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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. Our model shows that delegation is optimal when delay is sufficiently costly, variation across firms in the impact of funds is small, and the alignment between public 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 social objectives are sufficiently aligned that delegation was likely superior to delaying loans to improve targeting.

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 social and private incentives. In early 2021, private hospitals were charged with administering vaccines, but some distributed

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[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.

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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 rate, 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.

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[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 whether delegation is optimal: 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. 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 probabilities on April 25, 2020. Steering is not. In other words, having a primary lender that was

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[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.

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 provide initial suggestive
evidence 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
these 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. However, covariate balance analysis suggests that any violations of the exclusion restriction
are likely to be minor.

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
characteristic. We find little statistically significant evidence of heterogeneous treatment effects.
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; treatment effects on long-run employment per $100,000 in lending were 5-10% 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 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 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, 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 optimal 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.[5] 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

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[5] 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).

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.[6] 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.[7] 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, or the merits of alternative designs of the PPP program. 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.

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[6] 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 earlier loans resulted in better short-run performance.

[7] One other paper, Granja, et al (2022), does 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 affectiveness 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.

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.[8] 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 each business was only allowed a single PPP loan 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

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[8] 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

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.

Delegation has the advantage of speed, which is valuable because firms may permanently fail if they do not receive aid quickly. However, delegation has the disadvantage that the service will be allocated based on the preferences of the private actor, not the government. Regulation is also fast, but we will assume that the public can do no more than mandate a single service level for all recipients. 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. 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

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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 $T = \int_\alpha x f(\alpha) d\alpha$. 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.[9]

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.[10] We assume that delayed

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[9] 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.

[10] 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.

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):

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 γ, θ, 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 $\gamma, \theta, \sigma_\xi^2$ 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 optimal. 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. 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). [11] The 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 16,914 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.

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.

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

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[11] 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 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.

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). 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. 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.

We supplement this novel data with administrative loan-level data from the SBA. This data is comprehensive, covering all loans made by PPP. 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.[12] 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 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 A1 shows that our borrowers are slightly smaller

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[12] 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.

than the typical SBA borrower, but we have reasonable coverage across most of the loan size distribution.[13] The exception is coverage of very large loans. Second, Figure A2 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. 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 A3 and A4 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 in our sample is similar to the median payroll imputed from 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?

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

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[13] 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.

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.[14]

VI.   Bank Steering versus Speed

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

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[14] 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.

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 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 increase in survival expectations, and we can reject that it is associated with a 1 percentage point increase 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, and the average proximity of workers to one another in the firm's zip code as a proxy for the impact COVID-19 would have on the area based on the industry distribution of firms. 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. This suggests that our results are not driven by the fact that our survey sample overweights the smallest firms eligible for PPP. Appendix Table

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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 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.

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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 unchanged when we add firm and bank characteristics to the regression. Many of these characteristics enter independently—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 characteristics do not alter the relationship between outcomes and the instrument.

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, which explain about 3 percent of the variation in the instrument when we include state fixed effects.

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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 tiny. Firm size, payroll, expenses, and zipcode characteristics 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).[15] 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. Zip code level covariates explain virtually none of the variation in the instrument.

Although 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. OLS estimates of survival expectations treatment effects from April of 2020 range from 0.09 to 0.15, while IV estimates are larger in magnitude but generally include the OLS estimates in the confidence interval. 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

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[15] 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.

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, defined as industries where face-to-face interaction is necessary for many workers. 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 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. That the average treatment effect for approved firms is larger 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. This cuts against the notion 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. 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), and whether the average proximity of workers to one another in the firm's zip code was high. None of the estimated interaction terms is statistically significant. 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 1.4 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 instrumental variables approach to understanding treatment effect heterogeneity for a number of 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.

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 public objective function. With this motivation, we explore gains and losses to alternative approaches to allocating PPP loans rather than the one chosen.

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.[16] Column (2) reports estimates from a Lasso procedure that uses raw PPP approval interacted with firm characteristics; this approach does not use instruments and instead assumes that PPP receipt is uncorrelated with treatment gains after conditioning on covariates.[17] For both 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, and two-digit industry dummies. We partial out state dummies to account for unequal timing in the spread of Covid across places.

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[16] 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.

[17] 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.

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, applicants, and all firms, with all three groups having mean survival expectation treatment effects of about 14.4 percent.

This low estimated treatment effect heterogeneity may reflect the high sampling variance of instrumental variables estimates. As a result of this variability, the cross-validation of the IV GRF parameters significantly regularize the estimates, reducing heterogeneity.[18] 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.

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. [19] Like our results in Table 4, this cuts against the idea that banks channeled funds

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[18] In Appendix Table A8, we take a more naïve approach and simply extend 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.
[19] 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.

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.

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 3A, we simply plot the cumulative distribution function (CDF) of estimated treatment effects for approved firms, applicants, and all firms. We plot the Lasso estimates to maximize the spread in treatment effects.

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.[20]

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 the IV-GRF estimates, the relatively homogenous treatment effects imply very small gains incremental gains by targeting the highest treatment effect firms. With Lasso, the best possible allocation does result in higher average treatment effects. The first tranche of PPP would have raised average survival expectations 1.9 percentage points more 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.[21]

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[20] These results are somewhat in tension with some work (Dalton, 2022; Doniger and Kay, 2020) that finds heterogeneity in treatment effects by firm size among firms with less than 100 employees. These papers use differential 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 w do. An important question for future research is understanding which of these factors is most important in explaining these differences.

[21] 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

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 survival and (ii) employees who lost their jobs due to firm failure did not find new jobs.[22] 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: 1.2-1.5 jobs saved per $100,000. The ex post best allocation, although likely infeasible, would have been substantially more cost effective: saving about 3 jobs per $100,000. Figure 3B shows the full CDFs of these treatment effects for approved firms, applicants, and all firms for the Lasso estimates. Panel B of Table 6 repeats these exercises with employment growth between January and April 25, 2020 as the dependent variable. The point estimates here are significantly larger because we focus on jobs saved in the very short run. However, the qualitative comparisons remain similar.

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.

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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.

[22] 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.

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 social 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), which studies 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 optimal. 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 optimal if the required delay were less than 4.4 weeks.[23] This is obviously a very rough estimate but gives a sense of what our results imply.[24]

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. Figure A5 shows the time series of disbursements for the EIDL Covid-19 program. Though the program opened at

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[23] 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.

[24] 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.

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.[25]

IX. Conclusion

Was it optimal 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, bank delegation was likely optimal 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.

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[25] However, the optimal delegation choice could have been different under different institutaionl 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.

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 optimal 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 optimality of delegation.

