loans (number or volume) and small-business lending (SBL) from schedule RC-C, Part II of the Call Reports. Mathematically, $PPPE_b = 0.5 \times \frac{\text{Share PPP}-\text{Share SBL}}{\text{Share PPP}+\text{Share SBL}}$ for either volume or number of loans. The share of PPP is computed from PPP loans made in the first round (until April 16, 2020). Log(Assets) is the natural logarithm of assets in 2019Q4 from the Call Reports. Each dot represents an individual bank, and the dark blue dots are the conditional averages of PPPE by log(assets).

Table 5: Potential Instruments and County Characteristics

	CB Share	PPPE	PPP 1st Round (Share)	Share Delayed
		Daily Economic Indicators		
Revenue	0.273***	0.255***	0.25***	0.033
	(0.041)	(0.071)	(0.038)	(0.053)
Spending	0.373***	0.32***	0.392***	0.012
	(0.06)	(0.071)	(0.073)	(0.058)
		COVID Indicators		
Case Rate	-0.145*	-0.399	-0.578*	0.211
	(0.072)	(0.274)	(0.314)	(0.165)
Mobility	0.464***	0.554***	0.571***	-0.061
	(0.055)	(0.101)	(0.078)	(0.131)
		Firm Size		
0–20 (%)	0.014	-0.17	-0.517***	0.194***
	(0.051)	(0.105)	(0.067)	(0.065)
20-100 (%)	-0.118**	0.18*	0.531***	-0.235***
	(0.049)	(0.105)	(0.071)	(0.065)
100-500 (%)	0.176***	0.128	0.399***	-0.086
	(0.048)	(0.092)	(0.06)	(0.056)

Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Standard Errors clustered at the state level in parenthesis. In this table we test if the potential instruments are correlated with county characteristics. Each data point in this table corresponds to a regression using county-level data. The independent variables are the four potential instruments we show in Figure A.1: the community bank share (branches), county PPPE (weighted average of bank PPPE), the share of PPP funds allocated in the first round, and the share of funds delayed (those between April 26 and 28 over all loans from April 14 through April 28). The dependent variables are in the rows. For a description of each, see Table A.1. Mathematically, we estimate, for each day t, $X_{r,0} = \alpha + \beta I_r + \gamma_s + \varepsilon_r$, where γ_s are state fixed effects, $X_{r,0}$ are the county characteristics, and I_r are one of the four potential instruments. To facilitate the interpretation, we normalize dependent and independent variables such that coefficients can be interpreted as a one standard deviation change in X causing a β standard deviations change in Y. Regressions are weighted by the number of employees at eligible firms.

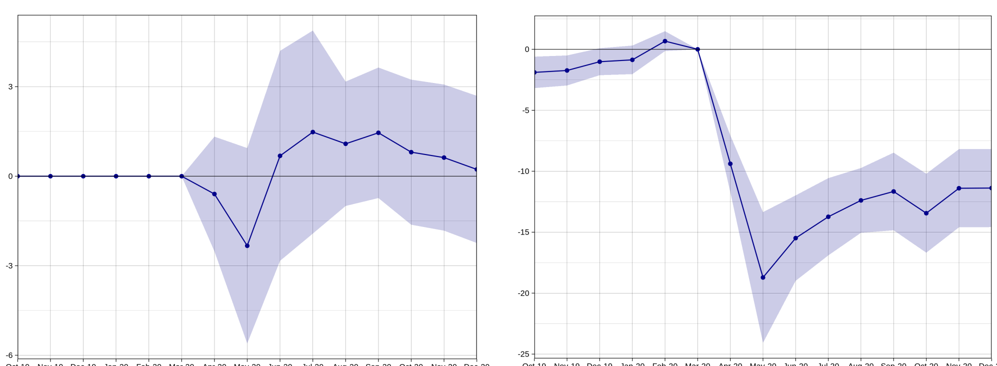
Figure 5: Empirical Support of Shock Exposure η



Note: This figure illustrates $\underline{\eta}_{\mathcal{E}}$, which is the minimum support in η consistent with the empirical evidence.

Figure 6: OLS Regression of PPP Allocation and Nonemployment

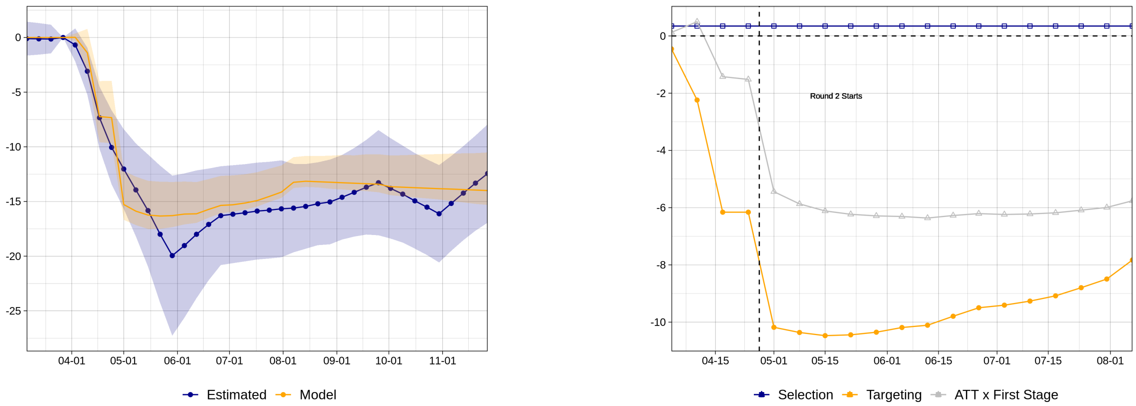
(a) PPP^n_r (b) PPP^n_r, Apr–16



Note: Data from the SBA/Treasury PPP Release, County Business Patterns/Survey of US Businesses and Local Area Unemployment Statistics from the BLS. Each plot represents the coefficients of a regression. The dependent variable in all cases is nonemployment rate (Nonemployed workers over the labor force in December 2019). The independent variable, $P_{r,t}$, corresponds to the share of eligible firms with PPP loans, either at t (Panel A) or on April 16 (Panel B), both cumulative. The regression is run at the month t, county r level. The regression specification is : Non-employment$_{r,t} = \alpha_r + \gamma_{s,t} + \xi_t \Xi_r + \sum_t \beta_t P_{r,t} + \epsilon_{r,t}$, where $\gamma_{s,t}$ are state-time fixed effects and $\xi_t \Xi_r$ is the share of firms from a 2-digit NAICS sector in a given county r interacted with time dummies. We plot the coefficients $\beta_t \pm 1.96 \times se$, where the standard error is clustered at the county level. Regressions are weighted by the number of employees in eligible firms.

