# Internet Appendix for The Targeting and Impact of Paycheck Protection Program Loans to Small Businesses

## APPENDIX A: Additional Model Results and Proofs of Propositions

In this section, we prove the main model proposition on the conditions under which direct provision, delegation, and regulation are optimal. We also provide an additional model result that makes the model more comparable to the actual design of the PPP program.

*Proof of Proposition 1:* If the banks hand out cash $e^{\phi\alpha+\xi}x^{\gamma-1}$ is constant over borrowers or $x = \left(e^{\beta}/k_B\right)^{\frac{1}{1-\gamma}}$, where $k_B$ solves the adding up constraint of $T = \int_\beta \left(e^{\beta}/k_B\right)^{\frac{1}{1-\gamma}} g(\beta)d\alpha$, or $T = (k_B)^{\frac{-1}{1-\gamma}} e^{\frac{\sigma_\beta^2}{2(1-\gamma)^2}}$, where $\beta = \phi\alpha + \xi$ and $\sigma_\beta^2$ is the variance of $\beta$. This implies that under bank landing, $x = Te^{\frac{\beta}{1-\gamma}-\frac{\sigma_\beta^2}{2(1-\gamma)^2}}$. Condition upon $\alpha$, welfare based on bank discretion is $\int_\xi e^{\alpha}\left(Te^{\frac{\phi\alpha+\xi}{1-\gamma}-\frac{\sigma_\beta^2}{2(1-\gamma)^2}}\right)^{\gamma} h(\xi)d\xi = e^{\frac{(1-\gamma+\phi\gamma)\alpha}{1-\gamma}} T^{\gamma} e^{-\frac{\gamma(\phi^2\sigma_\alpha^2)}{2(1-\gamma)^2}} e^{-\frac{\gamma\sigma_\xi^2}{2(1-\gamma)}}$. Integrating over $\alpha$ then yields total social welfare of $T^{\gamma}e^{\frac{(1-\gamma(1-\phi)^2)\sigma_\alpha^2-\gamma\sigma_\xi^2}{2(1-\gamma)}}$.

Under public lending, $e^{\theta\alpha+\zeta}x^{\gamma-1}$ is constant over borrowers and $x = \frac{T}{\delta}e^{\frac{q}{1-\gamma}-\frac{\sigma_q^2}{2(1-\gamma)^2}}$, where $q = \theta\alpha + \zeta$ and $\sigma_q^2$ is the variance of q. Welfare equals $\int_\alpha \delta e^{\alpha}\left(\frac{T}{\delta}e^{\frac{q}{1-\gamma}-\frac{\sigma_q^2}{2(1-\gamma)^2}},\right)^{\gamma} m(q)dq = \delta^{1-\gamma}T^{\gamma}e^{\frac{(1-\gamma(1-\theta)^2)\sigma_\alpha^2-\gamma\sigma_\zeta^2}{2(1-\gamma)}}$.

If loan sizes are fixed at a level T so that everyone receives a loan, then total public welfare is $T^{\gamma}e^{\frac{\sigma_\alpha^2}{2}}$ since the average value of $e^{\alpha}$ is $e^{\frac{\sigma_\alpha^2}{2}}$.

Comparing these three quantities, we have that private delivery with discretion yields higher social welfare than private delivery with fixed loan sizes if and only if $\sigma_\alpha^2(1-\gamma) > (1-\gamma(1-\phi)^2)\sigma_\alpha^2 - \gamma\sigma_\xi^2$ or $\frac{1}{2} > \frac{\phi\sigma_\alpha^2}{\sigma_\xi^2+\phi^2\sigma_\alpha^2}$ or $\frac{\sigma_\xi^2}{\sigma_\alpha^2} > 2\phi - \phi^2$, or $\phi < 1 - \sqrt{1-\frac{\sigma_\xi^2}{\sigma_\alpha^2}}$.

Welfare is higher with public delay than with private flexible allocation if and only if $\delta^{1-\gamma}T^{\gamma}e^{\frac{(1-\gamma(1-\theta)^2)\sigma_\alpha^2-\gamma\sigma_\zeta^2}{2(1-\gamma)}} > T^{\gamma}e^{\frac{(1-\gamma(1-\phi)^2)\sigma_\alpha^2-\gamma\sigma_\xi^2}{2(1-\gamma)}}$ or $\delta > e^{\gamma\frac{[(1-\theta)^2-(1-\phi)^2]\sigma_\alpha^2+\sigma_\zeta^2-\sigma_\xi^2}{2(1-\gamma)^2}}$. This condition clearly does not hold when $\delta = 0$ and must hold when $\delta = 1$ as we have assumed that $[(1-\phi)^2-(1-\theta)^2]\sigma_\alpha^2+\sigma_\xi^2-\sigma_\zeta^2 > 0$. As the left hand side is monotonic and continuous in $\delta$, there must exist a value of a firm survival rate, denoted $\delta^*$ between zero and 1, for which public welfare with immediate bank lending is equal to the public welfare with delayed targeting.

Delay and public discretion yields greater welfare than fixed loans if and only if $\delta^{1-\gamma}T^{\gamma}e^{\frac{(1-\gamma(1-\theta)^2)\sigma_\alpha^2-\gamma\sigma_\zeta^2}{2(1-\gamma)}} > T^{\gamma}e^{\frac{\sigma_\alpha^2}{2}}$ or $\delta > e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}}$. The function $e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}}$ is greater than $e^{\gamma\frac{[(1-\theta)^2-(1-\phi)^2]\sigma_\alpha^2+\sigma_\zeta^2-\sigma_\xi^2}{2(1-\gamma)^2}}$ if and only if $\frac{\sigma_\xi^2}{\sigma_\alpha^2} > 2\phi-\phi^2$. Our assumptions ensure that $[(1-\phi)^2-(1-\theta)^2]\sigma_\alpha^2+\sigma_\xi^2-\sigma_\zeta^2 > 0$, $-(\theta^2-2\theta)\sigma_\alpha^2-\sigma_\zeta^2 > 0$ and $e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}} < 1$ and hence there always there for $\delta$ close enough to one, public control dominates immediate fixed allocations, and for $\delta$ close to zero, fixed allocations dominate public delay. In this region, public delay provides higher welfare that immediate lending if and only if $\delta > \delta^*$, where $\delta^* = e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}}$.

Consequently, for all values of $\phi$, there exists a value of $\delta$ denoted $\delta^*$, such that delayed public allocation dominates either alternative if and only if $\delta > \delta^*$. If $\phi < 1-\sqrt{1-\frac{\sigma_\xi^2}{\sigma_\alpha^2}}$, then $\delta^*$ is falling with $\gamma, \theta$, and $\sigma_\alpha^2$, rising with $\sigma_\zeta^2$ and independent of $\sigma_\xi^2$ and $\phi$, and if $\delta < \delta^*$, then providing loans of fixed size generates higher social welfare than the two other alternatives. If $\phi > 1-\sqrt{1-\frac{\sigma_\xi^2}{\sigma_\alpha^2}}$, then $\delta^*$ is falling with $\gamma, \theta, \sigma_\xi^2$ and $\sigma_\alpha^2$, and rising with $\phi$ and $\sigma_\zeta^2$, and if $\delta < \delta^*$, then allowing private providers to allocate loans flexibly generates higher social welfare than either delayed public allocated or fixed loans sizes.

In the paper, we have either allowed total flexibility or a low fixed loan size, but neither of those assumptions fits perfectly with the implementation of the PPP in April 2020. There was a cap on loan size, but many loans came in below that cap. We now compare loans that are fixed in size at $T$, with loans that are fixed in size at $T' > T$. We continue to hold the total amount of funds fixed at $T$, so that banks can allocate more financing to the firms that they favor, but these larger loans cannot be distributed to the full measure 1 of firms. This proposition formally analyzes the recommendation of Hanson et al. (2020) that more smaller loans may be more advantageous than fewer larger loans. We now assume that $\beta = \phi\alpha + \vartheta\xi$, where $\vartheta = \sqrt{(\sigma_\beta^2 - \phi^2\sigma_\alpha^2)/\sigma_\xi^2}$ . This assumption allows us to vary the correlation between bank preferences and social preference ($\phi$), without varying the variance of $\beta$.

*Proposition 2: (i) If banks allocate loans of fixed size T', then if $\phi \leq 0$, it is never optimal to set $T' > T$.*

*(ii) If $\phi > 0$, then the optimal value of T' is greater than T.*

*(iii) If a loan size value T' yields the same social welfare as a loan size of T for a given value of $\gamma$, denoted $\hat{\gamma}$, then for all values of $\gamma > \hat{\gamma}$, a loan size of $T' > T$ will yield higher welfare than a loan size of T.*

*(iv) If a loan size value T' yields the same social welfare as a loan size of T for a given value of $\phi$, denoted $\hat{\phi}$, and if $\sigma_\xi^2 = K - \phi^2\sigma_\alpha^2$ for some constant K, then for all values of $\phi > \hat{\phi}$, a loan size of $T' > T$ will yield higher welfare than a loan size of T.*

Proposition 2 makes four claims about fixed loan amounts. If $\phi \leq 0$, then loans should be allocated equally across all firms. This case corresponds to zero or negative correlation between the desires of the bank and the social desirability of targeting a particular buyer. If $\phi > 0$, then some targeting is optimal. The case for targeting is stronger when $\gamma$ is higher, i.e., diminishing returns involved in lending are weaker. The case for targeting is also stronger when $\phi$ is higher, as long as the total variance of bank preferences is held constant. The implication is that better alignment of bank preferences and social preferences should lead to higher lending limits.

*Proof of Proposition 2:* If loan sizes are fixed at T'>T, then there will be a minimum value of $\beta = \phi\alpha + \vartheta\xi$ (where $\vartheta = \sqrt{(\sigma_\beta^2 - \phi^2\sigma_\alpha^2)/\sigma_\xi^2}$) that is serviced by the banks, and we denote that minimum $\hat{\beta}$, which solves $\frac{T}{T'} = 1 - G(\hat{\beta})$ or $T' = \frac{T}{\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta}$.

As T' determines $\hat{\beta}$ exactly, we will think of the social planner as choosing $\hat{\beta}$ rather than T' for mathematical convenience. Social welfare from lending equals $\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} T^\gamma \int_{\beta>\hat{\beta}} E(e^\alpha|\beta)\, g(\beta) d\beta$where $E(e^\alpha|\beta) = \frac{\int_\alpha e^\alpha h\left(\frac{\beta-\phi\alpha}{\vartheta}\right) f(\alpha) d\alpha}{\int_\alpha h\left(\frac{\beta-\phi\alpha}{\vartheta}\right) f(\alpha) d\alpha} = \frac{\int_\alpha e^\alpha e^{-\frac{(\beta-\phi\alpha)^2}{2\vartheta^2\sigma_\xi^2}} e^{-\frac{\alpha^2}{2\sigma_\alpha^2}} d\alpha}{\int_\alpha e^{-\frac{(\beta-\phi\alpha)^2}{2\vartheta^2\sigma_\xi^2}} e^{-\frac{\alpha^2}{2\sigma_\alpha^2}} d\alpha} = e^{\frac{2\beta\phi\sigma_\alpha^2 + \vartheta^2\sigma_\xi^2\sigma_\alpha^2}{2\sigma_\beta^2}}$

Hence the overall objective function is $e^{\frac{\sigma_\alpha^2}{2}} T^\gamma$ times $\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} \int_{\beta>\hat{\beta}} e^{\frac{-(\beta-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta = V(\hat{\beta}; Z)$, were Z is a vector of exogenous variables.

Welfare when everyone gets T equals $e^{\frac{\sigma_\alpha^2}{2}} T^\gamma$. Welfare when selected individuals receive T'>T, equals $T^\gamma e^{\frac{\sigma_\alpha^2}{2}}$ times $\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} \int_{\beta>\hat{\beta}} e^{\frac{-(\beta-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta = V(\hat{\beta}; Z)$, were Z is a vector of exogenous variables. We also know that $\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta = \frac{T}{T'}$, and that (using a simple change of variable so that $x = \beta - \phi\sigma_\alpha^2$, we have $\int_{\beta>\hat{\beta}} e^{\frac{-(\beta-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta = \int_{x>\hat{\beta}-\phi\sigma_\alpha^2} e^{\frac{-x^2}{2\sigma_\beta^2}} dx$.

Hence $\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} \int_{\beta>\hat{\beta}} e^{\frac{-(\beta-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta = \left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} \int_{\beta>\hat{\beta}-\phi\sigma_\alpha^2} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta$

If $\phi = 0$, then this equals $\left(\frac{T}{T'}\right)^{1-\gamma}$, which will be less than 1 whenever T'>T and $1 > \gamma$.

If $\phi < 0$, then $\int_{x>\hat{\beta}-\phi\sigma_\alpha^2} e^{\frac{-x^2}{2\sigma_\beta^2}} dx < \int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta = \frac{T}{T\prime}$ and so $V(\hat{\beta}; Z) < \left(\frac{T}{T\prime}\right)^{1-\gamma} \leq 1$, whenever T'>T and $1 > \gamma$. Consequently, it is never welfare enhancing to let T'>T if $\phi \leq 0$.

The derivative of $V(\hat{\beta}; Z)$ with respect to $\hat{\beta}$ yields:

$\gamma e^{\frac{-\hat{\beta}^2}{2\sigma_\beta^2}} \left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma-1} \int_{\beta>\hat{\beta}-\phi\sigma_\alpha^2} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta - \left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} e^{\frac{-(\hat{\beta}-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}}$, which is positive if and only if $\gamma > \frac{e^{\frac{2\phi\sigma_\alpha^2\hat{\beta}-\phi^2\sigma_\alpha^4}{2\sigma_\beta^2}} \int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta}{\int_{\beta>\hat{\beta}-\phi\sigma_\alpha^2} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta}$. If $\phi > 0$, then as $\hat{\beta}$ goes to negative infinity (which corresponds to T=T'), the right hand side of the equation goes to zero, and consequently, increasing T' above T is optimal.

The derivative of $V(\hat{\beta}; Z)$ with respect to $\gamma$ is $-\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} \int_{\beta>\hat{\beta}} e^{\frac{-(\beta-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta \, ln\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right) > 0$, and so that if some value of T'>T yields the same welfare as T for any value of $\gamma$, then for all values of $\gamma' > \gamma$, an allocation of T' will yield higher welfare than T.

If the variance of $\beta$ is independent of $\phi$, then the derivative of $V(\hat{\beta}; Z)$ with respect to $\phi$ (holding $\sigma_\beta^2$ constant) is positive, and given by $\sigma_\alpha^2 e^{\frac{-\hat{\beta}^2+2\phi\sigma_\alpha^2\hat{\beta}-\phi^2\sigma_\alpha^4}{2\left(\phi^2\sigma_\alpha^2+\sigma_\xi^2\right)}} \left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} > 0$. Consequently, if some value of T'>T yields the same welfare as T for any value of $\phi$,then for all values of $\phi' > \phi$ (holding $\sigma_\beta^2$ constant), T' will yield higher values of $V(\hat{\beta}; Z)$.

## APPENDIX B: Details about Machine Learning Approaches

In Section VII, we discuss the firm-level estimates of heterogeneous treatment effects presented in Table 5. This table reports estimates of treatment effects under different treatment allocation procedures using two approaches to estimating heterogeneous treatment effects: Generalized Random Forest IV (GRF IV) and LASSO. In this appendix, we discuss the details of these two estimators in more detail. We also discuss Appendix Table 9, which reports results from a number of alternative estimators to estimate firm level treatment effects.

