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Allocation and Employment Effect of the Paycheck Protection Program

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The Paycheck Protection Program (PPP) was a large and unprecedented small-business support program enacted as a response to the COVID-19 crisis in the United States. The PPP administered almost $800 billion in loans and grants to small businesses through the banking system. However, there is still limited consensus on its overall effect on employment. This paper explores why it is challenging to estimate the effect of the PPP. To do so, we first focus on the timing of the allocation of PPP funds across regions and firms. Counties less affected by COVID-19 and with a larger presence of community banks, as well as larger firms, received loans earlier in the program. This differential timing observed in the data suggests that the current estimates of the effect of the PPP are not representative of the overall effect of the program. We qualitatively reconcile some of the conflicting results in the empirical literature and point to key questions surrounding the program.

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I. Introduction

The COVID-19 pandemic led to an unprecedented decrease in economic activity affecting small businesses in particular. In April 2020, revenues of small businesses decreased more than 40 percent compared with January of the same year and were still down 20 percent in August 2020. As a response, Congress created the novel Paycheck Protection Program (PPP) as part of the larger Coronavirus Aid, Relief, and Economic Security (CARES) Act. The program provided loans, which could turn into grants, with the goal of preserving jobs of small and medium-sized businesses that were substantially affected by COVID-19. In 2020 and 2021, almost $800 billion in forgivable loans were made through the program. From a policymaker's perspective, it is paramount to understand how successful the program was in preserving jobs at small businesses and how the program could have been more effective.

However, despite the enormous size and importance of the program, there is still limited consensus on its overall effect on employment. We still can't accurately pinpoint the extent to which the program was simply a transfer from taxpayers to small-business owners and the extent to which it did help to maintain jobs at small businesses. Table 1 displays a non-exhaustive list of papers that estimate the effect of PPP on employment, a brief description of how these papers estimate that effect, and the estimates themselves. The estimates range from 1.5 million to 18.6 million jobs saved by the program (in a universe of 70 million jobs at eligible firms according to Autor et al. (2020)).[1]

This note explores why it is challenging to estimate the effect of the PPP. We do so by first discussing the allocation of PPP funds throughout the course of the program. We show that PPP disbursement is very heterogeneous across banks, in particular in the first phase of the program. Moreover, we show that almost all changes in commercial and industrial (C&I) lending in 2020 can be attributed to the PPP, with little evidence of crowding out. We also find that regions less affected by COVID-19 (for instance, those that were not under a government-mandated closure or that saw a smaller decline in spending) received loans earlier in the program. Similarly, larger firms received loans earlier in the program. We develop

[1] It is worth noting that the 18.6 million estimate from Faulkender, Jackman and Miran (2021) is not directly comparable with the estimates from the other papers in Table 1. However, even if we remove this particular estimate, the estimates range from 1.5 million to 13 million jobs saved by the program, which is still a substantial discrepancy.

a stylized economic model to illustrate how this differential timing affects the estimation of the effect on employment of the PPP. We show that it is possible to estimate the effect of the PPP for a very specific subset of firms by using exposure to some type of bank or regional variation in PPP disbursement, as done by some of the papers in Table 1. This subset of firms, however, is not representative of the overall set of firms that received PPP loans, and it changed over time.

For concreteness, consider the papers by Granja et al. (2020) and Doniger and Kay (2020). We observe in the data that the average size of a firm receiving PPP loans decreased over time, such that larger firms received loans earlier. Moreover, let's suppose that larger firms were also those with the lowest treatment effects.[2] Granja et al. (2020) use bank variation in PPP lending in the first two weeks of the program: Some banks processed more than their expected share of PPP loans, while others processed less. Thus, firms that were clients of banks that processed more loans early were more likely to receive an early loan. Since larger firms were more likely to receive an early loan and have lower treatment effects, Granja et al. (2020) are bound to find a low treatment effect of the program. The same rationale applies to studies by Autor et al. (2020), Chetty et al. (2020*b*), and Bartik et al. (2020). Doniger and Kay (2020), on the other hand, use regional variation in PPP disbursement in the middle of the program, when smaller firms—and therefore those with higher treatment effects—received loans. Therefore, by comparing firms that received loans later in the program with those that did not, Doniger and Kay (2020) find much larger effects of the PPP.

In our final section, we discuss two natural questions that come from our analysis—which we tackle in separate papers. First, if none of the earlier papers estimates the effect of the PPP, then what was the overall effect of the PPP? We tackle this question in Joaquim and Netto (2021*b*). Second, one can alternatively ask: given that we observe a differential timing of PPP allocation based on firm characteristics (such as size), what *should have been* the allocation of the PPP loan if the government wanted to minimize job losses at small businesses? We tackle this question in Joaquim and Netto (2021*a*).

[2] In general, this assumption has support in other programs—see Zwick and Mahon (2017). Specifically for the PPP, see the evidence of Bartlett and Morse (2020).

II. The Paycheck Protection Program

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

In this paper, we consider only the first draw of the PPP program, which ran from April 3 through August 8, 2020.[3] Given the PPP's small-business focus, only firms with fewer than 500 employees were eligible to apply,[4] and each firm could apply for only one loan in the first draw of the PPP. The maximum loan amount was 2.5 times the firm's average monthly payroll costs in the preceding year up to $10 million. PPP loans have an interest rate of 1 percent, deferred payments for six months, and maturity of two years for loans issued in the first phase of the program and five years for loans issued after June 5, 2020. Moreover, PPP loans did not require collateral or personal guarantees.

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

Each loan application was processed by an eligible financial intermediary (referred to as banks for simplicity), for example, a federally insured depository institution or a credit union, which were responsible for checking documentation submitted by an applicant. Banks were paid a fee by the federal government to cover these processing costs. Importantly, loans from the PPP are fully guaranteed by the government and carry a zero-risk weight for the calculation of risk-weighted assets, with the purpose of minimizing the impact on banks'

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

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

capital requirements. Additionally, Federal Reserve Banks were authorized to provide liquidity to banks through the Paycheck Protection Program Lending Facility (PPPLF Facility). This allowed Federal Reserve Banks to extend loans to institutions that were eligible to make PPP loans using such loans as collateral. Overall, the program was designed to allow a large number of institutions to process loan requests while minimizing the impact on their balance sheet structure.