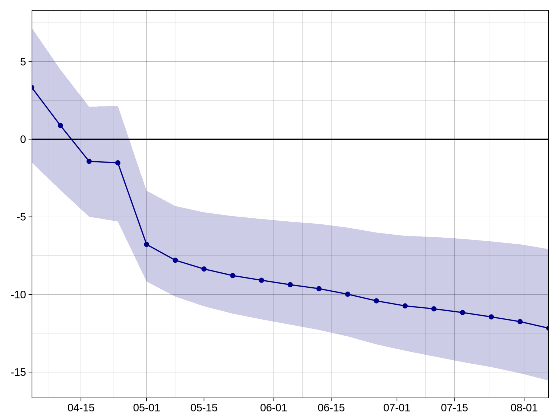
Figure 7: Structural Estimation: Model Fit and Decomposition

(a) Fit (b) Decomposition



Note: **Panel A.** reduced-form versus model implied β_t in Eq. (37). The blue series corresponds to the estimation of Eq. (37). For details, see Table 6. The only difference with respect to Figure 6 is that we use the weekly measure of nonemployed constructed through a linear interpolation of the monthly series and county-level UI claims. **Panel B.** Decomposition in Eq. (29) of the implicit $\beta(\hat{\zeta}, \hat{\delta}_t)$.

Figure 8: Structural Estimation: The Overall effect of the PPP



Note: We plot in this figure the implied ATT of PPP on nonemployment for our structural estimation (Eq. 40). Standard errors are computed through 1,000 bootstrap repetitions.

Appendix

A. Additional Figures and Tables

Table A.1: County Summary Statistics (Baseline)

	Mean	SD	Weighted Mean	Weighted SD	Obs
			Economic Indicators		
Revenue	-0.412	0.190	-0.493	0.126	1913
Spending	-0.266	0.109	-0.312	0.075	1580
Employment	-0.150	0.076	-0.148	0.055	770
			COVID Indicators		
Case Rate (per 1k)	.256	.743	.874	1.823	3015
Mobility	-0.190	0.048	-0.238	0.055	1248
			Firm Size % (<500 Employees)		
0–20 (%)	85.411	4.272	85.559	3.097	3015
20–100 (%)	10.171	2.641	10.619	2.080	2977
100–500 (%)	4.907	2.123	3.827	1.241	2908
			Banking		
CB Branches (Share)	0.469	0.312	0.209	0.202	3015
CB Deposits (Share)	0.453	0.336	0.166	0.208	3015

Note: Data at the county level. The economic indicators come from the Chetty et al. (2020*a*) and represent the change since January 2020 (seasonally adjusted) on April 2, 2020. The COVID indicators also come from the Chetty et al. (2020*a*). Case and death rates are cumulative. The mobility index is the google mobility index of time spent outside of residential locations, normalized to one from January 3 to February 6, 2020. Business closed is a dummy that is 1 when a county is in a state with a government-mandated business closure on April 2, 2020. Firm-size data come from a combination of the County Business Patterns (2019) and the Survey of US Businesses (2017). We show here the share of firms with 0–20, 20–100, and 100–500 among firms below 500 employees. The weighted columns refer to the county summary statistics weighted by the number of employees at eligible firms per county (which we will use extensively in our estimation). The banking variables come from a combination of the Summary of Deposits (2019) data set. *CB* branches are the share of branches in a county from community banks (and similarly for deposits).

Table A.2: Supply vs. Demand of PPP during the First Round Across Industries

	PPP Requested	PPP Received	PPP Gap
	(1)	(2)	(3)
Revenue Decrease	1.162***	0.3718***	0.7898***
(Share of Firms)	(0.1246)	(0.1139)	(0.1505)
Observations	18	18	18
R^2	0.67	0.18	0.49

Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1, robust standard errors in parentheses. Data at the industry level (NAICS 2-Digit) come from the Small Business Pulse Survey (SBPS) collected from April 26 to May 2, 2020, the first week for which the data are available. We run the following regression: $Y_s = \alpha + \beta X_s + \varepsilon_s$ across industries s, and display the estimate of β in the table. The dependent variable Y_s is the share of firms that report (i) requesting PPP (column 1), (ii) the share receiving PPP (column 2), and (iii) the difference between the share of firms that request and receive, which we denote by the PPP Gap (column 3). The independent variable X_s is the share of firms that report a decrease in their revenue.

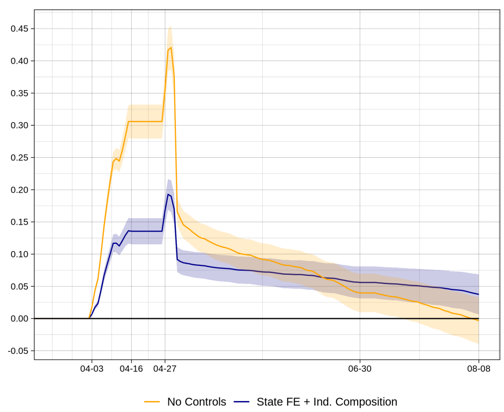
Table A.3: OLS Regression of PPP Allocation and Nonemployment

	Apr-16		May-15		Current	
	Share	Volume	Share	Volume	Share	Volume
	(1)	(2)	(3)	(4)	(5)	(6)
2019-10	-1.891***	-0.0824**	-1.082**	-0.0099		
	(0.6574)	(0.0355)	(0.4525)	(0.0373)		
2019-11	-1.735***	-0.0916***	-0.9594**	-0.0210		
	(0.6288)	(0.0344)	(0.3997)	(0.0366)		
2019-12	-1.016*	-0.0428	-0.6853*	0.0109		
	(0.5645)	(0.0311)	(0.3773)	(0.0341)		
2020-01	-0.8635	-0.0403	-0.5528	0.0097		
	(0.5975)	(0.0313)	(0.3809)	(0.0348)		
2020-02	0.6717	0.0298	0.3818	0.0269		
	(0.4164)	(0.0221)	(0.2840)	(0.0252)		
2020-04	-9.389***	-0.4093***	-2.755***	-0.1575**	-0.5966	-0.1310*
	(1.190)	(0.0603)	(0.9752)	(0.0668)	(0.9806)	(0.0742)
2020-05	-18.72***	-0.8878***	-6.778***	-0.3420**	-2.331	-0.2497
	(2.735)	(0.1388)	(1.579)	(0.1608)	(1.671)	(0.1634)
2020-06	-15.49***	-0.6461***	-5.622***	-0.2426**	0.6826	-0.1070
	(1.788)	(0.0948)	(1.261)	(0.1127)	(1.795)	(0.1243)
2020-07	-13.74***	-0.5639***	-5.001***	-0.2200**	1.480	-0.0810
	(1.616)	(0.0863)	(1.162)	(0.1071)	(1.736)	(0.1182)
2020-08	-12.39***	-0.4872***	-4.916***	-0.2079***	1.087	-0.1085
	(1.356)	(0.0696)	(1.033)	(0.0805)	(1.063)	(0.0786)
2020-09	-11.66***	-0.5123***	-4.403***	-0.2263**	1.455	-0.1161
	(1.624)	(0.0842)	(1.045)	(0.0956)	(1.116)	(0.0975)
2020-10	-13.44***	-0.5397***	-5.719***	-0.2070**	0.8057	-0.0885
	(1.652)	(0.0813)	(1.084)	(0.0906)	(1.240)	(0.0925)
2020-11	-11.40***	-0.5056***	-5.132***	-0.2348**	0.6241	-0.1109
	(1.636)	(0.0813)	(1.003)	(0.0940)	(1.249)	(0.0987)
2020-12	-11.38***	-0.4807***	-5.619***	-0.2121**	0.2278	-0.0847
	(1.633)	(0.0833)	(1.022)	(0.0962)	(1.258)	(0.1010)
Observations	40,170	40,170	40,170	40,170	40,170	40,170
R^2	0.94	0.94	0.94	0.94	0.94	0.94
County FE	Y	Y	Y	Y	Y	Y
State × Time FE	Y	Y	Y	Y	Y	Y
Industry Exposure	Y	Y	Y	Y	Y	Y

