Ten Days Late and Billions of Dollars Short: The Employment Effects of Delays in Paycheck Protection Program Financing

Cynthia L. Doniger
Federal Reserve Board

Benjamin Kay
Federal Reserve Board

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Abstract

Delay in provision of Paycheck Protection Program (PPP) loans due to insufficient initial funding under the CARES Act substantially and persistently reduced employment. Delay of loans increased job losses in May and persistently reduced recalls throughout the summer. Effects are inequitably distributed. Employment effects are larger among the self-employed, less well paid, less well educated and—importantly for the design of future programs—in small firms. Our estimates imply the PPP saved millions of jobs but larger initial funding could have saved millions more, particularly if it had been directed toward the smallest firms. More than half of jobs lost to delayed PPP funding are lost to firms with fewer than 10 employees, despite such firms accounting for less than 20 percent of employment.

JEL Classification:
E24: Employment • Unemployment • Wages • Intergenerational Income Distribution • Aggregate Human Capital • Aggregate Labor Productivity
H81: Governmental Loans • Loan Guarantees • Credits • Grants • Bailouts
J21: Labor Force and Employment, Size, and Structure
G32: Financing Policy • Financial Risk and Risk Management • Capital and Ownership Structure • Value of Firms • Goodwill

Keywords:
PPP, Countercyclical Fiscal Policy, Covid-19, Kurzarbeit, Income Support, Small Business Lending

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1 Introduction

1.1 Motivation

In the United States, the economic devastation of caused by COVID-19 has hit smaller firms particularly hard. Unlike larger firms with their better access to credit and substantial cash reserves (Hankins and Petersen (2020)), smaller firms typically have less than a month of cash reserves to insulate them (Farrell and Wheat (2016)). The shock of COVID-19, combined with these limited financial reserves, imperils the jobs of the the 60 million workers or 48% of the workforce (SBA (2018)) employed at smaller firms. The CARES Act[1] provided for funding for loans to small businesses under the Paycheck Protection Program (PPP) in response to the COVID-19 pandemic and the economic havoc wreaked by non-pharmaceutical interventions to fight the pandemic. Traditional de facto and de jure American income support programs like unemployment insurance, stimulus grants, disability insurance, and the Supplemental Nutrition Assistance Program do not promote or preserve the employee-employer relationship. In contrast, the PPP set aside hundreds of billions of dollars to keep employees attached to their existing small business employers.

1.2 Research Questions

This paper asks three main questions. First, how effective was the PPP in preventing job losses? Second, were jobs saved by the PPP long lasting or did they disappear when the funding was exhausted? Third, were the effects equally distributed? These research questions fit within a broad literature evaluating the efficacy of counter-cyclical fiscal policy. In addition, the unique design, and design failures, of the PPP program allow our results to speak to a broader literature on the employment effects of firm liquidity, particularly during times of economic crisis and within very small businesses.

[1] The CARES act is also known as H. R. 748 and the Coronavirus Aid, Relief, and Economic Security Act

1.3 Contributions

We make three contributions to answering these questions. First, we find that expedient PPP loan issuance causes an economically large and a statistically robust improvement in labor market outcomes, with particularly large effects for employees of small businesses. Thus, in notable contrast to prominent preliminary analyses by Chetty et al. (2020) and Autor et al. (2020), the PPP appears to have been quite effective: delay in provision of PPP loans due to insufficient initial funding under the CARES Act causes sizable and persistent negative labor market outcomes. These employment benefits extend through September, which is after the three months of payroll support provided by the PPP loan are exhausted, suggesting the PPP promoted lasting firm health. The effects of delayed PPP loans are inequitably distributed, with self-employed workers and workers at the smallest firms particularly hard hit.

Second, the natural experiment we study illustrates the importance of cash on hand for small businesses' employment decisions. It is well documented that small businesses use limited external financing (Vos (1992), Bitler et al. (2001), Mach et al. (2006)). However, there is also evidence from surveys of small businesses (Vos et al. (2007)) that that, under normal conditions, 90 percent or more such firms have sufficient external financing. Of course, the period of the US COVID-19 epidemic is not normal conditions, and this paper contributes to a growing literature establishing that limited cash-on-hand and working capital lowers firm labor demand and increases the sensitivity of labor demand to shocks (Barrot and Nanda (forthcoming), Bacchetta et al. (2019), Mehrotra and Sergeyev (2020), and Ghaly et al. (2017)).[2]

Third, we use these estimates to model the aggregate jobs lost by the ten day delay in the PPP, the number of jobs saved by the PPP, and the cost per job saved by the PPP. Our calculations imply that the PPP saved millions of jobs, and did so in a cost effective way, particularly because of the temporarily elevated replacement rate of unemployment

[2]Important additional papers in this literature include Chodorow-Reich (2014), Benmelech et al. (2011), and Greenstone et al. (2020). Key theory papers include Wasmer and Weil (2004), Eckstein et al. (2019), Gertler and Karadi (2015), and Petrosky-Nadeau and Wasmer (2013)

insurance during the crisis. Further, our results suggest that a program focused on getting funding quickly to the smallest firms would have saved jobs more cost effectively.

1.4 Methods

The PPP met unexpectedly high demand and exhausted the initial congressionally allocated \$349 billion in funding on April 16^{th}, less than two weeks after opening. Congress subsequently allocated an additional \$321 Billion in PPP funds, and the program resumed making loans on April 27^{th}. To measure the effects of PPP on employment, we exploit a ten day delay between April 16^{th} and 27^{th} in the approval of PPP loans. We identify the effect of delayed financing by examining the impact of loan timing in the window of time around April 14^{th}-28^{th}. In particular, we examine the labor market consequences of delaying a PPP loan by 10 days using a difference-in-difference with dynamic treatment effects estimation strategy. Our key identifying assumption is that borrowers approved on the 16^{th} and the 27^{th} are identical (conditional on controls) and that delay does not alter loan demand. To validate our assumptions, we document that while the characteristics of PPP loan recipients vary dramatically over the course of the program these trends halt during the 10 days of loan delay around which we construct our event window.

Our data and identification strategy enable us to document heterogeneity along key dimensions. In particular, we investigate the differential effects of the PPP by employer size and self-employment as well as by education and earning ability.

1.5 Findings

A ten day delay in funding PPP loans had substantial negative labor market consequences. We find strong and persistent effects of loan delay from May through September. For a core based statistical area (CBSA)[3] with 1 percentage point fewer loans in the early part of our event window, the unemployment rate is 10 basis points lower, the labor force participation

[3] One or more adjacent counties anchored by a city-like area.

rate is 5 basis points higher, and a measure of the nonemployment rate[4] is 14 basis points higher in May with effects remaining above 10 basis points through the summer. These point estimates imply that if the PPP funding under the CARES Act had been 10% larger, enabling the share of delayed loans in our event window to be 20% lower, the unemployment rate would have been about 2 percent lower, non-participation about 1 percent lower, and our measure of non-employment about 2.8 percent lower.

Delayed financing drives labor market divergence through a combination of differential job-loss and job-finding rates. In the spring, job-losses is relatively more important (like Bacchetta et al. (2019)); however, over the summer persistent differences in job-finding, primarily driven by the absence of recall from layoff, accumulate to become the dominant driver of differentials (like Mehrotra and Sergeyev (2020)). These results are consistent with conceiving of business owners as risk averse entrepreneurs with risky income deriving from their small business and facing a cash-in-advance constraint that must be internally financed. Delay in receiving PPP loans forced business owners to run down their buffer stock of liquidity, driving them closer to the internally financed cash-on-hand constraint and thus convexifying their value function (Carroll and Kimball (2001)). As a result, recipients of delayed loans recover employment less quickly as they seek to rebuild their buffer-stock.

There are important heterogeneities in these effects. Employment effects for employees at smaller firms and among the self-employed are larger, potentially because they have smaller cash reserves (Farrell and Wheat (2016)) and poorer access to credit (Berger and Udell (2002). These differences, which our identification strategy and data uniquely allow us to observe, reconcile our estimated effects, which are large, with those of prominent competing studies, which are small (Autor et al., 2020; Chetty et al., 2020). Indeed, our point estimates suggest that nearly half of the jobs lost were lost due to insufficient funding under the CARES

[4] To construct the nonemployment rate we construct a broader notion of labor force participation that includes nonparticipants who were recently participating. In our view, this more accurately reflects changing labor market conditions during the COVID-19 shock since the nature of the shock induces larger than typical flows out of the labor force.

Act were in firms with less than 10 employees while such firms typically account for less than 20 percent of employment.

Our analysis also reveals greater job-loss among the self-employed than the employees of private firms, We also find greater losses among those predicted to have low earnings and those with less education. While, these individuals' unemployment insurance replacement rates were most greatly increased by the CARES Act and subsequent federal interventions in the unemployment insurances system, our findings regarding differential job loss for these groups are robust to detailed controls for these variations.[5]

1.6 Robustness

Our results are robust to alternative definitions of the event window. A narrower window produces job-losses over time that are more persistent and more back loaded while a slightly wider event window produces nearly identical results as our preferred specification. We also estimate an alternative, counter-factual event windows away from the 10-day delay. Loan delay outside the window is likely correlated with unobserved attributes that predict employment differentials prior to treatment. In particular, for alternative event windows we observe non-trivial pre-trends, indicative of non-random selection.

In addition, our results are robust to a broad set of controls for industry, occupation, demographics, non-pharmaceutical interventions (NPIs), other fiscal stimulus (in particular unemployment insurance), survey design, and the ongoing evolution of the COVID-19 pandemic itself.

1.7 Road Map

Section 2 overviews the features of the PPP essential for our analysis. Section 3 discusses the sources of our data and how we aggregate it. Section 4 details our identification strategy,

[5]Supplemental regressions, available upon request. show no causal evidence that PPP loan delay and variation in the unemployment insurance system had any reinforcing or offsetting effects on the baseline PPP effect.

econometric specifications, and describes our model. Section 5 presents the results of our empirical and modeling analysis. Section 6 shows the robustness of ours results to narrower and wider windows around the ten day delay and shows the results in other problematic windows. Section 7 explains how our paper fits in with the related PPP and Kurzarbeit literature and lays out our thought experiment which estimates the total jobs saved and costs per job saved of the PPP. We conclude with Section 8.

2 The Paycheck Protection Program

The PPP, created by the CARES Act, provided small businesses, small nonprofit and religious organizations, and sole-proprietors and independent contractors with loans to help their employees during the extreme COVID-19 related economic difficulties they faced during the spring and summer of 2020. Here we provide a brief summary of the parameters of the program as they applied to the loans in our studied time-frame.[6] As our study focuses on the consequence of exhaustion of the PPP funding supplied by the CARES Act, we then provide detail of the timing exhaustion and related events.

2.1 PPP Design

The PPP was run by the SBA in consultation with the US Department of the Treasury. Borrowers had to meet a multi-part test to be eligible for a PPP loan. However, unlike virtually all other forms of credit, these were not tested for collateral or ability to repay and the paperwork requirements were modest (estimated by the SBA at about two hours). Once a borrower had their PPP application approved, their lender was supposed to send the funds to the borrower within 10 calendar days.[7] The qualifying loan amount per employee was 2.5

[6] In the Appendix A we provide significantly more detail about program as a whole.

[7] This changed somewhat over the program. Initial program documentation said that lenders had 10 days to make their distributions, but was unclear on what this actually required and when the clock started ticking. Eventually the Treasury and the SBA issued a rule that for loans approved on or before April 28, 2020, lenders had 10 calendar days from April 28, 2020 to fund the loan. For loans after this, the loan had to fund in 10 calendar days.

times the average total monthly per-employee payments for payroll costs for the year prior to the loan date (or, at the option of the borrower, for 2019) including up to $100,000 in annualized cash compensation (wages, salaries, and cash tips) plus group insurance premiums (including health care benefits). The PPP loans studied in this paper had a term of two years at a cost of one percent per year interest, but were forgivable if they were used for qualified expenses during a specified period. For the loans we focus on, borrowers were told they had 8 weeks after fund distribution to spend the proceeds on qualified expenses and at least 75% of funds had to be spent on qualified payroll expenses.

2.2 Exhaustion of the CARES Act Funding Allocation

Table 1 presents a a timeline of the major events in the first few weeks of the PPP. The CARES Act was signed into law on March 27, 2020 and appropriated $349 billion in PPP loans. The first PPP loans were approved on April 3, 2020, and it was almost immediately clear that these fund were insufficient to meet PPP loan demand, with major political figures discussing in the business press that additional appropriations were needed. Press accounts of the early days of the PPP highlight inequities and start up pains as banks introduce PPP offerings: some banks are slow participate, with many prioritizing their existing customers and larger firms. PPP loan volumes were high but could have been much higher. Capacity constraints seemed binding and many ready, qualified, and willing borrowers could not get a loan. These early approved loans were unusually large loans, to unusually large firms, from unusually small banks. Figure 4, Panel E shows the average loan in the first week was for about three times the dollars and saved three times the jobs of typical loans throughout the program. The ten largest US commercial banks did one-quarter of the PPP loans over the life of the program, but only 8 percent in the first week.

By the second week in April, it was clear that the program was overwhelmed. PPP loan applications poured into the banks, and banks responded by continuing to prioritize larger and especially existing customers. Banks were also directed to do so by guidance from the

Treasury and the SBA. Funding was being depleted rapidly (see Figure 1). Congress was working on a second round of PPP appropriations and it looked likely that this would happen but not when the additional appropriations would become available.