Table 2 shows aggregate statistics of the program on key dates, and Figure 1 shows the cumulative volume of loans made through the first draw of the program. We provide a description of the data used in Appendix A. Through a combination of the disbursement and application data, we define four distinct phases in the evolution of the first draw of the PPP:

1. **Phase 1: April 3 to April 16, 2020**. The first PPP loan was approved on April 3, 2020. During the first days of the program, PPP loans were being made at a fast pace, but PPP loan demand vastly exceeded supply. For instance, we see in Figure 2 that 72 percent of firms reported applying for the program, but only 36 percent reported receiving a PPP loan at the end of phase 1. This excess loan demand gave banks a significant role in the allocation of PPP funds. As we can see in Table 2, more than 1.6 million loans were made in this phase, with an average loan size of approximately $198,000.

2. **Phase 2: April 17 to April 26, 2020.** As a consequence of the enormous demand for PPP loans, the program ran out of funding on April 16, and there was a 10-day hiatus when no PPP loans were made. On April 24, in response to the enormous demand for PPP loans, Congress enacted the PPP Act, which appropriated an additional $321 billion (for a total of $670 billion) for PPP loans. Banks resumed approving PPP applications on April 27.

3. **Phase 3: April 27 to May 1, 2020.** There was still a backlog of applications, and loans were being made at a fast pace. At this stage, demand for PPP loans still outpaced supply, and banks continued to play some role in the overall allocation of PPP funds. As we can see in Table 2, more than 3.7 million loans had been made up to May 1 (70 percent of all loans made in the program). The cumulative average loan size decreased from $198,000 to $129,000, indicating that loans made from April 27 to May 1 were significantly smaller than those made in the first phase of the program.

4. **Phase 4: May 2 to August 8, 2020.** Demand for PPP loans was more subdued, and there was an excess supply of PPP loans, which reduced the role of banks in the allocation of PPP funds. This change in the role of banks from the first to the last phase is key to our empirical and theoretical analyses. The program stopped accepting applications on August 8 with $144 billion remaining from the PPP Act appropriation. More than 5 million loans were granted for a total amount of approximately $526 billion. More than 61.1 million workers were employed by firms that received a PPP loan.

III. PPP Disbursement and Bank Heterogeneity

This section explores the disbursement of PPP loans from the perspective of banks. We have three main results. First, disbursement of PPP loans was very heterogeneous across banks, in particular in the first phase of the program. Second, almost all of the change in C&I loans in 2020 was due to the PPP, and there is little evidence of crowding out of private loans.

Following the events of March 2020, banks saw a large increase in their amount of C&I loans as corporations dashed for cash. Li, Strahan and Zhang (2020) show that firms drew funds from preexisting credit lines at the fastest rate ever. This channel was first highlighted by Li and Strahan (2020) and can be seen in the increase in outstanding C&I loans in Figure 3. This effect was more pronounced for larger banks, given their higher share of unused C&I loan commitments (Table 3).[5] Banks were able to satisfy this demand for liquidity by the corporate sector due to a combination of deposit inflows and the Federal Reserve's interventions. As a result, this drawdown effect was short-lived. On the other hand, we see a significant increase in the volume of outstanding C&I loans after the beginning of the PPP on April 3, 2020, in particular for the smaller banks.

One potential explanation for the results in Figure 3 is that the largest banks were not the main providers of loans to small businesses before the pandemic—and thus didn't expand their C&I loan portfolios following the introduction of the program. To test this hypothesis, we follow Granja et al. (2020) and measure the gap between the market share of a bank in PPP lending in the first phase and its pre-pandemic small-business lending as a measure of

[5]Larger banks generally serve the largest firms, which are exactly those with a higher share of unused credit lines—see, for instance, the evidence in Chodorow-Reich et al. (2021).

a bank-level PPP shock, as in Eq.(1)

$$PPPE_b = \frac{\text{Share PPP}_b - \text{Share SBL}_b}{\text{Share PPP}_b + \text{Share SBL}_b} \times 0.5, \tag{1}$$

where Share PPP_b is the share of PPP lending from bank b at the end of the first phase of the program, and Share SBL_b is the share of bank b lending in small-business loans in 2019Q4 (in terms of dollar volume). We plot community bank status and bank $PPPE_b$ as a function of assets in Figure 4. [6] We find that $PPPE_b$ is increasing in bank size for small and mid-sized banks and decreasing for the largest banks. With the exclusion of the largest banks, we do not see a significant difference among banks in terms of community bank status.

Finally, we show in Table 3 that the majority of the change in C&I credit (loans and unused commitments) in 2020 came through the PPP. For instance, from a total increase of 5.78 percentage points of C&I credit relative to assets (compared with 2019Q4), 5.49 percentage points came from the program in Q2. Table 3 suggests that there was no crowding out of private lending. After the introduction of the program, the relative change of non-PPP C&I lending to PPP was small in 2020Q2 and negligible in 2020Q4. Although we can't interpret this as causal evidence of no crowding out, we can conclude that conditional on the economic conditions and all other government interventions, the outstanding amount of non-PPP C&I loans did not decrease in the presence of the PPP.

IV. Allocation of the PPP

In this section, we discuss the allocation of the PPP loans across counties and firms. First, we present visual evidence that counties receiving PPP loans early in the program had characteristics that were different from those of firms receiving PPP loans later in the program. We do so by following Doniger and Kay (2020) and take a set of county r characteristics in the baseline (before the PPP was introduced), $X_{0,r}$, and take the daily weighted average of those characteristics using PPP disbursement by counties as weights. Mathematically, if county r

[6] We use the FDIC's database of community banks to establish which financial institutions are community banks. An institution is a community bank if it satisfies various criteria (see, for instance, https://www.fdic.gov/resources/community-banking/report/2020/2020-cbi-study-app-a.pdf). Overall, community banks are those that provide loans and deposits and have assets below a certain threshold ($ 1.65 billion in 2019).

had $PPP_{r,t}$ volume of loans allocated to that county in day t, we compute

$$\bar{X}_t = \frac{\sum_r X_{0,r} PPP_{r,t}}{\sum_r PPP_{r,t}}. \tag{2}$$

To facilitate the interpretation of the output, we normalize $\bar{X}_t$ by its mean over time and plot the percentage deviations from this mean; that is

$$\tilde{X}_t = 100 \cdot \left[\frac{\bar{X}_t}{T^{-1} \sum_{t=1}^{T} \bar{X}_t} - 1 \right]. \tag{3}$$

The idea behind the index in Eq.(3) is to measure the average characteristic across counties relative to their PPP allocation. Therefore, if we find that $\tilde{X}_{Apr--3}$ is small relative to $\tilde{X}_{May--15}$, we can conclude that counties with a low $X_{0,r}$ received PPP loans earlier in the program.