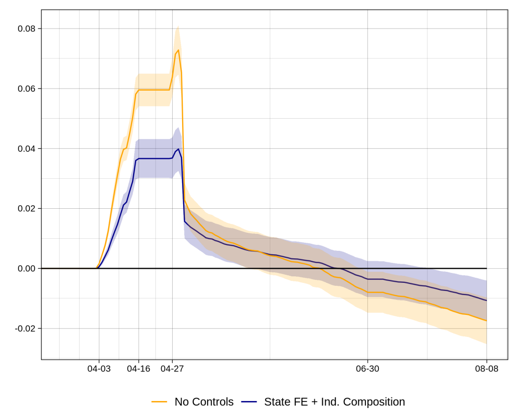
Note: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Data are from the SBA/Treasury PPP release, County Business Patterns/Survey of US Businesses, and Local Area Unemployment Statistics from the BLS. Each plot represents the coefficients of a regression. The dependent variable in all cases is nonemployment rate (nonemployed workers over the labor force in December 2019). The regression is run at the month t, county r level. The regression specification is : $\text{Nonemployment}_{r,t} = \alpha_r + \gamma_{s,t} + \xi_t \Xi_r + \sum_t \beta_t P_{r,t} + \epsilon_{r,t}$, where $\gamma_{s,t}$ are state-time fixed effects and $\xi_t \Xi_r$ is the share of firms from a 2-digit NAICS sector in a given county r interacted with time dummies. We plot the coefficients $\beta_t \pm 1.96$ the standard error clustered at the county level. The independent variables $P_{r,t}$ are in the columns. They are either the share of eligible firms or weeks' worth of payroll from eligible firms in a county. For Columns 1–4, we use these variables computed at the time indicated. For Columns 5 and 6, we use the contemporaneous PPP allocation. Standard errors clustered at the county level. Regressions are weighted by the number of eligible firms in a county.

Figure A.1: PPP County Allocation over Time: # of PPP Loans per Eligible Firm

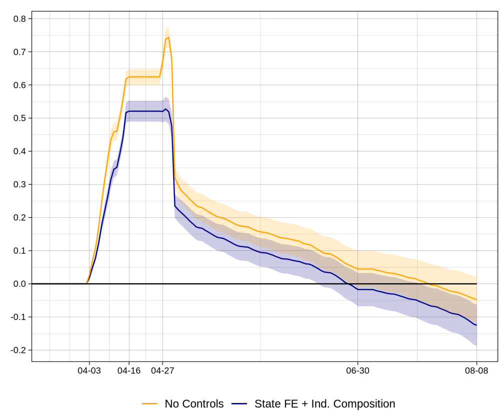
(a) CB Share (Branches)



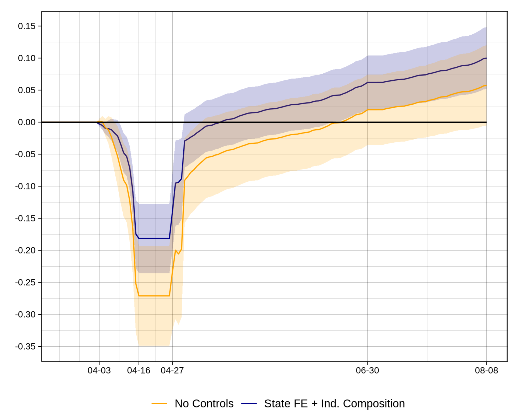
(b) $PPPE_r$



(c) PPP 1st Round (Share)



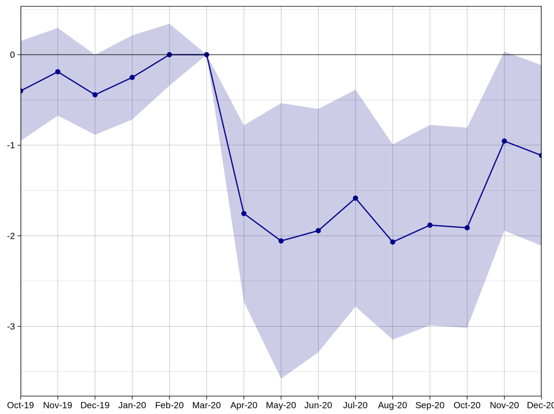
(d) Share Delayed



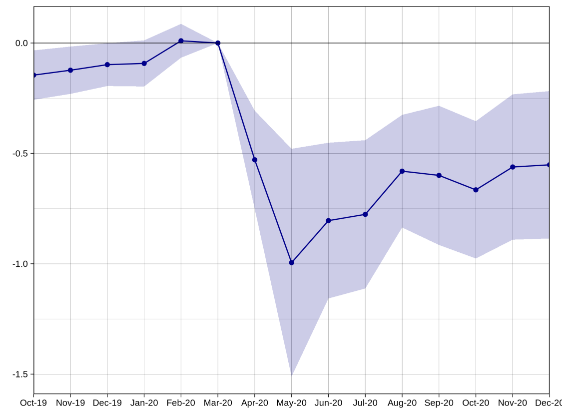
Note: Data from the SBA/Treasury PPP release, Summary of Deposits, and Call Reports. For county r, we run a daily regression $PPP^n_{r,t} = \alpha_r + \gamma_{s,t} + \xi_t \Xi_r + \sum_t \beta_t I_r + \epsilon_{r,t}$, where $PPP^n_{r,t}$ corresponds to the cumulative share of eligible firms with PPP loans, γ_s are state fixed effects and $NAICS_{i,r}$ is the share of firms from a 2-digit NAICS sector in a given county r. The independent variable, I_r, represents potential instruments. Potential instruments I_r are the share of branches from community banks in 2019, the county level PPPE (Eq. (5)), the share of PPP loan amount received in the first round (until April 16, 2020), and the share delayed (share of loan amount from April 26 to 28 relative to all PPP loans from April 14 to 28). The regression is run at the month t, county r level. We plot the coefficients $\beta_t \pm 1.96 \times se$. Regressions are weighted by the number of eligible firms in a county.

