When should public programs be privately administered? Theory and evidence from the Paycheck Protection Program[1]

Alexander W. Bartik, Zoe Cullen, Edward L. Glaeser, Michael Luca, Christopher Stanton, and Adi Sunderam

Abstract

What happens when public resources are allocated by private companies whose objectives may be imperfectly aligned with policy goals? We study this question in the context of the Paycheck Protection Program (PPP), which relied on private banks to disburse aid to small businesses rapidly. In our model, delegation is attractive when delay is sufficiently costly, variation across firms in the impact of funds is small, and the alignment between government and private objectives is high. We use novel firm-level survey data that contains information on banking relationships to measure heterogeneity in the impact of PPP and to assess whether banks targeted loans to high-impact firms. Banks did target loans to their most valuable pre-existing customers. However, using an instrumental variables approach that exploits variation in banks' loan processing speeds, we find that treatment effect heterogeneity is sufficiently moderate, delay is sufficiently costly, and bank and government objectives are sufficiently aligned that delegation was likely superior from the government's perspective to delaying loans to improve targeting.

[1] This paper was previously circulated under the title "The Targeting and Impact of Paycheck Protection Program Loans to Small Business." We thank Karen Mills for connecting us to Alignable and Alignable's founders for providing data. Author contact information is: Alexander Bartik is at the University of Illinois: abartik@illinois.edu, Zoe Cullen is at Harvard Business School: zcullen@hbs.edu, Ed Glaeser is at the Harvard Department of Economics: eglaeser@harvard.edu, Michael Luca is at Harvard Business School: mluca@hbs.edu, Christopher Stanton is at Harvard Business School: cstanton@hbs.edu, Adi Sunderam is at Harvard Business School: asunderam@hbs.edu.

I. Introduction

When do the benefits of delegating public programs, like the Paycheck Protection Program (PPP), to private companies, such as banks, outweigh the costs of allowing these entities to skew those programs towards their own objectives? A vast literature now documents how PPP loans were allocated to banks' preferred borrowers (e.g., Balyuk et al, 2021, Chernenko and Scharfstein, 2021, Duchin et al., 2022, Granja et al., 2022, Humphries et al., 2020, and Joaquim and Netto, 2021). But could the Small Business Administration have speedily allocated trillions of dollars of loans? The key policy question is whether the advantage of delegation – the speedier disbursal of loans – outweighs the cost of banks' favoritism.

Governments have often collaborated with private entities in responding to emergencies. After the great Mississippi Flood of 1927, then-Commerce Secretary Herbert Hoover enlisted the armed forces and the American Red Cross (a nonprofit). Hoover would again turn to non-governmental actors in 1932 with the Reconstruction Finance Corporation, which funneled public dollars to banks that would then allocate funds to businesses. During the Great Recession, the government hired BlackRock to manage assets that were previously owned by AIG and Bear Sterns. In 2020, the development of COVID-19 vaccines involved the collaboration of researchers at the public National Institute for Allergy and Infectious Diseases and private companies like Moderna. The UK Government provides much of the financing for the British Red Cross, which uses those funds to ameliorate disasters and provide victims with financial assistance.[2]

Such cases are fraught with potential misalignment between government and private incentives. In early 2021, private hospitals were charged with administering vaccines, but some

[2] AIG: https://www.americanbanker.com/news/blackrock-to-get-71m-on-maiden-lane; UK Red Cross: https://register-of-charities.charitycommission.gov.uk/charity-search/-/charity-details/220949.

distributed shots to their own donors and executives. The Red Cross discriminated against minorities during disasters, such as the 1906 San Francisco earthquake and the Mississippi Flood of 1927. In 2005, Red Cross contract workers allegedly directed Hurricane Katrina-related relief to friends and family members. Italy turned to private contractors to rebuild after the massive 2009 L'Aquila earthquake, and "emergency procedures enabled [two local building firms] to engage in irregular subcontracting, false invoicing and fraud."[3]

PPP, which allocated nearly one trillion dollars in loans, may be the single-largest example of delegating public funds to private entities. To cope with COVID-19, PPP allowed small and medium-sized firms to take out low-interest, potentially forgivable loans. The loans were guaranteed by the Small Business Administration (SBA), which meant that banks had no direct risk from providing the loans. However, banks had considerable discretion about how to prioritize among eligible businesses. The program's design was meant to expedite fund delivery, but delegating control reduced the government's ability to target funds to businesses that would have the greatest benefit.

We model the delegation of public tasks to private entities which have pre-existing capacity and private incentives. Pre-existing capacity implies that private entities can work more quickly than the government, but speed comes at the cost of allocating control to an entity with its own goals, such as favoring existing clients. The divergence between private incentives and public welfare is central to most papers on private provision of public services (Hart, Shleifer, and Vishny, 1998; Engel, Fischer and Galetovic, 2014), but we differ from this literature by focusing on the speed enabled by the private sector's specialized human and physical capital.

---

[3] See, e.g., https://apnews.com/article/technology-washington-coronavirus-pandemic-russ-seattle-c453fc84e9378ba4259715d3e0ad50d9; https://reliefweb.int/report/united-states-america/usa-red-cross-contract-workers-indicted-stealing-katrina-aid; https://www.ncbi.nlm.nih.gov/pmc/articles/PMC8246578/#disa12431-bib-0042; https://scholarlycommons.law.wlu.edu/cgi/viewcontent.cgi?article=1341&context=crsj;

The model implies that three factors determine how attractive delegation is: the cost of delay, the value of targeting, and the alignment of incentives. In the context of PPP, the cost of delay is the number of firm failures or jobs lost as a result of firms' waiting on funds; the value of targeting is a function of the heterogeneity in the treatment effect of aid across firms; and the alignment of incentives is captured by the correlation between the preferences of the government and private entities. If private entities have similar preferences to the government, or if a dollar spent in one place is as good as a dollar spent elsewhere, then delegation has limited costs.

Our analysis provides evidence on these factors by investigating the allocation and impact of first-tranche PPP loans, which included funds for $349 billion of loans. Although the program later received more funding, we focus on the first tranche because this period of excess loan demand was when bank discretion was most relevant. We use original survey data gathered through the Alignable small business network between April 25 and April 27, 2020, which captures business employment and expectations of survival in a period right after loan funding in the first tranche had been exhausted but before significant loan volume had been processed in the second tranche. The survey data are particularly useful as they allow us to observe a number of firm characteristics for PPP applicants and recipients, including the firm's own expectations of survival, its cash on hand, employment, its primary lender, and the depth of its relationship with that lender.

We start with descriptive evidence on the relationship between firm characteristics and PPP applications and approvals. Firm characteristics are not strongly correlated with the propensity to apply for PPP. In contrast, there is a strong, positive relationship between the probability of being approved for PPP in the first tranche and firm size, age, and cash on hand. This is consistent with other evidence suggesting that loans were allocated to more sophisticated applicants, stronger firms, or banks' preferred clients (e.g., Balyuk et al, 2021, Chernenko and Scharfstein, 2021, Duchin et al., 2022, Granja et al., 2022, and Joaquim and Netto, 2021). The key question is how this preferential allocation affected overall program efficacy.

We then turn to the tradeoff between speed and bank preferences. We examine the relationship between firm outcomes like survival expectations and employment and bank speed and the degree to which banks appeared to steer loans to certain firms. We measure bank speed using administrative data from the SBA as the number of PPP loans made by the bank in the first tranche of the program divided by the cumulative number of loans made by the bank in both the first tranche and the first 21 days of the second tranche. This measure captures the fact that relatively faster banks were able to frontload more of their PPP loans, delivering more in the first tranche than in the early days of the second tranche. Applicants that had relationships with faster banks were thus more likely to receive funding, while applicants who banked with slower banks were more likely to experience a delay to the second tranche of the program. To get at banks' bias in the administrative data, we measure steering as the difference between jobs saved per loan, as reported by firms to the SBA, in the first tranche of the program and jobs saved per loan in the first 21 days of the second tranche. If application timing by firm size is similar across banks, then differences in relative firm size between earlier and later borrowers reveal potential steering to larger clients.[4]

Consistent with the idea that speed was crucial in the early stages of the Covid-19 pandemic, we find that processing speed is positively correlated with firms' self-reported survival

[4] In this particular analysis of bank steering that is based on measures from SBA administrative data, we cannot rule out all demand differences that might vary across banks. Even if our measure of steering is contaminated by demand differences (e.g., firm sophistication that differs across banks), it is still useful to understand how private program administration interacts with sophistication in influencing who receives aid. Additional exercises with firm-level survey data allow us to condition on applicants (which are unobserved in the SBA data), capturing bank-level demand by firms with different characteristics.

probabilities on April 25, 2020. Steering is not. In other words, having a primary lender that was relatively efficient is associated with higher firm expectations of survival, but the primary lenders' attempt to prioritize certain types of loans–in this case loans from larger borrowers–had little effect on program treatment effects. To corroborate our main results and shed light on realized closure rates, we conducted a phone survey in July 2020, where we called businesses in our survey data to see if they were open. The results are consistent with our main analysis on survival expectations, suggesting that PPP funding in the first tranche ultimately led to fewer small business closures, and in doing so, preserved jobs in the medium to long run. These results suggest that in the first tranche of PPP, speed dominated concerns about preferential allocations by banks.

We then examine the consequences of potential misalignment between banks and society in more detail. We estimate the causal effects of PPP on firm survival and employment, allowing treatment effects to vary with firm characteristics. To identify the causal effect of PPP, we use the bank speed variable as an instrument for PPP approval conditional on application. The exclusion restriction is that banks' speed affects firm survival and employment only through receipt of PPP. The natural threat to this assumption is that strong firms match with more efficient banks, but due to the nature of the shock, in this case both the largest banks and small credit unions were the least efficient application processors.[5]

We use the instrument to estimate treatment effect heterogeneity in two ways. We first examine dimensions of heterogeneity one at a time, simply instrumenting for the interaction of PPP approval and a firm characteristic with the interaction of the bank speed instrument and that

[5]While we find a negative correlation between bank speed and local Covid cases when we test for covariate balance with respect to the instruments, observable geographic and firm characteristics explain only a small share of the overall variation in bank speed. The correlation between local Covid cases and bank speed occurs through a bank size channel, where bigger banks had greater market shares in denser cities that had the earliest Covid outbreaks.

characteristic. We find little statistically significant evidence of heterogeneous treatment effects across industries, geographies, firm characteristics, or relationships with banks. To assess the economic significance of the point estimates, we use the fitted value of the regression to compute the average estimated treatment effect for three groups of firms: firms approved for PPP by banks, the full set of applicants, and all firms in our data. In effect, we are comparing the average estimated treatment effect for the allocation of PPP funds that banks actually chose with averages for a random allocation to either all applicants or all firms. The differences in average estimated treatment effects across these groups are small, suggesting that banks did not significantly reduce the efficacy of PPP through their allocation decisions.

We then use machine learning techniques to examine multiple dimensions of heterogeneity at the same time, obtaining similar results. Using these estimates, we evaluate the overall targeting quality of the PPP program. We concentrate on the long-run impact on jobs, assuming that the long-run impact is entirely driven by effects on firm survival, with no effects on always operating businesses or new businesses, consistent with evidence from Kurmann et al. (2021). In our preferred specifications, we find recipient firms averted about 1.5 job losses per $100,000 in program costs, while allocating funds at random to the average applicant firm would have averted 1.3 job losses per $100,000. On balance, banks' targeting appears to do better than a random allocation of the loans to applicants or to all firms; across specifications, treatment effects on long-run employment per $100,000 in lending were between 6 - 13% higher than they would have been under random loan allocation among PPP applicants. This targeting improvement was driven by banks being less likely to approve/process loans for the lowest treatment effect firms. Similar results hold for other allocation schemes that were ex ante feasible to implement. For instance, giving priority to firms in frontline industries, defined as industries where face-to-face interaction is necessary for many workersm or smaller firms would not have resulted in significantly higher average treatment effects (although our estimates for large firms, those with over 100 employees, come from very sparse samples).

We can also compare how banks performed relative to the best possible allocation, whereby applicants with the highest estimated treatment effects received priority for funding in the first tranche. Under this counterfactual scenario, our analysis suggests that treatment effects on long-run employment would have been about 110 percent larger in our preferred specification. Note, however, that such an allocation assumes full knowledge of the relationship between observables and treatment effects, which we only estimate with hindsight. While it is possible that the true treatment heterogeneity may have been larger than our estimates, it is unlikely that a government agency would have been better at targeting than our model, especially since we are basing our heterogeneity estimates on ex post outcomes that would not have been observable to the government ex ante. Thus, this likely reflects an upper bound on the value of targeting in practice.

We can also calculate, under perfect targeting, how much of a delay would have been worth the wait when compared to the realized allocation of private banks. Given that approximately three weeks passed between the middle of PPP tranche 1 and the middle of tranche 2, and assuming costs are linear over that time window, the cost per week of delay in terms of long-run employment was roughly 0.36 jobs per $100,000 of loans delivered. We estimate that the gains to perfect targeting relative to the observed allocation are approximately 1.6 jobs per $100,000 of loans delivered. Combining our results, delaying the rollout of PPP to allow the loans to be perfectly targeted would only have been preferable if the delay were less than 4.4 weeks.