On April 16, the CARES Act PPP funding was depleted. Political discussions of a second PPP appropriation were ongoing, resulting in a senate bill on April 21, a house bill on April 23, and the president signing the PPP and Health Care Enhancement Act of 2020 (PPP Act) into law on April 24. The PPP Act appropriates an additional $321 billion dollars (for a total of $670 billion) for PPP loans and banks began issuing additional PPP loans on April 27. Loan demand was strong in the initial two weeks of the PPP Act funding before becoming subdued through the remainder of the summer, as can be seen in Figure 1.

Importantly, strong trends in borrower's observables abated in the period around the 10 days where there were no PPP loans. These are illustrated in Figure 4 and discussed in greater detail in Section 4. In addition, while there clearly is discernible geographic variation in areas that received relatively more loans early on from the CARES Act funding and that received funds later from the PPP Act funding (Figure 6), the variation in earlier and later loans within the narrow window surrounding the exhaustion of CARES Act funding and resumption of lending under PPP Act funding has no clear geographic concentrations (Figure 3).

3 Data

This section describes our main data sources: SBA, PPP Loan Level Data, and the Bureau of Labor Statistics' Current Population Survey (CPS). In Appendix B we provide information about auxiliary data sources utilized as controls and in robustness checks.

3.1 SBA PPP Loan Level Data

The U.S. Department of the Treasury and the Small Business Administration (SBA) provides comprehensive loan level data on PPP loans in all states, major territories (American Samoa, Virgin Islands, Guam, and Puerto Rico), and the District of Columbia (U.S. Treasury (2020)). In the August release of PPP data we use, for loans for amounts less than \$150,000, the data include the Loan Amount, City, State, ZIP Code, NAICS Code (industry), Business Type (incorporation type or non-profit status), Number of Jobs Retained, Approval Data, and Lender (Bank).[8] For the larger loans (\$150,000 to \$10 million), the exact loan amounts are replaced with loan ranges (specifically \$5-10 million, \$2-5 million, \$1-2 million, \$350,000-1 million, \$150,000-350,000), which we map to the middle of their ranges (e.g., a loan coded as \$1-2 million we treat as a loan for \$1.5 million).

We aggregate loan activity by date and the county or CBSA (in general, county and CBSA results are very similar but a CBSA better corresponds to a labor market and is our preferred level of aggregation). Neither County nor CBSA are fields in the PPP data. We use the HUD ZIP to FIPS and HUD ZIP to CBSA cross-walks respectively to map each loan to a county and a CBSA (HUD (2020)). In some cases, the geographic areas of a ZIP Code is not contained entirely within a county (about 28 percent of ZIP Codes are in more than one county). More rarely, the geographic areas of a ZIP Code is not contained entirely within a CBSA (about 17 percent of ZIP Codes are in more than one CBSA). In these cases, we map the ZIP Code to the county and CBSA which contains the plurality of the businesses of that ZIP Code.

We want to minimize the measurement error introduced by these cross-walks. For the majority of the loans we cannot check the resulting mapping. However, the larger loans ($\geq \$150k$) also report a street address in addition to State, City, and ZIP Code. Using ARC GIS, we map each one of these complete addresses (Street Information, State, City, and ZIP

[8] The loan data has additional fields devoted to borrower race, gender, and veteran status, but these fields are overwhelmingly unanswered, missing for more than 90% of loans.

Code) to a county. Comparing the county mapped by our HUD cross-walk with plurality of businesses approach to the ARCGIS approach, the results agree for more than 98% of these large loans. This gives confidence that we are correctly attributing PPP loans to the county (and therefore CBSA as well) in which the borrowing firm operate.

Table 2 reports basic summary statistics about the number of PPP loans, broken down by loan size. The PPP funded almost 4.9 million loans, of which 4.2 million were for less than \$150,000. However, the roughly 650,000 (14%) PPP loans for \$150,000 or more were awarded 74% of the funds. The distribution of PPP loans was very broad. Using the 2018 Census County Business Patterns to estimate the number if establishments, we find that in a typical county about 60% of small businesses received a PPP loan (Bureau (2020a)). To avoid selection issues induced by firm and managerial quality (much more on this in Section 4), we focus on the PPP loans issued in the window of April 14th and 28th 2020, about a third of the total. About 44% of the loans in this event window were late (approved on April 27th or 28th with PPP Act funding). Table 3 shows similar statistics of overall and late loan activity for the 20 most active CBSAs in the U.S. Treasury (2020) data. Even among large metropolitan areas there is considerable variation, with Miami having 48% of loans late in the window but Philadelphia having only 35% of loans late in the window. Across all CBSAs the variation is even larger, with a standard deviation of late loan share of about 10%.

3.2 Current Population Survey

At the core of our analysis are the labor market outcomes for American workers, which we measure using microdata from the Bureau of Labor Statistics' (BLS) Current Population Survey (CPS) data from 2019 to 2020. As of the time of writing the most recent available microdata cover through September 2020. The CPS is a monthly rotating panel of approximately 65,000 U.S. households. Perhaps the most notable use of CPS micro-data is the the BLS use it calculate key official statistics including the unemployment rate.

The CPS is designed as short rotating panel with a core monthly survey administered to each household each wave. Each household is surveyed monthly for four consecutive months and then re-surveyed in the same four months one year later. Each monthly survey contains a core set of detailed questions about labor market activities during the week containing the 12^{th} day of the month. These questions are used to classify each adult non-institutionalized, civilian into employed, unemployed, or not participating in the labor force. Linking the survey across waves enables us to observe if an individual transits between these states.

In addition to the variables needed to construct the national unemployment rate and other aggregates, the core survey questions contain demographic and job characteristics such as industry, occupation, and class of worker (e.g. self-employed, private sector employee, public sector employee). In addition, each March respondents report the size of their employer, inclusive of all of its establishments. Using the panel structure, we can link each responses December through June to respondents employer size in the nearest March.

Our empirical strategy, coupled with these data, allows us to identify the effects of the PPP on key sub-populations, namely employees of small firms and the self-employed that are either unmeasurable or unobserved or both in key complementary studies (Autor et al., 2020; Chetty et al., 2020).

Additional details of the data, which we utilize in robustness checks, are discussed in the Appendix B. This includes features related to unemployment insurance and the survey instrument's sensitivity to the Covid-19 outbreak.

3.3 Geographic Merge

Due to insufficient sample size, the CPS cannot be used to construct aggregate statistics at geographies smaller than a state. In addition, geographic information is suppressed in the micro-data when geographic identification would violate confidentiality requirements. In practice, this means that we can identify survey respondents' county of residence for 281 counties. Just over 40 percent of the sampled population live in these counties. Grouping counties into core-based statistical areas (CBSA) improves geographic coverage: reidentification risk is reduced by pooling counties within a CBSA. We can identify 258 CBSA in which 75 percent of the sampled population live. Our primary analysis matches loans in the SBA, NPI, and case-count data data to individuals in the CPS according to CBSA.[9] We match SBA loan data, to individuals residing outside of identified CBSAs according to their state excluding the identified areas, with a distinction between the rural and urban regions of the state.[10]

4 Identifying the Effects of Loan Delay

We identify the effects of a delay in PPP funding on employment of current and recent employees of small firms. To measure the effects of PPP on employment, we exploit a ten day delay between April 16th and 27th in the approval of PPP loans caused by the exhaustion and replenishment of PPP funds. See Table 1 for an overview of the key events of the PPP as reported in the Wall Street Journal. The program met unexpectedly high demand and exhausted the initial appropriation of \$349 billion on April 16^{th}, less than two weeks after opening, but congress subsequently appropriated an additional \$320 Billion in PPP funds, and the program resumed making loans on April 27^{th}. In our view, due to bank processing constraints and other complications of participating in a novel program, firms did not have knowledge of the precise timing of the approval of their PPP loans. By and large (all the more so conditional on appropriate controls), which firms received early and late PPP loans

[9]Our general results are very similar when we repeat our analysis based on a county-level match. The county-level match affords greater geographic precision at the expense of sample size and a greater skew toward counties with greater population density. In addition, in the context of our paper, greater geographic precision is not necessarily an advantage. Even if the county of residence of a respondent is disclosed in the CPS, the respondents relevant labor market is likely comprised of their county of residence and counties adjacent or nearby where they reside. We view the CBSA as more appropriately capturing the relevant labor market.

[10]For example, Montana contains seven CBSAs but only a single identified CBSA in the CPS: Billings. First, we match outcomes for Billings CBSA residents in the CPS to PPP loans issued to firms in the Billings CBSA. Next, we match agglomerated outcomes for residents of the other six CBSAs in the CPS to agglomerated PPP loans across those six CBSAs. Finally, we match agglomerated outcomes for all other Montana residents (the rural ones) in the CPS to agglomerated loans to firms outside of the CBSAs.

reflected chance rather than selection, allowing us to interpret this as a natural experiment on the effects of loan delay during an economic calamity.

Identification requires two assumptions:

Assumption 1. *Delay in loan timing from the* 16^{th} *to the* 27^{th} *is independent of pre-existing firm attributes.*

This is a two part assumption. First, more viable, productive, or better run firms are not more likely to get loans just before the April 16^{th} exhaustion of initial PPP funds than other firms. If this assumption were badly violated, it would compromise our identification by conflating these selection effects with the effects of delay.

Figure 4 plots the characteristics of the average CBSA receiving a PPP loan (CBSA characteristics weighted by share of loan dollars in that CBSA) by day from the advent of the PPP program until several weeks after our event window. These plots show clear trends in the characteristics of CBSA's receiving loans over time. In particular, in the early days of the PPP, the places that received more loans had higher banking density and concentration. These places were contained relatively more White people and Republican voters and were more likely to be rural.

While there is a distinct time trend in these observable, trend temporarily stabilizes across the 10-day gap of interest in our event window. This suggest that, while time trends exist over a longer horizon, our results are robust to spurious covariation with these observables. We conduct additional robustness checks of this assumption in Section 6. Meanwhile, as we discuss in section 7, these trends cast doubt on the identification strategy of Granja et al. (2020).

Assumption 2. *Loan demand is not a function of loan timing.*

We investigate robustness to this second assumption in section 6.

Whether or a not a firm gets early or late PPP funding (or none at all) is a firm level characteristic. Ideally, we would have firm-level labor market outcomes for PPP eligible firms matched to firm level loan information (particularly if they received early or late PPP loans). Unfortunately, this is not possible in our data. The CPS does not capture if individual firms utilized PPP loans, nor their timing if they did. To work around this data limitation, our analysis is at the core-based statistical area (CBSA) level instead of firm level. We explore how the share of early loans across CBSAs drives CBSA labor market outcomes. To work at the aggregated level, we must further assume that loan delay is independently assigned after controlling for CBSA characteristics, In various specifications, we control for state-month fixed effects (capturing a variety of state level policies including most of the variation in NPIs), industrial mix, occupation mix, COVID-19 cases and deaths, county level NPIs, unemployment insurance replacement rates, and the CPS interview type. Our results are robust to all of these controls.

The last day of loan making under the initial appropriation was April 16th and the first day of loan issuance under the second appropriation was April 27th. Our preferred specification use a measure of loan delay, *share delayed*, as the share of PPP loans issued to an CBSA in the window of April 14th to 28th, which includes the last three days of the original appropriation and the first two days of the second and final appropriation. Figure 1 depicts the cumulative loan issuance over the program's life and the uptake during our event window. Our window is not symmetric because the data suggest that funding was exhausted mid-day on April 16th, and thus we have the last two full days under the the first appropriation and the first two full days under the second appropriation. In a classic bias-variance trade-off, the narrower the window, the more plausible that our results are uncontaminated by selection effects. However, the wider the window, the more precise our estimates are and the less measurement error we have in our proxy for financing delay at the county and CBSA level. Our preferred specification matches nicely with the information readily available to small business owners at the time, and largely reflects loans applied for before it became obvious that loan funds would be quickly exhausted. Table 1 shows an overview of the key PPP events and what potential PPP borrowers would have known about the program from reading the Wall Street Journal at the time. For those interested in who knew precisely what when, Appendix D has detailed timeline of news related to PPP and associated sources.

In actuality, loan issuance spiked in the three days concluding with April 16th and in the two days beginning with April 27th. Figure 2 also shows that there was a large spike in loan demand around the April 17th-26th dates with no loans, and that loan issuance spike lasted longer than just one day on each side. This figure also shows that loan issuance on April 16^{th} fell short of that on the $14^{th}$and 15^{th}, indicating that funds were exhausted mid-day. In sensitivity analysis in Table 11, we find similar results for alternative event windows.