The results are in Figure 5. Counties that received PPP loans earlier had a larger share of deposits and branches of community banks (Panel A), had a larger share of firms with more than 20 employees (Panel B), were less likely to be under government-mandated closures (Panel C), had fewer COVID-19 deaths/cases (Panel D), saw a smaller decrease in spending and revenues (Panel E), and overall had more branches per capita (Panel F). Overall, the results in Figure 5 indicate that counties less affected by COVID-19, that have larger firms, and that had a higher share of community banks were the ones that received loans earlier in the program.

We show in Figure 6 the heterogeneous allocation across firms. The average loan size and number of employees of PPP recipients were decreasing over the course of the program. For instance, the average loan size dropped from \$300,000 on the first day of the program, April 3, 2020, to approximately \$25,000 by May 15, 2020. Importantly, note that the changes in county (Figure 5) and firm (Figure 6) characteristics receiving PPP loans were concentrated in Phases 1 through 3 of the program (from April 3 to May 1), exactly when demand for PPP funds outpaced supply and banks played a major role in the allocation of PPP funds.

The results in this section are not necessarily causal. That is, the figures do not imply that more PPP funds went earlier to counties *because* they were less affected by COVID-19. What we show is that there was significant heterogeneity in the allocation of PPP loans across counties and firms, and that this heterogeneity was related to deep economic factors. These factors were very likely linked to both (1) employment in the absence of PPP and (2) the effect of the PPP on employment. As we show in the next section, these correlations are important for understanding the challenges with estimating the effect of PPP on employment.

Although we focus here on the visual evidence of Figures 5 and 6, we provide in Joaquim and Netto (2021*b*) a more complete statistical analysis of these effects. For instance, we show that the county heterogeneous allocation effects existed—and were statistically significant—even in the comparison of counties within a given state and relative to the number of eligible firms in a given county. Similarly, we show that even in a within-bank comparison, the size of the average firm receiving a PPP loan was decreasing over the course of the program.

V. Estimating the Effects of the PPP

This section explores the implications of the results of Section IV for the estimation of the overall effect of the PPP on employment. We conduct this exploration using a stylized economic model. In our model, there are two regions, A and B. Both regions have a continuum of firms. For simplicity, we assume that for each firm in region A, there is an identical firm in region B—with the only difference between them being a potential PPP loan. We index different firms by j in each region $r = A, B$. Each region has a unique bank disbursing PPP loans, which we denote by the same name as the region. Firms can borrow only from the bank in their region. Bank A disburses more PPP loans than bank B at any moment in time (for instance, bank A is a community bank). We define two key characteristics of firm j in region r:

- $\theta_{j,r}$: represents the probability of firm closure *without* a PPP loan.
- $T_{j,r}$: represents the *change* in the probability of firm closure upon receipt of a PPP loan.

From our previous definitions, the probability that a firm survives is given by

$$\mathbb{P}(j,\text{ in region } r,\text{ survives at time } t) = \theta^t_{j,r} + PPP^t_{j,r} T^t_{j,r},$$

where $PPP^t_{j,r} = 1$ for firms that receive PPP loans up to time t, and $PPP^t_{j,r} = 0$ otherwise.

Our model has four periods, each corresponding to a phase of the program we described in Section II:

- Period $t = 1$: Banks play a key role in the allocation of PPP loans.
- Period $t = 2$: No PPP loans are made.
- Period $t = 3$: Banks still have a role in the allocation of PPP loans.
- Period $t = 4$: The supply of PPP loans exceeds demand, and banks play no role in the allocation of PPP loans.

The setting described above is very stylized, but it captures the key economic channels we want to highlight. A few comments are in order. First, we implicitly assume that firms in the model either survive or don't, but in reality, they could downsize after the COVID-19 shock. To take downsizing into account, we could simply rewrite our model at the job level (rather than at the firm level). Second, we could extend our model to feature banks that make loans in multiple regions or firms that borrow from banks in regions other than the one where they are located. Third, we also could extend our model to feature regions of different sizes, different distributions of firms, or other dimensions of heterogeneity. We develop a model with these extensions in Joaquim and Netto (2021*b*).

The first result we can show in our setting is that we can't simply compare firms that received PPP loans with those that didn't receive PPP loans (even conditional on application) at any given moment in time to estimate the effect of the program. We show this mathematically in *Example 1*. Intuitively, if firms that received PPP loans early (for instance, because they are larger) had a different $\theta^t_{j,r}$ compared with firms that didn't receive PPP loans early, the firms that didn't receive PPP loans are not a good control group.

Example 1: Why can't we just compare firms that received PPP loans with those that didn't? *Consider only region A in our setting, and assume for this example that region A has two types of firms: H and L. We assume that all firms have the same effect of receiving PPP loans on employment; that is,* $T_H = T_L = T \in (0,1)$*. Firms of type H survive with probability* $\theta_H = 1 - T > 0$ *without PPP funds, while of firms of type L survive with* $\theta_L = 0$ *without PPP funds. In this setting, the effect of the PPP is* T*. Moreover, suppose that bank A has enough funds to* *allocate to half of the firms and chooses to allocate those funds to firms of type H, that is, those with a higher probability of survival without PPP funds (for instance, the larger firms). In this case, all firms that receive PPP loans will survive. All firms that don't receive PPP are of type L and won't survive. The comparison between firms that received PPP loans and those that didn't would lead to the conclusion that the effect of the PPP is 1, even though the true effect of the PPP is given by* $T < 1$. ■

Given that we can't simply compare firms that received PPP loans with those than didn't, we can use the fact that firms in region A have a bank that was better at disbursing PPP loans in every period and thus are more likely to have received PPP funds—not because they are of a different type (as in *Example 1*), but simply because they are clients of a different bank, which is exogenous in our stylized setting. To be able to discuss the implications of banks choosing which firms to lend to at each moment in time, we need to introduce the concept of *compliers*:

Definition. Complier: *A complier at time t is a firm that did receive a loan from bank A but wouldn't have received a loan from bank B by time t.*

The second result in our setting is that we can, at any moment in time, estimate only the effect of the PPP on compliers. Consider firm j in region A and its identical counterpart in region B (except potentially for PPP receipt). Let their difference in probability of survival at a moment t be given by $\Delta_{j,t}$. We have that

$$\Delta_{j,t} = \theta^t_{j,A} + PPP^t_{j,A} T^t_{j,A} - \left[\theta^t_{j,B} + PPP^t_{j,B} T_{j,B}\right].$$

Since these are identical firms, $T^t_j \equiv T^t_{j,A} = T^t_{j,B}$ (and analogously for θ^t_j). Let C^t be the set of firms that are compliers; that is, a firm j is in C^t if $PPP^t_{j,A} = 1$ and $PPP^t_{j,B} = 0$.[7] We can rewrite

[7] We assume here that since bank A is better at disbursing PPP loans, there is no firm j such that $PPP^t_{j,A} = 0$ and $PPP^t_{j,B} = 1$; that is, there is no firm that did not receive a PPP loan in region A that would have received one in region B.

the pair-wise difference as

$$\Delta_{j,t} = \left(PPP_{j,A}^{t} - PPP_{j,A}^{t}\right) T_j = \begin{cases} 0, \text{ if firm} j \text{ in } C^t \\ T_j, \ 0, \text{ if firm} j \text{ not in } C^t \end{cases}.$$

And let T_C^t be the average effect of the PPP on the firms that are compliers. Aggregating across all pairs of firms, we find that the difference Δ in the share of firms that survive in A versus B is given by

$$\Delta_t \equiv \int_j \Delta_{j,t} dj = \left[PPP_A^t - PPP_B^t\right] \cdot \int_{j \text{ in } C^t} T_j^t dj = \left[PPP_A^t - PPP_B^t\right] \cdot T_C^t.$$

Therefore, the difference in survival relative to the difference in PPP allocation is given by T_C^t, that is, the effect of the PPP on compliers. We provide in *Example 2* a version of this result with only two types of firms. Intuitively, firms that are not compliers either receive or don't receive PPP loans regardless of their region, and thus when we compare across regions (or across banks), their effects cancel out. The crux of the argument of this paper involves which firms are in the set of compliers at each moment in time.

Example 2: Why can we estimate the employment effect only on compliers? *Suppose that there are only two types of firms, H and L, and each consists of half of the population of firms in each region. Firms of type H have a treatment effect of T_H, larger than the treatment effect of firms of type L, T_L; that is, $T_H > T_L$. The bank in region A can provide PPP funds for 100 percent of the firms, while the bank in region B can provide funds to 50 percent of the firms. Suppose that banks choose to allocate funds first to the firms with the lowest treatment effects (for instance, the largest firms). In region B, where 50 percent of the firms receive PPP funds, all of the type L firms and none of the type H will receive PPP funds. The type H firms in region B are the compliers in this setting. The overall effect of the PPP (adjusted by the share of firms that receive PPP loans, which in this case is three-quarters) is given by:*

$$\text{PPP Effect} = \frac{4}{3}\left[\frac{1}{2}\left[\frac{1}{2}T_H + \frac{1}{2}T_L\right] + \frac{1}{4}T_L\right] = \frac{2}{3}T_L + \frac{1}{3}T_H.$$

Comparing survival in region A with survival in region B (adjusted by the differences in PPP disbursement, which in this case is one-half) delivers

$$\Delta = 2\left[\frac{1}{2}T_L + \frac{1}{2}T_H - \frac{1}{2}T_L\right] = T_H,$$

that is, the effect of the PPP on compliers. ■

Which firms are compliers at each moment in time? To characterize who the set of compliers are at each phase of the program, we need to introduce some additional assumptions and notations. We assume that there is some firm characteristic η (such as size, the degree to which this firm was affected by COVID-19, or another characteristic) and that banks choose which firms to lend to based on this characteristic. Banks have a decreasing profit in η, such that they prefer to allocate funds to low-η firms. We assume that η is also related to the treatment effect of firms. In particular, we assume for exposition purposes that T_j is hump shaped in η, as depicted in Figure 7. This shape for the T_j curve is based on the idea that firms less affected by COVID-19 (low-η) and firms too affected by COVID-19 (high-η) have similar treatment effects but for different reasons: Low-η firms are likely to survive regardless of their PPP allocation, while high-η firms are unlikely to survive regardless of their PPP allocation. At each moment in time, we define the set of firms that receive loans in regions A and B by, respectively, A_t and B_t.

We show in Figure 7 which firms are compliers at each phase of the program. At $t = 1$, where banks play a key role in the allocation of PPP funds, the set of compliers is given by the firms in A_t and not in B_t. Note that at this stage the overall effect of the program (T^1_{All}) is low and different from the effect of the program on the compliers (T^1_C). A similar situation is present at $t = 3$. However, note how the set of compliers shifts, and thus the set of firms for which the PPP effect is being estimated shifts as well. More specifically, we find that the effect of the PPP on $t = 3$ compliers is larger than the effect on $t = 1$ compliers; that is, $T^3_C > T^1_C$. In $t = 4$, we suppose that $A_4 = B_4$; that is, banks A and B serve the same set of firms in terms of their type (even though bank A still serves a higher number of firms). At this stage, banks don't play a role in the allocation of PPP funds, and the set of compliers is simply a random set of the firms that receive PPP loans at this stage. In summary, Figure 7 shows that in the case of the PPP, the set of compliers (1) was changing over time and is (2) not representative of the overall set of firms that received PPP loans.