The first approach to estimating firm-level treatment effect heterogeneity that we use in Table 5 is the GRF-IV approach from Athey, Tibshirani, and Wagner (2019). Standard errors in the table come from 150 bootstrap replications of the predictions. The characteristics we use are the months of cash available, monthly fixed expenditures pre-COVID, number of employees pre-COVID, total payroll pre-COVID, an indicator for a bank loan, an indicator for a loan officer relationship, two-digit industry dummies, the share of the local population receiving UI, the local COVID case rate, and state dummies. We incorporate dummy variables into the GRF model by calculating the average value of the outcome variable for each value of the dummy variable.

The second approach that we use in Table 5 is LASSO. Specifically, we fit models of the form:

$$y_i = X_i' \beta_1 + D_i \beta_2 + X_i' \times D_i \beta_3 + \epsilon_i$$

where $X_i'$ is a vector of characteristics for firm i. The estimated treatment effect for firm i is then $\widehat{\tau(X_\imath)} = y\_\imath(\widehat{X_\imath | D_\imath} = 1) - y\_\imath(\widehat{X_\imath | D_\imath} = 0)$. In the framework of Kunzel, et al (2019), this is an "S-Learner" where outcomes are predicted as a function of treatment, covariates, and treatment covariate interactions treating the treatment variable identically with other covariates. To avoid overfitting, we estimate this model using the Least Absolute Shrinkage and Selection Operator

(LASSO) with the plug-in value of the penalty term from Belloni, et al (2013). We will call the broad approach to computing heterogeneous treatment effects the "meta-learner" (S-learner in this case) and the approach to implementing the meta-learner the base-learner (LASSO in this case). The lasso models are fit using the rlasso Stata package with heteroskedastic errors after netting out state fixed effects (see Belloni et al. 2013). Standard errors in the table come from 150 bootstrap replications of the projections, with resampling over the banks used for the instrumental variables analysis in other treatment effect estimates and for the GRF IV model. The covariates included (other than PPP approval) are the same as those used in the GRF IV model above (except dummy variables are directly entered into the LASSO model rather than being incorporated as group means).

Appendix Table 9 then explores how these estimates of treatment effect heterogeneity and estimated average impacts under alternative allocation regimes vary with alternative approaches for estimating heterogeneous treatment effects. Different columns report results for different approaches. We explore different meta-learner approaches (i.e. what is predicted and how that is used to compute treatment effects), base-learner approaches (i.e. what prediction algorithm is used given the meta-learner approach), and methods of determining penalty parameters. For comparison, Columns (1) through (3) then report models fitted using additional versions of S-learners, with Column (1) reporting OLS estimates, Column (2) using LASSO with a cross-validated mean squared error minimizing penalty term rather than the plug-in estimator we use in Table 5, and Column (3) reporting results from a random forest model.

A drawback to S-learners is that, if the treatment variable is weak relative to other variables in explaining outcomes then the treatment variable may not be selected as a predictor, possibly biasing treatment effects towards zero. To understand the extent to which this concern is a problem, we explore alternative meta-learners that treat the treatment variable differently from other covariates, potentially alleviating this problem. Columns (4) through (13) report results for alternative meta-learners, reporting results using both LASSO and random forests as the base-learner for each meta-learner. Specifically, Columns (4) through (6) report results with a T-learner adopted from Foster (2013) and Athey and Imbens (2016) where outcomes are separately predicted conditional on covariates for treatment and control and then the difference in predicted outcomes at a particular covariate value is the predicted treatment effect. Columns (7) and (9) report results using X-learners, which were proposed by Kunzel, et al (2019) and separately estimate treatment effects within treated and control to address situations where potential outcomes are non-linear and treatment and control are very unbalanced in size. Columns (10) and (12) report results using an R-learner developed by Nie and Wager (2021), which takes advantage of the Robinson (1988) decomposition of treatment effects in terms of outcomes residuals and treatment residuals to produce a treatment effect estimator that is consistent even in high dimensional settings and attains uniformly valid inference. Finally, Column (13) reports results from the Causal Forest approach in Wager and Athey (2018).[1]

[1] Specifically, these different meta-learners are implemented as follows. Define $\mu_1(x) = E[Y\_i|X_i = x, D_i = 1]$ and $\mu_0(x) = E[Y_i|\, X_i = x, D_i = 0]$. In a T-learner, we first separately estimate $\widehat{\mu_1(X_\imath)}$ and $\widehat{\mu_0(X_\imath)}$ using the base-learner of choice, then we estimate the treatment effect as $\widehat{\tau(X_\imath)} = \widehat{\mu_1(X_\imath)} - \widehat{\mu_0(X_\imath)}$. Implementing an X-learner, starts in the same way by estimating $\widehat{\mu_1(X_\imath)}$ and $\widehat{\mu_0(X_\imath)}$. Then we compute $\widehat{\tau_1(X_\imath)} = Y_i - \widehat{\mu_0(X_\imath)}$ for observations where $D_i = 1$ and $\widehat{\tau_0(X_\imath)} = \widehat{\mu_1(X_\imath)} - Y_i$ for observations where $D_i = 0$. We then estimate the treatment effect for firm $i$ as $\widehat{\tau(X_\imath)} = g(X_i)\,\widehat{\tau_1(X_\imath)} + \big(1 - g(X_i)\big)\widehat{\tau_0(X_\imath)}$ where $g(X_i)$ is some weighting function, often the propensity score $e(\widehat{X_\imath})$. The R-learner was proposed by Nie and Wager (2021). Let $\widehat{m(X_\imath)} = E[\widehat{Y_\imath|X\_\imath}]$ and $\widehat{e(X_\imath)} = E[\widehat{D\_\imath|\mathrm{X\_\imath}}]$. Then we take advantage of the decomposition from Robinson (1998), which shows that $Y_i - m(X_i) = [\,D_i - \; e(X_i)]\tau(X_i) + \epsilon_i$. We estimate these conditional expectation functions, $\widehat{m(X_\imath)}$ and $\widehat{e(X_\imath)}$, using the base learner of choice. Using these estimates, we can then estimate the treatment effect by solving $\widehat{\tau()} = argmin_\tau \left\{ \sum_{i=1}^{n} \left( \left[ Y_i - \; m^{\widehat{-\imath}(X_\imath)} \right] - \left[ D_i - e^{\widehat{-\imath}(X_\imath)} \right] \tau(X_i) \right)^2 + \Lambda_{\mathrm{n}}(\tau(.)) \right\} \}$ where $\Lambda\_n(\tau(.))$ is a penalty on the complexity of the treatment effect heterogeneity function. The superscripts $\tau^{-i}$ indicate that we use cross-fitting to estimate the nuisance functions $\widehat{m(X_\imath)}$ and $\widehat{e(X_\imath)}$. Causal forests are an approach developed by Athey and Imbens (2016) that modifies random forests to minimize error in estimated treatment effects rather than error in

The results in Appendix Table 9 are qualitatively consistent with our estimates in Table 5 across all of the alternative specifications. In every empirical approach except one, the bank allocation has higher average treatment effects than a random allocation would have and, in most cases, the improvement is meaningful in magnitude, leading to an increase in jobs saved per $100,000 of over 1.4 in the short-run and over 0.13 in the long-run. Targeting small firms similarly performs worse than the observed allocation across all specifications, while targeting based on high frontline worker share also generally performs worse than observed bank targeting, though the magnitude of the difference is smaller than random targeting or small firm targeting. Assuming it were feasible, perfect targeting would cause more substantial gains in long-run average treatment effects, ranging from 150 to 400 percent. However, as we discuss in more detail in the main text, perfect targeting was likely infeasible. Furthermore, within-sample estimates of gains from alternative targeting regimes may tend to overstate potential gains from targeting.

predicting the outcome. Specifically, partitions of the covariate space are chosen to minimize within partition error in average treatment effects.

**Appendix C: Survey Instrument**

What impact are you currently experiencing from the Coronavirus Outbreak?

- ○ It's not impacting my business
- ○ It's starting to impact my business
- ○ It's really impacting my business
- ○ The impact is on the decline
- ○ The impact is over

Have you applied for any loans or assistance under the government's Payroll Protection Plan?

- ○ Approved, and I have received the funds
- ○ Approved, but I have not yet received the funds
- ○ Application is pending
- ○ Application was denied
- ○ I tried to apply but was unable to submit an application
- ○ I did not apply

When did you first apply for a loan?

Did your bank give you any of the following reasons for the denied loan application? (Please select all that apply)

- ☐ Insufficient documentation
- ☐ Did not meet federal qualification criteria
- ☐ Did not apply in time to receive funds
- ☐ Not a priority customer
- ☐ I received a different reason (not listed here)
- ☐ I did not receive a reason

How much assistance did you receive?

- Less than $10k
- Between $10-25k
- Between $25-50k
- Between $50-75k
- Between $75-100k
- Between $100-150k
- Between $150-300k
- Between $300-500k
- Between $500k-$1 million
- $1 million - $2 million
- $2 million - $3 million
- $3 million - $4 million
- $4 million - $5 million
- $5 million - $6 million
- $6 million - $7 million
- $7 million - $8 million
- $8 million - $9 million
- $9 million - $10 million

- $10 million - $20 million
- More than $20 million

Which of the following reasons describes why you did not apply? Please select all that apply.

- ☐ I can remain operational without extra cash
- ☐ I've already taken out a business loan and don't want to take on any more loans
- ☐ I don't want to deal with the hassle of applying
- ☐ I don't think I would receive the money in time for it to help my business
- ☐ I don't feel confident I can maintain my payroll for the loan to be forgiven
- ☐ I don't trust that the government will forgive my loan even if I maintain my payroll
- ☐ I don't trust that my bank will forgive my loan even if I maintain my payroll
- ☐ I don't believe I qualify for this loan (credit history, size of business, etc.)
- ☐ I don't trust that the COVID-19 disruptions will be over soon enough for my business to recover so I can maintain my payroll or pay back the loan
- ☐ I'm confused about the terms of the loan
- ☐ I would prefer other assistance that does not risk going into debt and being unable to pay it back
- ☐ I've applied for a loan before and was denied
- ☐ Closure is inevitable, even with the cash
- ☐ Other, please specify: ____________________________________________

How many of the following types of workers, **including yourself,** will your business employ in the first week of May?

_______ Full-Time employees
_______ Part-Time / Temporary employees

What is the likelihood of your business remaining operational by Dec. 31, 2020? Please provide your best guess.

- ○ Extremely Likely
- ○ Very Likely
- ○ Somewhat Likely
- ○ Somewhat Unlikely
- ○ Extremely Unlikely

What is the likelihood of your business remaining operational by Dec. 31, 2020? Please provide your best guess.

- ○ 0% Extremely Unlikely
- ○ 10%
- ○ 20%
- ○ 30%
- ○ 40%
- ○ 50%
- ○ 60%
- ○ 70%
- ○ 80%
- ○ 90%
- ○ 100% Extremely Likely

Is your business open?

- ○ Yes, it is currently open.
- ○ No, it is temporarily closed due to COVID-19, but I intend to reopen.
- ○ No, it is temporarily closed for other reasons, but I intend to reopen.
- ○ No, it is permanently closed due to COVID-19.
- ○ No, it is permanently closed for other reasons.

How many of the following types of workers, **including yourself,** did this business employ on January 31st before COVID-19 disruptions?

________ Full-Time employees
________ Part-Time / Temporary employees

How much was your typical monthly payroll before COVID-19 disruptions?

- Less than $10k
- Between $10-25k
- Between $25-50k
- Between $50-75k
- Between $75-100k
- Between $100-150k
- Between $150-300k
- Between $300-500k
- Between $500k-$1 million
- $1 million - $2 million
- $2 million - $3 million
- $3 million - $4 million
- $4 million - $5 million
- $5 million - $6 million
- $6 million - $7 million
- $7 million - $8 million
- $8 million - $9 million
- $9 million - $10 million

- More than $10 million

Some of your business expenses, like rent and interest payments, don't change even when you're not open. What was the total of these fixed expenses before COVID-19 disruptions, each month?

- Less than $10k
- Between $10-25k
- Between $25-50k
- Between $50-75k
- Between $75-100k
- Between $100-150k
- Between $150-300k
- Between $300-500k
- Between $500k-$1 million
- $1 million - $2 million
- $2 million - $3 million
- $3 million - $4 million
- $4 million - $5 million
- $5 million - $6 million
- $6 million - $7 million
- $7 million - $8 million
- $8 million - $9 million

- $9 million - $10 million
- More than $10 million

Consider the cash you have on hand today. How long will the cash you have today last under the current COVID-19 disruptions?

- Already gone
- Less than 2 weeks
- 2 weeks to 1 months
- 1 to 2 months
- 3 months or more

Have you taken the following actions? (select all that apply)

- ☐ Reduced Pay Rates (per person)
- ☐ Reduced Rent Payments
- ☐ Reduced Loan Payments
- ☐ Reduced Mortgage Payments
- ☐ None of the above

Who is your primary bank? (start typing, then select a name)

________________________________________________________

How likely are you to recommend your bank to someone else?

- ○ 0
- ○ 1
- ○ 2
- ○ 3
- ○ 4
- ○ 5
- ○ 6
- ○ 7
- ○ 8
- ○ 9
- ○ 10

What was the nature of your relationship with that bank? (Please select all that apply)

- [ ] I had a loan or credit card from the bank
- [ ] I had a business bank account
- [ ] I used the bank for services other than loans or a bank account
- [ ] I had a relationship with a banker or loan officer
- [ ] None of the above

How large was your typical loan balance with the bank in total ($) before COVID-19 disruptions?

- ◯ Less than $10k
- ◯ Between $10-25k
- ◯ Between $25-50k
- ◯ Between $50-75k
- ◯ Between $75-100k
- ◯ Between $100-150k
- ◯ Between $150-300k
- ◯ Between $300-500k
- ◯ Between $500k-$1 million
- ◯ $1 million - $2 million
- ◯ $2 million - $3 million
- ◯ $3 million - $4 million
- ◯ $4 million - $5 million
- ◯ $5 million - $6 million
- ◯ $6 million - $7 million
- ◯ $7 million - $8 million
- ◯ $8 million - $9 million
- ◯ $9 million - $10 million

○ More than $10 million

What is your main industry?

- Agriculture, Forestry, Fishing and Hunting
- Mining, Quarrying, and Oil and Gas Extraction
- Utilities
- Construction
- Manufacturing
- Wholesale Trade
- Retail Trade
- Transportation and Warehousing
- Information
- Finance and Insurance
- Real Estate and Rental and Leasing
- Professional, Scientific, and Technical Services
- Management of Companies and Enterprises
- Administrative Support or Waste Remediation Services
- Educational Services
- Health Care and Social Assistance
- Arts, Entertainment, and Recreation
- Accommodation and Food Services

- ○ Other Services (except Public Administration)
- ○ Public Administration

Once more for the books! What is the likelihood of your business remaining operational by Dec. 31, 2020? Please provide your best guess.

- ○ Extremely Likely
- ○ Very Likely
- ○ Somewhat Likely
- ○ Somewhat Unlikely
- ○ Extremely Unlikely

**Appendix D. Implied Excess Death Rates**

In this appendix, we assess the implications of our estimates of the effect of PPP on firm survival probabilities for aggregate firm death rates. According to Crane et al (2022), there are approximately 5.3 million firms in the US with typical exit rates of approximately 7.5% per year in the pre-Covid period, or approximately 400 thousand firms per year. Exit rates are substantially higher for small firms, as the employment-weighted death rate is around 2.5% per year, suggesting larger firms are more resilient. Combining data from a number of sources, including ADP, Homebase, Womply, and SafeGraph, Crane et al (2022) conclude that there were 100-200 thousand excess firm deaths in the first year of the Covid-19 pandemic. In other words, exit rates rose 2-4 percentage points.