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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.
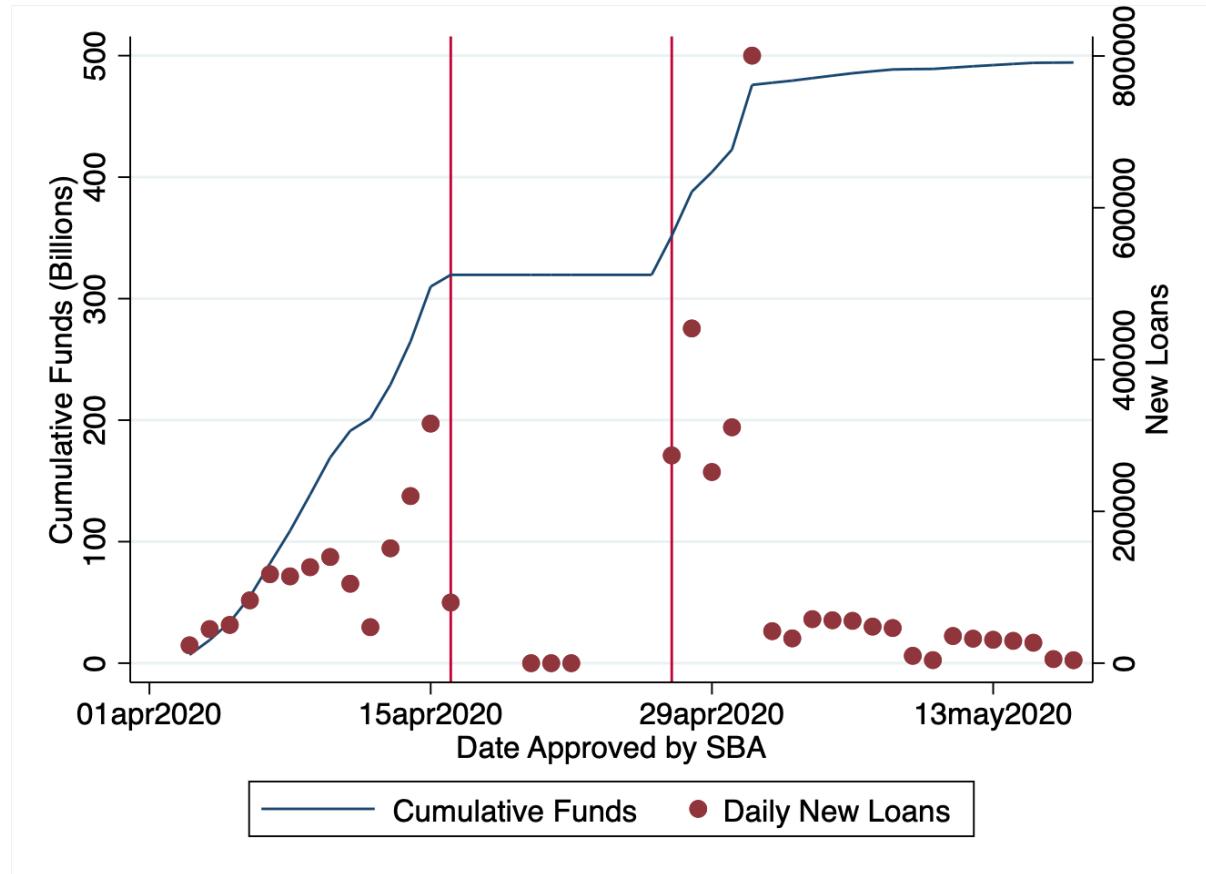
Figure 2. Fraction of respondents applying for PPP and application outcomes by respondent characteristics. This figure plots application and approval rates conditional on application by firm characteristics. The first set of characteristics is cash on hand. Respondents were asked "Consider the cash you 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.
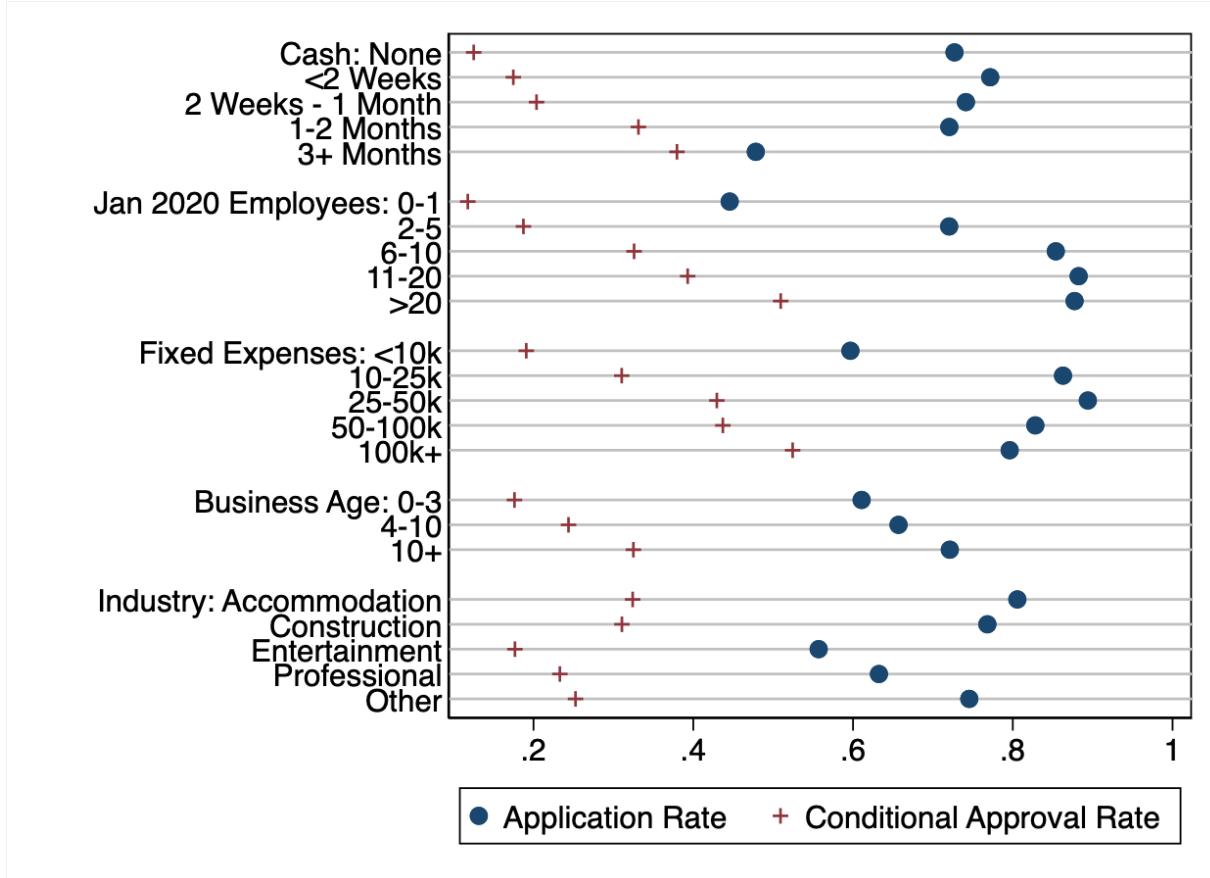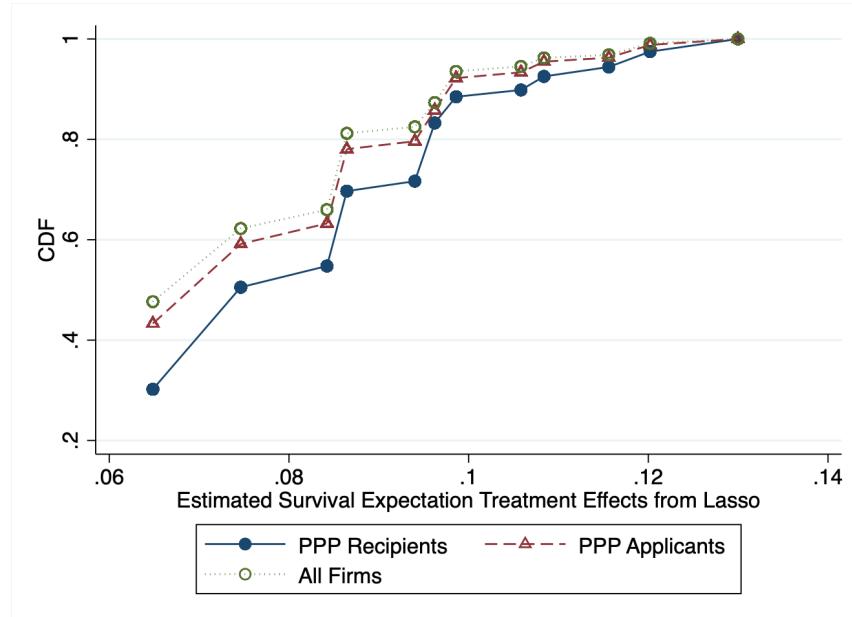
Figure 3. Estimated distributions of heterogeneous treatment effects on firm survival expectations. Treatment effects are estimated among applicants by fitting Lasso models of approval interacted with firm-level covariates, bank-relationship information, and industry characteristics. The possible interactions 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: Effects on Long-run Employment per Dollar of Program Cost.
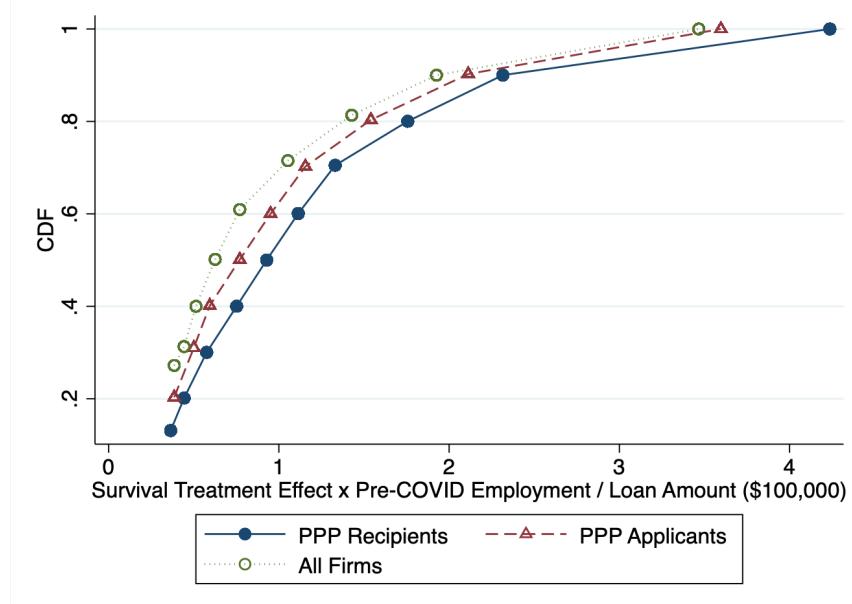
Table 1: Summary Statistics

This table reports summary statistics for the sample with comprehensive data coverage. 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 denial/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 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 for each respondents' bank in SBA administrative data. Bank Steering is the difference in average borrower headcount for tranche 1 loans and the first 21 days of tranche 2 loans. 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. 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 averaged at the 3-digit zipcode level based on the 2019 County Business Patterns. In later analyses, we fill in missing zipcode data with a missing dummy. Frontline industries include accommodation/food services, entertainment, construction, healthcare services, manufacturing, retail, wholesale trade, and transportation.

	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

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, and the average proximity of employees to one another in the zipcode based on the industry distribution in the County Business Patterns data. 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***	0.053***	0.030*	0.107***	0.069***	0.044*	0.122***
	(0.012)	(0.013)	(0.012)	(0.014)	(0.014)	(0.015)	(0.019)	(0.016)
Bank Steering Based on Jobs Saved/10	-0.000	-0.000	-0.001	-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***	0.064	0.072	0.094
	(0.033)				(0.034)	(0.039)	(0.045)	(0.049)
Bank Steering Based on Jobs Saved/10	0.019*				0.012	0.011	0.012	0.015
	(0.009)				(0.008)	(0.009)	(0.009)	(0.011)
R-Squared	0.01				0.06	0.10	0.10	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***	2.070***	1.589*	3.563***	3.052***	2.668***	4.534***
	(0.732)	(0.692)	(0.573)	(0.649)	(0.782)	(0.686)	(0.787)	(1.195)
Bank Steering Based on Jobs Saved/10	0.119	0.121	0.070	0.087	0.389*	0.284	0.287	0.656**
	(0.108)	(0.116)	(0.119)	(0.118)	(0.154)	(0.158)	(0.156)	(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***	0.192***	0.191***	0.287***	0.254***	0.265***	0.334***
	(0.036)	(0.044)	(0.049)	(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)	(0.012)	(0.011)	(0.013)	(0.014)	(0.014)	(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 FE	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 displayed regressors plus fixed effects for state, 2-digit naics code, business status, and missing zipcode. 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	0.45***	-0.10**	0.32***
	(0.02)	(0.04)	(0.04)	(0.03)
High Payroll	0.21***	0.13***	-0.15***	0.15***
	(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	-0.13	-0.05	0.19	-0.08
	(0.14)	(0.19)	(0.18)	(0.14)
R2	0.16	0.21	0.08	0.18
N	6640	4452	4452	6640

Table 4: IV Estimates of Heterogeneous Treatment Effects

This table relates firm outcomes to PPP approval as of April 25, 2020 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 fixed effects for industry, state, business status as-of the survey, and each of the dimensions of baseline heterogeneity 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.

	Panel A: Survival Expectations							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Z=	Frontline Industry	High Cash	Loan	Officer	Payroll	Fixed Exp	B2B	Emp Proximity
PPP approved x high Z	0.04	-0.08	-0.04	-0.05	0.01	-0.04	0.07	-0.06
	(0.05)	(0.06)	(0.05)	(0.04)	(0.06)	(0.05)	(0.07)	(0.03)
PPP approved	0.11**	0.18***	0.16***	0.15***	0.13*	0.15***	0.11**	0.16***
	(0.05)	(0.07)	(0.05)	(0.04)	(0.07)	(0.05)	(0.04)	(0.04)
Approved Treatment Effects (TEs)	0.134	0.133	0.133	0.131	0.131	0.127	0.131	0.139
All Applicant TEs	0.132	0.147	0.136	0.138	0.130	0.134	0.133	0.141
Non-Applicant TEs	0.129	0.135	0.140	0.143	0.127	0.144	0.136	0.139

PPP approved x high Z	0.03	0.11	0.02	-0.20	0.01	0.01	0.00	0.08
	(0.12)	(0.13)	(0.15)	(0.13)	(0.13)	(0.12)	(0.11)	(0.07)
PPP approved	0.07	0.03	0.08	0.17	0.08	0.08	0.09	0.05
	(0.10)	(0.11)	(0.15)	(0.11)	(0.13)	(0.11)	(0.08)	(0.08)
Approved Treatment Effects (TEs)	0.092	0.092	0.090	0.088	0.091	0.092	0.090	0.087
All Applicant TEs	0.091	0.073	0.088	0.115	0.088	0.089	0.090	0.086
Non-Applicant TEs	0.088	0.090	0.087	0.138	0.084	0.086	0.091	0.087

Panel C: Employment as of April 25, 2020

PPP approved x high Z	-1.10	-0.90	0.38	0.39	7.56***	6.84**	-5.45**	-1.01
	(2.16)	(3.09)	(2.07)	(3.02)	(2.24)	(3.13)	(2.14)	(1.72)
PPP approved	6.39***	6.40**	5.60**	5.65**	0.64	3.02**	7.41***	6.52***
	(2.27)	(3.11)	(2.38)	(2.73)	(1.59)	(1.37)	(2.13)	(2.14)
Approved Treatment Effects (TEs)	5.73	5.82	5.79	5.80	5.77	6.50	5.80	6.08
All Applicant TEs	5.77	5.99	5.76	5.75	4.23	5.46	5.69	6.11
Non-Applicant TEs	5.88	5.84	5.73	5.70	1.99	3.81	5.49	6.09

Panel D: Employment Pct Changes (Inv. Hyperbolic Sine)

PPP approved x high Z	-0.04	-0.18	0.22	0.41**	0.17	0.18	-0.15	0.08
	(0.15)	(0.36)	(0.22)	(0.18)	(0.17)	(0.24)	(0.20)	(0.12)
PPP approved	0.42**	0.52	0.28	0.23	0.29	0.33**	0.45**	0.37*
	(0.17)	(0.39)	(0.28)	(0.23)	(0.25)	(0.14)	(0.22)	(0.20)
Approved Treatment Effects (TEs)	0.397	0.405	0.392	0.402	0.399	0.418	0.400	0.400
All Applicant TEs	0.399	0.437	0.376	0.348	0.365	0.390	0.397	0.398
Non-Applicant TEs	0.403	0.407	0.356	0.300	0.315	0.347	0.391	0.400

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 or Lasso regressions fit on the sample of applicants. In the Lasso, 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, 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 3 and 4 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 A8, which reports similar patterns to this exercise.

