For Online Publication

Appendix A. Data Appendix

We draw upon microdata made available through the Small Business Administration (SBA) and the Department of Treasury containing all PPP loans, allowing us to observe all loans approved under the program.[1] For all loans, the data include loan amount, lender name, the borrower's self-reported industry and corporate form, workers covered by the loan, and some demographic data on firm owners, including borrower name. Our targeting analysis and bank exposure research design use data for all loans aggregated to either the regional or local geography level.

We merge this data set with the Reports of Condition and Income (Call Reports) filed by all active commercial banks as of 2020:Q1. Specifically, we use a bigram string comparator for the lender name to match the lender names in the PPP data set to commercial and savings banks in the Call Reports data set. The main challenge in this process is that many lender names are matched to multiple distinct banks with the same legal name. For instance, there are fifteen distinct banks whose legal name is "Community State Bank" filing a call report in the first quarter of 2020. We address this issue by assigning each loan made by these distinct banks with similar legal names to the similarly-named bank with the branch that is closest to the zip code where the loan was made.[2] We are able to match 4,370 bank participants in the PPP program to the Call Reports data set. We did not match 795 commercial and savings banks that filed a Call Report in the first quarter of 2020. We assume that these banks did not participate in the PPP program and made no PPP loans. Overall, lenders in the PPP sample that we matched to the Call Report account for 90.5% of all loans disbursed under the PPP.

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

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

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

of the loans made by Fintech lenders.

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

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

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

We use the Homebase data in our bank exposure and matched sample analysis to measure the impact of PPP funding on employment and business shutdowns. To broaden this analysis, we supplement the Homebase data with three additional data sources. First, we obtain county-by-week initial unemployment insurance claims from state web sites or by contacting state employment offices for data. We use initial unemployment insurance claims as a measure of flows into unemployment. Second, we supplement the Homebase data with data from Womply, a company that aggregates data from credit card processors. The Womply data includes aggregate card spending at small businesses at the county-industry level, defined by the location where a transaction occurred. We aggregate the Womply dataset at the county level to harmonize the level of aggregation we use across different analyses. We find similar results when we use the Womply dataset at the county-industry level. Small businesses are defined as businesses with revenues below SBA thresholds. We complement these data sources with additional county-level employment data from Opportunity Insights, which are described in detail in Chetty et al. (2020).[3] The employment rates are based on employment data that Opportunity Insights obtained from Paychex, Earnin, Intuit, and Kronos. The data are at the county/week level and span the period from January 2020 to the end of August.

We obtain counts of COVID-19 cases by county and state from the Center for Disease Control and use data on the effectiveness of social distancing from Unacast. Unacast provides a social distancing scoreboard that describes daily changes in average physical mobility. Unacast measures the change in average distance travelled using individuals' GPS signals. The data are available on a daily basis at the county level. We obtain information on the effective dates of statewide shelter-in-place orders from the New York Times.[4]

To understand the mechanisms underlying our results, we draw on data from the Census Bureau's Small Business Pulse Survey (SBPS), launched within seven weeks of the national emergency declaration in March (Buffington et al., 2020). To obtain real-time information tailored towards small businesses, the SBPS was run weekly from April 26th to June 27th with businesses contacted via email based on the Census Bureau's Business Register, which is popu-

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

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

lated using responses to the Economic Census across the 50 states (and D.C. and Puerto Rico).[5] Furthermore, the SBPS focuses on businesses with receipts that are greater than or equal to $1,000 but retain 500 employees or fewer. This sampling frame closely fits the target population for the PPP. Each week, the sample weights are adjusted to maintain representativeness.

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

[5] We focus on the first phase of the survey that spans the period from April 26th to June 27th because the second phase of the survey only started mid-August and ended in October after the PPP ended.

Appendix B. PPPE Statistics and Heterogeneous Demand for PPP Loans

This Appendix provides further details about some statistical properties of our PPPE measure of local exposure to banks that over- or underperformed in the deployment of PPP funds and further investigates the impact that local differences in demand for PPP funds might have on the empirical analyses.

Figure B.1 is a histogram of the distribution of bank PPPE at the end of the first round and during the second round of the program. Both histograms show a wide dispersion of relative performance. The second round histogram shows a shift in PPPE, with some banks that barely participated in the first round considerably improving their performance subsequently.

Figure B.2 provides a description of the spatial distribution of PPPE. Panel A plots the spatial distribution of county-level PPPE across the United States and Panels B and C describe the spatial distribution of ZIP-level PPPE in Chicago and New York City, respectively. Panel A indicates that the Midwest and New England regions were exposed to banks that performed well in delivering PPP during the first round whereas Western areas were less exposed to banks that performed well in deploying PPP. Panels B and C show that, even within cities, there could be relevant differences in local exposure to banks that performed well. The less affluent areas of the city of Chicago, such as the South Side of the city, were exposed to banks that performed well, whereas firms in the downtown area were served by banks that did not do so well in deploying PPP funds. On the other hand, in New York City, less affluent and less densely populated areas were served by banks that performed worse in the first round relative to banks elsewhere in the city. These differences in exposure between Chicago and New York City probably reflect the heterogenous role of exposure to bank PPP performance.

Figure B.3 is a survival function of the time to receive PPP funds after partitioning firms in the sample based on their local exposure to ZIP-PPPE. The figure shows that exposure to underperforming PPPE banks is associated with meaningful differences in when borrowers could access funds. Only 25% of all PPP borrowers located in ZIP codes whose banks underperformed obtained PPP approval prior to the end of the first round. By contrast, approximately 42% of all PPP borrowers in ZIP codes whose banks overperformed had access to funds in the first round.

Figure B.4 shows that we find a similar relationship between our state exposure to PPPE and the share of firms that report receiving and requesting PPP funds at the end of the first round when we measure bank PPP performance based on the share of the total amount of PPP and total amount of small business loans disbursed by each bank, rather than the share of the number of PPP and small business loans by each bank.

Table B.1 shows summary statistics of the PPPE variable and other outcome variables in the paper.

We believe the differences in PPPE mostly capture initial variation in banks' abilities to process PPP loans. Here, we present some supplementary evidence to support this conjecture. In Appendix Table B.2, we show that the ability of banks to include information about the program in their websites and to receive online applications is positively associated with bank performance.

In Table B.3, we repeat the empirical analysis of Table 4 but include county×industry fixed effects rather than state×industry fixed effects. By including county×industry fixed effects, we ensure that our results compare firms that are exposed to banks with weak performance during the first round with firms in the same county and in the same industry that were close to banks that were quick to disburse PPP loans and had a strong performance during the first round. This empirical strategy further assuages concerns that PPPE captures differences in demand, since it is comparing firms that are exposed to very similar economic and health conditions (same county and same industry) but that happened to be located in areas close to banks that were swift in processing PPP applications.

To further allay concerns about the possibility that PPPE captures differences in demand for PPP loans across regions, we created a predicted PPPE measure that uses variation in supply-side restrictions that limit banks' ability to process PPP loans. Panel A of Figure B.5 suggests that banks that had greater labor intensity ratios performed relatively better in deploying PPP loans during the first round. Panel B of Figure B.5 shows that banks with no prior relationship over the past three years performed worse, on average, than banks that originated at least one SBA guaranteed loan over the past three years. Moreover, the intensity of the SBA relationship is also associated with improved bank performance. Banks with a greater fraction of their small business lending in the form of SBA-guaranteed loans performed relatively better than banks that only sporadically interacted with the SBA.

The generous terms of the PPP and the broad eligibility criteria likely meant that demand for the program was very high almost everywhere. Our evidence in Figure 3 suggests that, even at the end of the first round, approximately 75% of establishments surveyed in the Census Pulse Survey had applied for PPP funds. Our conjecture is that differences in access to banks that quickly processed PPP were the main factor explaining differences in timing of receipt of PPP funds. Because of capacity constraints and difficulties in processing the sudden influx of PPP applications, banks prioritized their existing relationships. Small businesses that had relationships with banks that were swift in deploying PPP obtained access to PPP funds quickly.

Other businesses that were close to or were in relationships with banks that were unable or unwilling to process their applications either waited until their bank was able to process their application or found another bank that was willing and able to process their application.

To provide further evidence supporting this conjecture, we obtained access to a proprietary dataset from a Chicago community bank. This bank performed well in delivering PPP to their clients, i.e., it had a high bank PPPE. In this proprietary dataset, we are able to observe the date in which each PPP loan was fully funded and, more importantly, we also learn about the location of each small business that received a PPP loan from that bank and whether it is a new or old client of the bank. In Figure B.6, we show that this bank delivered funds to its existing clients much quicker than it did to small businesses that opened an account with the bank after the announcement of the program. Consistent with the idea that new clients were likely in relationships with other banks that were not able to process their applications quickly, in Table B.4 we find that the new clients that were served by this community banks came from areas with lower PPPE, lower predicted PPPE, and greater exposure to Wells Fargo. We also find that these new clients came from farther away and were smaller than existing clients of the community bank. This pattern suggests that these new clients likely were in relationships with other banks that were not processing PPP applications quickly. Alternatively, their banks did not prioritize theses applications, and these firms had to search for other, more distant banks to obtain access to PPP funds.

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

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

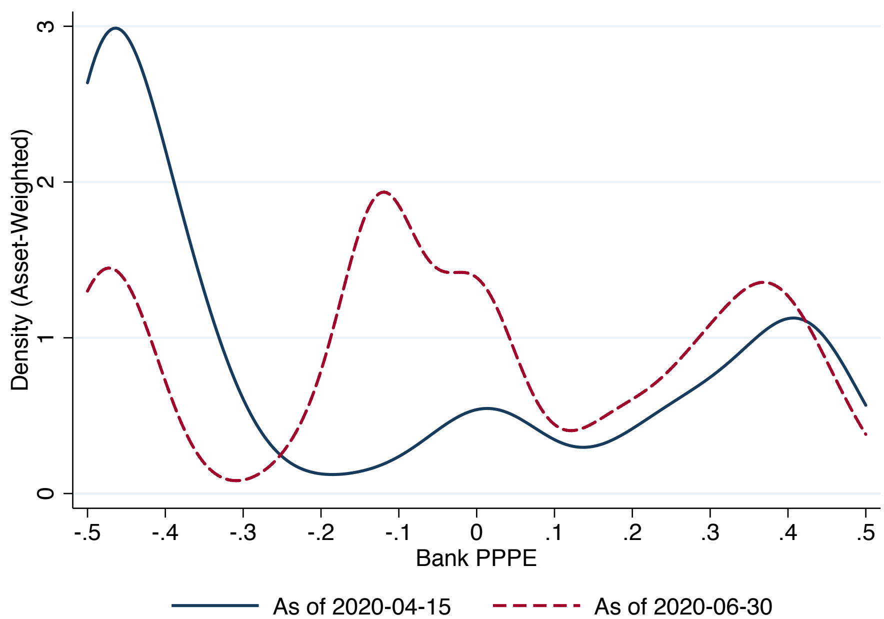
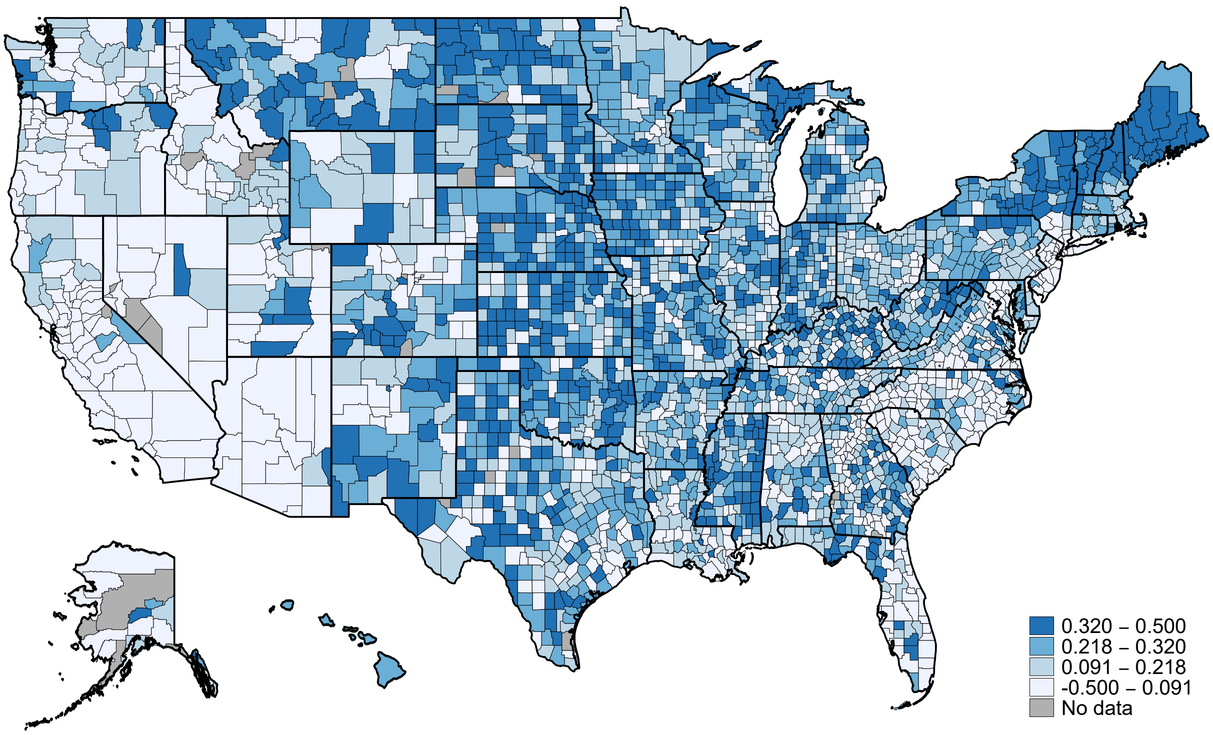


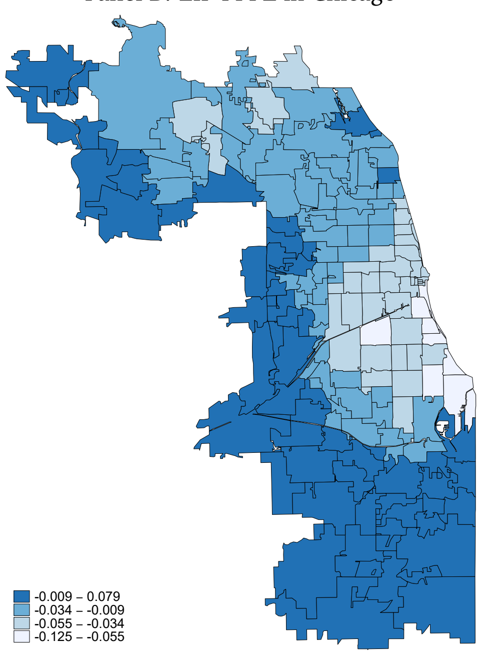
Figure B.2: Map of Exposure to PPPE

Figure B.2 shows the spatial distribution of our PPPE measure across the U.S. Panel A shows the average exposure of each county to the PPPE. County exposure to PPPE is computed as the average of the PPPE of each bank with a branch presence in the county. The PPPE of each bank is weighted by the share of the number of branches of the bank in the county as of June 30th, 2019. Panels B and C show the average exposure of each zip code in Chicago and New York City, respectively. Zip exposure to PPPE is the weighted average of bank PPPE during the first round at the ZIP level. The weights are defined by the share of the number of branches of each bank within 10 miles of the center of the respective ZIP. Data is from the SBA, Call Reports, and FDIC's Summary of Deposits.

Panel A: County PPPE



Panel B: ZIP PPPE in Chicago



Panel C: Zip PPPE in New York City

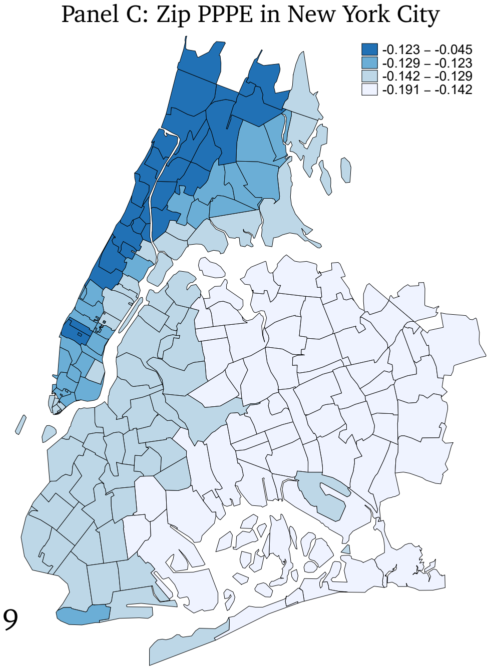


Figure B.3: Kaplan-Meier Survival Functions

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

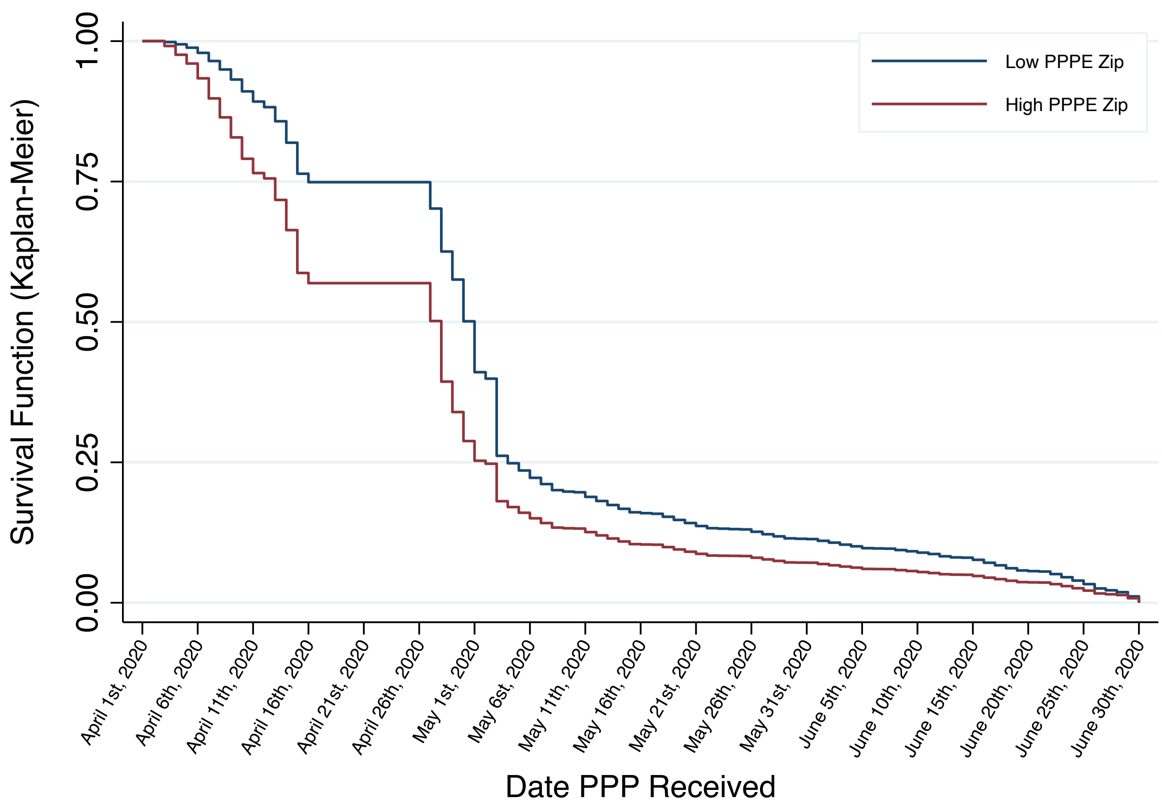
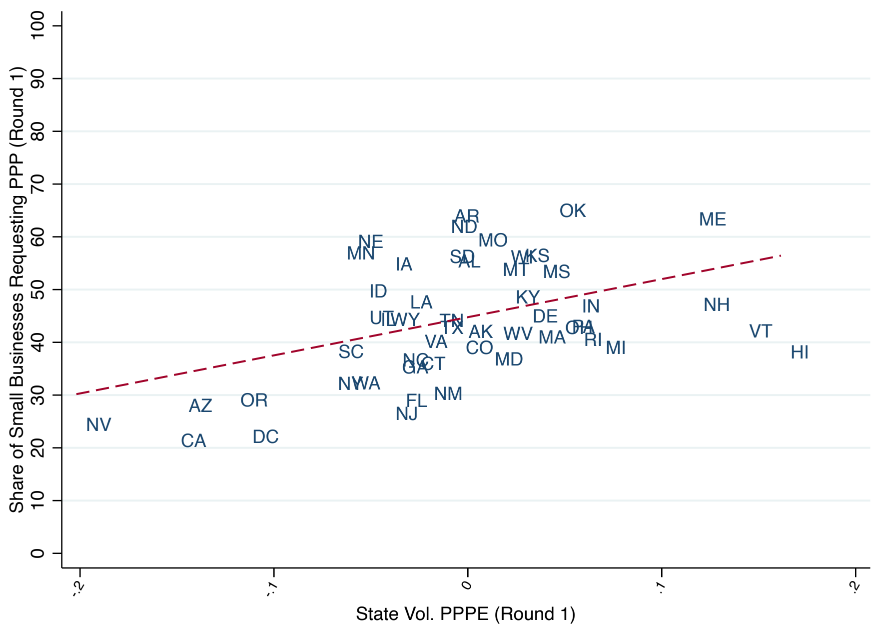


Figure B.4: State Exposure to PPPE and PPP per Establishment

Figure B.4 are scatterplots of state exposure to the volume-based PPPE at the end of Round #1 of the PPP and the percentage of firms reporting receiving PPP funds at the end of the first round (Panel A) and of state exposure to the volume-based PPPE at the end of Round #1 and the percentage of firms in each state reporting requesting PPP funds (Panel B). Data comes from the Census Bureau Small Business Pulse Survey, SBA, Call Reports, Summary of Deposits, and County Business Patterns.