Equation 1 shows our general linear probability model specification:

$$
\begin{aligned}
\mathbb{I}[employed]_{i,t} = \alpha + \sum_{m=1}^{1} 0[\beta_m \mathbb{I}[month = m, year = 2020]\{share\,delayed_c\} + \\
\mathbb{I}[month = m, year = 2020] \times controls_{i,c,t} \times \Gamma_m] + \\
\sum_{\mu=1}^{12} [\mathbb{I}[month = \mu] controls_{i,c,t} \times \Psi_\mu] + \\
FE_{i,t} + \varepsilon_{i,c,t}
\end{aligned}
\tag{1}
$$

where *controls* contains an indicator for census region; urban, suburban, rural; interview type; and a month fixed effect. Note that since the CPS is a location-based survey c and i are co-linear and, as such, controlling for the individual fixed effect de facto controls for the CBSA fixed effect. We choose a linear probability model because our specification contains a large number of fixed effects.[11] Results are robust to adding additional fixed effects for two-digit NIACS industry, broad occupations, and state by time fixed effects, see Appendix C. We view state by time fixed effects as a flexible functional form, which captures the effects of NPIs, public policy, and to some extent the effects of density, weather and climate, and use

[11] Non-linear models such as probit and logit models may produce spurious results in a model with a large number of fixed effects, in particular, when the number of observations identifying a particular configuration of fixed effects is small.

of public transportation. Our preferred specification controls for direct measures of these, which, being more parsimonious, exhausts fewer degrees of freedom.

The β_m (monthly sensitivity of the probability of employment to share delayed) are the coefficients of interest. January 2020 is $m = 0$ and October 2020 is $m = 9$. Given the timing of the CPS reference week April 2020 ($m = 4$) is a pre-event window observation. The coefficients $\beta_5 - \beta_9$ trace out the evolving effect of loan delay through the summer and fall of 2020 while $\beta_1 - \beta_3$ test for the presence of a pre-trend. We view β_4 as comprising part of the pre-trend, but refrain from interpreting this coefficient due to the coincidence of the CPS reference week and the exhaustion of the initial PPP funding allocation.

This flexible specification allows us to test for heterogeneous effects by interacting *share delayed* or $(share delayed) \times (month\ FE)$ with firm, worker, and neighborhood covariates (e.g. race). In addition to using Equation 1 to measure employment sensitivities to loan delay, we can use the same specification to estimate the effect on labor force participation and flows into and out employment by replacing the left hand side variable with an indicator for participation, employment to non-employment transition, and so on.

5 Results

5.1 Employment

We find that a one percentage point increase in loan delay increased unemployment by approximately 10 basis points in May (Table 5). The effect gradually fades through the summer. These effects are illustrated in Figure 5 Panel A.I, which plots coefficients $\beta_0 - \beta_9$ and their 90, 95, and 99 percent confidence intervals from a model in which the only control is an indicator for the month interacted with the year 2020. Panel B.I plots the coefficients on the $month \times 2020$ fixed effects along with a 95 percent confidence interval, tracing out the baseline COVID-19 shock. These are the model implied average monthly

CBSA unemployment rates with the average percentage of delayed loans of 42. The baseline COVID-19 shock hits in April and the delayed PPP shock hits in May, as expected.[12]

Changes in the unemployment rate does not capture the full employment effects of the COVID-19 shock because that shock also triggered flows from employment to out of the labor force at a rate unprecedented in past recessions. To present a fuller picture of the true employment effects, we expand our sample to include those workers recently observed in employment or unemployment.[13] Panels A.II and B.II trace out the effect of delayed PPP financing and the COVID-19 shock on the labor force participation rate of the expanded sample. Both shocks increase non-participation with the COVID-19 shock's effect leading the PPP delay effect by about a one month. Panels A.III and B.III trace out the effect of delayed PPP financing and the COVID-19 shock on the nonemployment rate of the expanded sample of potential workers. As anticipated given heavy flows from employment to out of the labor force, the effects on nonemployment in May 2020 are about a third larger than the effects solely on unemployment.

A though experiment translates these regression coefficients into macroeconomic aggregates. Increasing the funding allocation under the CARES Act by 10 percent—an increase in the funding allocation in the CARES Act of about $35 billion—would have enabled reducing delay with our event window by roughly 20 percent in all locations. This would have boosted employment by nearly 2.8 million jobs. Further, this money would then not have needed to be allocated in the PPP Act.[14] Our results imply that these effects persisted long after the PPP funds were spent and through the end of our observation in the fall. As noted in Section 6, these figures are robust to the width of the event window.

[12]Recall that the CPS survey is on or near the 12th of the month, so that the workers at firms getting PPP loans in window (both early and late) did not have their funds when their employees were surveyed in April. Similarly, most firms did not experience the serious March COVID-19 shock before March 12th. Thus, in our data, the COVID-19 shock hits in April 2020 and the PPP loan delay shock hits in May.

[13]Specifically, we include all respondents who reported employment or unemployment in any of the potential seven CPS interviews prior to the month in question. Hall and Kudlyak (2019) find that past labor market experience is a better predictor of job finding than any standard BLS notion of marginally attached, and therefore we use this more accurate measure.

[14]for additional context, the average CBSA has a 44 percent share delayed with a standard deviation of 8 percent.

These effects are enormous. To understand these estimates, it is helpful to relate them to the recent and highly related contribution of Barrot and Nanda (forthcoming). Barrot and Nanda (forthcoming) find a 5.7 percent increase in employment for a 15 day earlier payment of 100 percent of payroll. This figure rises to roughly 7.2 percent in a "slack" labor market.[15] We find a 14.1 percent increase in employment for a 10 day earlier payment of 250 percent of payroll. Meanwhile, assuming, valiantly, that everything is linear Barrot and Nanda (forthcoming) numbers suggest a 12.16 % increase in employment for a 10 day earlier payment of 250 percent in a "slack" labor market. This places our estimated employment effect only slightly above theirs and, referring to Barrot and Nanda (forthcoming) Table V, suggests an external financing cost of 40 to 50 percent in Spring of 2020. This is not above the external financing costs implied by the estimates of Barrot and Nanda (forthcoming)—which they argue pertain in non-crisis times—in an economically significant way.

Our estimates reflect the effect of *earlier spending* but not *greater spending*, which complicates the comparison with other papers that focus on the aggregate effects of spending regardless of timing. For example, most assessments of the American Recovery and Reinvestment Act (ARRA) also find large employment effects, but for much greater funding outlays. Wilson (2012) finds the entire the $262 billion in ARRA government spending created 2.1 million jobs or a reduction in the unemployment rate of 1.4 percentage points. Feyrer and Sacerdote (2011) find that the $85 billion in local ARRA spending reduced unemployment by 0.4 percentage points. Chodorow-Reich et al. (2012) found the $88 billion medicaid grant as part of the ARRA lowered the national unemployment rate by about 2.1 percentage points.

Tables 5 shows that the unemployment and nonemployment effect estimates with controls for industry fixed effects; occupational exposure to the COVID-19 shock; new COVID-19 cases and deaths in each CBSA each month; controls for variation in survey non-response due to inability to conduct in-person interviews in spring and early summer; the Oxford index

[15] Barrot and Nanda (forthcoming) provide no information about the degree of slackness that they call "slack" or the semi-elasticity of their estimate to slackness, but it is safe to assume that May-October of 2020 is "slack" by any metric.

for the severity of COVID-19 state level non-pharmaceutical interventions; and controls for the state-level timing of federally mandated changes to unemployment insurance interacted with worker's eligibility for the programs. The effects depicted in A.I and A.III are robust. Column (1) and (3) present the coefficients plot in Figure 5 while columns (2) and (4) present the coefficients in the model with controls. Appendix C shows that results are robust to inclusion of various subsets of these controls; controlling for occupation fixed effects rather than occupational exposure to COVID-19; and controlling for state-by-month fixed effects in lieu of measures of state-level NPIs, local outbreak severity, and state-level variation in implementation of Federal changes to unemployment insurance.

These jobs gains, which stem solely from the *timing* of PPP loan allocation, are large relative to largest alternative estimate of the efficacy of the entire PPP program (Autor et al. (2020)). At first glance this seems shocking; however, it is easily reconciled by the heterogeneity in effects. Estimated marginal effects for small firms and the self-employed far exceed those for firms near the 500 employee cutoff: the variation which underlies the Autor et al. (2020) identification strategy. Indeed, our point estimates suggest that more than half of the jobs lost were lost due to insufficient funding under the CARES Act were in firms with less than 10 employees. In normal times, such firms account for less than 20 percent of employment.

5.2 Job Flows

The nonemployment effects of the PPP loan delay are driven by excess layoffs and labor market exit in May, combined with depressed recalls through the summer and early fall. Table 6 quantifies these results. Column (1) and (5) record the effects of loan delay each month on outflows to unemployment and inflows to employment. Columns (2)-(4) dis-aggregate the outflows into outflows to job seeking (excluding layoff), labeled "Separation"; to non-participation, labeled "Exits"; and to layoff. Layoffs are the primary driver of outflows with exit close behind. Columns (6)-(8) dis-aggregate the inflows into inflows from job seeking (excluding layoff), labeled "Accessions"; from non-participation, labeled "Reentry"; and from layoff, labeled "Recalls". Absence of recalls are the primary driver of depressed inflows. Note, due to the identification of flows based on information collected from non-employed individuals, outflows are roughly the sum of constituent parts while inflows are the weighted average.

On average, outflows from employment are 4 percent and inflows to employment are percent (Shimer, 2012). Volatility around these figures is 0.4 and 5 percent, respectively. Outflows and inflows reached their pre-COVID-19 peak and trough of 5.8 and 22 percent during the Great Recession.[16] Compared with these historical figures, our marginal effects, which imply a 0.95 percent increase in job loss and a 2.15 percent decrease in job finding for a 4 percent increase in the CARES Act funding allocation, are very large.

The shock to outflows from employment, while dramatic, it is temporary. In contrast, the shock to inflows to employment is sustained (the coefficients on through September are jointly statistically significant at the 1 percent level). Further, the May outflow from unemployment coefficient—coupled with the fraction of individuals in employment, unemployment, and nonemployeemnt at the onset of the pandemic—imply that May non-employment effects documented in the preceding subsection are accounted for by excess job-losses. However, as documented in Shimer (2012) and elsewhere, job finding in the U.S. is rapid and, as a result, spikes in job-loss do not result in persistently elevated unemployment. Historically, elevated unemployment stems in large part from depressed rates of job finding. We find a similar pattern in the response to the PPP funding delay shock. By August the nonemployment effects we observe are nearly fully accounted for by the accumulated effects of depressed job finding rates over the summer months.

[16]Note, all numbers quoted here are based on the quarterly averaged series to avoid excessive volatility.

5.3 Firm Heterogeneity

Effects of PPP loan delay are heterogeneous with respect to employer type and size. Table 7 reports effects by the self-employed and the employees of private companies.[17] As a robustness check, it also reports the effects on employee of local, state, or federal government, which are ineligible for PPP loans. Self-employed are highly sensitive to the timing of financing. These workers experience the most dramatic non-employment immediately following PPP loan delay, recover during the eight week term of employment specified for loan forgiveness, and then fall to non-employment again. By comparison, employees of private firms experience smaller but more persistent increases in non-employment. Statistically and economically insignificant coefficients for government employees serves to validate our empirical strategy, as these are ineligible for the program.

Table 8 reports effects by size for the months in which employer size can be identified using the CPS.[18] This analysis reveals that the aggregate effect is driven almost exclusively by the effect of PPP loan delay on the smallest firms. Of the 2.8 million jobs we estimate could have been saved by increasing the funding allocation in the CARES Act by 10 percent, 1.6 million—more than half—would have been in firms with less than 10 employees. For context, firms with less than 10 employees account for roughly 20 percent of employment in normal times.

5.4 Worker Heterogeneity

The rich worker characteristics data in the CPS enables us to explore the effect of PPP loan delay on different kinds of workers. Table 9, columns I - III shows that workers in the

[17] Note, we classify incorporated self-employed as self-employed rather than employees of private companies, due to their treatment as self-employed under the PPP forgiveness criteria. This is contrary to the usual BLS convention.

[18] As noted in Section 3.2 employer size is reported by CPS respondents who were surveyed in March. This rotating design of the CPS panel means that we are able to identify employer size as of the most recent March for nearly all respondents in March, at most 3/4ths of respondents in April and February; 1/2 of the sample in May and January; and 1/4th of the sample in December and June. In practice the month-to-month record linkage is closer to 70 percent (Nekarda, 2009). Thus, the precision of our coefficients is not equal across months, and in particular June sample sizes smallest and therefore the estimates are weakest.

lowest two terciles of predicted wages suffered the most from PPP loan delay, even after controlling for occupational exposure to the COVID-19 shock.[19] Controls for occupational exposure are important because essential workers are paid less than non-essential workers (McNicholas and Poydock (2020)) and because essential workers were less likely to work for firms that ceased operating from COVID-19 shock. Columns IV - VI show the employment effects by worker education. The effects of loan delay also hit less educated workers harder, again controlling for occupational exposure to the COVID-19 shock. These controls are again important since less educated workers are nearly 70 percent of the essential workforce (McNicholas and Poydock (2020)).[20]

This finding provides a counterpoint to our results on employment flows. While the employment flows point to longer-term scaring effects of the delay in PPP funding as entrepreneurs rebuild buffers stocks, these results show that the burden of displacement has fallen on workers for whom the long term costs of displacement are smallest.[21]

6 Robustness

6.1 Event Window

Figures 3 show that the window around the 17-26th of April helps minimize is confounding spatial variation in our data.[22] Figure 3 shows there is a high degree of variation in the timing of PPP loans within this window between adjacent counties such that there are very few clusters or other obvious geographic patterns. Other periods of PPP loan issuance suffer from

[19] We predict wages based on an augmented Mincer (1974) regression augmented to include industry, occupation, race/ethnicity, and sex fixed effects. We prefer terciles of imputed wages because these are available for all respondents.