How do these estimates compare with our findings? Panels C and D of Appendix Table A4 study the impact of PPP receipt on the probability a firm is open during our follow-up phone audit in July 2020. In our specification which reweights our sample to match the population of PPP recipients, PPP raises the probability the firm is open by 9 percentage points.

Define the treatment effect of PPP on eligible firms as

$$\tau = E\left[\mathit{death} \text{ without } \mathit{PPP} \mid \mathit{Covid}, \mathit{Eligible}\right] - \\ E\left[\mathit{death} \text{ with } \mathit{PPP} \mid \mathit{Covid}, \mathit{Eligible}\right].$$

In addition, define the rise in the death rate for eligible firms that would have occurred in the absence of PPP:

$$d_{elig} = E[\mathit{death} \text{ without } \mathit{PPP} \mid \mathit{Covid}, \mathit{Eligible}] - E[\mathit{death} \mid \mathit{nonCovid}, \mathit{Eligible}].$$

and define the efficacy of PPP as the treatment effect relative to this rise in death rates: $e = \tau / d_{elig}$.

With these definitions and a few assumptions, we can compare our estimates to the numbers in Crane et al (2022). First, we need to assume that impacts measured in July 2020 last through the end of the year. Second, we need an assumption on the efficacy of PPP to link our estimates of PPP's treatment effect to excess deaths for PPP-eligible firms. For simplicity, we will assume that *e = 100%*. While it may seem optimistic to assume that there were no excess deaths among PPP recipients, the evidence in Wang et al (2022) suggests that *e* may have even exceeded 100%—PPP may have forestalled failures that would have happened in normal, non-Covid times. Third, we need an assumption about excess deaths for PPP-ineligible firms. For simplicity, we assume that the excess death rate was the same as it would have been for eligible firms in the absence of PPP:

$$d_{inelig} = E[\mathit{death} \mid \mathit{Covid}, \mathit{Ineligible}] - E[\mathit{death} \mid \mathit{nonCovid}, \mathit{Ineligible}] \\ = d_{elig}.$$

This may be an overstatement, given that ineligible firms were larger. Under these assumptions, we have $\tau = d_{elig} = d_{inelig}$. We can then rewrite the aggregate excess death rate as

$$E\left[death \mid Covid\right] - E\left[death \mid noCovid\right]$$
$$= \Pr\left(Eligible\right)\left[d_{elig} - \Pr\left(Received \mid Eligible\right)\tau\right] + \left(1 - \Pr\left(Eligible\right)\right)d_{inelig}.$$

Roughly 70% of firms were eligible for PPP and roughly 95% of eligible firms received a loan (SBA 2021); our estimate of $\tau$ is 9%. With our assumptions, these numbers imply that the excess death rate would have been roughly 3%, which is consistent with the estimates of Crane et al (2022).

# Appendix Figures and Tables

### Figure A1: Distribution of Geographic Characteristics for Observations in Final Sample versus Observations that are Dropped

This figure displays densities of zipcode level characteristics for firms in our final sample and firms that were dropped due to incomplete survey data.  Each panel separately plots the distribution of 3-digit zipcode characteristics, such as population, median age, fraction college educated, per capita income, cumulative initial UI claims during Covid (from Opportunity Insights data) and cumulative Covid cases (from Opportunity Insights data).

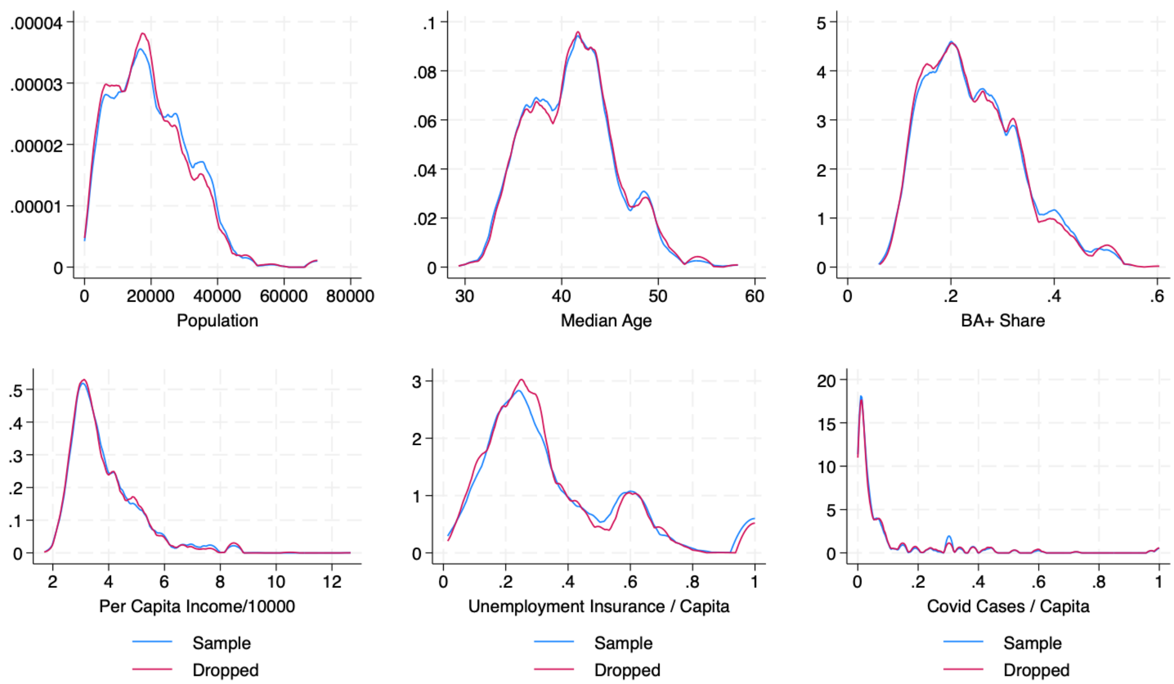


**Figure A2: Loan Sizes in the SBA Data and the Survey from Tranche 1**

Notes: Figure displays a cdf of loan sizes, which is right-continuous, meaning 80% of the survey loans are under $575,000 and 74% of the SBA loans in tranche 1 are under $575,000.

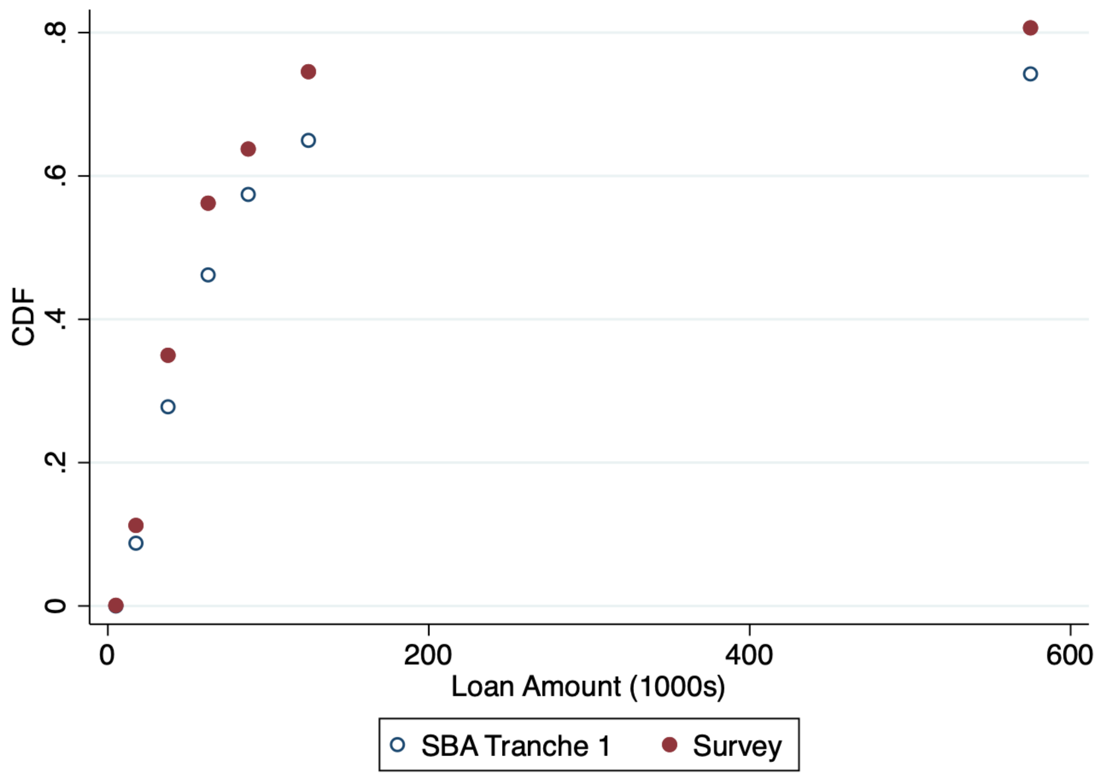
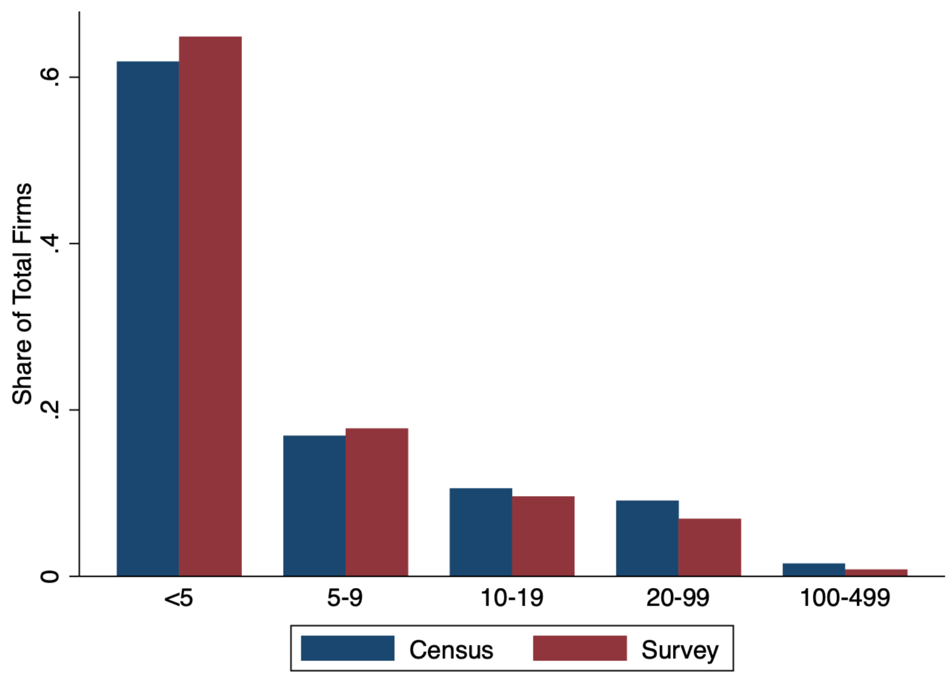


**Figure A3: Comparison of Survey and 2017 Census Firms**

Panel A: Firm Size by Employment for Those Under 500 Employees



Panel B: Top 10 States

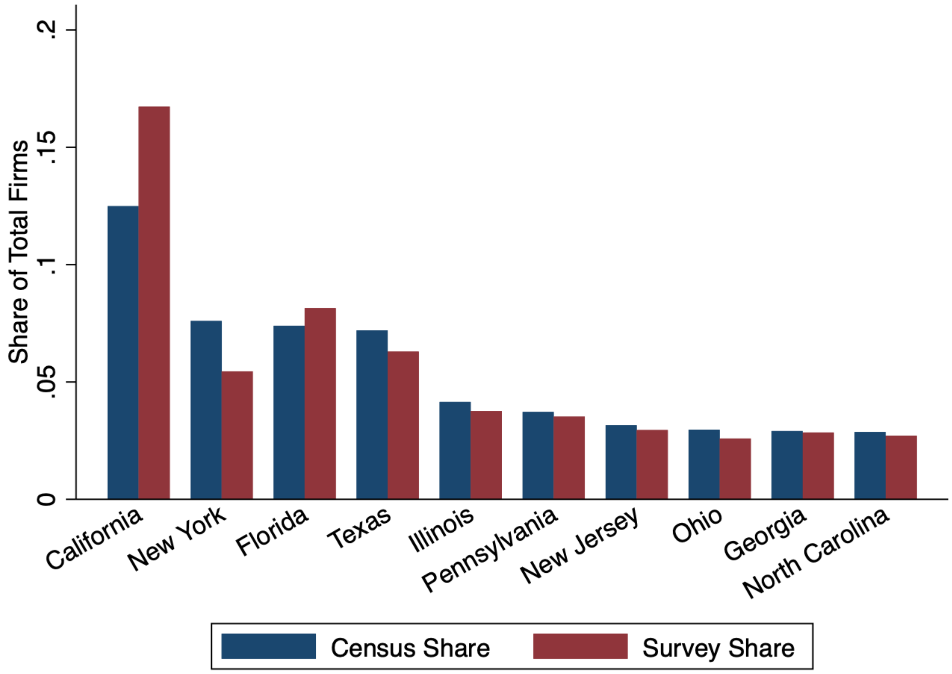


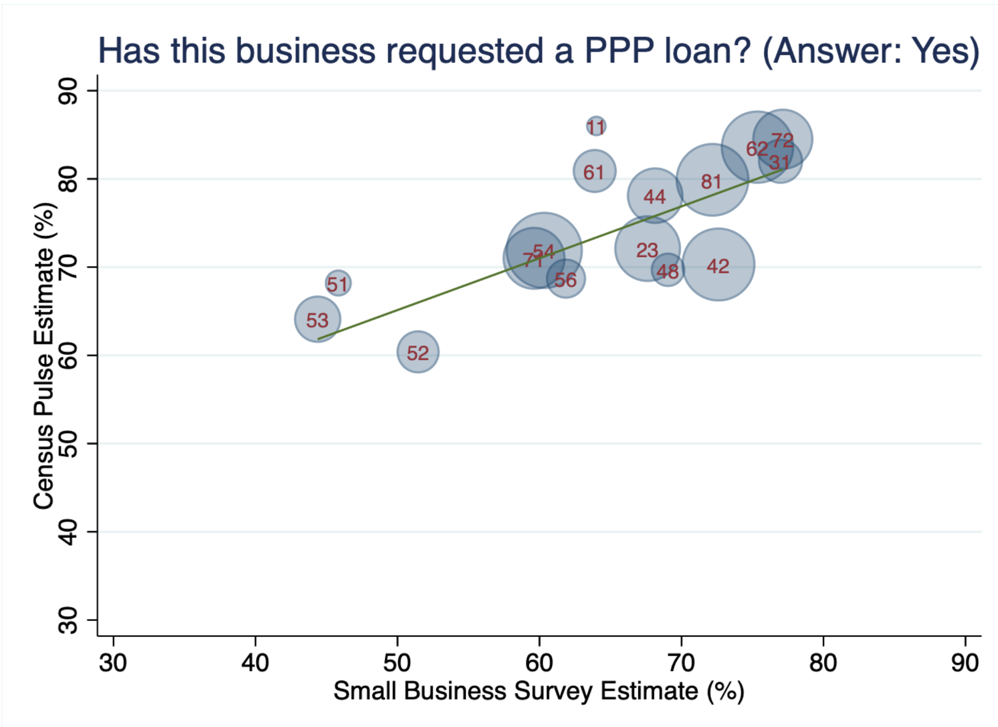
**Figure A4: Representativeness of Survey as Compared to Census Pulse**