Panel A: State Exposure to Volume-based PPPE and Share Receiving PPP



Panel B: State Exposure to Volume-Based PPPE and Share Requesting PPP

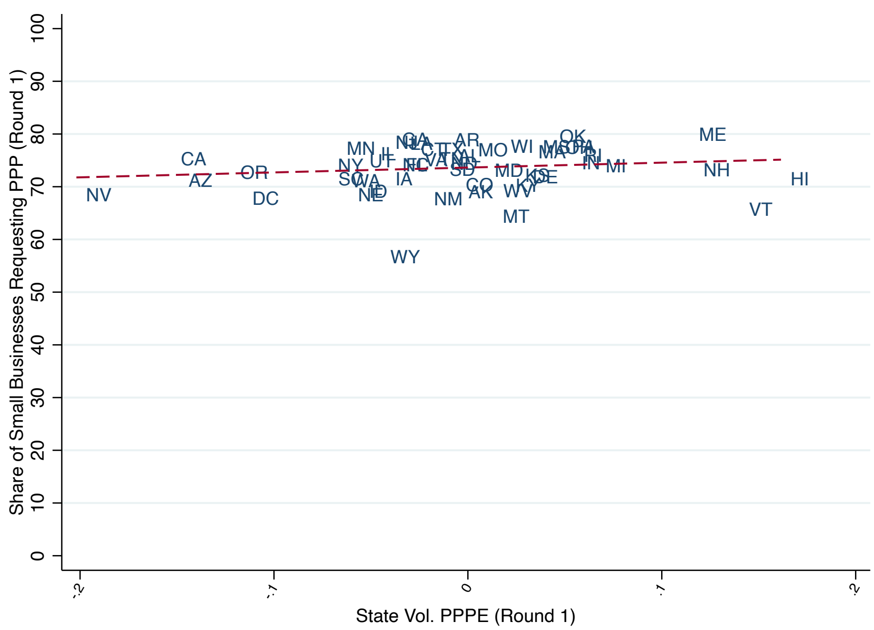


Figure B.5: Bank PPPE and Supply Side Factors

Figure B.5 plots the average bank PPPE of each bank decile based on partitioning banks on their ratio of wage expenses to data processing expenses (Panel A) and on the ratio between the number of SBA loans and small business loans originated in the past three years (Panel B). Data is obtained from the SBA and call reports

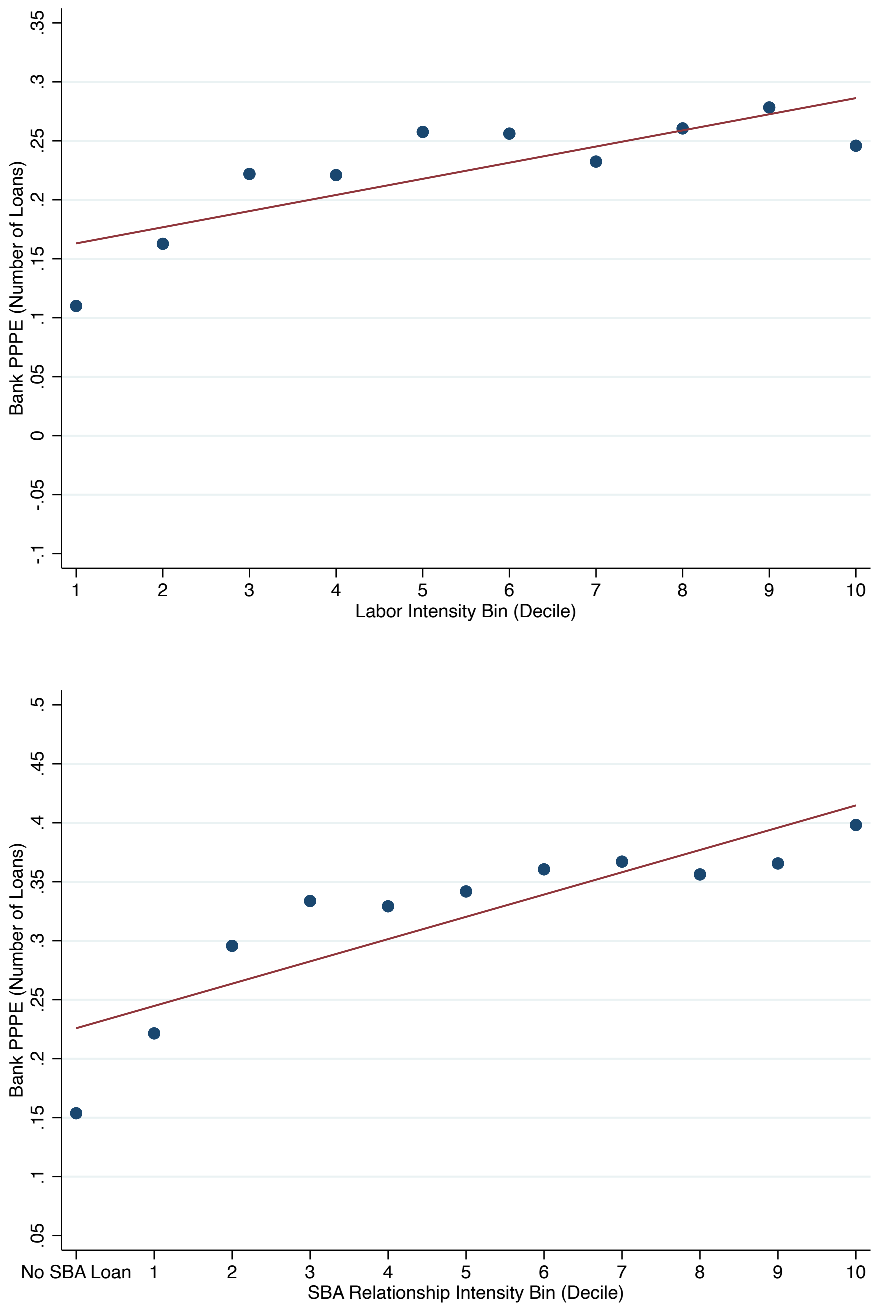


Figure B.6: Kaplan-Meier Survival Functions by Bank Relationship

Figure B.6 plots Kaplan-Meier survival functions of the time that took the PPP applicants of a small community bank to receive PPP funds. The blue line represents the survival function for the group of firms that represented a new relationship for the bank and the red line plots the survival function for the group of firms that had a banking relationship with the bank that had begun prior to March 2020. Data is obtained from the SBA, call reports, and from a proprietary dataset obtained from a small community bank.

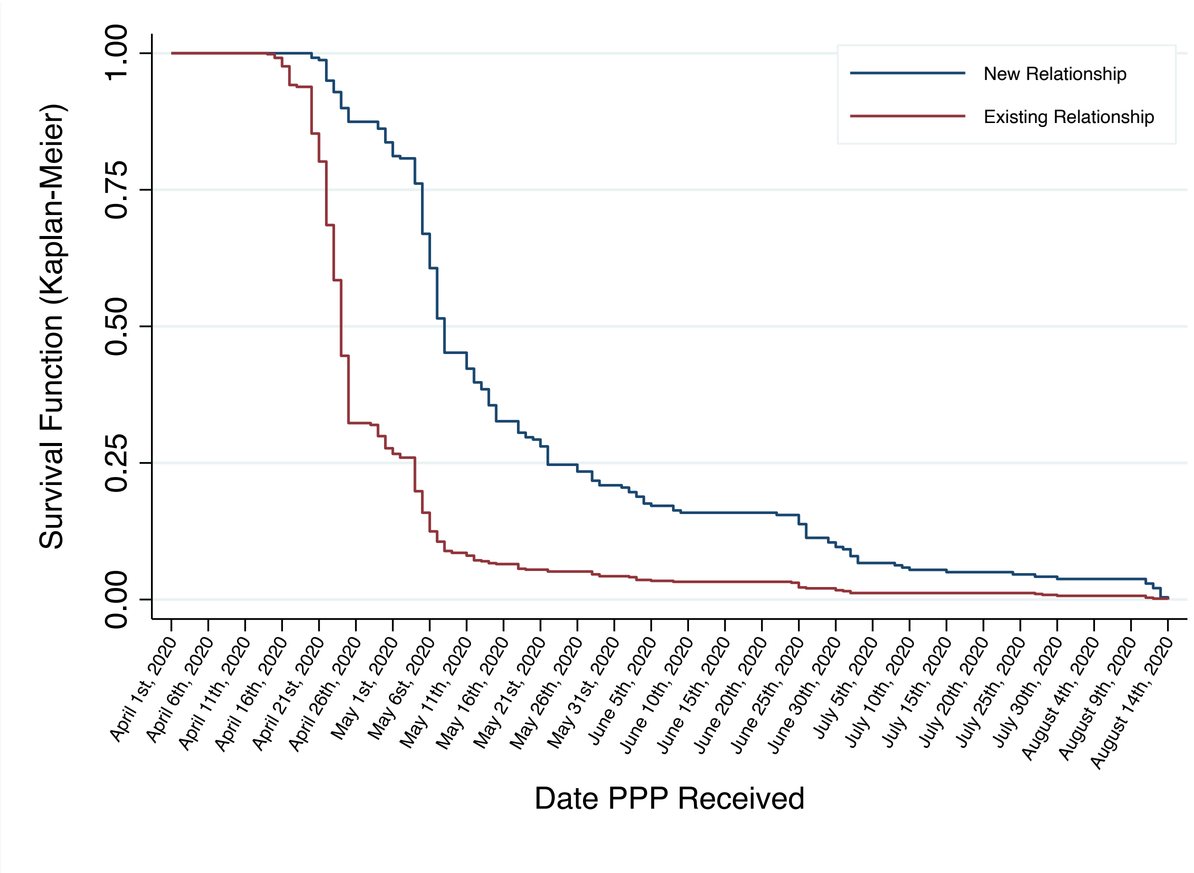


Table B.1: Summary Statistics for Bank Exposure Analysis

Table B.1 reports summary statistics for the analyses in Tables 5 and 6. See the notes to those tables for variable definitions.

	Mean	10th	Median	90th	N
Outcomes					
Δ Bus. Shutdown (ZIP/Week)	-0.08	-1.00	0.00	0.00	819834
Δ Hours Worked (ZIP/Week)	0.19	-0.13	0.06	0.73	819834
Δ Nbr. Employees	0.22	-0.14	0.06	0.78	819834
Δ UI Claims (Cnty/Week)	-1.78	-4.05	-1.55	0.01	46092
Δ Small Bus. Rev. (Cnty)	0.19	-0.05	0.18	0.47	43930
Δ OI Emp. (Cnty/Week)	-0.01	-0.11	-0.00	0.09	17112
Exposure					
PPPE in Round 1 (ZIP)	0.02	-0.18	0.01	0.24	35645
PPPE in Round 1 (Cnty)	0.05	-1.28	0.20	1.20	2111
Cross Sectional Characteristics					
Pre-PPP Soc. Dist. Index (ZIP)	-0.01	-1.03	-0.12	1.21	35645
Pre-PPP Cases per cap $\times 10^3$ (ZIP)	0.05	0.00	0.02	0.09	35645
Pre-PPP Deaths per cap $\times 10^6$ (ZIP)	0.99	0.00	0.00	1.66	35645
Pre-PPP Soc. Dist. Index (Cnty)	-0.30	-0.47	-0.34	-0.09	2081
Pre-PPP Cases per cap $\times 10^3$ (Cnty)	0.02	0.00	0.00	0.04	2110
Pre-PPP Deaths per cap $\times 10^6$ (Cnty)	0.37	0.00	0.00	0.00	2110

Table B.2: PPP in Bank Websites and PPP Performance

Table B.2 reports results of OLS regressions examining the relation between bank PPPE and the availability of information about applications to the PPP program in each bank's internet websites as of April 10th, 2020. The dependent variables are *PPP info*, *Receiving PPP applications*, and *Online Application*. *PPP info* is an indicator variable that takes the value of one if the bank provides any information about the PPP program in its internet website. *Receiving PPP applications* is an indicator variable that takes the value of one if the bank states in its website that is receiving applications to the PPP program as of April 10th. *Online Application* is an indicator variable that takes the value of one if the bank receives online applications through its internet website. *Bank PPPE* is the bank PPPE measured as of the end of round one of the PPP. *Bank Predicted PPPE* is the predicted bank PPPE. The predicted values of bank PPPE are obtained from estimating the empirical specifications of column (8) of table 2. All specifications include controls for size deciles. Data about the PPP offerings in bank's websites was hand-collected from banks' websites during April 9th and April 10th, 2020. Standard errors are presented in parentheses, and are clustered at the level of the state. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	PPP info		Receiving PPP applications		Online Application	
Bank PPPE	0.159***	0.145***	0.154***	0.139***	0.041**	0.032
	(0.025)	(0.027)	(0.024)	(0.025)	(0.019)	(0.020)
Observations	4857	4856	4857	4856	4857	4856
Adjusted R^2	0.184	0.196	0.145	0.153	0.055	0.061
State Fixed Effects	No	Yes	No	Yes	No	Yes
Size Deciles	Yes	Yes	Yes	Yes	Yes	Yes
	PPP info		Receiving PPP applications		Online Application	
Bank Predicted PPPE	0.832***	0.815***	0.807***	0.788***	0.426***	0.420***
	(0.094)	(0.099)	(0.090)	(0.094)	(0.059)	(0.058)
Observations	4852	4851	4852	4851	4852	4851
Adjusted R^2	0.194	0.207	0.156	0.164	0.064	0.070
State Fixed Effects	No	Yes	No	Yes	No	Yes
Size Deciles	Yes	Yes	Yes	Yes	Yes	Yes

Table B.3: ZIP PPPE in Round 1 and PPP Reallocation (County×Year Fixed Effects)

Table B.3 shows the correlation between PPPE and the fraction of establishments receiving PPP loans from different sources in the first and second rounds of the program. The left-hand-side variable in column (1) is the fraction of establishments within a ZIP and 2-digit NAICS industry that received PPP in the first round in Panel A and in both rounds in Panel B. Left-hand-side variables in other columns represent a decomposition of the dependent variable in column (1) into the fraction of establishments within a ZIP and 2-digit NAICS industry that received PPP from local banks, non-local banks, credit unions, FinTech companies, and other nonbanks. *ZIP PPPE (Round 1)* is the weighted average of bank PPPE during the first round at the ZIP level. The weights are defined by the share of the number of branches of each bank within 10 miles of the center of the respective ZIP. ZIP PPPE is standardized to permit coefficients to be interpreted as the effect of a one-standard-deviation increase in ZIP PPPE and observations are weighted by the number of eligible establishments in each zip-industry pair. *Predicted PPPE* is the weighted average of predicted bank PPPE during the first round at the ZIP level. The predicted values of bank PPPE are obtained from estimating the empirical specification of column (8) of table 2. The weights are defined by the share of the number of branches of each bank in the zip code or within 10 miles of the center of the respective ZIP. Eligible establishment counts equal all establishments in a ZIP less an estimate of the share of establishments with more than 500 employees (which are not eligible for PPP) plus an estimate of the number of proprietorships likely to apply for PPP. All regressions include county-by-NAICS fixed effects. Standard errors are presented in parentheses, and are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

Panel A: Allocation in Round 1

	(1)	(2)	(3)	(4)	(5)	(6)
		PPP Loans Relative to All Establishments by Lender Source				
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Zip PPPE (Round #1)	2.237***	2.714***	-0.245**	-0.090***	-0.110***	-0.032
	(0.229)	(0.239)	(0.115)	(0.026)	(0.016)	(0.019)
Observations	239411	239411	239411	239411	239411	239411
Adjusted R^2	0.477	0.470	0.225	0.315	0.374	0.192
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Predicted PPPE	0.775***	0.786***	0.034	-0.008	-0.037**	-0.002
	(0.185)	(0.250)	(0.139)	(0.034)	(0.014)	(0.004)
Observations	239411	239411	239411	239411	239411	239411
Adjusted R^2	0.475	0.466	0.224	0.315	0.373	0.192
County×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Allocation in Round 1 and 2

	(1)	(2)	(3)	(4)	(5)	(6)
		PPP Loans Relative to All Establishments by Lender Source				
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Zip PPPE (Round #1)	0.321	2.075***	-0.744**	-0.264***	-0.612***	-0.133***
	(0.418)	(0.468)	(0.335)	(0.064)	(0.127)	(0.048)
Observations	239411	239411	239411	239411	239411	239411
Adjusted R^2	0.466	0.483	0.274	0.413	0.258	0.215
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
Predicted PPPE	0.150	0.345	0.104	-0.030	-0.249**	-0.021*
	(0.291)	(0.443)	(0.323)	(0.092)	(0.105)	(0.012)
Observations	239411	239411	239411	239411	239411	239411
Adjusted R^2	0.466	0.482	0.273	0.412	0.257	0.215
County×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table B.4: Differences in Characteristics of PPP Applicants With and Without Previous Bank Relationship

Table B.4 uses a proprietary dataset obtained from a small community bank to examine the characteristics of firms that received PPP funds from that bank. We examine the differences in characteristics of PPP applicants with and without a previous bank relationship. We define a new bank relationship as a firm that did not have any account with the bank prior to the start of the pandemic. Data is from a proprietary dataset of a small community bank, SBA, and Call Reports.

