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# What Do 25 Million Records of Small Businesses Say about the Effects of the PPP?

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**Abstract:**
We utilize Dun & Bradstreet data on firms' financial condition to examine the allocation of Paycheck Protection Program (PPP) loans and their impact. Three main findings emerge. First, firms in better financial condition prior to the COVID outbreak were advantaged in the allocation of PPP loans. Second, firms' financial condition improved significantly and persistently after receiving a loan, and this effect was more pronounced among the smaller and less financially sound firms. Third, we demonstrate empirically that the heterogeneity in firms' financial condition must be accounted for to correctly identify and estimate the overall effect of the PPP.

**JEL Classifications:** H81 ,G28 ,J21 ,E24
**Keywords:** Paycheck Protection Program, small business lending, financial frictions

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# 1 Introduction

The COVID-19 pandemic led to an unprecedented and precipitous contraction in economic activity that struck small businesses especially hard (see, for example, Bartik et al., 2020a). In response, Congress created the novel Paycheck Protection Program (PPP), a government-guaranteed loan and grant program to small and medium-sized businesses, with the goal of preserving jobs at those firms. In this study, we use a database that reports the financial condition and overall commercial viability of nearly 25 million firms—making it the most comprehensive commercial database of private businesses in the United States—to answer three questions.[1] First, how did the PPP allocation differ across firms with disparate pre-COVID financial conditions? Second, how did receiving the low-cost liquidity injection through a PPP loan affect a firm's financial condition in the short and medium runs, and how did the effects depend on firms' pre-COVID health? Third, how does accounting for firms' pre-COVID financial condition affect the estimated effects of the PPP on employment?

The PPP disbursed roughly \$800 billion in loans in two separate rounds.[2] The first round ran from April 3 through August 8, 2020, approving more than 5 million loans amounting to a total of just over \$525 billion. The first round is the focus of our analysis in the main text because we can map the majority of those borrowers to the Dun & Bradstreet (D&B) database and study subsequent effects of those loans over a longer period.[3] Only small businesses with 500 or fewer employees were eligible for the program.[4] The loans were fully guaranteed by the government, and the maximum loan amount was 2.5 times a firm's average monthly payroll costs in the preceding year, up to \$10 million. PPP loans did not require collateral or personal guarantees and would be fully forgiven if funds were spent in accordance with the rules, such as those on permitted expenses (chiefly payroll) and on maintaining employment levels to the extent feasible.[5]

The first round of the PPP comprised two disjointed stages due to a temporary funding gap. The first stage ran from April 3 to April 16, 2020, when the initial funding was exhausted owing to enormous excess demand. Additional funding did not arrive until 10 days later. Banks resumed approving PPP applications on April 27, and the second stage of the program ran through August 8, 2020. From April 27 to May 1, 2020, PPP loans

[1]The data are compiled by Dun & Bradstreet (D&B), effectively a commercial credit bureau and the leading provider of payment records and business credit scores. For years now, every government contractor has been required to obtain a D&B ID to be eligible to bid for contracts.

[2]See A.2 for additional relevant details of the PPP.

[3]We confirm that patterns found among the 2020 borrowers are echoed among the 2021 borrowers, which received PPP loans from January 11 through May 31, 2021 (see Appendix D).

[4]There were some exceptions to this rule, such as for businesses in the accommodation and food services industry.

[5]Current data show that more than 95 percent of the total PPP loan value has been forgiven.

were still made at a fast pace, in large part to clear the backlog of applications that had accumulated before the initial PPP funding was depleted. By May 2, 2020, demand for PPP loans was mostly satiated. Of the total first-round appropriation of $670 billion for the PPP, $145 billion remained when the program closed on August 8, 2020, indicating that all of the demand up to that point had been satisfied. We thus define three distinct phases in the first round of the program. The first phase covers April 3 through April 16, 2020; the second phase covers April 27 through May 1, 2020; and the third phase encompasses the remainder of the 2020 PPP. Each loan and the associated borrower are referenced by the corresponding phase.

Our answer to the first question is yes, more creditworthy firms were advantaged—they were more likely to receive a PPP loan and to receive it earlier. Firms that received PPP loans in Phase 1 were 18 percent less risky than those that received PPP loans in Phase 3, even among firms that operated in the same state, industry, age group, size group, and other, even more stringent within-group comparisons (such as county, Zip code, and three- and four-digit North American Industry Classification System [NAICS]). Those Phase-3 borrowers were 26 percent less risky than firms that did not receive PPP loans in 2020.

We answer the second question by showing that a PPP loan significantly improved the recipient firm's financial condition—on average, it led to an 18 percent reduction in credit risk when we use the difference-in-differences estimator à la Sun and Abraham (2021). Moreover, later loan recipients exhibited greater improvement compared with earlier recipients within the same state, industry, age group, and size group—and even more so if we further restrict the comparison to firms with the same pre-COVID financial and commercial viability. In addition, the treatment effect's heterogeneity is also sizable across firms with different pre-COVID viability: The ex ante least viable firms experienced a 22 to 29 percent larger reduction in credit risk after receiving a PPP loan in 2020 compared with the ex ante most viable firms. This disparity in credit risk reduction is 1.25 to 1.6 times the average risk reduction associated with a PPP loan.

Finally, we demonstrate that accounting for firms' pre-COVID financial condition makes a qualitative difference for estimates of the PPP's effects on employment. To be comparable with most of the other empirical studies, we conduct this part of our analysis at the county level.[6] We show that once we account for firms' pre-COVID financial and commercial viability, an instrumental variable for PPP allocation based on community banks' share of branches in a county (proposed in studies such as Faulkender et al., 2021) loses its explanatory power for the allocation of PPP loans and subsequent employment recovery in the county. This is because, at the county level, the community banks' share of branches is

[6] We also provide firm-level evidence that confirms our county-level findings.

strongly correlated with firms' average pre-COVID financial condition. We reach a qualitatively similar conclusion regarding the instrument developed in Granja et al. (2022), which relies on the gap between a bank's PPP lending and its pre-COVID small-business lending.

Our paper joins two strands of literature, one on the impact of credit constraints on firms' real activity and the other on government loan-guarantee programs—a common form of intervention in credit markets.[7,8] More specifically, our paper contributes to a growing literature exploring the economic impact of the policy responses to COVID-19, in particular the PPP.[9] Few of the previous studies, however, explore how firms' pre-COVID financial health affected their outcome during the pandemic. We fill this gap by presenting robust evidence from a comprehensive data set of small businesses that the preexisting financial condition of firms is highly correlated with both the allocation of PPP loans and the subsequent effects on the borrowers.

Our results have far-reaching implications for any analysis estimating the effects of the PPP, or other government-guaranteed credit programs more generally, on firm outcomes.[10] First, we show that firms in better financial condition prior to the COVID outbreak were advantaged in the allocation of PPP loans. Importantly, once firms' pre-COVID financial health is accounted for, instrumental variables proposed in previous empirical studies tend to lose their relevance for the PPP allocation. Second, we show that firms' financial condition is an economically significant source of both selection into treatment and heterogeneity in treatment effects. Consequently, our findings imply that firms' financial condition and the resulting selection into treatment must be directly accounted for to arrive at estimates of the PPP's effects on a representative set of borrowers and not on a subset of firms that is endogenously determined.

Our paper is purposefully short. Our core message is consistent across different variables and regression specifications so that the main text can focus on a small set of key findings.

[7]Some classic studies of financial constraints include Myers and Majluf (1984), Holmstrom and Tirole (1997), and Kaplan and Zingales (1997). For a recent example, see Barrot and Nanda (2020), who also use the D&B data.

[8] Theoretical studies of loan-guarantee programs include Gale (1990, 1991), while empirical studies include Lelarge et al. (2010), Mullins and Toro (2016), Brown and Earle (2017), de Blasio et al. (2018), Bachas et al. (2021), Gonzalez-Uribe and Wang (2019), and Jean-Noël et al. (2020). (For a comprehensive review, see Beck et al., 2010).

[9]A partial list of such studies includes Humphries et al. (2020), Erel and Liebersohn (2022), Chodorow-Reich et al. (2022), Hassan et al. (2020), Elenev et al. (2020), Faria-e Castro (2021), Cororaton and Rosen (2021), Barrios et al. (2020), Balyuk et al. (2021), Bartlett and Morse (2021), Chetty et al. (2020), Li and Strahan (2021), and Amiram and Rabetti (2020). Hubbard and Strain (2020) use the D&B data to estimate the PPP's effect on employment.

[10]For the employment effects of the PPP at the firm and regional levels, see Dalton (2021), Kurmann et al. (2021), Faulkender et al. (2021), Granja et al. (2022), Autor et al. (2022a,b), Doniger and Kay (2021), Bartik et al. (2020b), and Joaquim and Netto (2021), among others.

All the robustness checks and extensions are left to the online appendices for interested readers. Most of the appendices conduct robustness exercises or apply the same analyses to other comparable outcome variables. All of our main findings are corroborated. Two appendices are natural extensions of our primary analysis: One analyzes the first and second rounds of PPP loans jointly, while the other uses our firm-level risk score data to evaluate the instrumental variable (based on the temporary funding gap between Phase 1 and Phase 2 of the PPP) developed in Doniger and Kay (2021).

# 2 Data Description

The primary data source for our study is compiled by Dun & Bradstreet (D&B), effectively a commercial credit bureau. It is the most comprehensive commercial database of private businesses in the United States. D&B risk scores are generally considered the leading scores used by a broad range of lenders and suppliers. For instance, the primary credit score used by Bank of America for approving small-business loans is provided by D&B.[11] We provide additional details on the D&B data set and our cleaning procedure in Appendix A.

D&B risk scores enable us to adequately account for small businesses' financial health and commercial viability before and during the COVID outbreak. We focus our analysis of firm risk on the D&B Commercial Credit Score (CCS), which is similar to consumer credit scores. The CCS is a risk score that measures the risk of delinquency in the next 12 months. It ranges from 101 to 670, with each 40-point increase halving the risk of delinquency. All of the alternative risk scores in the D&B database are (unsurprisingly) highly correlated (see Table A.1). We focus on the CCS in the main text for three reasons. First, the CCS is available for the vast majority of firms. Second, it is the risk score that has the highest correlation (–0.73) with delinquency, and this correlation is persistent over time (Table A.2). We do not directly use delinquency because it is available for only 25 percent of the firms in our sample, which are also generally the most commercially active. Third, among the widely available risk scores, CCS is updated the most frequently. For robustness, we replicate all of our results with the other risk scores. When interpreting our estimates, we use the following formula to map a change in the CCS to a change in the delinquency probability:

$$\%\Delta Risk = \exp\{-\xi \cdot \Delta\text{CCS}\} - 1, \text{ where } \xi \equiv \ln(2)/40. \quad (1)$$

Thus, $\Delta$CCS = 40 corresponds to a 50 percent lower risk, and $\Delta$CCS = 12 approximately maps to an 18 percent reduction in risk.

[11] Source: https://www.bankofamerica.com/smallbusiness/education/business-credit-score/

Because of the impractically large scale of the D&B data set, we conduct firm-level analyses using a representative random sample of firms drawn by strata of geography (state), industry (two-digit NAICS), and size (range of employee count).[12] Our sample is taken from the population of US-domiciled firms (single location or headquarters) in the D&B database that were active as of January 2019 and employed one to 1,500 workers at the firm level. All our analyses use data at the firm level.[13] Firm attributes are measured either as of February 2020, our baseline pre-treatment period, or as 2019 averages for variables whose values fluctuate so often that the value in a single month may contain too much idiosyncratic variation. After all the steps of the cleaning procedure (detailed in the appendix), the final sample contains 413,865 firms for February 2020 and a balanced panel of 391,076 firms for our dynamic analysis. The balanced panel contains fewer firms because we consider only firms that are classified as commercially active and have valid data throughout the entire sample period. This ensures that our dynamic findings cannot be attributed to changes in the sample composition. All of our results are robust to using either sample. The comparison of our sample with the full D&B data is reported in Table A.3. The summary statistics of our regression sample are available in Table A.4.

The set of nonfinancial variables we use as controls are common to most studies: state, industry (by two-digit NAICS), firm-age bin (baseline value as of February 2020), and employment bin (baseline value). We divide firm age into three bins: $[0, 2]$ years, $(2, 10]$ years, and more than 10 years. Firm employment in February 2020 is categorized into nine bins: 1 employee, 2 to 4, 5 to 9, 10 to 19, 20 to 49, 50 to 99, 100 to 249, 250 to 499, and 500 or more employees. In addition, we include the following financial variables as controls: an indicator variable equal to 1 if the firm has payment records (available for only about 25 percent of the firms, which are generally the most commercially active), and the average D&B Viability Score (VS) in 2019.[14] The VS is constructed as the most comprehensive measure of a firm's odds of survival— it is a discrete variable, and the higher the value (ranging from 1 to 9), the higher the probability that a company will no longer be in business within the next 12 months. Since we take the average value in 2019, it is a nearly continuous control variable. Although the D&B data are available monthly, we conduct our analysis at the quarterly frequency because quarterly averages smooth out occasional transitory fluctuations from month to month in some risk-score variables. To align the event time definition with the timing of the COVID shock and PPP allocations, we define quarters as follows: quarter $t = -1$ in our analysis corresponds to December 2019 through February 2020, while $t = 0$ corresponds to

[12] We have verified all the findings using a separate random sample. Results are available upon request.

[13] More precisely, we conduct our analysis at the enterprise level, and we refer to enterprise as "firm" for short. Some variables, such as D&B risk scores, are measured only at the firm level.

[14] We define payment records to include either D&B's Paydex score or delinquency data.

March 2020 through May 2020, and so on.

We also use data from the Small Business Administration/Treasury on PPP loans (July 2021 vintage), which include complete loan-level data for the program. We used D&B's proprietary name-matching algorithm to identify PPP borrowers in the database by the firms' DUNS numbers (Data Universal Numbering System, the unique identifier in the D&B database). For PPP loans made in 2020, D&B's algorithm matches more than 91 percent of the program's loan volume (Table A.5). In our regional analysis, we also use data from the County Business Patterns (CBP 2020) and Local Area Unemployment Statistics (LAUS). For our analysis of the instrumental variables used in the literature, we also use the FDIC's Summary of Deposits (SOD) and Call Reports. All of the data are processed as in Joaquim and Netto (2021).

# 3 The Timing and Ultimate Allocation of PPP Loans Favored Firms in Better Financial Health

This section documents how the timing and ultimate allocation of PPP loans are correlated with firms' creditworthiness. More creditworthy firms were significantly more likely to receive PPP loans and receive them earlier, even when compared with similar firms.

To document this heterogeneity, we categorize firms into four subsets by PPP status and timing: those that did not receive PPP loans (referred to as non-borrowers), and those that received a PPP loan in each of the three phases of the program.[15] We then compare the average of a pre-COVID firm characteristic across borrowers in each phase (all relative to the non-borrowers). These conditional mean comparisons are implemented using the following regression:

$$y_{f,-1} = \sum_i \beta_i \text{Phase}_{f,i} + \zeta_s + \eta_n + \theta_a + \kappa_e + \lambda_d + \varepsilon_{f,-1}, \tag{2}$$

where $y_{f,-1}$ is a characteristic $y$ of firm $f$ at time $t = -1$, that is, pre-COVID. $\text{Phase}_{f,i}$ are indicator variables that equal 1 if firm $f$ received a loan in Phase $i \in \{1, 2, 3\}$ in 2020. The omitted category is firms without PPP loans. Our coefficients of interest are the $\beta_i$'s, that is, the difference between phase-$i$ borrowers' average characteristic $y$ and non-borrowers' average $y$. Note that Equation (2) does *not* use treatment timing to explain pre-COVID firm characteristics. Rather, the $\beta_i$'s are simply differences in conditional means whose estimation is facilitated by a regression. We compare the statistical significance and magnitude of these conditional mean differences with different conditioning variables (that is, fixed effects

[15] Recall the three phases correspond to April 3 through 16, April 27 through May 1, and May 2 through August 8, 2020.

and controls). The full set of fixed effects covers state ($\zeta_s$), industry ($\eta_n$), age bin ($\theta_a$), employment bin ($\kappa_e$), and an indicator variable for how active a firm was prior to COVID ($\lambda_{d,-1}$, equal to 1 if payment records were available at $t = -1$). We cluster standard errors at the state-industry level.

Estimates of the conditional mean differences for CCS from Equation (2) are reported in Table 1. Estimates with no fixed effect (column 1) reveal that Phase-1 borrowers were approximately half as risky as firms that did not receive PPP loans in 2020.[16] By comparison, firms that received PPP loans later were, on average, less creditworthy prior to the onset of COVID. For instance, Phase-3 borrowers were 35 percent more risky relative to Phase-1 borrowers, although these late borrowers were still about 30 percent less risky than the non-borrowers. Note that these estimates of the CCS differential between borrowers and non-borrowers likely understate the true difference because we designate as PPP borrowers only those firms matched in the D&B data with high confidence. Thus, some of the 2020 PPP borrowers are tagged as non-borrowers in the regressions, and they likely drive up the mean CCS (that is, reduce the perceived risk) for the omitted category. The relative riskiness across 2020 borrowers in the three phases versus non-borrowers remains economically meaningful even after we control for state, industry, age bins, and employment bins (as independent terms or interacted terms), as reported in columns 2 and 3 of Table 1. Importantly, our results remain robust if we include county, Zip-code, three-digit, or four-digit NAICS fixed effects (see Table B.1).

Perhaps not surprisingly, the difference in pre-COVID creditworthiness across firms by PPP status and timing narrows notably once we include the indicator for whether a firm has payment records (column 4), since $\beta_i$ now estimates the relative mean of CCS points within each set of more comparable firms. The cross-firm difference in baseline CCS shrinks somewhat more if we instead account for each firm's pre-COVID financial condition using its 2019 monthly average Viability Score, $VS_{f,2019}$ (column 5).[17] When both the data signal indicator and the 2019 mean Viability Score are controlled for (column 6), the cross-phase heterogeneity in firm creditworthiness becomes much smaller economically. Phase-1 borrowers were only 8 percent less risky relative to Phase-3 borrowers, and the latter were a mere 3 percent less risky than the non-borrowers.

It should be noted that the findings reported in columns 5 and 6 are to be expected, because the Viability Score on the RHS and the CCS on the LHS are highly correlated.

[16] This difference also corresponds to a sizable portion, about 60 percent, of the cross-firm standard deviation of CCS in our sample, which is approximately 72 at $t = -1$.

[17] Our results remain quantitatively the same if we instead use the categorical (1 to 9) measure of the Viability Score, or the continuous underlying raw Viability Score Points (VS points, ranging from 101 to 800), or any other financial indicators, all at $t = -1$, or even lagged CCS.