[20] In supplemental regressions, not reported but available upon request, we find economically important but statistically insignificant evidence that ability to work from home and employment in an essential occupation insulated workers from the effects of PPP delay, at least in the short run.

[21] Doniger (2019) finds that more highly educated workers experience more persistent negative wage effects from displacement during a recession.

[22] Figure 4 shows that it also helps control for a number of loan, borrower, public policy, and firm characteristics that vary over the life of the program.

pronounced geographic clustering. Figure 6 shows that there is a strong spatial correlation to the share of loans made to a county from the second pool of PPP funding (Panel A) and the second half of an arbitrary window in late April and early May (Panel B). This is part of a general pattern (though not during our window), where obtaining loans earlier loans is correlated with urbanity and geographic region (along with firm size and sophistication) in likely spurious ways.

Table 10 contrasts our baseline unemployment results with the results of estimating them on the problematic alternative windows shown in Figures 3. Unlike the case of our instrument, these correlations present spurious pre-trends (particularly for the case of the late April and early May window) that likely reflect differences in the quality of loan applicants.

However, our results are robust to making the event window narrower or wider (subject to the usual unknown bias-variance trade-off of varying the window width). Table 11 contrasts our baseline unemployment results with the results of estimating them on these wider and narrower windows around the April 17 to 26. The baseline specification coefficients are economically similar and statistically indistinguishable from those estimated on the two alternatives windows. Unsurprisingly, the estimates of the jobs that would have been saved via a 10% increase in the initial allocation of PPP funding under the CARES Act are very similar across the three windows. Our preferred specification suggests jobs savings on the order of 3.5 million persisting through September. Alternative windows suggest nearly identical values. While the more broadly defined window suggests mildly lesser employment effects through the summer than our baseline the more narrowly defined window suggests employment effects more than 1.5 times as large.

6.2 Adjusting for firm failure

Our baseline results assume that loan demand is invariant to PPP loan delay. However, firms which missed the first tranche of PPP funding may have failed or found alternative financing on less favorable terms. If so, the the delay from the exhaustion of PPP funds would reduce PPP loan demand in the second round and bias our estimate of the share of loans delayed. Specifically, we measure:

$$\textit{Share Delayed} = \frac{L}{E+L} \tag{2}$$

where L is the volume of late window PPP loans on April 27-28, E is the volume of early window PPP loans on April 14-16, and $E+L$ is the volume of PPP loans in the April 14-28 window. However, allowing for failure and alternative financing, the share of loan demand that existed April 14-16 and was not met is

$$\textit{Unmet Demand} = \frac{L+M}{E+L+M} \tag{3}$$

where M is the missing loan demand from firms that either fail or find alternative financing by April 27 and consequently never seek a PPP loan. Next we make assumptions to adjust for this unobserved shifting PPP loan demand to recover the true effect of insufficient funding under CARES on employment and isolate the true effect of a 10-day delay in financing on survivors.

Assumption 3. *Only a fraction S of loan demand not met by April 16 eventually seeks a PPP loan.*

With this assumption we can write

$$L = (1-E)S \qquad \textit{and} \qquad M = (1-E)(1-S). \tag{4}$$

So we have:

$$\textit{Share Delayed} = \frac{(1-E)S}{E+(1-E)S}$$

and solving for $(1-E)$ gives

$$Unmet\ Demand = 1 - E = \frac{Share\ Delayed}{Share\ Delayed(1-S)+S}.$$

Now, the linear probability model

$$\begin{aligned}\mathbb{I}[not\ employed]_{i,c,t} =& \alpha + \\ & \sum_{m=0}^{8}[\beta_m \mathbb{I}[month = m, year = 2020]\{Unmet\ Demand_c\} + \\ & \mathbb{I}[month = m, year = 2020] \times controls_{i,c,t} \times \Gamma_m] + \\ & \sum_{\mu=0}^{12}[\mathbb{I}[month = \mu]controls_{i,c,t} X \Psi_\mu] + \\ & FE\ +\ \varepsilon_{i,c,t},\end{aligned} \tag{5}$$

recovers the effect of loan delay on the nonemployment rate, accounting for firm failure.

In simulations (not shown), we found that if enough firms fail as a result of the delay of PPP loans (and never get PPP loans or get them outside of our event window), this can meaningfully bias our estimates even if all other identification assumptions are met. However, empirically, not enough US firms have failed to make this a problem. Bartik et al. (2020a) estimate that 100,000 US firms failed during this period.[23] Bialik and Gole (2020) estimate essentially the same figure, with 97,966 permanently closed during this period. This might not even be unusually high The SBA estimates that about 45,000 small firms close monthly under normal conditions (Marks (2020)). But, even assuming these are 100 percent attributable to the COVID-19 shock, the effects on our estimates are very small. Circa 2017, the Census estimates that there are six million US firms (Bureau (2020b)). If all firm failures and permanent closures were caused by the PPP running out of funding, these figures imply

[23]The find 100 of their surveyed 4,969 firms is permanently closed, which is equivalent to about 100,000 of the 4.6 million small businesses they sample from.

that 1.7 percent of firms permanently closed as a result of the delay and never received PPP funding.[24] A two percent bias is well below the threshold at which our estimates are biased in any meaningful way. Further, using county bankruptcy data obtained from RAND State Statistics (2020), we test if county level loan delay is associated with changes in the fraction of businesses filings for bankruptcy in federal courts. We find no statistically or economically significant relationship between share delayed and increased incidence of any chapter of business bankruptcy.

7 Discussion

Here we provide additional context for the results in Section 5. First, we compare our employment effects of PPP loan delay to other papers studying the efficacy of the PPP using other metrics and identification strategies. Second, we compare our findings with estimates of the benefits of the long established Kurzarbeit job programs in Germany. From counter-cyclical jobs support programs in developed countries, the Kurzarbeit is most similar in design to the PPP. Third, we use our estimates of the effect of a ten day delay to roughly estimate of the aggregate job effects of the PPP program. Forth and finally, we use our aggregate PPP job savings to estimate the cost per job saved of PPP program and compare it with the stimulus literature.

[24] We estimate the bias in our coefficients resulting from unobserved firm failure. We find that bias becomes a serious problem (coefficients off by more than 10 percent) only when the probability of firm failure conditional on loan delay is about 50 percent. Even if 100 percent of the firm failures were caused by loan delay (that is all failed firms were PPP eligible and would have been in our event window without failure), and these failures were 100 percent concentrated in the smallest firms where we find the largest effects, this would only allow for a 13 percent failure rate. In our \$150k or smaller loans, there are 1.4 million in the event window with 46% late or 644k late loans. If 100k firms fail and thus have no loan demand, the conditional failure probability is 100 / (100 + 644) or 13%. In our simulations, this biases coefficients by less than 2 percent.

7.1 Comparison with Other Studies of the Labor Market Effects of the PPP

When studying the labor market consequences of the PPP, other authors have found a variety of effects and effect sizes, which vary considerably in their magnitudes and timing. On the lower end, Chetty et al. (2020) finds almost no employment effect for the program (albeit with large standard errors). Granja et al. (2020) finds no employment effects in April and very small effects in May and June, on the order of 0.8-1.3% of hours worked, which is roughly equivalent to one to two million full time jobs. On the higher end, Autor et al. (2020) find the PPP saved 1.4 million to 3.2 million jobs—using data through June—by which point 98% of PPP loans had been issued. To put these figures into context, PPP loan participants said that the loans would support about 51 million jobs in aggregate and SBA (2018) estimates that about 59 million workers work at at firms with 500 or fewer employees. These papers use different data and vary in their econometric specifications and controls, and this contributes to differences between their estimates and ours.

Granja et al. (2020), similarly to our paper, uses geographic variation in loan level timing to measure the effects of the PPP. They use the Homebase and Earning databases for their primary labor force analysis, which are opportunity samples of predominantly lower income workers who sign up using a cell phone app. The extent to which the geographic, education, race, and industry mixture of the workers in these databases matches the broader population is unknown. Selection into the use of these apps may have further unknown effects on the external validity of their results. In contrast, we use the CPS, is a large stratified random sample run by the Bureau of Labor Statistics and its methodology has been vetted over decades. Second, as we see in Figure 6, there is a strong geographic component to the timing of loan funding, with Southwest, upper Midwest, and urban East Coast all substantially more delayed than the country as a whole. Larger firms also were substantially less delayed. For example, 55 percent of PPP loans over $5 million were in the first funding round, but only 17 percent of loans under $150,000 were. Furthermore, as we showed in Section 3, the demographic, economic, educational, and geographic composition of the areas where PPP loans occur shifts considerable over the PPP sample. By comparing the counties by their exposure to the first and second PPP waves, Granja et al. (2020) assumes that the ZIP Codes they study are otherwise comparable conditional on their fixed effects specification. However, we show in Section 3 that the areas receiving early and late loans differ in several important areas influence the changes (and not just the levels, which would be absorbed by fixed effects), and these differences may instead be driving their effects. Table 10, column (2) shows estimates using our preferred CPS derived outcome variable with a first wave share instrument, similar to what is done in Granja et al. (2020). We obtain results similar to our baseline. However, in Table 10, column (3) we show that loans outside our narrowly defined event window co-vary with unobserved covariates and deliver spurious pre-trends. This illustrates why our event-window design is more robust.

Instead of looking at cross county differences, Chetty et al. (2020) and Autor et al. (2020) use a difference-in-difference estimation strategy to compare the abnormal employment effects in earning data of firms employing 100-500 employees (most of whom were PPP eligible), with firms employing 501-800 employees (generally PPP ineligible). Similar to Granja et al. (2020), Chetty et al. (2020) use an opportunity sample of lower income wage earners that makes it difficult to extrapolate to broader effects. Because larger firms secured PPP loans relatively early in the sample and the PPP likely saturated eligible demand (because it ultimately expired in August with excess funds), their estimates are contaminated by time-varying treatment in the two firm groups they study. Figure 4, Panel E shows that early on in the program the average loan size was for about $300,000 and saved 30 jobs. In our study window, this is stable at about $100,000 and 10 jobs per loan, and in the later part of the sample this falls to $30,000 and three jobs per loan. At no point was the sorts of larger loans (to firms with 100 employee or more) considered by Chetty et al. (2020) a "typical" loan, and indeed, there is evidence (along with the results in Figure 4) of big shifts in loan composition through the PPP.

Autor et al. (2020) measure the effects of the PPP using payroll data from ADP, a payroll processing firm, and an intent-to-treat approach, comparing PPP-eligible and PPP-ineligible firms using industry level size thresholds. This is also an opportunity sample, and not designed to be statistically representative. For example, Cajner et al. (2018) notes the ADP data are relatively skewed towards the North East and towards larger firms. According to Grigsby et al. (2019), only about half of firms use payroll processing services, and the ADP sample skews towards large (but not very large) firms, and pays hourly workers about 6% more than comparable workers in the CPS. We are particularly concerned about the use of ADP by very small firms. In a survey of firms with 20 or fewer employees, Surepayroll (2020) found only a third use payroll software (like ADP).

The most significant drawback the approach of Chetty et al. (2020) and Autor et al. (2020); however, is that the exogenous variation—ineligibility of firms with more than 500 employees—that each exploits does not allow them to measure how PPP loans might vary in job-creating effectiveness by firm size, particularly for the smaller firms that received most of the the loan dollars and indicated they would save most of the jobs. Our identification strategy and econometric specification allows us test if the employment effects on larger PPP eligible firms are representative of overall PPP loan effects on smaller firms.[25] Table 8 shows the effects on private sector employment by number of firm employees in May, where the large sample size of CPS workers observable pre- and post-PPP and for whom we can also identify firm size and thus allows the most precisely estimated comparison. The substantially larger coefficients for smaller firms show that the PPP had the biggest employment effects on the smallest firms, the very same ones for which Chetty et al. (2020) cannot estimate a causal effect. In addition, we report no effect–as expected–for firms larger than 500 employees.[26]

Like in our paper, Humphries et al. (2020) finds that early loans went to disproportionately larger firms and that early loan sizes were disproportionatly large. Their paper uses

[25] Because we do not observe loan receipt, our estimates constitute intent-to-treat effects.

[26] The CPS and the PPP define firm size differently. CPS records total employees across all establishments while PPP size restrictions applied at the establishment level in key industries. However, this discrepancy would bias us against finding stronger effects at smaller establishments and, thus, reinforces our findings.

an opportunity sample internet survey to study the effects of the PPP. Like in our administrative data, they also find an employment effect of PPP funding. Their results inform our thinking regarding the aggregate effects implied by our estimates. Our estimates show that small firms benefit the most from expediently delivered loans. Unfortunately, the roll out of the PPP favored expedient deliver of funds to larger firms. In our opinion, this substantively blunted the efficacy of the program.