Reconciling the evidence from the papers in Table 1. We focus here on the difference between the findings of Granja et al. (2020) and Doniger and Kay (2020). The reason for this is that the other papers either do not have a precise estimate of PPP on employment (Bartik et al. (2020)), do not allow for a direct comparison with other estimates (Faulkender, Jackman and Miran (2021)), or use the 500-employee eligibility cutoff to estimate the effect of the PPP (Autor et al. (2020), Chetty et al. (2020*b*)) and thus can credibly estimate only the effect on the largest firms eligible for the program. Granja et al. (2020) use variation in PPP allocation in the first phase ($t = 1$) in our model. At this phase, the set of compliers, that is, firms in B_1 and not in A_1, is composed of the smaller firms among the largest firms. That is, the true effect of the program at this stage is small, and the estimated effect of the PPP is higher than the true effect of the PPP at $t = 1$.

Doniger and Kay (2020) leverage the fact that the PPP program did not approve any loans from April 16 through 26, 2020, to identify the effects of the PPP. The idea is that around this 10-day window the timing variation in PPP disbursement was as good as random. As our analysis shows, firms that received funds around the 10-day window were not similar to those that did not. At this phase ($t = 3$), the true effect of the program is higher than $t = 1$, but the set of compliers is still different from the set of overall firms that received PPP loans, and the estimated effect of the PPP is *higher* than the true effect of the PPP at $t = 3$.

Our stylized model can thus provide a unifying explanation for the results found in the literature: Firms of different sizes that were affected differently by COVID-19 received PPP loans at different moments in time. Thus, depending on when the variation in PPP occurs in an empirical strategy will also determine the set of compliers, which will be different from the overall set of firms. Studies that leverage early variation in PPP allocation will tend to find lower treatment effects, while studies that leverage variation later will likely find higher treatment effects. Neither of the estimated effects, however, corresponds to the overall effect of the program at any moment in time.

VI. The $800 Billion Questions

The PPP was a key part of the policy response of the U.S. government to the COVID-19 crisis. Throughout 2020 and 2021 the program ultimately disbursed almost $800 billion dollars in forgivable loans to small businesses in order to reduce job losses during the COVID-19 pandemic. Given the size of the program, extensive research has tried to estimate its effect on employment, but no consensus has emerged from the empirical literature. We have shown in this paper that the allocation of these loans across regions and firms was not random. Regions less affected by the pandemic, as well as larger firms, received PPP loans earlier. We have illustrated that this different timing in allocation can affect our interpretation of the empirical evidence and that apparently conflicting empirical results can be reconciled—although none captures the overall effect of the program.

Two natural questions emerge from this note, which we tackle in two separate papers. First, what is the overall effect of the PPP on employment? In Joaquim and Netto (2021*b*), we provide comprehensive evidence of heterogeneous allocations of PPP funds. How the targeting of PPP funds affects our interpretation of the estimated effect of PPP on employment is formalized in a model that features rich bank and regional heterogeneity. We estimate the effect of the program to be a reduction in nonemployment at eligible firms of 12.9 percentage points, which corresponds to roughly 7.5 million jobs at a cost of $70,000 per job. Second, if this effect of PPP on employment depends on the allocation of funds across firms, how should the government want these funds to be allocated? Alternatively, what could the effect of the PPP have been if funds were optimally allocated across firms and over time? We tackle this question in Joaquim and Netto (2021*a*). We develop an economic model in which the government must choose which firms to allocate PPP loans to in order to maximize employment at those firms. We show that funds were misallocated mostly in the first phase of the PPP, and that a policy targeting the smallest firms could have increased the program's effectiveness significantly.

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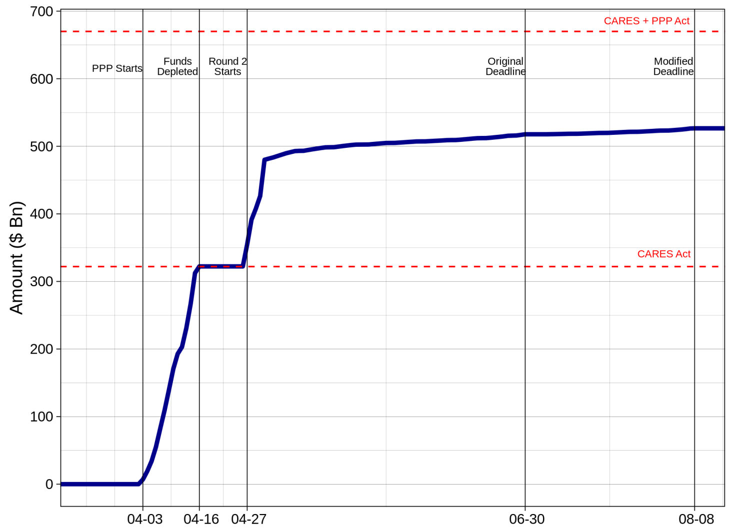
Figures and Tables

Table 1: Non-exhaustive List of Papers and Estimates of the Effect of PPP on Employment

Paper	Identification	Jobs Saved (Million)
Chetty et al. (2020*a*)	500 Eligibility Cutoff	1.51
Autor et al. (2020)	500 Eligibility Cutoff	2.31
Granja et al. (2020)	Bank Heterogeneity in PPP Disbursement in the 1st Phase	3.2-4.8
Doniger and Kay (2020)	Share of Loans Delayed between 1st and 3rd Phases	13
Faulkender, Jackman and Miran (2021)	Community Bank Heterogeneity in PPP Disbursement	18.6
Bartik et al. (2020)	Bank Heterogeneity in PPP Disbursement in the 1st Phase	Wide CIs

Note: Compiled by the author. Whenever the cost per job estimate is available, we use those provided in the papers. Faulkender, Jackman and Miran (2021) estimate a dynamic effect of the PPP and interpret their results as an interquartile change, so the numbers are not directly comparable with those from other studies.

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



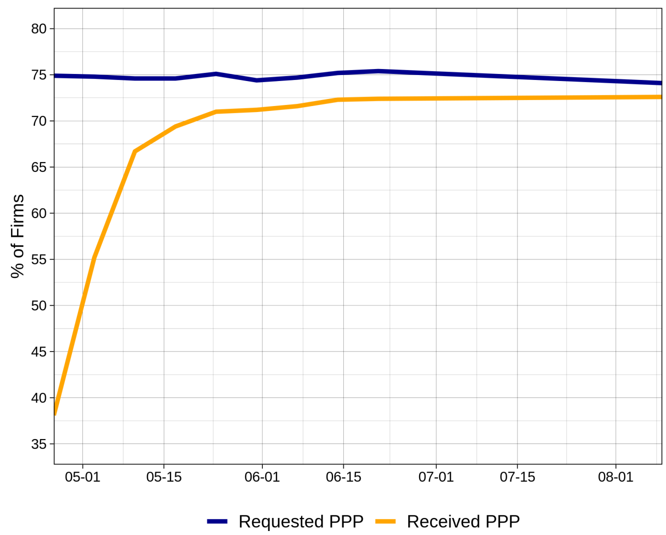
Note: Aggregation of loan-level data from SBA/Treasury February 2021 PPP release. Billions of dollars of PPP loans approved by day, from April 3, 2020 (CARES Act) through Aug. 8, 2020 (Modified deadline for applications). Dashed horizontal lines represent the cumulative capacity of the program.