Our paper contributes to a growing literature studying the effects of the CARES Act. We follow the now-vast literature estimating the impact of PPP loans on employment and firm closure.[6] While much of this literature typically focuses on the average treatment effect of these loans, our work is particularly related to the papers that have documented treatment effect heterogeneity, especially between large and small firms (Autor et al., 2022b, Berger, 2021, Dalton, 2021, Doniger and Kay, 2022). Our paper is also closely related to research examining the impact of PPP delay.[7] In Appendix Table A1, we offer an in-depth summary of related research. While the overall impact of the program depends on the average treatment effect, the decision to delegate depends on heterogeneity in treatment effects, private bank incentives, and the cost of delay.

We differ from these papers because treatment effect heterogeneity is our primary empirical focus and because we use that heterogeneity to estimate the costs of delegation to private entities.[8] The main contribution of our paper is to shed theoretical and empirical light on an important decision faced by policymakers around the administration of public programs. Focusing on the distribution of PPP loans, we provide evidence on whether the program could have had a larger impact, holding fixed overall program size – and whether this would have been worth doing even if it meant delaying the administration of funds. It is important to note that our analysis does not evaluate the opportunity cost of the program, the overall benefits relative to other uses of funds,

[6] Approaches include using PPP eligibility thresholds (Chetty et al. 2020, Autor et al. 2022a, and Hubbard and Strain 2020), to geographic variation in recipients (Granja et al. 2022, Faulkender, Jackman, and Miran 2020, Bartik et al. 2020c, Doniger and Kay 2021, Kurmann et al. 2021), to comparing similar firms that differed in PPP receipt or receipt timing (Humphries at al. 2020, Granja et al. 2022, Joaquim and Netto 2021, Elenev et al. 2020, Barrios et al. 2020, Cororaton and Rosen 2020, Denes at al. 2021).

[7] Exploiting the delay in loan approval of PPP due to exhaustion of funds, Doniger and Kay (2021) and Kurmann et al. (2021) find that PPP had meaningful effects on firm survival and employment. Denes et al. (2021) show that delay weakens firms through a credit-supply channel and reduced transaction volume. Staples and Krumel (2022) focus on craft brewers and find that earlier loans resulted in better short-run performance.

[8] Granja et al (2022) also investigate the targeting effects of delegating loan allocation to banks. However, as we discuss in more detail in Section VII, Granja, et al (2022) take a fundamentally different (and complementary) approach to assessing the targeting effectiveness of the delegation of PPP lending: measuring how the zip-code level bank PPP approval rate is correlated with geographic measures of exposure to the economic effects of COVID-19. This contrasts with our approach of directly estimating firm-level treatment effects.

or the merits of alternative designs of the PPP program, including delegation with further restrictions on private banks. We instead focus on the delivery of the program given a dedicated amount of funding and the rules surrounding loan sizes and forgiveness. The key takeaway of our analysis is as follows: while banks did favor their own preferred clients, the estimated gains from improved targeting were likely not large enough to warrant significant delays in the disbursement of funds to remove banks as the allocation channel, holding fixed other program characteristics.

II. The Paycheck Protection Program and the CARES Act

PPP was established as part of the CARES Act in late March 2020. The bill authorized $349 billion of forgivable loans, which constituted the first tranche of the program. The program was restricted to firms with less than 500 employees and firms could borrow up to 2.5 times monthly payroll, with a maximum loan size of $10 million.[9] Any recipient of a PPP loan had to make a "good faith certification" acknowledging "that funds will be used to retain workers and maintain payroll or make mortgage payments, lease payments, and utility payments." The share of the loan that was spent on payroll, mortgage, rent and utilities could be forgiven if the firm did not reduce its number of workers.

Figure 1 displays the timing of loan approvals and fund deployment through July of 2020, as recorded by the SBA. The first round of the program ran out on April 16, and lending approvals stopped until April 27. This excess demand for the first round of PPP initially gave banks a significant role in allocating funds. After April 27, Congress added $300 billion more to the program. This extra sum proved to be sufficient to meet demand given the program parameters, as

[9] The size limit of PPP to firms with under 500 employees was relaxed in some industries, most notably for industries in NAICS code 72, which includes restaurants, leisure, and hospitality, where firms were eligible for PPP as long as they had fewer than 500 employees at each location. See https://www.sba.gov/funding-programs/loans/coronavirus-relief-options/paycheck-protection-program

each business was only allowed a single PPP loan (through the first 2 program tranches) and loan amounts were capped.

Since evaluating the impact of a program that is open to all is difficult, we focus on the impact of receiving a PPP loan prior to April 27, which corresponds to the period where PPP lending was constrained by limited funds. We now turn to our model, which attempts to illustrate the conditions under which the PPP's structure, which includes delegating the allocation of funds to banks and setting a cap on loan amounts, may be optimal.

III. The Proper Scope of Government During a Crisis

We present a model where the government must decide how to deliver a service, such as a loan or a vaccine, to a population of at-risk recipients. We consider three options: (1) the government can delegate authority to a private actor (or actors) who can decide how much of the service to allocate to each recipient, (2) the government can "regulate", fixing the amount of the service per recipient, but still having the private actor deliver the service, and (3) the government can set up a public bureaucracy to allocate the service, and decide how much to allocate to each recipient. We refer to these three options as delegation, regulation, and public provision.

As long as the private sector can scale up capacity more quickly than a public bureaucracy, delegation will have the advantage of speed, which is valuable because firms may permanently fail if they do not receive aid quickly. However, delegation has the disadvantage—from the perspective of the public sector— that the service will be allocated based on the preferences of the private actor, not the government. We examine this problem throughout from the perspective of the government's objective function (rather than the social planner's), and so delegating control always risks misallocation. Regulation can also be fast, but we will assume that the public can do no more than mandate a single service level for all recipients.[10] Public provision allows the government to better allocate service levels according to its own preferences, but there will be delay, which will mean that some of the recipients have died between crisis onset and when the government begins its operations.

We believe that these are plausible assumptions about government decision-making in a crisis. In particular, as we discuss in Section VIII, Economic Injury Disaster Loans (EIDL), another small business loan program that was directly administered by SBA, distributed funds at a slower pace than PPP. Thus, this tradeoff between government control of loan allocation and speed of loan delivery existed even between different small business assistance programs during the early stages of the Covid-19 pandemic.

More formally, we consider the optimization problem of a benevolent social planner that has a total fund of T units of aid to allocate across a unit measure of recipients. Receipt of x units of aid by recipient i, from either a bank or a public authority, will generate $v(x, \alpha_i)$ dollars of public benefit. This benefit can include extra employment, loan repayment, reduced bankruptcy probabilities, reduced illness or any other positive benefit that might flow from public largesse. The term α_i represents the recipient-specific benefit of receiving funding, where $v_x(x, \alpha) > 0$, $v_{x\alpha}(x, \alpha) > 0$ and $v_{xx}(x, \alpha) < 0$. We will assume that $v(x, \alpha) = e^{\alpha} x^{\gamma}$ and $\gamma < 1$. The model is agnostic with respect to the form of the aid (i.e., loan versus grant) as we model net benefits in reduced form.

If α_i were observable and the social planner directly controlled lending, then it would allocate the T funds to maximize: $\int_{\alpha} v(x, \alpha) f(\alpha) d\alpha$ subject to the constraint $T = \int_{\alpha} x f(\alpha) d\alpha$.

[10] In reality, regulation is a continuum: the government can impose additional mandates on the private sector, but this will come at the cost of some delay. Since the model is only illustrative, we do not model this full continuum.

If second order conditions hold, then socially optimal lending implies that for all values of α that receive loans, $v_x(x,\alpha) = \lambda$, where λ is a constant, so that the marginal social value of lending is equalized across borrowers. If the planner chooses public provision, then a random fraction $(1 - \delta)$ of recipients will die before the aid is delivered. These deaths are the only costs of delay.[11]

Relative to the planner, if the private entity distributes the funds immediately, it will allocate aid to maximize $v(x, \phi\alpha + \xi) = e^{\phi\alpha+\xi}x^{\gamma}$ instead of $v(x,\alpha) = e^{\alpha}x^{\gamma}$. The variables α and ξ are normally distributed, mean zero independent random variables with variances σ_α^2 and σ_ξ^2, respectively. ϕ and ξ capture the mismatch between public and private objectives.

If the government delays and designs a more targeted allocation mechanism, then public bureaucracy will allocate funds to maximize $v(x, \theta\alpha + \zeta) = e^{\theta\alpha+\zeta}x^{\gamma}$, where ζ is a third independent mean zero normal random variable with variance σ_ζ^2.[12] We assume that delayed targeting increases the correlation of the decision-making with true social value, either because the public sector can set up its own bureaucracy to better target loans or because it can design better rules to improve targeting by the private sector.

The third possibility is regulation of private delivery so that the government will fix loan sizes at T, which means that all recipients get the same service level. In the appendix, we consider a fixed service level of T'>T, which means that there is private provision and shortages. Proposition 1 follows (all proofs are in Appendix A):

[11] Although standard intuition would suggest that the first businesses to die are likely the most fragile and hence would have been close to the exit margin absent the pandemic, Bartlett and Morse (2020) indicate that the death process may have looked very different during COVID-19 because extreme demand reductions fall hardest on businesses with high levels of committed capital. Hanson et al (2020) similarly point to a potentially low correlation between firm revenues during the pandemic and long-run viability as an economic rationale for aid to firms.

[12] We assume that $1 \geq \theta > \phi$, and $\sigma_\zeta^2 < Min[\sigma_\xi^2, \sigma_\alpha^2]$. If $1 = \theta$ and $\sigma_\zeta^2 = 0$, then the delay allows for perfect allocation. We assume that the variance relationship is strict, so that there is some benefit of delay, even if it is small.

Proposition 1. *There exists a firm survival rate, denoted δ^* between 0 and 1, such that delayed public administration creates higher levels of welfare than either alternative if and only $\delta > \delta^*$.*

(i) If $\phi < 1 - \sqrt{1 - \frac{\sigma_\xi^2}{\sigma_\alpha^2}}$, and $\delta < \delta^*$, then fixed size loans generate higher welfare than the two alternatives, and δ^* is falling with $\gamma, \theta,$ and σ_α^2 , rising with σ_ζ^2 and independent of σ_ξ^2 and ϕ.

(ii) If $\phi > 1 - \sqrt{1 - \frac{\sigma_\xi^2}{\sigma_\alpha^2}}$, and $\delta < \delta^*$, then private flexible allocation generates higher welfare than the two alternatives, and δ^* is falling with $\gamma, \theta,$ σ_ξ^2 and σ_α^2 , and rising with ϕ and σ_ζ^2.

The proposition provides the basic intuition that might justify the speedy action taken by the CARES Act and the Paycheck Protection Program. There is a minimum survival rate δ^* that determines whether delayed targeting is preferable. That survival rate depends on ϕ, the alignment between public goals and private incentives. If ϕ is high, $\phi > 1 - \sqrt{1 - \frac{\sigma_\xi^2}{\sigma_\alpha^2}}$, the incentives of the private entity are well-aligned with public goals, and delegation provides higher social welfare than regulation or public provision. If ϕ is low, then fixing the aid level through regulation will generate higher welfare levels.

The minimum survival rate required for public provision also depends on whether delayed targeting aligns decision-making more closely with social welfare (θ is high and σ_ζ^2 is low). In other words, one can support delegation either because of faith in banks' decision-making or skepticism about the public sector's ability to improve on that decision-making.

The survival threshold that determines whether delegating improves welfare is everywhere falling with heterogeneity in treatment effects (σ_α^2) and returns to scale in aid (γ). When the heterogeneity in the social welfare of helping different entities is high, i.e. σ_α^2 is larger, then it is more valuable to delay funding to improve targeting. A higher value of γ means that the diminishing returns involved in providing aid to any one recipient become weaker, and there may be almost as many benefits to helping a smaller number of surviving recipients as there are to aiding a larger number of initial recipients.

We now take the logic of this model to our data. We do not mean to conclude that delegation was obviously right or wrong thing. Given any set of empirical results, it would be possible to conclude that delegation was right (if you believe that the preferences of private sector actors were superior to those of the public sector) or wrong (if you believe that firms deaths provided beneficial creative destruction). Yet we believe that the following empirical work, which is guided by the our framework with the government's objective of saving jobs, is relevant for judging the delegation of PPP.

Abundant evidence suggests that the firm closing rate during this period was high (e.g., Bartik et al., 2020a), and a number of papers provide evidence on the benefits of receiving PPP loans earlier rather than later (Denes et al., 2021, Doniger and Kay, 2022, Kurmann et al., 2022, Staples and Krumel, 2022). The empirical question that we address next is whether public allocation of PPP could have led to better social outcomes.

IV. Data Description

We evaluate the efficacy of the PPP program and the nature of bank delegation using unique survey data on small businesses. The survey was conducted by Alignable (www.alignable.com), the largest network of small businesses across North America, with nearly 5 million members. Alignable regularly sends out polls to network members, which have been used previously to assess pandemic effects on the financial health of small businesses, remote work, and business reopening decisions (Bartik et al. 2020a, Bartik et al. 2020b, Balla-Elliott et al. 2020).[13] The

[13] Alignable begins with a survey distribution list, which consists of at least one million businesses. We do not know the characteristics of the businesses in the distribution list, since the survey was conducted by Alignable. From

primary survey wave underpinning our analysis of PPP was distributed on April 25, 2020, nine days after the final approvals for the first tranche of PPP funding but prior to the second tranche of funds being approved for loans by the SBA. Ninety percent of the survey responses were received prior to April 27, 2020, the first day of the second tranche of the PPP program. The survey received 16,826 responses from U.S.-based firms. We restrict the sample to observations for which our main variables of interest—expected survival probability, whether the firm applied for PPP, and information on the firm's banking relationship and characteristics—are not missing. This leaves us with 6,640 observations. Appendix Table A2 provides details about our sample selection relative to all respondents, and Appendix Figure A1 shows that there are no systematic biases based on geography between those who enter the survey and have missing data and the final estimation sample.