7.2 Comparison with Kurzarbeit programs

By sharing the burden of keeping temporarily less-productive workers employed between employers and the state, the PPP has more in common with German Kurzarbeit programs than traditional US income support programs. Under the Kurzarbeit program, (literally German for "short work"), the German government compensates employers for most of the wage, pension, and insurance costs of keeping them employed during recessions. After the relative out-performance of the German labor market relative to peers in the global financial crisis, Contessi et al. (2013) and Felter (2012) argued that a Kurzarbeit-style program could be cost-effective tool for fighting unemployment in recessions. In a calibrated model of the labor market effects of fiscal policy in Europe, Faia et al. (2013) find that hiring subsidies (like Kurzarbeit and the PPP) of 0.5% of GDP, about the same size as the PPP loans in our windows, could reduce unemployment by 1.8 percentage points. The United States has some similar programs with limited availability in the great recession (17 states and about 1 percent of hours of unemployment claimed), called "short-time compensation" (STC) programs (Abraham and Houseman (2014)). Unlike the PPP, the Kurzarbeit program is open to firms of any size, provides funding for much longer than three months, and does not pay the full wage and benefits costs of workers (Felter (2012)). Nevertheless, the PPP does represent the US's first national and widely available attempt to implement a Kurzarbeit-style program, and the results appear to be very successful and in line with theoretical predictions.

7.3 Estimates of Aggregate Jobs Saved by the PPP

Where possible, the natural inclination in program evaluation is attempt to answer if the benefits exceeds the costs. A cost-benefit analysis of the PPP is difficult and probably misguided because, strictly speaking, jobs costs and not benefits when engaging in cost-benefit analysis. Those PPP jobs supported jobs are associated with benefits like increased productivity but we cannot observe those benefits, all we see clearly is the costs. [27] Still, creating and preserving jobs was the purpose of the PPP so instead we consider a cost-effectiveness analysis: how much did the PPP cost per job it saved.[28] To estimate the cost per job saved necessitates estimating the jobs saved by the program as a whole. This section lays out the (perhaps valiant) assumptions necessary to translate our results in Section 5 into an estimate of jobs saved by the PPP and then calculates that estimate. Section 7.4 translates this estimate of the number of jobs saved into an estimate of costs per job saved (our measure of cost effectiveness) and compares that with the cost-effectiveness of job saving programs from the US financial crisis.

First, calculation of jobs saves requires assuming that our estimate of the cost in jobs of loan delay is invariant across the entire horizon of the PPP program. For several reasons, this external validity assumption is questionable. For one, the observed and unobserved attributes of PPP loan recipients are not time invariant. For another, the economic environment, and in particular the availability and interest rates of alternative loans or grants varied over time. We partially address this by allowing for different parameters for small and large firms in our aggregation. While imperfect, this partly addresses both concerns since the size of PPP recipients fell dramatically over time and access to alternative funding sources likely covaries

[27] Human labor not used in one endeavor can be used in another or consumed as leisure. Therefore, in a typical cost-benefit analyses, jobs and associated labor costs are in the cost part of the analysis. As we understand it, the jobs saved by the PPP are proxies for benefits we actually care about but cannot measure—stable productive matches between employees that produce significant output that can then be consumed.

[28] Cost effectiveness analyses are a common alternative to cost-benefit analysis (CBA). It is common to use cost effectiveness analysis when a full CBA is impossible, such as when there are measures of policy effectiveness (here the number of jobs saved) but that these measures capture only a part of the social benefits of a policy (Boardman et al. (2001)).

with size. To implement this in our job saved figures, we construct our job saving estimates by binned firm size, taking seriously the coefficients on delay obtained by firm size reported in Table 8, and then aggregating over all the firm size bins.

Second, converting our point estimates, which measure jobs lost due to delay in funding, into estimates of jobs saved due to the *existence* PPP loans requires us to make an assumption about the amount of time required for a business to obtain funding from its next-best funding source relative to the time required to receive funding from the PPP. We know of no estimates of this time to alternative funding (alone or by firm size). Instead, we posit a difference in the timing of the receipt of funding from a PPP and the next best alternative of 15 days and assume that firms could have started either application on the opening date of the PPP program. Using these assumptions, we weight PPP loan amounts by hypothesized difference in the number of days between PPP loan receipt and the next best alternative. Using these weights, we cumulate delay weighted loans within firm size categories.[29]

This calculation suggests that the PPP program was equivalent to accelerating the delivery of funding to businesses with less than 10, 10 to 99, and 100-499 employees respectively by an equivalent of 8, 15 and 20 days respectively. Applying these factors to the coefficients on effect on nonemployment of a 10-day delay in funding implies that the PPP program preserved nearly 13 million jobs.[30][31] Of these 13 million nearly 9 million, or two thirds, were in firms with fewer than 10 employees. For contest, firms of this size account for roughly 20 percent of employment in normal times. Meanwhile, these assumptions suggest approximately

[29] Larger firms may need less time than smaller firms to line up alternative funding. Though larger firms have better average credit market access, they also need much larger loans, whereas smaller firms may be able to rely on faster retail credit products like home equity lines of credit and credit card loans. For simplicity, our primary estimate holds this delay to alternative funding sources fixed by firm size.

[30] When we instead assume that it takes 100-499 employee firms 5 days, 10-99 employee firms 10 days, and less than 10 employee firms 15 days to secure alternative funding. The results are similar (because the largest effects are in the smallest firms), with the program estimated to save 10 million instead of 12.7 million jobs.

[31] This calculation assumes that individuals who were nonemployed at the onset of the pandemic and subsequently found jobs as a result of the PPP program found them in proportion to historical levels of employment in firms of these sizes.

1.3 million were in firms with 100-499 employees, which is comparable to the estimates of Autor et al. (2020) for PPP job savings for firms of this size.

7.4 Costs per Job Saved by the PPP

Although the PPP was appropriated a total of $670 billion dollars that ($349 billion through the CARES act and $321 billion under the PPP act), not all funds were ultimately dispersed out. We calculate (because of binning) that the PPP funded $555 billion in loans. Because we estimate the PPP saved 13 million jobs (Section 7.3), this implies a cost per job saved of about $43,000. Our PPP job saving estimate is substantially larger than that of other papers and so our estimate of the cost per job saved is also much lower. Chetty et al. (2020) estimates a cost per job saved of $289,000 and Autor et al. (2020) implies a cost per job saved of $173,000 to $396,000 per job saved. However, our cost per job saved figure is very much in line with estimates of the job creation costs of the US stimulus programs of the financial crisis. Chodorow-Reich (2019) reviews seven studies of the American Recovery and Reinvestment Act (ARRA) and finds the ARRA programs save jobs at a cost of about $50,000 each, very much in line with our estimates. If anything, PPP loans might be expected to be even cheaper than these ARRA programs. The PPP is explicitly a job saving program and it seems reasonable that such a program should save jobs at a lower cost (of course, other programs have other benefits). Given the estimates of ARRA cost per job saved, it is the smaller employment results of other PPP papers that are surprising. ARRA estimates imply a typical government stimulus program of $555 billion should save about 11 million jobs. We estimate that the PPP saved 13 million jobs or about 15 percent more than expected from ARRA estimates, which seems reasonable, since again the PPP had a primary objective to save jobs.

However, while asking "what is the gross-cost per job saved of the PPP?" is a well posed question, it not the right question, even when we are unable to measure the benefits of jobs saved by the PPP. Rather, the right question is "what is the opportunity cost to the government of saving these jobs?" The CARES Act, like the Emergency Unemployment Compensation Act of 2008 (EUC 2008) passed during the US during the financial crisis, extended unemployment insurance of 13 weeks for a total in most states of 39 weeks. The federal government further enhanced the unemployment insurance program during the COVID recession (also in the CARES Act) in two important ways above and beyond the the changes in the EUC 2008. First, it expanded the set of workers who are eligible for unemployment insurance to include self-employed and contract workers. Second, it increased all weekly benefits by $600, which alone is worth as much as $10,000 to a worker collecting it for the maximum of 3/27/20 to 7/25/20. Combined, this was a substantial increase in the generosity of the unemployment insurance system that gave three-quarters of unemployment insurable eligible workers an income replacement rate above 100 percent (Ganong et al. (2020)).

To the extent that the alternative to PPP funding is workers at small firms collecting unemployment insurance, these costs should be subtracted from the cost of the PPP to estimate a more accurate net-cost per job saved that better captures the opportunity cost of the federal government of implementing the PPP. Within this institutional setting of greater than 100 percent unemployment insurance replacement rate, paying workers their full salaries may, even if they are completely unproductive for the spell of employment funded by the PPP loan, save the government money if the alternative is unemployment. In actuality, such workers are not completely unproductive and PPP-style program preserves quality matches both of which add to the benefits of the PPP.[32]

The quantitative effects of this are substantial. A typical job funded by a PPP loan, as measured by the PPP data, pays $43,000 per year. The 3 month wage bill (the PPP loan size) for this worker is $11,000. Such a worker is likely eligible for a 145 percent unemployment replacement rate for about three months and regular unemployment benefits

[32] This is true for any period where the unemployment insurance system is unusually comprehensive or generous. The gross-costs per job saved in ARRA estimates are probably also an overestimate of the true opportunity cost per job saved, because of these supplements to unemployment insurance, but the COVID-19 era changes were larger than those in the EUC 2008.

after that through the 39th week (Ganong et al. (2020)). If we assume that workers retained due to PPP loans are 50 percent as productive as during normal times (and their normal output is at least their compensation) and it otherwise takes the workers 6 months to find a new job with the same productivity as the job they would have kept with a PPP loan, we can estimate the net-opportunity cost of saving job. The $43,000 cost per job saved, less the $5,000 costs of six months of unemployment insurance (3 months at a replacement rate of 145 percent (Ganong et al. (2020)) and 3 months under normal generosity of 45 percent (Evermore (2019))) and $11,000 in additional output for the retained workers gives a opportunity cost per job saved of about $27,000.[33] Given that, like in Kurzarbeit programs, these funds preserve workers' idiosyncratic human capital and other valuable aspects of the employer-employee match, this is an attractive alternative to paying workers unemployment insurance, which effectively pays them to look for work instead of the PPP which pays them to do that work or at minimum remain productively matched in the face of transient shocks.

8 Conclusion

Our estimates indicate a very substantial effect of PPP loan delay with the majority of the effect being concentrated in the smallest firms, self-employed, lower-income, and less-educated workers. In particular, we conclude that a more targeted program that got more money to the smallest firms faster could have delivered most of the job saving of the PPP as a whole for a fraction of the price.

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[33]If job losses would be for longer or the PPP retained workers were more productive, the opportunity cost per job saved would be even lower. If workers would otherwise remain unemployed for nine months and the workers are fully productive when funded under the PPP, the social cost of the program per job saved is about $4,000, even without including any multiplier effects. By a year to find a new job and full productivity for PPP funded workers, the opportunity cost per job saved is negative (meaning the the program pays for itself).

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Table 1: Timeline of Major PPP Events

Period	Major Events
March 27th to April 6th	PPP Passed Into Law as part of the CARES ACT (3/27). Senator Rubio announces PPP expected to run out in late May and will require more appropriations.
April 7th to 13th	Congress expresses intent to appropriate $250 billion in additional PPP funding by end of week. SBA loan infrastructure cannot process volume, experiencing notable crashes. Banks limit loans primarily to firms with existing credit relationships due to availability of funding and program interest.
April 14th to 20th	Businesses still report significant delays in receiving PPP funds. Some banks stop taking new applications in anticipation of funding exhaustion. Heavy PPP issuance until program exhausts initial appropriation (4/16). Congressional negotiations over second PPP appropriations resume.
April 21th to 30th	Political discussions over second PPP appropriations continue. Bill in the house and senate, then signed into law (4/24). PPP resumes making loans and at high volumes (4/27). SBA encourages small businesses to apply for PPP funds as soon as possible.
May	Major slowdown in PPP loans. Congress explores changing PPP loan requirements to give more time and flexibility to spend funds. Senate adjourns for recess without extending term for PPP loans.
June	Initial end of PPP (6/30).
July	PPP terms adjusted in law to extend loan forgiveness timeline from eight to twenty four weeks. Reduces payroll requirements to allow 60% of the loan to go towards payroll, compared to the previous requirement of 75%. Extension of PPP loan deadline (8/8).
August	PPP loan program expires (8/8).

Sources: Various WSJ articles in 2020.

Table 2: Summary Statistics of PPP Loans by Headcount and Loan Size.

Loan Size	Loan (#)	Loan ($)	Jobs Retained	Window† Loans ($)	Window† Loans (#)	Late‡ Loans (%)	PPP Act§ Loans (%)
A) [$0, $150k]	4,224,172	141,804,112,463	19,669,493	42,537,113,452	1,124,214	56	27
B) ($150k-$350k]	379,062	75,812,379,056	8,727,023	24,420,472,101	122,102	44	32
C) ($350k-$1M]	199,456	134,632,999,456	10,015,580	42,439,337,873	62,873	40	32
D) ($1M-$2M]	53,030	79,545,053,030	5,906,794	24,027,016,018	16,018	39	30
E) ($2M-$5M]	24,838	86,933,024,838	5,167,844	25,935,007,410	7,410	38	30
F) ($5M-$10M]	4,840	36,300,004,840	1,639,326	11,797,501,573	1,573	41	32
All Loan Sizes	4,885,398	555,027,573,683	51,126,060	171,156,448,427	1,334,190	54	27

Headcount*	Loan (#)	Loan ($)	Jobs Retained*	Window† Loans ($)	Window† Loans (#)	Late‡ Loans (%)	PPP Act§ Loans (%)
1) [0, 9]	3,630,053	96,520,713,219	11,768,615	28,125,628,870	939,940	57	26
2) [10, 99]	1,172,183	286,449,242,433	29,412,071	90,865,561,688	370,358	46	32
3) [100, 499]	79,380	158,655,830,222	15,001,340	47,529,721,213	22,897	40	29
4) [500]	3,782	13,401,787,809	1,891,000	4,635,536,656	995	47	26
All Headcount	4,885,398	555,027,573,683	58,073,026	171,156,448,427	1,334,190	54	27

Source: SBA / Treasury (2020)

* - Actual headcount ("jobs retained" in the SBA / Treasury data) if $headcount > 0$ or $loansize/headcount < 40,000$, which is 81 percent of the observations (reasonable jobs saved sample). Where headcount is missing, reported as zero, or the average loan size per employee is well outside the PPP loan parameters, we impute the headcount with a linear model calibrated on the reasonable jobs saved sample using an OLS regression of the of *logjobsretained* on *logloansize* and dummy variables for major industries. Within this sample, the model predicts the reported employee size groups in Table 2 with about 90 percent accuracy.