	IV GRF	Lasso OLS	IV GRF	Lasso OLS
	Unweighted		Employment / Imputed Program Cost ($100000)	
	(1)	(2)	(3)	(4)
Panel A: Survival Expectations and End-of-Year (Longer-Run) Jobs-Per-Dollar				
Average Treatment Effect Moments for:				
Recipients	0.1437	0.083	2.63	1.47
	(0.0039)	(0.009)	(0.07)	(0.18)
Applicants	0.1441	0.079	2.46	1.30
	(0.0120)	(0.01)	(0.10)	(0.17)
All Firms	0.1439	0.076	2.31	1.19
	(0.0105)	(0.01)	(0.09)	(0.16)
Difference in treatment effects for recipients relative to:				
Random allocation among applicants	0.0004	0.004	0.32	0.17
	(0.0093)	(0.003)	(0.12)	(0.07)
Frontline Industry Prioritization	0.0003	0.004	0.10	0.24
	(0.0123)	(0.003)	(0.09)	(0.07)
Small Firm Prioritization	0.0002	-0.001	0.49	0.01
	(0.0098)	(0.005)	(0.13)	(0.08)
Best Possible Allocation	-0.0017	-0.019	-4.13	-1.61
	(0.0977)	(0.009)	(0.69)	(0.24)

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

Average Treatment Effect Moments for:

Recipients	0.453	0.251	9.34	4.74
	(0.014)	(0.058)	(0.52)	(1.31)
Applicants	0.408	0.191	8.13	3.56
	(0.008)	(0.044)	(0.36)	(0.93)
All Firms	0.366	0.174	7.13	3.1
	(0.013)	(0.041)	(0.24)	(0.77)

Difference in treatment effects for recipients relative to:

Random allocation among applicants	0.087	0.06	2.27	1.18
	(0.022)	(0.021)	(0.33)	(0.51)
Frontline Industry Prioritization	0.053	0.067	1.46	1.54
	(0.015)	(0.024)	(0.17)	(0.58)
Small Firm Prioritization	0.093	0.045	3.03	0.63
	(0.024)	(0.021)	(0.41)	(0.46)
Best Possible Allocation	-0.312	-0.109	-17.15	-5.22
	(0.122)	(0.045)	(0.199)	(2.35)

Internet Appendix for When should public programs be privately administered?

APPENDIX A: Additional Model Results and Proofs of Propositions

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

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

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

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

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

Welfare is higher with public delay than with private flexible allocation if and only if

$$\delta^{1-\gamma}T^\gamma e^{\frac{(1-\gamma(1-\theta)^2)\sigma_\alpha^2-\gamma\sigma_\zeta^2}{2(1-\gamma)}} > T^\gamma e^{\frac{(1-\gamma(1-\phi)^2)\sigma_\alpha^2-\gamma\sigma_\zeta^2}{2(1-\gamma)}} \text{ or } \delta > e^{\gamma\frac{[(1-\theta)^2-(1-\phi)^2]\sigma_\alpha^2+\sigma_\zeta^2-\sigma_\xi^2}{2(1-\gamma)^2}}.$$ This condition

clearly does not hold when $\delta = 0$ and must hold when $\delta = 1$ as we have assumed that

$[(1-\phi)^2 - (1-\theta)^2]\sigma_\alpha^2 + \sigma_\xi^2 - \sigma_\zeta^2 > 0$. As the left hand side is monotonic and continuous in

δ, there must exist a value of a firm survival rate, denoted δ^* between zero and 1, for which public

welfare with immediate bank lending is equal to the public welfare with delayed targeting.

Delay and public discretion yields greater welfare than fixed loans if and only if

$$\delta^{1-\gamma}T^\gamma e^{\frac{(1-\gamma(1-\theta)^2)\sigma_\alpha^2-\gamma\sigma_\zeta^2}{2(1-\gamma)}} > T^\gamma e^{\frac{\sigma_\alpha^2}{2}} \text{ or } \delta > e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}}.$$ The function $e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}}$ is greater

than $e^{\gamma\frac{[(1-\theta)^2-(1-\phi)^2]\sigma_\alpha^2+\sigma_\zeta^2-\sigma_\xi^2}{2(1-\gamma)^2}}$ if and only if $\frac{\sigma_\xi^2}{\sigma_\alpha^2} > 2\phi - \phi^2$. Our assumptions ensure that

$[(1-\phi)^2 - (1-\theta)^2]\sigma_\alpha^2 + \sigma_\xi^2 - \sigma_\zeta^2 > 0$, $-(\theta^2-2\theta)\sigma_\alpha^2 - \sigma_\zeta^2 > 0$ and $e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}} < 1$ and

hence there always there for δ close enough to one, public control dominates immediate fixed

allocations, and for δ close to zero, fixed allocations dominate public delay. In this region, public

delay provides higher welfare that immediate lending if and only if $\delta > \delta^*$, where $\delta^* =$

$e^{\gamma\frac{(\theta^2-2\theta)\sigma_\alpha^2+\sigma_\zeta^2}{2(1-\gamma)^2}}$.

Consequently, for all values of ϕ, there exists a value of δ denoted δ^*, such that delayed

public allocation dominates either alternative if and only if $\delta > \delta^*$. If $\phi < 1 - \sqrt{1 - \frac{\sigma_\xi^2}{\sigma_\alpha^2}}$, then δ^*

is falling with γ, θ, and σ_α^2, rising with σ_ζ^2 and independent of σ_ξ^2 and ϕ, and if $\delta < \delta^*$, then

providing loans of fixed size generates higher social welfare than the two other alternatives. If

$\phi > 1 - \sqrt{1 - \frac{\sigma_\xi^2}{\sigma_\alpha^2}}$, then δ^* is falling with $\gamma, \theta, \sigma_\xi^2$ and σ_α^2, and rising with ϕ and σ_ζ^2, and if $\delta < \delta^*$,

then allowing private providers to allocate loans flexibly generates higher social welfare than either

delayed public allocated or fixed loans sizes.

In the paper, we have either allowed total flexibility or a low fixed loan size, but neither of

those assumptions fits perfectly with the implementation of the PPP in April 2020. There was a

cap on loan size, but many loans came in below that cap. We now compare loans that are fixed in

size at *T*, with loans that are fixed in size at *T'>T*. We continue to hold the total amount of funds fixed at *T*, so that banks can allocate more financing to the firms that they favor, but these larger loans cannot be distributed to the full measure 1 of firms. This proposition formally analyzes the recommendation of Hanson et al. (2020) that more smaller loans may be more advantageous than fewer larger loans. We now assume that $\beta = \phi\alpha + \vartheta\xi$, where $\vartheta = \sqrt{(\sigma_\beta^2 - \phi^2\sigma_\alpha^2)/\sigma_\xi^2}$. This assumption allows us to vary the correlation between bank preferences and social preference (ϕ), without varying the variance of β.

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

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

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

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

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

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

As T' determines $\hat{\beta}$ exactly, we will think of the social planner as choosing $\hat{\beta}$ rather than T' for mathematical convenience. Social welfare from lending equals

$$\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} T^\gamma \int_{\beta>\hat{\beta}} E(e^\alpha|\beta)\, g(\beta) d\beta \text{ where } \qquad E(e^\alpha|\beta) = \frac{\int_\alpha e^\alpha h\left(\frac{\beta-\phi\alpha}{\vartheta}\right) f(\alpha) d\alpha}{\int_\alpha h\left(\frac{\beta-\phi\alpha}{\vartheta}\right) f(\alpha) d\alpha} =$$

$$\frac{\int_\alpha e^\alpha e^{-\frac{(\beta-\phi\alpha)^2}{2\vartheta^2\sigma_\xi^2}} e^{-\frac{\alpha^2}{2\sigma_\alpha^2}} d\alpha}{\int_\alpha e^{-\frac{(\beta-\phi\alpha)^2}{2\vartheta^2\sigma_\xi^2}} e^{-\frac{\alpha^2}{2\sigma_\alpha^2}} d\alpha} = e^{\frac{2\beta\phi\sigma_\alpha^2 + \vartheta^2\sigma_\xi^2\sigma_\alpha^2}{2\sigma_\beta^2}}$$

Hence the overall objective function is $e^{\frac{\sigma_\alpha^2}{2}} T^\gamma$ times

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

Welfare when everyone gets T equals $e^{\frac{\sigma_\alpha^2}{2}} T^\gamma$. Welfare when selected individuals receive T'>T,

equals $T^\gamma e^{\frac{\sigma_\alpha^2}{2}}$ times $\left(\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta\right)^{-\gamma} \int_{\beta>\hat{\beta}} e^{\frac{-(\beta-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta = V(\hat{\beta};Z)$, were Z is a vector of

exogenous variables. We also know that $\int_{\beta>\hat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta = \frac{T}{T'}$, and that (using a simple change of

variable so that $x = \beta - \phi\sigma_\alpha^2$, we have $\int_{\beta>\hat{\beta}} e^{\frac{-(\beta-\phi\sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta = \int_{x>\hat{\beta}-\phi\sigma_\alpha^2} e^{\frac{-x^2}{2\sigma_\beta^2}} dx$.

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

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

If $\phi < 0$, then $\int_{x > \widehat{\beta} - \phi \sigma_\alpha^2} e^{\frac{-x^2}{2\sigma_\beta^2}} dx < \int_{\beta > \widehat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta = \frac{T}{T'}$ and so $V(\widehat{\beta}; Z) < \left(\frac{T}{T'}\right)^{1-\gamma} \leq 1$,

whenever T'>T and $1 > \gamma$. Consequently, it is never welfare enhancing to let T'>T if $\phi \leq 0$.

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

$$\gamma e^{\frac{-\widehat{\beta}^2}{2\sigma_\beta^2}} \left(\int_{\beta > \widehat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta \right)^{-\gamma - 1} \int_{\beta > \widehat{\beta} - \phi \sigma_\alpha^2} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta - \left(\int_{\beta > \widehat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta \right)^{-\gamma} e^{\frac{-(\widehat{\beta} - \phi \sigma_\alpha^2)^2}{2\sigma_\beta^2}} \text{, which is positive if}$$

and only if $\gamma > \dfrac{e^{\frac{2\phi \sigma_\alpha^2 \widehat{\beta} - \phi^2 \sigma_\alpha^4}{2\sigma_\beta^2}} \int_{\beta > \widehat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta}{\int_{\beta > \widehat{\beta} - \phi \sigma_\alpha^2} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta}$. If $\phi > 0$, then as $\widehat{\beta}$ goes to negative infinity (which

corresponds to T=T'), the right hand side of the equation goes to zero, and consequently, increasing T' above T is optimal.

The derivative of $V(\widehat{\beta}; Z)$ with respect to γ is

$$-\left(\int_{\beta > \widehat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta \right)^{-\gamma} \int_{\beta > \widehat{\beta}} e^{\frac{-(\beta - \phi \sigma_\alpha^2)^2}{2\sigma_\beta^2}} d\beta \, ln \left(\int_{\beta > \widehat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta \right) > 0, \text{ and so that if some value of T'>T}$$

yields the same welfare as T for any value of γ, then for all values of $\gamma' > \gamma$, an allocation of T' will yield higher welfare than T.