This figure displays national-level Census Pulse estimates and Alignable survey estimates (using the main estimation sample) for the share of firms that apply to the PPP program and the share of firms that receive PPP loans. Note that we use the earliest Census Pulse, but it will have responses for firms that received loans in tranche 2 (which began 4/27/2020), whereas 90% of our sample was collected by 4/27/2020. This means that the Census Pulse should have a higher approval/receipt rate than our sample. In addition, note that the Pulse survey was also a non-representative sample, but was drawn from a different universe. The sampling frame was based on the census having an email address for the owner. As a result, the pulse under-represents firms with under 4 employees, as described here:
https://portal.census.gov/pulse/data/downloads/research/2021_sbps_nonresponse_bias_analysis_final.pdf

**Figure A5: Comparison of Census Pulse and Alignable Survey Across Industries**

This figure compares the Census Pulse and Alignable samples after excluding mining and utilities. The line of best fit is weighted by the number of businesses in the Alignable sample, as indicated by marker size. Employer-run businesses are excluded

Panel A: Applied for / requested PPP




Panel B: Received PPP

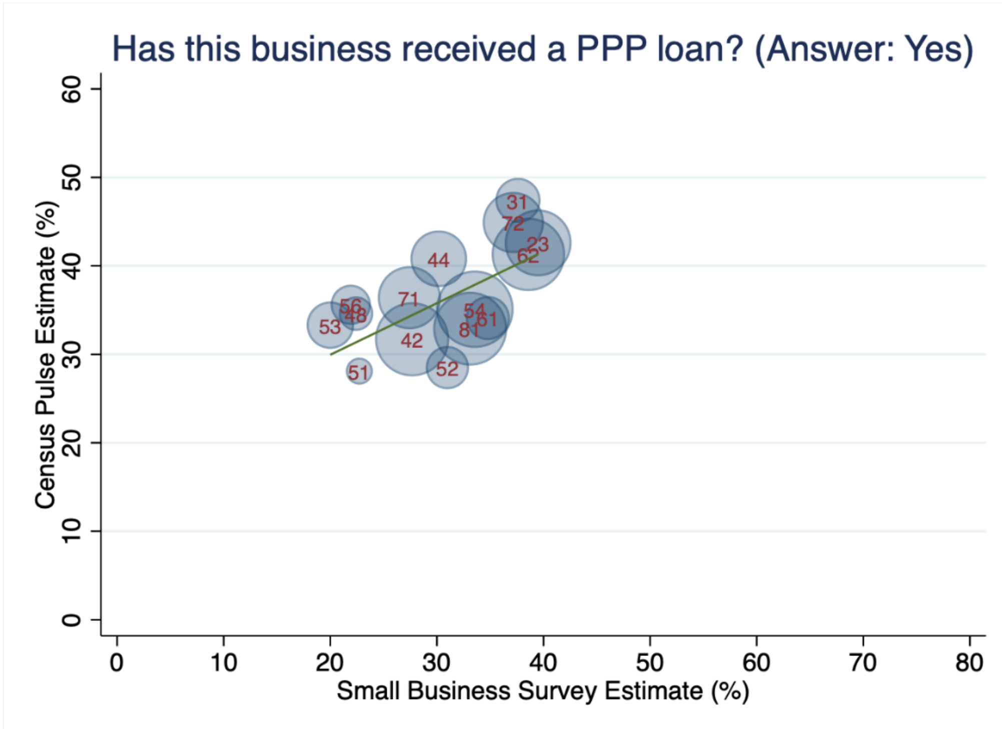

Has this business received a PPP loan? (Answer: Yes)
Census Pulse Estimate (%)
Small Business Survey Estimate (%)
0
10
20
30
40
50
60
70
80
31
72
23
62
44
71
56
48
53
54
61
81
42
52
51

**Figure A6: Propensity Scores by Treatment Group**

This figure displays treatment propensity scores densities as a function of covariates. We estimate propensity score models predicting being approved for PPP in the first round using the same controls we use to characterize treatment effect heterogeneity, specifically: months of cash available, monthly fixed expenditures pre-COVID, pre-Covid payroll, pre-COVID employment (categorical dummies for terciles), an indicator for a bank loan, an indicator for a loan officer relationship, and 2-digit industry dummies.

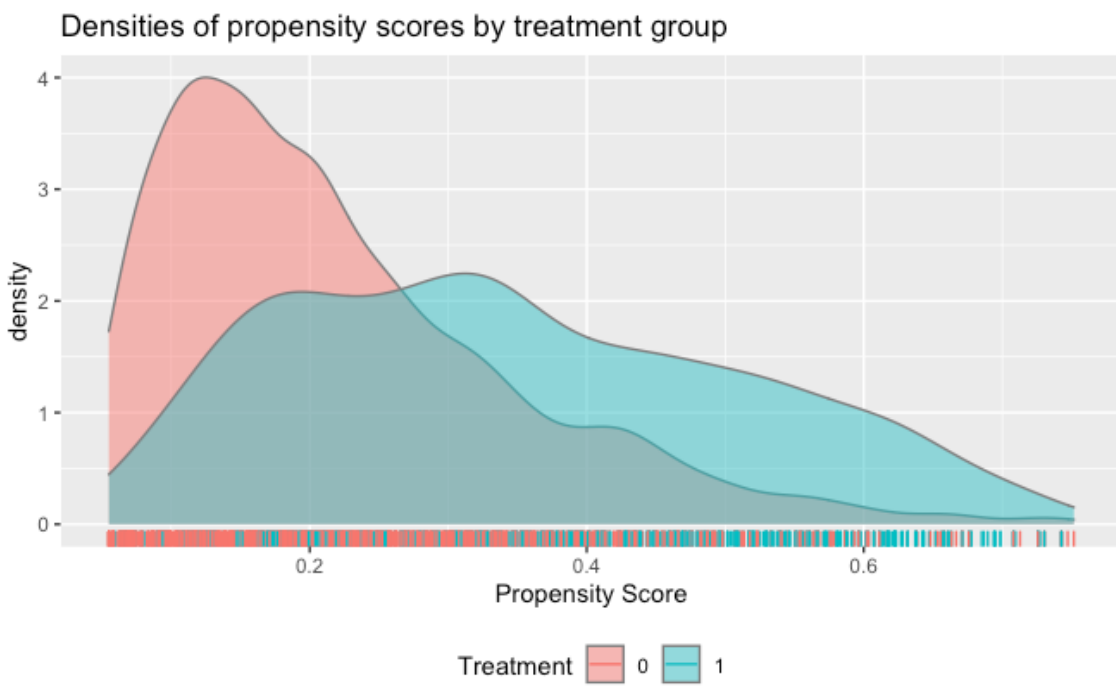


**Figure A7: Marginal Treatment Effect Estimates Using Alternative Specifications**

This figure reports estimates of the marginal treatment effect of PPP on firm self-reported survival probabilities by the unobserved resistance to treatment using alternative functional forms for modeling the marginal treatment effects. The solid line reports results assuming that the unobserved resistance to treatment and potential outcomes are jointly normally distributed, the dashed line relaxes this normality assumption by allowing potential outcomes to be a quadratic function of propensity scores, while the dotted line presents semi-parametric results by estimating the relationship between propensity scores and potential outcomes non-parametrically. In all specifications, we assume a linear relationship between covariates and potential outcomes. All specifications use the same covariates as our main specification, which include the months of cash available, monthly fixed expenditures pre-COVID, number of employees pre-COVID, total payroll pre-COVID, an indicator for a bank loan, an indicator for a loan officer relationship, zipcode characteristics about COVID cases and UI, and two-digit industry dummies. As in our other analysis, we instrument for PPP approval using the share of the PPP loans made during the first tranche and first 21 days of the second tranche of PPP that were made during the first tranche. We trim observations with propensity scores in the bottom or top one percent of our sample.

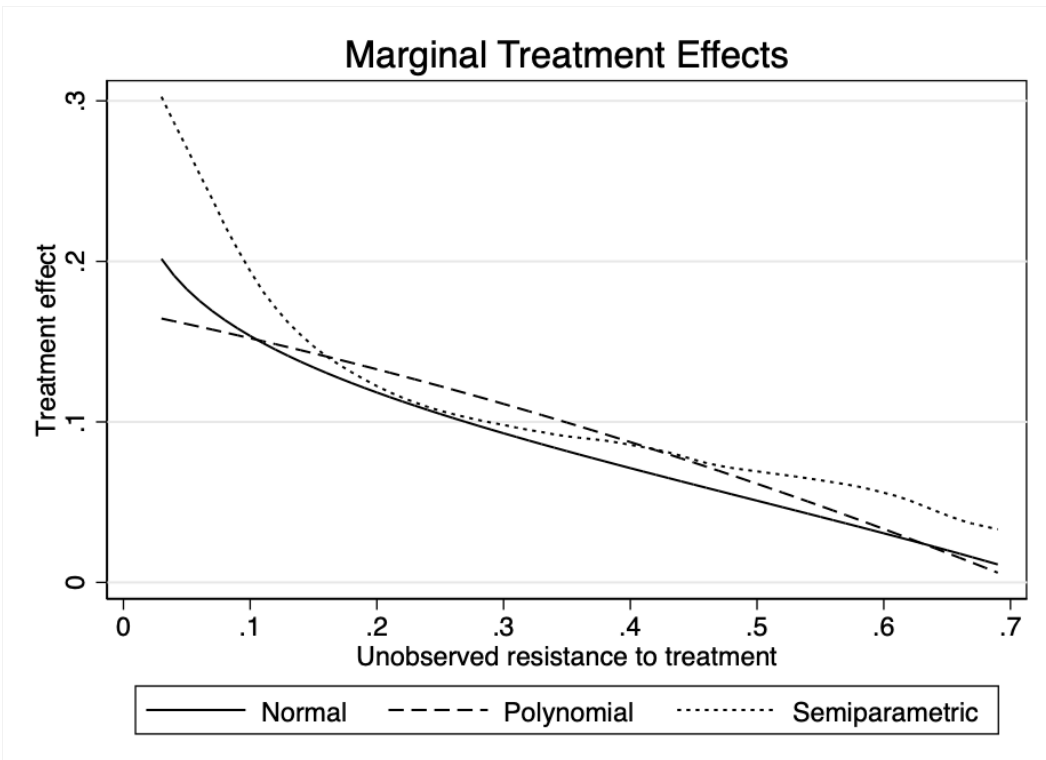


**Figure A8: Timing of EIDL Loan Disbursement as Reported in SBA Data**

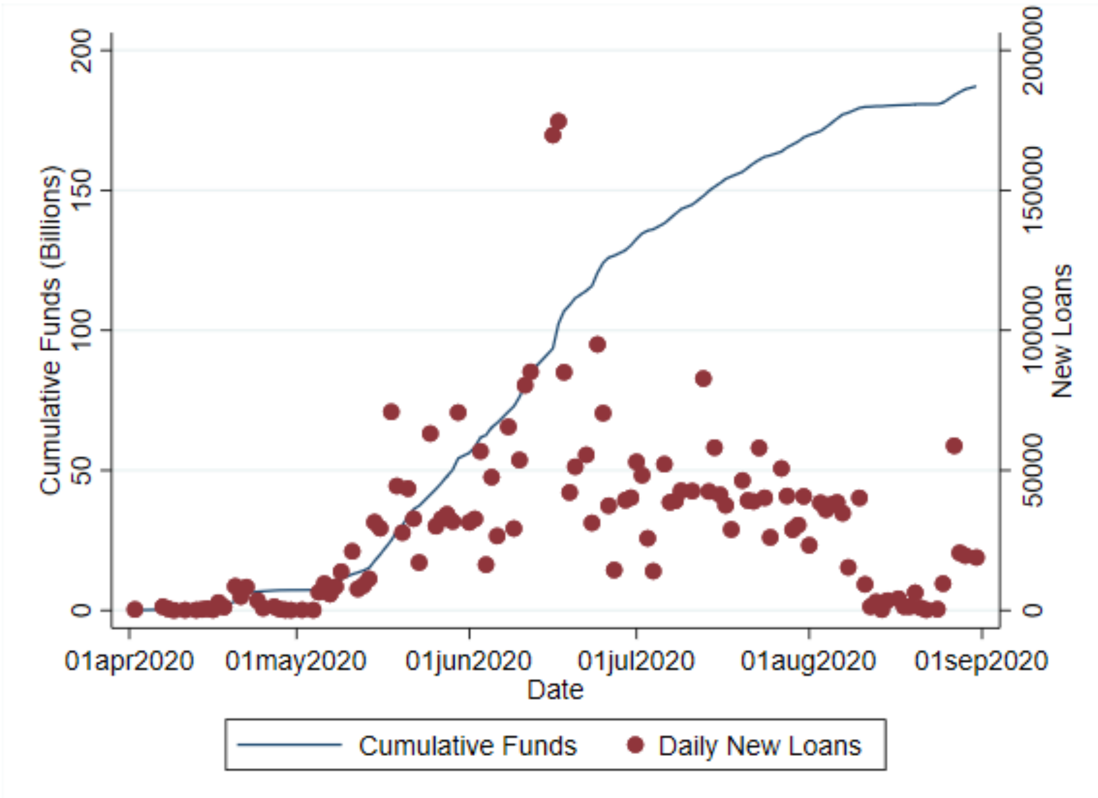


**Figure A9: Share of Loan Funds By 3-digit Zip Code for PPP and EIDL**

This figure presents a binned scatterplot with 50 bins at the 3-digit zipcode level, showing that the share of PPP dollars and the share of EIDL dollars overlap well. There is little evidence that the EIDL allocation differed systematically across zipcodes relative to the PPP allocation. The correlation between the two series is 0.79.