	New Bank Relationship			Existing Bank Relationship				
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Diff	t-stat
Zip PPPE (Round #1)	-0.0116	0.0884	239	0.0280	0.0944	584	-0.0396	-5.568
Predicted PPPE (Round #1)	0.237	0.0245	239	0.248	0.0264	585	-0.0115	-5.776
Zip Exposure to Wells Fargo	0.00750	0.0275	239	0.00390	0.0183	584	0.00360	2.210
Distance to Bank	43.07	210.8	239	15.10	93.18	585	27.97	2.641
PPP Loan Amount	59670	116015	239	192471	418803	585	-132801	-4.825
Number of Employees	7.864	14.17	236	19.58	42.51	569	-11.72	-4.140

Appendix C. Wells Fargo Appendix

This appendix repeats the main empirical analyses of the paper using the local exposure to Wells Fargo as our main variable of interest. Wells Fargo did not accept PPP applications out of concerns that it might breach the asset cap restriction imposed by the Office of the Comptroller of the Currency in the aftermath of the 2016 fake accounts scandal. On April 8th, the Federal Reserve issued a press release exempting PPP loans from counting toward the total assets formula used to determine compliance with the asset cap restriction. Given that this restriction was externally imposed and Wells Fargo is a very large depository institution in the United States, we use each local area exposure to Wells Fargo's branches as an alternative measure of exposure to supply-side difficulties in accessing the PPP. Table C.1 and Figure C.1 show that local areas that were more exposed to Wells Fargo branches received fewer loans during the first round. Figure C.2 and Table C.2 use local differences in exposure to Wells Fargo to examine the effect of the program on employment outcomes using Homebase data. Figure C.3 and Table C.3 use local differences in exposure to Wells Fargo to examine the effect of the program on employment outcomes using county unemployment filings, Womply, and Opportunity Insights Tracker data. Overall, the estimated results when we use this alternative empirical strategy are quantitively and qualitatively similar to those presented in the paper.

Figure C.1: Exposure to Wells Fargo and PPP Coverage over Time

Figure C.1 plots binned scatter plots of the average fraction of small business establishments that received a PPP loan versus exposure to Wells Fargo branches at the Zip level. Eligible establishment counts equal all establishments in a ZIP less an estimate of the share of establishments with more than 500 employees (which are not eligible for PPP) plus an estimate of the number of proprietorships likely to apply for PPP. Both variables are demeaned at the state level to present the within-state relationship. Data come from SBA, Call Reports, Summary of Deposits, and County Business Patterns.

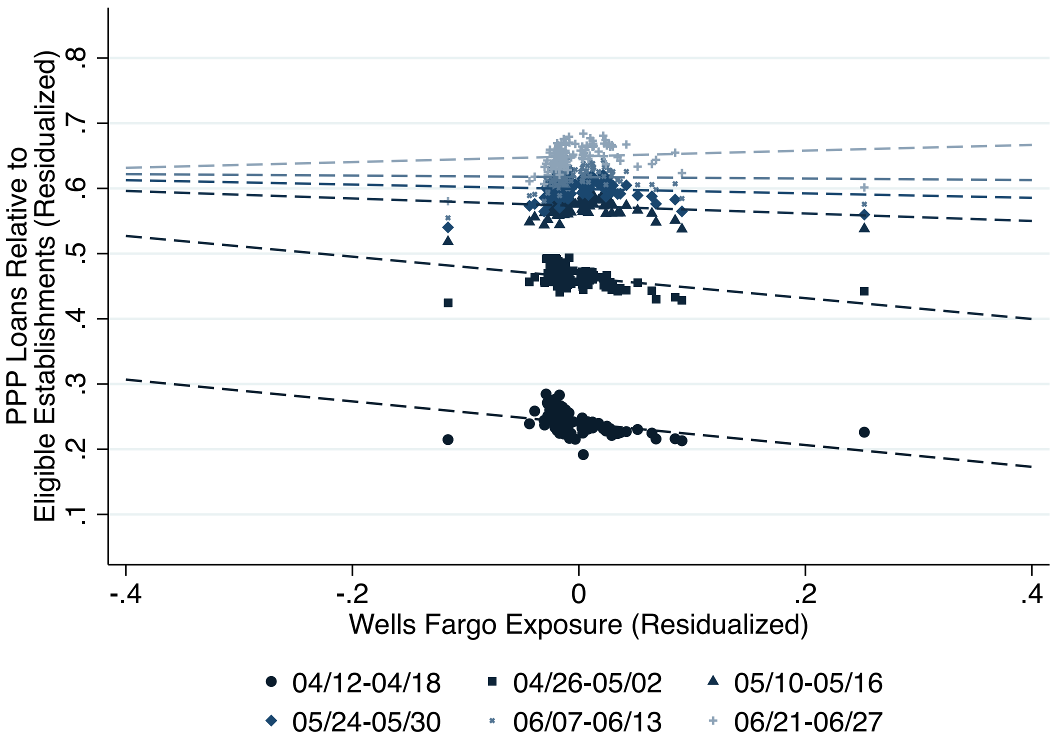


Figure C.2: Wells Fargo Exposure and Evolution of Homebase Employment Outcomes

The panels on the left of Figure C.2 shows the ratio of hours worked over time, the percent of businesses shut down, and the ratio of number of employees splitting the sample into firms with above- and below-median exposure to Wells Fargo branches. The panels on the right of Figure C.2 plot coefficients and standard errors of regressions investigating the impact of exposure to Wells Fargo on employment and firm outcomes. The regressions are similar to those of Figure 8 in the main paper. Data are from SBA, Homebase, County Business Patterns.

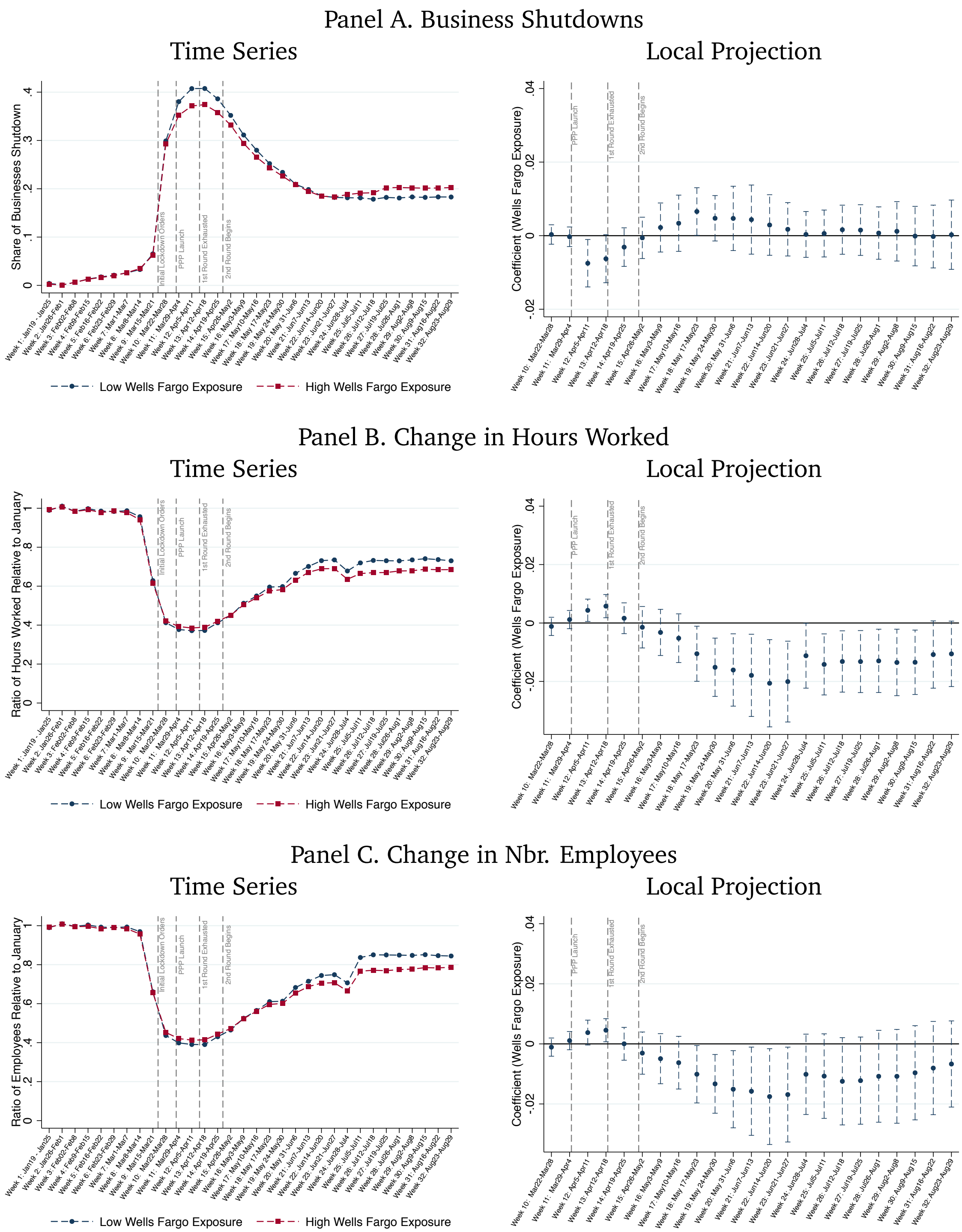
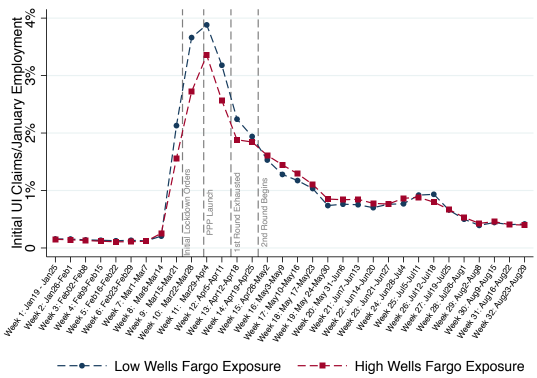


Figure C.3: Wells Fargo Exposure and Other Local Labor Market and Economic Effects

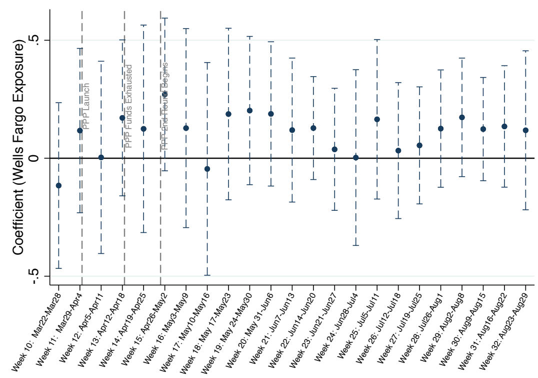
The panels on the left of Figure C.3 show the evolution of the ratio of weekly initial unemployment filing claims at the county level and total county employment (Panel A), the change in aggregate small business revenue at the county level relative to January (Panel B), and the ratio of county employment relative to January (Panel C) splitting the sample into firms with above- and below-median exposure to Wells Fargo branches. The panels on the right of Figure C.3 plot coefficients and standard errors of regressions investigating the impact of exposure to Wells Fargo on the same outcomes. The regressions are similar to those of Figure 10 in the main paper. Data are from SBA, Homebase, County Business Patterns.

Panel A. UI Claims

Time Series

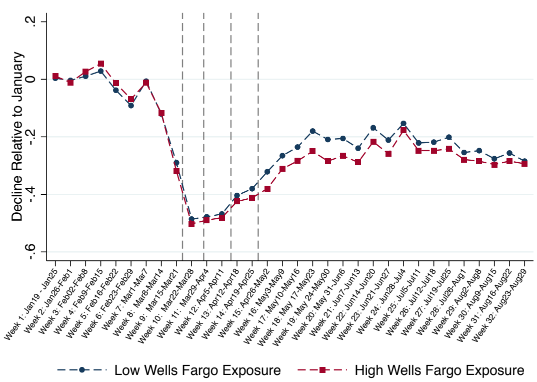


Local Projection

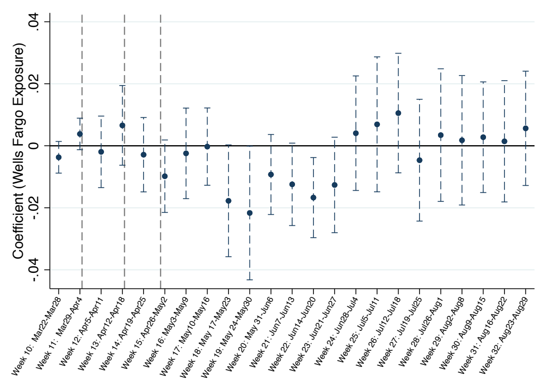


Panel B. Small Business Revenues

Time Series

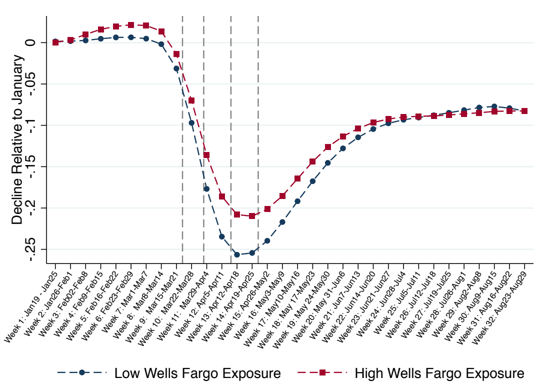


Local Projection



Panel C. County Employment

Time Series



Local Projection

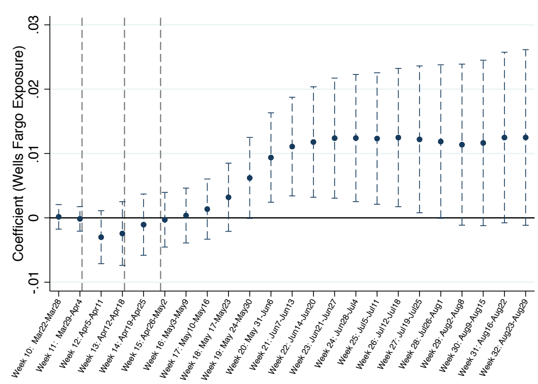


Table C.1: Wells Exposure in Round 1 and PPP Reallocation across Funding Sources

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

Panel A: Allocation in Round 1

	(1)	(2)	(3)	(4)	(5)	(6)
		PPP Loans Relative to All Establishments by Lender Source				
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
% Wells (Zip 10 miles)	-0.017***	-0.014***	-0.004***	0.000	0.001***	0.000
	(0.004)	(0.004)	(0.001)	(0.001)	(0.000)	(0.000)
Observations	251511	251511	251511	251511	251511	251511
Adjusted R^2	0.356	0.329	0.126	0.166	0.285	0.043
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Allocation in Round 1 and 2

	(1)	(2)	(3)	(4)	(5)	(6)
		PPP Loans Relative to All Establishments by Lender Source				
	PPP/Est (%)	Local Banks	Non-Local Banks	Credit Unions	FinTech	Nonbanks
% Wells (Zip 10 miles)	0.008*	0.002	-0.002	0.002	0.006***	0.001***
	(0.005)	(0.004)	(0.003)	(0.002)	(0.002)	(0.000)
Observations	251511	251511	251511	251511	251511	251511
Adjusted R^2	0.399	0.392	0.187	0.219	0.194	0.090
State×Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table C.2: Wells Exposure and Homebase Employment Outcomes

Table C.2 reports the results of OLS regressions examining the relation between exposure to Wells Fargo and the difference between a firm's average employment outcomes in the two weeks prior to the launch of PPP and the firm's outcomes in each of the following months. The left-hand-side variable in Panel A, Δ *Bus. Shutdown*, is the difference between the firm's shutdown status in a week and its average shutdown status in weeks 10 and 11, where shutdown status takes a value of one if the business reported zero hours worked over the entire week. The left-hand-side variable in Panel B, Δ *Hours Worked*, is the difference in the ratio of hours worked in each establishment in a week and the average ratio of hours worked in that establishment in weeks 10 and 11. The ratio of hours worked in each establishment is measured as the hours worked in that week relative to the hours worked in that same establishment during the last two weeks of January. Δ *Nbr. Employees*, is the difference in the ratio of the number of employees in each establishment in a week and the average ratio of number of employees in that establishment in weeks 10 and 11. The ratio of number of employees in each establishment is measured as the number of distinct employees that worked in the establishment in that week relative to the number of distinct employees working in that same establishment during the last two weeks of January. *% Wells* is the share of the number of branches in the zip code or within 10 miles of the center of the ZIP that belongs to Wells Fargo. *I(Month='M')*, where $M = \{April, May, June, July, August\}$ are indicator variables for the weeks that span those respective months. Other control variables include interactions between the median household income, social distance index, COVID cases per capita and deaths per capita measured as of week 9 interacted with the indicator variables for April, May, June, July, and August and controls for the average tier 1 capital and core deposit ratios of all banks within the zip code or within a 10 miles radius of the zip code also interacted with the indicator variables for April, May, June, July, and August. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ Bus. Shutdown		Δ Hours Worked		Δ Nbr. Employees	
% Wells × I(Month=April)	-0.004*	-0.004*	0.002	0.003	0.002	0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
% Wells × I(Month=May)	0.002	0.004	-0.012**	-0.009**	-0.011**	-0.009**
	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)
% Wells × I(Month=June)	-0.002	0.003	-0.021***	-0.018***	-0.017**	-0.016**
	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)
% Wells × I(Month=July)	-0.005	0.001	-0.015**	-0.015***	-0.010	-0.013*
	(0.003)	(0.003)	(0.006)	(0.005)	(0.007)	(0.007)
% Wells × I(Month=August)	-0.006	0.000	-0.014**	-0.013**	-0.007	-0.010
	(0.004)	(0.004)	(0.006)	(0.005)	(0.007)	(0.007)
Observations	819834	819834	819834	819834	819834	819834
Adjusted R^2	0.058	0.602	0.133	0.629	0.110	0.571
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes	No	Yes
Firm Fixed Effects	No	Yes	No	Yes	No	Yes

Table C.3: Wells Fargo Exposure and Local Labor Market and Economic Effects

Table C.3 reports the results of OLS regressions examining the relation between exposure to Wells Fargo and county-level unemployment filings, small business revenue from Womply, and employment growth from Opportunity Insights. Δ *UI Claims* is the difference between the county unemployment filings during a week and the average unemployment filings in the county in weeks 10 and 11. Δ *Small Business Revenue* is the difference between the county aggregate change in small business revenue relative to January and the average change in small business revenue in weeks 10 and 11 relative to January. Aggregate change in small business revenue is from Womply. Δ *OI Emp.*, is the difference between county employment growth relative to January in a week and the average county employment growth relative to January in weeks 10 and 11. The county-level employment data come from Opportunity Insights. *County % Wells* is the share of the number of Wells Fargo branches in the county. *I(Month='M')*, where $M = \{April, May, June, July, August\}$ are indicator variables for the weeks that span those respective months. Other control variables include interactions between the median household income, social distance index, COVID cases per capita and deaths per capita measured as of week 9 interacted with the indicator variables for April, May, June, July, and August and controls for the average tier 1 capital and core deposit ratios of all banks within the county also interacted with the indicator variables for April, May, June, July, and August. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ UI claims		Δ Small Bus. Rev.		Δ OI Emp.	
County % Wells × I(Month=April)	-0.020	-0.003	-0.014***	-0.002	0.002	0.001
	(0.072)	(0.053)	(0.003)	(0.003)	(0.004)	(0.004)
County % Wells × I(Month=May)	-0.006	0.013	-0.023**	-0.005	-0.001	0.002
	(0.074)	(0.058)	(0.009)	(0.008)	(0.004)	(0.004)
County % Wells × I(Month=June)	-0.006	0.025	-0.011	-0.006	-0.004	0.001
	(0.071)	(0.058)	(0.010)	(0.010)	(0.006)	(0.003)
County % Wells × I(Month=July)	-0.094	0.016	-0.003	-0.001	-0.004	0.001
	(0.075)	(0.059)	(0.006)	(0.006)	(0.007)	(0.003)
County % Wells × I(Month=August)	-0.147*	-0.047	0.001	-0.001	-0.003	0.003
	(0.080)	(0.062)	(0.007)	(0.007)	(0.008)	(0.004)
Observations	46092	45533	43886	43863	17112	17112
Adjusted R^2	0.744	0.949	0.489	0.734	0.682	0.878
State×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	No	Yes	No	Yes	No	Yes
County Fixed Effects	No	Yes	No	Yes	No	Yes

Appendix D. Pre-Targeting Appendix

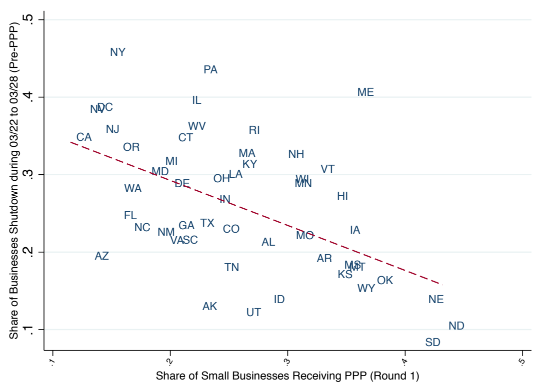
This Appendix expands the pre-targeting analysis of section 5.5 using the Homebase data and other data sources. The purpose of this section is to show that the pre-targeting results presented in the paper generalize to other outcomes and other levels of aggregation. Figure D.1 shows that the Homebase pre-PPP employment outcomes at the state level are worse in states that received a smaller PPP allocation in the first round. Figure D.2 shows that similar patterns hold when we examine other state-level outcomes such as pre-PPP initial unemployment insurance filings, pre-PPP change in small business revenue from Womply, and pre-PPP change in employment from the Opportunity Insights tracker. Figure D.3 shows that the pre-PPP disease spread was greater in states that received a smaller allocation of the PPP during the first round and Figure D.4 shows that states receiving greater PPP allocation in the first round had less pre-PPP social distancing and issued shelter-in-place orders later. Figures D.5 and D.6 show similar relations between pre-PPP employment outcomes and PPP allocation during the first round when we implement the analysis at the county level. Figure D.7 shows that the pre-PPP evolution of Homebase employment outcomes did not vary with the exposure of firms to banks with supply-side constraints. When we partition firms based on deciles of predicted PPPE, we see similar pre-PPP levels of business shutdown, declines in hours worked, and declines in number of workers.