† - Loans issued on or between 4/14/20 and 4/28/20.

‡ - The share of loans on or between 4/14/20 and 4/28/20 issued on 4/27/20 or 4/28/20.

§ - The share of loans issued on or after 4/17/20.

Table 3: Top 20 CBSA Loan Statistics.

	Loan Count	Loan Dollars	Avg. Loan Size	Jobs Retained	CBSA	Window Loans†	% Late Loans‡
1	154,319	13,047,214,281	84,547	1,167,888	Miami-Fort Lauderdale-West Palm Beach, FL	29,473	47.97
2	111,196	13,533,912,137	121,712	1,244,557	Dallas-Fort Worth-Arlington, TX	33678	47.63
3	352,739	44,520,958,165	126,215	3,294,144	New York-Newark-Jersey City, NY-NJ-PA	90,856	47.36
4	102,586	10,469,857,213	102,059	909,686	Atlanta-Sandy Springs-Roswell, GA	25,293	47.03
5	59,544	7,045,425,371	118,323	696,042	Phoenix-Mesa-Scottsdale, AZ	14937	46.35
6	240,666	28,327,940,293	117,706	2,560,080	Los Angeles-Long Beach-Anaheim, CA	53,210	45.70
7	56,706	8,184,737,556	144,336	705,578	Detroit-Warren-Dearborn, MI	19677	45.04
8	100,788	11,994,684,185	119,009	1,309,297	Houston-The Woodlands-Sugar Land, TX	28,862	44.75
9	52,223	6,255,607,650	119,786	515,238	Denver-Aurora-Lakewood, CO	14100	44.34
10	56,672	8,039,701,898	141,863	511,740	Seattle-Tacoma-Bellevue, WA	16590	44.19
11	51,472	6,265,959,580	121,735	640,596	San Diego-Carlsbad, CA	11918	43.74
12	42,432	5,460,393,911	128,685	479,621	St. Louis, MO-IL	14,587	43.63
13	57,036	8,543,943,197	149,799	715,974	Minneapolis-St. Paul-Bloomington, MN-WI	18,641	43.44
14	82,149	12,202,968,669	148,546	870,623	San Francisco-Oakland-Hayward, CA	21,579	43.39
15	38,765	5,012,347,193	129,300	373,960	Portland-Vancouver-Hillsboro, OR-WA	11,749	41.89
16	90,426	13,196,823,594	145,940	996,487	Washington-Arlington-Alexandria, DC-VA-MD-WV	28,201	41.45
17	152,617	19,967,777,000	130,835	1,709,888	Chicago-Naperville-Elgin, IL-IN-WI	43,360	38.74
18	84,633	11,919,347,729	140,835	825,082	Boston-Cambridge-Newton, MA-NH	33,181	37.56
19	86,792	11,964,920,975	137,857	862,339	Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	32,615	35.10
20	39,433	53,95,347,424	136,823	460,481	Baltimore-Columbia-Towson, MD	13,867	34.74

(Source: U.S. Treasury (2020))
† - Loans issued on or between 4/14/20 and 4/28/20.
‡ - The share of loans on or between 4/14/20 and 4/28/20 issued on 4/27/20 or 4/28/20.

Table 4: Comparison of Counties by Share of Delayed PPP Loans.

	Characteristics of Counties by Share of PPP Loans Delayed.					
	Below Median		Above Median		Above - Below	
	Mean	Std. Dev.	Mean	Std. Dev.	Difference	t-stat
Demographics						
Rural County	0.48	0.50	0.69	0.46	0.22	10.70
Fraction Black Residents	0.10	0.14	0.08	0.15	-0.01	-2.01
Fraction White Residents	0.75	0.19	0.75	0.21	0.01	0.75
Fraction Hispanic Residents	0.09	0.13	0.10	0.14	0.01	1.97
Republican 2016 Pres. Vote Share	0.65	0.16	0.68	0.17	0.03	4.81
Per Capita Income	26,888	6,923	25,851	6,248	-1,037	-3.75
Population (Non-institutional)	140,982	335,878	98,344	417,790	-42,638	-2.68
Log Per Capita Income	10.17	0.24	10.13	0.24	-0.04	-3.63
Log Population (Non-institutional)	10.53	1.63	9.78	1.54	-0.75	-11.30
Fraction Residents < H.S. Edu.	0.14	0.06	0.14	0.07	0.00	1.37
Fraction Residents H.S. or Some College	0.55	0.08	0.57	0.07	0.02	5.56
Fraction Residents College Deg. or More	0.31	0.11	0.29	0.09	-0.02	-4.89
Frac. Workers with Employer Insurance	0.31	0.07	0.30	0.06	-0.02	-5.89
Fraction Self-employed Workers	0.11	0.04	0.13	0.06	0.02	7.88
Fraction Private Workers	0.65	0.08	0.62	0.09	-0.03	-8.00
Fraction Public Sector Workers	0.16	0.06	0.18	0.07	0.01	5.21
Banking						
Branches per 100k Population	7.65	15.60	12.57	19.55	4.92	6.60
Deposits per 100k Population	304,949	696,544	464,737	634,684	159,788	5.69
County HHI (Branch Share)	2,667	2,171	3,188	2,307	521	5.52
County HHI (Deposit Share)	3,234	2,191	3,746	2,296	512	5.42
COVID-19						
Cases per 100k Population (4/11/20)	62	118	63	176	1	0.18
Deaths per 100k Population (4/11/20)	2	5	2	10	0	1.32
NPI: School Closure (4/11/20)	1.00	0.00	1.00	0.00	0.00	.
NPI: Any Serious (4/11/20)	1.00	0.00	1.00	0.00	0.00	.
Cases per 100k Population (9/11/20)	1,593	1,330	1,511	1,369	-82	-1.45
Deaths per 100k Population (9/11/20)	38	44	37	54	-1	-0.34
Business Bankruptcies						
Chapter 7 per 100k Population (12/31/19)	4.01	8.02	3.65	8.21	-0.36	-1.05
Chapter 11 per 100k Population (12/31/19)	1.19	8.59	0.92	4.19	-0.27	-0.94
Chapter 13 per 100k Population (12/31/19)	0.63	2.36	0.64	3.08	0.01	0.07
Chapter 7 per 100k Population (6/30/20)	3.40	6.29	3.55	8.35	0.15	0.48
Chapter 11 per 100k Population (6/30/20)	0.95	3.66	0.73	2.53	-0.22	-1.65
Chapter 13 per 100k Population (6/30/20)	0.62	3.63	0.60	3.03	-0.02	-0.14
Observations	1139		1138		2277	

Sources: Census Bureau (2020), Keystone Strategy (2020), JHU CSSE (2020)
RAND State Statistics (2020), MIT Election Data and Science Lab (2018) and SBA (2020).

Table 5: Employment Effects of Delay

	Panel A: (p.p, Unemployment per p.p Delay)		Panel B: (p.p, Nonemployment per p.p Delay)	
Share delayed × Month in 2020 ×				
January	-0.00912	-0.0108	-0.0138	-0.0150
	(0.0154)	(0.0154)	(0.0217)	(0.0215)
February	-0.0193	-0.0224	-0.0155	-0.0179
	(0.0191)	(0.0189)	(0.0251)	(0.0245)
March	0.00459	-0.00432	0.0227	0.0121
	(0.0231)	(0.0226)	(0.0286)	(0.0276)
April	-0.00208	-0.0399	0.0112	-0.0453
	(0.0541)	(0.0429)	(0.0604)	(0.0461)
May	0.116**	0.105***	0.157***	0.141***
	(0.0503)	(0.0387)	(0.0553)	(0.0421)
June	0.115**	0.0932**	0.123**	0.108***
	(0.0499)	(0.0372)	(0.0545)	(0.0405)
July	0.113***	0.0827**	0.115**	0.0827**
	(0.0439)	(0.0347)	(0.0481)	(0.0389)
August	0.109***	0.0842***	0.140***	0.106***
	(0.0379)	(0.0307)	(0.0428)	(0.0359)
September	0.126***	0.103***	0.134***	0.103***
	(0.0363)	(0.0305)	(0.0435)	(0.0361)
October	0.0799**	0.0662**	0.0850**	0.0541
	(0.0316)	(0.0273)	(0.0411)	(0.0348)
r2	0.665	0.675	0.639	0.649
N	792,197	792,197	831,737	831,737
Fixed Effects				
Individual	X	X	X	X
Month	X	X	X	X
Month-in-2020	X	X	X	X
Controls				
Industry and Occupation[a]		X		X
Cases and Deaths[b]		X		X
Non-pharmaceutical Interventions[c]		X		X
Unemployment Insurance[d]		X		X
Covid Induced Measurement Changes[e]		X		X

Note: Standard errors, clustered at the CBSA X Industry, in parentheses. * p<0.10, ** p<0.05, *** p<0.01.
Source: Current Population Survey, SBA data, and authors' calculations. Additional data noted below.

[a] Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020).

[b]: New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Data:JHU CSSE (2020).

[c]: Stringency Index: Oxford COVID-19 Government Response Tracker. Hale et al. (2020).

[d]: Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020).

[e]: Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

Table 6: Employment Flows (p.p, Flow per p.p Delay)..

	Panel A: Outflow from Employment				Panel B: Inflow to Employment			
	All	Separation	Exit	Layoff	All	Accession	Reentry	Recall
Share delayed ×								
January	-0.0154	-0.00307	-0.00910	-0.00447	0.108	0.201	0.0204	-0.781
	(0.0200)	(0.00957)	(0.0173)	(0.00603)	(0.165)	(0.258)	(0.235)	(0.625)
February	-0.0241	-0.00122	-0.0168	-0.00720	-0.262*	-0.265	-0.257	-0.235
	(0.0183)	(0.00782)	(0.0157)	(0.00623)	(0.157)	(0.236)	(0.251)	(0.498)
March	0.00719	-0.00217	-0.00177	0.0115	-0.0498	0.0114	-0.0937	-0.422
	(0.0219)	(0.00779)	(0.0183)	(0.00984)	(0.160)	(0.237)	(0.225)	(0.599)
April	-0.0290	-0.00751	0.0317	-0.0581*	0.0749	0.137	0.135	-0.618
	(0.0392)	(0.0129)	(0.0281)	(0.0325)	(0.134)	(0.217)	(0.188)	(0.451)
May	0.116***	0.00598	0.0479**	0.0753***	-0.263**	-0.220	-0.175	-0.500**
	(0.0273)	(0.00814)	(0.0201)	(0.0186)	(0.117)	(0.254)	(0.205)	(0.238)
June	-0.0229	-0.00644	0.00554	-0.0232	-0.194	-0.502*	0.179	-0.548**
	(0.0294)	(0.00994)	(0.0209)	(0.0195)	(0.134)	(0.276)	(0.206)	(0.249)
July	-0.0182	-0.000635	-0.0172	-0.00134	0.0549	0.175	0.219	-0.330
	(0.0284)	(0.00818)	(0.0211)	(0.0182)	(0.141)	(0.220)	(0.223)	(0.277)
August	-0.00546	0.00361	-0.00854	-0.0000866	-0.0934	0.0628	-0.604**	-0.351
	(0.0270)	(0.00915)	(0.0227)	(0.0122)	(0.133)	(0.219)	(0.266)	(0.273)
September	0.0176	-0.000408	0.000595	0.0194**	-0.0969	-0.125	0.497**	-0.740***
	(0.0244)	(0.0104)	(0.0208)	(0.00867)	(0.135)	(0.199)	(0.232)	(0.282)
October	-0.0166	-0.00819	-0.0169	0.00846	0.0386	-0.0742	-0.00285	-0.246
	(0.0194)	(0.00928)	(0.0164)	(0.00653)	(0.134)	(0.205)	(0.214)	(0.347)
R2	0.043	0.005	0.019	0.079	0.042	0.079	0.074	0.150
N	600,468	575,129	591,162	577,377	50,541	17,955	20,788	11,798

Note: Standard errors, clustered at the CBSA X Industry, in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Source: Current Population Survey, SBA data, and authors' calculations. Additional data noted below.

Controls: Individual, month, and month-in-2020 fixed effects. Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020). New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Source: JHU CSSE (2020). Stringency Index of non-pharmesutical interventions. Source: Oxford COVID-19 Government Response Tracker. Hale et al. (2020). Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

Table 7: Unemployment by Class of Worker (p.p, Nonemployment per p.p Delay).