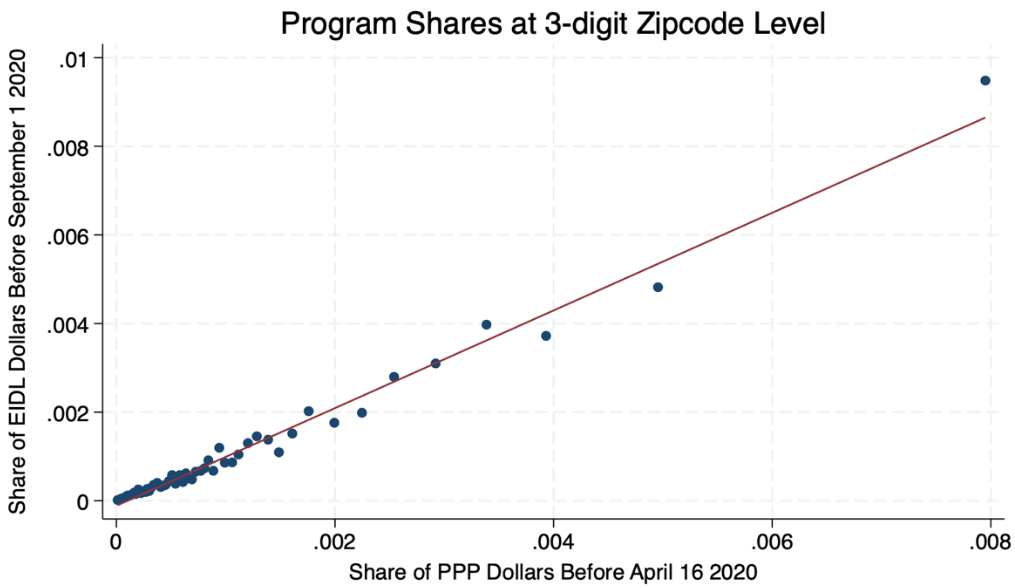
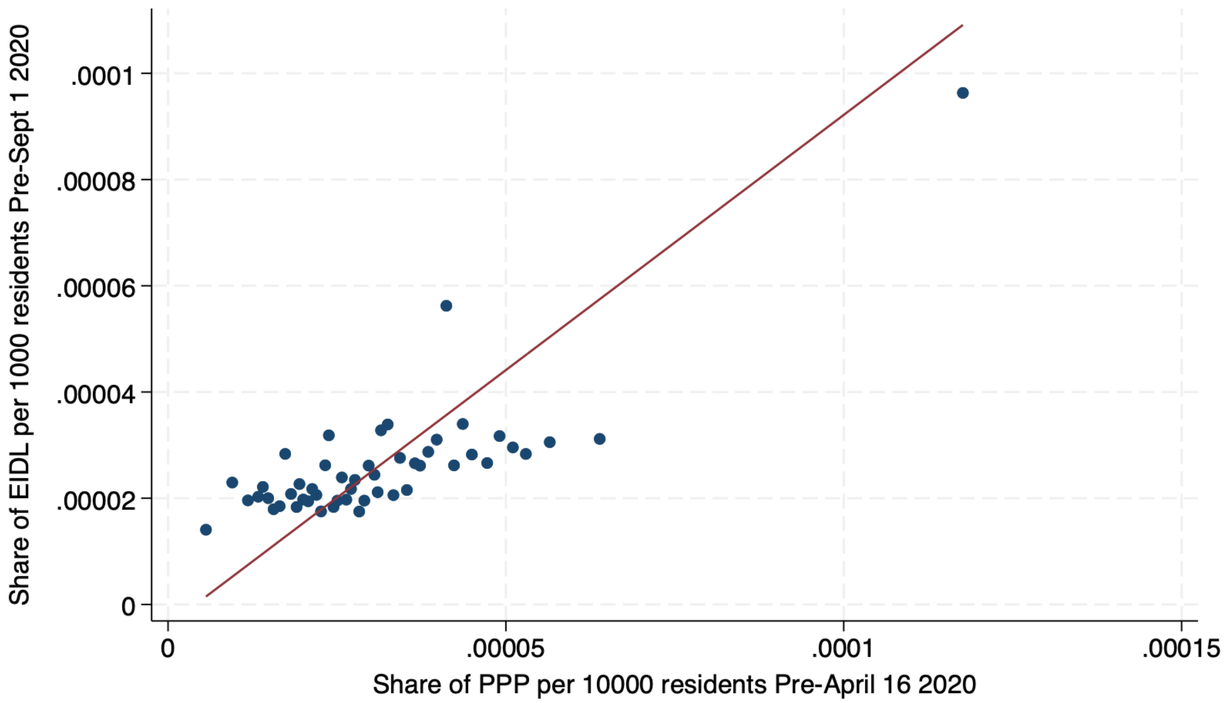


**Figure A10: Share of Per-Capita Loan Funds By 3-digit Zip Code for PPP and EIDL**

This figure plots per-capita loan funds (per-10,000 residents) at the 3-digit zipcode level. The correlation between the two series is 0.58.



**Appendix Table 1: Related Literature (Wraps over the next 9 pages)**

**Hubbard and Strain (2020)**

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** Effect of applying for PPP (no round distinction)
- **Who results apply to given data and strategy:** Eligible Establishments (1-500 employees) that applied for loans of $150,000 or more (in both tranches of PPP loans)
- **Time horizon:** Nov 2019 - Aug 2020
- **Data sources / Sample:** Dun & Bradstreet Corporation-- identify businesses that applied for a loan of $150,000 or above. Sample includes establishments active in D&B in October 2019
- **Estimated Effects on Firm Survival:**
  - 0.24 pp reduction in probability of an establishment going out of business
  - 0.47 pp reduction in probability of an establishment going out of business
  - 0.22 pp reduction - ITT estimate
- **Estimated effects (% effect on employment per firm)**
  - 0.9% increase in employment relative to control group of similar establishment size (1-500 employees) - TOT estimate
  - 1.78% increase in employment on establishments relative to the control group of ineligible establishments (501-1000 employees) - TOT estimate
  - 1.38% increase in employment - ITT estimate
- **Heterogeneity in employment effects:**
  - Split sample into firms with 1-250 employees and 251-500 employees. Find a 0.94% increase in employment for firms with size 1-250 that applied for PPP; for the firms with 251-500 employees that applied for PPP, they find a 3.2% decrease in employment
  - Restrict sample to firms in size group 400-475 and compare to firms with 525-600 employees regardless of whether they applied for loans: find no effect (very wide standard errors)
- **Heterogeneity in firm survival effects:**
  - Split sample into firms with 1-250 employees and 251-500 employees. Find a 0.24 pp increase in probability of going out of business for firms with size 1-250; for the firms with 251-500 employees, they find a 0.61 pp reduction in probability of going out of business
  - Restrict sample to firms in size group 400-475 and compare to firms with 525-600 employees regardless of whether they applied for loans: find no effect (very wide standard errors)

**Autor et al (2022a)**

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** Effect of *eligibility* for PPP in either round

- **Who results apply to given data and strategy:** Firms *eligible* for PPP (firms with 250-500 employees)
- **Time horizon:** Jan 2020 - Dec 2020
- **Data sources / Sample:** Panel of firm-week from ADP, a private firm that processes payroll data for about 26 million workers in the US in a month; PPP data from SBA; BLS
- **Estimated Effects (total jobs saved):**
  - PPP increased aggregate employment by 3.6 million in mid-May 2020; 1.4 million at the beginning of December 2020
- **Estimated Effects ($/jobs saved):**
  - $169,000 - $258,000
- **Estimated effects (% effect on employment per firm)**
  - 2-5% increase in employment around mid-May 2020; 0-3 % increase (imprecise) in early December 2020 - ITT estimate
- **Heterogeneity in employment effects:**
  - Restrict samples to different employment size thresholds around PPP eligibility thresholds. Peak estimated effects in mid-May of around 2% (+/- 250 around threshold); 3.5% (+/- 150 around threshold); 5% (+/- 100 around threshold)

**Autor et al (2022b)**

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** 1) Effect of *receiving* PPP loan in either round 2) Effect of eligibility for PPP in 2021 round of PPP
- **Who results apply to given data and strategy:** 1) Firms that received PPP (firms with 1-49 employees) 2) Firms eligible for PPP (firms with 200-300 employees)
- **Time horizon:** 1) Jan 2020-July 2020 2) Dec 2020-Apr 2021
- **Data sources / Sample:** Panel of firm-week from ADP, a private firm that processes payroll data for about 26 million workers in the US in a month; PPP data from SBA; BLS
- **Estimated Effects (total jobs saved):**
  - Combining estimates for larger firms (250-500 employees), estimate 3 million job-year saved by PPP
- **Estimated Effects ($/jobs saved):**
  - Avg. cost of $169,300 per job-year saved
- **Estimated Effects on Firm Survival:**
  - 8 pp effect reduction in employment losses due to firm closures
- **Estimated effects (% effect on employment per firm)**
  - 12% increase in employment 5 weeks after
- **Heterogeneity in firm survival effects:**
  - They have a descriptive plot for other firm size classes (Fig 4) but only estimate the effect of PPP for firms with less than 50 employees

**Chetty et al (2020)**

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** Effect of *eligibility* for PPP in either round
- **Who results apply to given data and strategy:** Firms *eligible* for PPP (firms with 100-500 employees, excluding food services)

- **Time horizon:** Feb 2020-Aug 2020
- **Data sources / Sample:** Consumer spending data from Affinity solutions; Small Business Revenue Data from Womply; Job Postings data from Lightcast; employment data from Paychex, Intuit, Earnin; UI data from state agencies; COVID incidence from NYT, JHU, CDC and Dept. of Health and Human Services; Mobility data from Google Mobility Reports
- **Estimated Effects ($/jobs saved):**
  - $377,000; $359,000 after netting out potential UI payments
- **Estimated effects (% effect on employment per firm)**
  - 1.01-1.78 pp increase in employment (imprecise)

**Doniger and Kay (2021)**

- **Empirical approach:** Difference-in-differences
- **Estimand:** Effect of delay in PPP loans (1st round vs second round). Loan delay is measured as the share of PPP ($ value) issued between in first 2 days of second tranche as a share of $ value of loans issued in a CBSA in last 2 days of first trance and first 2 days on second tranche
- **Who results apply to given data and strategy:** Firms that got PPP approved in last 3 days of first round vs firms that got PPP in the first 2 days of second round PPP
- **Time horizon:** Jan 2020 - Dec 2020
- **Data sources / Sample:** SBA PPP Loan Level Data, aggregated by date and CBSA using HUD crosswalks; Labor market data from Current Population Survey (CPS)
- **Estimated Effects (total jobs saved):**
  - 3.5 million jobs
- **Estimated Effects ($/jobs saved):**
  - $150,000
- **Estimated effects (% effect on employment per firm):**
  - 1 pp increase in PPP loan delay increased unemployment by 0.083 pp (8.3 basis points) in May 2020; persistent effects of ~5 basis points until December 2020. Using a broader measure of nonemployment to include nonparticipants, they find a 12.5 basis points effect in May 2020
- **Heterogeneity in employment effects:**
  - Find significant heterogeneity by class of worker and size of firm. Aggregate effect driven largely by the effect on smallest firms (less than 10 employees) ~3.5x (29 basis points in May 2020) larger effect on smallest firms. No effect (insignificant) for other firm size classes.  14.5 basis point increase in unemployment for self-employed in May 2020; 8.2 basis points for private employees; insignificant effect for public employees

**Granja et al (2022)**

- **Empirical approach:** Instrument (PPP bank exposure) - use the gap between market share of a bank in PPP lending in the first round and its pre-pandemic small business lending
- **Estimand:** They don't explicitly address the first vs second round in their research design, but argue that their estimates for April and May reflects effect of receiving funds vs not receiving funds, and those of June reflects effects of early vs late receipt
- **Who results apply to given data and strategy:** Compare firms in areas with high bank PPP exposure to those in low bank PPP exposure
- **Time horizon:** Jan 2020 - Aug 2020

- **Data sources / Sample:** PPP data - SBA and Department of Treasury; Reports of Condition and Income (Call Reports) filed by all active commercial banks in 1st quarter of 2020; Homebase (private firm that manages scheduling and time clock) data -- establishment level weekly employment indicators. This data disproportionately covers small firms and in F&B service and retail; County-by-week initial unemployment claims from state agencies; Small business revenue data (county aggregate of credit card spending) from Womply; additional county-level employment data from Opportunity Insights
- **Estimated Effects (total jobs saved):**
    - 2.02 - 3.28 million jobs saved in first 5 months
- **Estimated Effects ($/jobs saved):**
    - $175,000
- **Estimated Effects on Firm Survival:**
    - Effectively no effect on firm survival. 0.6 pp decrease in business shutdowns in areas with high PPP exposure
- **Estimated effects (% effect on employment per firm):**
    - 1 pp increase in employment in May, 2pp increase in employment in June

**Dalton (2021)**

- **Empirical approach:** Doubly robust dynamic difference-in-difference design (Callaway and Sant'Anna (2020))
- **Estimand:** Effect of receiving a PPP loan
- **Who results apply to given data and strategy:** Universe of UI-paying establishments covering 95% of employment in the US
- **Time horizon:** Feb 2020-March 2021
- **Data sources / Sample:** PPP loan data from SBA; Employment and wages data from Quarterly Census of Employment and Wages (QCEW) -- covers more than 95% of all employment; Business Response Survey - online survey related to pandemic and federal grants; Occupational Employment and Wage Statistics Survey (OEWS) ; Current employment statistics (CES) survey
- **Estimated Effects ($/jobs saved):**
    - $20,000-$34,000 per employee month retained; $240,000-$408,000 per employee year
- **Estimated Effects on Firm Survival:**
    - 5.8% drop in closure in first month after loan receipt, drop to ~3.5 after 7 months
- **Estimated effects (% effect on employment per firm):**
    - 6.7-8 % increase in employment in first month after loan receipt, persistent at ~ 4% after 7 months
- **Heterogeneity in employment effects:**
    - Small, young and low-wage firms have largest effect; 9% increase in employment for firm size 1-10 in first month after PPP approval, persistent effect ~4% until 7 months after; 6% increase in first month for firm size class 10-50; 2% for firm size class 50-100; zero for firms size class 100+. $ per employee month retained ranges from ~21,000 for firms with <10 employees; ~27k for firms with size 10-50; ~58k for firms with size 50-100; to ~$291,000 for firms with 100+ employees. $ per employee month retained ranges from ~6,800 for firms in wage class <20k; ~13k for firms in wage class 20-40k; ~27k for firms in wage class 40-60k; ~$55k for firms in wage class >80k. They also estimate heterogenous effects by firm age but do not report the estimated effects in the paper

**Faulkender, Jackman and Miran (2020)**

- **Empirical approach:** Instrument for loan receipt with local banking market structure (community bank market share) across counties
- **Estimand:** Dynamic effect of early PPP loans on county-level UI claims (focus on 1st tranche)
- **Who results apply to given data and strategy:** The estimates apply to smaller firms as well with a weaker financial situation (in contrast to eligibility-based approaches)
- **Time horizon:** Feb 2020 - Oct 2020
- **Data sources / Sample:** PPP loan data from SBA; County-level unemployment insurance claims data from BLS; Community bank penetration data using FDIC Summary of Deposits data; County-level payroll eligible for PPP from Census Statistics of Business 2017
- **Estimated Effects (total jobs saved):**
    - 18.6 million (moving from 25th pctile to 75th pctile of counties by PPP coverage, resulted in unemployment rate improvement by 12 pp, extrapolated nationally assuming marginal effects equal the average treatment effect for within this IQR
- **Estimated effects (% effect on employment per firm):**
    - 10 pp increase in early PPP coverage of eligible payroll led to a smaller jump in initial UI claims between 1-2 pp of UI covered employment

**Li and Strahan (2020)**

- **Empirical approach:** Reduced form estimate of predicted PPP using variation in lending across counties.
- **Estimand:** Instrument for county level PPP lending
- **Time horizon:** Jan 2020-July 2020
- **Data sources / Sample:** Bank level lending data by combining Call Reports data with PPP data from SBA. County unemployment data from BLS. Small Business Revenue and Local Spending are from tracktherecovery.org (Opportunity Insights team)
- **Estimated Effects (total jobs saved):**
    - In the absence of delay, employment would have been 3 million more in July 2020 (10% higher); 1.8 million higher in January 2021 (6% higher)
- **Estimated effects (% effect on employment per firm):**
    - Only provide reduced form estimate of predicted PPP. 1 standard deviation increase in predicted PPP lowers unemployment rate in June by 0.21 pp

**Kurmann et al (2021)**

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** Effect of delay in PPP loans (1st round vs second round). Loan delay is measured as the share of PPP ($ value) issued in first week of second tranche as a share of dollar value of loans issued last week of 1st tranche and 1st week of second tranche in a county. Similar to Doninger and Kay (2022)
- **Who results apply to given data and strategy:** Firms that got PPP approved in last week of first round vs firms that got PPP in the first week of second round PPP. Focus on four sectors: Retail trade Education and Health, Leisure and Hospitality and Other Services
- **Time horizon:** Feb 2020 - Nov 2021