Figure D.1: Pre-PPP Homebase Employment Outcomes and PPP Allocation by State (Round 1)

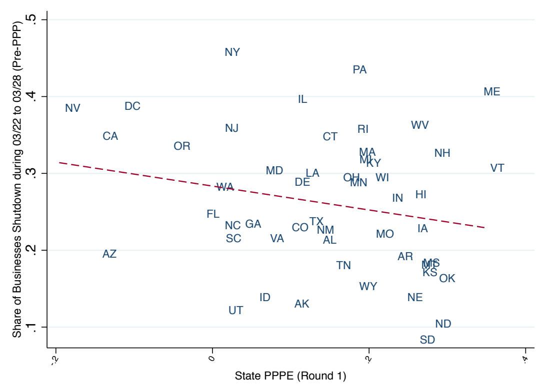
Figure D.1 presents scatterplots of the share of businesses in each state that shutdown in the week of March 22nd to March 28th (Panel A), of the decline in hours worked in each state in the week of March 22nd to March 28th relative to a January Baseline (Panel B), and of the decline in the number of employee counts in each state in the week of March 22nd to March 28th relative to a January baseline (Panel C). The figures on the left plot the pre-PPP state-level employment outcomes from Homebase against the number of PPP loans received by small businesses in each state during the first round of the program divided by the total number of small businesses in the state. The figures on the right plot the same pre-PPP employment outcomes and the state-level PPPE measure. Data is from Homebase, SBA, and Call Reports.

Panel A. Business Shutdowns

Fraction Receiving PPP

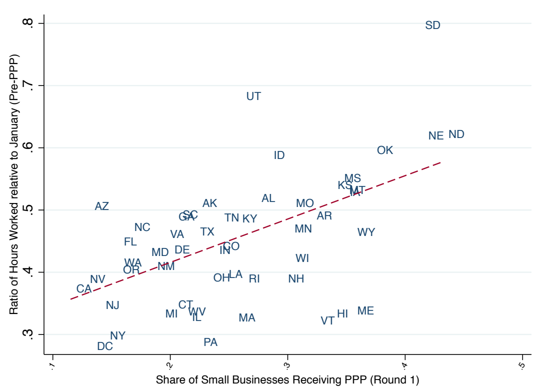


State PPPE



Panel B. Decline in Hours Worked

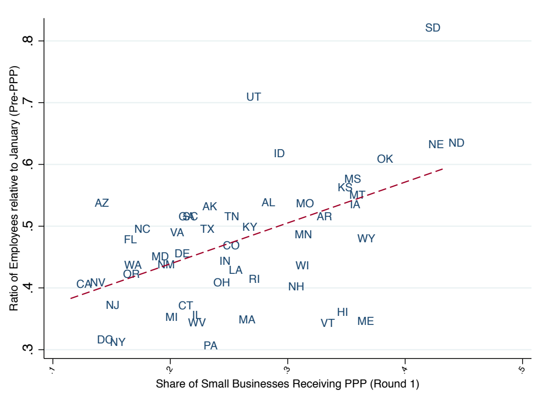
Fraction Receiving PPP



State PPPE

Panel C. Decline in Number of Employees

Fraction Receiving PPP



State PPPE

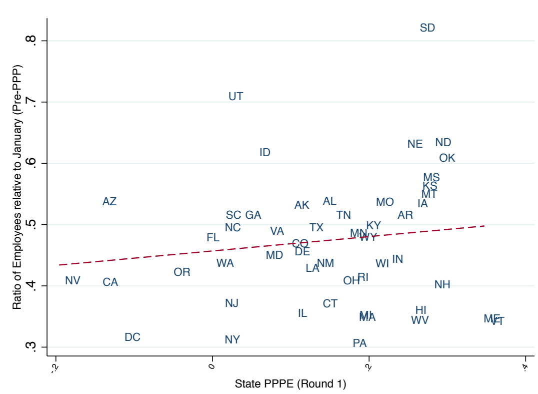
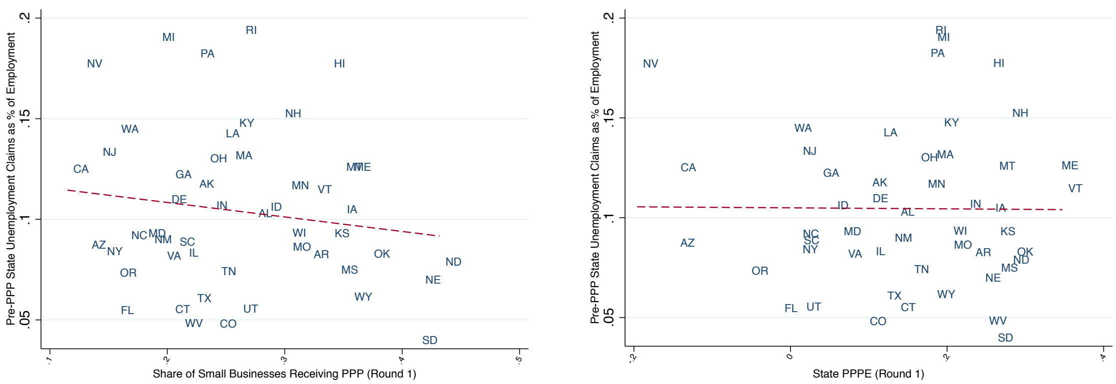


Figure D.2: Other Labor and Economic Outcomes and PPP Allocation by State (Round 1)

Figure D.2 presents scatterplots of the average pre-PPP UI claims as a percentage of employment (Panel A), of the average decline in small business revenues at the state level from Womply (Panel B), and of the average decline in employment at the state level from OI (Panel C). The figures on the left plot the pre-PPP outcomes against the number of PPP loans received by small businesses in each state during the first round of the program divided by the total number of small businesses in the state. The figures on the right plot the same pre-PPP outcomes and the state-level PPPE measure. Data is from State Labor Departments, Womply, Opportunity Insights, SBA, and Call Reports.

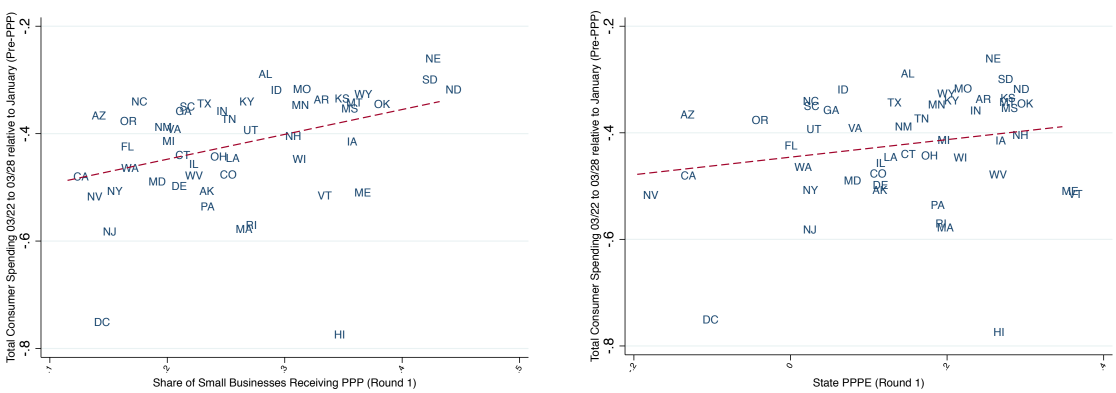
Panel A. UI Claims

Fraction Receiving PPP | State PPPE



Panel B. Δ Small Business Revenues

Fraction Receiving PPP | State PPPE



Panel C. Δ OI Employment

Fraction Receiving PPP | State PPPE

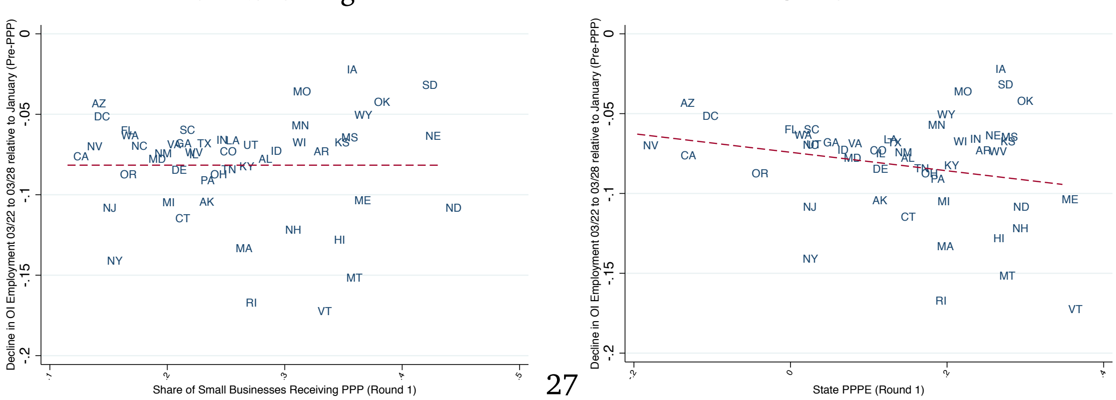
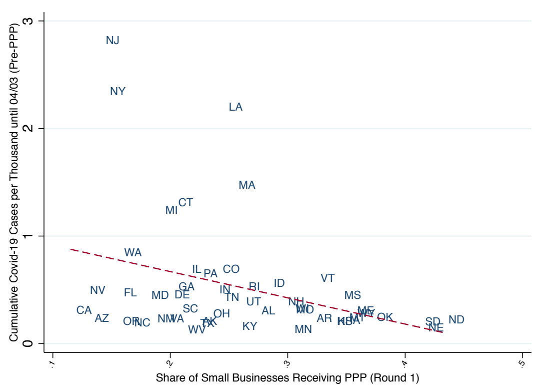


Figure D.3: Pre-PPP Health Outcomes and PPP Allocation by State

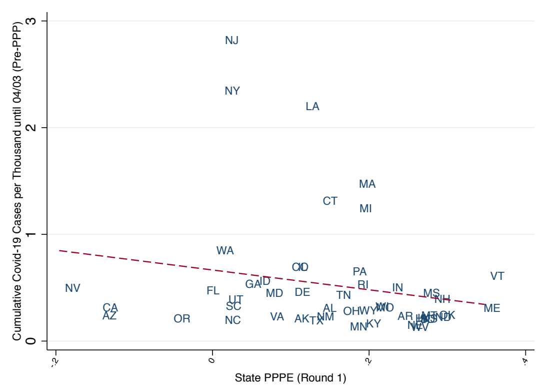
Figure D.3 presents scatterplots of the cumulative number of pre-PPP COVID-19 cases and deaths per thousand in each state as of April, 3rd 2020. The figures on the left plot the pre-PPP health outcomes against the number of PPP loans received by small businesses in each state during the first round of the program divided by the total number of small businesses in the state. The figures on the right plot the same pre-PPP health outcomes and the state-level PPPE measure. Data comes from the Center for Disease Control, SBA, Call Reports, and FDIC Summary of Deposits.

Panel A. Covid Cases per Capita

Fraction Receiving PPP

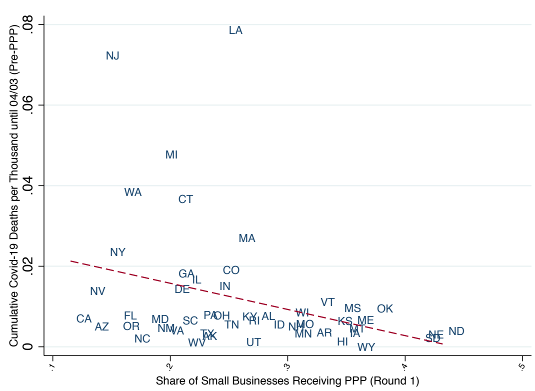


State PPPE



Panel B. Covid Deaths per Capita

Fraction Receiving PPP



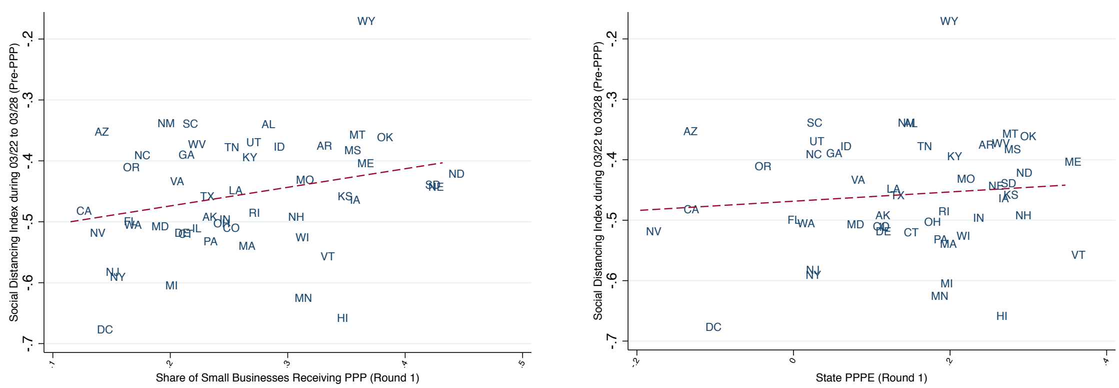
State PPPE

Figure D.4: Pre-PPP Social Distancing and Public Health Interventions and PPP Allocation by State

Figure D.4 presents scatterplots of measures of pre-PPP social distancing (Panel A) and of the timing of statewide shelter-in-place orders (Panel B). The figures on the left plot these pre-PPP social distancing and public health intervention outcomes against the number of PPP loans received by small businesses in each state during the first round of the program divided by the total number of small businesses in the state. The figures on the right plot the same outcomes and the state-level PPPE measure. Data comes from the New York Times, SBA, Call Reports, and FDIC Summary of Deposits.

Panel A. Social Distancing Index

Fraction Receiving PPP | State PPPE



Panel B. Shelter in Place Orders

Fraction Receiving PPP | State PPPE

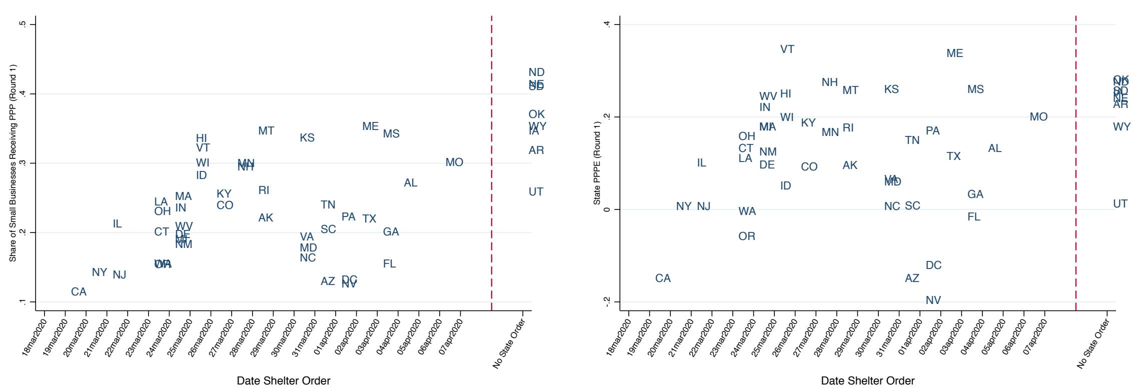
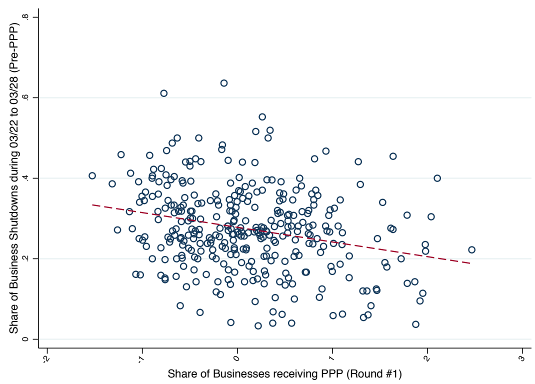


Figure D.5: Pre-PPP Homebase Employment Outcomes and PPP Allocation by County (Round 1)

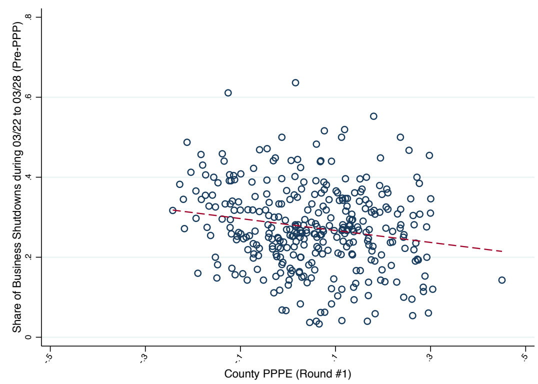
Figure D.5 presents scatterplots of the share of businesses in each county that shutdown in the week of March 22nd to March 28th (Panel A), of the decline in hours worked in each county in the week of March 22nd to March 28th relative to a January Baseline (Panel B), and of the decline in the number of employee counts in each county in the week of March 22nd to March 28th relative to a January baseline (Panel C). The figures on the left plot the pre-PPP county-level employment outcomes from Homebase against the average number of PPP loans received by small businesses in each county during the first round of the program divided by the total number of small businesses in the county. The figures on the right plot the same pre-PPP employment outcomes and the average county-level PPPE measure. Data is from Homebase, SBA, and Call Reports.