Table 2: Summary Statistics of the Paycheck Protection Program

	Apr-16	May-1	Jun-30	Aug-08
Loan Amount ($Billions)	322.28	480.0	517.8	526.6
# Loans (,000)	1619.7	3700.02	4820.45	5,147.6
Jobs Reported (Millions)	33.2	54.62	59.96	61.1
Average Loan Size ($Thousands)	198.96	129.74	107.42	102.30
Average Jobs Supported	20.5	14.76	12.44	11.8

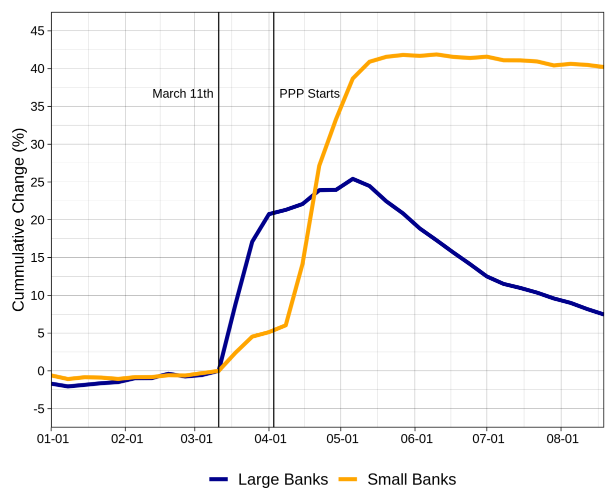
Note: Aggregation of loan-level data from the SBA/Treasury February 2021 release. Loan amount (in billions of dollars) and number of loans (in thousands) cumulated from the start of the program (April 3, 2020). Average loan size is the ratio of the cumulative loan amount over the number of loans. Jobs supported are reported by the firms during the PPP application. The top 4 banks (by assets in 2019Q4) are (1) JP Morgan Chase, (2) Bank of America, (3) Wells Fargo, and (4) Citibank, N.A.

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



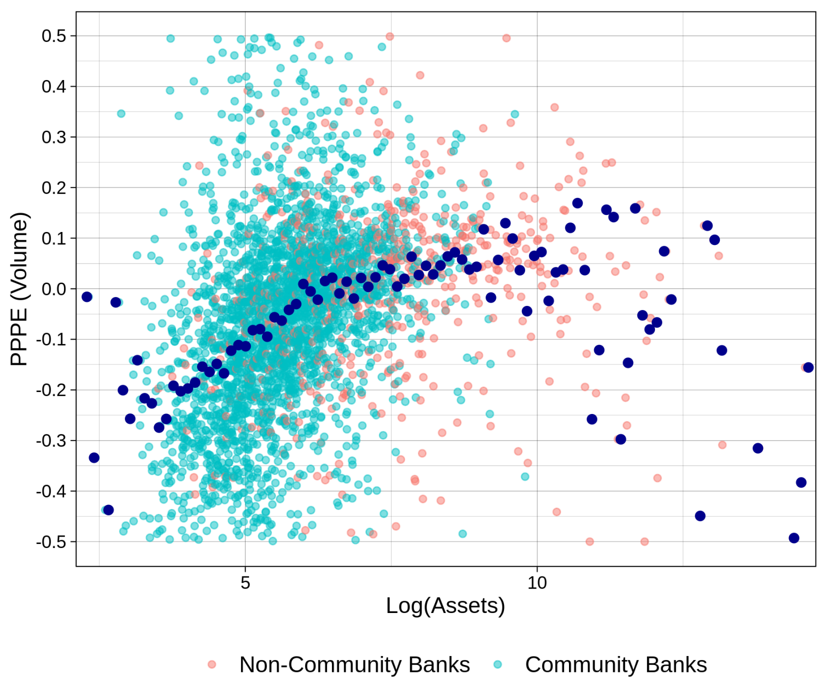
Note: U.S.-level data from the Small Business Pulse Survey (SBPS) collected weekly from April 26, 2020 through Aug. 9, 2020). Blue line denotes the percentage of firms that report applying for a PPP loan. Yellow line denotes the firms that report receiving a PPP loan. For details on data collection,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

Figure 3: Small vs. Large Banks C&I Lending During PPP Program



Note: Data from the H.8 Schedule: *Assets and Liabilities of Commercial Banks in the United States*. We plot the cumulative change in C&I lending from March 6, 2020 (normalized to zero). Large domestically chartered commercial banks are defined as the top 25 domestically chartered commercial banks, ranked by domestic assets as of the previous commercial bank Call Report to which the H.8 release data were benchmarked.