Thus, not surprisingly, controlling for the former absorbs most of the variation in the latter. But this simply highlights the key message we want to convey: The cross-phase variations in borrower creditworthiness are not accounted for by any of the typical controls used in the other studies (that is, geography, industry, firm age, and firm size), and only variables specifically compiled to measure firms' financial health and viability can account for the sizable heterogeneity in this dimension across PPP borrowers. At the same time, the cross-phase differences in average CCS (that is, $\beta_i$'s from Equation (2)) do not necessarily have a causal interpretation, that is, a firm in better pre-COVID financial condition was more likely to receive a PPP loan and receive it earlier not necessarily because the bank favored it for its better financial health. We carry out an extensive set of robustness checks for all of the above findings in Appendix B. Similar results are obtained using weights that match the census data (Table B.2), for alternative subsets of firms (Table B.3), and for other dependent variables (Figures B.1, B.2 and B.3).

# 4 The PPP Improved Firms' Financial Condition

This section examines how the receipt of a PPP loan and its timing affected a borrower's financial condition. We first estimate the following two-way fixed-effects (TWFE) regression at the firm $f$ and quarter $t$ level.

$$y_{f,t} = \sum_{t \neq -1} \sum_{i} \delta_{i,t} \text{Phase}_{f,i} + \alpha_f + \zeta_{s,t} + \eta_{n,t} + \theta_{a,t} + \kappa_{e,t} + \lambda_{p,t} + \mu_t \cdot \text{VS}_{f,2019} + \varepsilon_{f,t}, \tag{3}$$

where $y_{f,t}$ is a characteristic $y$ of firm $f$ in calendar quarter $t$, and $\text{Phase}_{f,i}$ are the same PPP-timing indicators as in Equation (2). The omitted category is again firms without PPP loans in 2020. Our coefficients of interest are the $\delta_{i,t}$'s on the interaction of time with PPP phase indicators, which measure the difference in calendar quarter $t$ relative to $t = -1$ (December 2019 – February 2020) in borrowers' financial condition relative to the control group of non-borrowers in 2020.[18] As for Equation (2) above, we estimate $\delta_{i,t}$ with several sets of fixed effects. The fully saturated specification (shown in Equation (3)) includes: firm ($\alpha_f$), state-time ($\zeta_{s,t}$), industry-time ($\eta_{n,t}$), age-bin-time ($\theta_{a,t}$), employment-bin-time ($\kappa_{e,t}$), availability-of-payment-records-time ($\lambda_{p,t}$) and time interacted with the firm's 2019 average Viability Score, $\text{VS}_{f,2019}$.

The estimation results are depicted in Figure 1. The regression underlying Panel A includes the firm, state-time, industry-time, age-bin-time, and employment-bin-time fixed effects, while the regression behind Panel B includes all the fixed effects. Accounting for the

[18] We take into account the staggered design of the PPP explicitly in the next subsection.

heterogeneity in pre-COVID viability has two main effects. First, it compresses the disparity in the PPP's treatment effects across borrowers in different phases, with the effects on the later two phases now statistically indistinguishable. Second, it also results in noticeably larger treatment effects, averaging five CCS points higher across the phases.[19] These patterns indicate some degree of "diminishing returns" to the effect of the PPP: The Phase-1 borrowers had better credit scores (including the CCS) than the later borrowers prior to the onset of COVID, and their CCS thus did not increase as much after they received the PPP loans relative to the CCS of the later borrowers and the non-borrowers. However, when the comparison is made among firms with the same pre-COVID Viability Score, the relative improvement experienced by the PPP borrowers becomes more pronounced.

To account for the staggered design of PPP receipt and provide a consistent aggregation of the treatment effects across firms that received loans at different times, we apply the method of Sun and Abraham (2021) (see Appendix C for details).[20] It uses a regression with an exhaustive set of group-by-event-time (that is, time relative to treatment) indicators. In our case, group is defined by the quarter of PPP receipt, and event time is measured in quarters relative to PPP receipt. The treatment effect for a given event quarter (relative to PPP receipt) is the weighted average of coefficients across different groups, with the number of firms in each group as weights. Estimates of the treatment effect on the treated by event quarter, plotted in Figure 2, reveal that receiving a PPP loan is associated with an increase in CCS of more than four points after one quarter and nearly 10 points after two quarters. The gain largely flattens out at around 12 points after four quarters, which corresponds to a roughly 18 percent reduction in delinquency risk.[21]

We further explore whether there are clear heterogeneous effects of receiving PPP loans, depending on the quarter (March through May versus June through August 2020) of a firm's PPP loan receipt and its pre-COVID viability. To facilitate the exposition of these estimates, we discretize the mean 2019 Viability Score $\mathrm{VS}_{f,2019}$ into four categories. The benchmark (omitted) category consists of the most viable firms, defined as $\mathrm{VS}_{f,2019} \leq 2$. The other categories are defined as high ($(2, 4]$) , medium ($(4, 6]$), and low ($(6, 9]$) viability. Let $q$

[19]The increase is the largest for the first two phases. For instance, the PPP's effect on Phase-1 borrowers' CCS rises from 6 points at $t = 3$ (Panel A) to nearly 12 points once viability-time controls are added (Panel B).

[20]The receipt of a PPP loan in 2020 is a staggered treatment even at the quarterly frequency. Recall that the quarter $t = 0$ comprises March through May 2020. Thus, those Phase-3 borrowers who received PPP loans in June through August 2020 were "treated" in quarter $t = 1$. A flourishing literature demonstrates that TWFE estimations (such as that specified in Equation (3)) applied to staggered treatments can potentially lead to nontrivial biases. See Roth et al. (2022) for an excellent review. The problems highlighted in this literature are mild in our setting, since the vast majority of borrowers received treatment at $t = 0$, and there is a large group of non-treated firms.

[21]For reference, in the pre-COVID period, the average within-firm CCS standard deviation was 9.

denote the quarter a firm received a PPP loan and $v$ its (discretized) pre-COVID viability. We estimate the following equation:

$$y_{f,t} = \sum_{\tau \neq -1} \sum_{v \neq 1} \sum_{q} \varphi_{q,v,\tau} D_{q,v,\tau} + \sigma_{q,t} + \mu_{v,t} + \alpha_f + \zeta_{s,t} + \eta_{n,t} + \theta_{a,t} + \kappa_{e,t} + \varepsilon_{f,t}, \tag{4}$$

where $D_{q,v,\tau}$ denotes indicator variables for receipt quarter ($q$)-baseline viability bin $v$-event time ($\tau$), $\sigma_{q,t}$ are receipt quarter-time fixed effects, and $\mu_{v,t}$ are binned viability-time fixed effects.[22] To facilitate the interpretation of the heterogeneous effects, we report the estimate of $\varphi_{q,v,\tau}$ averaged over event quarters $\tau$, denoted as $\overline{\varphi}_{q,v}$. These estimates (shown in Table 2) reveal substantial heterogeneity in the PPP's treatment effects: Firms less viable before the pandemic experienced notably larger reductions in delinquency risk after receiving PPP funds. In terms of magnitude, relative to the most viable firms in 2019, the least viable firms saw a 50 percent reduction in their delinquency risk after receiving PPP loans.

Appendix C presents an extensive set of robustness checks. Qualitatively similar patterns are found in a simple comparison of unconditional means by group (Figure C.1), for other risk scores (Figure C.2), using weights that match census data (Figure C.3), and for the subset of employer small businesses (Figure C.4). We further corroborate the above findings by examining the PPP's effect on more discrete outcomes that are unlikely the result of noisy fluctuations in risk scores. We confirm that PPP borrowers were more likely to experience large positive changes in CCS (Table C.1) while less likely to become classified as commercially inactive or distressed (Figure C.5).

# 5 Effect of the PPP on Employment Must Account for Heterogeneity in Firms' Financial Condition

We now examine how estimates of the PPP's effects on employment may change once we adequately account for the heterogeneity in firms' pre-COVID credit and Viability Scores. Our findings suggest that results in existing studies may need to be reassessed. For this analysis, we use Bureau of Labor Statistics (BLS) employment data and D&B risk scores aggregated to the county level. Two reasons motivate our decision to conduct a county-level analysis. First, although firms' credit and viability scores are regularly updated at least once each quarter in the D&B data, employee counts are only infrequently or irregularly updated, especially for small firms (for more detail, see Wang et al., 2021; Barnatchez et al., 2017), and

[22] To be consistent with the definition of the $D_{q,v,\tau}$'s, here we replace the continuous control of viability in Equation (3) ($\text{VS}_{f,2019}$) with the binned viability indicator interacted with time.

this can potentially bias the estimates of the employment effects of the program. Second, using BLS data enables us to directly compare our estimates with those from studies that use variation in PPP allocations and employment growth by locality to evaluate the effects of the program. Nevertheless, the county-level results are corroborated with firm-level data (Appendix F).

To explore the causal effects of PPP funding, we examine how accounting for firms' pre-COVID financial condition alters the dependence of PPP status and timing on instrumental variables (IVs) proposed in previous studies. The primary IV, analyzed in the main text, is the share of bank branches in a county belonging to community banks (used in Faulkender et al., 2021)). It is argued that the community bank share is pre-determined and thus satisfies the exclusion restriction, and its relevance for PPP lending rests on the idea that small-business lending is local (see, for example, Granja et al., 2018; Li and Strahan, 2021), while community banks were faster in disbursing PPP loans (as reviewed above). Two other IVs are likewise analyzed in the appendix: the PPP exposure IV used in Granja et al. (2022) (Appendix F.1) and the share of 10-day funding delay proposed in Doniger and Kay (2021) (Appendix E).[23]

First, we estimate a first-stage regression at the county ($c$) and month ($m$) level to assess how the explanatory power of the community bank share in a county for the intensity of PPP lending at the county level diminishes once we control for the commercial viability of firms in that county:

$$\frac{\text{Cumulative PPP Amount}_{c,m}}{\text{Weeks of Payroll (Eligible)}_{c,m}} = \zeta_{s,m} + \beta_m CB_c + \delta_m \overline{VS}_{c,2019} + \varepsilon_{c,m}. \tag{5}$$

The dependent variable is the total volume of PPP loans relative to the 2019 weekly payroll in eligible firms. We normalize PPP allocation relative to payroll because the PPP loan size is based on 2019 payroll. $\zeta_{s,m}$ are state-month fixed effects. $CB_c$ is the community bank share in a county (normalized to have a standard deviation of 1). $\overline{VS}_{c,2019}$ is the 2019 average Viability Score (that is, $\text{VS}_{f,2019}$ in Equation (2)) for all firms in county $c$. Our objective is to estimate how much $\beta_m$ changes once we control for the average firm viability in that county. Using this single control of mean risk score is meant to illustrate parsimoniously the influence of firms' financial and commercial viability on PPP receipt and in turn on employment recovery.[24]

[23]Once pre-COVID firm financial health is accounted for, the PPP exposure IV loses its ability to explain the PPP uptake at the firm level and the number of PPP loans at the county level, but it retains a fraction of its explanatory power for the amount of PPP lending at the county level. Appendix F.1 explains the reasons for this pattern.

[24]We could use multiple nonlinear functions (such as values at different percentiles) of $\text{VS}_{f,2019}$ as controls,

Estimates of $\beta_m$ in Equation (5), plotted in Panel (a) of Figure 3, reveal that once we account for firms' pre-COVID viability ($\overline{VS}_{c,2019}$), counties with higher shares of community bank branches did not receive more PPP loans except in the first month of the program (April 2020). Even for April 2020, community banks' outperformance shrinks by more than two-thirds once firm viability is accounted for. These findings have two implications. First, the community bank share loses much of its explanatory power in the first-stage regression, thus it potentially no longer provides enough variation to estimate the effect of the PPP. Second, its loss of explanatory power implies that the community bank share is strongly correlated with firms' financial health at the county level, violating the exclusion restriction needed for the community bank share to be a valid IV. We confirm in an equivalent first-stage regression at the firm level, reported in Table F.3, that the county-level community bank share is also highly correlated with individual firms' financial condition in that county. Moreover, firms' average pre-COVID financial condition is a much stronger predictor of PPP allocation when directly compared with the community bank share (Figure F.1).

Furthermore, we estimate how the PPP's effect on employment recovery in a county may change once we account for $\overline{VS}_{c,2019}$, using the following estimation:

$$\%\mathrm{Emp}_{c,m} = \alpha_c + \zeta_{s,m} + \sum_{m \neq -1} \beta_m CB_c + \delta_m \overline{VS}_{c,2019} + \varepsilon_{c,m}, \tag{6}$$

where $\%\mathrm{Emp}_{c,m}$ is employment in county $c$ in month $m$ relative to its pre-COVID labor force (in percent). The regressors are defined as in Equation (5). Estimates of $\beta_m$ in Equation (6), plotted in Panel B of Figure 3, show that the explanatory power of the community bank share vanishes once we account for firms' pre-COVID financial health. This is hardly surprising given the estimates from Equation (5) that the community bank share no longer explains PPP allocation once we control for firm viability.

Given the finding in Section 3 that firms in better financial condition before the COVID outbreak were advantaged in the allocation of PPP loans, this variable should be controlled for in estimating the PPP's effect on firm outcomes. The relevance of firms' pre-COVID financial condition does not per se invalidate the IVs proposed in the existing studies. For example, community banks could have facilitated access to the PPP for all small businesses, and those in better financial health benefited more than others. Findings reported in this section, however, demonstrate a strong correlation between the community bank share and firms' financial health, which means that we cannot identify the effect of the PPP using this or other similar IVs.

or include all the D&B risk scores as well as delinquency variables as additional controls, which would certainly further diminish the explanatory power of the community bank share.

# 6 Conclusion

We use a comprehensive database of businesses' financial condition to understand the allocation and effects of the Paycheck Protection Program (PPP), a government funding program of unprecedented scale that was launched in response to the COVID-19 crisis and designed to support jobs at small businesses. We show that the allocation of PPP loans favored firms in better financial condition at the onset of the pandemic, even after we account for a rich set of firm characteristics. Subsequently, PPP borrowers became 18 percent less risky four quarters after they received a PPP loan relative to their peers that did not receive a PPP loan in 2020, once this heterogeneity in firms' pre-COVID financial health is accounted for. Perhaps more importantly, once we control for firms' financial condition, instrumental variables used in the empirical literature lose their validity or relevance for identifying the effect of the program. Moreover, given the sizable selection into treatment and the associated heterogeneous treatment effects documented in this study, any IV strategies that do not take into account firms' financial condition would most likely capture the PPP's effect on a subset of firms that is endogenously determined and thus not representative of the population of PPP borrowers.

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Table 1: Pre-COVID Average Firm Commercial Credit Score (CCS) by 2020 PPP Loan Status and Timing

*Note:* This table shows the average differences in Commercial Credit Score (CCS) in the base period (February 2020) across different firms grouped by the timing of PPP receipt, estimated using Equation (2). For details on variable definitions, see Section 2. *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. Firms that did not receive a PPP loan include both eligible and ineligible firms in our analysis. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively. Sources: D&B and authors' calculations.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Phase 1 | 44.45*** | 29.87*** | 30.21*** | 14.41*** | 10.01*** | 6.085*** |
| | (0.8366) | (0.5799) | (0.5835) | (0.5826) | (0.4965) | (0.5077) |
| Phase 2 | 33.81*** | 25.88*** | 26.00*** | 11.92*** | 8.183*** | 4.620*** |
| | (0.7529) | (0.5818) | (0.5819) | (0.5496) | (0.5146) | (0.4943) |
| Phase 3 | 20.93*** | 17.86*** | 17.99*** | 6.932*** | 4.558*** | 1.712** |
| | (0.8840) | (0.7731) | (0.7849) | (0.7638) | (0.6725) | (0.6881) |
| *Fixed-effects* | | | | | | |
| State | | Yes | | Yes | Yes | Yes |
| Industry | | Yes | | Yes | Yes | Yes |
| Firm Age | | Yes | | Yes | Yes | Yes |
| Employment Bins | | Yes | | Yes | Yes | Yes |
| State-Ind.- Age-Emp. | | | Yes | | | |
| Payment Records | | | | Yes | | Yes |
| *Continous Controls* | | | | | | |
| Overall Via Rating (2019) | | | | | Yes | Yes |
| *Fit statistics* | | | | | | |
| Observations | 413,865 | 413,865 | 410,737 | 413,865 | 413,865 | 413,865 |
| $R^2$ | 0.03471 | 0.12174 | 0.15313 | 0.18524 | 0.28371 | 0.29147 |

Table 2: Heterogeneous Effects of the PPP on Commercial Credit Score (CCS) by Firm Pre-COVID Financial Condition

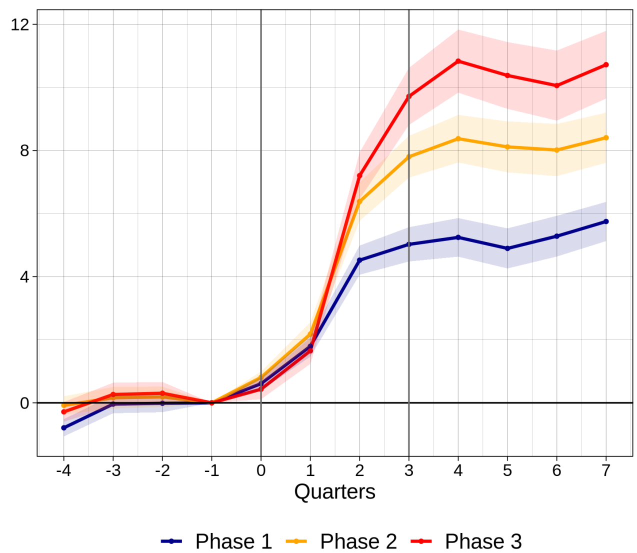
*Note*: This table presents the average effect of a PPP loan on CCS for receipt quarter $q$ with baseline Viability Score range $v$, with the average taken over all the post-PPP loan quarters, that is, $\hat{\bar{\varphi}}_{q,v}$, as defined in the main text. $PPP$ in Mar-May denotes borrowers receiving a loan in the first quarter (that is, March, April, or May 2020) of the PPP's operation, while $PPP$ in Jun-Aug denotes all the later borrowers in the first round. Firms are considered *high viability* when $VS_{f,2019} \in (2, 4]$, *medium viability* when $VS_{f,2019} \in (4, 6]$, and *low viability* when $VS_{f,2019} \in (6, 9]$. Within each quarter of PPP receipt, the omitted benchmark category consists of firms with the lowest Viability Scores ($VS \leq 2$), that is, the most viable firms. The estimation uses firm ($f$)-quarter ($t$) level data with two different sets of fixed effects as listed in the table. Robust standard errors two-way clustered at the state-industry and time level are in parentheses; ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively. Sources: D&B and authors' calculations.

| | (1) | (2) |
|---|---|---|
| $PPP$ in Mar-May, High Viability | $3.688^{***}$ | $3.674^{***}$ |
| | (0.4228) | (0.4296) |
| $PPP$ in Mar-May, Medium Viability | $11.89^{***}$ | $11.86^{***}$ |
| | (0.5300) | (0.5355) |
| $PPP$ in Mar-May, Low Viability | $20.88^{***}$ | $20.63^{***}$ |
| | (1.758) | (1.752) |
| $PPP$ in Jun-Aug, High Viability | $4.595^{***}$ | $4.576^{***}$ |
| | (1.682) | (1.675) |
| $PPP$ in Jun-Aug, Medium Viability | $13.24^{***}$ | $13.15^{***}$ |
| | (1.717) | (1.707) |
| $PPP$ in Jun-Aug, Low Viability | $15.86^{***}$ | $16.32^{***}$ |
| | (4.056) | (4.049) |
| *Fixed-effects* | | |
| Firm | Yes | Yes |
| Time | Yes | Yes |
| Time-PPP Quarter | Yes | Yes |
| Time-VS Bins | Yes | Yes |
| State-Time | | Yes |
| Industry-Time | | Yes |
| Employment Bins-Time | | Yes |
| Firm Age-Time | | Yes |
| *Fit statistics* | | |
| Observations | 4,656,787 | 4,656,787 |
| $R^2$ | 0.82965 | 0.82988 |

Figure 1: Effects of the PPP on Firms' Commercial Credit Score (CCS)

*Note*: These figures plot the average effect on firms' CCS of receiving a PPP loan by the timing of loan receipt (corresponding to $\delta_{i,t}$ in Equation (3)), estimated using firm-quarter ($t$) data with two different sets of controls. The regression underlying Panel A contains firm, state-time, industry-time, age-bin-time, and employment-bin-time fixed effects. The regression underlying Panel B contains additional fixed effects of time interacted with both the payment data indicator and the Viability Score. *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. The omitted category are firms that did not receive a PPP loan in 2020. The vertical line at $t = 0$ (which encompasses March through May 2020) denotes the first quarter when first-round PPP loans were made. The vertical line at $t = 3$ corresponds to the first quarter (December 2020 through February 2021) when second-round PPP loans were made. Shaded areas are 95% confidence bands; standard errors are two-way clustered at the state-industry and time levels. Sources: D&B and authors' calculations.