Panel A. Business Shutdowns

Fraction Receiving PPP

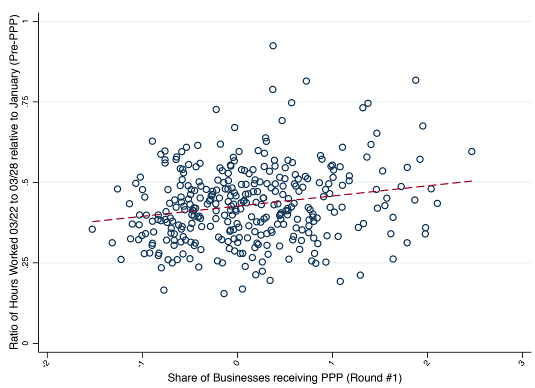


County PPPE

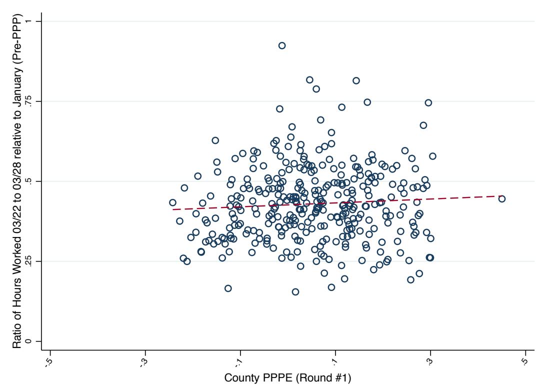


Panel B. Decline in Hours Worked

Fraction Receiving PPP

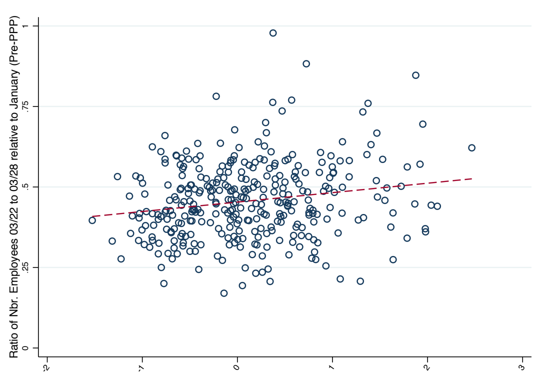


County PPPE



Panel C. Decline in Number of Employees

Fraction Receiving PPP



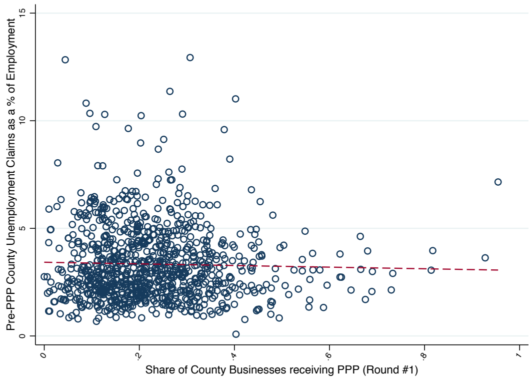
County PPPE

Figure D.6: Pre-PPP Other Employment and Economic Outcomes and PPP Allocation by County (Round 1)

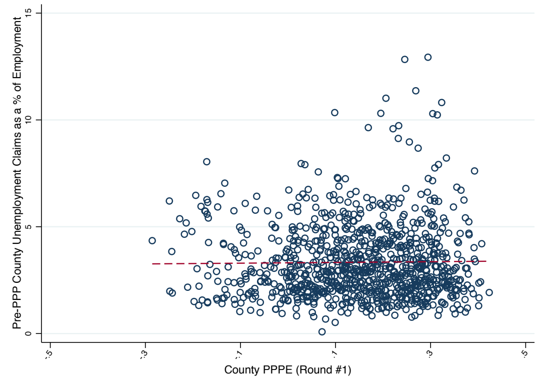
Figure D.6 presents scatterplots of the average county pre-PPP UI claims as a percentage of employment (Panel A), of the average pre-PPP decline in small business revenues at the county level from Womply (Panel B), and of the pre-PPP decline in employment at the county level from OI (Panel C). The figures on the left plot the pre-PPP county-level employment outcomes against the average number of PPP loans received by small businesses in each county during the first round of the program divided by the total number of small businesses in the county. The figures on the right plot the same pre-PPP employment outcomes and the average county-level PPPE measure. Data is from State Labor Departments, Womply, Opportunity Insights, SBA, and Call Reports.

Panel A. UI Claims

Fraction Receiving PPP

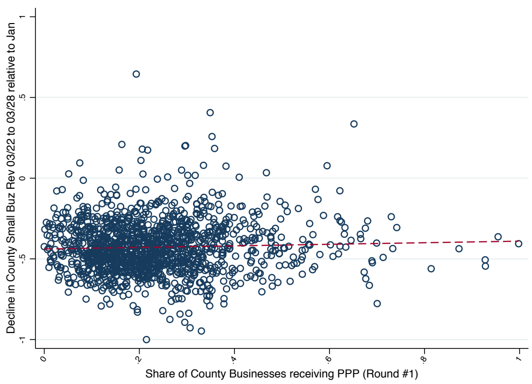


County PPPE

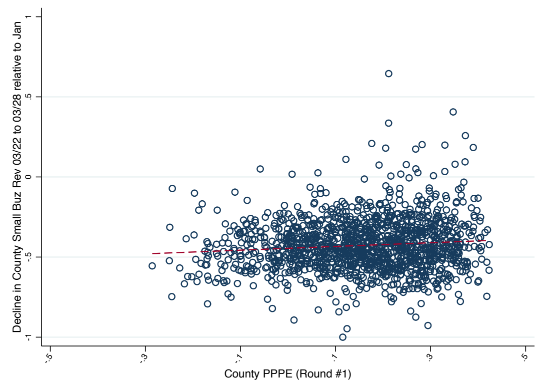


Panel B. Δ Small Business Revenues

Fraction Receiving PPP

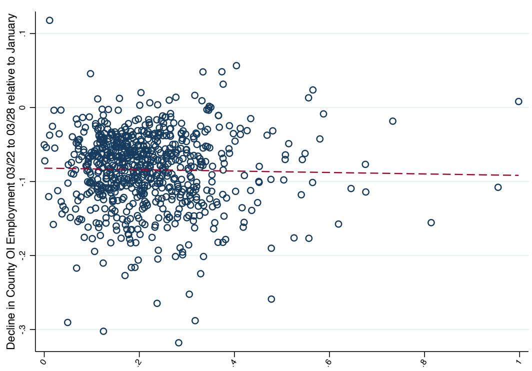


County PPPE



Panel C. Δ OI Employment

Fraction Receiving PPP



County PPPE

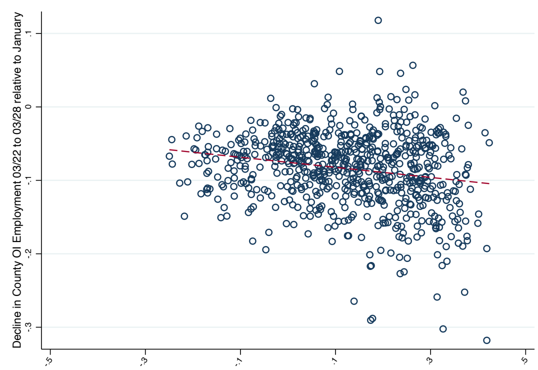
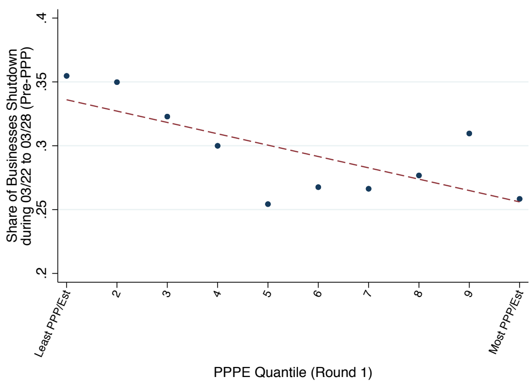


Figure D.7: Targeting of PPP Allocation across Quantiles of PPPE and Predicted PPPE (First Round)

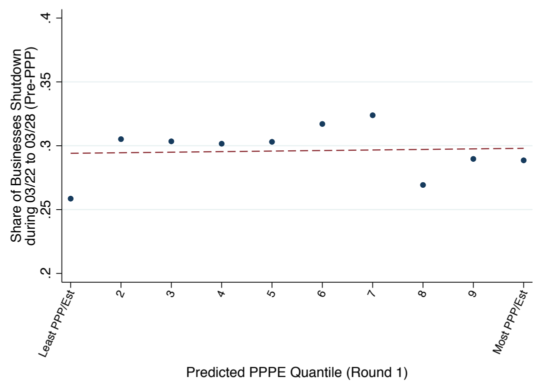
Figure D.7 stratifies all businesses in Homebase in 10 bins based on the PPPE (left panels) and Predicted PPPE (right panels) of the their ZIP codes during the first round. Panel A plots for each bin the share of Homebase businesses that shut down in the week of March 22nd–March 28th. Panel B plots for each bin the average decline in hours worked in the week of March 22nd–March 28th relative to a baseline of the average weekly hours worked in the last two weeks of January. Panel C plots for each bin the average decline in the number of employees in the week of March 22nd–March 28th relative to a baseline of the average number of employees in the last two weeks of January. Data are from SBA, Call Reports, Homebase, and County Business Patterns.

Panel A. Business Shutdowns and Fraction of Establishments Receiving PPP

PPPE (Round 1)

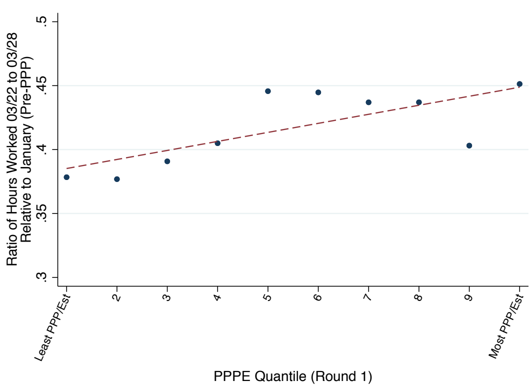


Predicted PPPE (Round 1)

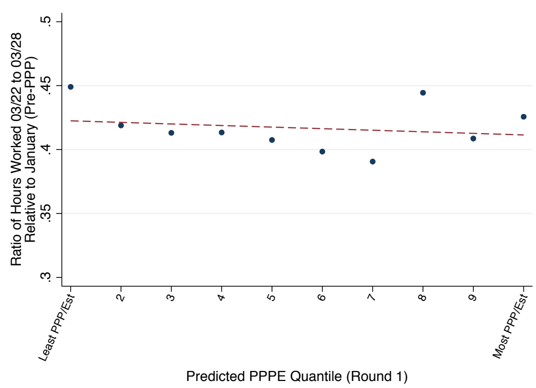


Panel B. Decline in Hours Worked and Fraction of Establishments Receiving PPP

PPPE (Round 1)

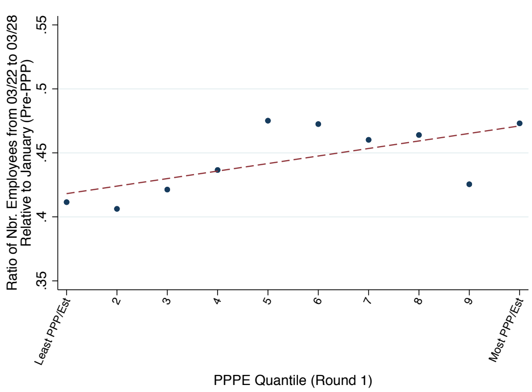


Predicted PPPE (Round 1)

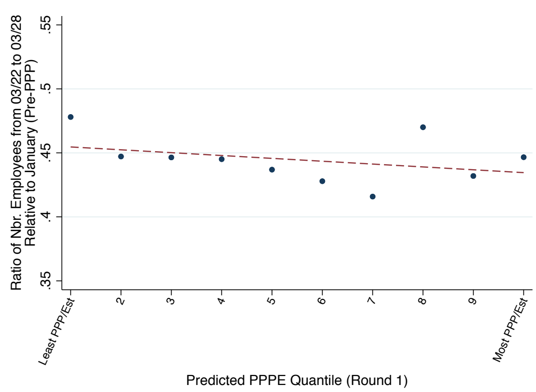


Panel C. Decline in Number of Employees and Fraction of Establishments Receiving PPP

PPPE (Round 1)



Predicted PPPE (Round 1)



Appendix E. Crowd-out Appendix

This appendix presents evidence that suggests that the PPP crowded out some private loans. We find some evidence of modest crowd-out, but the results suggest that magnitudes are small and private lending would not have fully offset PPP lending.

Appendix Figure E.1 shows suggestive evidence of crowd-out from California Uniform Commercial Code (UCC) filings.[1] The figure shows a significant spike in UCC filings in May, following the exhaustion of PPP funds, which is consistent with the program crowding-out private lending. On the other hand, the time series could simply reflect bureaucratic delays in filing or the recovery. Appendix Figure E.2 shows scatterplots of the ratio of UCC filings per establishment and county-level PPPE under different time horizons. The relationship is relatively flat, suggesting little relationship between the availability of PPP loans and commercial lending.[2]

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

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

The coefficient γ captures crowd-out. If there is no crowd-out, an additional PPP loan leads to one additional total loan, and $\gamma = 1$. Under full crowd-out, an additional PPP loan is offset by a reduction in another commercial loan, and $\gamma = 0$. Appendix Figure E.3 plots the ratio of Commercial and Industrial Loans in Q2 2020 and Commercial and Industrial Loans in Q1 2020 and the ratio of PPP loans and Commercial and Industrial Loans in Q1 2020. Appendix Table E.1 reports OLS and IV regressions examining the relation between the ratio of Commercial and Industrial Loans in Q2 2020 and Commercial and Industrial Loans in Q1 2020 ($\frac{C\&ILoans_{Q2}}{C\&ILoans_{Q1}}$) and the ratio of PPP loans and Commercial and Industrial Loans in Q1 2020 ($\frac{PPPLoans}{C\&ILoans_{Q1}}$). Column

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

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

(2) instruments using lender PPPE. In column (1), the coefficient γ is 0.558 and statistically significant at the one-percent level. This is suggestive of some crowd-out, but not full crowd-out. The OLS estimates may be biased if $\frac{PPPLoans}{C\&ILoans_{Q1}}$ and ζ are correlated, so in column (2) we instrument using lender PPPE. The IV estimate in column (2) provides an estimate of $\gamma = 0.991$, and we cannot reject no crowd out. The confidence interval of the IV estimate allows us to reject full crowd-out.

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

Appendix Figure E.4 shows cumulative PPP and EIDL lending over time. Similar to UCC filings, we see an uptick in EIDL loans after PPP funds level off in May. The SBA was slower to open up the expanded EIDL provisions of the CARES Act, which may also account for this lagged increase. The fact that EIDL loans only start rising in late May implies that crowd-out of EIDL is unlikely to explain the modest effects we estimate for April and May.

Appendix Figure E.5 shows scatterplots of the average fraction of small business establishments that received an EIDL loan in each percentile bin based on state PPPE in the weeks of May 3-9 and June 28-July 4. The figure shows a weak relationship in the early period with very few firms receiving EIDL loans. In the later period, there is a strong negative relationship, consistent with crowd-out and indicating that higher PPP exposure is associated with fewer EIDL loans. If firms that were unable to access PPP were more likely to apply for and receive an EIDL loan, and if EIDL loans were sufficiently good substitutes for PPP loans, this fact could help account for modest estimated effects of PPP in June.

Figure E.1: UCC Filings Over Time

Figure E.1 shows the number of UCC filings between January and July 2020. Data comes from California UCC filings.

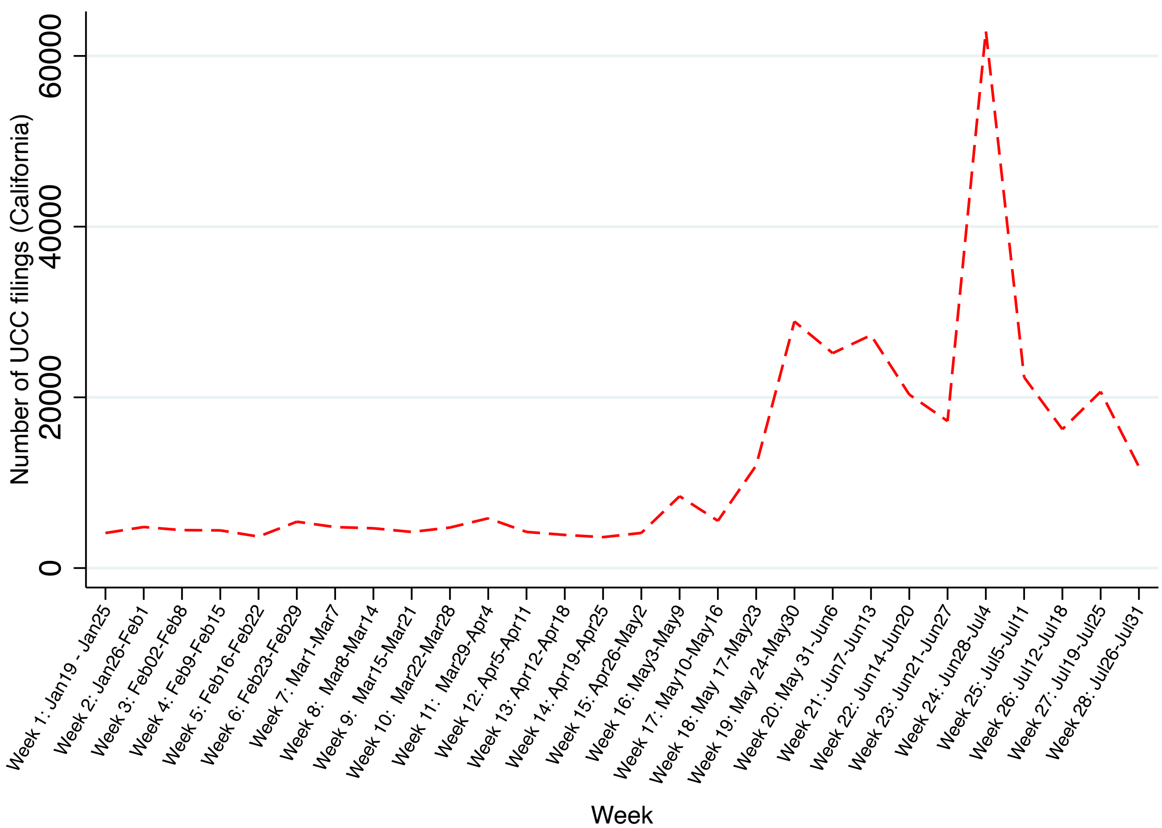


Figure E.2: UCC Filings and PPPE

Figure E.2 are scatterplots of the the ratio of UCC filings per establishment in each county and the county exposure to the number-based PPPE during the first round of PPP. Data comes from SBA, Call Reports, Summary of Deposits, County Business Patterns and California UCC filings.