- **Data sources / Sample:** Homebase data - establishment level weekly employment indicators; Match Homebase data to SafeGraph, Google and Facebook; PPP data from SBA
- **Estimated Effects (total jobs saved):**
    - In the absence of delay, employment would have been 3 million more in July 2020 (10% higher); 1.8 million higher in January 2021 (6% higher)
- **Estimated Effects on Firm Survival:**
    - 1 pp increase in PPP loan delay increased business closing in a by 0.1 pp in May 2020; peaking around 0.2 pp in Fall 2020, with effect declining back to 0.1pp by Feb 2021 (relative to mid-Feb 2020). They estimate aggregate reduction in permanent closing of about 5% in the absence of delay in PPP loans
- **Estimated effects (% effect on employment per firm):**
    - 1 pp increase in PPP loan delay decreased county employment by 0.1 pp in May 2020; 0.25 pp in August 2020, with effect persisting up to Feb 2021 (relative to mid-Feb 2020 employment)

**Humphries et al (2020)**

- **Empirical approach:** Simple regression with controls using survey data. They only report associations and don't allude to any effect of PPP as causal.
- **Who results apply to given data and strategy:** Small firms with less than 50 FTE in January 2020
- **Time horizon:** March 2020-May 2020
- **Data sources / Sample:** Survey data on small and medium sized (<50 FTE employees) businesses about employment and future expectations. Survey not designed to be nationally representative
- **Estimated Effects on Firm Survival:**
    - Conditional on applying for PPP loan, those that are approved are, on average, 12 pp more likely to report that they will recover in the next 2 years. Additionally, the reported probabilities of bankruptcy or closure in next 6 months are 11 pp lower.
- **Estimated effects (% effect on employment per firm):**
    - Conditional on applying for PPP loan, those that are approved are, on average, 11 pp less likely to have fewer employees than in January
- **Heterogeneity in employment effects:**
    - Most of the effects are driven by firms with 0-4.5 FTE and 5-9.5 FTE. For firms with 10-50 FTE, only the probability of closure or bankruptcy (~10pp) is statistically indistinguishable from zero

**Joaquim and Wang (2022)**

- **Empirical approach:** Instrument PPP loans received by firms in a county by the community bank share. They argue that community bank share is not a valid instrument without accounting for firm's pre-covid financial condition.
- **Time horizon:** Jan 2019 - Aug 2021
- **Data sources / Sample:** Dun & Bradstreet Corporation data for firms' financial health; PPP data from SBA; County Business Patterns (2020); Local Area Unemployment Statistics; FDIC Summary of Deposits for community bank exposure
- **Estimated effects (% effect on employment per firm):**

    - 0.5-1 pp increase in county level employment in first 5 months after Apr 2020 (using community bank share instrument); effects become indistinguishable from zero after accounting for firm's pre-covid financial condition. (figure 3)
- **Heterogeneity in employment effects:** Find large heterogeneity in the effect of PPP on firm's financial condition

**Joaquim and Netto (2021)**

- **Empirical approach:** Model based estimation to decompose the reduced form estimate into ATT, selection effects, and targeting effects using a two-stage M-estimator
- **Estimand:** Effect of receiving PPP loan on county level nonemployment
- **Time horizon:** March 2020-Aug 2020
- **Data sources / Sample:** PPP data from SBA; FDIC Summary of Deposits, Call reports to construct bank exposure instrument; County Business Patterns (2020) and Survey of US Businesses (2017) ; BLS Local Area Unemployment Statistics; Small Business Pulse Survey for firms' self-reported effects of the pandemic; tracktherecovery.org (Opportunity Insights team) for county level measures of employment,, revenue, spending, Covid-cases and deaths, mobility and unemployment insurance claims
- **Estimated Effects (total jobs saved):**
    - PPP reduced non-employeemnt by 7.5 million jobs by end of second tranche
- **Estimated Effects ($/jobs saved):**
    - $70,000 per job
- **Estimated effects (% effect on employment per firm):**
    - 12.5 pp increase in employment for firms that received PPP (model-based decomposition). They argue that first round had very limited employment effects due to targeting of firms with lowest treatment effects (small and imprecise estimate for April). Relatively larger and persistent effects until end of their study period with ATT growing over time (~7 pp in May, ~12.5 pp in August)

**Staples and Krumel (2022)**

- **Empirical approach:** Difference in difference and Propensity Score Matching; LPM with controls
- **Estimand:** Effect of receiving PPP in last week of first tranche vs receiving PPP in first week of second tranche
- **Who results apply to given data and strategy:** Craft breweries that received PPP late in first tranche (for YoY production effects); Craft breweries that received PPP and were operational on first day of first tranche (Apr 3, 2020)
- **Time horizon:** 2019-2020 for YoY production changes; July 2021 for firm survival effects
- **Data sources / Sample:** PPP data from SBA; Data on production and revenue changes of craft breweries from Brewers Association; operational status in July 2021 using Google searches (temporarliy close, permanently closed) and Brewers Association data
- **Estimated Effects on Firm Survival:**
    - Estimate the probability of being open in July 2021 using LPM with controls and county fixed effects. Receiving PPP in first round (both tranche) increased brewery survival probability by 5 pp and receiving PPP in second round (late 2020 and 2021) increased probability of being open by 7pp. Underestimate firm survival effects as they exclude firms that closed before first day of PPP receipts

**Denes, Lagaras and Tsoutsora (2021)**

- **Empirical approach:** Difference-in-differences
- **Estimand:** Effect of delay in receiving PPP (end of first tranche vs early second tranche). Relatively longer run effects on firm survival
- **Who results apply to given data and strategy:** Exploit the abrupt ending of PPP funds in the first tranche on April 16 and consider it an exogenous shock to the timing of PPP allocation to firms. Compare the firms that receive PPP at the beginning of second tranche (first 4 days starting April 27) to those that receive PPP at the end of first tranche (last 4 days leading up to April 16) in a difference-in-difference design. This allows them to estimate the marginal effect of a delay in receiving PPP loans under the identifying assumption that without a delay between first and second tranche, firms in both groups would follow parallel trends
- **Time horizon:** Apr-21
- **Data sources / Sample:** Experian Credit Risk Dataset; Your-Economy Time Series Data from Business Dynamics Research Consortium for firm-year level data on location, employment and sales; SafeGraph Data to measure in-store activity; Operational status data (temporarily, permanently closed) of firms from Google Maps in April 2021
- **Estimated Effects on Firm Survival:**
    - 0.3 pp increase in temporary closure and 0.3 pp increase in permanent shutdowns using firm's status on Google Maps in April 2021. The estimated effect corresponds to a 17% increase relative to sample mean

**Berger et al (2022)**

- **Empirical approach:** Linear probability model with controls using firm level survey data; matching
- **Estimand:** Effect of reeving PPP in first round (both tranche)
- **Who results apply to given data and strategy:** Firms that received PPP in first round
- **Time horizon:** Apr 2020 - Feb 2021
- **Data sources / Sample:** Survey data from National Federation of Independent Businesses (NFIB) Small Business Economic Trends between Apr 2020 - Feb 2021; PPP data from SBA; Covid related case rate and lockdowns data from tracktherecovery.org
- **Estimated effects (% effect on employment per firm):**
    - 4.8 pp increase in probability of increasing hiring in next 3 months after receiving PPP. 4 pp increase in probability of increasing employee compensation in next 3 months
- **Heterogeneity in employment effects:**
    - Analyze heterogeneity by industry and firm size. Effects largely driven by firms in industries hit hard by COVID (hospitality and construction), by smaller firms (with less than 10 employees) and those in low-income counties

**Barraza et al. (2020)**

- **Empirical approach:** Instrument for PPP loans using pre-pandemic density of SBA member bank offices in a county
- **Who results apply to given data and strategy:** Compare firms in counties high PPP exposure to those in low PPP exposure using bank density as instrument
- **Time horizon:** Jan 2020- Apr 2020

- **Data sources / Sample:** County-level labor market data from BLS and from tracktherecovery.org (Opportunity Insights Team); Banking data from Summary of Deposits; PPP data from SBA
- **Estimated effects (% effect on employment per firm):**
    - 1.4 % lower unemployment in counties with higher SBA member office densities

**Cole (2022)**

- **Empirical approach:** Dynamic difference-in-difference event study
- **Estimand:** Effect of applying for PPP (no round distinction)
- **Who results apply to given data and strategy:** Very small firms (median of 5 employees) that applied to PPP in the Southwest US
- **Time horizon:** Jan 2020 - Sept 2020
- **Data sources / Sample:** Monthly firm level data from a private payroll processor in American Southwest (don't mention the name of the provider)
- **Estimated Effects (total jobs saved):**
    - 3.43 million jobs saved
- **Estimated Effects ($/jobs saved):**
    - $270,000 - $313,000 per job per year
- **Estimated effects (% effect on employment per firm):**
    - 7.5% increase in employment in 5 months after applying for PPP. 14 % increase in first month with effects disappearing after 5 months. No effect of PPP on new hiring,
- **Heterogeneity in employment effects:**
    - Analyzes heterogeneity along prevalence of hourly workers in an industry, ability to work from home and for essential businesses. 8.5% increase in employment in industries in the lowest tercile of hourly wage workers, no effect on high hourly worker industries; 19% increase in average employment for most remote capabilities, 9% increase for low remote capabilities; 11.6% increase in essential businesses, 4.6% increase for non-essential businesses (all effects in the 5 months following PPP)

**Bartlett and Morse (2021)**

- **Empirical approach:** Logit with controls and fixed effects
- **Estimand:** Effect of applying and receiving PPP
- **Who results apply to given data and strategy:** Firms in City of Oakland, California. Survival effects estimated on a small sample of 278 firms
- **Time horizon:** Follow-up Survey conducted in June 2020
- **Data sources / Sample:** City of Oakland Small Business Survey about resiliency during Covid pandemic (round 1 in March 2020; follow-up round 2 on a small subset in early June 2020); Hand-collected data from Google Maps and Company websites on operational status of firms between April 24, 2020-May 3, 2020; Homebase data for employment indicators; SafeGraph foot traffic data between Jan-Apr 2020
- **Estimated Effects on Firm Survival:**
    - 20.5% increase in self-reported medium-run survival probability (within 6 months from June 2020 for microbusinesses with 1-5 employees). No effect for short-term closure
- **Heterogeneity in firm survival effects:**
    - They find that medium-run survival probability to varies with firm size (negative estimate for interaction of pre-COVID size and PPP), suggesting that the increase in

survival probability decreases with firm size and the effect becomes immaterial after ~20 employee size

**Table A2: Summary Statistics For the Full and Final Samples**

This table reports summary statistics for survival expectations and January employment across different samples. Survival expectations are the probability a firm expects to be open in December 2020. January 2020 employment is the number employees prior to Covid. Missing cells reflect that either survival expectations or employment measures are not fully populated for the sub-sample in a given row. Later rows report these measures for different sub-samples where they are observed. The sample with the complete set of regressors means that we observe PPP application and receipt status, cash on hand, survival expectations, January 2020 and contemporaneous employment, pre-Covid payroll, fixed expenses, banking relationship information, naics industry codes, and contemporaneous operating status. The final two rows compare the estimation sample with the sample not used in estimation but with data on the outcomes of interest. The 75th (90th) percentile of January 2020 employment in the estimation sample is 7 (15) and in the non-estimation sample it is 6 (16).

| | | N | Mean Survival Expectation | Std. Dev. Survival Expectation | Mean Jan 2020 Employment | Std. Dev. Jan 2020 Employment |
|---|---|---|---|---|---|---|
| (1) | All Responses from US Firms | 16826 | | | | |
| (2) | (1) and dropping firms with over 500 employees or implausible payroll to employment values | 16808 | | | | |
| (3) | (2) and dropping firms that didn't answer the PPP application question | 11472 | | | | |
| | *For firms that answered the PPP question and pass employment filters:* | | | | | |
| (4) | (3) and dropping firms that didn't answer survival expectations question | 7906 | 0.734 | 0.281 | | |
| (5) | (3) and dropping firms that didn't answer Jan 2020 employment | 9576 | | | 8.060 | 24.554 |
| (6) | (3) and dropping firms that didn't answer banking relationship question | 8798 | | | | |
| (7) | (3) with matched instrument in SBA Data | 8741 | | | | |
| (8) | (3) with complete set of regressors | 6640 | 0.732 | 0.282 | 7.750 | 22.463 |
| | *Firms that are not in the final sample in (8), with N denoting available data* | | | | | |
| (9) | Firms in '(2) but not in '(8) with survival expectations | 3076 | 0.742 | 0.266 | | |
| (10) | Firms in '(2) but not in '(8) with Jan 2020 employment | 2963 | | | 8.760 | 28.718 |

**Table A3: Summary Statistics Illustrating Speed versus Steering Impacts**

This table displays different cuts of bank steering and speed based on the SBA and survey data at the bank-level. Bank speed is based on the number of loans in tranche 1 over total loans in tranche 1 and the first 21 days of tranche 2. Bank steering is the difference in the average number of jobs per loan in Tranche 1 and the average number of jobs per loan in the first 21 days of tranche 2. We classify high speed and high steering banks based on the sample median using the survey data regardless of PPP application. The survey data may differ from the representativeness of the SBA data due to nonrandom sampling. To account for this, we compute an adjustment based on the difference in medians in the raw SBA and the survey data for borrowers with approved loans before April 17, 2020. We then apply this adjustment to the median in the survey data for all firms to get the implied median in the SBA data.

| | Classification Based on Survey Median | | Classification Adjusted for SBA Thresholds | |
|---|---|---|---|---|
| | **Low Speed** | **High Speed** | **Low Speed** | **High Speed** |
| **Low Steering Banks** | | | | |
| Approval Given Application | 0.13 | 0.39 | 0.15 | 0.42 |
| Survival Expectation Given Application | 0.68 | 0.73 | 0.68 | 0.72 |
| Open in July 2020 Given Application | 0.60 | 0.70 | 0.60 | 0.70 |
| Number of Applicants | 865 | 1,275 | 928 | 854 |
| Application Rate | 0.66 | 0.65 | 0.66 | 0.64 |
| **High Steering Banks** | | | | |
| Approval Given Application | 0.13 | 0.33 | 0.16 | 0.38 |
| Survival Expectation Given Application | 0.68 | 0.73 | 0.69 | 0.74 |
| Open in July 2020 Given Application | 0.64 | 0.70 | 0.66 | 0.70 |
| Number of Applicants | 1,258 | 987 | 1,745 | 858 |
| Application Rate | 0.67 | 0.71 | 0.68 | 0.71 |