If the variance of β is independent of ϕ, then the derivative of $V(\widehat{\beta}; Z)$ with respect to ϕ

(holding σ_β^2 constant) is positive, and given by $\sigma_\alpha^2 e^{\frac{-\widehat{\beta}^2 + 2\phi \sigma_\alpha^2 \widehat{\beta} - \phi^2 \sigma_\alpha^4}{2\left(\phi^2 \sigma_\alpha^2 + \sigma_\xi^2\right)}} \left(\int_{\beta > \widehat{\beta}} e^{\frac{-\beta^2}{2\sigma_\beta^2}} d\beta \right)^{-\gamma} > 0$.

Consequently, if some value of T'>T yields the same welfare as T for any value of ϕ, then for all values of $\phi' > \phi$ (holding σ_β^2 constant), T' will yield higher values of $V(\widehat{\beta}; Z)$.

APPENDIX B: Details about Machine Learning Approaches

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

The first approach to estimating firm-level treatment effect heterogeneity that we use in Table 5 is the GRF-IV approach from Athey, Tibshirani, and Wagner (2019). We cross-validate the key GRF parameters, including the number of variables tried in each split and the minimum node size. We 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. 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, and two-digit industry dummies. We incorporate dummy variables into the GRF model by calculating the average value of the outcome variable for each value of the dummy variable.

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

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

where X_i' is a vector of characteristics for firm i. The estimated treatment effect for firm i is then $\widehat{\tau(X_i)} = y\_1(\widehat{X_i | D_i} = 1) - y\_1(\widehat{X_i | D_i} = 0)$. In the framework of Kunzel, et al (2019), this is an "S-Learner" where outcomes are predicted as a function of treatment, covariates, and treatment

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

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

A drawback to S-learners is that, if the treatment variable is weak relative to other variables in explaining outcomes then the treatment variable may not be selected as a predictor, possibly biasing treatment effects towards zero. To understand the extent to which this concern is a problem, we explore alternative meta-learners that treat the treatment variable differently from other covariates, potentially alleviating this problem. Columns (4) through (13) report results for alternative meta-learners, reporting results using both LASSO and random forests as the base-learner for each meta-learner.[1] Specifically, Columns (4) through (6) report results with a T-learner adopted from Foster (2013) and Athey and Imbens (2016) where outcomes are separately predicted conditional on covariates for treatment and control and then the difference in predicted outcomes at a particular covariate value is the predicted treatment effect. Columns (7) and (9) report results using X-learners, which were proposed by Kunzel, et al (2019) and separately estimate treatment effects within treated and control to address situations where potential outcomes are non-linear and treatment and control are very unbalanced in size. Columns (10) and (12) report results using an R-learner developed by Nie and Wager (2021), which takes advantage of the Robinson (1988) decomposition of treatment effects in terms of outcomes residuals and treatment residuals to produce a treatment effect estimator that is consistent even in high dimensional settings and attains uniformly valid inference. Finally, Column (13) reports results from the Causal Forest approach in Wager and Athey (2018).[2]

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[1] In our random forest models, we use a maximum tree depth of three and fit 100 trees.

[2] Specifically, these different meta-learners are implemented as follows. Define $\mu_1(x) = E[Y\_i|X_i = x, D_i = 1]$ and $\mu_0(x) = E[Y_i| X_i = x, D_i = 0]$. In a T-learner, we first separately estimate $\widehat{\mu_1(X_i)}$ and $\widehat{\mu_0(X_i)}$ using the base-learner of choice, then we estimate the treatment effect as $\widehat{\tau(X_i)} = \widehat{\mu_1(X_i)} - \widehat{\mu_0(X_i)}$. Implementing an X-learner, starts in the same way by estimating $\widehat{\mu_1(X_i)}$ and $\widehat{\mu_0(X_i)}$. Then we compute $\widehat{\tau_1(X_i)} = Y_i - \widehat{\mu_0(X_i)}$ for observations where $D_i = 1$ and $\widehat{\tau_0(X_i)} = \widehat{\mu_1(X_i)} - Y_i$ for observations where $D_i = 0$. We then estimate the treatment effect for firm i as $\widehat{\tau(X_i)} = g(X_i)\widehat{\tau_1(X_i)} + (1 - g(X_i))\widehat{\tau_0(X_i)}$ where $g(X_i)$ is some weighting function, often the propensity score $\widehat{e(X_i)}$. The R-learner was proposed by Nie and Wager (2021). Let $\widehat{m(X_i)} = E[Y_i|X\_i]$ and $\widehat{e(X_i)} = E[D\_i|X\_i]$. Then we take advantage of the decomposition from Robinson (1998), which shows that $Y_i - m(X_i) = [D_i - e(X_i)]\tau(X_i) + \epsilon_i$. We estimate these conditional expectation functions, $\widehat{m(X_i)}$ and $\widehat{e(X_i)}$, using the base-

The results in Appendix Table 9 are qualitatively consistent with our estimates in Table 5 across all of the alternative specifications. In every empirical approach, the bank allocation has higher average treatment effects than a random allocation would have and, in most cases, the improvement is meaningful in magnitude, leading to an increase in jobs saved per \$100,000 of over 1.5 in the short-run and over 0.2 in the long-run. Targeting small firms similarly performs worse than the observed allocation across all specifications, while targeting based on high frontline worker share also generally performs worse than observed bank targeting, though the magnitude of the difference is smaller than random targeting or small firm targeting. Assuming it were feasible, perfect targeting would cause more substantial gains in long-run average treatment effects, ranging from 150 to 400 percent. However, as we discuss in more detail in the main text, perfect targeting was likely infeasible. Furthermore, within-sample estimates of gains from alternative targeting regimes may tend to overstate potential gains from targeting.

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learner of choice. Using these estimates, we can then estimate the treatment effect by solving $\widehat{\tau()} =$

$argmin_{\tau} \left\{ \sum_{i=1}^{n} \left(\left[Y_i - \widehat{m^{-i}(X_i)} \right] - \left[D_i - \widehat{e^{-i}(X_i)} \right] \tau(X_i) \right)^2 + \Lambda_n(\tau(.)) \right\} \right\}$ where $\Lambda\_n(\tau(.))$ is a penalty on the complexity of the treatment effect heterogeneity function. The superscripts τ^{-i} indicate that we use cross-fitting to estimate the nuisance functions $\widehat{m(X_i)}$ and $\widehat{e(X_i)}$. Causal forests are an approach developed by Athey and Imbens (2016) that modifies random forests to minimize error in estimated treatment effects rather than error in predicting the outcome. Specifically, partitions of the covariate space are chosen to minimize within partition error in average treatment effects.

Appendix C: Survey Instrument

What impact are you currently experiencing from the Coronavirus Outbreak?

○ It's not impacting my business

○ It's starting to impact my business

○ It's really impacting my business

○ The impact is on the decline

○ The impact is over

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

○ Approved, and I have received the funds

○ Approved, but I have not yet received the funds

○ Application is pending

○ Application was denied

○ I tried to apply but was unable to submit an application

○ I did not apply

When did you first apply for a loan?

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

- ☐ Insufficient documentation

- ☐ Did not meet federal qualification criteria

- ☐ Did not apply in time to receive funds

- ☐ Not a priority customer

- ☐ I received a different reason (not listed here)

- ☐ I did not receive a reason

How much assistance did you receive?

○ Less than $10k

○ Between $10-25k

○ Between $25-50k

○ Between $50-75k

○ Between $75-100k

○ Between $100-150k

○ Between $150-300k

○ Between $300-500k

○ Between $500k-$1 million

○ $1 million - $2 million

○ $2 million - $3 million

○ $3 million - $4 million

○ $4 million - $5 million

○ $5 million - $6 million

○ $6 million - $7 million

○ $7 million - $8 million

○ $8 million - $9 million

○ $9 million - $10 million

○ $10 million - $20 million

○ More than $20 million

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

- ☐ I can remain operational without extra cash

- ☐ I've already taken out a business loan and don't want to take on any more loans

- ☐ I don't want to deal with the hassle of applying

- ☐ I don't think I would receive the money in time for it to help my business

- ☐ I don't feel confident I can maintain my payroll for the loan to be forgiven

- ☐ I don't trust that the government will forgive my loan even if I maintain my payroll

- ☐ I don't trust that my bank will forgive my loan even if I maintain my payroll

- ☐ I don't believe I qualify for this loan (credit history, size of business, etc.)

- ☐ I don't trust that the COVID-19 disruptions will be over soon enough for my business to recover so I can maintain my payroll or pay back the loan

- ☐ I'm confused about the terms of the loan

- ☐ I would prefer other assistance that does not risk going into debt and being unable to pay it back

- ☐ I've applied for a loan before and was denied

- ☐ Closure is inevitable, even with the cash

- ☐ Other, please specify: _____

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

_____ Full-Time employees
_____ Part-Time / Temporary employees

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

○ Extremely Likely

○ Very Likely

○ Somewhat Likely

○ Somewhat Unlikely

○ Extremely Unlikely

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

○ 0% Extremely Unlikely

○ 10%

○ 20%

○ 30%

○ 40%

○ 50%

○ 60%

○ 70%

○ 80%

○ 90%

○ 100% Extremely Likely

Is your business open?

○ Yes, it is currently open.

○ No, it is temporarily closed due to COVID-19, but I intend to reopen.

○ No, it is temporarily closed for other reasons, but I intend to reopen.

○ No, it is permanently closed due to COVID-19.

○ No, it is permanently closed for other reasons.

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

_____ Full-Time employees
_____ Part-Time / Temporary employees

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

○ Less than $10k

○ Between $10-25k

○ Between $25-50k

○ Between $50-75k

○ Between $75-100k

○ Between $100-150k

○ Between $150-300k

○ Between $300-500k

○ Between $500k-$1 million

○ $1 million - $2 million

○ $2 million - $3 million

○ $3 million - $4 million

○ $4 million - $5 million

○ $5 million - $6 million

○ $6 million - $7 million

○ $7 million - $8 million

○ $8 million - $9 million

○ $9 million - $10 million

○ More than $10 million

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

○ Less than $10k

○ Between $10-25k

○ Between $25-50k

○ Between $50-75k

○ Between $75-100k

○ Between $100-150k

○ Between $150-300k

○ Between $300-500k

○ Between $500k-$1 million

○ $1 million - $2 million

○ $2 million - $3 million

○ $3 million - $4 million

○ $4 million - $5 million

○ $5 million - $6 million

○ $6 million - $7 million

○ $7 million - $8 million

○ $8 million - $9 million

○ $9 million - $10 million

○ More than $10 million

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

○ Already gone

○ Less than 2 weeks

○ 2 weeks to 1 months

○ 1 to 2 months

○ 3 months or more

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

- ☐ Reduced Pay Rates (per person)

- ☐ Reduced Rent Payments

- ☐ Reduced Loan Payments

- ☐ Reduced Mortgage Payments

- ☐ None of the above

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

_____

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

○ 0

○ 1

○ 2

○ 3

○ 4

○ 5

○ 6

○ 7

○ 8

○ 9

○ 10

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

- [ ] I had a loan or credit card from the bank

- [ ] I had a business bank account

- [ ] I used the bank for services other than loans or a bank account

- [ ] I had a relationship with a banker or loan officer

- [ ] None of the above

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

○ Less than $10k

○ Between $10-25k

○ Between $25-50k

○ Between $50-75k

○ Between $75-100k

○ Between $100-150k

○ Between $150-300k

○ Between $300-500k

○ Between $500k-$1 million

○ $1 million - $2 million

○ $2 million - $3 million

○ $3 million - $4 million

○ $4 million - $5 million

○ $5 million - $6 million

○ $6 million - $7 million

○ $7 million - $8 million

○ $8 million - $9 million

○ $9 million - $10 million

○ More than $10 million

What is your main industry?