Share delayed × ...	Self-employed × ...	Private Employee × ...	Public Employee × ...
January	0.00787	-0.0161	0.0107
	(0.0692)	(0.0222)	(0.0448)
February	0.0243	-0.0221	0.00788
	(0.0734)	(0.0259)	(0.0451)
March	0.0402	0.0133	-0.0322
	(0.0774)	(0.0291)	(0.0547)
April	0.0164	-0.0412	-0.0820
	(0.115)	(0.0474)	(0.0768)
May	0.233**	0.133***	0.00123
	(0.0968)	(0.0440)	(0.0766)
June	0.159	0.105**	-0.0320
	(0.104)	(0.0436)	(0.0704)
July	0.130	0.0738*	-0.0417
	(0.0934)	(0.0423)	(0.0796)
August	0.122	0.104***	-0.0378
	(0.0899)	(0.0385)	(0.0781)
September	0.210**	0.0925**	0.0998
	(0.0889)	(0.0390)	(0.0655)
October	0.0109	0.0606*	0.0694
	(0.0855)	(0.0367)	(0.0593)
Observations		915488	
R-squared		0.643	

Note: Standard errors, clustered at the individual X Industry, in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Source: Current Population Survey, SBA DATA, and authors' calculations. Additional datasets noted below.

Controls: Individual, month, and month-in-2020 fixed effects. Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020). New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Source: JHU CSSE (2020). Stringency Index of non-pharmesutical interventions. Source: Oxford COVID-19 Government Response Tracker. Hale et al. (2020). Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

Table 8: Nonemployment by Firm Size (p.p, Nonemployment per p.p Delay).

		Eligible[a]			Ineligible[a]	
	Unemployed	Number of Employees				
Share delayed ×		less than 10 ×	10 to 99 ×	100 to 499 ×	500 to 999 ×	1,000 or more
January	-0.611	0.0106	-0.0722	-0.0632	0.151	-0.0147
	(0.426)	(0.0997)	(0.0849)	(0.118)	(0.207)	(0.0656)
February	-0.383	0.0582	-0.0973	0.000185	0.145	-0.0375
	(0.367)	(0.0937)	(0.0784)	(0.112)	(0.204)	(0.0604)
March	-0.0989	0.0666	-0.0761	-0.0217	0.0100	0.0195
	(0.361)	(0.0896)	(0.0785)	(0.120)	(0.207)	(0.0577)
April	-0.261	0.0302	-0.139	-0.0584	0.173	0.0333
	(0.380)	(0.145)	(0.133)	(0.162)	(0.264)	(0.0931)
May	0.144	0.454***	0.0282	0.0468	0.113	0.0788
	(0.376)	(0.168)	(0.144)	(0.167)	(0.287)	(0.0975)
June	0.0552	0.218	-0.111	0.217	-0.0729	0.0705
	(0.476)	(0.225)	(0.190)	(0.224)	(0.377)	(0.119)
Observations				0.624		
R-squared				263,330		

Note: Standard errors, clustered at the CBSA X Industry, in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Source: Current Population Survey, SBA Data, and authors' calculations. Additional data sets noted below.

Controls: Individual, month, and month-in-2020 fixed effects. Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020). New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Source: JHU CSSE (2020). Stringency Index of non-pharmesutical interventions. Source: Oxford COVID-19 Government Response Tracker. Hale et al. (2020). Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

Table 9: Nonemployment by Worker Characteristics (p.p, Nonenemployment per p.p Delay).

	Tercile of Predicted Wages[a]			Educational Attainment		
Share delayed ×	I	II	III	Less than	High School	Some College
January	0.00980	-0.0168	-0.0402	0.135	-0.0210	-0.0344
	(0.0450)	(0.0340)	(0.0271)	(0.102)	(0.0296)	(0.0276)
February	-0.0275	-0.0103	-0.0162	0.0221	-0.0420	0.0122
	(0.0528)	(0.0403)	(0.0299)	(0.121)	(0.0340)	(0.0328)
March	0.0276	0.00817	-0.00203	0.127	0.0158	-0.0154
	(0.0611)	(0.0465)	(0.0317)	(0.124)	(0.0406)	(0.0330)
April	-0.154	0.0338	-0.0123	0.0709	-0.0983	0.00509
	(0.0939)	(0.0760)	(0.0508)	(0.175)	(0.0661)	(0.0511)
May	0.177**	0.204***	0.0407	0.318*	0.149**	0.0960*
	(0.0872)	(0.0646)	(0.0452)	(0.166)	(0.0598)	(0.0511)
June	0.123	0.156**	0.0474	0.313*	0.0938	0.0894*
	(0.0867)	(0.0623)	(0.0477)	(0.162)	(0.0598)	(0.0474)
July	0.126	0.122**	0.00379	0.430**	0.0702	0.0457
	(0.0872)	(0.0623)	(0.0400)	(0.173)	(0.0577)	(0.0446)
August	0.211***	0.0482	0.0517	0.255	0.102*	0.0943**
	(0.0777)	(0.0614)	(0.0393)	(0.159)	(0.0544)	(0.0455)
September	0.139*	0.0915*	0.0709*	0.0889	0.0995*	0.119***
	(0.0771)	(0.0553)	(0.0408)	(0.134)	(0.0518)	(0.0431)
October	0.0435	0.0506	0.0661	-0.00548	0.0527	0.0719
	(0.0734)	(0.0577)	(0.0411)	(0.142)	(0.0485)	(0.0449)
Observations		830,401			830,401	
R-squared		0.649			0.649	

Note: Standard errors, clustered at the CBSA X Industry, in parenthesise. * p<0.10, ** p<0.05, *** p<0.01.

Source: Current Population Survey, SBA DATA, and authors' calculations. Additional datasets noted below.

[a] Predicted wages conditional on education, potential experience and it's square, sex, race, ethnicity, broad industries occupations, and CBSA.

Controls: Individual, month, and month-in-2020 fixed effects. Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020). New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Source: JHU CSSE (2020). Stringency Index of non-pharmesutical interventions. Source: Oxford COVID-19 Government Response Tracker. Hale et al. (2020). Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

Table 10: Falsification Tests (p.p, Unemployment per p.p Delay).

	Baseline Specification	Second Round / Total Loans	May 2-3 / April 30-May 3
Share Delayed X			
January	-0.0150	-0.0101	-0.00754
	(0.0215)	(0.00996)	(0.00968)
February	-0.0179	-0.00926	-0.0183
	(0.0245)	(0.0111)	(0.0112)
March	0.0121	0.00127	-0.00233
	(0.0276)	(0.0128)	(0.0120)
April	-0.0453	0.0285	0.0644***
	(0.0461)	(0.0206)	(0.0211)
May	0.141***	0.118***	0.0606***
	(0.0421)	(0.0208)	(0.0203)
June	0.108***	0.0936***	0.0561***
	(0.0405)	(0.0176)	(0.0190)
July	0.0827**	0.0733***	0.0274
	(0.0389)	(0.0184)	(0.0169)
August	0.106***	0.0711***	0.0297*
	(0.0359)	(0.0167)	(0.0169)
September	0.103***	0.0972***	0.0532***
	(0.0361)	(0.0166)	(0.0168)
October	0.0541	0.0609***	0.0305*
	(0.0348)	(0.0164)	(0.0164)
Observations	0.649	0.649	0.649
R-squared	831,737	831,737	831,737

Note: Standard errors, clustered at the CBSA X Industry, in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Source: Current Population Survey, SBA DATA, and authors' calculations. Additional data sets noted below.

Controls: Individual, month, and month-in-2020 fixed effects. Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020). New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Source: JHU CSSE (2020). Stringency Index of non-pharmesutical interventions. Source: Oxford COVID-19 Government Response Tracker. Hale et al. (2020). Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

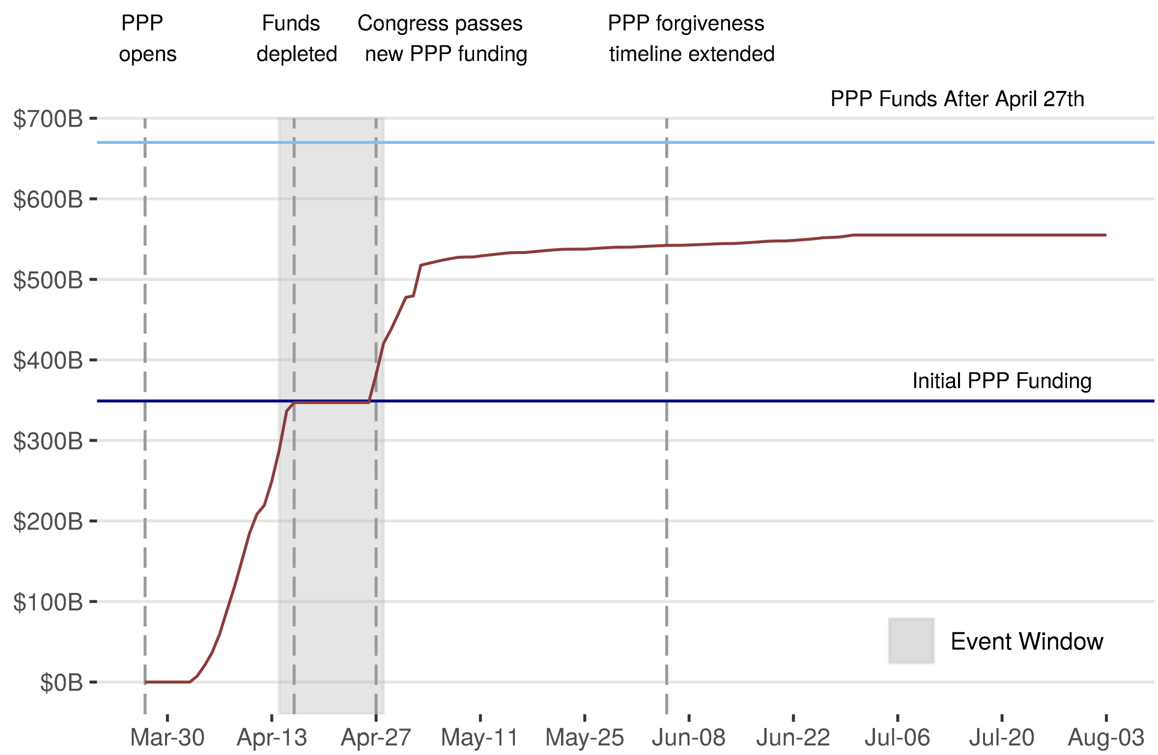
Table 11: Narrower and Wider Windows. (p.p, Nonemployment per p.p Delay).

	Baseline Specification	$\frac{27}{15\text{-}27}$	$\frac{27\text{-}30}{13\text{-}30}$
January	-0.0150	-0.0125	-0.0193
	(0.0215)	(0.0190)	(0.0214)
February	-0.0179	-0.00442	-0.0207
	(0.0245)	(0.0219)	(0.0245)
March	0.0121	0.0205	0.0167
	(0.0276)	(0.0247)	(0.0273)
April	-0.0453	-0.0549	-0.0148
	(0.0461)	(0.0394)	(0.0465)
May	0.141***	0.0764**	0.189***
	(0.0421)	(0.0358)	(0.0435)
June	0.108***	0.0696**	0.140***
	(0.0405)	(0.0345)	(0.0404)
July	0.0827**	0.0770**	0.101**
	(0.0389)	(0.0338)	(0.0395)
August	0.106***	0.0895***	0.121***
	(0.0359)	(0.0331)	(0.0356)
September	0.103***	0.0735**	0.119***
	(0.0361)	(0.0317)	(0.0366)
October	0.0541	0.0417	0.0689*
	(0.0348)	(0.0308)	(0.0355)
Observations	0.649	0.649	0.649
R-squared	831,737	831,737	831,737

Note: Standard errors, clustered at the CBSA X Industry, in parentheses.
* p<0.10, ** p<0.05, *** p<0.01.
Source: Current Population Survey, SBA DATA, and authors' calculations.
Additional data sets noted below.

Controls: Individual, month, and month-in-2020 fixed effects. Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020). New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Source: JHU CSSE (2020). Stringency Index of non-pharmesutical interventions. Source: Oxford COVID-19 Government Response Tracker. Hale et al. (2020). Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

Figure 1: Evolution of PPP Loan Issuance

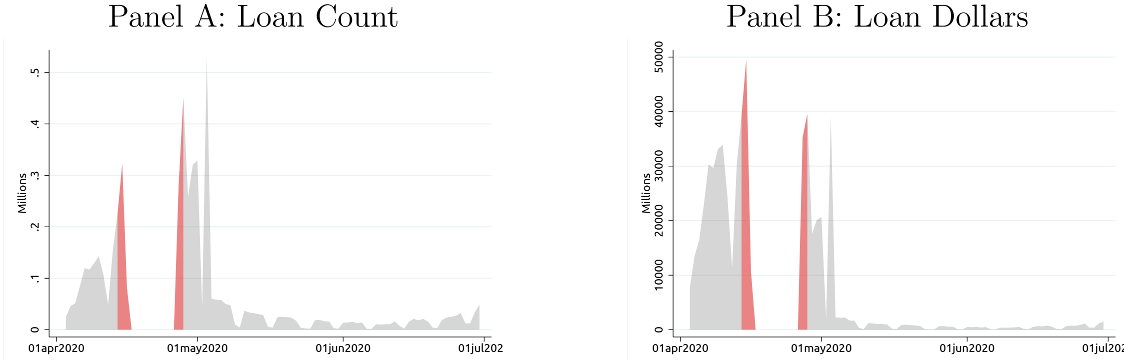


Source: U.S. Treasury (2020) and authors' Calculations

Figure 2: PPP Loan Timing.