(a) Including Firm + Time + Non-Financial Fixed Effects




(b) Including All Fixed Effects (Fully Saturated)

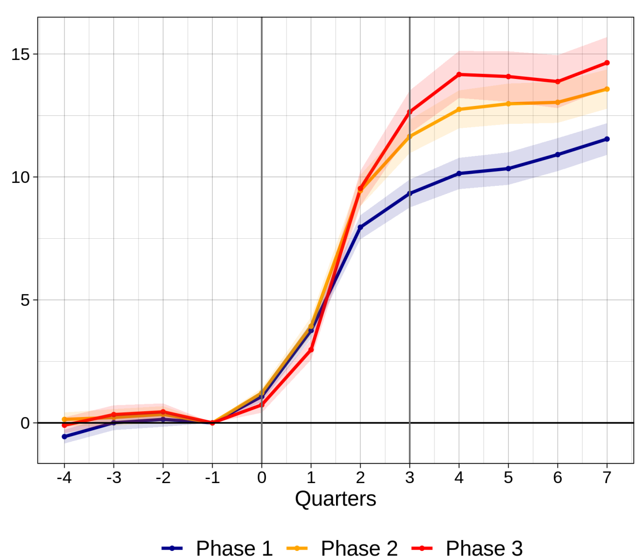


Figure 2: Effects of the PPP on Commercial Credit Score (CCS): Difference-in-Differences Estimates for Staggered Treatment

*Note*: This figure shows estimates of the average (across all PPP borrowers) effect over time $\tau$ (relative to the quarter of loan receipt $q$) of receiving a PPP loan. These correspond to estimates of $\overline{\psi}_\tau \equiv \sum_q w_q \psi_{q,\tau}$ in Equation (C.1) (based on Sun and Abraham (2021)). They are estimated using data at the firm ($f$)-quarter ($t$) level with all the fixed effects: state-time, industry-time, age-bin-time, employment-bin-time, indicator for availability-of-payment-records-time, and 2019 average Viability Score (continuous) interacted with time fixed effects. The dependent variable is CCS. Shaded areas are 95% confidence bands; standard errors are two-way clustered at the state-industry and time levels. The horizontal axis indexes event time, that is, time (quarter) relative to when when a firm received its 2020 PPP loan (marked by the vertical line at $t = 0$). A given event time thus maps to a different calendar time depending on the timing of a firm's PPP loan receipt. This plot corresponds to column 3 of Table C.2. Sources: D&B and authors' calculations.

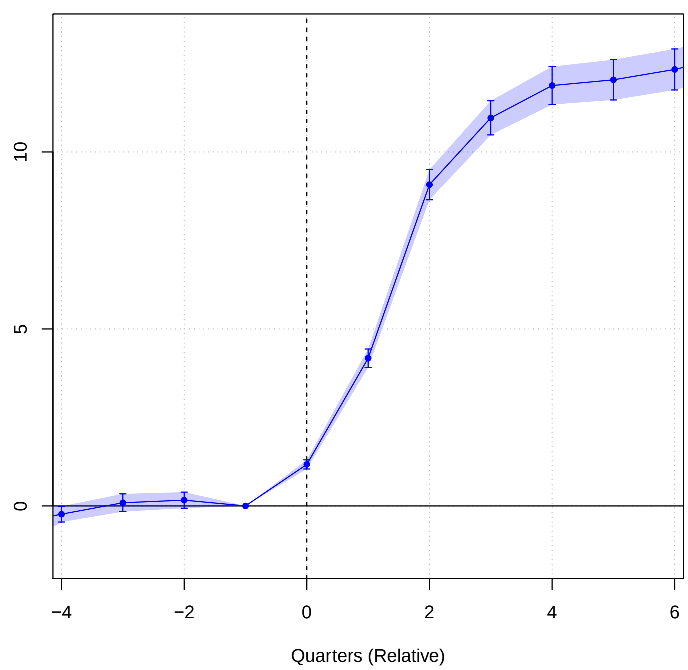
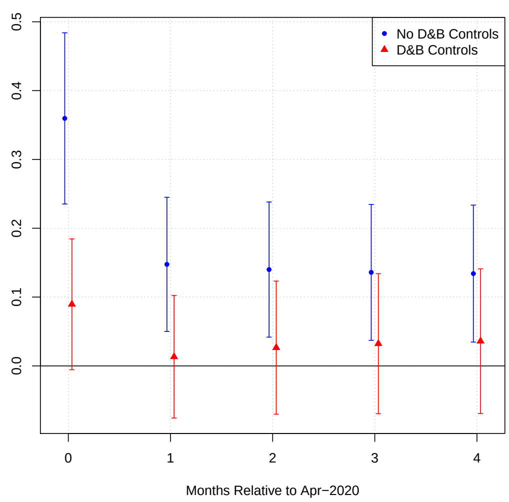
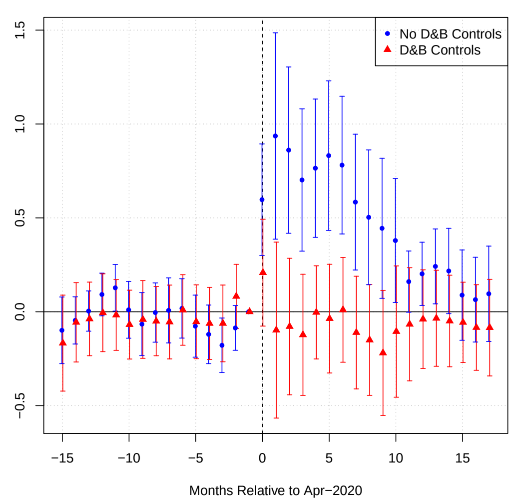


Figure 3: PPP Allocation and Its Effect on Employment: Instrumented by community bank Share

*Note:* This figure shows the estimated effect of community bank branches comprising a higher share of a county's bank branches on the amount of PPP loans received by small businesses in a county and the subsequent effect on the employment rate in that county. Specifically, the estimates correspond to $\beta_m$ in Equations (5) and (6), the coefficient on the independent variable equal to the share of branches in a county belonging to community banks, which is normalized to have a standard deviation of 1. The dependent variable underlying Panel A is the cumulative volume of PPP loans over weeks of payroll at firms eligible to receive PPP loans in county $c$ at time $t$ (shown in Equation (5)). The dependent variable underlying Panel B is employment rate (percentage points) (shown in Equation (6)). In both Panels A and B, we estimate two models using data at the county ($c$)-month ($m$) level: one with and the other without firms' average Viability Score during 2019 in that county, $\overline{VS}_{c,2019}$, as a control; both models include all the non-financial fixed effects (firm, state-time, industry-time, age-bin-time and employment-bin-time). Vertical bars depict 95% confidence bands; standard errors are clustered at the state level. Regressions are weighted across counties by the number of workers in eligible firms in 2019. The vertical line (at $t = 0$) represents April 2020. Sources: D&B, BLS, County Business Patterns, Summary of Deposits, and authors' calculations.




(a) Normalized PPP Amount




(b) Employment Rate (% pt.)

# Online Appendix — Not For Publication

## Table of Contents

# A Data Description: Additional Details

This section details the primary data source used in this study (the Dun & Bradstreet database), describes how we construct the stratified random sample and how we identify Paycheck Protection Program (PPP) borrowers in this database, provides details of the program, and reports the summary statistics for the full database as well as the random sample.

## A.1 The Dun & Bradstreet Database

The D&B database has contained, on average, nearly 25 million records of active businesses each month in recent years, making it the most comprehensive commercial database of private businesses in the United States. For our analysis, we exclude sole proprietorships, establishments with zero reported employees, and establishments with missing state and industry codes. For some specifications, we further restrict the sample to establishments with 3 to 500 employees to focus on employer businesses eligible for the program, because there is evidence that D&B tends to count the proprietor as an employee as well (see Barnatchez et al., 2017). Last, we exclude establishments with missing state and industry codes, which account for a rather small share of the data. In D&B data, establishments approximately maps into DUNS ID, some of which can refer to branches. D&B also provides HQ DUNS, which corresponds to the headquarter ID of each distinct enterprise according to their data. We conduct analysis using data aggregated up to the HQ DUNS level, which we refer to as "firm" for short.

D&B provides data on an array of firm attributes, although its primary comparative advantage lies in its maintaining payment records for a large portion of the businesses covered and assigning credit scores to nearly all of the businesses. In this respect, D&B is akin to a credit bureau for businesses. It updates the risk scores on an ongoing basis, based on the stream of signals it receives from a network of information providers (such as landlords, lenders, utility companies, suppliers, postal services, secretaries of state, etc.).

D&B credit risk scores are generally considered the leading scores used by a broad range of creditors (lenders and suppliers). Bank of America, Credit Karma, Brex, and many other popular online marketplaces for credit products highlight the importance of D&B credit scores of small businesses.[1] These risk scores thus enable us to adequately account for small businesses' financial health and commercial viability prior to and following the COVID outbreak. Furthermore, D&B data enable us to examine the impact of the PPP on small businesses in terms of their ability to pay bills on time and the evolution of their risk scores more generally.

**Commercial Credit Score**

The main indicator of firm risk used in our analysis—D&B's Commercial Credit Score (CCS)—measures the risk of delinquency in the next 12 months and can be regarded as the forward-looking counterpart

[1] Sources: https://www.creditkarma.com/advice/i/d-and-b-rating, https://www.brex.com/learn/loans-credit-scores/what-is-business-credit-score/

to the better known D&B Paydex score, which is a summary statistic of a firm's past payment behavior. Scores range from 101 to 670, with each 40-point increase halving the risk of delinquency. For example, a business with a score of 240 is half as risky as a business with a score of 200. When interpreting our estimates, we use the formula in (1) to convert a change in the CCS to a change in the delinquency probability.

**Other Risk Scores**

Beyond the CCS, we also consider two other risk scores compiled by D&B—Viability (Score) Points and the Financial Stress Score (FSS)—both of which are meant to be forward looking. We use the raw scores for all three; a higher value signifies lower risk (similar to a consumer credit score). As previously noted, these risk scores are influential and widely used by lenders and suppliers to gauge a small business's financial risk and commercial viability.

Viability (Score) Points, ranging from 101 to 800, is designed to be a comprehensive measure assessing a business's overall likelihood of going out of business (which includes becoming inactive or filing for bankruptcy) over the next 12 months.[2] The Financial Stress Score (also known as the Failure Score), ranging from 1,001 to 1,875, predicts the likelihood over the next 12 months that a business will incur financial distress (such as ceasing operations, leaving unpaid obligations to creditors, moving into receivership) or file for bankruptcy.

D&B also provides a discretized counterpart to Viability Points, called the Viability Score, which ranges from 1 (least risky) to 9 (most risky), akin to credit ratings. As described in the main text, we use the 2019 average of the Viability Score as a control in our regressions.[3] As we show later in this section, all of the risk scores are highly correlated.

**Payment Records**

The next set of variables records the degree of a business's payment delinquency (if it has been delinquent) over the preceding 3 to 24 months (depending on the variable). We focus on the more timely records over the preceding three months, specifically, the portion of the total amount owed over the most recent three months that is 31-plus or 61-plus days past due.[4]

As a sufficient statistic of these indicators for how punctually a business has been paying its bills, D&B compiles the Paydex score. The most commonly used version of Paydex is based on payment behavior over the preceding 24 months. Values of 80 and higher mean a firm pays on time; 70 equals 15 days

[2] Viability Points provide an essentially continuous measure, while the Viability Score (to be further explained below) provides a discretized measure of the out-of-business risk. In other words, the former is the more granular counterpart to the latter.

[3] The Viability Score is accompanied by a portfolio Viability Rating, which is a firm's score measured against those of its peers in the market segment defined by D&B, based on firm size and age bins (as summarized by the variable viability profile), as well as the quantity of data signals D&B receives on the given firm (as summarized by the variable viability data depth). We dispense of the Portfolio Rating because we apply our own set of fixed effects to obtain a firm's relative score or rating vis-á-vis its peers.

[4] We verify that the patterns found with these delinquency variables based on the *value* of bills due are similar to those using the portion past due based on the *number* of accounts, or the persistence (the portion of months over the preceding 12 months) of past due.

beyond terms; 60 equals 22 days beyond terms; 50 equals 30 days beyond terms; 40 equals 60 days beyond terms; 30 equals 90 days beyond terms; 20 equals 120 days beyond terms. A missing Paydex score (coded as 0 or 999) is due to an insufficient number of qualified transaction experiences and is thus much more prevalent among new firms or firms newly added to the D&B database. This score is available for somewhat fewer firms compared with the number of firms that have data for recent (over the preceding three months) payment records, because data are not consistently available over time for some firms.[5] The 24-month Paydex score is available for about 25 percent of the firms after January 2020 among mostly active firms, while payment records over the preceding three months are available for about one-third of the firms. Unlike the forward-looking modeled risk scores, the more commonly referenced Paydex score is backward looking, determined entirely by a firm's own payment records. As we show later in this section, all of the risk scores are highly correlated with payment records.

**Employment**

D&B offers two types of data on firm employment: (1) the actual employee count for a firm, either directly reported by the firm or gleaned from other sources, and (2) modeled (imputed) employment, which is estimated using D&B's proprietary model along with other data items D&B collects on the firm. About 25 percent of the firms report actual employment. Note, however, that the actual employment data are updated only with a lag and often at different times for different firms, or even any given firm, depending on the arrival of new information. This means that the actual employment may be out of date to varying degrees across firms. For that reason, we conduct our employment analysis at the regional level using data from the Bureau of Labor Statistics (BLS).

**Quarterly Aggregation and Smoothing**

To ensure that our estimates capture medium- and short-term effects and to minimize the influence of transitory but large fluctuations (mostly in risk scores) for a small number of firms, we remove monthly spikes in all the variables used in the analysis. For this purpose, we carry out the following operations for each variable and each firm: (1) compute the within-firm standard deviation over the entire sample period; (2) compute the two-month lead and lag changes for each month $t$; (3) for any $t$, if these lead and lag changes are of different signs and their absolute sum exceeds 0.5 within-firm standard deviation, the value for month $t$ is replaced by the average of values in months $t-2$ and $t+2$. This smoothing affects only 0.2 percent of the observations in our sample. We then take within-quarter averages of these adjusted values to arrive at our final data set at the firm-quarter level. To align the event time definition with the timing of the COVID shock and PPP allocations, we define quarters as follows: quarter $t=-1$ in our analysis corresponds to December 2019 through February 2020, while $t=0$ corresponds to March 2020 through May 2020, and so on. In sum, our sample runs from $t=-4$, which corresponds to March 2019 through May 2019, to $t=7$, which corresponds to December 2021 through February 2022.

**Additional Data Cleaning**

[5]The Paydex score is available only for firms with three or more payment experiences from at least two trade providers.

We keep only firms in the 50 states (that is, state Federal Information Processing Standard (FIPS) code lower than 52) with 1,500 or fewer employees and with valid values for all the variables used in each analysis, all of which require values for state, NAICS industry code, age, and employment used to define the fixed effects. Moreover, we require firms to be classified as active in the pre-COVID period (from January 2019 through February 2020) to be included in our analysis, and they cannot have gaps in their times series (that is, classified as inactive or out of business on some interim dates and then returned to active again on a later date). In addition, to study the dynamic treatment effects of the PPP on risk scores, we omit borrowers that appear to have received more than one loan in 2020, or if they are later classified as being inactive or distressed in the data set. This guarantees that we have a balanced panel, enabling us to compare a consistent set of firms over time. In Figure C.5 we analyze the likelihood that a firm is classified as being inactive or distressed based on its PPP allocation.

**Financial Conditions, Viability, and Default Correlations**

To understand the relationship between all of the risk measures, we compute their correlation at the baseline ($t = -1$). First, we run the following regression

$$y_{f,-1} = \zeta_s + \eta_n + \theta_a + \kappa_e + \lambda_d + \varepsilon^y_{f,-1}, \tag{A.1}$$

where the definitions of the fixed effects are like those in the main text (Equation (2)) and the dependent variables are the three risk scores (CCS, FSS, Viability Points) and the Payment Records (Paydex, % Past Due (+31 Days), % Past Due (+61 Days)). We compute the residuals $\hat{\varepsilon}^y_{f,-1}$ and in Table A.1 we report the correlation between the residuals. We do this additional step of computing the residuals to guarantee that the correlation between risk measures is not attributable to other characteristics. Our results are nearly identical if we use the raw measures of the risk scores. As can be seen in in Table A.1, all of these measures are highly correlated, and the CCS is actually the measure that is more correlated with Payment Records (for the set of firms with observed Payment Records). In Table A.2, we report the correlations of the residuals of each variable $h$ quarters ahead with CCS measured in 2019Q1. We see that the correlation between CCS and the other variables is persistent over time.[6] All of the correlations in Tables A.1 and A.2 are statistically significant at 1 percent.