○ Agriculture, Forestry, Fishing and Hunting

○ Mining, Quarrying, and Oil and Gas Extraction

○ Utilities

○ Construction

○ Manufacturing

○ Wholesale Trade

○ Retail Trade

○ Transportation and Warehousing

○ Information

○ Finance and Insurance

○ Real Estate and Rental and Leasing

○ Professional, Scientific, and Technical Services

○ Management of Companies and Enterprises

○ Administrative Support or Waste Remediation Services

○ Educational Services

○ Health Care and Social Assistance

○ Arts, Entertainment, and Recreation

○ Accommodation and Food Services

○ Other Services (except Public Administration)

○ Public Administration

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

○ Extremely Likely

○ Very Likely

○ Somewhat Likely

○ Somewhat Unlikely

○ Extremely Unlikely

Appendix D. Implied Excess Death Rates

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

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

Define the treatment effect of PPP on eligible firms as

$$\tau = E\big[death \text{ without } PPP\,|\,Covid, Eligible\big] -$$
$$E\big[death \text{ with } PPP\,|\,Covid, Eligible\big].$$

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

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

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

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

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

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

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

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

Appendix Figures and Tables

Figure A1: Loan Sizes in the SBA Data and the Survey from Tranche 1

Notes: Figure displays a cdf of loan sizes, which is right-continuous, meaning 80% of the survey loans are under $575,000 and 74% of the SBA loans in tranche 1 are under $575,000.
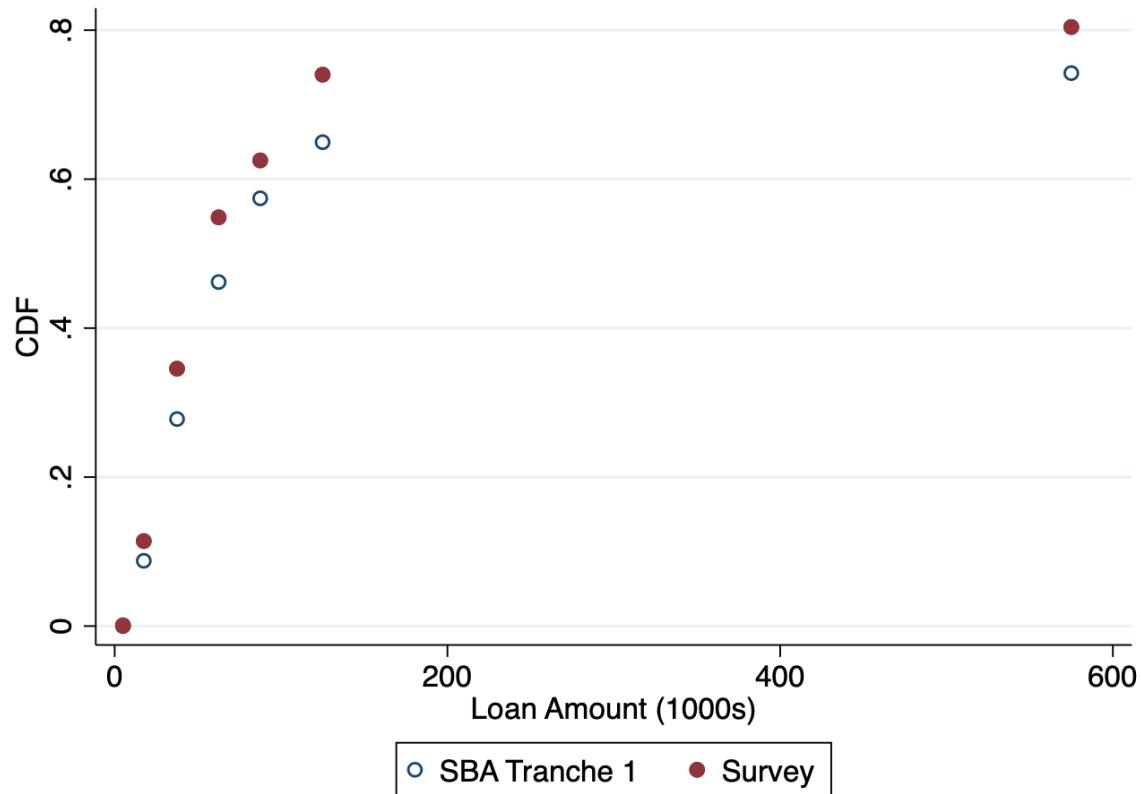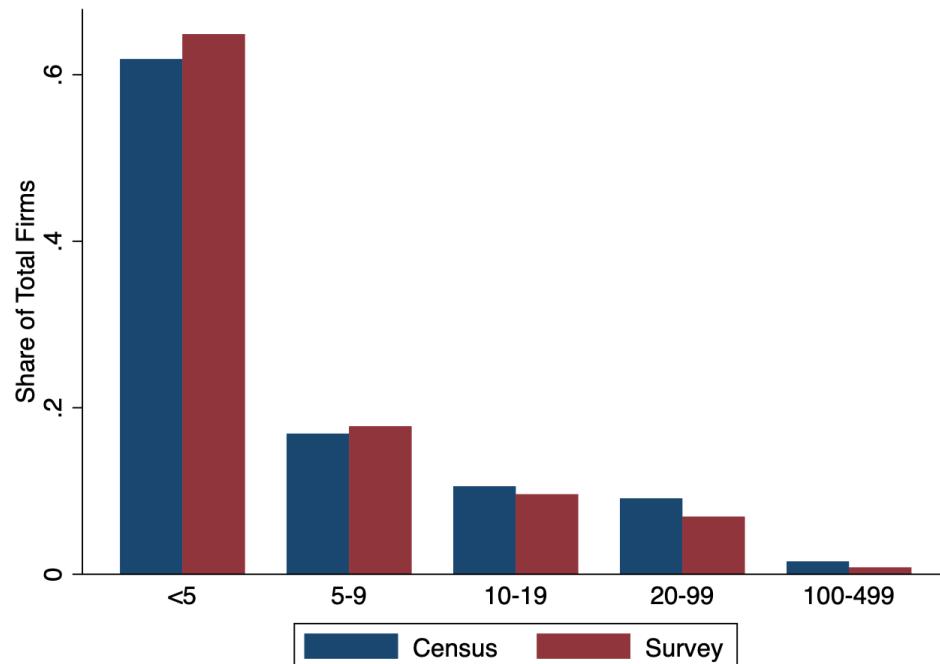
Figure A2: Comparison of Survey and 2017 Census of Business Firms

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

Panel B: Top 10 States
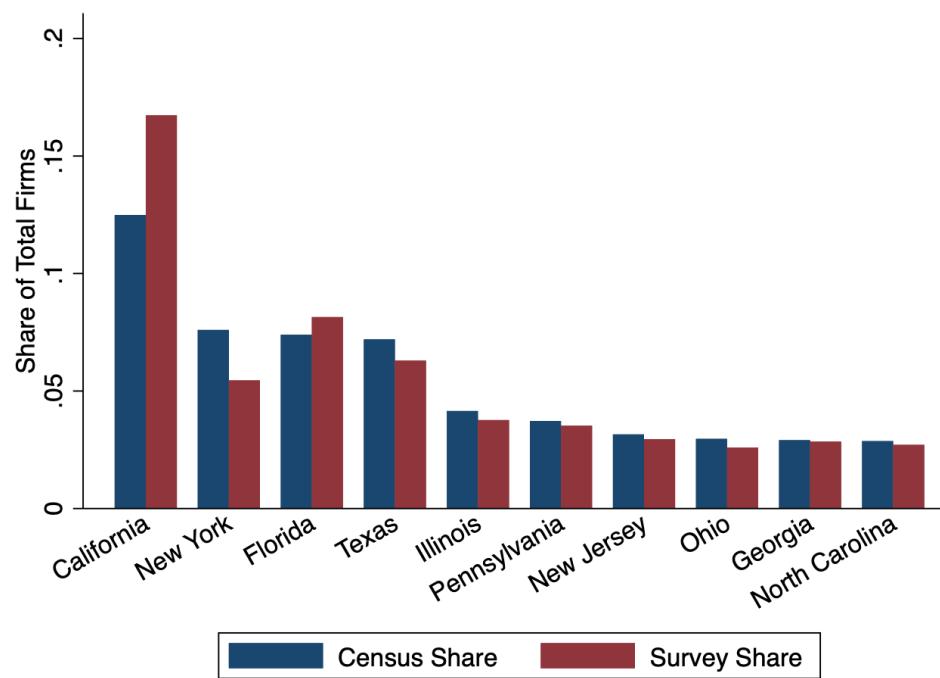
Figure A3: Representativeness of Survey as Compared to Census Pulse