The key advantage of using survey data is that we observe detailed information about each respondent, which is crucial for studying heterogeneity in treatment effects. Specifically, we observe the business's primary bank, its relationship with that bank, cash on hand, employment, and beliefs about the likelihood the firm would remain operational over the following 8 months. The survey also included a series of retrospective questions covering employment, typical monthly payroll, fixed expenditures, and typical loan balances with their bank prior to the pandemic. Finally, we observe information about the business's application status for PPP funding, including whether funds had been approved, denied, or a decision was in process. For approved applicants, owners also reported the size of the loan. We include the main survey instrument in Appendix C.

---

conversations with Alignable, we know that non-response comes from both not opening the email and then, conditional on opening the email, from not proceeding to the survey–though we do not know response rates. Given these limitations, validation of the data against other benchmarks, and reweighting when relevant, are important steps in guiding and interpreting the analysis.

To cross-validate our main results, we hired five research assistants to conduct phone calls to businesses and inquire about their operating status. Alignable provided us with a list of 3,086 business phone numbers. After eliminating invalid numbers, the research assistants called 3,040 businesses between July 21 and July 28 of 2020 during typical business hours (between 9 am and 5 pm).[14] If the business answered the first phone call, the research assistant asked if they were currently open for business—research assistants were instructed to clarify that we were interested in their operating status, as opposed to whether they were open at that exact time. If a business did not answer the first call, research assistants were instructed to call again the next day, at a different time of day to increase the chances of reaching the business during operating hours. Out of the businesses called, 2,133 answered the phone on either the first or second call. In our main analysis of these results, we count a business as open if it answered the phone and said that they were open.[15]

We supplement this novel data with administrative loan-level data from the SBA. This data is comprehensive, covering all PPP loans made. However, it is relatively sparse, where the only reliably populated fields contain information on firm location, loan amount, lender, date of application and approval, industry code, and jobs saved as self-reported by firms.

Sample Validation

Our sample is a convenience sample of respondents who voluntarily took the Alignable survey.[16] This same approach was used to accurately predict aggregate PPP loan demand (Bartik et al. 2020a). To understand sample representativeness, we undertake two exercises. First, we ask

[14] 2,749 of the 3,040 businesses called have full covariate availability.
[15] In later analyses where we use these data, we find qualitatively similar results if we limit the sample to only look at businesses that answered the phone, and use an indicator for whether they stated they were open as a dependent variable.
[16] We were not given the number of emails sent to Alignable members soliciting survey responses, the response rates, or how the characteristics of respondents compare to non-respondents.

whether the loans that we observe in the survey appear similar to those in the SBA data for recipients of Tranche 1 loans. Appendix Figure A2 shows that our borrowers are slightly smaller than the typical SBA borrower, but we have reasonable coverage across most of the loan size distribution.[17] The exception is coverage of very large loans. Second, Appendix Figure A3 benchmarks our sample against firms in the 2017 Census regardless of loan receipt status. Our sample slightly over-represents firms with under 10 employees. While we over-weight California and under-weight New York, we otherwise fit the geographical distribution of firms well. To address potential sensitivities to our approach due to sampling variation, we later report re-weighting estimators that allow us to make our sample comparable to either the SBA loan distribution or the Census on observables. Finally, Appendix Figures A4 and A5 show that our data closely matches the Census Pulse survey in terms of PPP applicants, recipients, and industry distribution.

Summary Statistics

Table 1 provides summary statistics. The average reported probability of surviving to December 2020 was 73%. At the time of the survey, 44% of firms had closed (either temporarily or permanently), and the average firm had 5.5 employees, down from an average of 7.8 employees in January 2020. Firms reported \$24,100 in average monthly payroll expenses and \$14,600 in fixed monthly expenditures. While the median payroll of PPP recipients in our sample is similar to the median payroll in the SBA data (\$17,500 versus \$21,800), there is a bigger wedge at the 75th percentile (\$37,000 vs \$59,000), reflecting the fact that we under sample the largest eligible firms.

V. Which Businesses Received a PPP Loan?

[17] Eleven percent of our respondents and 9% of SBA borrowers have loan amounts under \$17,500; 64% of our respondents have loans that were under \$87,500 compared to 57% of SBA loans; 81% of survey loans are under \$575,000 compared to 74% of SBA loans.

Concerns about PPP targeting arose from correlations (or lack thereof) between key business characteristics and the receipt of a first-round PPP loan. Figure 2 shows that despite having higher rates of applications from low cash-on-hand firms, loans were most likely to go to firms that had the most cash available. The approval rate for firms with three or more months of cash on hand is 38%. The approval rate for firms with two weeks or less of cash on hand is less than 20%. This may have been socially optimal because the firms with little cash may have folded even if they received a loan. However, the higher approval rate for more cash-rich firms could also reflect banks' private incentives. Figure 2 also shows that approval rates increase with firm size and firm age. These patterns might reflect the greater capacity of large firms or older firms to apply for a loan quickly with the appropriate documentation. Alternatively, lenders might have been favoring older, larger firms because they had a more established relationship with the lender.

There are three main reasons why banks might have the incentive to steer loans to preferred clients. First, from our conversations with banks, there were operational challenges to disbursing this volume of loans in a short period of time. Prioritizing existing clients made this process easier. Second, providing PPP loans to clients with existing non-PPP loans might increase the chance that a customer pays their preexisting loans, as documented by Chodorow-Reich et. al. (2021). Third, favoring certain clients in the allocation of PPP could increase lenders' ability to sell those clients other products in the future.[18]

VI. Bank Steering versus Speed

[18] The incentives discussed here are largely indirect incentives that accrue to banks slowly over time. For instance, the benefits to a bank of cross selling accumulate over many years of a relationship with a given firm. Banks also faced direct incentives from the PPP program: lending fees paid by the SBA. The direct incentive effects of these fees are unclear for two reasons. First, smaller loans earned larger percentage fees. Second, the fees were meaningful in the aggregate but likely small for the average lender. According to the SBA data, the average lender participating in the first tranche earned $2.1 million in fees in that tranche. The largest PPP lender, JPMorgan, earned $299 million in fees in the first round. For comparison, JPMorgan's Consumer and Community Banking division had revenues of $52 billion and net income of $8 billion in 2020.

Figure 2 suggests that banks may have allocated the first tranche of PPP loans to favored clients. In this section, we analyze how these allocation choices affected the ultimate impact of the program. We start by analyzing the correlation between firm outcomes and bank speed and propensity to steer PPP loans. While these results do not reflect the treatment effects of the PPP, they offer a simple starting point for our analysis. We will later provide our own estimates of the causal impact of PPP loans on firm survival and employment as a function of firm size and other characteristics.

Table 2 presents these results. For each firm in our survey data, we measure the speed of the firm's primary lender, as well as how aggressively that lender steered loans in the early days of PPP. Speed is measured as the bank's tranche 1 share in the SBA data relative to loan applications that likely arrived in tranche 1 but were not processed until tranche 2. To capture this, we use the number of PPP loans made by the bank in the first tranche of the program, divided by the total loans made by the bank in the first tranche and the first 21 days of the second tranche. Relatively faster banks frontloaded more of their PPP loans, delivering more in the first tranche than in the first few weeks of the second tranche.

Bank steering is difficult to measure directly, as it may operate in different ways. In this section, we take a simple approach using the jobs reported saved variable in SBA data. The variable is self-reported by firms and reflects the size of the firm rather than the program treatment effect. Our steering measure is the difference in firm size (jobs saved per loan) between the first tranche of the program and the first 21 days of the second tranche. Banks that prioritized larger clients will score highly on this measure, although, as we note in the introduction, this measure may partially reflect demand differences across banks.

In Table 2 Panel A, we regress firm survival expectations as of April 25th, 2020 on our measures of bank speed and steering, both of which are computed directly from the SBA's administrative data. Standard errors are clustered by bank. In the first four columns, the sample is all firms, including those that did not apply for PPP. Including firms in our survey that did not apply for the program helps to rule out demand-based explanations for any impact of the steering measure, as we capture firms' expectations regardless of whether they applied for the program. The sample is slightly smaller than our full sample because we cannot compute the steering measure for banks that do not make PPP loans in both tranche 1 and the first 21 days of tranche 2.

Column 1 shows that bank speed is positively and significantly associated with survival expectations, while bank steering is not. A one-standard deviation increase in speed is associated with a 2 percentage point increase in survival expectations. A one-standard deviation increase in steering is associated with a 0 percentage point change in survival expectations, and we can reject that it is associated with a 1 percentage point change at the 5% significance level. Column 2 adds industry and state fixed effects, and column 3 adds additional controls for the firm's payroll and fixed expenses, cash on hand, the average proximity of workers to one another in the firm's zip code (proxying for contagion risk), and Opportunity Insights measures of Covid cases per-capita and UI claims since the beginning of 2020 in the 3-digit zipcode. Column 4 includes bank-type fixed effects, capturing the possibility that firms whose primary banks are credit unions or large national banks may differ from firms that have relationships with small, regional banks. We note, however, that bank-type fixed effects likely absorb "good" variation, as it was large national banks that tended to be swamped by applications. Across these specifications, bank speed remains positive and significant, while bank steering remains small and insignificant. Columns 5-7 show that we get somewhat stronger effects when we restrict the sample to firms that applied for PPP.

Column 8 re-weights the sample to match industry characteristics (employee proximity), Census regions, and the payroll amounts for the population of firms receiving PPP in SBA data. The similarity of these estimates suggests that our results are not driven by the fact that our survey sample overweights small firms. Appendix Table A3 shows that similar results obtain if we simply examine firms with banks that are above- versus below-median in speed and above- versus below-median in steering.

Panel B of Table 2 examines the probability the firm was open in July 2020, measured from our follow-up phone survey. We omit columns 2-4 because we only surveyed PPP applicants, so the phone survey sample conditions on PPP application. The results are similar. A one-standard deviation increase in speed is associated with a 1.6-4 percentage point increase in the probability of being open. Statistical significance varies across specifications, which is unsurprising given that our phone survey sample is less than half the size of our full sample. Bank steering is small and typically insignificant across specifications.

Panel C shows similar results when examining the level of employment on April 25, 2020. Across specifications, bank speed is positively and significantly associated with employment, while bank steering is generally not. A one-standard deviation increase in speed is associated with roughly 0.6 additional employees. Finally, Panel D studies changes in the inverse hyperbolic sine of employment (roughly percent changes) between January 2020 and April 25, 2020. Bank speed is positively and significantly associated with changes in employment in all specifications. When we restrict the sample to firms that applied for PPP in columns 5-8, the coefficient on bank speed remains similar, but bank steering now enters positively and significantly. Among the set of firms that applied for PPP, bank steering is associated with higher employment growth.

VII. Heterogenous Treatment Effects and the Allocation of PPP

The descriptive results in the previous section suggest that bank speed had a larger impact on firm outcomes than banks' steering of loans. We now turn to estimating the causal impact of PPP on firms' survival and employment as a function of firm characteristics. We use the bank speed variable as an instrument for PPP approval conditional on application.

The relevance of the instrument is clear—firms working with faster banks are naturally more likely to have their PPP applications approved before April 25, 2020. Table 3 provides formal evidence. In Panel A, we show that the instrument has a small and insignificant univariate correlation with a firm's propensity to apply for PPP. However, the instrument is strongly positively correlated with the likelihood the firm is approved for PPP, conditional on application, and the probability it had already received the funds on April 25, 2020. In contrast, the instrument is negatively correlated with the likelihood the firm's PPP application was rejected. Panel B shows that these conclusions remain when we add firm, zipcode, and bank characteristics to the regression. Many characteristics are potentially correlated with both application and approval—for instance, high payroll firms were both more likely to apply for PPP and were more likely to be approved conditional on application. However, the relationship between approval outcomes and the instrument remains strong, although the magnitude of the coefficients falls somewhat.

The exclusion restriction is that bank speed affects firm survival and employment only through receipt of PPP. The key threat to the exclusion restriction, matching between firms and banks, may affect the denominator of the speed variable—the total number of firms receiving PPP in tranche 1 or the first 21 days of tranche 2, but is less likely to affect the fraction of those firms that received PPP in tranche 1. For instance, the denominator of the speed variable may be correlated with regional differences in Covid impact—banks in areas hit particularly hard by Covid-19 may have given out more total PPP loans across tranche 1 and the early days of tranche

2. However, whether banks disbursed those loans in tranche 1 or early in tranche 2 is less likely to be correlated with firm characteristics. Appendix Table A4 provides covariate balance evidence consistent with this idea. We report regressions of the instrument on firm characteristics and zipcode characteristics, which explain about 3.5 percent of the variation in the instrument when we do not include state fixed effects. Where we do find that firm characteristics load on the bank speed instrument, it is for firms in frontline industries and those with more cash, but the effect sizes are small. Firm size, payroll, and expenses do little to load on the instrument. We do find that relationships with a bank, through a loan or a bank officer, matter, but these regressors become less important when we include bank type fixed effects (e.g. large banks, small banks, and credit unions).[19] However, our instrument is not perfect, as we find that firms were more likely to have relationships with faster banks in zipcodes that had lower numbers of Covid cases per capita. This relationship disappears when we include bank type fixed effects, as it comes from the fact that firms in denser, larger metro areas (that had higher rates of Covid) were more likely to use larger banks that were slow to process loans. Appendix Table A5 provides covariate balance evidence at the geographical level in the full SBA administrative data rather than the survey. For this exercise, we compute the average value of the instrument for all firms in a given zip code in tranche 1 of the SBA data and then regress that average value on zip code characteristics, including measures of the initial severity of the pandemic. Although we again find statistically significant results, the overall fitted values explain very little of the variation in the instrument.