**Table A4: Regressions Assessing How Firm Characteristics Vary with the Instrument**

This table displays regressions of the speed instrument on firm characteristics in the survey. Columns 1-3 do not condition on PPP applicant, while Columns 4-6 do. Columns with bank type fixed effects use 3 categories: large banks, small banks, and credit unions. Large banks are Bank of America, Capital One, Citibank, Citizens, Fifth Third, JPMorgan Chase, Key, PNC, TD Bank, Truist, US Bank, and Wells Fargo. Small banks are all other non-credit union banks. The standard deviation of fitted values is the standard deviation of the instrument explained by the characteristics in each column and does not include the fixed effects. The overall standard deviation of the instrument in the full sample and the applicant sample is 0.25.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| January 2020 Employees | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Monthly Payroll | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Fixed Expenses | -0.000 | 0.000 | -0.000 | -0.000 | 0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Average Zip Code Proximity | 0.005 | 0.041 | -0.030 | 0.005 | 0.057 | -0.009 |
| | (0.014) | (0.036) | (0.019) | (0.016) | (0.049) | (0.027) |
| Zipcode Covid Cases/Capita | -0.111* | -0.270*** | -0.157 | -0.127* | -0.273*** | -0.156 |
| | (0.057) | (0.064) | (0.082) | (0.055) | (0.080) | (0.100) |
| Zipcode UI Claims/Capita | -0.163*** | 0.004 | 0.027 | -0.166*** | 0.016 | 0.035 |
| | (0.034) | (0.035) | (0.031) | (0.039) | (0.042) | (0.038) |
| Frontline Industry | 0.014* | 0.012* | 0.004 | 0.012 | 0.012 | 0.003 |
| | (0.006) | (0.006) | (0.004) | (0.007) | (0.007) | (0.006) |
| Weeks of Cash | 0.002** | 0.002** | 0.001 | 0.004*** | 0.004*** | 0.002** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Bank Loan | -0.048*** | -0.041*** | -0.010 | -0.044*** | -0.037*** | -0.009 |
| | (0.009) | (0.009) | (0.007) | (0.010) | (0.010) | (0.007) |
| Bank Officer | 0.098*** | 0.084*** | 0.040*** | 0.092*** | 0.078*** | 0.038*** |
| | (0.016) | (0.012) | (0.010) | (0.016) | (0.012) | (0.009) |
| R-Squared | 0.035 | 0.177 | 0.486 | 0.037 | 0.184 | 0.487 |
| Observations | 6640 | 6640 | 6640 | 4452 | 4452 | 4452 |
| Applicant Sample | N | N | N | Y | Y | Y |
| State Fixed Effects | N | Y | Y | N | Y | Y |
| Bank Type Fixed Effects | N | N | Y | N | N | Y |
| Standard Deviation of Fitted Values | 0.045 | 0.048 | 0.030 | 0.047 | 0.057 | 0.022 |

**Table A5: Characteristics and Potential Correlation with the Instrument Across Geographies**

Regressions of the tranche 1 ratio instrument in the SBA approved loan sample for tranche 1 borrowers on 5-digit zipcode characteristics from the 2019 5 year ACS data and county-level economic indicators from the first half of March 2020, taken from Opportunity Insights Womply and Paychex data. Opportunity Insights data coverage depends on the series, so columns 3-6 are run separately for their small business performance and employment series. Columns 7 and 8 use all of these measures. Standard errors are clustered by 3-digit zipcode. The standard deviation of fitted values is the standard deviation in the instrument explained by the characteristics in each column and does not include the fixed effects. The overall standard deviation of the instrument in the sample is 0.198.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Median Age in County | 0.000 | 0.000 | -0.000 | 0.001* | 0.000 | 0.001 | -0.000 | 0.001 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Share with BA+ Degree | -0.027 | -0.028 | 0.015 | -0.014 | 0.033 | -0.008 | 0.030 | -0.000 |
| | (0.026) | (0.025) | (0.026) | (0.024) | (0.032) | (0.027) | (0.029) | (0.025) |
| Per Capita Income / 1000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.001 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Zipcode Covid Cases/Capita | -0.140*** | -0.164*** | -0.132*** | -0.163*** | -0.131*** | -0.170*** | -0.129*** | -0.165*** |
| | (0.019) | (0.026) | (0.018) | (0.027) | (0.018) | (0.028) | (0.018) | (0.028) |
| Zipcode UI Claims/Capita | -0.043* | 0.023 | -0.039 | 0.023 | -0.038 | 0.027 | -0.034 | 0.024 |
| | (0.020) | (0.026) | (0.022) | (0.029) | (0.023) | (0.031) | (0.023) | (0.032) |
| Small Businesses Open Relative to Jan 2020 | | | -0.266** | -0.187* | | | -0.375*** | -0.300** |
| | | | (0.081) | (0.087) | | | (0.093) | (0.100) |
| Small Bus Revenue Relative to Jan 2020 | | | 0.230*** | 0.129** | | | 0.240*** | 0.139** |
| | | | (0.040) | (0.043) | | | (0.052) | (0.053) |
| Employment of First Quartile Earners | | | | | -0.060 | -0.075 | -0.025 | -0.066 |
| | | | | | (0.047) | (0.044) | (0.045) | (0.044) |
| Employment of Second Quartile Earners | | | | | -0.025 | 0.125* | -0.052 | 0.115 |
| | | | | | (0.076) | (0.063) | (0.076) | (0.068) |
| Employment of Third Quartile Earners | | | | | 0.111 | 0.048 | 0.083 | 0.034 |
| | | | | | (0.061) | (0.051) | (0.066) | (0.057) |
| Employment of Fourth Quartile Earners | | | | | -0.048 | -0.092 | -0.033 | -0.092 |
| | | | | | (0.060) | (0.053) | (0.069) | (0.059) |
| R-Squared | 0.02 | 0.06 | 0.02 | 0.06 | 0.02 | 0.06 | 0.02 | 0.06 |
| Observations | 1882567 | 1882567 | 1413459 | 1413459 | 1275066 | 1275066 | 1216029 | 1216029 |
| State FE | No | Yes | No | Yes | No | Yes | No | Yes |
| Standard Deviation of Fitted Values | 0.028 | 0.026 | 0.032 | 0.030 | 0.027 | 0.032 | 0.031 | 0.033 |

**Table A6: Estimates of PPP Effects on Survival Expectations and Future Operational Status**

This table reports OLS and IV estimates relating whether a firm was approved for the PPP program as of April 25, 2020 to its expectations of survival at the time of the survey, and operational status as-of late July 2020. The sample is restricted to firms that applied for PPP, including firms that were ultimately denied and firms that tried to apply but were unable to submit an application. In Panels A and B, the dependent variable is the probability a firm expects to be open in December 2020, reported in 10 percentage point increments. The dependent variable is in raw units. In Panels C and D, the dependent variable is an indicator that the firm is open in late July of 2020 from a phone audit of a random sample of respondent businesses. Answers are coded as 1 if the owner answers yes and 0 for owners who answer no or if there is no response to two separate phone calls. Column 1 includes the full sample without conditioning on covariate availability (except for banking relationship information to compute the instrument in Panels B and D). Column 2 restricts the sample to have available covariates, X. Column 3 controls for industry and state, Column 4 for whether the business was open at the time of the survey, and Column 5 for its remaining cash on hand. Column 6 controls for average proximity of employees to one another based on the zipcode's industry distribution in the County Business Patterns data, Covid cases per-capita, and UI claims from Opportunity Insights. Column 7 reweights the sample to match the industry and size composition of the population of firms receiving PPP in SBA data. There are 35 firms that have zero weights because of sparsity of some reweighting cells. Column 8 adds fixed effects for bank type (12 large banks, small banks, and credit unions). The instrument for PPP approval in Panels B and D is the tranche 1 share of loans approved relative to all loans within the first 3 weeks of tranche 2, calculated from the SBA administrative data. The first stage F statistics is reported below these panels. All standard errors are clustered by bank and reported in parentheses.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Panel A: OLS Estimates of Survival Expectations | | | | | | | | |
| PPP approved | 0.140*** | 0.14*** | 0.14*** | 0.13*** | 0.09*** | 0.09*** | 0.15*** | 0.08*** |
| | (0.00692) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| R2 | 0.0480 | 0.05 | 0.11 | 0.16 | 0.22 | 0.22 | 0.12 | 0.22 |
| N | 5038 | 4452 | 4452 | 4452 | 4452 | 4452 | 4414 | 4414 |
| Panel B: IV Estimates of Survival Expectations | | | | | | | | |
| PPP approved | 0.169*** | 0.19*** | 0.20*** | 0.18*** | 0.13*** | 0.13*** | 0.24*** | 0.15*** |
| | (0.0268) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.04) | (0.03) |
| N | 4634 | 4452 | 4452 | 4452 | 4452 | 4452 | 4414 | 4452 |
| First Stage F | 320.6 | 233.4 | 183.8 | 174.4 | 159.9 | 168.2 | 144.0 | 162.0 |
| Panel C: OLS Estimates of July 2020 Operational Status | | | | | | | | |
| PPP approved | 0.15*** | 0.16*** | 0.14*** | 0.13*** | 0.09*** | 0.09*** | 0.14*** | 0.09*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| R2 | 0.02 | 0.02 | 0.08 | 0.08 | 0.10 | 0.11 | 0.08 | 0.11 |
| N | 3040 | 2749 | 2749 | 2749 | 2749 | 2749 | 2726 | 2749 |
| Panel D: IV Estimates of July 2020 Operational Status | | | | | | | | |
| PPP approved | 0.26*** | 0.25*** | 0.20*** | 0.18*** | 0.13* | 0.10 | 0.13* | 0.08 |
| | (0.06) | (0.06) | (0.06) | (0.06) | (0.07) | (0.08) | (0.08) | (0.08) |
| N | 2815 | 2749 | 2749 | 2749 | 2749 | 2749 | 2726 | 2749 |
| First Stage F | 232.75 | 323.9 | 281.3 | 259.2 | 185.2 | 201.7 | 216.8 | 203.4 |
| Sample With Xs | N | Y | Y | Y | Y | Y | Y | Y |
| Industry FE | N | N | Y | Y | Y | Y | Y | Y |
| State FE | N | N | Y | Y | Y | Y | Y | Y |
| Bus Status FE | N | N | N | Y | Y | Y | N | Y |
| Cash FE | N | N | N | N | Y | Y | N | Y |
| Zipcode Controls | N | N | N | N | N | Y | N | Y |
| Reweighted | N | N | N | N | N | N | Y | N |
| Bank Type FE | N | N | N | N | N | N | N | Y |

**Table A7: Estimates of PPP Effects on Employment Levels and Percent Changes**

This table reports OLS and IV estimates relating whether a firm was approved for the PPP program as of April 25, 2022 to employment outcomes. The sample is restricted to firms that applied for PPP, including firms that were ultimately denied and firms that tried to apply but were unable to submit an application. In Panels A and B the dependent variable is the level of April 25, 2020 employment and we control for employment in January of 2020. In Panels C and D, the dependent variable is the inverse hyperbolic sine transformation of April 25 employment, and we control for the inverse hyperbolic sine of January employment. Column 1 includes the full sample without conditioning on covariate availabilit (except for banking relationship information to compute the instrument in Panels B and D). Column 2 restricts the sample to have available covariates X. Column 3 controls for industry and state, Column 4 for whether the business was open at the time of the survey, Column 5 for its remaining cash on hand. Column 6 controls for average proximity of employees to one another based on the zipcode's industry distribution in the County Business Patterns data, Covid cases per-capita, and UI claims from Opportunity Insights. Column 7 reweights the sample to match the industry and size composition of the population of firms receiving PPP in SBA data. There are 35 firms that have zero weights because of sparsity of some reweighting cells. Column 8 adds bank type fixed effects (large banks, small banks, credit unions). The instrument for PPP approval in Panels B and D is the tranche 1 share of loans approved relative to all loans within the first 3 weeks of tranche 2, calculated from the SBA administrative data. The first stage F statistics is reported below these panels.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Panel A: OLS Estimates of Employment Level Effects | | | | | | | | |
| PPP approved | 2.25** | 2.23** | 2.05** | 1.75* | 1.00 | 0.95 | 2.40** | 0.92 |
| | (1.01) | (0.83) | (0.74) | (0.74) | (0.52) | (0.53) | (0.89) | (0.56) |
| R2 | 0.557 | 0.53 | 0.55 | 0.55 | 0.57 | 0.57 | 0.59 | 0.57 |
| N | 6098 | 4452 | 4452 | 4452 | 4452 | 4452 | 4414 | 4452 |
| Panel B: IV Estimates of Employment Level Effects | | | | | | | | |
| PPP approved | 4.61*** | 6.45*** | 6.42*** | 5.92*** | 5.70*** | 5.25*** | 8.21*** | 5.07*** |
| | (1.07) | (1.72) | (1.92) | (1.89) | (1.88) | (1.84) | (2.94) | (1.94) |
| N | 5575 | 4452 | 4452 | 4452 | 4452 | 4452 | 4414 | 4452 |
| First Stage F | 293.2 | 211.5 | 172.2 | 166.3 | 157.6 | 165.7 | 137.7 | 160.0 |
| Panel C: OLS Estimates of Employment Pct Changes (Inv. Hyperbolic Sine) | | | | | | | | |
| PPP approved | 0.31*** | 0.31*** | 0.30*** | 0.26*** | 0.20*** | 0.20*** | 0.30*** | 0.20*** |
| | (0.03) | (0.04) | (0.03) | (0.03) | (0.03) | (0.03) | (0.04) | (0.03) |
| R2 | 0.499 | 0.50 | 0.52 | 0.55 | 0.57 | 0.57 | 0.53 | 0.57 |
| N | 6098 | 4452 | 4452 | 4452 | 4452 | 4452 | 4414 | 4452 |
| Panel D: IV Estimates of Employment Pct Changes (Inv. Hyperbolic Sine) | | | | | | | | |
| PPP approved | 0.47*** | 0.50*** | 0.52*** | 0.43*** | 0.40** | 0.36** | 0.64*** | 0.34* |
| | (0.074) | (0.13) | (0.17) | (0.16) | (0.17) | (0.17) | (0.23) | (0.18) |
| N | 5575 | 4452 | 4452 | 4452 | 4452 | 4452 | 4414 | 4452 |
| First Stage F | 312.7 | 210.18 | 161.92 | 154.22 | 152.35 | 160.81 | 127.33 | 154.75 |
| Sample With Xs | N | Y | Y | Y | Y | Y | Y | Y |
| Industry FE | N | N | Y | Y | Y | Y | Y | Y |
| State FE | N | N | Y | Y | Y | Y | Y | Y |
| Bus Status FE | N | N | N | Y | Y | Y | N | Y |
| Cash FE | N | N | N | N | Y | Y | N | Y |
| Zipcode Controls | N | N | N | N | N | Y | N | Y |
| Reweighted | N | N | N | N | N | N | Y | N |
| Bank Type FE | N | N | N | N | N | N | N | Y |