Figure A.2: Potential PPP Instruments and Nonemployment

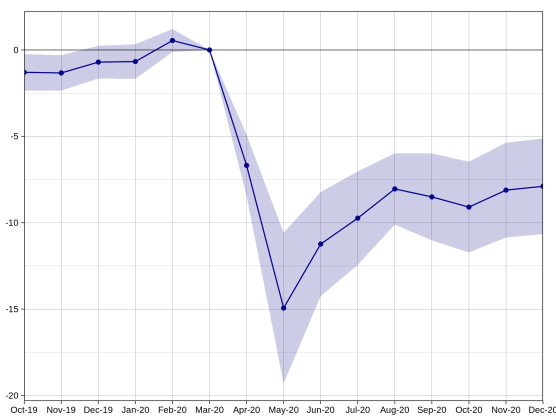
(a) CB Share (Branches)



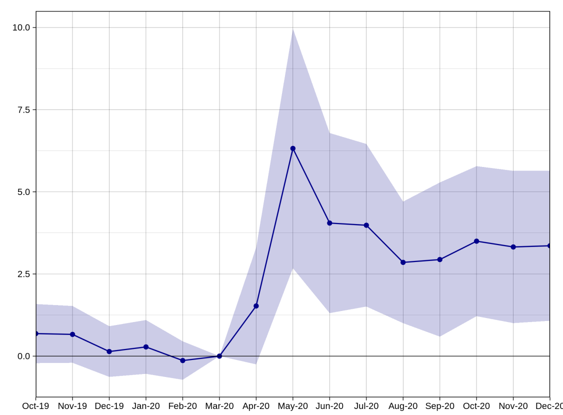
(b) *PPPE*



(c) PPP 1st Round



(d) Share Delayed



Note: Data from the SBA/Treasury PPP release, County Business Patterns/Survey of US Businesses, and Local Area Unemployment Statistics from the BLS. Each plot represents the coefficients of a regression. The dependent variable in all cases is nonemployment rate (nonemployed workers over the labor force in December 2019). The independent variable, I_r, represents potential instruments. Potential instruments I_r are the share of branches from community banks in 2019, the county level PPPE (Eq. (5)), the share of PPP loan amount received in the first round (until April 16, 2020), and the share delayed (share of loan amount from April 26 to 28 relative to all PPP loans from April 14 to 28). The regression is run at the month t, county r level. The regression specification is : $\text{Nonemployment}_{r,t} = \alpha_r + \gamma_{s,t} + \xi_t \Xi_r + \sum_t \beta_t I_{r,t} + \epsilon_{r,t}$, where $\gamma_{s,t}$ are state-time fixed effects and $\xi_t \Xi_r$ is the share of firms from a 2-digit NAICS sector in a given county r interacted with time dummies. We plot the coefficients $\beta_t \pm 1.96 \times se$, where the standard error is clustered at the county level. Regressions are weighted by the number of eligible firms in a county.

B. Data Appendix

B.1. Sources

SBA/PPP Release. Our primary source for data on the PPP comes from the SBA/Treasury (February 2021 version). The data set includes information self-reported by the borrower (name, address, Zip code, NAICS code, and jobs supported) as well as loan amount, approval date, and lender name. We analyze the loans made in the first and second rounds of the program (April 3 to August 8, 2020). No loans were made in the program from August 8, 2020, to January 11, 2021 (when the second draw of PPP loans began). The date of a loan is the date of approval (according to the rules of the program, loans must be disbursed within 10 calendar days of approval).

Zip Code to County Aggregation. To aggregate the data to the county level, we use the HUD Zip crosswalk to match each loan to a county (HUD (2020)). For the Zip codes that span two counties, we allocate the proportional share of loans in that Zip code based on the relative number of business in those counties. In our sample, 90 percent of all PPP loans are made to firms in Zip codes that have at least 95 percent of firms in a given county; that is, we can assign a county for this loan with a high level of confidence.

Call Reports. From the Call Reports, we obtain financial characteristics of all banks. Our data contain domestic total assets (RCON2170), Tier 1 leverage ratio (RCOA7204), C&I Lending (RCON1766), unused C&I committed amounts (RCONJ457), and liquid assets (cash plus securities ready to sale). From *Schedule RC-C Part II*, we get the outstanding volume of small-business loans by bank. More specifically, what we have is the number and amount currently outstanding as of the report date of business loans with "original amounts" of $1 million or less. From Schedule RC-M, we recover the outstanding amount of PPP loans for those banks (Schedule RC-M). Within the set of banks that file Call Reports, 846 out of 4,970 have no outstanding PPP loans in 2020Q2. We complement our Call Reports data from the equivalent fillings from credit unions (from NCUA). For credit unions, we observe assets, total C&I Lending (not broken down by loan size), and PPP outstanding amount.

Lender Name Matching. Our process of lender name matching from the PPP release to the institutions that are active in 2020 and registered in the National Information Center database follows Granja et al. (2020). We first use a string-matching procedure for the lender name. The main matching process made use of the *matchit* function in Stata, which gives a similarity score between two strings (the default bigram setting was used, but variations in n-gram did not affect results). In conjunction with this "fuzzy" match, we used geographic information from both data sources (city, state, Zip) to assign higher certainty to matches. Prior to the match, we also instituted a process to identify and group names by charter type. For example, to identify federal credit unions, we used regular expressions to separate out names that contained any iteration/version of: [FCU | F.C.U | F C U], etc. Extending the example, we then used both the short [FCU] and long [FEDERAL CREDIT UNION] versions of the charter-patterns (as well as specifying their expected location in the string/name), to create a list of FCU names from both data sources (that is, the SBA-FCUs and NIC-FCUs.). Ultimately, the goal here was to reduce ambiguity by removing the charter-indicating text entirely. The reasoning behind this being that this part of the name conveys limited distinguishing information (that is, charter), and at the same time is one of the most varied/unpredictable parts of the name. By grouping the names by charter, we captured the relevant information and were then able to make joins with a truncated version of the name. This reduced the string variation that matchit had to "interpret" while retaining relevant charter-type distinctions. Using several versions of the names created in the process above, successive joins were made, removing matched names from the cohort each round. In other words, if an un-truncated name matched exactly and had matching geographic information, the match was set aside, and the charter-identification process was factored in for unmatched names. There are two challenges in this procedure. There are lenders in the PPP for which we do not find a match in the NIC database. For the 10 largest PPP lenders where this happens, we match those by hand. These 10 lenders account for 50 percent of the unmatched loan volume from our first pass. Table B.1 provides details of the share of loans we can match (and from which source).