While our focus is on treatment effect heterogeneity, Appendix Tables A6 and A7 present OLS and IV estimates of the average treatment effects on survival and employment outcomes.

[19] Although bank types explain a significant fraction of variance in bank speed, firm characteristics conditional on bank type become less important. In Tables A6 and A7, we estimate models with bank type fixed effects, and we find qualitatively similar point estimates to models without the bank type fixed effects.

OLS estimates of survival expectations treatment effects from April of 2020 range from 0.08 to 0.15, while IV estimates are larger in magnitude but generally include the OLS estimates in the confidence interval.[20] The OLS point estimates for July 2020 operational status range from 0.09 to 0.16, while the IV estimates are broadly similar when controls are included. OLS estimates of employment changes range from approximately 1 job saved to 2.4 jobs saved per loan, while IV estimates are larger. For additional details about these estimates, see the notes in the tables.

Heterogeneity along a Single Dimension

We examine heterogeneity in PPP treatment effects using the bank speed instrument in two ways. In Table 4 we analyze firm characteristics one at a time. Specifically, in Panel A, we regress firm survival expectations on a dummy indicating whether the firm had been approved for PPP, a given firm characteristic, and the interaction of the two, instrumenting with the bank speed variable and its interaction with the firm characteristic. Standard errors are clustered by bank. In column 1, the characteristic we examine is whether the firm is in a frontline industry. The estimated interaction between PPP approval and being in a frontline industry is positive, suggesting that the causal effect of receiving PPP is larger for these firms. However, the estimated interaction is insignificant.

To assess the economic significance of this heterogeneity, we report three average fitted values below the regression coefficients for: (i) firms that were approved for PPP as of April 25, 2020, (ii) firms that applied for PPP, and (iii) firms that did not apply for PPP. Essentially, we are asking whether, considering firm heterogeneity along the frontline industry dimension, the estimated effect of PPP is meaningfully different for these three groups of firms. The three fitted values are all quite close together: the difference between the average treatment effect for approved

[20] In both the OLS and IV models, our point estimates are similar when we do and do not control for bank type fixed effects, the source of the potential differences in the instrument across zip codes.

firms and applicants was .002, i.e., a 0.2 percentage point difference in expected survival probabilities. The sign of the difference is also notable—the average treatment effect for applicants is essentially the treatment effect that would have obtained had PPP approval been randomly assigned until tranche 1 funds were exhausted. The treatment effect for approved firms is larger than for applicants, indicating that banks' allocation decisions in the first round of PPP were (slightly) better in terms of raising expected survival probabilities than random assignment of loans to applicants. This cuts against the notion that banks channeled funds to the firms that needed them the least. The difference between the treatment effects for applicants and nonapplicants is also small and positive. This suggests that firms for which PPP was more helpful were slightly more likely to apply.

The remaining columns consider heterogeneity in terms of cash holdings, whether the firm already had an outstanding loan with its primary lender, whether the firm had a relationship with a loan officer at their bank, whether the firm was large in terms of payroll or fixed expenses, whether the firm was in a business-to-business industry (and thus more central to other firms), whether the average proximity of workers to one another in the firm's zip code was high, and whether the firm was in a zipcode with high Covid cases per capita. The only interaction term that is statistically significant at the 10 percent level is on firms with high proximity, and it is negative. Furthermore, the average fitted values again suggest that the heterogeneity in treatment effects along each dimension is economically small. The largest difference in fitted values is for high-cash versus low-cash firms (column 2). In this case, the average estimated treatment effect for applicants was 1.3 percentage points or about 10% higher than the average estimated treatment effect for approved firms. In other words, by funneling PPP funds to high cash firms, banks did slightly worse than random assignment.

The remaining panels of Table 4 take the same approach to understand treatment effect heterogeneity for other outcome variables. Panel B examines whether firms are open or closed in our July 2020 phone survey; Panel C studies the level of employment on April 25, 2020 controlling for January 2020 employment; and Panel D studies employment growth rates between January and April 25, 2020. The patterns are qualitatively similar across these panels. We seldom find statistically significant interactions between firm characteristics and PPP approval. In addition, the average fitted values for approved firms, applicants, and non-applicants tend to be quite similar. The totality of the evidence suggests that banks' allocation decisions for the first tranche resulted in a causal effect of PPP similar to or slightly better than what would have been achieved with random assignment.[21]

Heterogeneity along Multiple Dimensions

In Table 5, we use machine learning techniques to examine multiple dimensions of heterogeneity simultaneously. This approach allows us to predict treatment effects using all available covariates for each observation, shedding light on key model inputs in Section III, such as the standard deviation of treatment effects and the correlation of the bank objective function and government objective function. With this motivation, we explore gains and losses to alternative approaches to allocating PPP loans rather than the one chosen.

[21] One concern with this analysis is that banks may have excluded some firms completely, making it impossible to estimate heterogeneous treatment effects for these firms. Appendix Figure 7 estimates propensities to receive PPP as a function of firm characteristics It shows that the distributions of propensity scores for the treatment and the control group are almost entirely overlapping and that even among firms with low propensity scores many observations were treated. In a similar vein, the IV estimates will not allow us to precisely characterize the full extent of treatment effect heterogeneity if there is no first-stage (i.e. no compliers) for some subsets of the data. We explore this in Appendix Table A11, where we separately estimate the first stage by different subsets of the baseline covariates that we use in our treatment effect heterogeneity analysis. We find that the instrument has a strong first stage relationship with being approved for PPP in each of these sub-samples, with the Montiel-Pfluger F-statistic being at least 18 in each subsample, and above 50 in the vast majority of the sub-samples.

Panel A examines firm expectations of survival. Column (1) reports estimates using the generalized random forest (GRF) approach of Athey, Tibshirani, and Wagner (2019) which allows for heterogeneous treatment effects in our IV model. We will refer to this estimator as the "IV GRF" model. Appendix B provides details about this and other machine learning estimators that we use to assess treatment effect heterogeneity. Intuitively, the IV GRF approach uses regression trees to estimate unique weights on each observation for each point in the covariate space to use in the IV moment conditions, generating a unique treatment effect estimate for each combination of covariates.[22] Column (2) reports estimates from a Lasso procedure that uses raw PPP approval interacted with firm characteristics.[23] Column (3) reports estimates from an OLS model again using raw PPP approval interacted with firm characteristics. The approaches in Columns (2) and (3) do not use instruments and instead assume that PPP receipt is uncorrelated with treatment gains after conditioning on covariates. For all three models, 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, zipcode characteristics about COVID cases and UI, and two-digit industry dummies. We also partial out state dummies to account for unequal timing in the spread of Covid across places.

We summarize the results of these firm-specific treatment effect estimates by reporting treatment effect moments for recipients (i.e., the observed allocation), all applicants, and all firms. There is very little heterogeneity in treatment effects using the IV GRF model between recipients,

[22] We cross-validate the key GRF parameters, including the number of variables tried in each split and the minimum node size, amongst others and grow 10,000 trees. The IV GRF models are fit using the grf R package using the tune all parameters setting. Standard errors in the table come from 150 bootstrap replications of the predictions.

[23] 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 bank groups used for the instrumental variables analysis.

applicants, and all firms, with all three groups having mean survival expectation treatment effects between 12.7 and 13 percent.

This low estimated treatment effect heterogeneity may reflect the high sampling variance of instrumental variables estimates. We take three approaches to addressing this concern. First, Table 5 reports 5-95% bootstrapped confidence intervals for our estimates. The confidence intervals are generally tight, demonstrating that our data are inconsistent with large degrees of treatment effect heterogeneity, even taking estimation error into account.

Second, one may worry that the high sampling variance of IV estimates may make it difficult to detect treatment effect heterogeneity in the GRF IV models. The Lasso models do not have issues with first-stage variability, but these estimates require a conditional independence assumption to be interpreted causally that is stronger than the assumption required for our instrumental variables estimators. However, the estimates are better powered to detect heterogeneity than the IV GRF estimates in Column (1). Furthermore, the similarity of the OLS and IV estimates in Appendix Tables A6 and A7 gives us some confidence that the conditional independence assumption may hold in this case.

In Appendix Table A8, we take a more third, naïve approach to uncovering treatment effect heterogeneity, simply extending our IV analysis in Table 4 to multiple dimensions. The conclusions are very similar. Interactions with firm characteristics are typically statistically insignificant, individually and jointly. In addition, the average fitted values for approved firms, applicants, and non-applicants are similar, indicating that banks' allocation decisions had similar impact to random assignment.

For the Lasso results in Table 5 Column 2, the estimated values are quite close together, although not as close as the IV GRF estimates: the difference between the average treatment effect for approved firms and applicants was .004, i.e., a 0.4 percentage point difference in expected survival probabilities. This indicates that banks' allocation decisions in the first round of PPP were (slightly) better in terms of raising expected survival probabilities than random assignment of loans to applicants. [24] Like our results in Table 4, this cuts against the idea that banks channeled funds to the firms that needed them the least. The difference between the average treatment effects for applicants and nonapplicants is also small and positive, indicating that firms for which PPP was more helpful were slightly more likely to apply.

One concern with the results from the GRF IV and Lasso models is that the regularization implicit in these models may cause us to understate the extent of treatment effect heterogeneity. In Columns (3) of Table 5, we explore this possibility by estimating OLS models of treatment effects where we fully interact an indicator for being approved for PPP with the baseline covariates described above. This is the same specification used in the Lasso models, but without regularization. As expected, these models find more treatment effect heterogeneity than the regularized Lasso estimates. Comparing the OLS estimates to the GRF IV results, there is no clear pattern, with the GRF IV results implying greater treatment effect heterogeneity in some cases and the OLS estimates in others. However, in both cases, these differences are modest and our general conclusions are unchanged.

Since we have estimated treatment effects for each firm in our sample, we can examine heterogeneity in treatment effects in a variety of other ways. In Figure 3 Panel A, we simply plot the cumulative distribution function (CDF) of estimated treatment effects for approved firms,

[24] Survival expectations are our preferred measure. Our estimates for realized operational status through July 2020 measured by phone survey are similar to the expectations treatment effects in Table A5, while the sample for the expectations measures is much larger.

applicants, and all firms. We plot the Lasso estimates to show a larger spread in treatment effects relative GRF IV.

Three features stand out. First, there is meaningful variation in estimated treatment effects across firms. The smallest treatment effects in our sample are roughly half as large as the largest ones. Second, the CDF for applicants is slightly to the right of the CDF for all firms, indicating that firms for which PPP was more helpful were slightly more likely to apply. Third, the CDF for approved firms is significantly further to the right of the other two. This indicates that, across the distribution of firms, banks channeled funds to firms for which PPP was more effective.

With our estimated treatment effects, we can also ask how different the average treatment effect would be under the alternative allocation schemes we briefly mentioned above. The bottom four rows of Table 5 Panel A report the results. The first alternative allocation scheme we consider is random assignment to applicants. Focusing on the Lasso results in Column 2, banks' actual allocation decisions for the first tranche of PPP raised average survival expectations 0.4 percentage points more than a random allocation of funds to applicants would have. The second alternative allocation scheme we consider prioritizes frontline industries—we allocate funds to firms in those industries first and assume they exhaust all funds. Banks' actual allocation decisions raised average survival expectations 0.4 percentage points more than this allocation scheme would have, consistent with our findings in Table 4 that treatment effects were not differentially higher for firms in frontline industries. The third alternative allocation scheme we consider prioritizes small firms, first distributing loans to applicants with under 50 employees; again, we assume that total demand for these firms exhausts program funds. Banks' actual allocation decisions resulted in slightly lower average treatment effects than this scheme. Survival expectations were raised 0.1 percentage points less under the actual allocation than a small-firm prioritization scheme. One important caveat applies to this result: our sample contains very few firms with more than 100 employees. Thus, our results should be interpreted to mean that there is little evidence of treatment effect heterogeneity among firms with fewer than 100 employees. In other words, our results do not contradict the prevailing view in the literature (e.g., Autor et al, 2022a; Chetty et al, 2020) that treatment effects for the largest eligible firms with close to 500 employees were likely small.[25]

Finally, we compute the best possible allocation of funds given our estimated treatment effects. We simply allocate loans to the firms with the highest estimated treatment effects until funds run out. With all three models, the best possible allocation does result in higher average treatment effects. The first tranche of PPP would have raised average survival expectations 1.8 to 7 percentage points more, depending on the model, under the best possible allocation than banks' actual allocation decisions. However, while random, industry-, and sized-based allocations were clearly feasible at the time, the best possible allocation was likely infeasible.[26]

The remaining columns of Table 5 Panel A report the same results on heterogeneous treatment effects in different units. Specifically, for each firm we take the estimated treatment effect on expected survival probability, multiply it by the number of employees the firm has, and divide by the size of the PPP loan the firm was eligible for (i.e., 2.5 times the firm's payroll), resulting in a jobs per dollar of PPP number. Essentially, this computation asks how many December 2020 jobs were saved under the assumption that (i) firms had rational expectations about

[25] These results are somewhat distinct from Dalton (2022) and Doniger and Kay (2020) that finds heterogeneity in treatment effects by firm size among firms with less than 100 employees. These papers use different empirical strategies (generally differences-in-differences based approaches rather than our bank relationship based instruments among applicants), use different data, and measure outcomes differently than we do. An important question for future research is understanding which of these factors is most important in explaining these differences.