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



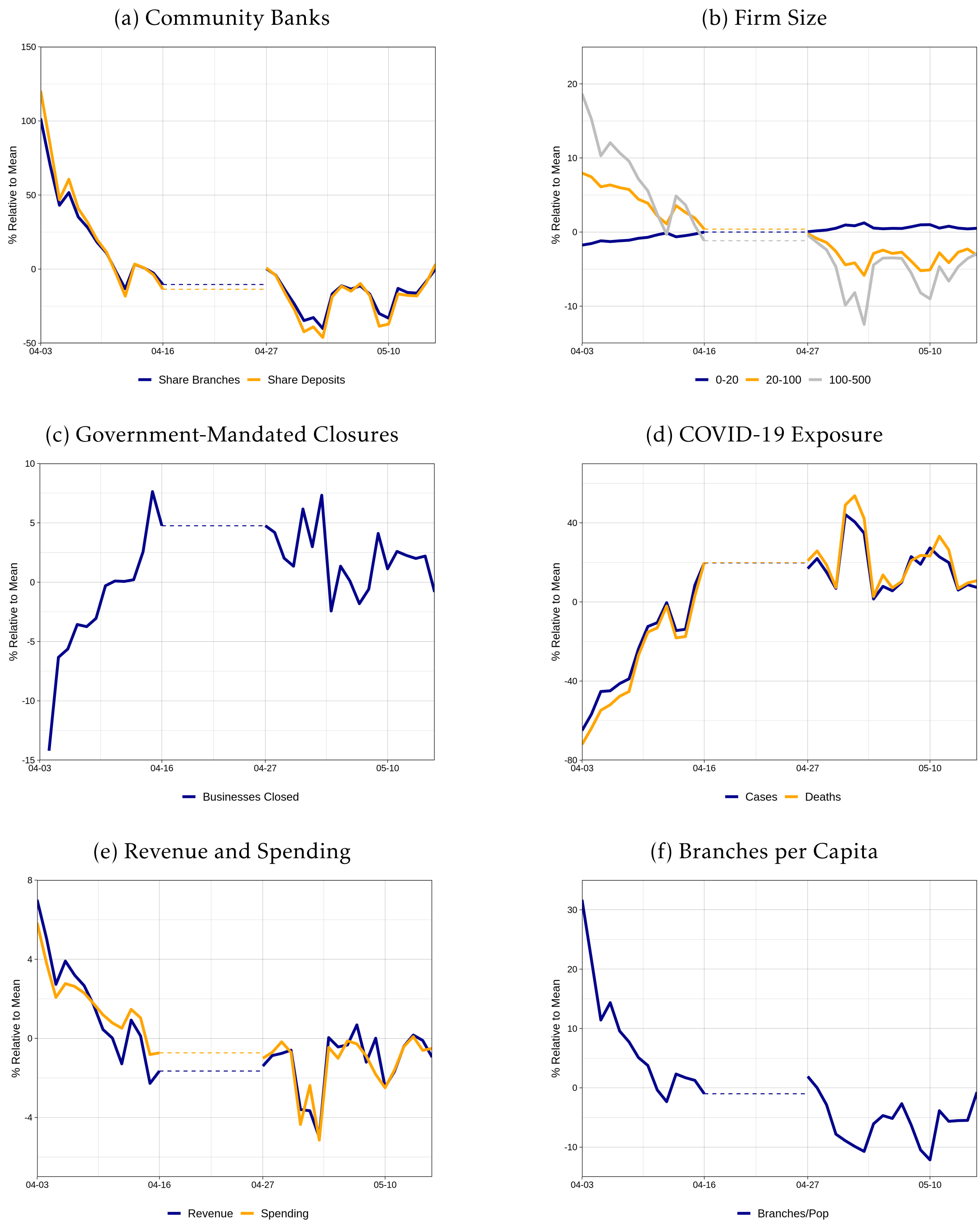
Note: Data from the SBA/Treasury February 2021 Release and Call Reports. *PPPE* is computed as in Granja et al. (2020). It is the symmetric difference of PPP loans and small-business lending (SBL) from schedule RC-C, Part II of the Call Reports. Mathematically, $PPPE_b = 0.5 \times \frac{\text{Share PPP} - \text{Share SBL}}{\text{Share PPP} + \text{Share SBL}}$ for the volume of loans. The share of PPP is computed from PPP loans made in the first phase (until April 16, 2020). Log(Assets) is the natural logarithm of assets in 2019Q4 from the Call Reports. Each dot represents an individual bank, and the dark blue dots are the conditional averages of PPPE by Log(Assets). We use the FDIC's database of community banks to establish which financial institutions are community banks. An institution is a community bank if it satisfies various criteria (see, for instance, https://www.fdic.gov/resources/community-banking/report/2020/2020-cbi-study-app-a.pdf). Overall, community banks are those that provide loans and deposits and have assets below a certain a threshold ($ 1.65 billion in 2019), although there are exceptions to this rule.

Table 3: Bank C&I and PPP Lending from Call Reports in 2020, Quarterly

		Change from 2019Q4			
Relative to Assets (p.p.)	Baseline	Q1	Q2	Q3	Q4
		Panel A. Unweighted			
C&I Credit	11.36	0.20	6.54	6.87	5.30
C&I Loans	8.16	0.20	6.14	6.33	4.65
C&I Unused	3.20	0.00	0.40	0.54	0.65
PPP	0.00	0.00	6.34	6.48	4.56
C&I Loans - PPP	8.16	0.20	-0.20	-0.15	0.09
		Panel B. Weighted by Assets			
C&I Credit	16.07	0.10	5.78	6.18	5.28
C&I Loans	10.35	0.57	5.49	5.60	4.55
C&I Unused	5.72	-0.46	0.29	0.57	0.72
PPP	0.00	0.03	5.71	5.83	4.58
C&I Loans - PPP	10.35	0.57	-0.22	-0.23	-0.02
Observations	4,970	4,970	4,970	4,970	4,970

Note: Data from merger-adjusted Call Reports, from 2019Q4 through 2020Q4. For each variable in rows X, we show the average, across banks, of $100 \times \frac{X - X_{2019-Q4}}{\text{Assets}_{2019-Q4}}$, that is, the average change in percentage points relative to assets in 2019Q4. C&I Credit refers to the extended loans and unused part of credit lines. Panel A shows a simple average across banks. Panel B shows the data averaged using assets in 2019Q4 as weights.