This figure displays national-level Census Pulse estimates and Alignable survey estimates (using the main estimation sample) for the share of firms that apply to the PPP program and the share of firms that receive PPP loans. Note that we use the earliest Census Pulse, but it will have responses for firms that received loans in tranche 2 (which began 4/27/2020), whereas 90% of our sample was collected by 4/27/2020. This means that the Census Pulse should have a higher approval/receipt rate than our sample. In addition, note that the Pulse survey was also a non-representative sample, but was drawn from a different universe. The sampling frame was based on the census having an email address for the owner. As a result, the pulse under-represents firms with under 4 employees, as described here:
https://portal.census.gov/pulse/data/downloads/research/2021_sbps_nonresponse_bias_analysis_final.pdf
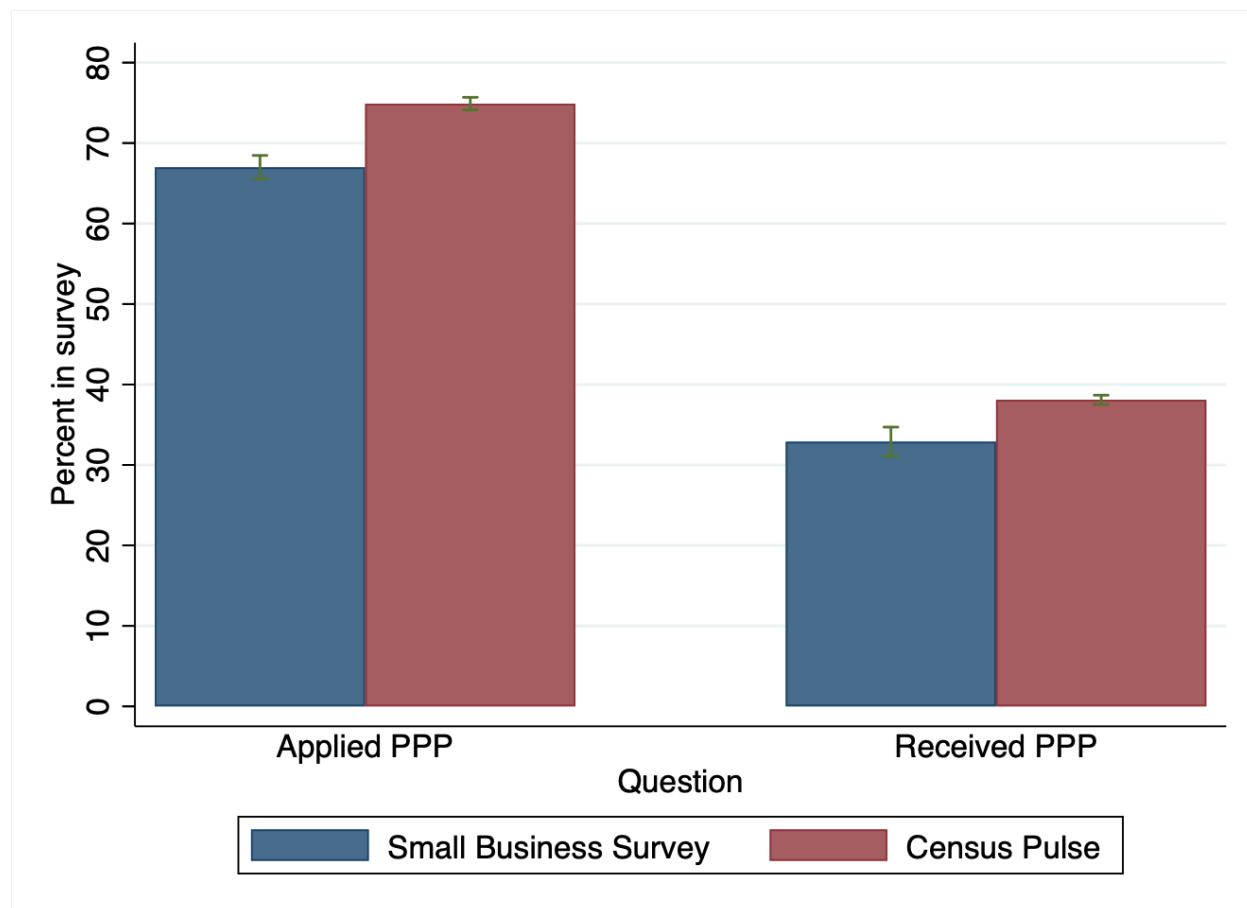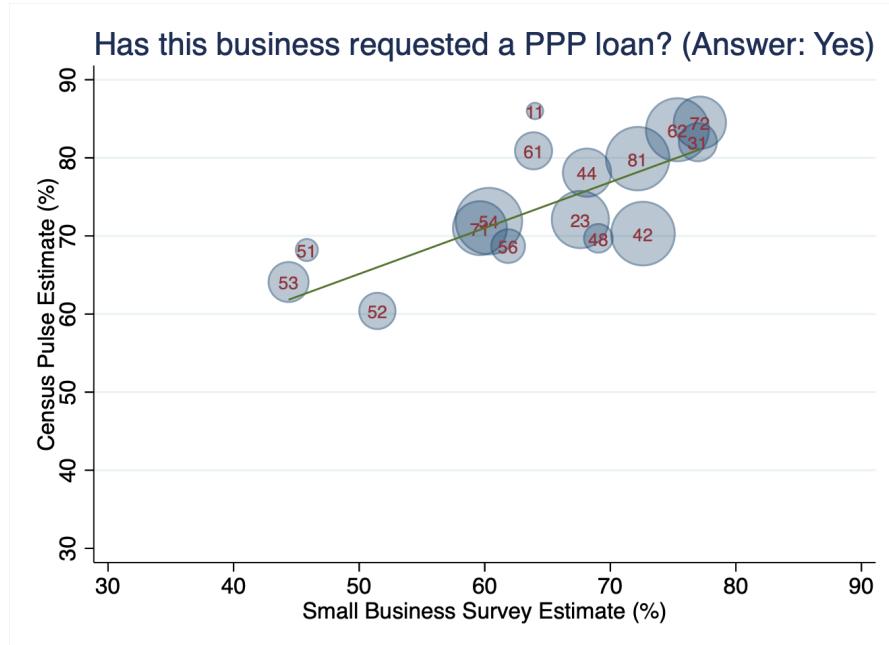
Figure A4: Comparison of Census Pulse and Alignable Survey Across Industries

Notes: Figure excludes mining and utilities. Line of best fit is weighted by the number of businesses in the Alignable sample, as indicated by marker size. Single-person firms excluded.

Panel A: Applied for / requested PPP

Panel B: Received PPP
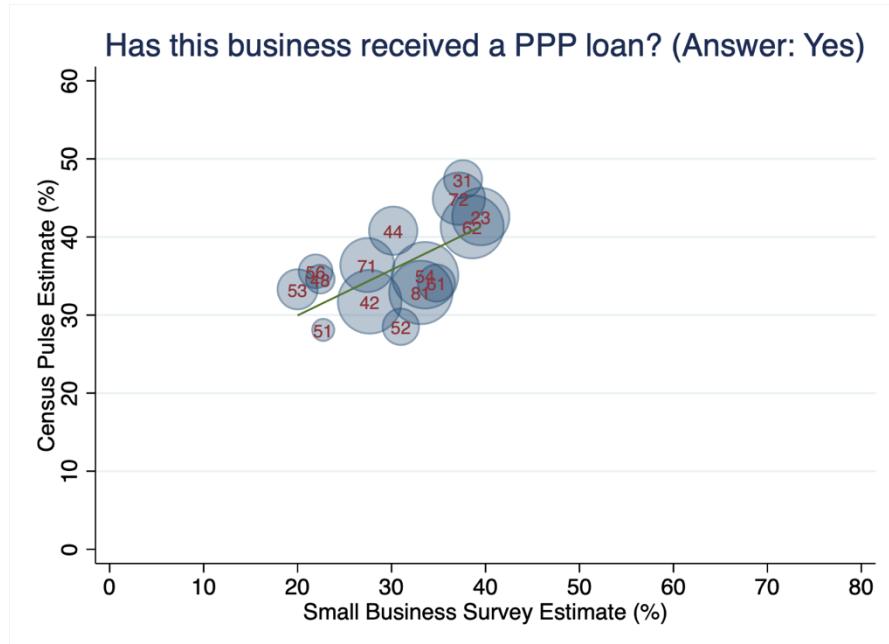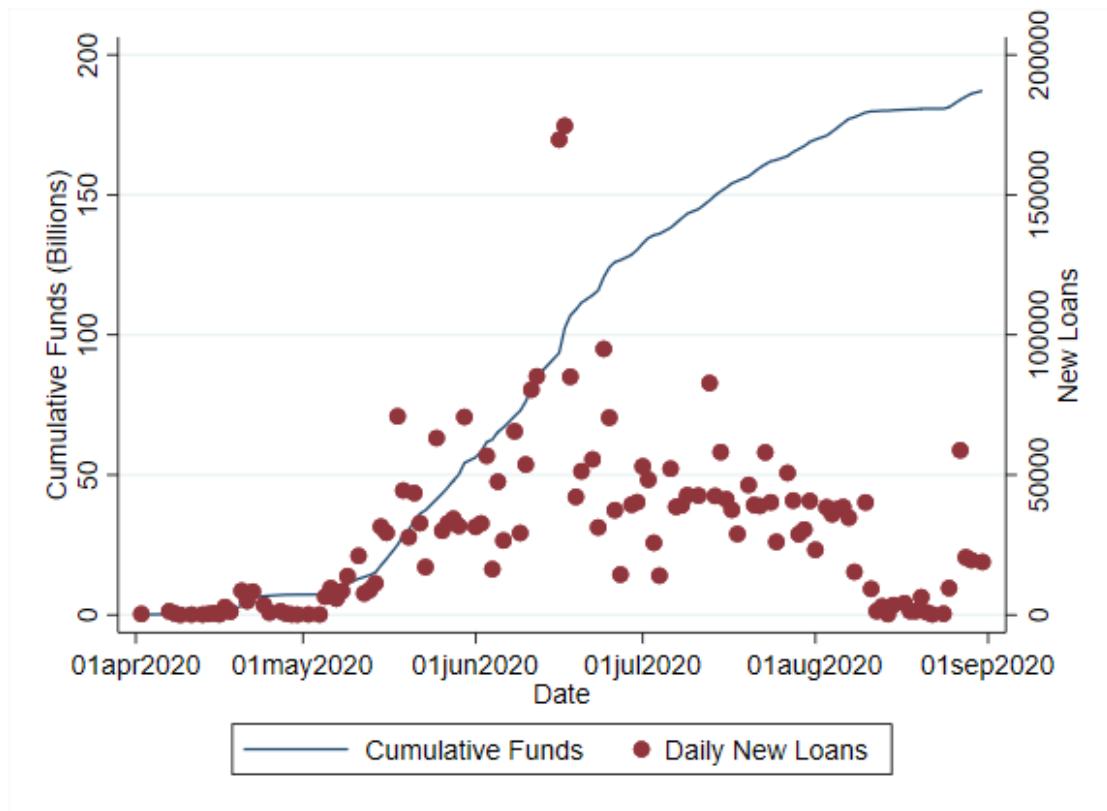
Figure A5: Timing of EIDL Loan Disbursement as Reported in SBA Data

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

Hubbard and Strain (2020)

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

Autor et al (2022a)

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** Effect of *eligibility* for PPP in either round
- **Who results apply to given data and strategy:** Firms *eligible* for PPP (firms with 250-500 employees)
- **Time horizon:** Jan 2020 - Dec 2020

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

Autor et al (2022b)

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

Chetty et al (2020)

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** Effect of *eligibility* for PPP in either round
- **Who results apply to given data and strategy:** Firms *eligible* for PPP (firms with 100-500 employees, excluding food services)
- **Time horizon:** Feb 2020-Aug 2020
- **Data sources / Sample:** Consumer spending data from Affinity solutions; Small Business Revenue Data from Womply; Job Postings data from Lightcast; employment data from Paychex,

Intuit, Earnin; UI data from state agencies; COVID incidence from NYT, JHU, CDC and Dept. of Health and Human Services; Mobility data from Google Mobility Reports
- **Estimated Effects ($/jobs saved):**
  - $377,000; $359,000 after netting out potential UI payments
- **Estimated effects (% effect on employment per firm)**
  - 1.01-1.78 pp increase in employment (imprecise)

Doniger and Kay (2021)

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

Granja et al (2022)

- **Empirical approach:** Instrument (PPP bank exposure) - use the gap between market share of a bank in PPP lending in the first round and its pre-pandemic small business lending
- **Estimand:** They don't explicitly address the first vs second round in their research design, but argue that their estimates for April and May reflects effect of receiving funds vs not receiving funds, and those of June reflects effects of early vs late receipt
- **Who results apply to given data and strategy:** Compare firms in areas with high bank PPP exposure to those in low bank PPP exposure
- **Time horizon:** Jan 2020 - Aug 2020
- **Data sources / Sample:** PPP data - SBA and Department of Treasury; Reports of Condition and Income (Call Reports) filed by all active commercial banks in 1st quarter of 2020; Homebase (private firm that manages scheduling and time clock) data -- establishment level weekly

employment indicators. This data disproportionately covers small firms and in F&B service and retail; County-by-week initial unemployment claims from state agencies; Small business revenue data (county aggregate of credit card spending) from Womply; additional county-level employment data from Opportunity Insights

- **Estimated Effects (total jobs saved):**
  - 2.02 - 3.28 million jobs saved in first 5 months
- **Estimated Effects ($/jobs saved):**
  - $175,000
- **Estimated Effects on Firm Survival:**
  - Effectively no effect on firm survival. 0.6 pp decrease in business shutdowns in areas with high PPP exposure
- **Estimated effects (% effect on employment per firm):**
  - 1 pp increase in employment in May, 2pp increase in employment in June

Dalton (2021)

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

Faulkender, Jackman and Miran (2020)

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

Li and Strahan (2020)

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

Kurmann et al (2021)