**Random Sample versus Full D&B Database**

To ascertain whether the random sample used for our firm-level analysis is representative of the full D&B database, we report the distribution of variables from our sample versus the distribution from the full D&B database. We use values as of the pre-COVID base period (that is, February 2020) for the comparison because this is the period used to define the fixed effects as controls in the regressions. To be consistent with the estimation sample, only firms with 1,500 or fewer employees are used in the comparison. Moreover, all the statistics are computed using raw data, that is, before any additional

[6] Note here that column (1) of Table A.2 is not equal to the column (1) of Table A.1 due to differences in when the variables are measured. In column (1) of Table A.2, all of the variables are measured at $t = -4$. In column (1) of Table A.1, all of the variables are measured at $t = -1$.

cleaning procedure is applied to smooth out transitory month-to-month fluctuations as described above. This ensures an apples-to-apples comparison. We do not apply the cleaning procedure to the full data set because the objective of this exercise is to compare the data input used in our cleaning procedure with the data we would have used if we had used the full D&B sample. Moreover, the smoothing involves time series operations and thus is too computationally expensive to apply to the full database. To the extent that our random sample is representative of the full data set based on raw data, there is every reason to expect that the cleaned random sample represents a cleaned full data set.

We report the comparison results for the main variables used in our analysis in Table A.3. (Results for the other variables are available upon request.) It is clear that the distribution of all the main variables for the sample is nearly the same as the distribution for the full data. In Table A.4, we report the summary statistics in our sample.

## A.2 The PPP and Identifying Borrowers in the D&B Database

Due to the COVID-19 pandemic and the strict public health measures introduced by all levels of the government, revenues of small businesses plummeted in April 2020 more than 40 percent from their January 2020 levels, and they were still down 20 percent in August 2020 (Chetty et al. (2020)). In response, Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act to provide substantial assistance to businesses and households. As part of the CARES Act, the PPP was designed to provide liquidity to small businesses so that they could retain workers. The total volume of loans and grants made over 2020 and 2021 through the PPP amounted to approximately $800 billion. The first round ran from April 3 through August 8, 2020, approving more than 5 million loans amounting to a total of just over $525 billion.

The PPP is discussed extensively in studies such as Autor et al. (2022a) and Doniger and Kay (2021), so we provide only a brief overview of its key features that are relevant for our analysis. Only small businesses (generally those with 500 or fewer employees, but with some exceptions) were eligible for the PPP. The D&B database thus is particularly suitable for studying the PPP because of its unrivaled coverage of small businesses. The loans were fully guaranteed by the government, but processing of the loan applications was delegated to financial institutions so that funds could be disbursed rapidly. The maximum loan amount was 2.5 times a firm's average monthly payroll costs in the preceding year, up to $10 million. PPP loans did not require collateral or personal guarantees and would be fully forgiven if funds were spent in accordance with the rules, such as those on permitted expenses (chiefly payroll) and on maintaining employment levels to the extent feasible. Apart from supporting the retention of employment, the almost costless liquidity provided by the PPP thus also likely enabled recipient firms to improve their solvency and risk scores.

D&B uses its proprietary name-matching algorithm to match PPP firms as reported by the Small Business Administration (SBA) by name, street address, city, and state. Of the $11,768,689$ PPP loans in the

SBA's July 2022 data release, 11,153,679 are matched to a Data Universal Numbering System (DUNS), the unique identifier in the D&B database).[7] Of these, we consider only the 7,005,863 loans matched with a confidence score of 8 or higher (which are deemed sufficiently precise by the algorithm). These correspond to 5,067,486 DUNS, of which 3,941,620 received loans in 2020. See Table A.5 for detailed counts of PPP loans matched to D&B data and the corresponding number of DUNS, reported separately for 2020 versus 2021 loans. In Table A.6, we show the timing of the allocation of the PPP in our sample, in the D&B matched database, and in the SBA release of PPP loans.

## A.3 Other Data Sets

To assess the PPP's effects on employment, we also use the following data sources. First, to more accurately measure the intensity of PPP funding directed to a locality, we normalize the volume of PPP loans received by firms within a county using the total annual payroll of eligible firms (that is, those with 500 or fewer employees), which is derived using data from the U.S. Census Bureau's County Business Patterns (CBP 2020) combined with data from the bureau's Statistics of U.S. Businesses (SUSB 2017). The CBP/SUSB data are also used to compute the fraction of eligible firms receiving PPP loans in a locality. Employment and labor force participation at the county level by month are obtained from the Bureau of Labor Statistics' (BLS) Local Area Unemployment Statistics (LAUS) database (and processed as in Joaquim and Netto (2021)). For instrumental variable estimation, we use data from the Federal Deposit Insurance Corporation's (FDIC) Summary of Deposits (SOD) to measure small businesses' access to community banks, which were more active in PPP lending, especially early on, using the share of bank branches in a county that belong to community banks. The 2019 SOD provides the location of all branches (and deposit balances) of all depository institutions that were operating in the United States as of June 2019. We aggregate the data from the SOD at the county level by summing the individual branch data. Community banks are identified according to the FDIC's institution directory. We measure community banks' importance locally by their branches' collective share of the total number of bank branches in a county.

[7] All the counts reported here are based on the number of distinct DUNS unless otherwise noted. Some distinct loan IDs as reported by the SBA are mapped into more than one DUNS numbers and vice versa.

Table A.1: Financial Conditions, Viability, and Default Correlations (Baseline)

*Note:* This table shows the correlation coefficients between our measures of financial condition and viability for the full sample (Panel A), and those variables and Paydex and default (+31 and +61 Days Past Due) for the set of firms that have all of the data available (Panel B). Before computing the correlations, we regress each variable on firm age (binned), # employees (binned), state, and industry fixed effects (the set of fixed effects in column 2 of Table 1), and then compute the correlations on the residuals. Results are quantitatively equivalent using the raw data. For details on variable definitions, see Section 2. All variables are measured at the baseline ( that is, $t = -1$, Dec 2019–Feb 2020). All of these correlations are statistically significant at 1%. Sources: D&B and authors' calculations.

**Panel A: Full Sample**

| | CCS | FSS | Via Points |
|---|---|---|---|
| CCS | 1.00 | 0.67 | 0.47 |
| FSS | 0.67 | 1.00 | 0.40 |
| Via Points | 0.47 | 0.40 | 1.00 |

**Panel B: Firms with Payment Data**

| | CCS | FSS | Via Points | Paydex | +31 | +61 |
|---|---|---|---|---|---|---|
| CCS Points | 1.00 | 0.73 | 0.62 | 0.74 | -0.73 | -0.69 |
| FSS Points | 0.73 | 1.00 | 0.70 | 0.74 | -0.48 | -0.44 |
| Via Points | 0.62 | 0.70 | 1.00 | 0.57 | -0.44 | -0.39 |
| Paydex | 0.74 | 0.74 | 0.57 | 1.00 | -0.58 | -0.55 |
| % Past Due (+31 Days) | -0.73 | -0.48 | -0.44 | -0.58 | 1.00 | 0.94 |
| % Past Due (+61 Days) | -0.69 | -0.44 | -0.39 | -0.55 | 0.94 | 1.00 |

Table A.2: Future Financial Conditions, Viability, and Default Correlations with Initial Commercial Credit Score

*Note:* This table shows the correlation coefficients between CCS Points at the beginning of the sample (2019Q1) with all of our measures of financial condition and viability $h$ quarters ahead. Our sample for this exercise is the set of firms with available data for all variables. Before computing the correlations, we regress each variable on firm age (binned), # employees (binned), state, and industry fixed effects (the set of fixed effects in column 2 of Table 1), and then compute the correlations on the residuals. Results are quantitatively equivalent using the raw data. For details on variable definitions, see Section 2. All of these correlations are statistically significant at 1%. Sources: D&B and authors' calculations.

$Corr(CCS_{2019Q1}, X_{2019Q1+h})$

| | Quarters ahead ($h$) | | | |
|---|---|---|---|---|
| $X_{2019Q1+h}$ | 0 | 1 | 2 | 3 |
| CCS Points | 1.00 | 0.93 | 0.85 | 0.76 |
| FSS Points | 0.73 | 0.72 | 0.70 | 0.66 |
| Via Points | 0.60 | 0.60 | 0.59 | 0.56 |
| Paydex | 0.74 | 0.73 | 0.71 | 0.66 |
| % Past Due (+31 Days) | -0.78 | -0.69 | -0.64 | -0.49 |
| % Past Due (+61 Days) | -0.75 | -0.68 | -0.63 | -0.48 |

Table A.3: Comparison of the Random Sample versus Full D&B Data Set

*Note:* This table compares the distributional statistics of the main variables used in our analysis from the random sample versus the full D&B data set. All the values are as of the pre-COVID base period (February 2020), and as is in the raw data, before any of the additional cleaning procedure (such as to smooth out transitory fluctuations within a quarter) is applied to derive the final sample for analysis. Only DUNS with 1,500 or fewer employees are included. Sources: D&B and authors' calculations.

| Variable | Data | Mean | Mean Diff. (%) | 5th Pctl. | 25th Pctl. | 50th Pctl. | 75th Pctl. | 95th Pctl. |
|---|---|---|---|---|---|---|---|---|
| Log Sales | Sample | 11.73 | 0.69 | 10.23 | 10.95 | 11.46 | 12.21 | 14.29 |
| Log Sales | Full D&B | 11.65 | | 10.24 | 11.06 | 11.46 | 11.96 | 13.91 |
| Firm Age | Sample | 13 | 22.43 | 1 | 4 | 9 | 15 | 44 |
| Firm Age | Full D&B | 11 | | 1 | 2 | 6 | 12 | 40 |
| Employment | Sample | 7 | 4.28 | 1 | 2 | 2 | 4 | 17 |
| Employment | Full D&B | 7 | | 1 | 2 | 3 | 7 | 13 |
| CSS Points | Sample | 504 | 1.09 | 414 | 472 | 496 | 545 | 607 |
| CSS Points | Full D&B | 498 | | 431 | 472 | 487 | 529 | 601 |
| FSS Points | Sample | 1,462 | 0.58 | 1,395 | 1,416 | 1,457 | 1,502 | 1,549 |
| FSS Points | Full D&B | 1,453 | | 1,393 | 1,408 | 1,449 | 1,486 | 1,543 |
| Viability Score | Sample | 489 | 2.36 | 428 | 454 | 484 | 518 | 566 |
| Viability Score | Full D&B | 478 | | 412 | 442 | 471 | 508 | 560 |
| Viaility Points | Sample | 5 | -6.07 | 2 | 4 | 5 | 6 | 6 |
| Viaility Points | Full D&B | 5 | | 2 | 4 | 6 | 6 | 7 |

Table A.4: Summary Statistics of the Regression Sample

*Note:* This table presents the summary statistics of our regression sample. All the values are as of December 2019 through February 2020, the pre-COVID base quarter, except for the Viability Score, for which the 2019 average value is reported (and used in the regressions). For variable definitions, see Section 2 and Appendix A. Sources: D&B and authors' calculations.

| Variable | Mean | Standard Deviation | Median | Obs |
|---|---|---|---|---|
| Via Score (2019) | 4.45 | 1.35 | 4.50 | 417136 |
| CCS Points | 509.04 | 72.39 | 505.33 | 413903 |
| FSS Points | 1468.33 | 52.59 | 1465.17 | 413836 |
| Via Points | 499.81 | 41.56 | 496.00 | 417136 |
| Age | 15.57 | 17.79 | 10.00 | 417136 |
| Employees | 7.73 | 41.47 | 2.00 | 417136 |
| Phase 1 | 0.07 | 0.25 | 0.00 | 417136 |
| Phase 2 | 0.07 | 0.25 | 0.00 | 417136 |
| Phase 3 | 0.03 | 0.17 | 0.00 | 417136 |
| Paydex | 72.99 | 13.81 | 80.00 | 133062 |
| % Past Due (+31 Days) | 0.08 | 0.22 | 0.00 | 108087 |
| % Past Due (+61 Days) | 0.06 | 0.21 | 0.00 | 108087 |

Table A.5: Statistics of PPP Loans Matched to D&B Data, 2020 versus 2021 Loans

*Note:* Counts and volume of PPP loans matched to the D&B data set using their proprietary name matching algorithm with a confidence code (ranging from 1-10) of 8 or higher. Sources: D&B and authors' calculations.

| | Loan Count | | DUNS Count | | Loan Volume | |
|---|---|---|---|---|---|---|
| | Count | % | Count | % | Millions, $ | % |
| **2020 Loans** | | | | | | |
| Full PPP Loan Data Set | 5,138,180 | 100.00 | – | – | 521,926 | 100.00 |
| Loans Matched by D&B | 5,044,447 | 98.18 | 4,816,432 | 100.00 | 520,401 | 99.71 |
| Loans Matched with Confidence $\geq$8 | 3,941,620 | 76.71 | 3,855,077 | 80.04 | 477,980 | 91.58 |
| Duplicates Loans Dropped | 3,939,166 | 76.66 | 3,855,077 | 80.04 | 477,897 | 91.56 |
| **2021 Loans** | | | | | | |
| Full PPP Loan Data Set | 6,630,509 | 100.00 | – | – | 276,767 | 100.00 |
| Loans Matched by D&B | 6,109,232 | 92.14 | 4,819,524 | 100.00 | 269,005 | 97.20 |
| Loans Matched with Confidence $\geq$ 8 | 3,064,243 | 46.21 | 2,775,889 | 57.60 | 209,053 | 75.53 |

Table A.6: PPP Loans by Allocation Timing: Sample, D&B and SBA/Treasury Release

*Note:* Shares by loan count and volume of PPP loans by allocation timing. We compute the shares for three sets of PPP loan data: the SBA/Treasury PPP release (which includes all loans), the matched firms from the PPP release to the D&B (see Appendix A.2), and, finally, the firms in our sample of the data. For Panels A and B, the denominator is the loan count (or volume) of all PPP loans in Round 1 of the program. For Panel C, the denominator is the loan count (or volume) of all PPP loans in both rounds 1 and 2 of the program. **Panel A:** *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. **Panel B:** We apply the classification of Doniger and Kay (2021) and divide PPP borrowers into four groups: (1) those that received funds early, (2) just before the 10-day window (April 16 through 26) of funding delay when no loans were made, (3) right after the 10-day window, and (4) later on. The exact dates are displayed in the table. For details, see Appendix E. **Panel C:** Round 1 loans are those made from April 3 through August 8, 2020. Round 2 loans are those made from January 11 through May 31, 2021. The ratios are computed at the loan level (and not at the firm level) for Panel C. Sources: SBA/Treasury PPP release, D&B, and authors' calculations.

| | # Loans | | | Amount | | |
|---|---|---|---|---|---|---|
| | SBA | D&B | Sample | SBA | D&B | Sample |
| **Panel A:** Round 1 Timing | | | | | | |
| Phase 1 | 0.32 | 0.36 | 0.40 | 0.61 | 0.63 | 0.66 |
| Phase 2 | 0.40 | 0.42 | 0.41 | 0.30 | 0.29 | 0.28 |
| Phase 3 | 0.28 | 0.23 | 0.19 | 0.09 | 0.07 | 0.07 |
| **Panel B:** Doniger and Kay (2021) Classification | | | | | | |
| Pre Apr-14 | 0.20 | 0.23 | 0.26 | 0.44 | 0.46 | 0.47 |
| Apr-14 to Apr-16 | 0.12 | 0.13 | 0.14 | 0.17 | 0.18 | 0.18 |
| Apr 26-28 | 0.14 | 0.14 | 0.15 | 0.13 | 0.13 | 0.12 |
| Post Apr-28 | 0.55 | 0.50 | 0.45 | 0.26 | 0.24 | 0.23 |
| **Panel C:** Round 1 and 2 | | | | | | |
| Round 1 | 0.44 | 0.58 | 0.63 | 0.66 | 0.70 | 0.72 |
| Round 2 | 0.56 | 0.42 | 0.37 | 0.34 | 0.30 | 0.28 |

# B The Timing and Ultimate Allocation of PPP Loans: Additional Results

In this appendix we present extensions to and robustness checks of the results from Section 3. All of our results in this appendix are consistent with those from Table 1. In Table B.1 we report the estimation of Equation (2) with alternative sets of fixed effects and controls. We use more detailed fixed effects (for instance, instead of state we use county and Zip code). In Table B.2 we report the estimation of Equation (2) with QCEW weights; that is, for each firm we use a weight that is given by the ratio of that firm's state-industry prevalence in QCEW relative to the D&B data. In Table B.3 we report the estimation of Equation (2) including only firms with 3 to 500 employees in the baseline. The reasoning behind this robustness is that the smallest firms are over-represented in the D&B relative to the census, and omitting the smaller firms based on an employment cutoff has been shown to address this repressiveness issue (see for instance, Rossi-Hansberg et al. (2021)). Moreover, firms with more than 500 employees were not eligible for the PPP (except for those in the accommodation and food services industries and some other exceptions). Our results are the same in samples that includes all firms with more than three employees *or* all firms with fewer than 500 employees. We opt for the version of firms with 3 to 500 employees for brevity.

In Figures B.1, B.2, and B.3 we show the results of the estimation of Equation (2) with alternative dependent variables. Figure B.1 corresponds to the model in Table 1, column (1); that is, there are no fixed effects. Figure B.2 corresponds to the model in Table 1, column (3); that is, we include a state-industry-age-size fixed effect. Figure B.3 corresponds to the model in Table 1, column (6); that is, we include state, industry, age, and size fixed effects and an indicator of availability of payment data and the $VS_{f,2019}$ as a control. For comparison purposes, we normalize all of the dependent variables, such that the coefficients can be interpreted as a one-standard-deviation change for each variable. Moreover, we normalize all variables to guarantee that higher values are "good" (for instance, we show the results of minus delinquency).