Summary of Deposits Merger Adjustment. As mentioned in the main text, we use data from the Summary of Deposits at the bank- or bank-county level. The Summary of Deposits data are collected for institutions operating in the United States as of June 30, 2019. As there was a significant number of financial institutions in the second half of 2019, we use the mergers file from the NIC to account for changes in bank ownership until the 2020Q1 Call Report filings. This correction is particularly relevant due to the merger of SunTrust Banks, Inc.

Table B.1: Paycheck Protection Program and Bank Data Crosswalk

	Apr-16	May-1	Jun-30	Aug-08
Share Matched - Volume	0.95	0.96	0.95	0.95
Share Matched - # Loans	0.93	0.94	0.94	0.94
Share Matched from Call Reports - Volume	0.88	0.90	0.87	0.86
Share Matched from Call Reports - # Loans	0.88	0.90	0.87	0.86

Note: Data from the SBA/Treasury February 2021 release merged with the merger adjusted Call Reports (banks and credit unions). We merge lender's name and location from the PPP release with the set of financial institutions with an RSSD from the NIC Information Center. Share Matched refers to the share of PPP loans (volume or amount) for which we can determine the RSSD of the lender over all approved PPP loans (cumulatively). Share Matched from Call Reports are those where the matched institution is an active bank that files a Call Report.

with Branch Banking and Trust Company (BB&T) on December 6, 2019, which created the sixth largest financial institutions in the United States.

County Business Patterns/Survey of U.S. Businesses. We use a combination of the County Business Patterns (CBP, 2020) and the Survey of U.S. Business (SUSB, 2017) to compute the total number of employees at the county level (by firm size and per NAICS 2 digits), the total number of eligible firms, and the total annual payroll of these firms. From the County Business Patterns, we recover breakdown of employees by establishment size and industry and total payroll in a county. From the Survey of U.S. Businesses (2017), we construct two ratios by county: (i) ratio of establishments to firms and (ii) share of payroll from firms with fewer than 500 employees. We then obtain the approximate number of eligible firms in a county by multiplying the ratio of establishments to firms in that county by the number of establishments with fewer than 500 employees in that county. Overall, this establishment/firm adjustment is small and not material for our results. We compute the total payroll in a county by multiplying the share of payroll from firms with fewer than 500 employees with the total payroll in 2019.

Track the Recovery. We use the high-frequency (daily) data from Chetty et al. (2020*a*) to obtain county-level measures of employment, revenue, spending, COVID-19 cases and deaths, mobility, and UI claims.[22] We present a brief description of the data here, but for details on the data collection, see Chetty et al. (2020*a*). Spending comes from aggregation of consumer

[22] Data downloaded on February 25, 2020. Given the nature of the primary sources of data, the data set keeps evolving and being changed and updated over time.

spending based on debit and credit card transactions from Affinity Solutions. Small business revenue comes from Womply, a company that aggregates data from card transactions to provide insights to small businesses. COVID-19 cases and deaths come from the CDC. Mobility is measured by time spent away from home, estimated using cellphone location data from Google users. Unemployment insurance claims come from the U.S. Department of Labor. The county-level series is available only for states whose respective state agencies publish county-level data.

SBPS. To complement our analysis of PPP targeting, we obtain data from the Census Bureau's Small Business Pulse Survey (SBPS). For details, see Buffington et al. (2020). The SBPS was designed to collect real-time information from small businesses during the pandemic. The target population is all nonfarm, single-location employer businesses with 1 to 499 employees and receipts of $1,000 or more. Data were collected weekly via email, from April 26 to June 21, 2020, based on the Census Bureau's Business Register. We use the state-sector (NAICS2) version of the data. The surveys are adjusted for non-response and re-weighted weekly to guarantee representativeness.

B.2. Final Data Sets.

Bank-Level Data. We aggregate the merged-PPP-Call data set at the bank-date level (for the banks we do match).

County-level Data. First, we aggregate the PPP from the Zip to the county level as described above. Second, we merge this county-day data set with the high-frequency data from Chetty et al. (2020*a*) by FIPS code. We merge the resulting data set with county cross-section information from the Summary of Deposits (concentration, branches per capita, share of branches/deposits from community banks) and the CBP/SUSB data set (number of eligible firms, total payroll at eligible firms, employees per 2-digit NAICS).

County-Bank-Level Data. We aggregate the PPP from the Zip code-bank level to the county-bank level as described above (for the banks we do match with information from the NIC Center). We then match this data with the county-bank data from the Summary of Deposits (number of branches, deposits). Finally, we merge this data set with our county-level data.

Loan-Level Data. We merge the county and lender information (from the bank-level data) into the SBA/Treasury PPP release.

C. Proofs and Derivations

C.1. Firm's Choice in the PPP

Auxiliary Result. For the distribution in (7), we have that $\mathbb{E}[\nu \mid \nu \leq X] = \frac{\eta}{\eta+1}X$

$$\mathbb{E}[\nu \mid \nu \leq X] = \left(\frac{X}{c_0}\right)^{-\eta} \int_0^X \eta t \frac{1}{c_0}\left(\frac{t}{c_0}\right)^{\eta-1} dt = (X)^{-\eta}\,\eta \int_0^X t^\eta dt = X^{-\eta}\eta \frac{X^{\eta+1}}{\eta+1} = \frac{\eta}{\eta+1}X \quad \blacksquare$$

Choice of a. From the firm objective function in (8), a firm chooses to apply if

$$\Phi_j(\varphi)\Big(\Pi_j(\varphi) - \mathbb{E}\big[\nu_j \mid \nu_j \leq \Gamma_j(\varphi)\big]\Big) - \Phi_j(0)\Big(\Pi_j(0) - \mathbb{E}\big[\nu_j \mid \nu_j \leq \Gamma_j(0)\big]\Big) > \frac{F}{N_j}$$

Therefore, $a_j^* = 1$ if :

$$T_j\Pi_j(0) - \Phi_j(\varphi) r_G \varphi - \int_{\Gamma_j(0)}^{\Gamma_j(\varphi)} \nu d\Phi(\nu \mid \eta_j) > \frac{F}{N_j}$$

which delivers (11). Using the distribution in (7):

$$T_j\Pi_j(0) - T_j\mathbb{E}\big[\nu_j \mid \nu_j \in [\Gamma_j(0), \Gamma_j(\varphi)]\big] = (c_j + \pi_j^{LR})T_j - \Phi_j(\varphi)\frac{\eta_j}{\eta_j+1}(c_j+\varphi) + \Phi_j(0)\frac{\eta_j}{\eta_j+1}c_j \quad (41)$$

which delivers (12). $\blacksquare$

C.2. Lemma 1

Proof. Let $\mathcal{B}_t(\{l_j^B\}_j)$ be the Lagrangian of the problem of the bank in (17). The derivative of the Lagrangian of $\mathcal{B}_t(.)$ with respect to $l_{j,t}^B$, that is, the marginal allocation

$$\mathcal{B}_{t,l} \equiv \frac{\partial B}{\partial l_j^B} = \Omega_{j,t} - \varphi\lambda$$

where λ is the Lagrange multiplier in the resource constraint.