[26] Appendix Table A10 examines properties of the best possible allocation, reporting the average characteristics of firms that we estimate to have the highest treatment effects and all other firms. The Lasso estimates, which we focus on since they imply more treatment effect heterogeneity, suggest that larger firms that are more likely to have a relationship with a bank have higher treatment effects.

survival and (ii) employees who lost their jobs due to firm failure did not find new jobs.[27] The results imply that banks' actual allocation decisions and feasible alternative allocation schemes (based on industry or firm size) had similar bang-for-the-buck: 1.1-1.5 jobs saved per $100,000 in the Lasso and OLS models and 1.9 to 2.3 jobs saved per $100,000 using the GRF IV model. The ex post best allocation, although likely infeasible, would have been more cost effective: saving about 1.9 to 5.9 jobs per $100,000, depending on the model. Figure 3 Panel B shows the full CDFs of these treatment effects for approved firms, applicants, and all firms for the Lasso estimates. Panel B of Table 5 repeats these exercises with employment growth between January and April 25, 2020 as the dependent variable. The point estimates here are significantly larger because the focus is on jobs saved in the very short run. However, the qualitative comparisons remain similar.

In Appendix Tables 9, and 11 and Appendix Figures 6 and 7 we further explore the robustness of these results to different estimation approaches and the plausibility of alternative explanations for these findings. Appendix Table A9 presents results from a wide variety of alternative approaches to characterizing treatment effect heterogeneity. The point estimates differ somewhat across approaches, but the qualitative findings are consistent across empirical approaches and estimators. Another concern is that our estimates are identified by variation in only a subset of the covariate space. We would then miss treatment effect heterogeneity in other parts of the covariate distribution. Appendix Figure A6 shows that this concern is not warranted in our setting; across the set of covariates, we observe some firms that are treated and some that are not. A related concern is that our instrument may not have a strong first stage for some parts

[27] These long-run estimates assume that firm closures match firm expectations in April and/or firm closure status in July 2020. If some firms re-open or closures are lower than expected, then the long-run effects of PPP would be smaller than our estimates indicate. Kurmann et al. (2021) estimate the effect of PPP on whether firms that use the Homebase time-card management software are open and find that the estimated effects decay by roughly 36 percent between August 2020 and December 2020, but remain fairly constant afterwards. Applying this decay rate from Kurmann et al. (2021) to our results, would imply an average long-run treatment effect of 1.18 jobs per $100,000.

of the covariate space, leading to imprecise estimates of the treatment effects using instrumental variables in those subsamples. Appendix Figure A7 shows that this is not the case and that there is a strong first stage throughout the covariate distribution.

A final concern is that, despite the modest treatment effect heterogeneity across observables, there may be substantial treatment effect heterogeneity across unobservable (to the econometrician) variables and these unobserved determinants of treatment effect heterogeneity could be correlated with having a loan approved by a bank. We investigate this possibility in Appendix Figure A7, where we present estimates of the marginal treatment effects. We cannot reject that the marginal treatment effects are positive everywhere or that they differ from the average treatment effect. Moreover, the estimated marginal treatment effects are largest for the firms least resistant to getting loans through the banks, suggesting the firms most able and interested in receiving loans through banks were also those with the highest treatment effects.

Overall, the results in this section point to three key conclusions. First, the amount of heterogeneity in treatment effects that was ex ante predictable, σ_{α}^{2} in the model in Section III, was moderate. Second, the fact that banks' allocation decisions resulted in average treatment effects that were higher than would have been obtained under a random allocation of funds suggest that the alignment of government and private incentives was not too perverse. In the language of the model, $\phi > 0$, though it may not have been much greater than zero. Third, how much direct provision by the government would have improved the efficacy of the program (θ in the model) depends on what was feasible. Clearly ex ante feasible schemes do not result in higher average treatment effects, while the best allocation does (conditional on there being no required further delay).

These results distinguish our approach from Granja et. Al. (2022), who study the targeting of PPP. Granja et al (2022) compare the geographic distributions of PPP lending and the severity of the initial Covid shock, which *a priori* one might think is correlated with PPP treatment effects. In contrast, we directly estimate firm level treatment effects of PPP and analyze alternative allocation approaches. Both approaches provide useful information on the targeting of PPP. Our approach allows for the fact that treatment effects may not perfectly co-vary with Covid-shock exposure. For example, the most Covid-exposed firms may have closed regardless of any assistance they received. Consequently, even *a priori* plausible targeting approaches may work poorly in practice. Our results suggest that this might be the case, as targeting firms in industries with a high share of frontline workers or with less than fifty employees does not perform substantially differently from the allocation chosen by the banks. However, the Granja et. Al. (2022) approach may be more robust to challenges in directly estimating firm level treatment effects. Additionally, the government may disproportionately care about saving jobs in the most Covid-exposed locations as a form of geographically targeted social insurance.

VIII. The Costs of Delay

Using our estimates, we can provide a back-of-the-envelope calculation of the maximum delay under which direct government provision of PPP with perfect targeting would have been preferable. We assume that costs of each week of delay are linear in time and use the estimates of the long-run employment effects of PPP per dollar loaned from Table 5, Panel A. Given the roughly three week gap between the middle of PPP tranche 1 and the middle of tranche 2, these assumptions imply that the cost per week of delay was roughly 0.4-0.5 jobs per $100,000 of loans delivered. We estimate that the gains to perfect targeting relative to the observed allocation are about 1.6 jobs per $100,000 of loans delivered. Combining these figures, delaying the rollout of

PPP to allow the loans to be perfectly targeted would only have been preferable, holding fixed other program characteristics, if the required delay were less than 4.4 weeks.[28] This is obviously a very rough estimate but gives a sense of what our results imply.[29]

How long would it have taken for the government to roll out its own infrastructure for delivering loans? One relevant benchmark is the Economic Injury Disaster Loans (EIDL) program that provided loans to small businesses in need of liquidity during the pandemic and was directly administered by the SBA. The EIDL program existed prior to the Covid-19 pandemic, but the program was massively expanded during the pandemic. EIDL loans totaled $98 million in 2019, while $194 billion in loans were approved from March-November 2020. Similar to the PPP program, to be eligible for an EIDL loan, businesses had to have fewer than 500 employees and demonstrate that they were suffering working capital losses due to the pandemic. Appendix Figure A8 shows the time series of disbursements for the EIDL Covid-19 program. Though the program opened at the same time as the PPP, the disbursements only began to ramp up in the first half of May, six-to-eight weeks after the passage of the CARES Act on March 27, 2020. Combined with our results on treatment effect heterogeneity, this suggests that the government's choice to delegate PPP lending to banks was likely a sensible one, given the goal of maximizing the total impact of the program on firm survival and employment.[30]

---

[28] While the IV-GRF gains from the best possible allocation are larger in Column (3) than in Column (4), the implied cost of delay is also larger. The IV-GRF estimates imply that delay could have been warranted if optimal targeting were feasible within 4.7 weeks. The pure OLS estimates imply a threshold of 5.2 weeks.

[29] The timing of our survey might have missed some businesses that were already closed and not responding. To the extent that those businesses were more financially fragile, this suggests that they might have had a larger cost of delay than the cost we observe in our estimate.

[30] However, the optimal delegation choice could have been different under different institutional arrangements. For example, if the SBA had greater capacity or experience administering many loans rapidly the delay required to not-delegate the loan administration to banks and better target the loans may have been smaller. This calculation also assumes similar amount of fraud in public and private delivery of PPP. According to the SBA's inspector general, rates of fraud were if anything higher in the EIDL program than in PPP. See: https://www.sba.gov/document/report-23-09-covid-19-pandemic-eidl-ppp-loan-fraud-landscape

IX. Conclusion

Was it sensible to delegate PPP decisions to banks? Our model points to three key considerations. First, how large was the treatment effect heterogeneity? Second, did banks have misaligned incentives that led them to prioritize businesses that benefited less from loans? Third, how important was speed? Our empirical analysis suggests that banks may have had misaligned targeting incentives to some extent—firms with stronger connections to banks were more likely to have their applications approved, and firms with less cash-on-hand were less likely to be approved. Yet, we also find that the heterogeneity in treatment effects was modest relative to the large costs of delaying loans. Furthermore, loan delay was costly, as evidenced by the fact that the treatment effects we estimate reflect the impact of getting PPP loans in the first tranche relative to the second tranche, a delay of only a few weeks for most borrowers.

On net, our analysis suggests that bank delegation was sensible in this setting, as the cost of delaying loan rollout outweighed the benefits of improved targeting unless the SBA could have perfectly targeted loans to the highest long-run employment effect firms in less than 4.4 weeks. Our results illustrate the tradeoff between delay and targeting quality faced by governments when deciding whether to delegate the allocation of time sensitive funds.

While our model is motivated by the PPP, similar tradeoffs exist when private hospitals or pharmacies administer publicly provided vaccines or when FEMA provides insurance payments directly after a natural disaster rather than relying on local entities. Guided by a model which characterizes the determinants of delegation, we find that delegation was unlikely to have severely distorted the impact of the PPP program. However, the parameters highlighted by the model may differ in other settings, resulting in a different answer on the desirability of delegation.

References

Autor, David, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz. 2022a. "An evaluation of the paycheck protection program using administrative payroll microdata." *Journal of Public Economics* 211.

Autor, David, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz. 2022b. "The $800 billion paycheck protection program: where did the money go and why did it go there?" Journal of Economic Perspectives 36, no. 2: 55-80.

Athey, Susan and Guido Imbens. 2016. "Recursive partitioning for heterogeneous causal effects." Proceedings of the National Academy of Sciences, 113(27): 7353-7360.

Balla-Elliott, Dylan, Zoë B. Cullen, Edward L. Glaeser, Michael Luca, and Christopher T. Stanton. 2020. "Business Reopening Decisions and Demand Forecasts During the COVID-19 Pandemic" (No. w27362). National Bureau of Economic Research.

Barraza, S., Rossi, M. and Yeager, T.J., 2020. The short-term effect of the Paycheck Protection Program on unemployment. *Available at SSRN 3667431*.

Barrios, John, Michael Minnis, William Minnis, and Joost Sijthoff. 2020. "Assessing the Payroll Protection Program: A Framework and Preliminary Results." Working paper.

Bartik, Alexander W., Marianne Bertrand, Zoë B. Cullen, Edward L. Glaeser, Michael Luca, and Christopher T. Stanton. 2020a. "The impact of COVID-19 on small business outcomes and expectations." Proceedings of the National Academy of Sciences.

Bartik, Alexander W., Zoë B. Cullen, Edward L. Glaeser, Michael Luca, and Christopher T. Stanton. 2020b. "What Jobs are Being Done at Home During the COVID-19 Crisis? Evidence from Firm-Level Surveys" (No. w27422). National Bureau of Economic Research.

Bartik, Alexander, Marianne Bertrand, Feng Lin, Jesse Rothstein, and Matt Unrath. 2020c. "Measuring the Labor Market at the Onset of the COVID-19 Crisis," Brookings Papers on Economic Activity.

Bartlett, Robert and Andi Morse. 2020. "Small Business Survival Capabilities and Policy Effectiveness: Evidence from Oakland" (No. w27629). National Bureau of Economic Research.

Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen. 2013. "Inference for high-dimensional sparse econometric models." In Advances in Economics and Econometrics: 10th World Congress, Vol. 3: Econometrics, Cambridge University Press: Cambridge, 245-295.

Belloni, Alexandre, Victor Chernozhukov, and Larry Wang. 2014. "Pivotal estimation via square-root-lasso in non-parametric regression." Annals of Statistics 42(2): 757-788.

Berger, A.N., Freed, P.G., Scott, J.A. and Zhang, S., 2021. The paycheck protection program (PPP) from the small business perspective: did the PPP help alleviate financial and economic constraints?. *Available at SSRN 3908707*.

Chetty, Raj, John N. Friedman, Nathan Hendren, and Michael Stepner. 2020. "How did covid-19 and stabilization policies affect spending and employment? a new real-time economic tracker based on private sector data" (No. w27431). National Bureau of Economic Research.

Chodorow-Reich, Gabriel, Olivier Darmouni, Stephan Luck, and Matthew C. Plosser. 2020. "Bank Liquidity Provision across the Firm Size Distribution."

Cole, A., 2022. The Impact of the Paycheck Protection Program on (Really) Small Businesses. *Available at SSRN 3730268*.

Cororaton, Anna, and Samuel Rosen. 2020. “Public Firm Borrowers of the US Paycheck Protection Program.” SSRN working paper.

Crane, Leland, Ryan Decker, Aaron Flaaen, Adrian Hamins-Puertolas, and Christopher Kurz. 2022. “Business Exit During the COVID-19 Pandemic : Non-Traditional Measures in Historical Context.” Journal of Macroeconomics 72 (June).