Table B.1: Pre-COVID Commercial Credit Score by 2020 PPP Loan Status and Timing: Alternative Fixed Effects and Controls

*Note:* This table shows the average difference of base-quarter (December 2019 through February 2020) CCS across firms grouped by the timing of PPP receipt, estimated according to Equation (2). Estimates in this table differ from those in Table 1 due to the set of continuous controls and fixed effects included (indicated in the rows) in the regression (that is, accounted for when computing the conditional averages). For details on variable definitions, see Section 2. *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Sources: D&B and authors' calculations.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Phase 1 | 29.87*** | 25.74*** | 30.61*** | 27.83*** | 27.67*** | 25.55*** |
| | (0.5799) | (0.5826) | (0.7061) | (0.5371) | (0.5365) | (0.5321) |
| Phase 2 | 25.88*** | 22.85*** | 26.75*** | 25.18*** | 24.99*** | 23.43*** |
| | (0.5818) | (0.5915) | (0.6463) | (0.5514) | (0.5413) | (0.5512) |
| Phase 3 | 17.86*** | 15.97*** | 17.97*** | 18.09*** | 18.47*** | 17.42*** |
| | (0.7731) | (0.7593) | (0.8680) | (0.7174) | (0.7025) | (0.6996) |
| *Fixed-effects* | | | | | | |
| State | Yes | Yes | | | | |
| Industry | Yes | Yes | | | | |
| Firm Age | Yes | Yes | | | | |
| Employment Bins | Yes | Yes | | Yes | Yes | Yes |
| County-Industry-Age -Emp. | | | Yes | | | |
| County | | | | Yes | | |
| Industry (3 Digit) | | | | Yes | | |
| Firm Age (Years) | | | | Yes | Yes | Yes |
| Zip-Code | | | | | Yes | Yes |
| Industry (4 Digit) | | | | | Yes | Yes |
| *Continuous Controls* | | | | | | |
| Ln(Sales) | | Yes | | | | Yes |
| *Fit statistics* | | | | | | |
| Observations | 413,865 | 413,005 | 342,483 | 413,782 | 408,191 | 407,332 |
| $R^2$ | 0.12174 | 0.12867 | 0.26337 | 0.17750 | 0.22352 | 0.22534 |

Table B.2: Pre-COVID Firm Commercial Credit Score by 2020 PPP Loan Status and Timing with QCEW Weights

*Note:* This table shows the average difference of base-quarter CCS by the timing of PPP receipt, estimated according to Equation (2). This table is equivalent to Table 1, except for the sample weighting scheme used. In the regression underlying this table, we re-weight each sample observation by the corresponding state and two-digit NAICS weight according to the QCEW data. Specifically, we first compute the share of observations in the D&B data set versus in the QCEW for a given state and two-digit NAICS pair. We then re-weight our observations by the ratio of the QCEW to the D&B shares. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Sources: D&B, BLS, and authors' calculations.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Phase 1 | 43.25*** | 30.10*** | 30.36*** | 14.72*** | 9.956*** | 6.039*** |
| | (0.7590) | (0.6718) | (0.6481) | (0.5761) | (0.4981) | (0.5014) |
| Phase 2 | 33.90*** | 26.25*** | 26.39*** | 12.26*** | 8.090*** | 4.509*** |
| | (1.075) | (0.6035) | (0.5877) | (0.5093) | (0.5433) | (0.5302) |
| Phase 3 | 21.30*** | 18.06*** | 18.22*** | 6.964*** | 4.103*** | 1.219 |
| | (1.168) | (0.7523) | (0.7607) | (0.7960) | (0.9605) | (0.9830) |
| *Fixed-effects* | | | | | | |
| State | | Yes | | Yes | Yes | Yes |
| Industry | | Yes | | Yes | Yes | Yes |
| Firm Age | | Yes | | Yes | Yes | Yes |
| Employment Bins | | Yes | | Yes | Yes | Yes |
| State-Industry- Age-Emp. | | | Yes | | | |
| Payment Records | | | | Yes | | Yes |
| *Continous Controls* | | | | | | |
| Via Score (2019) | | | | | Yes | Yes |
| *Fit statistics* | | | | | | |
| Observations | 413,849 | 413,849 | 410,737 | 413,849 | 413,849 | 413,849 |
| $R^2$ | 0.03675 | 0.11723 | 0.14994 | 0.18083 | 0.28667 | 0.29439 |

Table B.3: Pre-COVID Commercial Credit Score by 2020 PPP Loan Status and Timing for Firms with 3 to 500 Employees

*Note:* This table shows the average difference of base-quarter (December 2019 through February 2020) CCS across firms grouped by the timing of PPP receipt, estimated according to Equation (2). Estimates in this table differ from those in Table 1 due only to a difference in sample: only firms with 3 to 500 employees in February 2020 are included here. For details on variable definitions, see Section 2. *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Sources: D&B and authors' calculations.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Phase 1 | 36.63*** | 29.03*** | 29.50*** | 15.97*** | 6.040*** | 4.566*** |
| | (0.8494) | (0.6777) | (0.7040) | (0.6769) | (0.5407) | (0.5431) |
| Phase 2 | 29.37*** | 25.84*** | 26.46*** | 13.52*** | 4.460*** | 3.059*** |
| | (0.8053) | (0.6839) | (0.6921) | (0.6617) | (0.5850) | (0.5693) |
| Phase 3 | 18.94*** | 18.67*** | 19.27*** | 8.366*** | 1.552* | 0.3554 |
| | (1.073) | (0.9882) | (1.014) | (1.021) | (0.8646) | (0.8846) |
| *Fixed-effects* | | | | | | |
| State | | Yes | | Yes | Yes | Yes |
| Industry | | Yes | | Yes | Yes | Yes |
| Firm Age | | Yes | | Yes | Yes | Yes |
| Employment Bins | | Yes | | Yes | Yes | Yes |
| State-Ind.- Age-Emp. | | | Yes | | | |
| Payment Records | | | | Yes | | Yes |
| *Continous Controls* | | | | | | |
| Via Score (2019) | | | | | Yes | Yes |
| *Fit statistics* | | | | | | |
| Observations | 171,253 | 171,253 | 168,570 | 171,253 | 171,253 | 171,253 |
| $R^2$ | 0.03241 | 0.10180 | 0.14942 | 0.15188 | 0.32509 | 0.32631 |

Figure B.1: Pre-COVID Firm Characteristics by 2020 PPP Loan Status and Timing: Estimates without Fixed Effects

*Note*: This figure displays the pre-COVID characteristics of firms grouped by the timing of their 2020 PPP loan receipt, estimated according to Equation (2) with no fixed effects (as are estimates reported in column 1 in Table 1). Each row of the figure corresponds to estimating Equation (2) for a particular dependent variable ($y_{i,-1}$). For details on variable definitions, see Section 2. *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. Each dot corresponds to a point estimate of $\beta_{i,t}$, while the whiskers represent its 95% confidence interval. Standard errors are clustered at the state-industry level. To enable comparisons of the magnitude of $\beta_{i,t}$ estimated for different $y_{i,-1}$'s, we normalize each $\beta_i$ by the standard deviation of the corresponding $y_{i,-1}$. Moreover, for variables (such as default) where a "good" outcome maps to a lower value, we multiply by $-1$ so that a higher value in the plot always indicates a better outcome. A coefficient of $X$ can be interpreted as firms in category $i$ having values of $y_{i,-1}$ on average $X$ standard deviations higher than firms without PPP loans. Sources: D&B and authors' calculations.

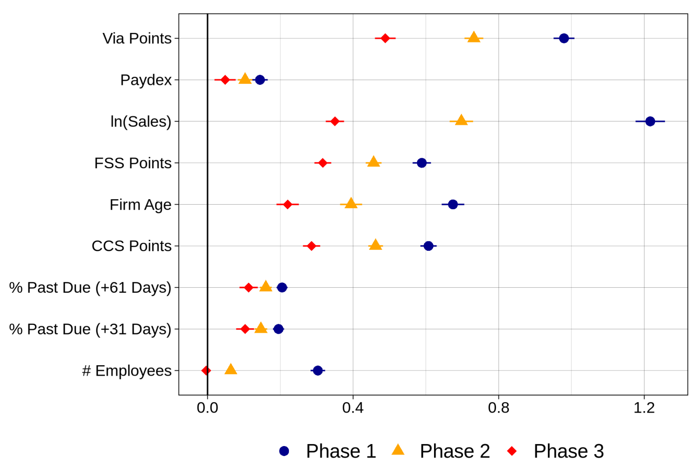


Figure B.2: Pre-COVID Firm Characteristics by 2020 PPP Loan Status and Timing: Estimates with State-Industry-Age-Size Fixed Effect

*Note*: This figure displays the pre-COVID characteristics of firms grouped by the timing of their 2020 PPP loan receipt, estimated according to Equation (2) with fixed effects for state, industry (two-Digit NAICS), firm-age bin, and employment-size bin. These estimates for a broader set of attributes are thus the counterpart to the estimates reported in column 3 in Table 1) for CCS. All variables are normalized as in Figure B.1. Sources: D&B and authors' calculations.

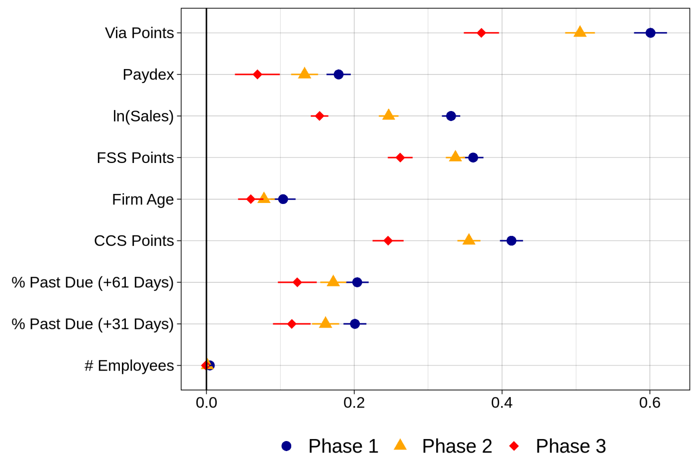
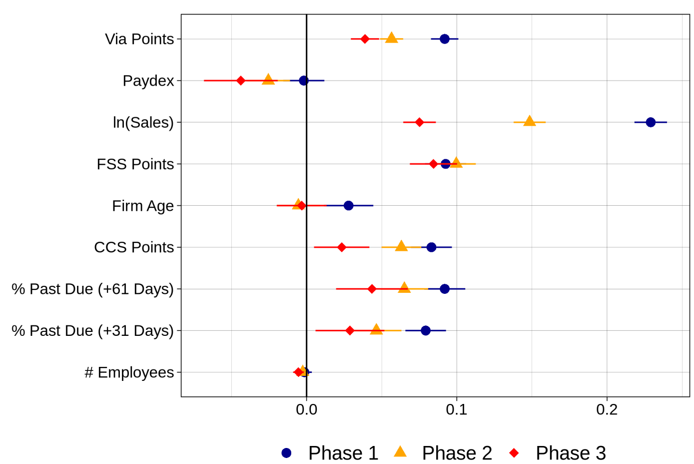



Figure B.3: Pre-COVID Firm Characteristics by 2020 PPP Loan Status and Timing: Estimates with All the Fixed Effects

*Note*: This figure displays the pre-COVID characteristics of firms grouped by the timing of their 2020 PPP loan receipt, estimated according to Equation (2) with all the fixed effects (for state, industry (two-Digit NAICS), firm-age bin, and employment-size bin; an indicator for availability of Paydex or default data; and the 2019 average Viability Score). These estimates for a broader set of attributes are thus the counterpart to the estimates reported in column 6 in Table 1) for CCS. All variables are normalized as in Figure B.1. Sources: D&B and authors' calculations.



# C The PPP Improved Firms' Financial Condition: Details and Additional Results

In this appendix we present details, extensions and robustness of the results from Section 4.

**Details on Figure 2**

We apply the method of Sun and Abraham (2021) to account for the staggered rollout of the PPP and to consistently aggregate the coefficients estimated for firms receiving treatment in different quarters. Specifically, we estimate the following regression:

$$y_{f,t} = \sum_{\tau \neq -1} \sum_{q} \psi_{q,\tau} D_{q,\tau} + \alpha_f + \zeta_{s,t} + \eta_{n,t} + \theta_{a,t} + \kappa_{e,t} + \lambda_{p,t} + \mu_t \times \mathrm{VS}_{f,2019} + \varepsilon_{f,t}, \tag{C.1}$$

where $D_{q,\tau}$ are indicator variables on the interaction terms between indicators for receipt quarter ($q$) and event time—defined as time relative to treatment—($\tau$). Each cohort here is defined by the quarter in which firms in the cohort received PPP loans. The fixed effects are the same as those in Equation (3).

To estimate the average dynamic effect in a given quarter after receiving a PPP loan, we aggregate the coefficients $\psi_{q,\tau}$ across receipt quarter ($q$) at a given $\tau$ using the number of treated firms in each quarter as weights (normalized by the total number of firms). That is, the estimate of the average dynamic treatment effect for period $\tau$ is denoted by $\overline{\psi}_\tau \equiv \sum_q w_q \psi_{q,\tau}$, where $w_q$ is the share of observations from firms in group $q$. Estimates of $\overline{\psi}_\tau$ are plotted in Figure 2. The exact estimates underlying this plot are reported in Table C.2 (column 3), which also reports the counterpart estimated from alternative specifications of Equation (C.1).

**Unconditional Means**

In Figure C.1 we report the unconditional means by timing of PPP allocation in the first round. These results do not account for differences in other firms' characteristics (state, size, industry, age etc.) that we consider in our difference-in-differences analysis, but they provide some insight in the general evolution of the risk scores in our sample. In Panel A, we show that pre-COVID, the average firm was experiencing a CCS decrease across all groups, and firms that received a PPP loan saw a large reversal of this trend later on. We find similar results for the other risk measures.

**Other Risk Scores**

In Figure C.2, we report the results of the estimation of equation (3) with alternative risk scores as dependent variables. We find less heterogeneity in the effects for the FSS (Panel a), but consistent with our results for CCS, significantly larger treatment effects when we account for firms' baseline viability (Panel b). For Viability Points, we find results very similar to the ones presented in the main text (Panels c and d).

**Alternative Weights and Sub-samples**

In Figure C.3 we report the results of the estimation of equation (3) with QCEW weights; that is, for each firm we use a weight that is given by the ratio of that firm's state-industry prevalence in QCEW relative to the D&B data. In Figure C.4 we report the estimation of Equation (2) including only firms with 3 to 500 employees in the baseline. We also conducted various other robustness exercises that produced results consistent with the results shown throughout the paper (available upon request). First, we replicated our analysis including only firms with fewer than 500 employees in the baseline (that is, those eligible for the program). Second, we replicated our analysis for a set of firms that had at least two changes in their CCS scores in the four quarters leading up to the COVID/PPP shocks. Third, even though we include a payment-records fixed effect in our specifications, we replicated our results for the set of firms with payment records in the baseline.

**Large Changes in CCS.**

To understand if the PPP was associated with large changes in CCS for firms, we estimate linear probability models where the dependent variable is an indicator of whether a given firm suffered an increase or decrease in its CCS of at least 20 or 40 points at the end of the sample relative to the baseline (recall that a 40 point increase means a 100 percent rise in the risk of delinquency). The linear probability model we estimate is given by:

$$y_{f,t} = \sum_i \delta_i^{LD} \text{Phase}_{f,i} + \zeta_s + \eta_n + \theta_a + \kappa_e + \lambda_d + \mu \cdot \overline{VS}_{f,2019} + \varepsilon_f, \tag{C.2}$$

where the fixed effects are as in Equation (2) and the dependent variables $y_{f,t}$ are indicators if the change in CCS relative to baseline ($t = -1$) is larger than a threshold; that is, for a threshold $T$: $y_{f,t} = \mathbb{1}\{CCS_{f,t} - CCS_{f,-1} > T\}$. Alternatively, we also construct indicators if the change in CCS relative to the baseline ($t = -1$) is lower than a threshold; that is, for a threshold $T$: $y_{f,t} = \mathbb{1}\{CCS_{f,t} - CCS_{f,t=-1} < -T\}$. We use $T = \{20, 40\}$, and we estimate the above model for $t = 7$ (our results are robust to both choices).

We report the results in Table C.1. Firms that received a PPP loan are 7 to 12 percentage points more likely to have a CCS that is 40 points larger at the end of the sample relative to the baseline (column 2). Similarly, firms that received a PPP loan are 2 to 3 percentage points less likely to have a CCS that is 40 points lower at the end of the sample relative to baseline (column 4).

**Commercially Inactive or Distressed Indicator**

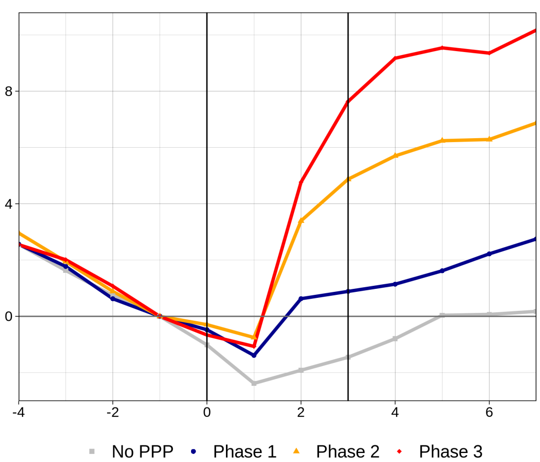
In Figure C.5, we report the results of the estimation of Equation (C.3) with a dependent variable that is an indicator equal to 1 if the firm is classified as commercially inactive or distressed, denoted by $1_{f,t}\{distress\}$. We refer to a firm as commercially inactive or distressed if it exhibits these signals: having an invalid Zip code or industry code, having its phone (landline or cellular) disconnected (at all locations), no mail delivery at the addresses on file for all locations, or formally classified as out of business by D&B. We find that firms that received PPP loans are 2 percentage points less likely to be classified as commercially inactive or distressed at the end of the sample.

$$1_{f,t}\{distress\} = \sum_{t>0} \sum_i \delta_{i,t} \text{Phase}_{f,i} + \zeta_{s,t} + \eta_{n,t} + \theta_{a,t} + \kappa_{e,t} + \lambda_{p,t} + \mu_t \cdot \text{VS}_{f,2019} + \varepsilon_{f,t}, \tag{C.3}$$

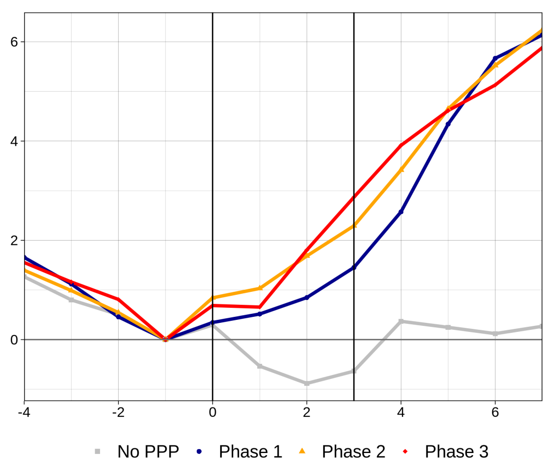
where the fixed effects are the same as in Equation (3). Since we consider only firms that were not distressed by $t = 0$, we do not include in Equation (C.3) firm fixed effects or any group-by-time fixed effects for $t \leq 0$. Moreover, standard errors are clustered at the state-industry level only (and not two-way clustered).