Case 1. Debt heterogeneity. Suppose that j, $\tilde{j}$ haven't received a PPP loan up to time t

(with t potentially being zero). When firms are heterogeneous only in b_j, we have $\frac{\partial \Omega_{j,t}}{\partial b_j} = T_{j,t}(1-\delta)(1+\psi_F) + \theta_{j,t}\psi_C\psi_F b_j > 0$; that is, if the bank is to allocate loans to either j or $\tilde{j}$ in t, it will do so for firm j.

Case 2. Shock Exposure Heterogeneity. Consider two firms that are the same except η_j. Then:

$$\frac{\partial \Omega_{j,t}}{\partial \eta_j} = \left[\kappa (c+\varphi)^{\eta_j} \cdot \ln(c+\varphi) - (\kappa - \tilde{\psi}) c^{\eta_j} \ln(c)\right]$$

where $\kappa \equiv ((1-\delta)+\psi_F)b + q\varphi$ and $\tilde{\psi} \equiv \psi_F\psi_C b + q\varphi$. Therefore

$$\frac{\partial \Omega_{j,t}}{\partial \eta_j} > 0 \Leftrightarrow (c+\varphi)^{\eta_j} \cdot \ln(c+\varphi) > \left[1 - \frac{\tilde{\psi}}{\kappa}\right] c^{\eta_j} \ln(c)$$

Which implies:

$$\eta_j \ln\left(1+\frac{\varphi}{c}\right) + \ln(-\ln(c+\varphi)) < \ln\left(-\left[1-\frac{\tilde{\psi}}{\kappa}\right]\ln(c)\right) \Leftrightarrow \eta_j < \eta^* \equiv \frac{\ln\left(\left[1-\frac{\tilde{\psi}}{\kappa}\right]\frac{\ln(c)}{\ln(c+\varphi)}\right)}{\ln(1+\frac{\varphi}{c})}$$

since $\tilde{\psi} < \kappa$.

Therefore, $\Omega_{j,t}$ is strictly increasing in η up to $\eta^* > 0$ and strictly decreasing afterward. The optimal allocation at time 0 is thus $l^B_{j,t=0} = 1$ if $\eta_j \in [\underline{\eta}(0), \overline{\eta}(0)]$, where:

1. $\Omega_{\underline{\eta}(0),t=0} = \Omega_{\overline{\eta}(0),t=0}$,[23] and
2. $\int_{\underline{\eta}(0)}^{\overline{\eta}(0)} \varphi dj = M_0$

Note that $\underline{\eta}(0), \overline{\eta}(0)$ (i) exist, since the resource constraint is binding and (ii) are unique, since $\Omega_{j,(0)}$ is quasi-concave.

We show by induction that the same is true for $t > 0$. For $t > 0$ $l^B_{j,t=0} = 1$ if $\eta_j \in [\underline{\eta}(t), \overline{\eta}(t)]$ and $\eta_j \notin \eta_j \in [\underline{\eta}(t-1), \overline{\eta}(t-1)]$ where analogous conditions must hold

1. $\Omega_{\underline{\eta}(t),t} = \Omega_{\overline{\eta}(t),t}$ and
2. $\int_{\underline{\eta}(t)}^{\overline{\eta}(t)} \varphi dj = M_t$

[23] Here it is possible that this condition is not satisfied with an equality if we reach the limits of the support of η. For instance, assume that $\underline{\eta}(0)$ is lower than the minimum of the support of η_0. Then, we set $\underline{\eta}(0)$ to this minimum and set $\overline{\eta}(0)$ to satisfy condition number 2.

Since $\Omega_{j,t}$ is quasi-concave and has a unique maximum in $\eta^* \in [\underline{\eta}(0), \overline{\eta}(0)] \subset [\underline{\eta}(t-1), \overline{\eta}(t-1)]$, our first condition implies that $\underline{\eta}(t) < \underline{\eta}(t-1)$ and $\overline{\eta}(t) > \overline{\eta}(t-1)$ (with an equality at the limits of the support of η).

■

C.3. Naive Regression

Firm Level. From the FWL theorem, we have that

$$\beta_{F,t} \xrightarrow[p]{} \frac{\text{Cov}(y_{j,t}, P\dot{P}P_{j,t})}{\mathbb{V}(P\dot{P}P_{j,t})}, \quad \text{for} \tag{42}$$

From our true model, we have that for $h \leq 0$, $\beta_{F,h}$ is not well defined (since all firms have no PPP, by definition). For $h \geq 1$, since $\mathbb{E}[P\dot{P}P_{j,t}] = 0$

$$\beta_{F,h} = \mathbb{V}(P\dot{P}P_{j,t})^{-1}\left[\mathbb{E}(\theta_j P\dot{P}P_{j,t}) + \mathbb{E}(T_{j,t}PPP_{j,t}P\dot{P}P_{j,t})\right]$$

Moreover

$$\mathbb{E}(T_{j,t}PPP_{j,t}P\dot{P}P_{j,t}) = \mathbb{E}\left[T_j|PPP_{j,t} = 1\right]E\left[PPP_{j,t}P\dot{P}P_{j,t}\right] = ATT_t\mathbb{V}(P\dot{P}P_{j,t})^{-1}$$

where in the first equality we use the fact that $P\dot{P}P_{j,t} > 0 \Leftrightarrow PPP_{j,t} = 1$

Regional Level. Following the same steps as above (using weights)

$$\beta_{R,h} = \mathbb{V}_w(P\dot{P}P_{r,t})^{-1}\left[\mathbb{E}_w(\theta_j P\dot{P}P_{r,t}) + \mathbb{E}_w(T_{r,t}PPP_{r,t}P\dot{P}P_{r,t})\right]$$

Moreover

$$\mathbb{E}_w((T_{r,t} - ATT + ATT)PPP_{j,t}P\dot{P}P_{j,t}) = ATT_t\mathbb{V}_w(P\dot{P}P_{j,t}) + \mathbb{E}_w((T_{r,t} - ATT)PPP_{j,t}P\dot{P}P_{j,t})$$

C.4. Lemma 2 (Firm and Regional Level)

Proof. From the FWL theorem, we have that

$$\beta_{F,t} \underset{p}{\longrightarrow} \frac{\mathrm{Cov}(y_{j,t}, \dot{\mathcal{I}}_{j,t})}{\mathbb{V}(\dot{\mathcal{I}}_{j,t})} - \beta_{F,-1} \tag{43}$$