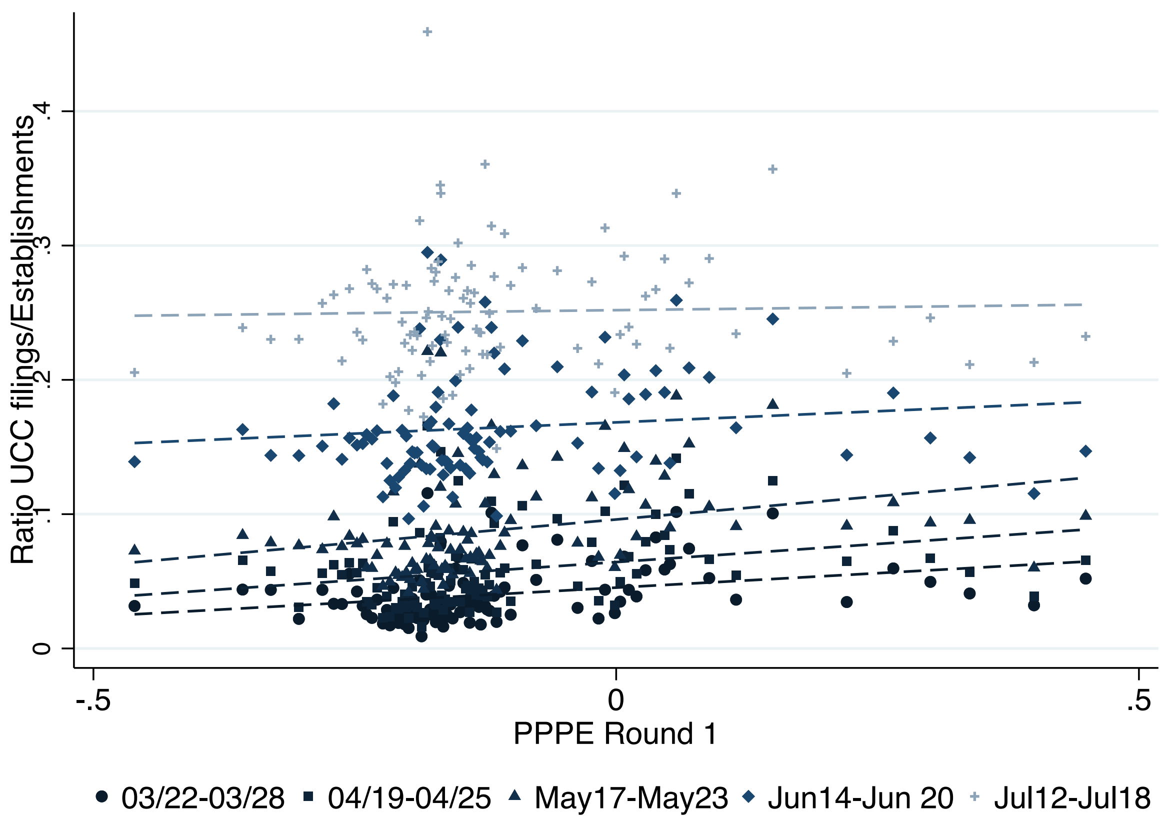


Figure E.3: PPP Lending and Commercial & Industrial Loans

Figure E.3 are scatterplots of the ratio of Commercial and Industrial Loans in Q2 2020 and Commercial and Industrial Loans in Q1 2020 ($\frac{C\&ILoans_{Q2}}{C\&ILoans_{Q1}}$) and the ratio of PPP loans and Commercial and Industrial Loans in Q1 2020 $\frac{PPPLoans}{C\&ILoans_{Q1}}$. Data comes from Federal Reserve Call Reports and SBA.

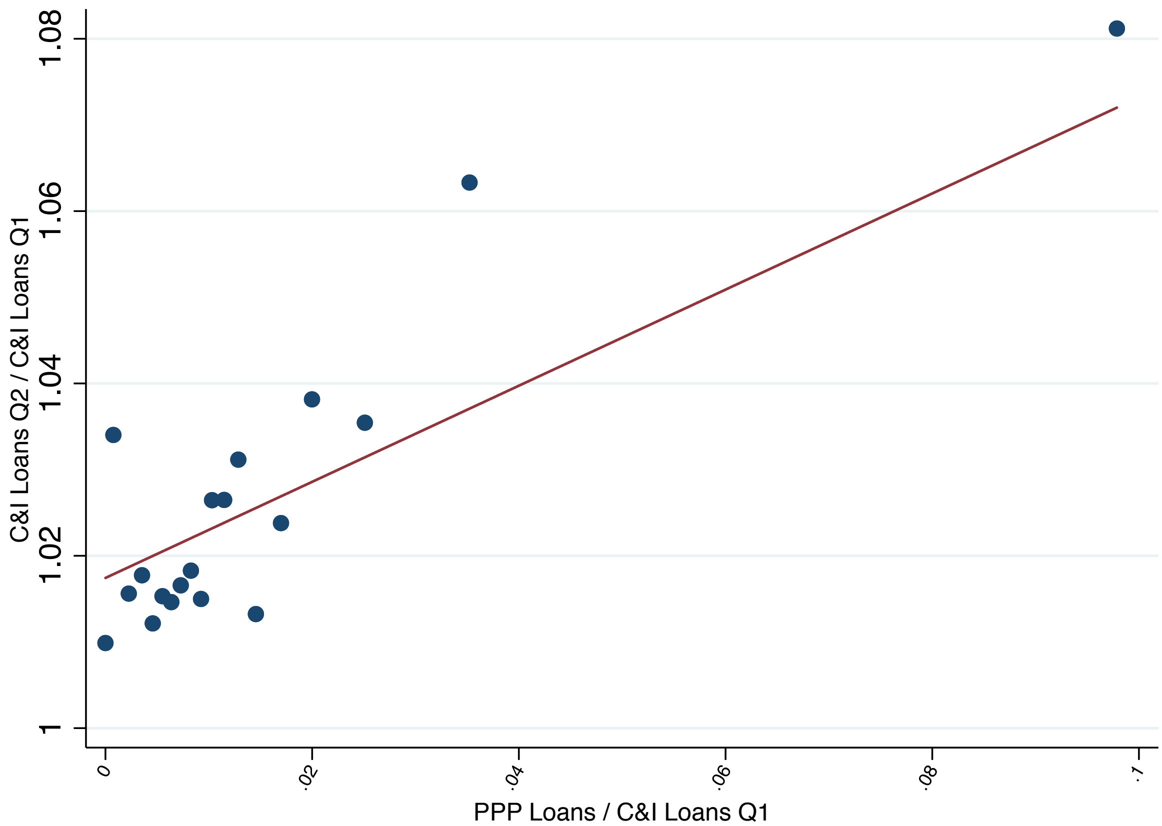


Table E.1: PPP Lending and Crowd-Out

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

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

Figure E.4: EIDL and PPP

Figure E.4 shows cumulative PPP and EIDL lending between April and July, 2020. Data comes from the SBA.

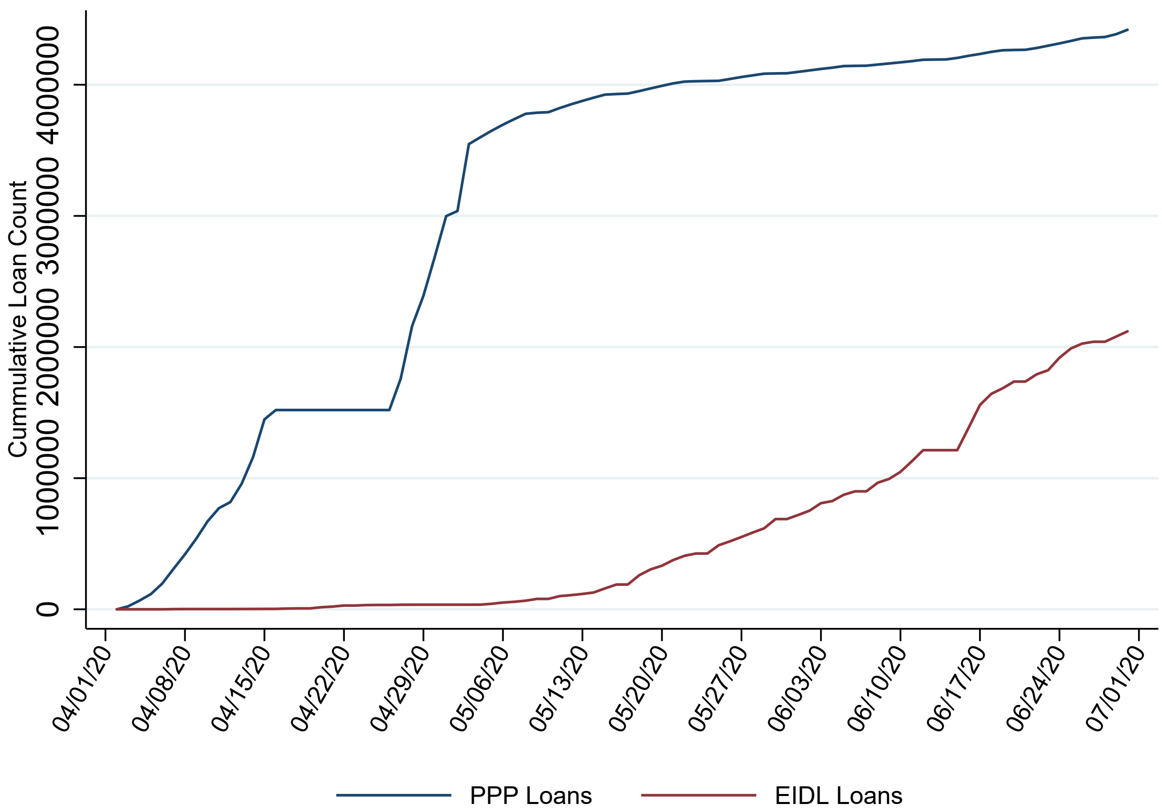
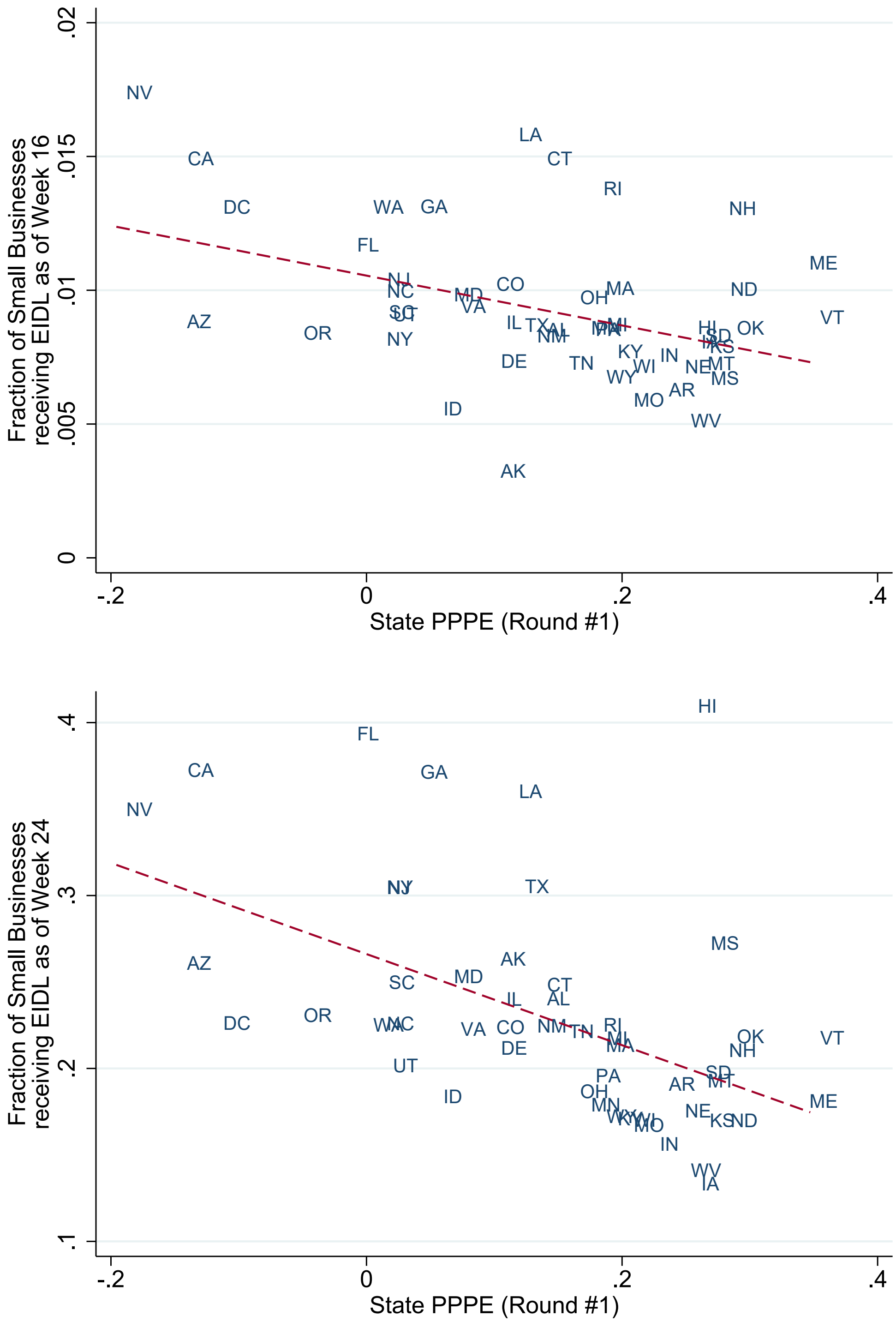


Figure E.5: EIDL and PPPE

Figure E.5 are scatterplots of the fraction of small business establishment that received an EIDL loan in each state and the respective State PPPE during the first round. Data comes from SBA, Call Reports, Summary of Deposits, and County Business Patterns.



Appendix F. Supplemental Analysis

This appendix presents additional robustness checks and additional empirical analyses exploring possible mechanisms that can account for the effect of the program on employment outcomes.

Figure F.1 examines the possibility that the program allowed firms to maintain their employees at home without pay. The plot suggests that conditional on households reporting not working in each week of the survey, more than 85% reported receiving no pay. Moreover, the share that report receiving no pay for staying at home does not have a strong relationship with State PPPE. This suggests that the weak relationship between access to the program and number of hours worked that we documented in Table 5 is likely not explained by the possibility that PPP recipients were keeping their workers at home with pay.

In Table F.1, we explore whether the expansion of unemployment insurance could have led to difficulties for firms in recalling workers and, in turn, attenuated the employment effects of the PPP. Following the work of Ganong et al. (2020), we partition the sample between state with above- and below-median state-level UI replacement rates. The results of Table F.1 do not suggest that the PPP was more effective in states with low UI replacement rates. In Table F.2 we examine the impact of PPP on changes in initial unemployment insurance claims, small business revenues, and Opportunity Insights employment after partitioning the sample into two subsamples based on the median state UI replacement rates. Again, we do not see striking differences between the measured impact of the program in both subsamples. In fact, with the exception of the empirical specifications using OI employment data as the outcome variable, greater access to PPP is more effective in states with relatively more generous employment benefits.

Figure F.2 uses data from Homebase to plot binned scatter plots of the share of small business establishments that "permanently" closed versus ZIP-level PPPE. Both variables are residualized with respect to the controls in Table 5, an additional control for the average number of shutdowns in a ZIP code in the two weeks prior to PPP, and demeaned at the state level to present the within-state relationship. Permanent shutdowns are defined as the establishment being closed for all weeks from the beginning of the PPP through the end of August. The graph reports regression coefficients with and without the Table 5 controls and standard errors clustered at the state level.

The graph suggests a non-trivial impact of PPP on firms over the medium run, as a one-standard-deviation change in PPPE is associated with a 0.97 percentage point reduction in permanent shutdowns. This effect is reasonably large compared to the overall permanent shutdown rate of 12%. The result is consistent with our interpretation that, by helping firms bolster their balance sheets during the crisis, receiving PPP funds enabled these firms to reopen at higher rates than if they had not received funds. Such impacts are also consistent with modest short-term employment impacts providing an incomplete overall picture of the impact of the program.

Figure F.3 uses the Homebase-PPP matched sample and shows the coefficients of week-by-week regressions that repeat the OLS and IV specifications of columns (1) and (3) of Table 8 for every week in the sample. We obtain results that are qualitatively similar to those of our regional analyses. Both the OLS and IV specifications show a small impact of receiving PPP earlier on business shutdowns. This impact is small and fades away over time. The coefficients of Panels B and C indicate firms receiving PPP earlier have better employment outcomes in terms of number of hours and number of employees employed and that these gaps in the number of employees and hours worked persist until August. Again, we caution that the OLS estimator may be biased if firms that obtained loans earlier are fundamentally different from firms that received PPP loans later. We note, however, that the IV coefficients show larger standard errors likely due to a smaller sample size relative to our regional analyses. These larger standard errors limit our ability to draw strong conclusions from this analysis.

Tables F.3 and F.4 partition the sample into two subsamples based on the median share of establishments in the county that are eligible for participation in the PPP. The goal is to investigate whether eligible firms might expand at the expense of local competitors. Such business stealing spillovers could account for low employment effects at the labor market level. Alternatively, the program might have positive local demand effects, for instance on the suppliers of treated firms. Given the scale and severity of the labor market disruption due to the pandemic, traditional measures of labor market tightness are unlikely to be useful. However, we can ask whether regions with a larger share of employment in PPP-eligible establishments exhibit different effects relative to those with fewer eligible establishments. Employment effects are generally similar or greater in regions where a larger share of establishments are eligible for funds, inconsistent with a business stealing effect and possibly consistent with the presence of some local demand effects. The results are also useful as a robustness check, since omitted variables would have to account for stronger effects in regions with a larger share of firms that are eligible for the program.

In Figure F.4 we repeat the empirical exercise in Figure 9 with and without the state of California. In Figure 9 of the main draft, we exclude California counties from the time series of Panel A due to a large outlier in UI claims that is likely due to a backlog in UI claims processing in that state. In this figure, we show sensitivity of this analysis to this sample selection criterion. In Panel C of this figure, we can see that this backlog likely created an abnormal jump in the number of UI claims in the week of April 26 to May 2nd. While, this jump is significant for the state of California, it would not materially change our interpretation of the results in the main draft.

Finally, in Table F.5 we evaluate heterogeneity in effects of the program across industries by repeating our empirical specification of column (6) of Table 5 for each industry in the Homebase sample. The estimated effects of the PPP are more pronounced for the Food & Drink and retail industry, which is consistent with the idea that the program was relatively more effective in contact-intensive industries whose businesses were more disrupted by the pandemic.