Figure C.1: Risk Scores by 2020 PPP Loan Status and Timing

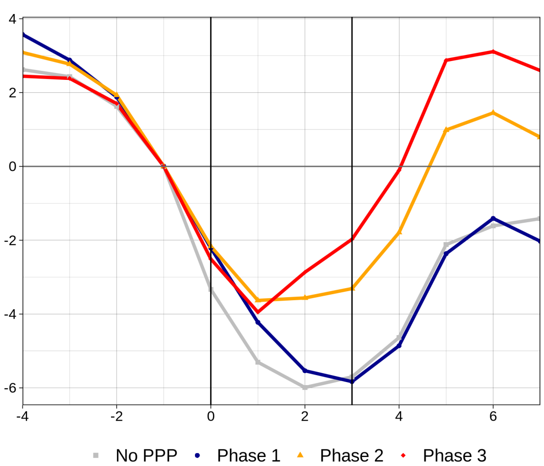
*Note*: Each panel in this figure plots the average difference of a variable from the base-quarter mean (computed over December 2019 through February 2020) by PPP status. Three risk scores are considered (see Section 2 for details on their definitions): CCS (Panel A), FSS (Panel B), and Viability Points (Panel C). PPP status is defined by the timing of the PPP receipt in the first round of the program. *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. Firms that did not receive PPP loans include both eligible and ineligible firms in our analysis. The vertical line at $t = 0$ (which encompasses March through May 2020) denotes the first quarter when first-round PPP loans were made. The vertical line at $t = 3$ corresponds to the first quarter (December 2020 through February 2021) when second-round PPP loans were made. Sources: D&B and authors' calculations.




(a) Commercial Credit Score



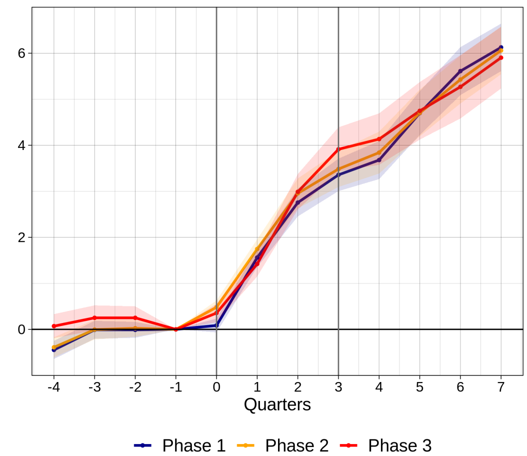

(b) Financial Stress Score




(c) Viability Points

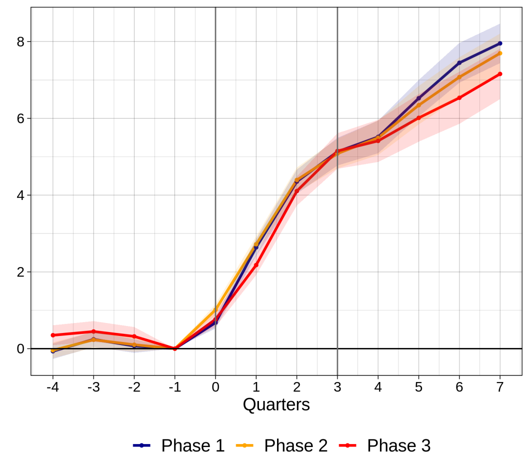
Figure C.2: Effects of the PPP on Financial Stress Score and Viability Points

*Note*: These figures show the average changes in FSS and Viability Points across firms grouped by their 2020 PPP loan status and timing, corresponding to $\delta_{i,t}$ in Equation (3). The set of fixed effects is the same as in Figure 1—the only difference are the dependent variables. Shaded areas are 95% confidence bands, and standard errors are two-way clustered at the state-industry and time levels. The vertical line at $t = 0$ (which encompasses March through May 2020) denotes the first quarter when first-round PPP loans were made. The vertical line at $t = 3$ corresponds to the first quarter (December 2020 through February 2021) when second-round PPP loans were made. Sources: D&B and authors' calculations.



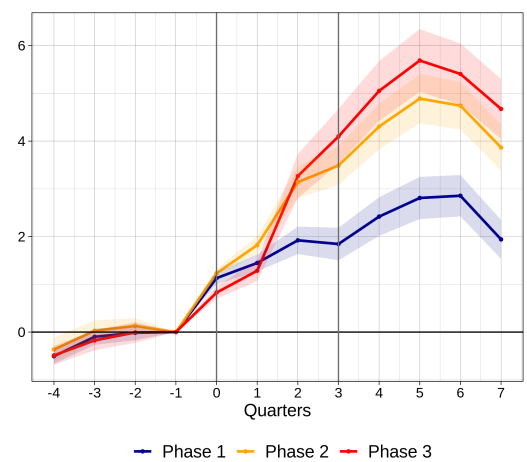

(a) Financial Stress Score

(FEs: Firm + Time $*$ Non-Financial Vars)



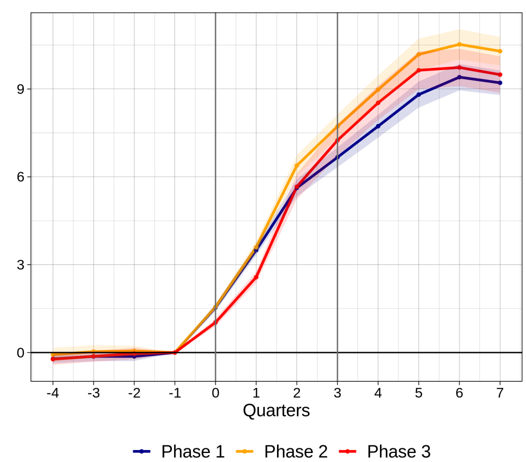

(b) Financial Stress Score

(All FEs, Saturated)




(c) Viability Points

(FEs: Firm + Time + Non-Financial)

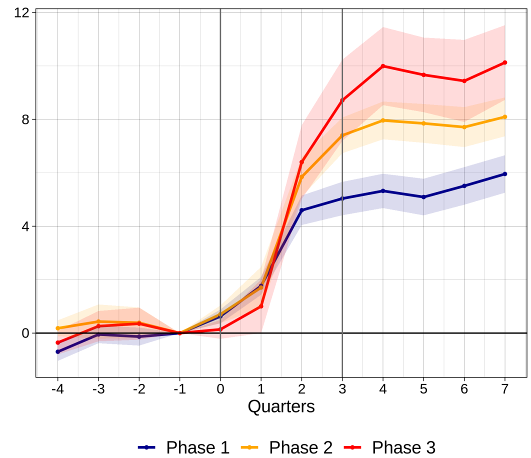



(d) Viability Points

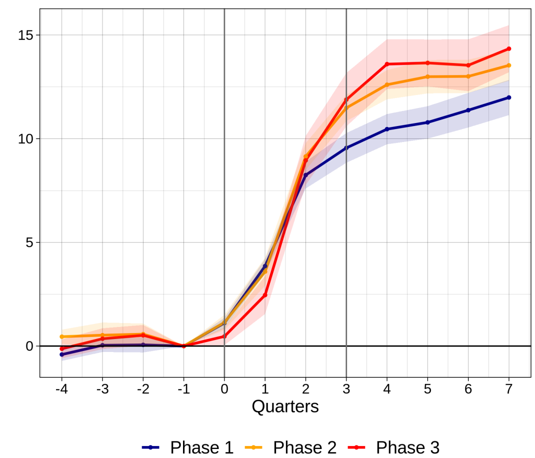
(All FEs, Saturated)

Figure C.3: Effects of the PPP on Commercial Credit Score with QCEW Weights

*Note*: This figure is equivalent to Figure 1 except for one difference: Here, each sample observation is re-weighted by its QCEW state and two-digit NAICS weight. Specifically, we first compute the share of observations in the D&B data set versus in the QCEW for a given state and two-digit NAICS pair. We then re-weight our observations by the ratio of the QCEW to the D&B shares. Sources: D&B, BLS, and authors' calculations.



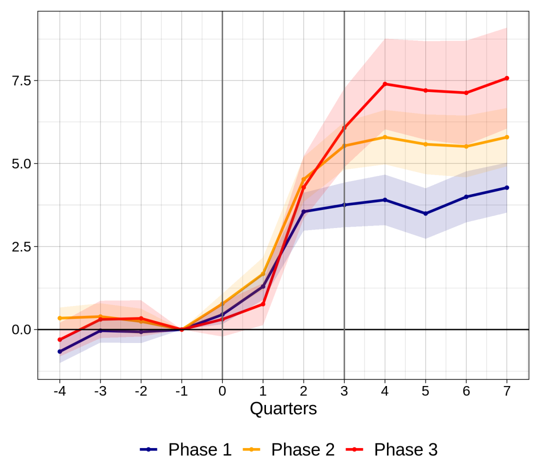

(a) FEs: Firm + Time ∗ Non-Financial



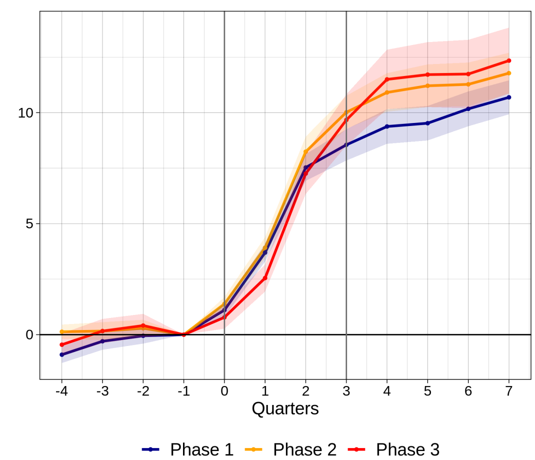

(b) All FEs (Saturated)

Figure C.4: Effects of the PPP on Commercial Credit Score for Firms with 3 to 500 Employees

*Note*: This figure is equivalent to Figure 1 except for one difference: Here, the sample is restricted to firms with 3 to 500 employees in the base quarter. Sources: D&B and authors' calculations.




(a) FEs: Firm + Time ∗ Non-Financial




(b) All FEs (Saturated)

Table C.1: Effects of the PPP on Commercial Credit Score: Long Differences on Large CCS increases

*Note*: This table reports estimates of the $\delta_{i,t}^{LD}$ in the linear probability model of Equation (C.2) at the firm level. The dependent variables are indicators if the change in the CCS relative to the baseline ($t = -1$) is larger than a threshold; that is, for a threshold $T$: $y_{f,t} = \mathbb{1}\{CCS_{f,t} - CCS_{f,-1} > T\}$. Alternatively, we also construct indicators if the change in the CCS relative to the baseline ($t = -1$) is lower than a threshold; that is, for a threshold $T$: $y_{f,t} = \mathbb{1}\{CCS_{f,t} - CCS_{f,t=-1} < -T\}$. We use $T = \{20, 40\}$, and we estimate the the model in Equation (C.2) setting $t = 7$. The set of fixed effects and controls is the same as in Table 1 and is described in the table. Standard errors clustered at the state-industry level are in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Sources: D&B and authors' calculations.

| | $\mathbb{1}\{CCS_{f,t} - CCS_{f,t-1} > T\}$ | | $\mathbb{1}\{CCS_{f,t} - CCS_{f,t-1} < -T\}$ | |
|---|---|---|---|---|
| | $T = 20$ | $T = 40$ | $T = 20$ | $T = 40$ |
| | (1) | (2) | (3) | (4) |
| Phase 1 | 0.1050*** | 0.0764*** | -0.0387*** | -0.0378*** |
| | (0.0036) | (0.0032) | (0.0031) | (0.0026) |
| Phase 2 | 0.1293*** | 0.1062*** | -0.0320*** | -0.0262*** |
| | (0.0039) | (0.0037) | (0.0030) | (0.0024) |
| Phase 3 | 0.1494*** | 0.1270*** | -0.0316*** | -0.0207*** |
| | (0.0048) | (0.0045) | (0.0036) | (0.0031) |
| *Fixed-effects* | | | | |
| State | Yes | Yes | Yes | Yes |
| Industry | Yes | Yes | Yes | Yes |
| Employment Bins | Yes | Yes | Yes | Yes |
| Payment Records | Yes | Yes | Yes | Yes |
| *Continuous Controls* | | | | |
| Via Score (2019) | Yes | Yes | Yes | Yes |
| *Fit statistics* | | | | |
| Observations | 387,786 | 387,786 | 387,786 | 387,786 |
| $R^2$ | 0.04312 | 0.03330 | 0.06281 | 0.04739 |

Figure C.5: Commercially Inactive or Distressed Indicator by PPP Timing

*Note*: This figure analyzes the probability of a firm being commercially inactive or in distress based on its first-round PPP loan status and timing. All firms in our sample are classified as showing a high level of activity from $t = -4$ to $t = 0$ (that is, from the beginning of the sample to when they could have received a PPP loan). *Phase 1* borrowers received a PPP loan during the April 3–16, 2020, period. *Phase 2* borrowers received a loan during the April 26–May 1, 2020, period. *Phase 3* borrowers received a loan during the May 2–August 8, 2020, period. This figure shows the $\delta_{i,t}$ from Equation (C.3). Shaded areas are 95% confidence bands; standard errors are clustered at the state-industry level. The vertical line at $t = 0$ (which encompasses March through May 2020) denotes the first quarter when first-round PPP loans were made. The vertical line at $t = 3$ corresponds to the first quarter (December 2020 through February 2021) when second-round PPP loans were made. Sources: D&B and authors' calculations.

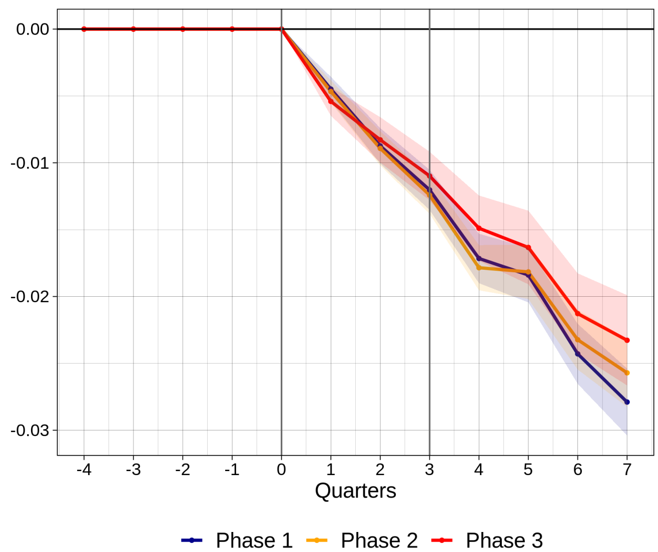


Table C.2: Effects of the PPP on Commercial Credit Score: Difference-in-Differences Estimates for Staggered Treatment

*Note*: This table reports estimates of the average (across all PPP borrowers) effect over time (relative to the quarter of loan receipt) of receiving a PPP loan. These correspond to $\overline{\psi}_\tau \equiv \sum_q w_q \psi_{q,\tau}$ in Equation (C.1) (based on Sun and Abraham (2021)). They are estimated using data at the firm ($f$)-quarter ($t$) level with three sets of fixed effects as listed. The dependent variable is CCS. Standard errors two-way clustered at the state-industry and time levels are in parentheses. Column (3) of this table corresponds to Figure 2, with both sets of results including all the fixed effects as controls. For visualization purposes, we report only the coefficients on a subset of estimated $\tau$'s. Sources: D&B and authors' calculations.

| | (1) | (2) | (3) |
|---|---|---|---|
| $t$ to treat = -3 | 0.1893 | 0.0511 | 0.0898 |
| | (0.1242) | (0.1236) | (0.1282) |
| $t$ to treat = -2 | -0.0290 | 0.0415 | 0.1637 |
| | (0.1084) | (0.1089) | (0.1158) |
| $t$ to treat = 0 | 0.6146*** | 0.7096*** | 1.173*** |
| | (0.0649) | (0.0640) | (0.0655) |
| $t$ to treat = 1 | 1.757*** | 2.368*** | 4.173*** |
| | (0.1388) | (0.1332) | (0.1329) |
| $t$ to treat = 3 | 5.264*** | 7.100*** | 10.96*** |
| | (0.2291) | (0.2373) | (0.2457) |
| $t$ to treat = 6 | 5.201*** | 7.309*** | 12.33*** |
| | (0.2678) | (0.2868) | (0.2954) |
| *Fixed-effects* | | | |
| Firm | Yes | Yes | Yes |
| Time | Yes | Yes | Yes |
| State-Time | | Yes | Yes |
| Industry-Time | | Yes | Yes |
| Employment Bins-Time | | Yes | Yes |
| Age Bins-Time | | Yes | Yes |
| Payment Records -Time | | | Yes |
| *Varying Slopes* | | | |
| Overall Via Rating (2019) (Time) | | | Yes |
| *Fit statistics* | | | |
| Observations | 4,656,787 | 4,656,787 | 4,656,787 |
| $R^2$ | 0.82755 | 0.82814 | 0.83014 |

# D First and Second Rounds of the PPP

The main text focuses on the first round of the PPP, encompassing loans made during the period of April 3 through August 8, 2020. Since every borrower could receive only one loan in 2020, the first round comprised only first-round loans. In December 2020, Congress authorized an additional $284 billion for the program as part of the $900 billion Coronavirus stimulus relief package. The PPP started disbursing funds again on January 11, 2021. The second round of the program ended on May 31, 2021. To qualify for a loan in the second round, a firm could have been eligible but did not borrow in the first round, or it could have been a 2020 PPP borrower seeking a second-round loan because it satisfied three additional conditions. Those condition were the following: (1) the firm had spent all of its first-round loan only on authorized expenses; (2) it had no more than 300 employees; (3) it could demonstrate at least a 25 percent reduction in gross receipts between comparable quarters in 2019 and 2020. The maximum loan size for a second-round PPP loan was $2 million. For brevity, we refer to loans made in 2021 simply as second-round loans, making no distinction between first-round and second-round loans.We conduct an analysis analogous to those presented in Sections 3 and 4 but group firms by whether they borrowed in neither, either, or both rounds of the PPP. As revealed in Table D.1, firms that received only first-round loans were more creditworthy (that is, had a higher average CCS) before the pandemic, even when compared with others in the same state, industry, age bin, and employment-size bin. As would be expected, the difference becomes much smaller once we account for firms' 2019 average Viability Score (column 6).

We likewise repeat our difference-in-differences analysis (based on Equation (5)), grouping firms by whether they borrowed in neither, either, or both rounds of the PPP. Estimates plotted in Figures D.1 and D.2 show that firms that received a PPP loan only in the first round experienced a faster improvement in their risk scores relative to firms that received loans only in the second round or no PPP loans at all. However, once second-round loans were disbursed, firms receiving those loans saw a larger increase in their risk scores relative to their peers that received only first-round loans or no PPP loans at all. Risk scores of firms that received loans in both rounds tracked those of the first-round-only borrowers until the second round of the PPP started. From that point on, the former surpassed the latter.