Dalton, M. 2021. Putting the Paycheck Protection Program into perspective: An analysis using administrative and survey data (No. 542). Bureau of Labor Statistics.

Davidson, Russell, and James G. MacKinnon, 2006. “The case against JIVE.” Journal of Applied Econometrics 21, no. 6: 827-833.

Denes, M., Lagaras, S. and Tsoutsoura, M. 2021. “First Come, First Served: The Timing of Government Support and Its Impact on Firms.” SSRN working paper

Doniger, C. and Kay, B., 2021. “Ten Days Late and Billions of Dollars Short: The Employment Effects of Delays in Paycheck Protection Program Financing.” Federal Reserve Working Paper.

Elenev, Vadim, Tim Landvoigt, and Stijn Van Nieuwerburgh. 2020. “Can the Covid Bailouts Save the Economy?” National Bureau of Economic Research.

Engel, Eduardo, Ronald D. Fischer, and Alexander Galetovic. The economics of public-private partnerships: A basic guide. Cambridge University Press, 2014.

Faulkender, R., Jackman, R., and Miran, S., 2020. The Job Preservation Effects of Paycheck Protection Program Loans. US Department of the Treasury, Office of Economic Policy. Working Paper 2020-01.

Fairlie, R. and Fossen, F, 2020. “Did the $660 Billion Paycheck Protection Program and $220 Billion Economic Injury Disaster Loan Program Get Disbursed to Minority Communities in the Early Stages of COVID-19?” National Bureau of Economic Research Working Paper 28321.

Fazzari, S.M., R.G. Hubbard and B.C. Petersen, 1988. “Financing Constraints and Corporate Investment.” Brookings Papers on Economic Activity, 141-195.

Foster, J.C. (2013). Subgroup Identification and Variable Selection from Randomized Clinical Trial Data. PhD Thesis.

Granja, J., Makridis, C., Yannelis, C. and Zwick, E., 2022. “Did the Paycheck Protection Program Hit the Target?” Journal of Financial Economics.

Hanson, S., Stein, J., Sunderam, A., and Zwick, E. 2020. “Business Credit Programs in the Pandemic Era.” Brookings Papers on Economic Activity, Fall 2020.

Holmstrom, B., and J. Tirole, 1997. “Financial intermediation, loanable funds, and the real sector.” Quarterly Journal of Economics 112:663−691.

Hubbard, R.G. and Strain, M., 2020. “Has the Paycheck Protection Program Succeeded?” National Bureau of Economics Working Paper 28032.

Humphries, J. and Neilsen, C. and Ulyssea, G, 2020. “Information Frictions and Access to the Paycheck Protection Program.” Journal of Public Economics 190: October 2020.

Joaquim, G., and Netto, F., 2021. “Bank Incentives and the Effect of the Paycheck Protection Programs.” Federal Reserve Working Paper.

Joaquim, G. and Wang, J.C., 2022. “What Do 25 Million Records of Small Businesses Say about the Effects of the PPP?” Working Paper.

Kaplan, S.N., and L. Zingales (1997), "Do investment-cash flow sensitivities provide useful measures of financing constraints?" Quarterly Journal of Economics 112:159−216.

Kurmann, A., Lale, E., and Ta, L. 2021. "The Impact of COVID-19 on Small Business Dynamics and Employment: Real-time Estimates with Homebase Data." Working Paper.

Kunzel, S. R., Sekhon, J.S., Bickel, P.J. and Yu, V. (2019). "Metalearners for estimating heterogeneous treatment effects using machine learning." Proceedings of the National Academy of Sciences, 116 (10): 4156-4165.

Li, L. and Strahan, P.E., 2021. Who supplies PPP loans (and does it matter)? Banks, relationships, and the COVID crisis. *Journal of Financial and Quantitative Analysis*, *56*(7), pp.2411-2438.

Mongey, S., L. Pilossoph, and A. Weinberg (2021), "Which workers bear the burden of social distancing?" Journal of Economic Inequality, forthcoming.

Myers, S.C., and N.C. Majluf (1984). "Corporate financing and investment decisions when firms have information that investors do not have." Journal of Financial Economics 13:187−222

Nie, X. and Wagter, S. (2021). "Quasi-oracle estimation of heterogenous treatment effects." Biometrika. 108(2): 299-319.

Robinson, P.M. (1988). "Root-n-consistent semiparametric estimation. Econometrica. pp. 931-954.

Staples, A.J. and Krumel Jr, T.P., 2022. The Paycheck Protection Program and small business performance: Evidence from craft breweries. *Small Business Economics*, pp.1-26.

Wager, S. and Athey, S. (2018). "Estimation and inference of heterogeneous treatment effects u sing random forests." Journal of the American Statistical Association, 113(523): 1228-1242.

Wang, J., J. Yang, B. Iverson, and R. Jiang. 2022. "Bankruptcy and the Covid-19 Crisis." Working paper.

Zwick, E., and J. Mahon, 2017. "Tax Policy and Heterogeneous Investment Behavior." American Economic Review, 107(1): 217-48.

Figures and Tables

Figure 1. Details about the PPP Program from the SBA data

PPP program daily new loan approvals and cumulative funds deployed over time, based on data provided by the SBA. Red lines indicate the end of tranche 1 on 4/16/2020 and the beginning of tranche 2 on 4/27/2020.

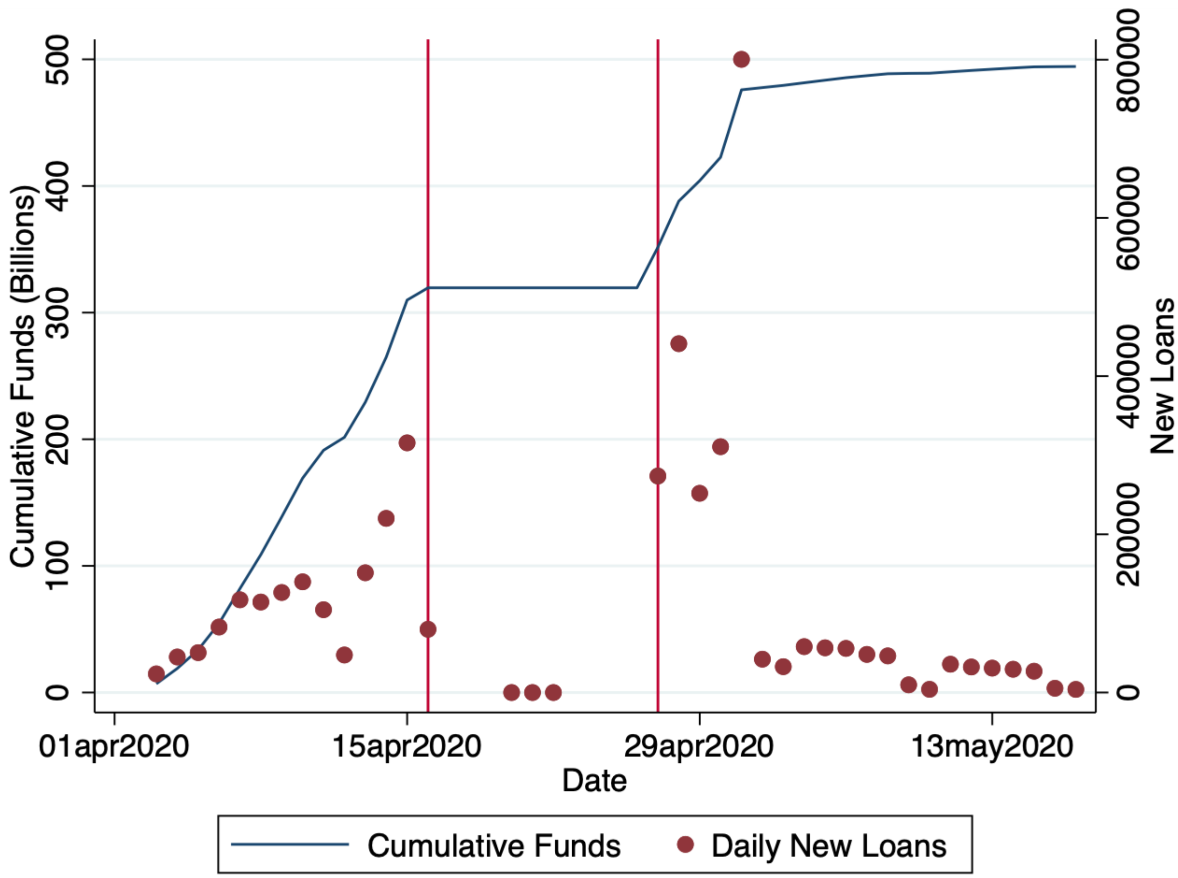


Figure 2. Fraction of respondents applying for PPP and application outcomes by respondent characteristics. This figure plots application and approval rates conditional on application by firm characteristics. The first set of characteristics is cash on hand. Respondents were asked "Consider the cash you have on hand today. How long will the cash you have today last under the current disruptions?" The second set of characteristics is the firm's number of employees in January 2020. The third set of characteristics is pre-Covid fixed monthly expenses ($000s). Fixed expenses come from the survey question "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?" The fourth set of characteristics is business age. The fifth set of characteristics is industry.

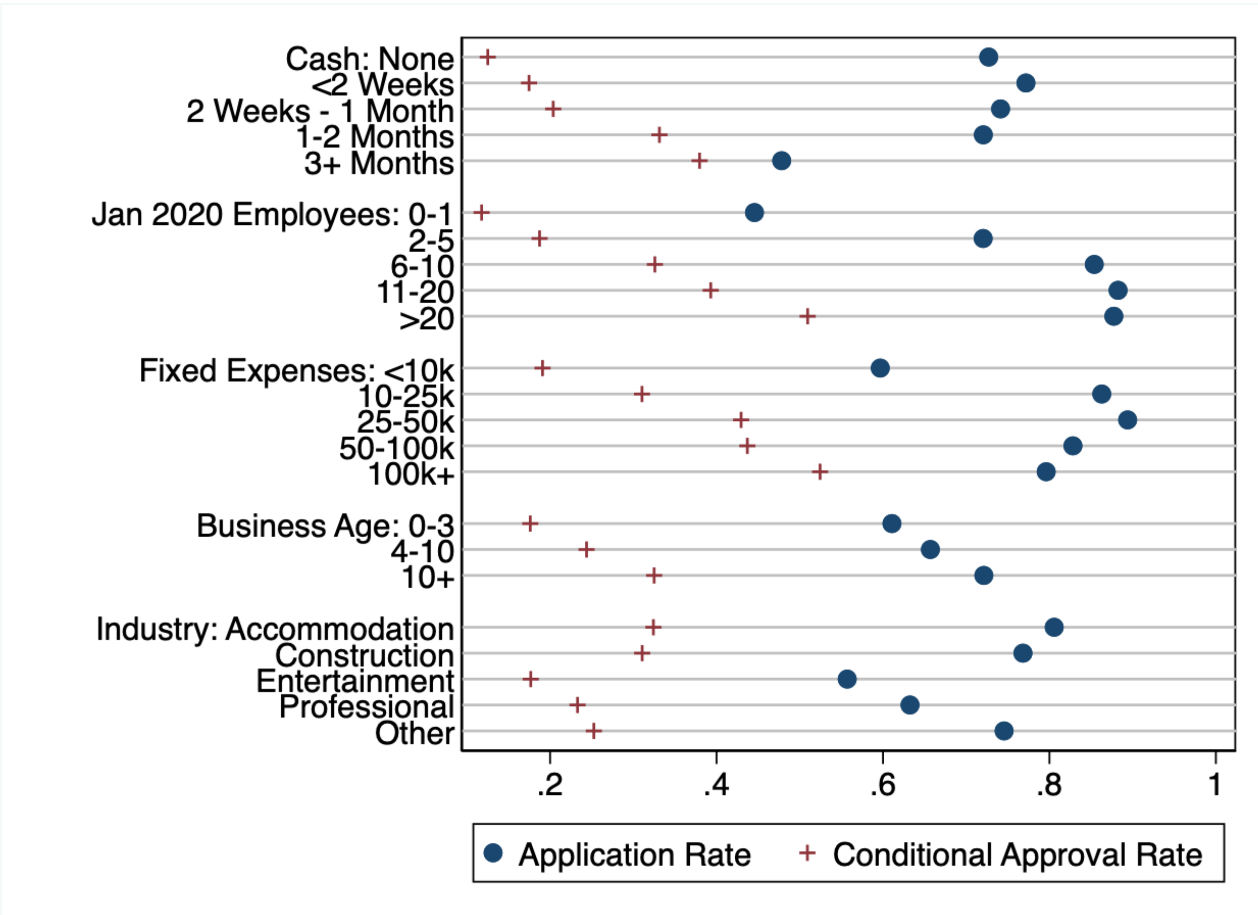
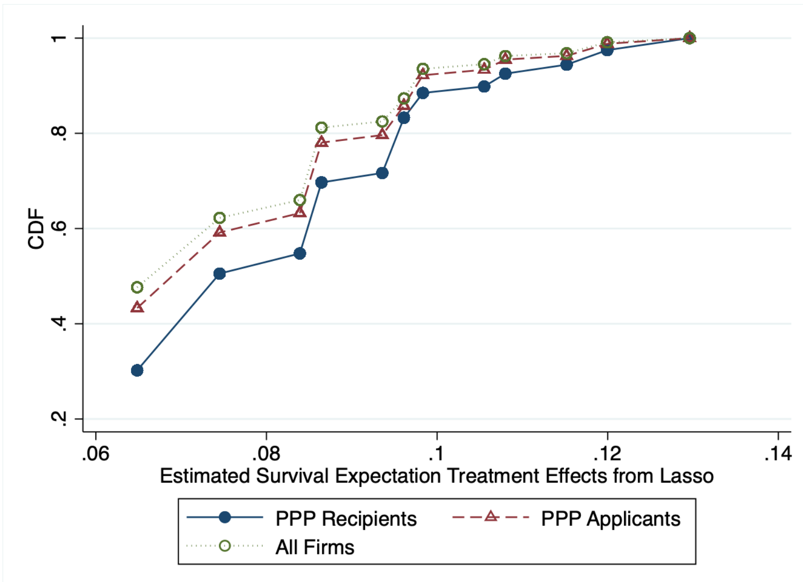


Figure 3. Estimated distributions of heterogeneous treatment effects on firm survival expectations. Treatment effects are estimated among applicants by fitting Lasso models of approval interacted with firm-level covariates, bank-relationship information, and industry characteristics. The possible interactions in the model are between PPP approval and months of cash available, pre-COVID monthly fixed expenditures, pre-COVID employment (categorical), an indicator for a bank loan, an indicator for a loan officer relationship, and 2-digit industry dummies. Treatment effects are projected to non-applicants based on these characteristics. The model is fit using the rlasso Stata package with heteroskedastic errors after netting out state fixed effects (see Belloni et al. 2013).