Figure F.1: Share of Households reporting receiving no pay and Exposure to State PPPE

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

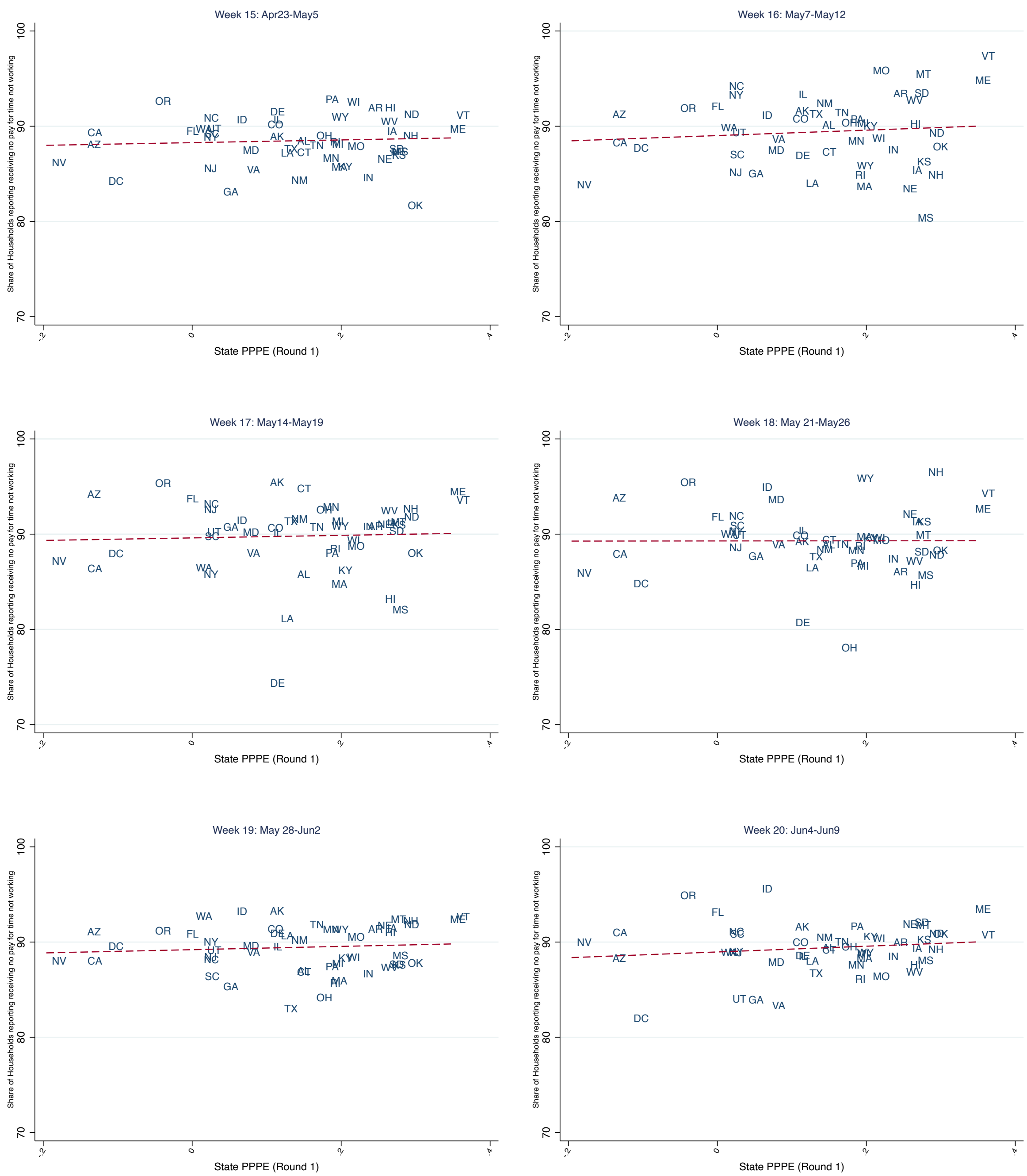


Figure F.2: PPP Exposure and Permanent Shutdowns

Figure F.2 plots binned scatter plots of the share of small business establishments that permanently closed versus ZIP-level PPPE. Both variables are residualized with respect to the controls in Table 5, an additional control for the average number of shutdowns in a ZIP code in the two weeks prior to PPP, and demeaned at the state level to present the within-state relationship. Permanent shutdowns are defined as the establishment being closed for all weeks from the beginning of the PPP through the end of August. The graph reports regression coefficients with and without the Table 5 controls and standard errors clustered at the state level. Outcome data come from Homebase.

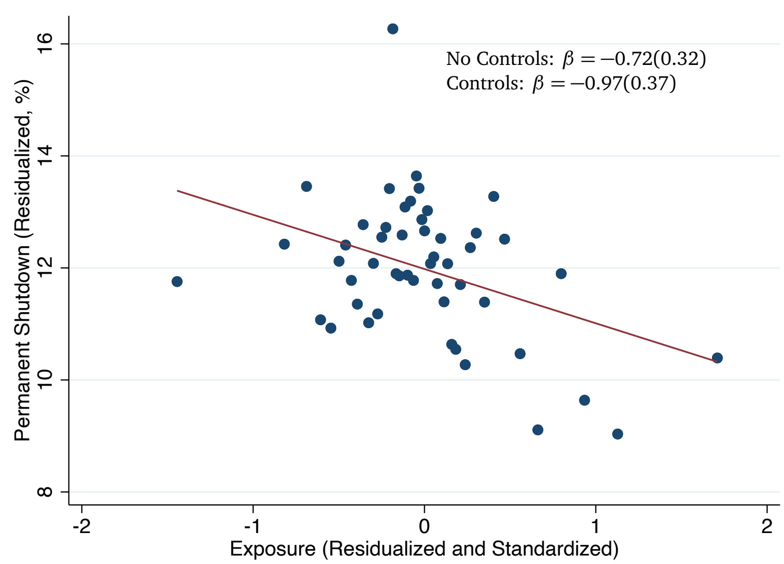
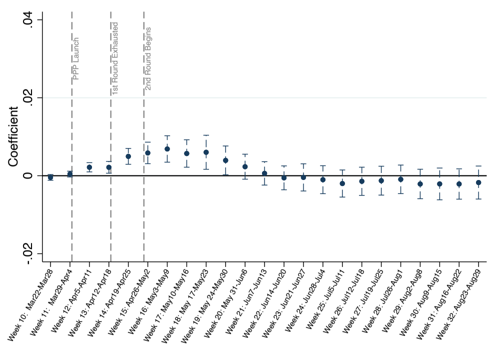


Figure F.3: PPPE and Post-PPP Outcomes in the Matched Sample (Dynamic Analysis)

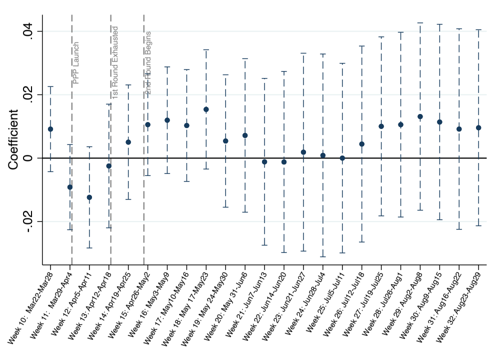
Figure F.3 investigates business shutdowns, changes in the ratio of hours worked, and changes in the number of employees for firms in the Homebase sample that are name-matched to the PPP data set from SBA. In the upper panels, we plot OLS estimates from week-by-week regressions examining the impact of delays in receiving PPP on differences in employment and firm outcomes. The OLS regressions repeat the specifications of column (1) of Table 8 for every week in the sample. In the bottom panels, we plot IV estimates from week-by-week regressions examining the impact of delays in receiving PPP on differences in employment and firm outcomes. The IV regressions repeat the specifications of column (3) of Table 8 for every week in the sample. Outcome variables are defined as the difference between measured outcomes in each week and their average in the two weeks preceding program launch. The right-hand-side variable is the week of PPP receipt. Data is from Call Reports, SBA, and Homebase.

Panel A. Business Shutdowns

Week of PPP Projections (OLS)

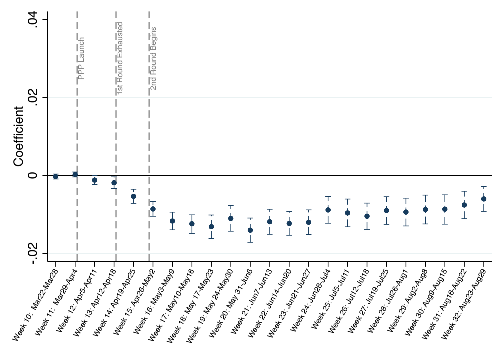


Week of PPP Projections (IV)

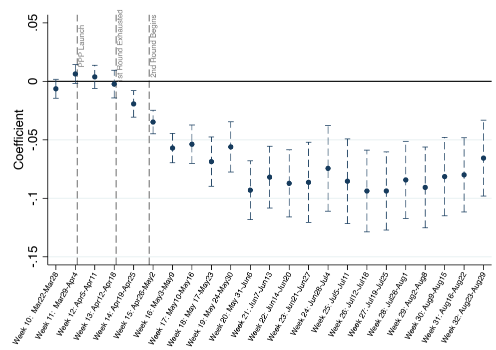


Panel B. Change in Hours Worked

Week of PPP Projections (OLS)

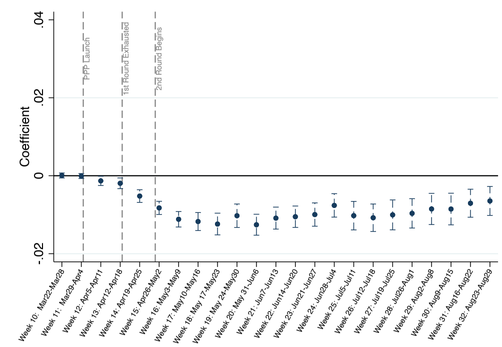


Week of PPP Projections (IV)



Panel C. Change in Nbr. Employees

Week of PPP Projections (OLS)



Week of PPP Projections (IV)

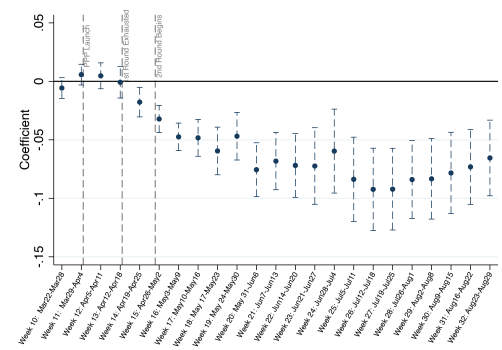


Figure F.4: PPP and UI Claims (Including the state of California)

Figure F.4 repeats the analysis of Panel A of Figure 9 excluding the state of California (Panel A), with all states including the state of California (Panel B) and with only the state of California. In Figure 9 of the main draft, we exclude California counties from the time series of Panel A due to a large outlier in UI claims that is likely due to a backlog in UI claims processing in that state. In Panel C of this figure, we can see that this backlog likely created an abnormal jump in the number of UI claims in the week of April 26 to May 2nd. The results of the other analysis include state × week fixed effects and therefore are not affected by these statewide reporting changes.Data is from Call Reports, SBA, and state labor deparments.

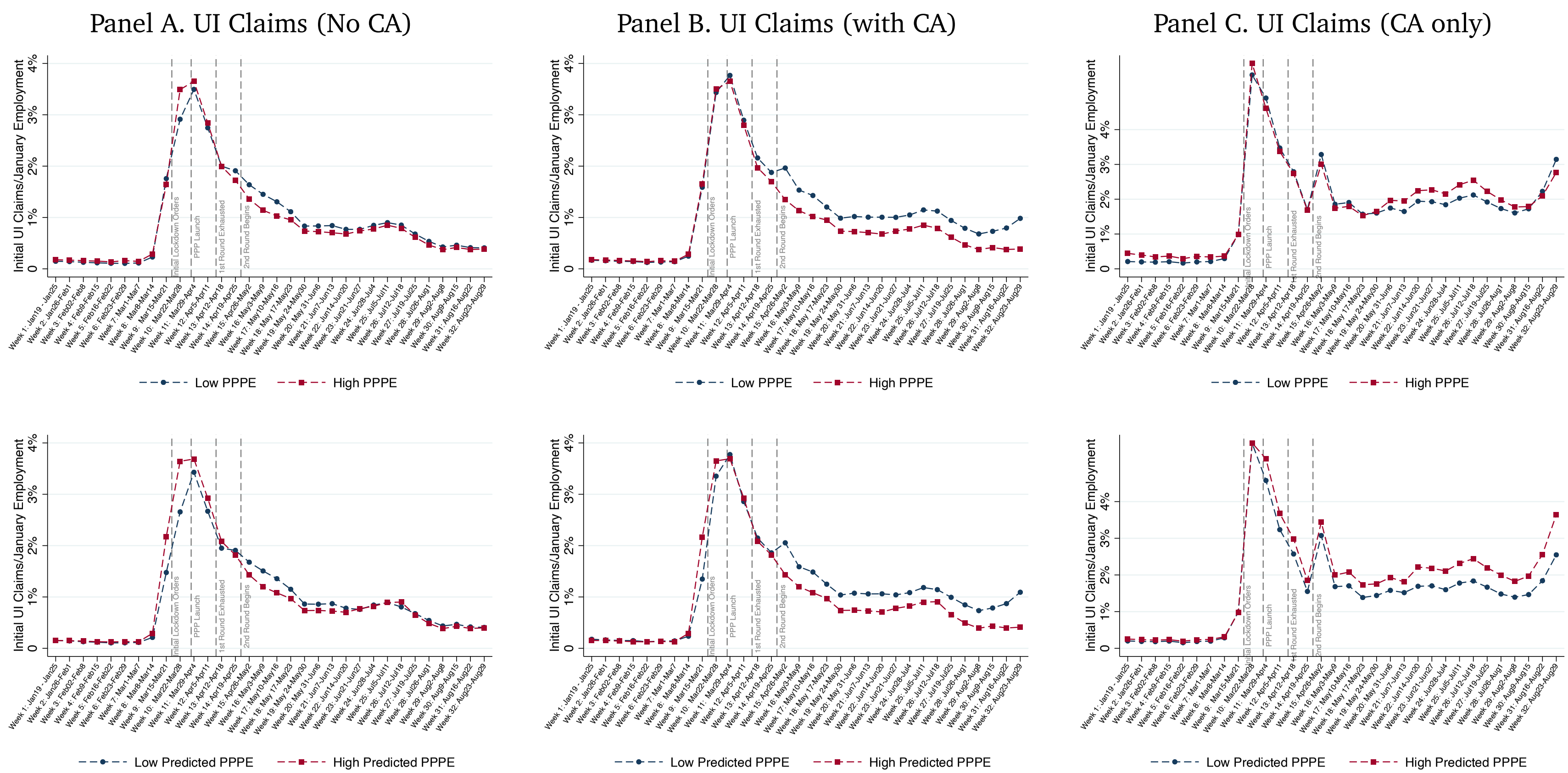


Table F.1: PPP Exposure and Homebase Employment Outcomes: Partition by State UI Replacement Rates

Table F.1 reports the results of OLS regressions repeating the analysis of Table 5 after partitioning the sample into two subsamples based on the median state UI replacement rates following the work of Ganong et al. (2020). Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ Bus. Shutdown		Δ Hours Worked		Δ Nbr. Employees	
Zip PPPE (Round #1) × I(Month=April)	0.007	-0.003	0.001	0.006**	0.001	0.005**
	(0.004)	(0.005)	(0.003)	(0.002)	(0.003)	(0.002)
Zip PPPE (Round #1) × I(Month=May)	-0.000	-0.007	0.021***	0.020***	0.022***	0.019**
	(0.007)	(0.004)	(0.005)	(0.007)	(0.006)	(0.007)
Zip PPPE (Round #1) × I(Month=June)	0.003	-0.009**	0.034***	0.035***	0.035***	0.031***
	(0.007)	(0.003)	(0.009)	(0.008)	(0.011)	(0.007)
Zip PPPE (Round #1) × I(Month=July)	0.007	-0.007	0.030**	0.035***	0.033**	0.033***
	(0.008)	(0.006)	(0.012)	(0.009)	(0.014)	(0.009)
Zip PPPE (Round #1) × I(Month=August)	0.007	-0.008*	0.029**	0.035***	0.028**	0.033***
	(0.009)	(0.004)	(0.012)	(0.010)	(0.014)	(0.009)
Observations	446476	373358	446476	373358	446476	373358
Adjusted R^2	0.606	0.597	0.635	0.619	0.556	0.595
Sample	High Rep	Low Rep	High Rep	Low Rep	High Rep	Low Rep
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
	Δ Bus. Shutdown		Δ Hours Worked		Δ Nbr. Employees	
Predicted PPPE × I(Month=April)	0.005	-0.006	-0.003	0.002	-0.002	0.003
	(0.003)	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)
Predicted PPPE × I(Month=May)	-0.004	-0.009	0.009**	0.008	0.009*	0.007
	(0.004)	(0.005)	(0.004)	(0.006)	(0.004)	(0.006)
Predicted PPPE × I(Month=June)	-0.007	-0.005	0.022***	0.014*	0.020**	0.013*
	(0.005)	(0.005)	(0.007)	(0.007)	(0.009)	(0.008)
Predicted PPPE × I(Month=July)	-0.003	-0.007	0.016***	0.019**	0.011	0.019*
	(0.004)	(0.006)	(0.005)	(0.008)	(0.009)	(0.010)
Predicted PPPE × I(Month=August)	-0.002	-0.006	0.014***	0.018**	0.007	0.018*
	(0.005)	(0.006)	(0.004)	(0.009)	(0.009)	(0.010)
Observations	446476	373358	446476	373358	446476	373358
Adjusted R^2	0.606	0.597	0.635	0.619	0.556	0.595
Sample	High Rep	Low Rep	High Rep	Low Rep	High Rep	Low Rep
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table F.2: PPP Exposure and Local Labor Market and Economic Effects: Partition by State UI Replacement Rates

Table F.2 reports the results of OLS regressions repeating the analysis of Table 6 after partitioning the sample into two subsamples based on the median state UI replacement rates following the work of Ganong et al. (2020). Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ UI claims		Δ Small Bus. Rev.		Δ OI Emp.	
County PPPE × I(Month=April)	-0.210**	-0.101	0.001	-0.001	-0.000	-0.001
	(0.084)	(0.062)	(0.006)	(0.007)	(0.007)	(0.002)
County PPPE × I(Month=May)	-0.282**	-0.143	0.010	0.014	0.006	0.009**
	(0.104)	(0.086)	(0.011)	(0.010)	(0.005)	(0.004)
County PPPE × I(Month=June)	-0.266**	-0.092	-0.006	-0.007	0.012	0.014***
	(0.115)	(0.105)	(0.015)	(0.012)	(0.011)	(0.004)
County PPPE × I(Month=July)	-0.258**	-0.093	-0.017	-0.027*	0.008	0.018***
	(0.108)	(0.102)	(0.013)	(0.014)	(0.009)	(0.006)
County PPPE × I(Month=August)	-0.256**	-0.051	-0.020	-0.013	0.013	0.019**
	(0.112)	(0.086)	(0.013)	(0.009)	(0.010)	(0.007)
Observations	23261	22272	13419	14974	4422	5801
Adjusted R^2	0.948	0.945	0.701	0.725	0.868	0.865
Sample	High Rep	Low Rep	High Rep	Low Rep	High Rep	Low Rep
State×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
	Δ UI claims		Δ Small Bus. Rev.		Δ OI Emp.	
County Predicted PPPE × I(Month=April)	-0.081	-0.068	0.001	0.001	-0.004*	0.000
	(0.061)	(0.080)	(0.005)	(0.007)	(0.002)	(0.002)
County Predicted PPPE × I(Month=May)	-0.081	-0.155	0.021	0.003	-0.007	0.010**
	(0.073)	(0.130)	(0.015)	(0.007)	(0.004)	(0.004)
County Predicted PPPE × I(Month=June)	-0.067	-0.160	0.023	-0.003	0.003	0.012
	(0.072)	(0.148)	(0.020)	(0.010)	(0.008)	(0.008)
County Predicted PPPE × I(Month=July)	-0.064	-0.165	0.003	-0.013	-0.009	0.018
	(0.080)	(0.141)	(0.013)	(0.010)	(0.017)	(0.012)
County Predicted PPPE × I(Month=August)	-0.076	-0.089	0.007	0.002	-0.009	0.023
	(0.082)	(0.126)	(0.015)	(0.010)	(0.017)	(0.014)
Observations	23261	22272	13419	14974	4422	5801
Adjusted R^2	0.947	0.945	0.701	0.723	0.868	0.864
Sample	High Rep	Low Rep	High Rep	Low Rep	High Rep	Low Rep
State×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table F.3: PPP Exposure and Homebase Employment Outcomes: Partition by Share Eligible for PPP in County