Note that these results are not causal and do not necessarily imply that the second round was more or less effective than the first round. Contrary to our analysis of the first-round PPP borrowers, the composition of firms across these four categories (neither, first round only, second round only, or both rounds of the PPP) is endogenous. This is due to firms' self-selection into applying for a second-round loan or not, which depended on the program's rule changes in the second round and the evolution of firms' commercial and financial condition after the onset of COVID and after receiving a first-round loan (for the borrowers). Notable evidence of this endogeneity can be found in the risk scores of second-round-only borrowers, which already exhibited improvement (relative to firms that did not receive a PPP loan in either round) *before* the second round of the PPP started. This pre-trend (for the second-round-only borrowers) likely reflects these firms' specific circumstances that steered them to receive loans only in the second round. For instance, they might have been able to reopen faster after the initial COVID outbreak due to better local health conditions but later experienced more difficulty and thus sought funding in the second round.

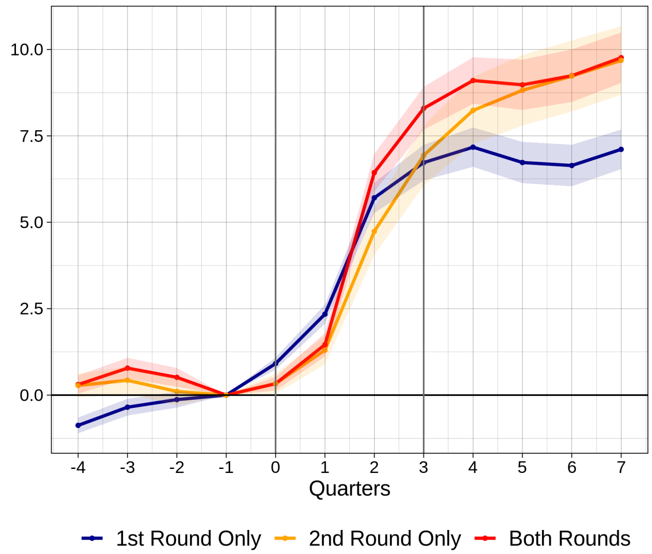
Table D.1: Pre-COVID Commercial Credit Score and PPP Allocation in the First and Second Rounds

*Note:* Analogous to Table 1 in the main text, this table shows the average difference of the CCS in February 2020 (the base quarter) across firms grouped by the status and timing of PPP approval, here by whether a firm borrowed in neither, either, or both rounds of the PPP. For details on variable definitions, see Section 2. Firms in *First Round Only* are those that received a loan only during the period of April 3 through August 8, 2020. Firms in *Second Round Only* received a PPP loan only in 2021. Firms in *Both Rounds* are those that received a PPP loan in both rounds. Firms that did not receive any PPP loans include both eligible and ineligible firms in our benchmark analysis. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5% and 10% levels, respectively. Sources: D&B and authors' calculations.

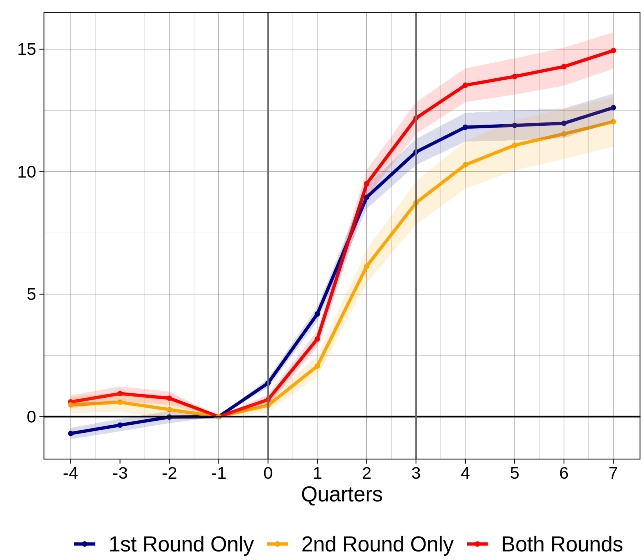
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| 1st Round Only | 38.30*** | 28.07*** | 27.97*** | 14.10*** | 9.489*** | 6.005*** |
| | (0.6715) | (0.5511) | (0.5591) | (0.5551) | (0.4888) | (0.4839) |
| 2nd Round Only | 16.13*** | 10.10*** | 10.10*** | 2.747*** | 2.886*** | 0.7868 |
| | (1.564) | (0.7939) | (0.7935) | (0.7970) | (0.7972) | (0.7902) |
| Both Rounds | 33.39*** | 23.53*** | 23.97*** | 8.972*** | 6.545*** | 2.668*** |
| | (0.8643) | (0.6132) | (0.6067) | (0.5848) | (0.5206) | (0.5173) |
| *Fixed-effects* | | | | | | |
| State | | Yes | | Yes | Yes | Yes |
| Industry | | Yes | | Yes | Yes | Yes |
| Firm Age | | Yes | | Yes | Yes | Yes |
| Employment Bins | | Yes | | Yes | Yes | Yes |
| State-Ind.- Age-Emp. | | | Yes | | | |
| Payment Records | | | | Yes | | Yes |
| *Continous Controls* | | | | | | |
| Via Score (2019) | | | | | Yes | Yes |
| *Fit statistics* | | | | | | |
| Observations | 413,865 | 413,865 | 410,737 | 413,865 | 413,865 | 413,865 |
| $R^2$ | 0.03395 | 0.12184 | 0.15319 | 0.18526 | 0.28371 | 0.29148 |

Figure D.1: Effects of the PPP on Commercial Credit Score: First versus Second Round

*Note*: This figure is equivalent to Figure 1 in the main text except for the grouping of firms. Firms in *First Round Only* are those that received a loan only during the period of April 3 through August 8, 2020. Firms in *Second Round Only* received a PPP loan only in 2021. Firms in *Both Rounds* are those that received a PPP loan in both rounds. Firms that did not receive any PPP loans include both eligible and ineligible firms in our benchmark analysis. Shaded areas are 95% confidence bands; standard errors are two-way clustered at the state-industry and time levels. Sources: D&B and authors' calculations.



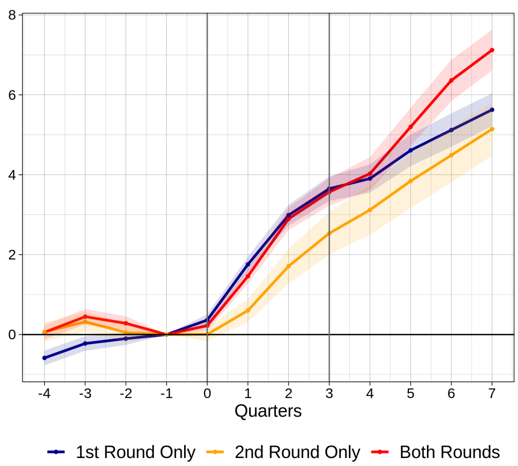

(a) FEs: Firm + Time + Non-Financial




(b) All FEs (Saturated)

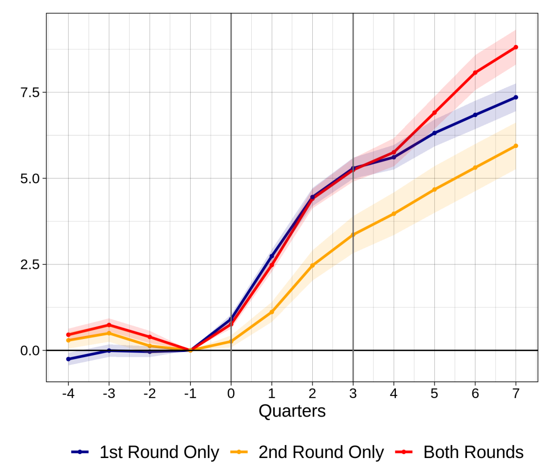
Figure D.2: Effects of the PPP on Financial Stress Score and Viability Points

*Note*: These figures are equivalent to those in Figure C.2 in the main text except for our classification of firms. Firms in *First Round Only* received a loan only during the period of April 3 through August 8, 2020. Firms in *Second Round Only* received a PPP loan only in 2021. Firms in *Both Rounds* received a PPP loan in both rounds. The dependent variables are FSS and Viability Points. Shaded areas are 95% confidence bands; standard errors are two-way clustered at the state-industry and time levels. The vertical line at $t = 0$ (which encompasses March through May 2020) denotes the first quarter when first-round loans were made. The vertical line at $t = 3$ corresponds to the first quarter (December 2020 through February 2021) when second-round loans were made. Sources: D&B and authors' calculations.



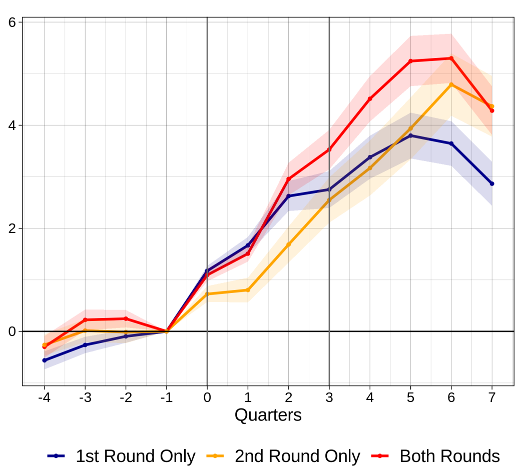

(a) Financial Stress Score

(FEs: Firm + Time + Non-Financial)



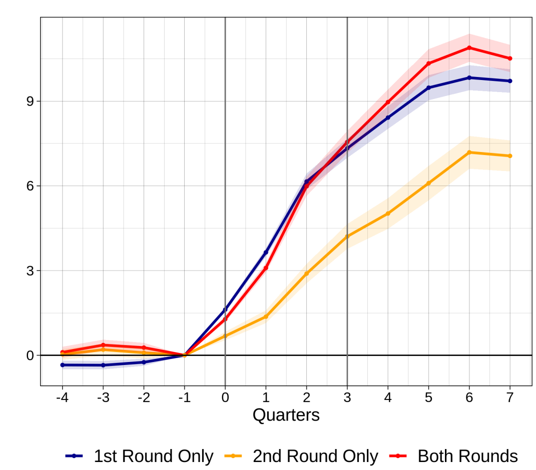

(b) FSS, Saturated

(All FEs, Saturated)




(c) Viability Points

(FEs: Firm + Time + Non-Financial)




(d) Viability Points

(All FEs, Saturated)

# E The 10-Day Window of Funding Delay

Doniger and Kay (2021) (DK) develop an IV based on the 10-day funding delay from April 16 through 27, 2020: the share of loans made right after the delay (on April 27 and 28) over the total amount made right around the window of delay (that is, from April 14 through 28). The exogeneity of this IV rests upon the assumption that no agents could have predicted exactly when the initial funding would become exhausted, thus the volume of loans made just before versus just after the window of delay was not correlated with any firm attributes that may have influenced the effects of receiving a loan. DK argue that the estimated treatment effects of the PPP using this IV should be free of the bias stemming from the systematic difference in firm characteristics between the early and the late PPP recipients (consistent with our findings in Section 3). Any difference in outcome, such as employment, therefore can be attributed solely to the 10-day delay in PPP disbursement.

In Table E.1 we replicate our analysis from Section 3 except that here we divide firms further by the 10-day window of delay. Estimates show that, indeed, firms receiving loans over the April 14–16, 2020, period had pre-COVID risk scores similar to those of firms receiving loans over the April 27–28, 2020, period. Nevertheless, these firms are evidently different from those that received PPP loans later in the program and even more different from firms that did not receive PPP loans. On average, firms that received loans in the few days right around the 10-day window were approximately 40 percent less risky than the non-borrowers. Our results here provide support for DK's identification strategy but also crystallize that the authors' estimates should be interpreted as local treatment effects applicable only to firms that would have received loans right around the 10-day window.

We also replicate our difference-in-differences analysis using groupings of firms based on the delay window. The estimates, as plotted in Figure E.1, reveal that those firms receiving funds just after the delay window showed a decrease in riskiness (over four to seven quarters after receiving the loan) that was about 3 to 5 percent larger compared with firms receiving funds just before the delay window.

Finally, we also replicate our employment analysis using the Doniger and Kay (2021) instrument—the ratio of the volume of PPP loans made during the April 14–16, 2020, period relative to the volume of PPP loans made during the April 14–28, 2020, period. Our exercise is based on the authors' exercise, but there are substantial differences. Their analysis uses the Current Population Survey (CPS) and runs a regression at the worker level, with worker, occupation, and other fixed effects. Our regression is at the county level and uses only state-time fixed effects. Similar to their paper, we cluster the standard errors at the county level. The results are in Figure E.2. We find that contrary to the estimates based on using pre-COVID share of community banks or PPP Exposure as instrumental variables, the estimates of the PPP on employment are essentially unchanged, and that the standard errors become *tighter* once we control for firms' viability in a county.

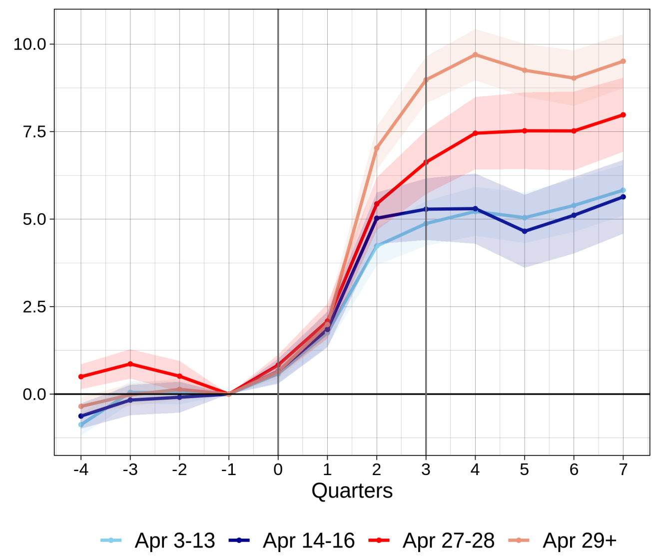
Table E.1: Pre-COVID Commercial Credit Score by 2020 PPP Loan Status and Timing (Relative to the 10-Day Window of Delay)

*Note:* The analysis underlying this table is structurally the same as the one behind Table 1 in the main text. The only difference is the classification of firms. We apply the classification of Doniger and Kay (2021) and divide PPP borrowers into four groups: (1) those that received funds early, (2) just before the 10-day window (April 16 through 26) of funding delay when no loans were made, (3) right after the 10-day window, and (4) later on. The exact dates are displayed in the table. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Sources: D&B and authors' calculations.

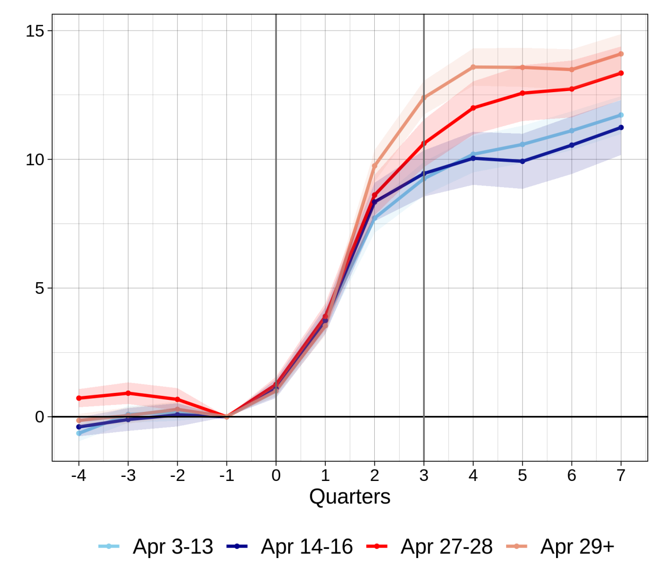
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Apr 3-13 | 46.13*** | 30.33*** | 30.82*** | 14.66*** | 10.07*** | 6.112*** |
| | (0.9257) | (0.6686) | (0.6846) | (0.6767) | (0.5608) | (0.5816) |
| Apr-14 to Apr-16 | 41.36*** | 28.95*** | 29.07*** | 13.84*** | 9.808*** | 5.952*** |
| | (0.9778) | (0.7711) | (0.7855) | (0.7527) | (0.6645) | (0.6621) |
| Apr 26-28 | 38.05*** | 26.36*** | 26.64*** | 11.84*** | 7.950*** | 4.247*** |
| | (0.9671) | (0.7749) | (0.8001) | (0.7615) | (0.6740) | (0.6753) |
| Apr-28 + | 27.09*** | 22.36*** | 22.43*** | 9.841*** | 6.730*** | 3.510*** |
| | (0.8019) | (0.6288) | (0.6361) | (0.5908) | (0.5367) | (0.5280) |
| *Fixed-effects* | | | | | | |
| State | | Yes | | Yes | Yes | Yes |
| Industry | | Yes | | Yes | Yes | Yes |
| Firm Age | | Yes | | Yes | Yes | Yes |
| Employment Bins | | Yes | | Yes | Yes | Yes |
| State-Ind.- Age-Emp. | | | Yes | | | |
| Payment Records | | | | Yes | | Yes |
| *Continous Controls* | | | | | | |
| Via Score (2019) | | | | | Yes | Yes |
| *Fit statistics* | | | | | | |
| Observations | 413,865 | 413,865 | 410,737 | 413,865 | 413,865 | 413,865 |
| $R^2$ | 0.03453 | 0.12155 | 0.15296 | 0.18516 | 0.28367 | 0.29144 |

Figure E.1: Effects of the PPP on Commercial Credit Score by 2020 PPP Loan Status and Timing (Relative to the 10-Day Window of Delay)

*Note*: This figure is equivalent to Figure 1 in the main text. The only difference is the classification of firms. We follow Doniger and Kay (2021) and divide PPP borrowers into four groups: (1) those that received funds early, (2) just before the 10-day window (April 16 through 26) of funding delay when no loans were made, (3) right after the 10-day window, and (4) later on. The exact dates are displayed in the legend. Shaded areas are 95% confidence bands; standard errors are two-way clustered at the state-industry and time levels. Sources: D&B and authors' calculations.