**Table A8: Joint Estimates of Heterogeneous Treatment Effects**

This table reports OLS and LIML estimates of the joint treatment effects from Table 5. Odd columns are OLS, even are LIML. LIML instruments interact the bank approval ratio with the indicator for each characteristic. Controls are the same as those in Table 4. Standard errors clustered by bank.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Survival Expectations | | July 2020 Operational Status | | Employment Levels | | Change in IHS(Employment) | |
| PPP Approved | 0.09*** | 0.24* | 0.13** | -0.11 | 0.52 | 2.32 | 0.08 | 0.38 |
| | (0.02) | (0.13) | (0.06) | (0.27) | (1.53) | (4.39) | (0.07) | (0.60) |
| Approved x Frontline Industry | 0.01 | 0.04 | -0.03 | 0.04 | 0.14 | -2.66 | 0.06 | -0.18 |
| | (0.01) | (0.06) | (0.04) | (0.14) | (1.27) | (2.28) | (0.05) | (0.16) |
| Approved x High Cash | -0.03 | -0.09 | 0.00 | 0.13 | -0.34 | 0.26 | -0.05 | -0.20 |
| | (0.02) | (0.07) | (0.03) | (0.14) | (1.18) | (3.49) | (0.07) | (0.34) |
| Approved x Bank Loan | -0.01 | -0.09 | -0.00 | 0.08 | -1.13 | 1.20 | 0.02 | 0.06 |
| | (0.02) | (0.06) | (0.03) | (0.15) | (1.30) | (2.52) | (0.05) | (0.25) |
| Approved x Bank Officer | 0.01 | -0.03 | -0.02 | -0.18 | 1.00 | -1.35 | 0.22*** | 0.39** |
| | (0.02) | (0.05) | (0.04) | (0.14) | (1.50) | (3.57) | (0.07) | (0.18) |
| Approved x High Payroll | 0.03 | 0.02 | -0.03 | 0.02 | 0.23 | 5.90*** | 0.06 | 0.03 |
| | (0.02) | (0.07) | (0.05) | (0.13) | (1.03) | (2.17) | (0.06) | (0.25) |
| Approved x High Fixed Expenses | -0.00 | -0.06 | -0.03 | 0.04 | 2.25* | 5.17 | 0.04 | 0.05 |
| | (0.02) | (0.05) | (0.05) | (0.15) | (1.24) | (3.52) | (0.07) | (0.27) |
| Approved x B2B Industry | 0.00 | 0.09 | -0.04 | 0.03 | -0.39 | -5.73*** | 0.00 | -0.13 |
| | (0.02) | (0.07) | (0.04) | (0.12) | (1.27) | (2.19) | (0.06) | (0.19) |
| Approved x High Proximity Zipcode | -0.02 | -0.06 | -0.04 | 0.09 | 0.43 | -1.20 | 0.02 | 0.07 |
| | (0.01) | (0.04) | (0.03) | (0.08) | (1.04) | (1.85) | (0.05) | (0.13) |
| Approved x High Cases / Capita | -0.01 | 0.01 | 0.05* | 0.09 | -0.82 | 1.86 | -0.06 | 0.07 |
| | (0.02) | (0.04) | (0.03) | (0.07) | (0.93) | (1.28) | (0.05) | (0.10) |
| R2 | 0.20 | 0.19 | 0.12 | 0.11 | 0.56 | 0.53 | 0.57 | 0.56 |
| N | 4234 | 4234 | 2638 | 2638 | 4234 | 4234 | 4234 | 4234 |
| Approved Treatment Effects (TEs) | 0.092 | 0.131 | 0.049 | 0.039 | 1.590 | 5.34 | 0.249 | 0.370 |
| All Applicant TEs | 0.091 | 0.165 | 0.062 | 0.022 | 1.194 | 3.39 | 0.208 | 0.339 |
| Non-Applicant TEs | 0.078 | 0.168 | 0.080 | 0.041 | 0.486 | 0.50 | 0.142 | 0.247 |

**Table A9. Alternative Approaches to Estimating Treatment Effect Heterogeneity**

This table reports results on jobs saved per $100,000 in program cost in the long run (based on survival in Panel A) and the short run (based on employment changes) under different allocation rules using a variety of approaches to estimating heterogeneous treatment effects. As in Table 5, to estimate jobs saved, we mulitiply the raw treatment effects by January 2020 employment divided by actual program costs for recipients and imputed program costs (based on payroll) for non-recipients. The first sub-panel in each Panel shows the mean treatment effect for recipients and standard deviation of treatment effects for all firms. The second sub-panel in each panel reports the difference in average jobs saved per $100,000 in program costs under five alternative procedures for allocating loans (keeping the total share of firms receiving loans constant). The alternative loan allocation procedures considered are: randomly allocating loans, targeting firms in industries with a high share of fronline workers, targeting firms with less than 50 employees, and allocating loans to the firms with the highest estimated treatment effects. Different columns report results using different approaches to estimating heterogenous treatment effects. Columns (1) through (3) then show results using S-learners with alternative base-learners, with Column (1) using OLS, (2) using LASSO with the MSE minimizing cross-validated penalty used and Column (3) using a random forest model. Columns (4)-(13) then report results using three alternative meta-learners: T-learners, X-learners, and R-learners (these are described in more detail in the text). For each of these, we report results using both LASSO (with a cross-validated MSE minimizing penalty) and a Random Forest model. Column (10) reports results using the Causal Forest approach from Athey and Imbens (2016). All random forest models use 100 total trees. All models use as covariates months of cash available, monthly fixed expenditures pre-COVID, pre-COVID employment (categorical dummies for terciles), an indicator for a bank loan, an indicator for a loan officer relationship, , the share of the local population receiving UI, the local COVID case rate, and 2-digit industry dummies. In columns (1) and (2) state fixed effects are partialed out (i.e. not interacted with treatment, while in columns (3)-(13) they're including symetrically with other covariates. In the random forest models, instead of industry and state-dummies we include target encoded variables (i.e. variables for the mean outcomes by industry and state). Standard errors are shown in parentheses and are constructed using 150 bootstrap samples.

| | | S-Learner | | | T-learner | | | X-learner | | | R-learner | | Causal Forest |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Base Leaner | OLS | Lasso | RF | OLS | Lasso | RF | OLS | Lasso | RF | OLS | Lasso | RF | - |
| Lasso Penalty Details | | CV-min | - | | CV-min | - | | CV-min | - | | CV-min | - | - |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) |
| **Panel A. Jobs saved per $100k in spending using survival expecations** | | | | | | | | | | | | | |
| *Panel A1. Treatment effects under observed allocation* | | | | | | | | | | | | | |
| Treatment effects for recipients | 1.496 | 1.542 | 1.353 | 1.509 | 1.519 | 1.554 | 1.509 | 1.572 | 1.454 | 1.481 | 1.188 | 1.088 | 1.514 |
| | (0.195) | (0.16) | (0.158) | (0.212) | (0.217) | (0.177) | (0.212) | (0.199) | (0.147) | (0.228) | (0.174) | (0.235) | (0.128) |
| SD of treatment effects (full sample) | 2.014 | 1.697 | 2.677 | 2.166 | 2.175 | 3.363 | 2.166 | 2.063 | 2.576 | 2.344 | 1.318 | 1.725 | 1.574 |
| | (0.379) | (0.294) | (0.415) | (0.474) | (0.473) | (0.479) | (0.474) | (0.412) | (0.365) | (0.414) | (0.404) | (0.371) | (0.191) |
| *Panel A2. Treatment effects under alternative loan allocations* | | | | | | | | | | | | | |
| Random allocation among applicants | 0.105 | 0.179 | 0.142 | 0.132 | 0.125 | 0.183 | 0.132 | 0.138 | 0.188 | 0.182 | 0.164 | -0.07 | 0.068 |
| | (0.115) | (0.088) | (0.115) | (0.147) | (0.149) | (0.149) | (0.147) | (0.138) | (0.122) | (0.159) | (0.133) | (0.159) | (0.075) |
| Frontline Industry Prioritization | 0.11 | 0.194 | 0.236 | 0.158 | 0.138 | 0.303 | 0.158 | 0.19 | 0.344 | 0.342 | 0.154 | -0.117 | -0.001 |
| | (0.213) | (0.179) | (0.149) | (0.226) | (0.234) | (0.206) | (0.226) | (0.206) | (0.158) | (0.261) | (0.214) | (0.175) | (0.104) |
| Small Firm Prioritization | 0.382 | 0.42 | 0.349 | 0.417 | 0.408 | 0.421 | 0.417 | 0.403 | 0.397 | 0.443 | 0.334 | 0.058 | 0.137 |
| | (0.129) | (0.102) | (0.112) | (0.15) | (0.154) | (0.142) | (0.15) | (0.138) | (0.112) | (0.172) | (0.136) | (0.177) | (0.069) |
| Best Possible Allocation | -2.509 | -2.113 | -3.743 | -2.689 | -2.686 | -4.603 | -2.689 | -2.59 | -3.044 | -2.686 | -1.66 | -2.563 | -1.894 |
| | (0.732) | (0.578) | (0.857) | (0.865) | (0.893) | (1.157) | (0.865) | (0.81) | (0.704) | (0.801) | (0.753) | (0.782) | (0.415) |
| **Panel B. Short-run employment effects per $100k in spending using employment percent change** | | | | | | | | | | | | | |
| *Panel B1. Treatment effects under observed allocation* | | | | | | | | | | | | | |
| Treatment effects for recipients | 5.114 | 5.026 | 3.748 | 5.134 | 5.26 | 5.635 | 5.134 | 5.168 | 5.377 | 4.895 | 4.226 | 1.838 | 4.876 |
| | (1.01) | (0.937) | (0.642) | (1.028) | (1.058) | (1.007) | (1.028) | (0.988) | (0.833) | (1.035) | (0.833) | (2.263) | (0.678) |
| SD of treatment effects (full sample) | 8.947 | 7.757 | 10.211 | 9.002 | 8.775 | 18.558 | 9.002 | 8.486 | 14.056 | 9.435 | 6.898 | 2.986 | 7.742 |
| | (2.194) | (2.296) | (2.386) | (2.177) | (2.197) | (3.94) | (2.177) | (2.087) | (3.087) | (2.334) | (2.396) | (1.704) | (1.985) |
| *Panel B2. Treatment effects under alternative loan allocations* | | | | | | | | | | | | | |
| Random allocation among applicants | 1.618 | 1.64 | 0.935 | 1.648 | 1.588 | 1.442 | 1.648 | 1.663 | 1.264 | 1.064 | 0.929 | -0.502 | 1.022 |
| | (0.388) | (0.31) | (0.379) | (0.493) | (0.526) | (0.81) | (0.493) | (0.496) | (0.587) | (0.467) | (0.336) | (0.958) | (0.395) |
| Frontline Industry Prioritization | 1.83 | 1.604 | 0.86 | 1.849 | 1.73 | 1.191 | 1.849 | 1.797 | 1.246 | 1.034 | 0.846 | -0.665 | 0.566 |
| | (0.608) | (0.453) | (0.448) | (0.673) | (0.75) | (0.974) | (0.673) | (0.676) | (0.631) | (0.547) | (0.389) | (1.064) | (0.419) |
| Small Firm Prioritization | 2.689 | 2.625 | 1.55 | 2.718 | 2.739 | 2.455 | 2.718 | 2.749 | 2.369 | 2.08 | 1.701 | -0.394 | 1.407 |
| | (0.553) | (0.452) | (0.386) | (0.606) | (0.634) | (0.809) | (0.606) | (0.582) | (0.668) | (0.588) | (0.426) | (1.541) | (0.453) |
| Best Possible Allocation | -8.762 | -6.924 | -10.277 | -9.118 | -8.879 | -17.95 | -9.118 | -8.248 | -12.018 | -10.657 | -6.728 | -4.737 | -5.981 |
| | (3.922) | (3.913) | (2.653) | (4.188) | (4.147) | (5.555) | (4.188) | (4.048) | (3.173) | (4.228) | (3.948) | (2.375) | (1.764) |

**Table A10: Firm Characteristics By Treatment Effect Heterogeneity**

This table reports characteristics of top treatment effect firms in the best allocation compared to other firms. The outcomes are the raw survival probabilities and jobs saved per dollar of spending. Each cell is an average of firm characteristics depending on whether the firm is classified as a top treatment effect firm or not.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Outcome | Probability of Survival | | | | Jobs Saved per Dollar of PPP Spending | | | |
| Estimator | GRF IV | | LASSO | | GRF IV | | LASSO | |
| Group | Other Firms | Top TE Firms | Other Firms | Top TE Firms | Other Firms | Top TE Firms | Other Firms | Top TE Firms |
| Payroll ($k) | 17.6 | 23.2 | 17.4 | 24.3 | 19.6 | 13.8 | 19.6 | 13.7 |
| Fixed Expenditures ($k) | 1.4 | 1.6 | 1.4 | 1.6 | 1.4 | 1.5 | 1.4 | 1.5 |
| Employees in January 2020 | 8.0 | 9.3 | 7.8 | 10.3 | 5.8 | 19.6 | 6.0 | 18.7 |
| Any Bank Loans | 0.4 | 0.4 | 0.39 | 0.48 | 0.4 | 0.4 | 0.40 | 0.41 |
| Bank Officer | 0.2 | 0.3 | 0.20 | 0.45 | 0.2 | 0.3 | 0.23 | 0.32 |
| Cash on Hand (in weeks) | 5.8 | 2.1 | 5.2 | 5.3 | 5.4 | 4.2 | 5.3 | 4.8 |
| Employees per Imputed 100k$ of PPP Loans | 16.3 | 14.7 | 16.3 | 14.6 | 9.5 | 46.8 | 9.6 | 46.6 |

**Table A11.** First Stage by Baseline Covariate Subsets

This table reports first stage specifications regressing an indicator for being approved for PPP on the instrument for the bank early PPP approval rate. Different rows nad columns report the first stage separately for different subsets of the sample based on baseline covariates. Each row shows a separate baseline covariate and then Column (1) reports first stage estiamtes for "high-values" of that covariate and Column (2) reports first stage estimates for "low-values" of that covariate. Standard errors are clustered at the bank level. In panel A, which reports estimates for baseline covariates that have more than two categorical values or are continuous, a "high-value" of the covariate is above the median and a ``low-value" of the coavarite is below the median. In Panel B, which reprots results by binary variables a "high-value" of the baseline covariate is the subsample where the baseline covariate is equal to 1, while a "low-value" is the subample where the baseline covariate is equal to 0. For each coavariate, we report the first stage estimate, the standard error in brackets, and the Montiel-Pfluger effective F-statistic. Standard errors are clustered at the bank level.

| Covariate | | Covariate Subset | |
|---|---|---|---|
| | | High Value | Low Value |
| | | (1) | (2) |
| **Panel A. Multi-valued Baseline Covariates** | | | |
| Share of Population Receiving UI | First Stage | 0.556 | 0.532 |
| | SE | [0.048] | [0.05] |
| | MP F-Stat | 136.3 | 112 |
| COVID Case share | First Stage | 0.502 | 0.583 |
| | SE | [0.047] | [0.047] |
| | MP F-Stat | 115.6 | 152.3 |
| Cash on Hand | First Stage | 0.737 | 0.478 |
| | SE | [0.055] | [0.045] |
| | MP F-Stat | 178.6 | 113.4 |
| Employment Level | First Stage | 0.746 | 0.423 |
| | SE | [0.046] | [0.048] |
| | MP F-Stat | 263 | 78.2 |
| Fixed Expenditures | First Stage | 0.662 | 0.462 |
| | SE | [0.054] | [0.051] |
| | MP F-Stat | 148.9 | 83.4 |
| Payroll | First Stage | 0.736 | 0.324 |
| | SE | [0.055] | [0.045] |
| | MP F-Stat | 180 | 52.8 |
| **Panel B. Binary Baseline Covariates** | | | |
| Any Bank Loans | First Stage | 0.678 | 0.446 |
| | SE | [0.065] | [0.034] |
| | MP F-Stat | 109.7 | 167.5 |
| Any Officer Relationship | First Stage | 0.717 | 0.42 |
| | SE | [0.068] | [0.037] |
| | MP F-Stat | 112 | 128.4 |
| Industry: Arts, Entertainment, Accomodation | First Stage | 0.334 | 0.56 |
| | SE | [0.053] | [0.047] |
| | MP F-Stat | 39.1 | 141.5 |
| Industry: Construction | First Stage | 0.737 | 0.523 |
| | SE | [0.099] | [0.04] |
| | MP F-Stat | 55.4 | 170.4 |
| Industry: Health | First Stage | 0.44 | 0.557 |
| | SE | [0.103] | [0.038] |
| | MP F-Stat | 18.3 | 212.9 |
| Industry: Other Services (non-public) | First Stage | 0.6 | 0.528 |
| | SE | [0.06] | [0.044] |
| | MP F-Stat | 101.1 | 143.2 |
| Industry: Professional, Technical, and Scientific | First Stage | 0.589 | 0.538 |
| | SE | [0.099] | [0.053] |
| | MP F-Stat | 35.2 | 104.5 |
| Industry: Retail Trade | First Stage | 0.375 | 0.568 |
| | SE | [0.068] | [0.044] |
| | MP F-Stat | 30.5 | 167 |