Panel A: Distribution of Raw Survival Expectation PPP Treatment Effects



Panel B: Estimated distributions of heterogeneous effects on long-run employment per dollar of program cost**.**

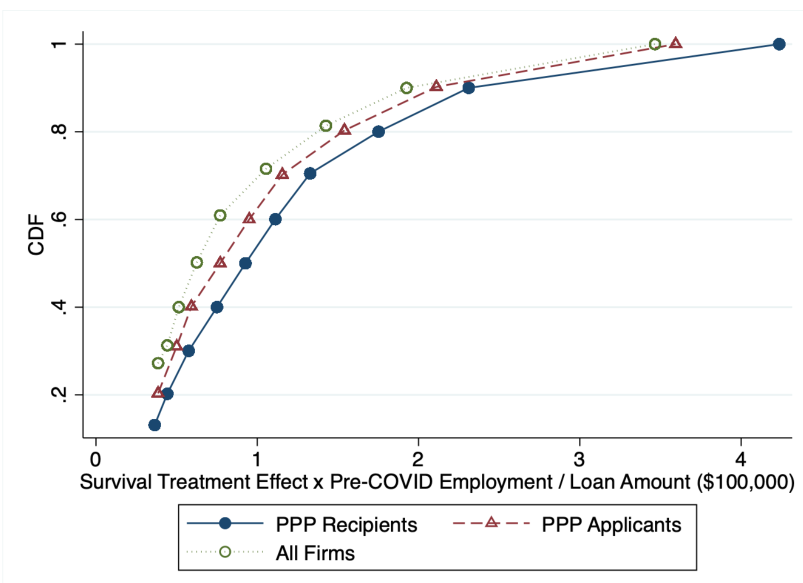


Table 1: Summary Statistics

This table reports summary statistics for the main survey estimation sample. Variables are defined as follows: Open on 4/25/2020 is an indicator that the firm was open for business at the time of the survey. Survival expectation is the probability a firm expects to be open in December 2020. Applied for PPP is an indicator that the firm applied or intended to apply for PPP in the past, with approval, pending, or denied/unable as outcomes. PPP denied/unable indicates that the firm was unable to apply for PPP or that the SBA denied the application. Jan 2020 Employees is the number of employees in January of 2020, prior to Covid. Cash is cash on hand, reported in terms of weeks the firm's cash will last if its current impact from Covid-19 persists. This variable was categorical in the survey and had a maximum value of "3 or more months". Payroll and fixed expenses are in thousands of dollars for the typical month before Covid-19 disruptions. Tranche 1 Share of Loans by Bank is calculated as the number of loans approved in tranche 1 of the program over the total number of loans approved through the first 21 days of tranche 2, as computed for each respondents' bank from SBA administrative data. Bank Steering Based on Jobs Saved is the difference in average borrower headcount for loans approved in tranche 1 and the first 21 days of tranche 2. Top 4, top 5-10, top 11-20, and credit union are all dummies indicating the type of bank the firm uses. Existing loan indicates the firm had a loan from its bank prior to PPP. Loan officer indicates the firm has a relationship with a loan officer at its bank. Frontline industries are those with a high number of frontline workers and include accommodation/food services, entertainment, construction, healthcare services, manufacturing, retail, wholesale trade, and transportation. Employee proximity is computed from O-NET measures, on a 1-5 scale, for distance to others at the occupation level. These measures are aggregated to the industry level, and we merge to the industry distrubution at the 3-digit zipcode level based on the 2019 County Business Patterns data. Zipcode Covid Cases and Zipcode UI Claims are Opportunity Insights measures of Covid cases per-capita and UI claims per capita since the beginning of 2020 in the 3-digit zipcode. In later analyses, we fill in missing zipcode data with a missing dummy.

	N	mean	sd	p25	p50	p75
Open on 4/25/2020	6640	0.56	0.50	0.00	1.00	1.00
Survival Expectation	6640	0.73	0.28	0.50	0.80	1.00
Applied for PPP	6640	0.67	0.47	0.00	1.00	1.00
PPP Approved	4452	0.25	0.43	0.00	0.00	0.00
PPP Pending	4452	0.51	0.50	0.00	1.00	1.00
PPP Denied/Unable	4452	0.24	0.43	0.00	0.00	0.00
4/25/2020 Employees	6640	5.50	17.65	1.00	2.00	5.00
January 2020 Employees	6640	7.75	22.46	1.00	3.00	7.00
Cash (weeks)	6640	5.23	4.50	1.00	6.00	12.00
Payroll ($k)	6640	24.09	74.02	5.00	5.00	17.50
Fixed Expenses ($k)	6640	14.57	38.03	5.00	5.00	17.50
Tranche 1 Share of Loans by Bank	6640	0.29	0.25	0.03	0.25	0.49
Bank Steering Based On Jobs Saved / 10	6508	1.46	1.65	0.51	1.34	1.91
Top 4 Bank	6640	0.34	0.48	0.00	0.00	1.00
Top 5-10 Bank	6640	0.13	0.34	0.00	0.00	0.00
Top 11-20 Bank	6640	0.03	0.18	0.00	0.00	0.00
Credit Union	6640	0.10	0.30	0.00	0.00	0.00
Existing Loan	6640	0.40	0.49	0.00	0.00	1.00
Loan Officer	6640	0.24	0.43	0.00	0.00	0.00
Frontline Industry	6640	0.53	0.50	0.00	1.00	1.00
Employee Proximity at Zipcode Level	6330	3.53	0.04	3.50	3.53	3.55
Zipcode Covid Cases/Capita	6243	0.09	0.17	0.01	0.03	0.07
Zipcode UI Claims/Capita	3744	0.36	0.24	0.20	0.28	0.48

Table 2: Reduced Form Estimates of Bank Speed and Steering

This table reports reduced form estimates of bank speed and steering using the survey sample. Columns 1-4 do not condition on applicants, while columns 5-8 condition on applicants. Details about the controls are below the final panel. Detailed controls include categorical indicators for the business' payroll and fixed expenses, the number of weeks of cash remaining, the average proximity of employees to one another in the zipcode based on the industry distribution in the County Business Patterns data, and Opportunity Insights measures of Covid cases per-capita and UI claims since the beginning of 2020 in the 3-digit zipcode. Columns 3 and 7 include bank type dummies to guard against the fact that firms banking with large banks or credit unions may differ. Column 8 reweights the sample to match terciles of the proximity of employees across industries, Census regions, and payroll for the population of firms receiving PPP in SBA data. There are 35 firms that have zero weights because of sparsity of some reweighting cells. The first regressor is the Tranche 1 Share of PPP Loans Approved by the Bank, which is computed at the bank level for the share of tranche 1 loans relative to all loans approved through the bank within the first 3 weeks of tranche 2. The second regressor is Bank Steering Based on Jobs Saved, which is the average number of jobs per loan in tranche 1 less the average loans in the first 21 days of tranche 2 as reported in SBA administrative data. Both of these regressors are calculated from the SBA administrative data. Steering cannot be computed for all banks, explaining the difference in sample size between this table and the summary statistics. We winsorize the steering measure at the 1% level. July 2020 operational status in Panel B was collected from a phone audit that only included PPP applicant firms, so we only report non-redundant specifications. All standard errors are clustered by bank and reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Survival Expectations								
Tranche 1 Share of PPP Loans by Bank	0.081***	0.078***	0.049***	0.030*	0.107***	0.067***	0.042*	0.122***
	(0.012)	(0.013)	(0.013)	(0.014)	(0.014)	(0.016)	(0.019)	(0.016)
Bank Steering Based on Jobs Saved/10	-0.000	-0.000	-0.000	-0.001	0.005	0.003	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
R-Squared	0.01	0.06	0.17	0.17	0.07	0.17	0.18	0.08
Observations	6508	6508	6508	6508	4385	4385	4385	4348
Panel B: July 2020 Operational Status								
Tranche 1 Share of PPP Loans by Bank	0.167***				0.125***	0.059	0.069	0.094
	(0.033)				(0.034)	(0.041)	(0.046)	(0.049)
Bank Steering Based on Jobs Saved/10	0.019*				0.012	0.012	0.012	0.015
	(0.009)				(0.008)	(0.009)	(0.009)	(0.011)
R-Squared	0.01				0.06	0.11	0.11	0.07
Observations	2704				2704	2704	2704	2682
Panel C: April 25, 2020 Employment								
Tranche 1 Share of PPP Loans by Bank	2.482***	2.381***	1.970***	1.589*	3.563***	2.955***	2.572**	4.534***
	(0.732)	(0.692)	(0.588)	(0.649)	(0.782)	(0.692)	(0.797)	(1.195)
Bank Steering Based on Jobs Saved/10	0.119	0.121	0.076	0.087	0.389*	0.286	0.288	0.656**
	(0.108)	(0.116)	(0.118)	(0.118)	(0.154)	(0.156)	(0.154)	(0.209)
R-Squared	0.58	0.59	0.60	0.60	0.55	0.56	0.56	0.59
Observations	6508	6508	6508	6508	4385	4385	4385	4348
Panel D: Employment Pct Changes								
Tranche 1 Share of PPP Loans by Bank	0.214***	0.216***	0.190***	0.191***	0.287***	0.248***	0.260***	0.334***
	(0.036)	(0.044)	(0.050)	(0.047)	(0.056)	(0.061)	(0.058)	(0.073)
Bank Steering Based on Jobs Saved/10	0.017*	0.017	0.015	0.015	0.041**	0.039**	0.040**	0.049**
	(0.009)	(0.011)	(0.011)	(0.011)	(0.013)	(0.014)	(0.013)	(0.016)
R-Squared	0.52	0.54	0.56	0.56	0.51	0.54	0.54	0.53
Observations	6508	6508	6508	6508	4385	4385	4385	4348
Only Applicants	N	N	N	N	Y	Y	Y	Y
State and Industry FE	N	Y	Y	Y	Y	Y	Y	Y
Detailed Controls	N	N	Y	Y	N	Y	Y	N
Bank Type FE	N	N	N	Y	N	N	Y	N
Reweighted	N	N	N	N	N	N	N	Y

Table 3: PPP Applications, Approvals, and Denials

This table displays regressions of PPP application, approval, and denial indicators as of April 25, 2020 on firm, banking relationship, and location characteristics. In columns 2 and 3, the sample is restricted to firms that applied for PPP. Receipt rate in column 4 does not condition on applying for PPP. Panel A includes simple bivariate regressions, whereas Panel B includes the displayed regressors plus fixed effects for state, 2-digit naics code, business status, missing zipcode and missing Opportunity Insights measures. The Tranche 1 Share of PPP Loans by Bank is the number of loans in tranche 1 over total loans in tranche 1 and the first 21 days of tranche 2. Standard errors clustered by bank (967 clusters) are reported in parentheses.