Table F.3 reports the results of OLS regressions repeating the analysis of Table 5 after partitioning the sample into two subsamples based on the median share of establishments in the county that are eligible for participation in the PPP. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ Bus. Shutdown		Δ Hours Worked		Δ Nbr. Employees	
Zip PPPE (Round #1) × I(Month=April)	0.004	0.003	0.003	0.001	0.003	0.001
	(0.004)	(0.006)	(0.002)	(0.004)	(0.002)	(0.004)
Zip PPPE (Round #1) × I(Month=May)	-0.005	0.001	0.017***	0.020***	0.016***	0.021***
	(0.005)	(0.008)	(0.005)	(0.007)	(0.005)	(0.007)
Zip PPPE (Round #1) × I(Month=June)	-0.008	0.003	0.033***	0.030***	0.029***	0.030**
	(0.006)	(0.006)	(0.006)	(0.010)	(0.008)	(0.011)
Zip PPPE (Round #1) × I(Month=July)	-0.010	0.012	0.038***	0.019	0.039***	0.020
	(0.008)	(0.007)	(0.008)	(0.013)	(0.011)	(0.015)
Zip PPPE (Round #1) × I(Month=August)	-0.014*	0.018**	0.038***	0.018	0.039***	0.016
	(0.007)	(0.008)	(0.008)	(0.013)	(0.011)	(0.016)
Observations	409565	409078	409565	409078	409565	409078
Adjusted R^2	0.601	0.603	0.634	0.628	0.606	0.551
Sample	Hi Elig	Low Elig	Hi Elig	Low Elig	Hi Elig	Low Elig
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
	Δ Bus. Shutdown		Δ Hours Worked		Δ Nbr. Employees	
Predicted PPPE × I(Month=April)	0.003	-0.003	-0.001	-0.001	-0.000	-0.001
	(0.003)	(0.005)	(0.002)	(0.004)	(0.002)	(0.004)
Predicted PPPE × I(Month=May)	-0.002	-0.011*	0.002	0.014**	0.001	0.013**
	(0.003)	(0.006)	(0.004)	(0.006)	(0.005)	(0.006)
Predicted PPPE × I(Month=June)	-0.003	-0.012*	0.006	0.027***	0.002	0.025**
	(0.004)	(0.007)	(0.005)	(0.009)	(0.009)	(0.010)
Predicted PPPE × I(Month=July)	-0.004	-0.006	0.006	0.022***	0.006	0.013
	(0.005)	(0.007)	(0.006)	(0.006)	(0.012)	(0.010)
Predicted PPPE × I(Month=August)	-0.004	-0.004	0.006	0.022***	0.005	0.013
	(0.005)	(0.007)	(0.006)	(0.006)	(0.013)	(0.010)
Observations	409565	409078	409565	409078	409565	409078
Adjusted R^2	0.601	0.603	0.633	0.628	0.606	0.551
Sample	Hi Elig	Low Elig	Hi Elig	Low Elig	Hi Elig	Low Elig
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table F.4: PPP Exposure and Local Labor Market and Economic Effects: Partition by Share Eligible for PPP in County

Table F.4 reports the results of OLS regressions repeating the analysis of Table 6 after partitioning the sample into two subsamples based on the median share of establishments in the county that are eligible for participation in the PPP. Standard errors are clustered at the state level. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Δ UI claims		Δ Small Bus. Rev.		Δ OI Emp.	
County PPPE × I(Month=April)	-0.113***	-0.057	-0.002	0.004	0.005	-0.004
	(0.035)	(0.074)	(0.006)	(0.003)	(0.004)	(0.006)
County PPPE × I(Month=May)	-0.141**	-0.119	-0.001	0.017*	0.009**	0.008
	(0.057)	(0.094)	(0.012)	(0.008)	(0.004)	(0.005)
County PPPE × I(Month=June)	-0.103	-0.111	-0.027**	0.004	0.013***	0.010*
	(0.066)	(0.091)	(0.011)	(0.009)	(0.005)	(0.005)
County PPPE × I(Month=July)	-0.107	-0.098	-0.037**	-0.011	0.017***	0.009*
	(0.065)	(0.095)	(0.016)	(0.007)	(0.006)	(0.005)
County PPPE × I(Month=August)	-0.089	-0.061	-0.031***	-0.012	0.015**	0.009
	(0.067)	(0.096)	(0.011)	(0.008)	(0.007)	(0.006)
Observations	22426	22774	20973	21487	7728	8050
Adjusted R^2	0.942	0.952	0.694	0.753	0.894	0.862
Sample	Hi Elig	Low Elig	Hi Elig	Low Elig	Hi Elig	Low Elig
State×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
	Δ UI claims		Δ Small Bus. Rev.		Δ OI Emp.	
County Predicted PPPE × I(Month=April)	-0.061	-0.028	-0.005	0.006	-0.004	0.000
	(0.053)	(0.091)	(0.006)	(0.004)	(0.005)	(0.003)
County Predicted PPPE × I(Month=May)	-0.082	-0.064	-0.004	0.014	0.008	-0.003
	(0.070)	(0.101)	(0.009)	(0.009)	(0.005)	(0.005)
County Predicted PPPE × I(Month=June)	-0.070	-0.069	-0.006	0.011	0.013**	0.001
	(0.065)	(0.108)	(0.008)	(0.012)	(0.006)	(0.007)
County Predicted PPPE × I(Month=July)	-0.072	-0.071	-0.018**	0.001	0.012	0.002
	(0.067)	(0.122)	(0.009)	(0.009)	(0.009)	(0.007)
County Predicted PPPE × I(Month=August)	-0.063	0.002	-0.016	-0.001	0.013	0.002
	(0.069)	(0.115)	(0.010)	(0.009)	(0.010)	(0.007)
Observations	22426	22774	20973	21487	7728	8050
Adjusted R^2	0.942	0.952	0.692	0.752	0.894	0.862
Sample	Hi Elig	Low Elig	Hi Elig	Low Elig	Hi Elig	Low Elig
State×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Table F.5: PPP Exposure and Homebase Employment Outcomes by Industry

This Table repeats the analysis of column (6) in Table 5 of the main draft for each Homebase Industry category

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Δ Bus. Shutdown				
Industry	Food & Drink	Retail	Health & Fit.	Prof. Serv	Education	Home & Repair	Leisure	Transp.	Beauty
Zip PPPE (Round #1) × I(Month=April)	0.002	0.009	0.009	-0.006	0.019	0.013	0.013	-0.046	0.042
	(0.006)	(0.006)	(0.015)	(0.011)	(0.016)	(0.023)	(0.032)	(0.028)	(0.048)
Zip PPPE (Round #1) × I(Month=May)	-0.005	-0.000	0.026**	0.003	-0.013	0.017	-0.035	-0.073*	0.108
	(0.006)	(0.010)	(0.012)	(0.016)	(0.021)	(0.028)	(0.038)	(0.036)	(0.130)
Zip PPPE (Round #1) × I(Month=June)	-0.008	0.003	0.032*	0.014	-0.018	0.032	-0.055	-0.043	0.095
	(0.007)	(0.011)	(0.018)	(0.019)	(0.021)	(0.030)	(0.043)	(0.040)	(0.143)
Zip PPPE (Round #1) × I(Month=July)	-0.006	0.006	0.035**	0.009	-0.001	0.052	-0.074	-0.036	0.106
	(0.008)	(0.010)	(0.014)	(0.021)	(0.025)	(0.033)	(0.045)	(0.043)	(0.137)
Zip PPPE (Round #1) × I(Month=August)	-0.007	0.004	0.039***	0.022	-0.007	0.056	-0.046	-0.078	0.121
	(0.009)	(0.010)	(0.013)	(0.024)	(0.027)	(0.036)	(0.045)	(0.053)	(0.146)
Observations	394857	109434	52639	24001	19586	14714	11337	6026	4659
Adjusted R^2	0.610	0.597	0.576	0.581	0.557	0.616	0.591	0.594	0.671
					Δ Hours Worked				
Industry	Food & Drink	Retail	Health & Fit.	Prof. Serv	Education	Home & Repair	Leisure	Transp.	Beauty
Zip PPPE (Round #1) × I(Month=April)	0.005**	-0.001	-0.005	0.010	0.012	-0.056***	-0.013	0.025	-0.011
	(0.002)	(0.005)	(0.005)	(0.017)	(0.011)	(0.018)	(0.017)	(0.030)	(0.047)
Zip PPPE (Round #1) × I(Month=May)	0.022***	0.024*	0.001	0.007	0.024	-0.053*	-0.017	0.041	-0.080
	(0.006)	(0.014)	(0.014)	(0.028)	(0.020)	(0.029)	(0.035)	(0.052)	(0.104)
Zip PPPE (Round #1) × I(Month=June)	0.041***	0.031*	0.011	0.006	0.045	-0.053	-0.018	0.042	-0.046
	(0.007)	(0.016)	(0.021)	(0.036)	(0.032)	(0.034)	(0.061)	(0.065)	(0.143)
Zip PPPE (Round #1) × I(Month=July)	0.043***	0.023	-0.007	-0.011	0.027	-0.038	0.013	0.021	-0.046
	(0.009)	(0.015)	(0.023)	(0.032)	(0.035)	(0.040)	(0.058)	(0.068)	(0.140)
Zip PPPE (Round #1) × I(Month=August)	0.043***	0.030**	-0.017	-0.039	0.029	-0.050	0.001	0.023	-0.020
	(0.009)	(0.014)	(0.025)	(0.034)	(0.030)	(0.037)	(0.051)	(0.081)	(0.147)
Observations	394857	109434	52639	24001	19586	14714	11337	6026	4659
Adjusted R^2	0.636	0.630	0.610	0.583	0.584	0.616	0.652	0.616	0.661
					Δ Nbr. Employees				
Industry	Food & Drink	Retail	Health & Fit.	Prof. Serv	Education	Home & Repair	Leisure	Transp.	Beauty
Zip PPPE (Round #1) × I(Month=April)	0.005**	-0.002	-0.005	0.015	0.000	-0.048***	-0.006	0.047	-0.036
	(0.002)	(0.005)	(0.007)	(0.019)	(0.012)	(0.014)	(0.017)	(0.030)	(0.059)
Zip PPPE (Round #1) × I(Month=May)	0.022***	0.014	-0.000	0.007	0.024	-0.038	-0.004	0.077	-0.111
	(0.006)	(0.017)	(0.011)	(0.029)	(0.021)	(0.031)	(0.030)	(0.059)	(0.115)
Zip PPPE (Round #1) × I(Month=June)	0.040***	0.026	0.003	0.005	0.052*	-0.033	-0.021	0.044	-0.101
	(0.008)	(0.017)	(0.017)	(0.041)	(0.028)	(0.036)	(0.069)	(0.084)	(0.153)
Zip PPPE (Round #1) × I(Month=July)	0.046***	0.022	-0.025	-0.020	0.033	-0.020	0.004	-0.170	-0.129
	(0.011)	(0.017)	(0.021)	(0.037)	(0.033)	(0.038)	(0.070)	(0.241)	(0.145)
Zip PPPE (Round #1) × I(Month=August)	0.044***	0.018	-0.028	-0.053	0.035	-0.022	-0.049	-0.167	-0.104
	(0.010)	(0.016)	(0.024)	(0.039)	(0.031)	(0.034)	(0.055)	(0.264)	(0.151)
Observations	394857	109434	52639	24001	19586	14714	11337	6026	4659
Adjusted R^2	0.583	0.620	0.508	0.570	0.568	0.750	0.663	0.319	0.666
State×Industry×Week Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix G. Bartik Diagnostics Appendix

This appendix presents diagnostic analysis of our PPP Exposure research design following the tests suggested in Goldsmith-Pinkham et al. (2020) for "unpacking the black box" of Bartik-style research designs. In Goldsmith-Pinkham et al. (2020), the Bartik estimator is shown to be a weighted average of just-identified, group-specific IV estimates β_k, where k denotes a group. The weights α_k are called Rotemberg weights. They combine the group-specific shock g_k with the covariance of group-region shares z_{lk} and the endogenous variable X to be instrumented. The estimand of interest is the effect of X on some outcome Y. (See Proposition 3, p.2600.)

In our setting, k refers to a bank, g_k is bank-level PPPE, and z_{lk} are bank-ZIP-level shares of the number of branches. In the main text, we focus on the reduced form relation between our Bartik instrument and various outcomes. Here, we use a two-stage setup where Y is the mean ZIP-level change in hours worked between the base period and week 18 (May 17–May 23) and X is the ZIP-level number of PPP loans received at the end of round one relative to the number of eligible establishments.

For reference, in this setup, the Bartik estimator β_{Bartik} estimated by 2SLS with standard errors clustered at the state-level is 0.41 (s.e.=0.099) with an F statistic of 1150. The regression in this setup conditions on the controls in our main regression in Table 5.

Table G.1 lists the ten banks with the most positive and most negative Rotemberg weights in the Bartik IV implementation of our PPPE research design. These weights reflect the banks' respective influence in the Bartik instrument, which combines bank-ZIP-level shares of the number of branches with bank-level PPPE. First, the table suggests that influential banks tend to be either large or mid-sized banks and those banks with PPPE that points to substantial overperformance or underperformance. For example, JP Morgan, Citibank, and Wells Fargo all have large Rotemberg weights. Second, Wells Fargo's weight is the only negative weight with absolute magnitude over 0.01. This fact indicates that more of the identifying variation comes from banks with positive weights, which enables the Bartik estimator to be interpreted as a LATE. Finally, the table suggests that the Bartik estimator reflects contributions from a large number of banks, as the top ten banks in terms of Rotemberg weight only contribute approximately 20% of total weight to the Bartik estimator. This result suggests our approach is not driven just by one or two banks, or even by the top-4 banks alone.

Table G.2 reports statistics about the Rotemberg weights in the Bartik IV implementation of our PPPE research design (following Table 1 in Goldsmith-Pinkham et al. (2020)). These weights reflect the banks' respective influence in the Bartik instrument, which combines bank-ZIP-level shares of the number of branches with bank-level PPPE. Panel A shows that banks with positive Rotemberg weights account for two-thirds of observations while banks with negative weights account for one-third of observations. However, Panel C shows that negative weights contribute relatively little to the overall estimator.

We also considered several of the Bartik diagnostics advocated by Goldsmith-Pinkham et al. (2020). Table G.3 presents bivariate regressions of bank branch shares (z_{lk}) on ZIP-level observables. Both branch shares and observables are residualized with respect to state dummies. Variables have been normalized, so the coefficients can be interpreted as a one-standard deviation change in x produces a β-standard deviation change in branch shares, where β is the reported coefficient. While bank-branch shares are modestly correlated with local observables—for example, branch concentration is higher in low income and less dense areas—all correlations are below 0.1 in absolute value, supporting our identification assumption of approximate independence of bank branch shares from the outcomes of interest.

Goldsmith-Pinkham et al. (2020) also advocate plotting pre-trends for research designs where regions are sorted based on the level of the Bartik instrument. We show in Figures 7 and 9 that pre-COVID, pre-PPP trends are very similar for high and low PPPE regions for a variety of outcomes. Our local projections in Figures 8 and 10 shows parallel trends in the short window after COVID lockdowns and prior to the PPP, once we condition on differences in targeting.

Table G.1: Unpacking PPP Exposure: Influential Banks

Table G.1 lists the ten banks with the most positive and most negative Rotemberg weights in the Bartik IV implementation of our PPPE research design. These weights reflect the banks' respective influence in the Bartik instrument, which combines bank-ZIP-level shares of the number of branches with bank-level PPPE. α_k refers to Rotemberg weight for bank k. g_k is bank-level PPPE. β_k is the just-identified coefficient. See Goldsmith-Pinkham et al. (2020) for additional discussion of these diagnostics.

Financial Institution Name	α_k	g_k	β_k
Top 10 Positive α_k			
KEYBANK NATIONAL ASSOCIATION	0.0667	0.3868	-0.1196
MANUFACTURERS AND TRADERS TRUST COMPANY	0.0585	0.3964	-0.2267
JPMORGAN CHASE BANK, NATIONAL ASSOCIATION	0.0264	-0.3596	-0.5714
CITIZENS BANK, NATIONAL ASSOCIATION	0.0244	0.2529	-0.2744
HUNTINGTON NATIONAL BANK, THE	0.0146	0.4204	1.4877
NBT BANK, NATIONAL ASSOCIATION	0.0143	0.3351	-0.2642
BANCORPSOUTH BANK	0.0123	0.3998	0.0079
CITIBANK, N.A.	0.0115	-0.4552	0.6701
CAPITAL ONE, NATIONAL ASSOCIATION	0.0113	-0.4988	0.2671
UNITED COMMUNITY BANK	0.0111	0.4436	0.6872
Bottom 10 Negative α_k			
WELLS FARGO BANK, NATIONAL ASSOCIATION	-0.0353	-0.4849	0.8005
TIMBERWOOD BANK	-0.0055	-0.5000	-0.8560
BANK OZK	-0.0053	0.4067	-2.0285
COMERICA BANK	-0.0048	0.2157	0.3192
BANK OF HAWAII	-0.0045	0.4407	0.5173
SOUTH STATE BANK	-0.0042	-0.5000	-1.7268
PEDESTAL BANK	-0.0041	-0.5000	-1.7775
BOKF, NATIONAL ASSOCIATION	-0.0041	0.4705	1.0714
TRI CITY NATIONAL BANK	-0.0040	0.4678	0.2811
WESBANCO BANK, INC.	-0.0040	0.3507	-1.9260

Table G.2: Unpacking PPP Exposure: Summary of Rotemberg Weights

Table G.2 reports statistics about the Rotemberg weights in the Bartik IV implementation of our PPPE research design (following Table 1 in Goldsmith-Pinkham et al. (2020)). These weights reflect the banks' respective influence in the Bartik instrument, which combines bank-ZIP-level shares of the number of branches with bank-level PPPE. α_k refers to Rotemberg weight for bank k. g_k is bank-level PPPE. β_k is the just-identified coefficient. $\mathbb{V}(z_k)$ refers to the variation in bank-ZIP-level shares within a bank across ZIPs. See Goldsmith-Pinkham et al. (2020) for additional discussion of these diagnostics.

	Sum	Mean	Share (N)	
Panel A. Negative and positive weights				
Negative	-0.6202	-0.0004	0.3348 (1,716)	
Positive	1.6202	0.0005	0.6652 (3,409)	
	α_k	g_k	β_k	$\mathbb{V}(z_k)$
Panel B. Correlations				
α_k	1.0000			
g_k	0.0405	1.0000		
β_k	0.0013	-0.0175	1.0000	
$\mathbb{V}(z_k)$	0.0475	-0.0296	-0.0026	1.0000
	α-weighted sum	Share of overall β	Mean	
Panel C. Estimates of β_k for positive and negative weights				
Negative	-0.0034	-0.0096	3.7104	
Positive	0.3578	1.0096	-5.7637	

Table G.3: Correlates of Bank Branch Shares

Table G.3 presents bivariate regressions of bank branch shares (z_{lk}) on ZIP-level observables. Both branch shares and observables are residualized with respect to state dummies. Variables have been normalized, so the coefficients can be interpreted as a one-standard deviation change in x produces a β-standard deviation change in branch shares, where β is the reported coefficient. ***, **, and *, represent statistical significance at 1%, 5%, and 10% levels, respectively, with standard errors clustered at the state level. See Goldsmith-Pinkham et al. (2020) for additional discussion of these diagnostics.

	LHS is Bank Deposit Shares		
	Coefficient	R^2	N
Log(Population)	-0.032***	0.0009	668583
	(0.004)		
Log(Population Density)	-0.068***	0.0034	668583
	(0.007)		
Social Distancing	0.068***	0.0023	669985
	(0.010)		
Covid Cases per Capita	-0.071***	0.0013	670219
	(0.008)		
Deaths per Capita	-0.030***	0.0005	670219
	(0.003)		
Unemployment Filing Ratios	0.023	0.0001	444375
	(0.016)		
Revenue Change of Small Business	-0.048***	0.0012	670219
	(0.012)		
Median Household Income	-0.055***	0.0019	670204
	(0.009)		