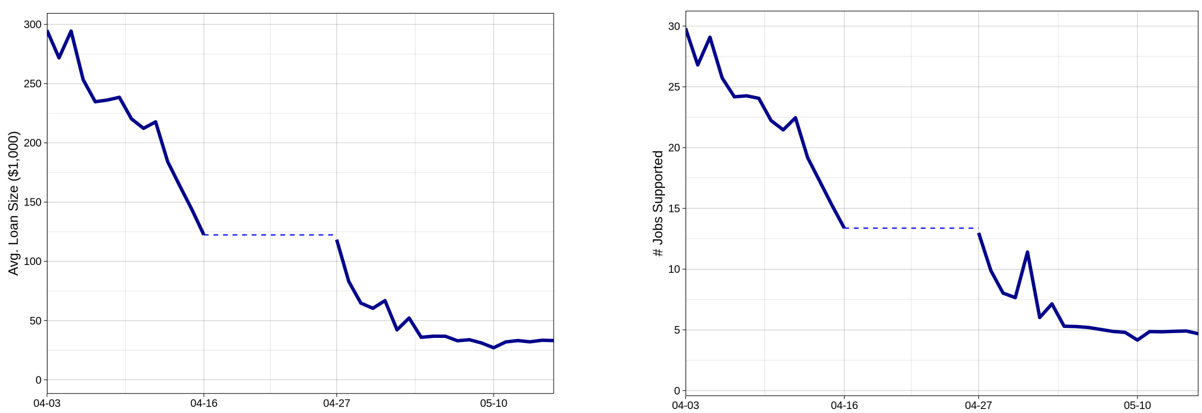
Figure 5: Average County Characteristics of PPP Recipients by Day



Note: PPP data come from the SBA/Treasury February 2021 PPP release. County-level data come from the Summary of Deposits (Panel A and F), County Business Patterns (Panel B), and Chetty et al. (2020*a*) (Panels C–E). For a baseline county characteristic X_0 (e.g., cumulative revenue shortfall from January to April 2), at county c, we plot percentage deviations from the mean (over time) of the weighted average of a county characteristics using PPP fund allocations as weights. The variables X_0 we use are the share of branches and deposits held in community banks (Panel A, see definition in Figure 4); the share of firms with 0 to 20, 20 to 100, and 100 to 500 employees (Panel B); a dummy that is 1 for counties in states with government-mandated closures (Panel C); COVID-19 cases and deaths *per capita* (Panel D); cumulative changes in revenue, spending, and employment (Panel E); and bank branches per capita (Panel F).

Figure 6: PPP Loans by Day: Average Loan Size and Jobs Reported

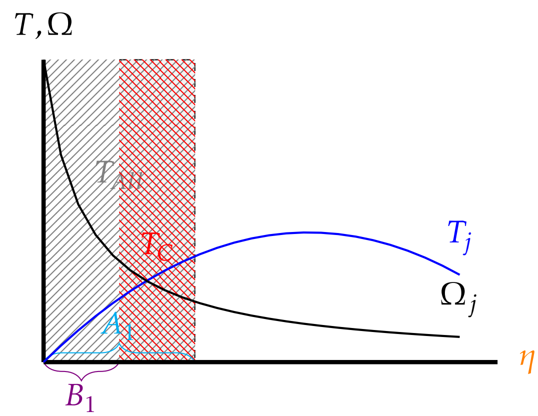
(a) Loan Size ($1,000) (b) Jobs Reported (Average)



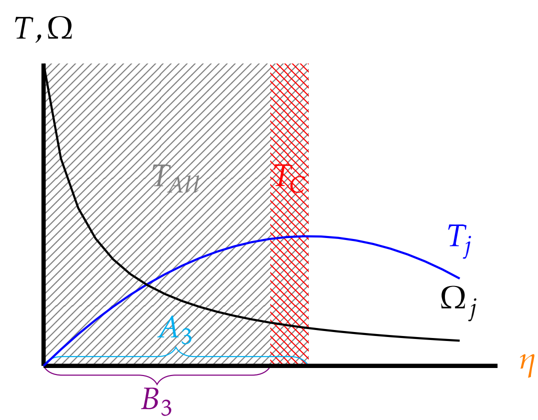
Note: SBA/Treasury February 2021 PPP release. Average size (in thousands of dollars) and reported number of jobs saved of PPP loans approved by day, from April 3,2020 (CARES Act) through May 15, 2020 (non-cumulative).

Figure 7: PPP Program Evolution and Identification of Treatment Effects

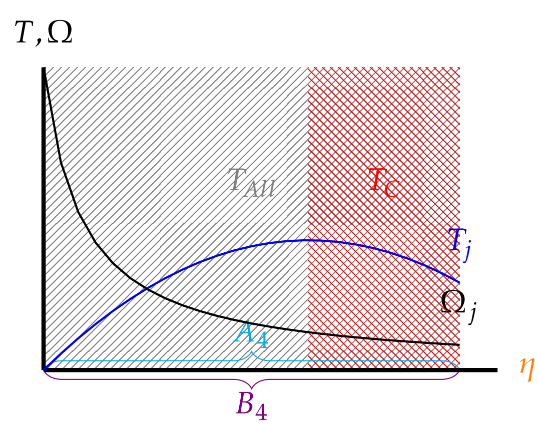
(a) First Phase ($t = 1$)



(b) Third Phase ($t = 3$)



(c) Remainder of the Program ($t = 4$)



Note: This figure illustrates the difference between the overall effect of the PPP (T_{All}) and the effect on compliers (T_C at different stages of the program based on stylized curves of treatment effects (T_j) and bank profits (Ω_j) as a function of firm characteristics η.

Appendix

A. Data

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

For our analysis at the bank- and county-bank levels, we merge the lenders in the PPP release by name with those institutions that were active in 2020 and registered in the National Information Center database (which includes, among others, commercial banks and credit unions). We are able to match 94 percent of the number and 95 percent of the volume of PPP loans. From the Call Reports, we obtain financial characteristics of all banks, the outstanding volume of small business loans (overall) and the outstanding volume in the PPP program. Within the set of banks that file Call Reports, 846 out of 4,970 had no outstanding PPP loans in 2020Q2. To check the quality of our merge procedure, we compare the PPP volume from the SBA/Treasury release on June 30, 2020, with that from the Call Report in 2020Q2. We find that the two alternative measures of PPP disbursement by bank are very close to each other. The correlation between them is 0.99. Additionally, for banks that have a zero amount of PPP loans outstanding in the Call Reports, our procedure does not match any loans from the PPP loan-level data.

We use the high-frequency (daily) data from Chetty et al. (2020*a*) to obtain county-level measures of employment, revenue, spending, COVID-19 cases and deaths, mobility and unemployment insurance claims. For details on the data collection, see Chetty et al. (2020*a*). From the Small Business Pulse Survey (SBPS), we obtain firms' application and approval status in the program (for a small subset of firms). From the Federal Reserve's H8 Schedule, we obtain C&I lending at a weekly frequency, broken down by large (top 25) and small banks.