	Application Rate	Approval Rate	Denial Rate	Receipt Rate
Panel A: Univariate Regressions				
Tranche 1 Share of PPP Loans by Bank	0.03	0.54***	-0.12*	0.37***
	(0.03)	(0.04)	(0.05)	(0.03)
R2	0.00	0.09	0.00	0.06
N	6640	4452	4452	6640
Panel B: Multivariate Regressions with Firm, Bank, and Local Characteristics				
Tranche 1 Share of PPP Loans by Bank	0.04	0.44***	-0.10**	0.32***
	(0.02)	(0.03)	(0.04)	(0.02)
High Payroll	0.21***	0.13***	-0.15***	0.14***
	(0.01)	(0.02)	(0.02)	(0.02)
High Fixed Expenses	0.11***	0.08***	-0.01	0.09***
	(0.01)	(0.01)	(0.01)	(0.01)
High Cash	-0.13***	0.15***	-0.07***	0.07***
	(0.01)	(0.02)	(0.02)	(0.01)
Existing Loan	0.05***	0.04**	-0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Loan Officer	0.07***	0.05**	-0.02	0.06***
	(0.01)	(0.02)	(0.01)	(0.02)
Average Zip Code Proximity	-0.12	-0.05	0.20	-0.08
	(0.14)	(0.20)	(0.18)	(0.14)
Zipcode Covid Cases/Capita	0.08	-0.04	0.02	-0.01
	(0.05)	(0.04)	(0.05)	(0.04)
Zipcode UI Claims/Capita	-0.00	0.04	-0.02	0.04
	(0.05)	(0.05)	(0.05)	(0.03)
R2	0.16	0.21	0.08	0.18
N	6640	4452	4452	6640

Table 4: IV Estimates of Heterogeneous Treatment Effects

This table displays heterogeneous effects of PPP approval as a function of firm characteristics (in column headings) denoted Z. "Frontline Industries" are Accommodation and Food Services; Arts, Entertainment, and Recreation; Construction; Healthcare and Social Assistance; Manufacturing; Retail Trade; Transportation and Warehousing; and Wholesale Trade. “Cash” is months of cash on hand at the time of the survey, “Loan” indicates the firm had a bank loan, “Officer” indicates the firm had a relationship with a loan officer, “Payroll” is the firm’s monthly wage bill, “Fixed expenses” are non-variable monthly expenses, B2B is the share of business-to-business sales for the industry, and "Employee Proximity in Zipcode" measures how close employees work to one another for all firms at the zipcode-level, based on the Census industry distribution. In Panel A the dependent variable is the probability a firm expects to be open in December 2020, reported in 10 percentage point increments. In Panel B the dependent variable is the probability of being open in the phone survey conducted in July of 2020. In Panel C, the dependent variable is employment as of April 25, 2020, controlling for employment in January. In Panel D, the dependent variable is the inverse hyperbolic sine transformed employment level. We instrument for PPP approval with the firm’s bank delay instrument described in Table 3 and interact the instrument with the characteristic Z. Characteristic Z is also included in each regression but coefficients are not reported for brevity. All specifications include the zipcode level controls and fixed effects for industry, state, business status as-of the survey, and each of the dimensions of baseline heterogeneity measures given in the column headings (e.g. detailed cash on hand, payroll, fixed expenses, etc.). For details about sensitivity to controls, see Appendix Tables A6 and A7. Panels C and D include controls for January employment in levels or the inverse hyperbolic sine of employment. Standard errors clustered by bank reported in brackets. Sample size is 4,452 in Panels A, C, and D and is 2,749 in Panel B. Below each panel, we display the mean treatment effect, which is the mean fitted value of the baseline approval coefficient and the heterogeneous treatment coefficient. We display these means for firms with approved loans, all applicant firms, and non-applicant firms.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Z=	Frontline Industry	High Cash	Loan	Officer	Payroll	Fixed Expenses	B2B	Employee Proximity in Zipcode	High Covid Cases in Zipcode
Panel A: Survival Expectations									
PPP approved x high Z	0.04	-0.07	-0.04	-0.05	0.01	-0.04	0.07	-0.06*	0.02
	(0.05)	(0.06)	(0.05)	(0.04)	(0.06)	(0.05)	(0.07)	(0.03)	(0.04)
PPP approved	0.11**	0.18***	0.15***	0.15***	0.12*	0.15***	0.11**	0.16***	0.12***
	(0.05)	(0.07)	(0.05)	(0.04)	(0.07)	(0.05)	(0.04)	(0.04)	(0.04)
Approved Treatment Effects (TEs)	0.131	0.131	0.130	0.128	0.129	0.125	0.129	0.137	0.128
All Applicant TEs	0.130	0.144	0.133	0.135	0.127	0.131	0.130	0.138	0.129
Non-Applicant TEs	0.126	0.132	0.137	0.140	0.123	0.141	0.133	0.137	0.129
Panel B: July 2020 Operational Status									
PPP approved x high Z	0.03	0.11	0.03	-0.19	0.02	0.02	0.01	0.08	0.08
	(0.12)	(0.13)	(0.15)	(0.14)	(0.13)	(0.13)	(0.11)	(0.07)	(0.06)
PPP approved	0.06	0.01	0.07	0.16	0.07	0.07	0.08	0.05	0.05
	(0.10)	(0.11)	(0.15)	(0.12)	(0.14)	(0.11)	(0.08)	(0.08)	(0.09)
Approved Treatment Effects (TEs)	0.083	0.082	0.080	0.079	0.081	0.083	0.081	0.082	0.081
All Applicant TEs	0.082	0.061	0.078	0.104	0.076	0.079	0.081	0.080	0.083
Non-Applicant TEs	0.078	0.080	0.075	0.127	0.069	0.074	0.082	0.081	0.081
Panel C: Employment as of April 25, 2020									
PPP approved x high Z	-1.10	-0.68	0.44	0.43	7.75***	6.93**	-5.47**	-1.12	2.12
	(2.14)	(3.09)	(2.02)	(2.98)	(2.27)	(3.17)	(2.13)	(1.68)	(1.33)
PPP approved	6.22***	6.08**	5.40**	5.46**	0.33	2.79**	7.24***	6.38***	4.61***
	(2.25)	(3.08)	(2.29)	(2.66)	(1.56)	(1.36)	(2.12)	(2.13)	(1.70)
Approved Treatment Effects (TEs)	5.57	5.66	5.62	5.64	5.59	6.31	5.63	5.90	5.58
All Applicant TEs	5.60	5.78	5.59	5.58	4.00	5.26	5.52	5.92	5.63
Non-Applicant TEs	5.72	5.67	5.55	5.53	1.71	3.59	5.31	5.90	5.60
Panel D: Employment Pct Changes (Inv. Hyperbolic Sine)									
PPP approved x high Z	-0.04	-0.18	0.22	0.41**	0.16	0.18	-0.16	0.08	0.07
	(0.15)	(0.36)	(0.22)	(0.18)	(0.17)	(0.24)	(0.20)	(0.12)	(0.09)
PPP approved	0.41**	0.52	0.28	0.23	0.28	0.32**	0.44**	0.36*	0.36**
	(0.17)	(0.38)	(0.28)	(0.22)	(0.24)	(0.13)	(0.22)	(0.20)	(0.18)
Approved Treatment Effects (TEs)	0.392	0.401	0.388	0.400	0.393	0.412	0.394	0.398	0.393
All Applicant TEs	0.393	0.434	0.373	0.346	0.360	0.385	0.391	0.396	0.394
Non-Applicant TEs	0.397	0.403	0.353	0.299	0.313	0.341	0.385	0.398	0.393

Table 5: Estimates of Treatment Effect Heterogeneity and Bank Bias

This table reports moments of the distribution of treatment effects, estimated from generalized random forest IV regressions, Lasso regressions fit on the sample of applicants, or OLS regressions fit on the sample of applicants. In the Lasso and OLS models, we interact PPP approval with applicant characteristics. The possible interactions are between PPP approval and 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, the share of the local population receiving UI, the local COVID case rate, and 2-digit industry dummies. We partial out state fixed effects. Given these characteristics, we project the potential treatment effects on all firms (not just applicants). Panel B also includes IHS employment in January 2020 as a regressor. Columns 4 through 6 scale the treatment effects to reflect 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). To estimate jobs saved, we rescale the unweighted treatment effects by January 2020 employment divided by actual program costs for recipients and imputed program costs (based on payroll) for non-recipients. The difference relative to random allocation gives the mean and standard error of approval bias relative to random sampling from applicants, computed as the average treatment effect among recipients less the average treatment effect among applicants. The difference relative to the frontline industry prioritization row is the difference in the observed allocation relative to the average treatment effect for applicants in industries with a high share of frontline workers. The difference relative to small firm prioritization does the same exercise for treatment effects for applicants with under 50 employees. The difference relative to the best possible allocation is the difference between the mean treatment effect for recipients and the mean treatment effect if funds were allocated to the highest treatment effect firms first. In columns 3 and 4 we weight by employment over program costs before calculating these differences relative to alternative allocations. For unweighted IV estimates that do not regularize the treatment effects, see Appendix Table A9, which reports similar patterns to this exercise. We use bootstrap for inference including 150 bootstrap samples. We use these bootstrap samples to compute standard errors, which are shown in parentheses under each estimate, and 5th and 95th percent confidence intervals, which are shown in brackets.

	IV (Generalized RF)	Lasso	OLS	IV (Generalized RF)	Lasso	OLS
		Unweighted		Rescaled by Jan 2020 Employment / Imputed Program Cost ($100000)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Survival Expectations and End-of-Year (Longer-Run) Jobs-Per-Dollar						
Average Treatment Effect Moments for:						
Recipients	0.127	0.083	0.091	2.32	1.47	1.47
	(0.039)	(0.009)	(0.007)	(0.76)	(0.18)	(0.2)
	[0.04,0.177]	[0.063, 0.093]	[0.081,0.101]	[0.68,3.22]	[1.09,1.66]	[1.12,1.79]
Applicants	0.13	0.079	0.089	2.2	1.30	1.36
	(0.041)	(0.01)	(0.006)	(0.73)	(0.17)	(0.17)
	[0.04,0.178]	[0.058,0.089]	[0.08,0.098]	[0.59,3.02]	[0.95,1.50]	[1.12,1.71]
All Firms	0.128	0.076	0.079	2.05	1.19	1.15
	(0.042)	(0.01)	(0.006)	(0.68)	(0.16)	(0.14)
	[0.038,0.178]	[0.056,0.088]	[0.069,0.089]	[0.54,2.83]	[0.86,1.36]	[0.96,1.45]
Difference in treatment effects for recipients relative to:						
Random allocation among applicants	-0.003	0.004	0.003	0.12	0.17	0.11
	(0.004)	(0.003)	(0.006)	(0.1)	(0.07)	(0.12)
	[-0.006,0.005]	[0.001,0.012]	[-0.006,0.011]	[-0.02,0.31]	[0.07,0.30]	[-0.09,0.27]
Frontline Industry Prioritization	-0.002	0.004	0.006	0.07	0.24	0.12
	(0.006)	(0.003)	(0.012)	(0.12)	(0.07)	(0.22)
	[-0.007,0.011]	[0.001,0.012]	[-0.011,0.029]	[-0.09,0.3]	[0.14,0.35]	[-0.21,0.52]
Small Firm Prioritization	-0.002	-0.001	0.013	0.41	0.01	0.38
	(0.005)	(0.005)	(0.006)	(0.15)	(0.08)	(0.12)
	[-0.005,0.008]	[-0.005,0.011]	[0.004,0.023]	[0.14,0.59]	[-0.11,0.16]	[0.19,0.57]
Best Possible Allocation	-0.018	-0.019	-0.075	-3.68	-1.61	-2.53
	(0.012)	(0.009)	(0.015)	(1.33)	(0.24)	(0.54)
	[-0.053,-0.015]	[-0.040,-0.011]	[-0.122,-0.071]	[-5.44,-1.26]	[-2.05,-1.25]	[-3.8,-2.01]

Table 5 Panel B: Employment Percent Changes (Inverse Hyperbolic Sine) and Short-Term Jobs-Per-Dollar

	IV (Generalized RF)	Lasso	OLS	IV (Generalized RF)	Lasso	OLS
		Unweighted		Rescaled by Jan 2020 Employment / Imputed Program Cost ($100000)		
	(1)	(2)	(3)	(4)	(5)	(6)
Average Treatment Effect Moments for:						
Recipients	0.385	0.251	0.264	7.43	4.74	4.97
	(0.135)	(0.058)	(0.046)	(2.6)	(1.31)	(1.09)
	[0.139,0.584]	[0.178,0.380]	[0.178,0.326]	[2.61,11.1]	[3.20,7.56]	[3.39,6.78]
Applicants	0.366	0.191	0.169	6.71	3.56	3.32
	(0.136)	(0.044)	(0.032)	(2.42)	(0.93)	(0.92)
	[0.112,0.552]	[0.145,0.289]	[0.115,0.215]	[2.16,9.91]	[2.53,5.73]	[2.02,5.24]
All Firms	0.355	0.174	0.124	6.2	3.1	2.66
	(0.135)	(0.041)	(0.028)	(2.26)	(0.77)	(0.77)
	[0.104,0.546]	[0.133,0.264]	[0.075,0.168]	[1.98,9.38]	[2.19,4.93]	[1.55,4.25]
Difference in treatment effects for recipients relative to:						
Random allocation among applicants	0.019	0.06	0.094	0.72	1.18	1.64
	(0.014)	(0.021)	(0.021)	(0.39)	(0.51)	(0.5)
	[-0.003,0.045]	[0.038,0.101]	[0.052,0.119]	[0.17,1.43]	[0.59,2.30]	[0.66,2.09]
Frontline Industry Prioritization	0.024	0.067	0.115	0.53	1.54	1.9
	(0.016)	(0.024)	(0.038)	(0.38)	(0.58)	(0.71)
	[0,0.053]	[0.041,0.113]	[0.044,0.163]	[-0.1,1.16]	[0.93,2.84]	[0.24,2.56]
Small Firm Prioritization	0.033	0.045	0.147	1.79	0.63	2.67
	(0.02)	(0.021)	(0.028)	(0.65)	(0.46)	(0.66)
	[0.001,0.066]	[0.023,0.086]	[0.096,0.182]	[0.77,2.87]	[0.06,1.51]	[1.4,3.44]
Best Possible Allocation	-0.068	-0.109	-0.255	-12.41	-5.22	-8.51
	(0.033)	(0.045)	(0.061)	(5.15)	(2.35)	(2.15)
	[-0.145,-0.032]	[-0.239,-0.093]	[-0.408,-0.217]	[-20.33,-4.26]	[-11.11,-4.00]	[-13.34,-6.29]