Given our true model in Eq. (21), we have that $\hat{\beta}_{F,-1} \underset{p}{\longrightarrow} 0$. Moreover

$$\begin{aligned}
\frac{\mathrm{Cov}(y_{j,t}, \dot{\mathcal{I}}_{j,t})}{\mathbb{V}(\dot{\mathcal{I}}_{j,t})} &= \mathbb{V}(PPP_{j,t})^{-1}\left[\mathrm{Cov}(\theta_j, \dot{I}_{j,t}) + \mathrm{Cov}(T_{j,t}PPP_{j,t}, \dot{I}_{j,t})\right] \\
&= ATT_t \cdot \frac{\mathrm{Cov}(PPP_{j,t}, \dot{\mathcal{I}}_{j,t})}{\mathbb{V}(\dot{\mathcal{I}}_{j,t})} + \frac{\mathbb{E}\left[\theta_j \dot{\mathcal{I}}_{j,t}\right]}{\mathbb{V}(\dot{\mathcal{I}}_{j,t})} + \frac{\mathbb{E}\left[\left(T_{j,t} - ATT_t\right)PPP_{j,t}\dot{\mathcal{I}}_j\right]}{\mathbb{V}(\dot{\mathcal{I}}_j)}
\end{aligned} \tag{44}$$

■

C.5. Exogenous Bank PPP Capacity and IV Estimation

Under the assumptions of Section VI.3, we have that the IV estimation of Eq. (32) using s_{b_j} as an instrument delivers

$$\hat{\beta}_{IV,F,t} \underset{p}{\longrightarrow} ATT_t + \frac{\varphi}{M_t}\frac{\mathrm{Cov}_\mu(\theta_b, s_b)}{\mathbb{V}_\mu(s_b)} + \frac{\mathbb{E}_\mu[(ATT_{b,t} - ATT_t)s_{b_j}^2]}{\mathbb{V}_\mu(s_b)} \tag{45}$$

$$= ATT_t + \frac{\mathbb{E}_\mu[(ATT_{b,t} - ATT_t)s_{b_j}^2]}{\mathbb{V}_\mu(s_b)} \tag{46}$$

where $\mathbb{E}_\mu$ denotes the weighted expectation of any variable using bank market shares μ_b, and let $\mathbb{V}_\mu$ and Cov_μ represent the analogous weighted variance and covariance, respectively. From the first to the second line, we use the assumption that θ_b is identical across banks. To see this (absorbing the time index for simplicity), note that:

$$\hat{\beta}_{IV,F} \underset{p}{\longrightarrow} \frac{\mathrm{Cov}(y_j, s_{b_j})}{\mathrm{Cov}(PPP_j, s_{b_j})} = \frac{\mathrm{Cov}(\theta_j, s_{b_j})}{\mathrm{Cov}(PPP_j, s_{b_j})} + \frac{\mathrm{Cov}(T_j PPP_j, s_{b_j})}{\mathrm{Cov}(PPP_j, s_{b_j})}$$

We can also write:

$$\text{Cov}(PPP_j, s_{b_j}) = \mathbb{E}[\mathbb{E}[PPP_j|s_{b_j}]s_{b_j}] - \frac{M}{\varphi}\bar{s}^2 = \frac{M}{\varphi}\left\{\mathbb{E}\left[s_{b_j}^2\mu_{b_j}\right] - \bar{s}^2\right\} = \frac{M}{\varphi}\mathbb{V}_\mu(s_b)$$

where we use the fact that $\overline{p} = \frac{M}{\varphi}\sum_b \mu_b s_b$. Moreover,

$$\text{Cov}(\theta_j, s_{b_j}) = \mathbb{E}[\theta_j s_{b_j}] - \mathbb{E}[\theta_j]\mathbb{E}[s_{b_j}] = \mathbb{E}\left[\left(\mathbb{E}[\theta_j|s_{b_j}] - \overline{\theta}\right)s_{b_j}\right] = \text{Cov}_\mu(\theta_b, s_b)$$

Finally, we have that

$$\begin{aligned}
\text{Cov}(T_j PPP_j, s_{b_j}) &= \mathbb{E}[T_j PPP_j s_{b_j}] - \mathbb{E}[T_j PPP_j]\mathbb{E}[s_{b_j}] = \mathbb{E}[\mathbb{E}[T_j|PPP_j, s_{b_j}]\mathbb{P}[PPP_j = 1|s_{b_j}]s_{b_j}] - ATT\frac{M}{\varphi}\bar{s} \\
&= \frac{M}{\varphi}\left\{\mathbb{E}_\mu[ATT_b s_{b_j}^2] - ATT\bar{s}\right\} = \frac{M}{\varphi}\left\{\mathbb{E}_\mu[(ATT_b - ATT)s_{b_j}^2] + ATT\left[\mathbb{E}_\mu[s_{b_j}^2] - \bar{s}\right]\right\} \\
&= \frac{M}{\varphi}\left\{\mathbb{E}_\mu[(ATT_b - ATT)s_{b_j}^2] + ATT \times \mathbb{V}_\mu(s_b)\right\}
\end{aligned}$$

Therefore:

$$\beta_{F,IV} \xrightarrow[p]{} ATT + \frac{\varphi}{M}\frac{\text{Cov}_\mu(\theta_b, s_b)}{\mathbb{V}_\mu(s_b)} + \frac{\mathbb{E}_\mu[(ATT_b - ATT)s_{b_j}^2]}{\mathbb{V}_\mu(s_b)}$$

C.6. Lemma 3

Proof. In this section we use the results of the IV estimation in Appendix C.5.

From Eq. (35), $\underline{\Omega}_{b,t}$ is strictly decreasing in s_b. Let

$$T_{b,t} \equiv \mathbb{E}\left[T_j \mid \Omega(\theta_j, T_j, b_j) > \underline{\Omega}_b\right]$$

We have that $T_{b,t}$ is a function of s_b. Although it is possible that $\text{Cov}(s_b, T_{b,t}) = 0$, this is a zero-probability event given the relation between s_b and $T_{b,t}$. From Eq. (45), this implies that $\beta_{F,IV} \not\xrightarrow[p]{} ATT$.

∎

C.7. Lemma 4

Our proof will be composed of two parts. First, we will show how the treatment effects for different banks evolve as t increases. Second, we show how these differences enter into the IV estimation. In this section we use the results of the IV estimation in Appendix C.5.