- **Empirical approach:** Difference-in-differences and event studies
- **Estimand:** Effect of delay in PPP loans (1st round vs second round). Loan delay is measured as the share of PPP ($ value) issued in first week of second tranche as a share of dollar value of loans issued last week of 1st tranche and 1st week of second tranche in a county. Similar to Doninger and Kay (2022)
- **Who results apply to given data and strategy:** Firms that got PPP approved in last week of first round vs firms that got PPP in the first week of second round PPP. Focus on four sectors: Retail trade Education and Health, Leisure and Hospitality and Other Services
- **Time horizon:** Feb 2020 - Nov 2021
- **Data sources / Sample:** Homebase data - establishment level weekly employment indicators; Match Homebase data to SafeGraph, Google and Facebook; PPP data from SBA
- **Estimated Effects (total jobs saved):**

- o In the absence of delay, employment would have been 3 million more in July 2020 (10% higher); 1.8 million higher in January 2021 (6% higher)
- **Estimated Effects on Firm Survival:**
  - o 1 pp increase in PPP loan delay increased business closing in a by 0.1 pp in May 2020; peaking around 0.2 pp in Fall 2020, with effect declining back to 0.1pp by Feb 2021 (relative to mid-Feb 2020). They estimate aggregate reduction in permanent closing of about 5% in the absence of delay in PPP loans
- **Estimated effects (% effect on employment per firm):**
  - o 1 pp increase in PPP loan delay decreased county employment by 0.1 pp in May 2020; 0.25 pp in August 2020, with effect persisting up to Feb 2021 (relative to mid-Feb 2020 employment)

Humphries et al (2020)

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

Joaquim and Wang (2022)

- **Empirical approach:** Instrument PPP loans received by firms in a county by the community bank share. They argue that community bank share is not a valid instrument without accounting for firm's pre-covid financial condition.
- **Time horizon:** Jan 2019 - Aug 2021
- **Data sources / Sample:** Dun & Bradstreet Corporation data for firms' financial health; PPP data from SBA; County Business Patterns (2020); Local Area Unemployment Statistics; FDIC Summary of Deposits for community bank exposure
- **Estimated effects (% effect on employment per firm):**
  - o 0.5-1 pp increase in county level employment in first 5 months after Apr 2020 (using community bank share instrument); effects become indistinguishable from zero after accounting for firm's pre-covid financial condition. (figure 3)
- **Heterogeneity in employment effects:** Find large heterogeneity in the effect of PPP on firm's financial condition

Joaquim and Netto (2021)

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

Staples and Krumel (2022)

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

Denes, Lagaras and Tsoutsora (2021)

- **Empirical approach:** Difference-in-differences
- **Estimand:** Effect of delay in receiving PPP (end of first tranche vs early second tranche). Relatively longer run effects on firm survival

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

Berger et al (2022)

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

Barraza et al. (2020)

- **Empirical approach:** Instrument for PPP loans using pre-pandemic density of SBA member bank offices in a county
- **Who results apply to given data and strategy:** Compare firms in counties high PPP exposure to those in low PPP exposure using bank density as instrument
- **Time horizon:** Jan 2020- Apr 2020
- **Data sources / Sample:** County-level labor market data from BLS and from tracktherecovery.org (Opportunity Insights Team); Banking data from Summary of Deposits; PPP data from SBA
- **Estimated effects (% effect on employment per firm):**
  - 1.4 % lower unemployment in counties with higher SBA member office densities

Cole (2022)

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

Bartlett and Morse (2021)

- **Empirical approach:** Logit with controls and fixed effects
- **Estimand:** Effect of applying and receiving PPP
- **Who results apply to given data and strategy:** Firms in City of Oakland, California. Survival effects estimated on a small sample of 278 firms
- **Time horizon:** Follow-up Survey conducted in June 2020
- **Data sources / Sample:** City of Oakland Small Business Survey about resiliency during Covid pandemic (round 1 in March 2020; follow-up round 2 on a small subset in early June 2020); Hand-collected data from Google Maps and Company websites on operational status of firms between April 24, 2020-May 3, 2020; Homebase data for employment indicators; SafeGraph foot traffic data between Jan-Apr 2020
- **Estimated Effects on Firm Survival:**
  - 20.5% increase in self-reported medium-run survival probability (within 6 months from June 2020 for microbusinesses with 1-5 employees). No effect for short-term closure
- **Heterogeneity in firm survival effects:**
  - They find that medium-run survival probability to varies with firm size (negative estimate for interaction of pre-COVID size and PPP), suggesting that the increase in survival probability decreases with firm size and the effect becomes immaterial after ~20 employee size

Table A2: Summary Statistics For the Full and Final Samples

This table reports summary statistics for survival expectations and January employment across different samples. Survival expectations are the probability a firm expects to be open in December 2020. The sample with the complete set of regressors means that we observe PPP application and receipt status, cash on hand, survival expectations, January 2020 and contemporaneous employment, pre-Covid payroll, fixed expenses, banking relationship information, naics industry, and contemporaneous operating status.

	N	Mean Survival Expectation	Std. Dev. Survival Expectation	Mean Jan 2020 Employment	Std. Dev. Jan 2020 Employment
All Responses from US Firms	16914				
Dropping firms with over 500 employees or implausible payroll to employment values	16896				
Answered the PPP application question	11472				
For firms that answered the PPP question and pass employment filters:					
Answered survival expectation	7906	0.734	0.281		
Answered Jan 2020 employment	9576			8.060	24.554
Answered bank relationship	8798				
With matched instrument in SBA Data	8741				
With Complete Set of Regressors	6640	0.732	0.282	7.750	22.463

Table A3: Summary Statistics Illustrating Speed versus Steering Impacts

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

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

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

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

	(1)	(2)	(3)	(4)	(5)	(6)
January 2020 Employees	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Monthly Payroll	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Fixed Expenses	-0.000	0.000	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Average Zip Code Proximity	-0.007	-0.007	-0.005	-0.005	-0.006	-0.004
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
Frontline Industry	0.014*	0.013*	0.004	0.012	0.012	0.003
	(0.006)	(0.005)	(0.004)	(0.007)	(0.007)	(0.006)
Weeks of Cash	0.002**	0.002***	0.001*	0.003***	0.004***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Bank Loan	-0.048***	-0.041***	-0.010	-0.045***	-0.038***	-0.010
	(0.010)	(0.009)	(0.007)	(0.011)	(0.010)	(0.007)
Bank Officer	0.101***	0.087***	0.041***	0.097***	0.080***	0.039***
	(0.017)	(0.012)	(0.011)	(0.017)	(0.013)	(0.009)
Constant	0.288***	0.287***	0.292***	0.278***	0.276***	0.286***
	(0.052)	(0.044)	(0.029)	(0.054)	(0.045)	(0.029)
R-Squared	0.035	0.177	0.486	0.037	0.184	0.487
Observations	6640	6640	6640	4452	4452	4452
Applicant Sample	N	N	N	Y	Y	Y
State Fixed Effects	N	Y	Y	N	Y	Y
Bank Type Fixed Effects	N	N	Y	N	N	Y
Standard Deviation of Fitted Values	0.046	0.041	0.019	0.048	0.043	0.020

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Median Age in County	0.000	0.001	-0.000	0.001**	0.000	0.001*	-0.000	0.001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Share with BA+ Degree	-0.030	-0.019	0.022	-0.005	0.029	-0.010	0.030	0.003
	(0.029)	(0.026)	(0.029)	(0.026)	(0.031)	(0.027)	(0.028)	(0.025)
Per Capita Income / 1000	-0.001*	-0.001	-0.001*	-0.000	-0.001*	-0.000	-0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Small Businesses Open Relative to Jan 2020			-0.246**	-0.198*			-0.318***	-0.283**
			(0.088)	(0.086)			(0.095)	(0.096)
Small Bus Revenue Relative to Jan 2020			0.245***	0.139***			0.235***	0.152**
			(0.041)	(0.042)			(0.049)	(0.052)
Employment of First Quartile Earners					-0.033	-0.060	-0.027	-0.069
					(0.041)	(0.037)	(0.043)	(0.039)
Employment of Second Quartile Earners					0.005	0.080	0.011	0.105
					(0.074)	(0.063)	(0.076)	(0.070)
Employment of Third Quartile Earners					0.200**	0.063	0.180*	0.056
					(0.064)	(0.055)	(0.070)	(0.064)
Employment of Fourth Quartile Earners					-0.021	-0.051	-0.024	-0.056
					(0.047)	(0.039)	(0.058)	(0.047)
Constant	0.567***	0.545***	0.574***	0.527***	0.549***	0.515***	0.565***	0.522***
	(0.011)	(0.008)	(0.012)	(0.010)	(0.013)	(0.011)	(0.014)	(0.012)
R-Squared	0.01	0.06	0.01	0.05	0.01	0.05	0.01	0.05
Observations	1882567	1882567	1413459	1413459	1345781	1345781	1266678	1266678
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Standard Deviation of Fitted Values	0.016	0.011	0.021	0.015	0.015	0.013	0.021	0.017

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS Estimates of Survival Expectations								
PPP approved	0.140***	0.14***	0.14***	0.13***	0.09***	0.09***	0.15***	0.08***
	(0.00692)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
R2	0.0480	0.05	0.11	0.16	0.22	0.22	0.12	0.22
N	5038	4452	4452	4452	4452	4452	4414	4414
Panel B: IV Estimates of Survival Expectations								
PPP approved	0.169***	0.19***	0.20***	0.18***	0.13***	0.13***	0.24***	0.09**
	(0.0268)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
N	4634	4452	4452	4452	4452	4452	4414	4452
First Stage F	320.6	233.7	187.0	177.4	163.2	164.3	146.5	79.8

Panel C: OLS Estimates of July 2020 Operational Status

PPP approved	0.15***	0.16***	0.14***	0.13***	0.09***	0.09***	0.14***	0.09***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
R2	0.02	0.02	0.08	0.08	0.10	0.11	0.08	0.11
N	3040	2749	2749	2749	2749	2749	2726	2749

Panel D: IV Estimates of July 2020 Operational Status

PPP approved	0.26***	0.25***	0.20***	0.18***	0.13*	0.11	0.13*	0.13
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.08)	(0.08)	(0.09)
N	2815	2749	2749	2749	2749	2749	2726	2749
First Stage F	232.75	323.8	281.2	259.1	185.1	193.4	216.8	117.70
Sample With Xs	N	Y	Y	Y	Y	Y	Y	Y
Industry FE	N	N	Y	Y	Y	Y	Y	Y
State FE	N	N	Y	Y	Y	Y	Y	Y
Bus Status FE	N	N	N	Y	Y	Y	N	Y
Cash FE	N	N	N	N	Y	Y	N	Y
Zipcode Proximity	N	N	N	N	N	Y	N	Y
Reweighted	N	N	N	N	N	N	Y	N
Bank Type FE	N	N	N	N	N	N	N	Y