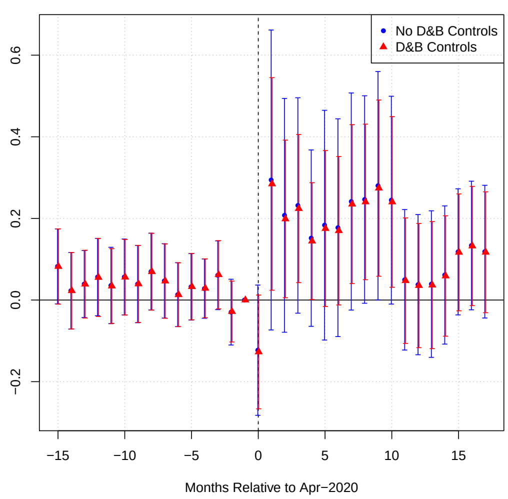

(a) Firm + Time + Non-Financial




(b) Saturated

Figure E.2: Effect of the 10-Day Funding Delay on Employment

*Note:* This figure is equivalent to Panel (b) of Figure 3 in the main text, except that we replace the regressor of community bank share with a different indicator of exogenous shock to PPP supply—the share of loans delayed as defined in Doniger and Kay (2021). In our analysis, it is defined as the share (by volume) of PPP loans made during the period of April 14 through 16, 2020, relative to all loans made during the period of April 14 through 28, 2020. Doniger and Kay (2021) find that the share of loans made just before the 10-day window of funding delay was random and not correlated with any pre-COVID attributes of small businesses even at the Core Based Statistical Area (CBSA) level. This regressor is normalized to have a standard deviation of one. Vertical bars depict 95% confidence bands; standard errors are clustered at the county level. The vertical line (at $t = 0$) represents April 2020. Sources: D&B, BLS, Census Bureau, SBA/Treasury PPP Release, and authors' calculations.



# F Effect of the PPP on Employment Must Account for Heterogeneity: Additional Results

In this appendix we report additional results of our employment analysis from Section 5.

**PPP Receipt and Community Banks**

In the main text, we show that the community bank share loses its ability to explain PPP allocation once we account for firms' financial condition at the county level. We show in Table F.1 that this correlation is also present when we use firm-level data. Even though the coefficient on community bank share is still statistically significantly, its magnitude falls by 82 percent once we account for firms' financial condition. In our sample of firms of 3 to 500 employees (Table F.2), the effect becomes insignificant.

**Firms' Financial Condition and Community Banks**

In the main text, we show that the community bank share is correlated with firms' financial health at the county level, violating the exclusion restriction needed for the community bank share to be a valid IV. Table F.3 corroborates that this correlation is also present with firm-level data.

**Firms' Financial Condition, PPP Receipt and Employment**

We use a variant of Equations (5) and (6) (that is, we include only $\overline{VS}_{c,2019}$ or only $CB_c$) to compare the effect of better pre-COVID firm health versus a higher community bank share on the volume of PPP loans in a county as well as the evolution of employment afterward. The coefficients, reported in F.1, show that a county would have received nearly twice the (normalized) volume of PPP loans every month of the program in 2020 and experienced a reduction in the employment rate nearly twice as large if its average Viability Score had been one standard deviation higher compared to if its community bank share had been one standard deviation higher. This corroborates the findings presented in Figure 3.

Table F.1: Community Bank Share and PPP Allocation: Firm Level

*Note:* In this table we show the relationship between the community bank share (branches) in a county with PPP receipt in the first round (April 3 to August 8, 2020). Fixed effects and controls are defined as in Table 1. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively. Sources: D&B, Summary of Deposits, and authors' calculations.

| | (1) | (2) | (3) |
|---|---|---|---|
| Community Bank Share (Branches) | 0.0837*** | 0.0283*** | 0.0158*** |
| | (0.0106) | (0.0045) | (0.0044) |
| *Fixed-effects* | | | |
| State | | Yes | Yes |
| Industry | | Yes | Yes |
| Age Bin | | Yes | Yes |
| Employment Bins | | Yes | Yes |
| Payment Records | | | Yes |
| *Continuous Controls* | | | |
| Via Score (2019) | | | Yes |
| *Fit statistics* | | | |
| Observations | 416,557 | 416,557 | 416,557 |
| $R^2$ | 0.00210 | 0.09410 | 0.16262 |
| Within $R^2$ | 0.00210 | 0.00014 | $4.7 \times 10^{-5}$ |

Table F.2: Community Bank Share and PPP Allocation: Firm Level for Firms with 3 to 500 Employees

*Note:* Estimates in this table differ from those in Table F.1 due only to a difference in sample: Only firms with 3 to 500 employees in February 2020 are included here. Fixed effects and controls are defined as in Table 1. Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively. Sources: D&B, Summary of Deposits, and authors' calculations.

| | (1) | (2) | (3) |
|---|---|---|---|
| Community Bank Share (Branches) | 0.1082*** | 0.0317*** | 0.0037 |
| | (0.0140) | (0.0073) | (0.0071) |
| *Fixed-effects* | | | |
| State | | Yes | Yes |
| Industry | | Yes | Yes |
| Age Bin | | Yes | Yes |
| Employment Bins | | Yes | Yes |
| Payment Records | | | Yes |
| *Continuous Controls* | | | |
| Via Score (2019) | | | Yes |
| *Fit statistics* | | | |
| Observations | 172,111 | 172,111 | 172,111 |
| $R^2$ | 0.00247 | 0.08089 | 0.16949 |
| Within $R^2$ | 0.00247 | 0.00012 | $1.89 \times 10^{-6}$ |

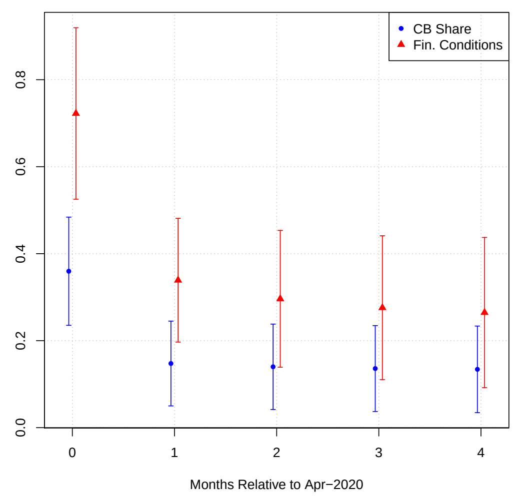
Table F.3: Pre-COVID Commercial Credit Score and Community Bank Share at the Firm Level

*Note:* This table is equivalent to Table F.1. The only difference is the dependent variable: Instead of a first-round PPP indicator, it is the CCS (as in Table 1). Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Sources: D&B, Summary of Deposits, and authors' calculations.

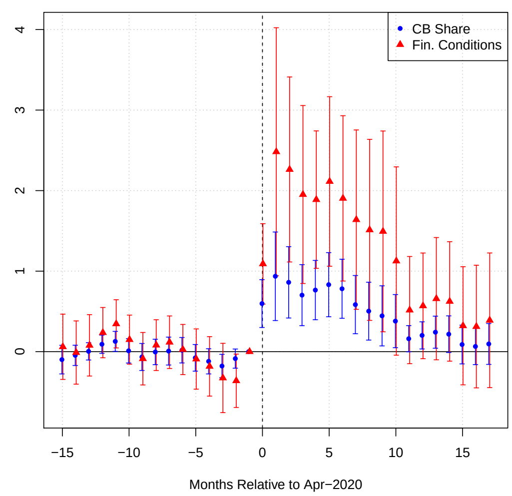
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Community Bank | 36.97*** | 13.54*** | 12.87*** | 12.86*** | 5.436*** | 6.114*** |
| Share (Branches) | (3.131) | (1.097) | (1.096) | (0.9662) | (0.9240) | (0.8908) |
| *Fixed-effects* | | | | | | |
| State | | Yes | | Yes | Yes | Yes |
| Industry | | Yes | | Yes | Yes | Yes |
| Firm Age | | Yes | | Yes | Yes | Yes |
| Employment Bins | | Yes | | Yes | Yes | Yes |
| State-Ind.-Age-Emp. | | | Yes | | | |
| Paydex and Default Data | | | | Yes | | Yes |
| *Continuous Controls* | | | | | | |
| Via Score (2019) | | | | | Yes | Yes |
| *Fit statistics* | | | | | | |
| Observations | 413,291 | 413,291 | 410,160 | 413,291 | 413,291 | 413,291 |
| $R^2$ | 0.01042 | 0.10635 | 0.13832 | 0.18250 | 0.28206 | 0.29091 |

Figure F.1: Effects of Community Bank Share versus Pre-COVID Financial Condition on PPP Allocation and Employment

*Note:* Panel (a) of this figure depicts the estimates of $\beta_m$ (if only $CB_c$ is included) versus $\delta_m$ (if only $\overline{VS}_{c,2019}$ is included) in a regression analogous to Equation (5) but including only one of these two regressors. The second regressor, the county average pre-COVID Viability Score, is standardized and multiplied by $-1$ (so that a higher score means better quality) to facilitate comparison. Dependent variable: cumulative volume of PPP loans over weeks of payroll at firms eligible for the PPP in county $c$ and month $t$. The coefficients depicted in Panel (b) are defined analogously, but for the dependent variable employment rate (in percentage points) as in Equation (6). Vertical bars depict 95% confidence bands; standard errors are clustered at the state level. Regressions are weighted across counties by the number of workers at eligible firms in 2019. The vertical line (at $t = 0$) represents April 2020. Sources: D&B, BLS, Census Bureau, SBA/Treasury PPP Release, Summary of Deposits, and authors' calculations.




(a) PPP Volume




(b) Employment Rate (% pt.)

## F.1 Alternative IV for PPP Lending à la Granja et al. (2022)

We replace $CB_c$ with an alternative bank-based supply shock to PPP lending constructed in Granja et al. (2022). At the bank level, the PPP exposure (PPPE) is defined as the gap between a bank's market share in Phase 1 of the 2020 PPP lending and its pre-pandemic small-business lending, that is,

$$PPPE_b = \frac{\text{Share PPP}_b - \text{Share SBL}_b}{(\text{Share PPP}_b + \text{Share SBL}_b)/2}, \tag{F.1}$$

where Share $\text{PPP}_b$ is bank $b$'s share of all PPP loans at the end of Phase 1, and Share $\text{SBL}_b$ is bank $b$'s share of small-business loans as reported in its 2019:Q4 regulatory filing (Call Reports). We compute county PPP exposure as the branch-weighted average of bank PPP exposure in that county.

Tables F.4 and F.5 are, respectivly, the equivalent of Tables F.1 and F.3in the main text, and Figure F.2 is the equivalent of 3.

The estimates exhibit qualitatively the same patterns as those using the community bank (branch) share in a county as the regressor, although some of the results are quantitatively different. As shown in Figure F.2, PPP exposure retains some of its ability to explain the *amount* of PPP allocation even after we include $\overline{VS}_{f,2019}$ as a control. This result is not surprising: This instrument is built on the early PPP loans, and those loans were, on average, significantly larger in size and thus constituted a dominant share of PPP lending to a locality, as highlighted by Granja et al. (2022). But this also means that this correlation between PPP exposure and PPP allocation is significantly weakened once we control for firm size and financial health (Table F.4). This is to be expected given our findings that early receipt of PPP funding is also correlated with (better) firm financial health even after we control for commonly observed non-financial attributes. Consistent with this firm-level result, Figure F.3 shows that PPP exposure is *negatively* correlated with the number of loans (relative to eligible firms) at the county level, and yet including the financial control also attenuates this relationship.[8] Overall, our estimates indicate that the underlying average financial condition of firms in a county is strongly correlated with the community bank share as well as the PPP exposure.

[8] For completeness, we also estimate how the community bank share affects the number of PPP loans in a county. We find that controlling for firms' financial condition *increases* the predictive power of this instrument. The likely explanation is that for a given level of firm creditworthiness, a greater presence of community banks resulted in a larger number of PPP loans.

Table F.4: PPP Exposure and PPP Allocation: Firm-Level

*Note:* This table is equivalent to Table F.1, except that the regressor community bank share (branches) is replaced with an alternative indicator: county PPP exposure as constructed in Granja et al. (2022). Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively. Sources: D&B, SBA/Treasury PPP Release, Call Reports, and authors' calculations.

| | (1) | (2) | (3) |
|---|---|---|---|
| PPPE (County) | 0.0915*** | 0.0388*** | 0.0123* |
| | (0.0164) | (0.0066) | (0.0064) |
| *Fixed-effects* | | | |
| State | | Yes | Yes |
| Industry | | Yes | Yes |
| Age Bin | | Yes | Yes |
| Employment Bins | | Yes | Yes |
| Payment Records | | | Yes |
| *Continuous Controls* | | | |
| Via Score (2019) | | | Yes |
| *Fit statistics* | | | |
| Observations | 416,557 | 416,557 | 416,557 |
| $R^2$ | 0.00157 | 0.09408 | 0.16259 |
| Within $R^2$ | 0.00157 | 0.00012 | $1.3 \times 10^{-5}$ |

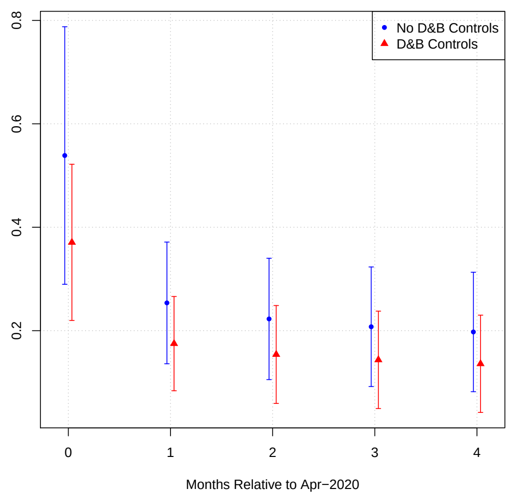
Table F.5: Pre-COVID Commercial Credit Score and PPP Exposure at the Firm Level

*Note:* This table is equivalent to Table F.3, except that the regressor community bank share (branches) is replaced with an alternative indicator: county PPP exposure as constructed in Granja et al. (2022). Robust standard errors clustered at the state-industry level are in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Sources: D&B, SBA/Treasury PPP Release, Call Reports, and authors' calculations.

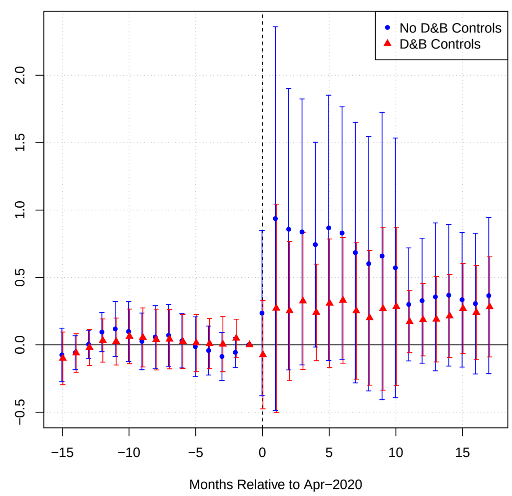
| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| PPPE (County) | 38.90*** | 13.71*** | 13.59*** | 10.40*** | 2.393 | 2.430 |
| | (6.683) | (2.852) | (2.958) | (2.405) | (2.053) | (1.975) |
| *Fixed-effects* | | | | | | |
| State | | Yes | | Yes | Yes | Yes |
| Industry | | Yes | | Yes | Yes | Yes |
| Firm Age | | Yes | | Yes | Yes | Yes |
| Employment Bins | | Yes | | Yes | Yes | Yes |
| State-Ind.-Age-Emp. | | | Yes | | | |
| Paydex and Default Data | | | | Yes | | Yes |
| *Continuous Controls* | | | | | | |
| Via Score (2019) | | | | | Yes | Yes |
| *Fit statistics* | | | | | | |
| Observations | 413,291 | 413,291 | 410,160 | 413,291 | 413,291 | 413,291 |
| $R^2$ | 0.00251 | 0.10575 | 0.13781 | 0.18191 | 0.28194 | 0.29077 |

Figure F.2: Explanatory Power of PPP Exposure for PPP Allocation and Employment

*Note:* This figure is equivalent to Figure 3 in the main text, except that the regressor community bank share (branches) is replaced with an alternative indicator: county PPP exposure as constructed in Granja et al. (2022). Vertical bars depict 95% confidence bands; standard errors are clustered at the state level. Regressions are weighted across counties by the number of workers in eligible firms in 2019. The vertical line (at $t = 0$) represents April 2020. Sources: D&B, Call Reports, Census Bureau, SBA/Treasury PPP Release, and authors' calculations.



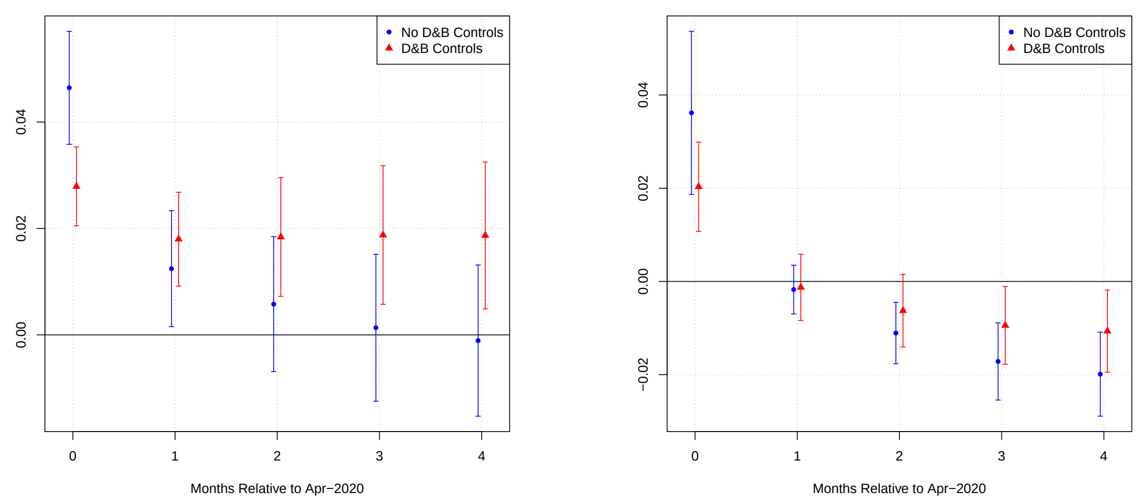

(a) PPP Allocation




(b) Employment Rate (% pt.)

Figure F.3: Explanatory Power of PPP Exposure for PPP Allocation and Employment: Number of PPP Loans Relative to Eligible Firms

*Note:* This figure is equivalent to Figure 3- Panel (a) in the main text, except that the dependent variable is replaced with the number of PPP loans relative to eligible firms. The independent variables of interest are either the community bank share (Panel a) or the county PPP exposure as constructed in Granja et al. (2022) (Panel b). Vertical bars depict 95% confidence bands; standard errors are clustered at the state level. Regressions are weighted across counties by the number of workers in eligible firms in 2019. The vertical line (at $t = 0$) represents April 2020. Sources: D&B, Call Reports, Census Bureau, SBA/Treasury PPP Release, and authors' calculations.




(a) Community Bank Share

(b) PPP Exposure