C.7.1 Part 1: Treatment Effects

Let the optimal bank allocation at time t be given by $[\underline{\eta}_{\mathcal{E}}, \overline{\eta}_{A,t}]$ for a given bank A, and let N_B denote the number of banks. Let $\overline{\eta}_{-A,t}$ be given by

$$\int_{\underline{\eta}_{\mathcal{E}}}^{\overline{\eta}_{-A,t}} dj = (N_B - 1)^{-1} \left[\int_{\underline{\eta}_{\mathcal{E}}}^{\overline{\eta}_{\tilde{b},t}} dj \right]$$

Let $T_{A,t} \equiv \mathbb{E}[T_j | \eta_j < \overline{\eta}_{A,t}]$ and $T_{-A,t} \equiv \mathbb{E}[T_j | \eta_j < \overline{\eta}_{-A,t}]$. Note that $T_{A,t}$ and $T_{-A,t}$ are continuous in t. Let $\Delta_{A,t} \equiv T_{A,t} - T_{-A,t}$.

Suppose that bank A is such that $s_A > 1$ (which implies that $(N_B - 1)^{-1} \sum_{b \neq A} s_b < 1$). Therefore, since $\overline{\eta}_{b,t}$ is increasing in s_b, we have that:

$$\eta_{A,t} > \eta_{-A,t}$$

We will show that (i) for t sufficiently small, $\Delta_{A,t} > 0$, and (ii) $\Delta_{A,t}$ crosses the x-axis only once, at t^*, from above; that is, $\Delta_{A,t} > 0$ iff $t < t^*$.

Step 1. Small t. For $t \to 0$ (since T_j is increasing up to η_T^*), we have that $\eta_T^* > \eta_{A,t} > \eta_{-A,t}$ and, thus:

$$T_{A,t} > T_{-A} \Rightarrow \Delta_{A,t} > 0$$

Step 2. Axis Crossing. We must separate our analysis in three cases. Note that both $\eta_{A,t}$ and $\eta_{-A,t}$ are strictly increasing in t. Therefore, we can define two limits $C_1 < C_2$ where:

1. At $t = C_1$, $\eta_T^* = \eta_{A,C_1} > \eta_{-A,C_1}$
2. At $t = C_2$, $\eta_{A,C_2} > \eta_T^* = \eta_{-A,C_2}$

If the program is not sufficiently large, we can have that C_1 or C_2 (or both) are not well defined, since at the end of the second round of the program we can theoretically still have

$\eta_T^* > \eta_{A,t_{end}}$ or $\eta_T^* > \eta_{-A,t_{end}}$. We consider these cases unlikely given the size of the PPP, but our arguments still apply in those situations. The only difference is that we do not know if t^*, the point at which $\Delta_{A,t^*} = 0$, will be finite. Abstracting from these corner cases:

For $t \leq C_1$, we replicate the argument in step 1 and conclude that $\Delta_{A,t} > 0$.

For $t \in (C_1, C_2)$, we have that $\Delta_{A,t}$ is strictly decreasing (since $T_{A,t}$ is decreasing and $T_{-A,t}$ is increasing).

For $t \geq C_2$, we have that T_j is decreasing in η, and therefore: $T_{A,t} < T_{-A,t}$; that is $\Delta_{A,t} < 0$.

Since $\Delta_{A,t}$ is continuous, positive for $t \leq C_1$, strictly decreasing for $t \in (C_1, C_2)$, and negative at $t \geq C_2$, we have that $\Delta_{A,t}$ crosses the axis only once, from above, at some $t^* \in (C_1, C_2)$.

C.7.2 Part 2: IV Estimation

We can write the β_{IV} (the probability limit of the IV estimator in this setting as

$$\beta_{IV} = ATT + \frac{\mathbb{E}_\mu\left[(T_b - ATT)s_b^2\right]}{\mathbb{V}_\mu(s_b)} = ATT + \frac{\text{Cov}_\mu((T_b - ATT)s_b, s_b - 1)}{\mathbb{V}_\mu(s_b)}$$

What we've shown in Part 1 is that $T_A > T_{-A}$ for $t < t^*$ and $T_A < T_{-A}$ for $t > t^*$. Therefore, for $t < t^*$, $T_b > ATT$ iff $s_b > 1$ and $T_b < ATT$ iff $s_b < 1$, which implies that

$$\text{Cov}_\mu((T_b - ATT)s_b, s_b - 1) > 0$$

and the opposite for $t > t^*$.

D. LATE, ATT, and the Targeting Channel: The Two-Bank Case

To provide a clear illustration of our targeting channel, we present a simple two-bank case. Consider that there are two banks, A and B, that are equivalent in every measure except their PPP capacity s_b, $b \in \{A, B\}$. Note that we must have $s_B = 1 - s_A$, since both banks have the same market share. Assume without loss that $s_A > s_B = 2 - s_A$. Let Z_j be equal to 1 if firm j is a client of bank A and 0 otherwise. We consider now the use of Z_j as an instrument for PPP at the firm level.

By assumption, the instrument Z_j is independent of the treatment effects in the population, the shocks in the second stage, and the potential outcomes of the dependent variable conditional on PPP allocation. Moreover, note that if $s_A > s_B$, $\underline{\Omega}_A < \underline{\Omega}_B$ and, therefore, the monotonicity condition is satisfied. In this case, we have that the IV estimation yields the LATE; that is,

$$\begin{aligned}\beta_{F,IV} &= \frac{\text{Cov}(y_j, Z_j)}{\text{Cov}(PPP_j, Z_j)} = \frac{\mathbb{E}[T_j PPP_j | Z_j = 1] - \mathbb{E}[T_j PPP_j | Z_j = 0]}{\mathbb{E}[PPP_j | Z_j = 1] - \mathbb{E}[PPP_j | Z_j = 0]} \\ &= \frac{T_A s_A - T_B s_B}{s_a - s_b} \equiv LATE\end{aligned}$$

Alternatively, we can follow the steps in the derivation of Appendix C.5 to write

$$LATE = ATT + \frac{\text{Cov}(T_j, PPP_j Z_j)}{\text{Cov}(PPP_j, Z_j)} = ATT + 2\frac{s_A}{s_A - s_B}[T_A - ATT] \tag{47}$$

If we further assume that each region has only either bank A or B, but not both, and use Z_r, we have that $\beta_{F,IV} = \beta_{R,IV}$, even if each region has a different distribution of firms and thus there is regional targeting in our sample.

Although the bank-IV strategy can recover the LATE, that is, the effect of the PPP on firms that would have received PPP with bank A, but that did not receive from bank B, the LATE is *systematically* different from the ATT in our model, since $T_A \neq ATT$. This means that we can recover the causal effect of the PPP from the bank-IV, but not the estimate the effect of the program. Intuitively, if banks select firms for a given variable that is negatively correlated with their treatment effect, then firms that are selected by bank A, but not bank B, are exactly those that have the highest treatment effect. Under the same conditions as in Lemma 4, we can show that early in the program, $s_A > s_B$ implies that $ATT < LATE$, whereas at the end of second round of the program we have a situation where $ATT > LATE$.