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

This table reports OLS and IV estimates relating whether a firm was approved for the PPP program as of April 25, 2022 to employment outcomes. The sample is restricted to firms that applied for PPP, including firms that were ultimately denied and firms that tried to apply but were unable to submit an application. In Panels A and B the dependent variable is the level of April 25, 2020 employment and we control for employment in January of 2020. In Panels C and D, the dependent variable is the inverse hyperbolic sine transformation of April 25 employment, and we control for the inverse hyperbolic sine of January employment. For details on specification, see Table A6. All standard errors are clustered by bank and reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS Estimates of Employment Level Effects								
PPP approved	2.25**	2.23**	2.05**	1.75*	1.00	0.97	2.40**	0.82
	(1.01)	(0.83)	(0.74)	(0.74)	(0.52)	(0.52)	(0.89)	(0.52)
R2	0.557	0.53	0.55	0.55	0.57	0.57	0.59	0.57
N	6098	4452	4452	4452	4452	4452	4414	4452
Panel B: IV Estimates of Employment Level Effects								
PPP approved	4.61***	6.45***	6.42***	5.92***	5.70***	5.40***	8.21***	4.94**
	(1.07)	(1.72)	(1.92)	(1.89)	(1.88)	(1.88)	(2.94)	(2.24)
N	5575	4452	4452	4452	4452	4452	4414	4452
First Stage F	293.2	211.8	175.2	169.2	160.9	162	140.1	77.29
Panel C: OLS Estimates of Employment Pct Changes (Inv. Hyperbolic Sine)								
PPP approved	0.31***	0.31***	0.30***	0.26***	0.20***	0.20***	0.30***	0.20***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
R2	0.499	0.50	0.52	0.55	0.57	0.57	0.53	0.57
N	6098	4452	4452	4452	4452	4452	4414	4452
Panel D: IV Estimates of Employment Pct Changes (Inv. Hyperbolic Sine)								
PPP approved	0.47***	0.50***	0.52***	0.43***	0.40**	0.37**	0.64***	0.42**
	(0.074)	(0.13)	(0.17)	(0.16)	(0.17)	(0.17)	(0.23)	(0.18)
N	5575	4452	4452	4452	4452	4452	4414	4452
First Stage F	312.7	210.5	164.7	157	155.5	155.5	129.5	73.82
Sample With Xs	N	Y	Y	Y	Y	Y	Y	Y
Industry FE	N	N	Y	Y	Y	Y	Y	Y
State FE	N	N	Y	Y	Y	Y	Y	Y
Bus Status FE	N	N	N	Y	Y	Y	N	Y
Cash FE	N	N	N	N	Y	Y	N	Y
Zipcode Proximity	N	N	N	N	N	Y	N	Y
Reweighted	N	N	N	N	N	N	Y	N
Bank Type FE	N	N	N	N	N	N	N	Y

Table A8: Joint Estimates of Heterogeneous Treatment Effects

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Survival Expectations		July 2020 Operational Status		Employment Levels		Change in IHS(Employment)	
PPP Approved	0.09***	0.25*	0.17***	-0.04	0.05	3.79	0.04	0.41
	(0.02)	(0.13)	(0.05)	(0.25)	(1.22)	(4.34)	(0.06)	(0.61)
Approved x Frontline Industry	0.01	0.04	-0.03	0.03	0.15	-2.62	0.06	-0.18
	(0.01)	(0.06)	(0.04)	(0.14)	(1.26)	(2.30)	(0.05)	(0.16)
Approved x High Cash	-0.03*	-0.10	-0.00	0.11	-0.35	-0.05	-0.05	-0.20
	(0.02)	(0.07)	(0.03)	(0.13)	(1.18)	(3.45)	(0.07)	(0.35)
Approved x Bank Loan	-0.01	-0.09	-0.00	0.08	-1.11	1.20	0.02	0.06
	(0.02)	(0.06)	(0.03)	(0.15)	(1.28)	(2.55)	(0.05)	(0.25)
Approved x Bank Officer	0.01	-0.03	-0.03	-0.19	1.06	-1.46	0.22***	0.39**
	(0.02)	(0.05)	(0.04)	(0.14)	(1.52)	(3.61)	(0.07)	(0.18)
Approved x High Payroll	0.03	0.02	-0.03	0.02	0.23	5.91***	0.06	0.03
	(0.02)	(0.07)	(0.05)	(0.14)	(1.03)	(2.17)	(0.06)	(0.25)
Approved x High Fixed Expenses	-0.00	-0.06	-0.03	0.04	2.25*	5.12	0.04	0.06
	(0.02)	(0.05)	(0.05)	(0.14)	(1.23)	(3.48)	(0.07)	(0.27)
Approved x B2B Industry	0.00	0.09	-0.04	0.01	-0.41	-5.74***	-0.00	-0.14
	(0.02)	(0.07)	(0.04)	(0.12)	(1.26)	(2.21)	(0.06)	(0.19)
Approved x High Proximity Zipcode	-0.01	-0.06*	-0.04	0.09	0.56	-1.32	0.03	0.06
	(0.01)	(0.04)	(0.03)	(0.08)	(1.07)	(1.86)	(0.05)	(0.13)
R2	0.20	0.19	0.12	0.11	0.56	0.53	0.57	0.56
N	4234	4234	2638	2638	4234	4234	4234	4234
Approved Treatment Effects (TEs)	0.087	0.138	0.075	0.088	01.20	06.53	0.220	0.407
All Applicant TEs	0.086	0.173	0.090	0.073	0.801	04.66	0.177	0.374
Non-Applicant TEs	0.073	0.177	0.108	0.089	0.082	01.72	0.110	0.279

Table A9: Characterizing Gains to Alternative Treatment Allocation Approaches

This table reports results on jobs saved per $100,000 in program cost in the long run (based on survival in Panel A) and the short run (based on employment changes) under different allocation rules using a variety of approaches to estimating heterogeneous treatment effects. As in Table 5, to estimate jobs saved, we multiply the raw treatment effects by January 2020 employment divided by actual program costs for recipients and imputed program costs (based on payroll) for non-recipients. The first sub-panel shows the mean treatment effect for recipients and the standard deviation of treatment effects for all firms. The second sub-panel reports the difference in average jobs saved per $100,000 in program costs under five alternative procedures for allocating loans (keeping the total share of firms receiving loans constant). The alternative loan allocation procedures considered are: randomly allocating loans, targeting firms in industries with a high share of fronline workers, targeting firms with less than 50 employees, and allocating loans to the firms with the highest estimated treatment effects. Standard errors are shown in parentheses for the estimated differences in jobs saved per $100,000 for every alternative allocation approach. Different columns report results using different approaches to estimating heterogenous treatment effectts. Columns (1) through (3) show results using S-learners with alternative base-learners, with Column (1) using OLS, (2) using LASSO with the MSE minimizing cross-validated penalty used and Column (3) using a random forest model. Columns (4)-(13) then report results using three alternative meta-learners: T-learners, X-learners, and R-learners (these are described in more detail in the text). For each of these, we report results using both LASSO (with a cross-validated MSE minimizing penalty) and a Random Forest model. Column (10) reports results using the Causal Forest approach from Athey and Imbens (2016). All random forest models use a maximum tree-depth of three and 100 total trees. All models use as covariates months of cash available, monthly fixed expenditures pre-COVID, pre-COVID employment (categorical dummies for terciles), an indicator for a bank loan, an indicator for a loan officer relationship, and 2-digit industry dummies. How these covariates are used depends on the meta-learner. In the random forest models, instead of industry and state-dummies variables for the mean outcomes by industry and state are included. See discussion in Appendix B.

	S-Learner			T-learner			X-learner			R-learner			Causal Forest
Base Leaner	OLS	Lasso	RF	OLS	Lasso	RF	OLS	Lasso	RF	OLS	Lasso	RF	-
Lasso Penalty Details		CV-min	-		CV-min	-		CV-min	-		CV-min	-	-
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A. Jobs saved per $100k in spending using survival expecations													
Panel A1. Treatment effects under observed allocation													
Treatment effects for recipients	1.47	1.40	1.40	1.49	1.49	1.49	1.49	1.49	1.44	1.22	1.18	1.07	1.53
SD of treatment effects (full sample)	1.83	1.51	2.88	2.41	1.94	3.44	2.41	1.87	2.64	2.6	2.67	2.56	1.62
Panel A2. Treatment effects under alternative loan allocations													
Random allocation among applicants	0.34	0.29	0.36	0.41	0.39	0.45	0.41	0.37	0.42	0.34	0.36	0.4	0.11
	(0.06)	(0.05)	(0.10)	(0.08)	(0.06)	(0.12)	(0.08)	(0.06)	(0.09)	(0.09)	(0.09)	(0.08)	(0.06)
Frontline Industry Prioritization	0.09	0.14	0.24	0.2	0.11	0.3	0.2	0.08	0.39	0.25	0.25	0.24	-0.04
	(0.07)	(0.05)	(0.11)	(0.08)	(0.07)	(0.13)	(0.08)	(0.07)	(0.09)	(0.09)	(0.09)	(0.09)	(0.06)
Small Firm Prioritization	0.41	0.37	0.41	0.46	0.46	0.5	0.46	0.43	0.47	0.38	0.39	0.44	0.18
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Best Possible Allocation	-2.27	-1.94	-3.75	-2.78	-2.25	-4.3	-2.78	-2.25	-2.91	-2.86	-2.9	-2.68	-1.82
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Panel B. Short-run employment effects per $100k in spending using employment percent change													
Panel B1. Treatment effects under observed allocation													
Treatment effects for recipients	4.69	4.4	3.46	4.62	4.77	4.69	4.62	4.83	4.52	4.7	4.41	4.56	4.57
SD of treatment effects (full sample)	7.92	6.22	11.08	8.56	8.02	18.21	8.56	7.82	14.1	8.3	8.27	8.06	8.28
Panel B2. Treatment effects under alternative loan allocations													
Random allocation among applicants	1.83	0.169	0.94	1.8	1.97	1.28	1.8	2.02	1.41	1.32	1.45	1.4	1.09
	(0.26)	(0.20)	(0.36)	(0.28)	(0.26)	(0.60)	(0.28)	(0.25)	(0.45)	(0.28)	(0.28)	(0.27)	(0.27)
Frontline Industry Prioritization	1.43	1.24	0.32	1.42	1.45	0.6	1.42	1.48	0.73	0.39	0.67	0.95	0.51
	(0.28)	(0.21)	(0.41)	(0.31)	(0.29)	(0.69)	(0.31)	(0.27)	(0.49)	(0.32)	(0.31)	(0.30)	(0.31)
Small Firm Prioritization	2.14	1.94	0.132	2.13	2.29	1.9	2.13	2.31	1.79	1.66	1.75	1.77	1.36
	(0.25)	(0.19)	(0.35)	(0.27)	(0.25)	(0.57)	(0.27)	(0.25)	(0.44)	(0.27)	(0.26)	(0.26)	(0.26)
Best Possible Allocation	-8.82	-5.88	-11.96	-10.18	-8.77	-190.33	-10.18	-8.24	-12.37	-11.33	-10.49	-10.6	-5.44
	(0.53)	(0.37)	(0.78)	(0.60)	(0.58)	(1.38)	(0.60)	(0.51)	(0.83)	(0.66)	(0.60)	(0.63)	(0.47)

Table A10: Firm Characteristics By Treatment Effect Heterogeneity

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome			Probability of Survival			Jobs Saved per Dollar of PPP Spending		
Estimator	GRF IV		LASSO		GRF IV		LASSO	
Group	Other Firms	Top TE Firms	Other Firms	Top TE Firms	Other Firms	Top TE Firms	Other Firms	Top TE Firms
Payroll ($k)	20.2	10.7	17.4	24.3	19.8	12.8	19.6	13.7
Fixed Expenditures ($k)	1.5	1.3	1.4	1.6	1.4	1.5	1.4	1.5
Employees in January 2020	9.1	4.0	7.8	10.3	5.9	19.1	6.0	18.7
Any Bank Loans	0.42	0.35	0.39	0.48	0.41	0.38	0.40	0.41
Bank Officer	0.24	0.25	0.20	0.45	0.24	0.25	0.23	0.32
Cash on Hand (in weeks)	6.2	0.4	5.2	5.3	5.4	4.1	5.3	4.8
Employees per Imputed 100k$ of PPP Loans	16.4	14.4	16.3	14.6	9.5	46.9	9.6	46.6