Panel A: Loan Count

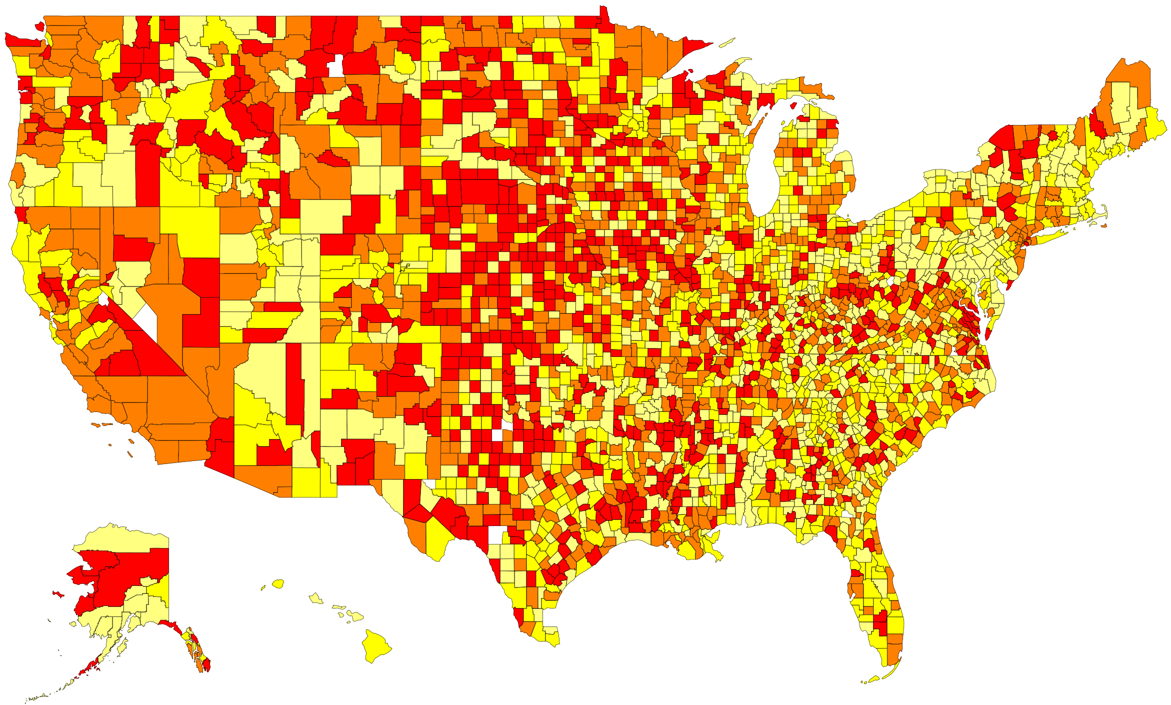
Panel B: Loan Dollars



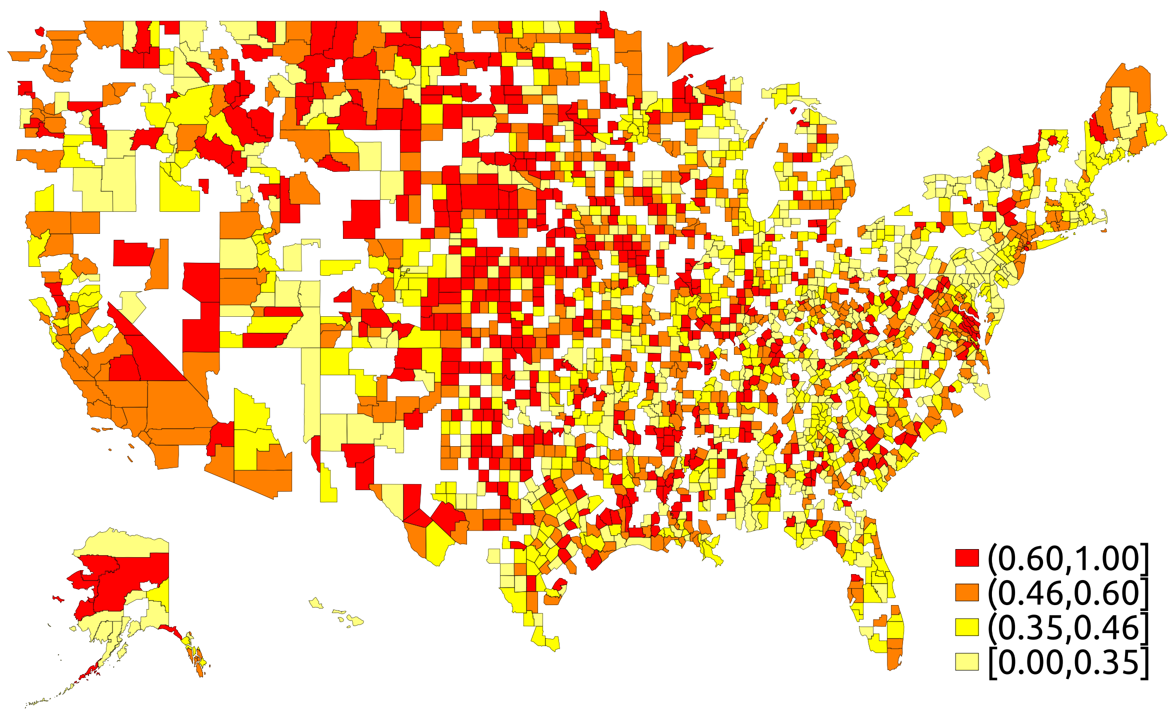
Source: U.S. Treasury (2020) and authors' Calculations

Figure 3: Geographic Distribution of Loan Timing.

Panel A: Window Share Late (All Counties)



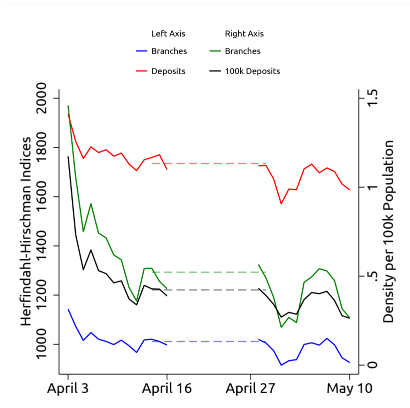
Panel B: Window Share Late (Observable in CPS)



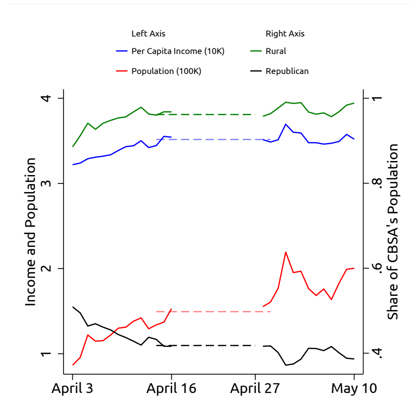
Source: U.S. Treasury (2020) and authors' Calculations

Figure 4: Time Variation in Characteristics of CBSA's Receiving Loans.

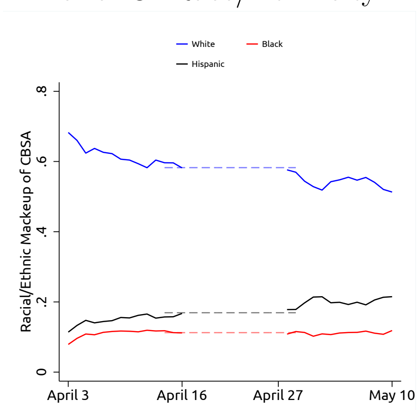
Panel A: Banking



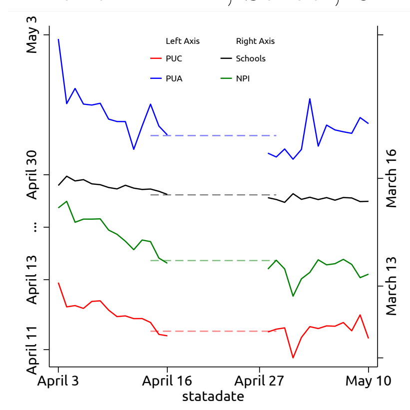
Panel B: Demographics



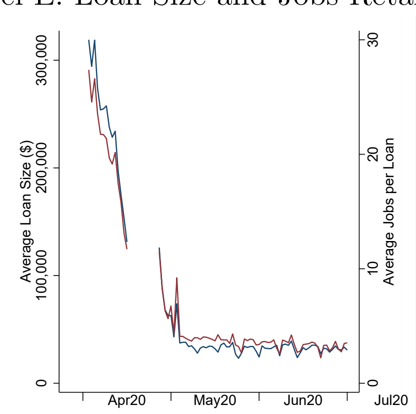
Panel C: Race/Ethnicity



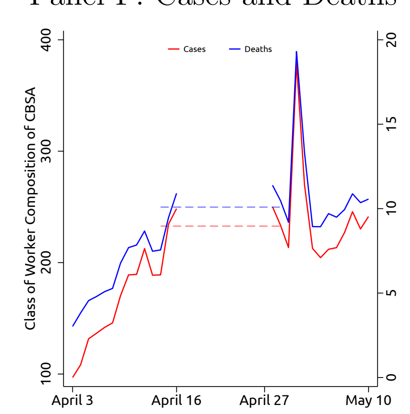
Panel D: NPI, School, UI



Panel E: Loan Size and Jobs Retained



Panel F: Cases and Deaths

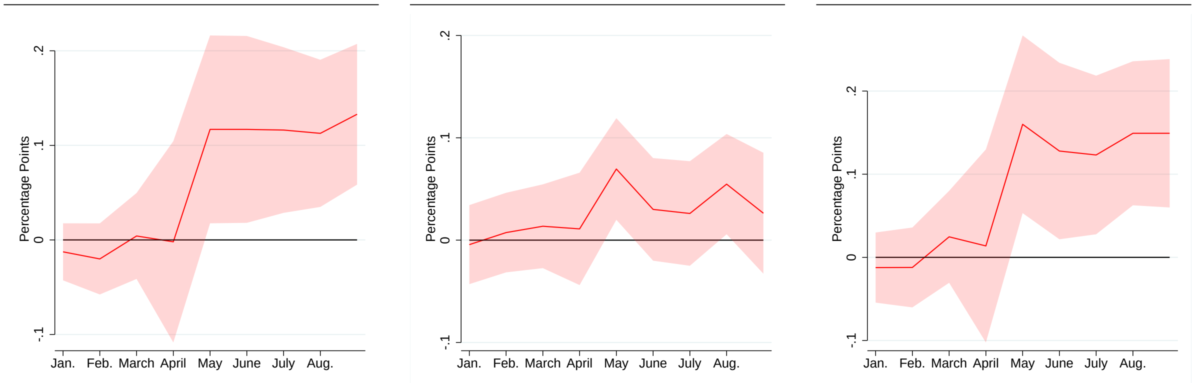


Source: U.S. Treasury (2020), FDIC Summary of Deposits (2019), Hale et al. (2020), Census Demographic Data (2018), JHU CSSE (2020) and authors' Calculations. Note: hashed lines plot the average of each series in the interval April 14-28.

Figure 5: Employment Effects (Coefficient Plots in p.p, Unemployment per p.p Delay).

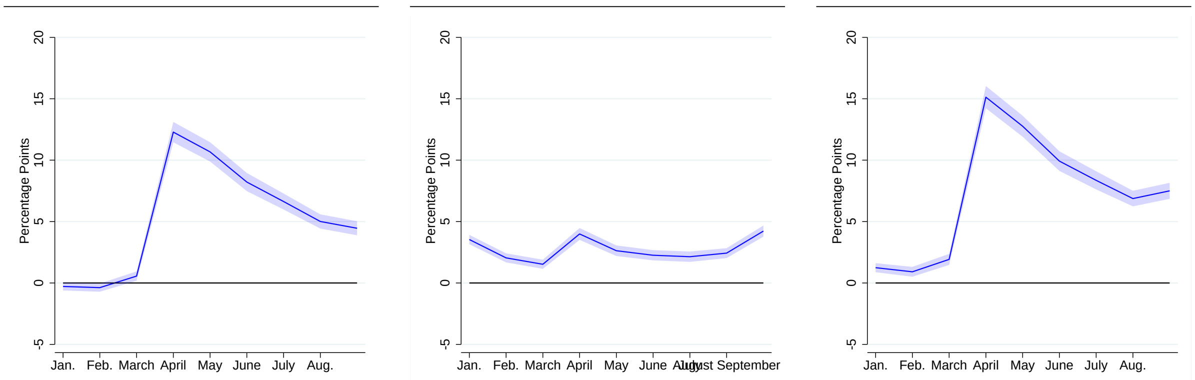
Panel A: marginal effect of delayed PPP financing

I: unemployment | II: labor force participation | III: nonemployment



Panel B: baseline COVID-19 shock[a]

I: unemployment | II: labor force participation | III: nonemployment



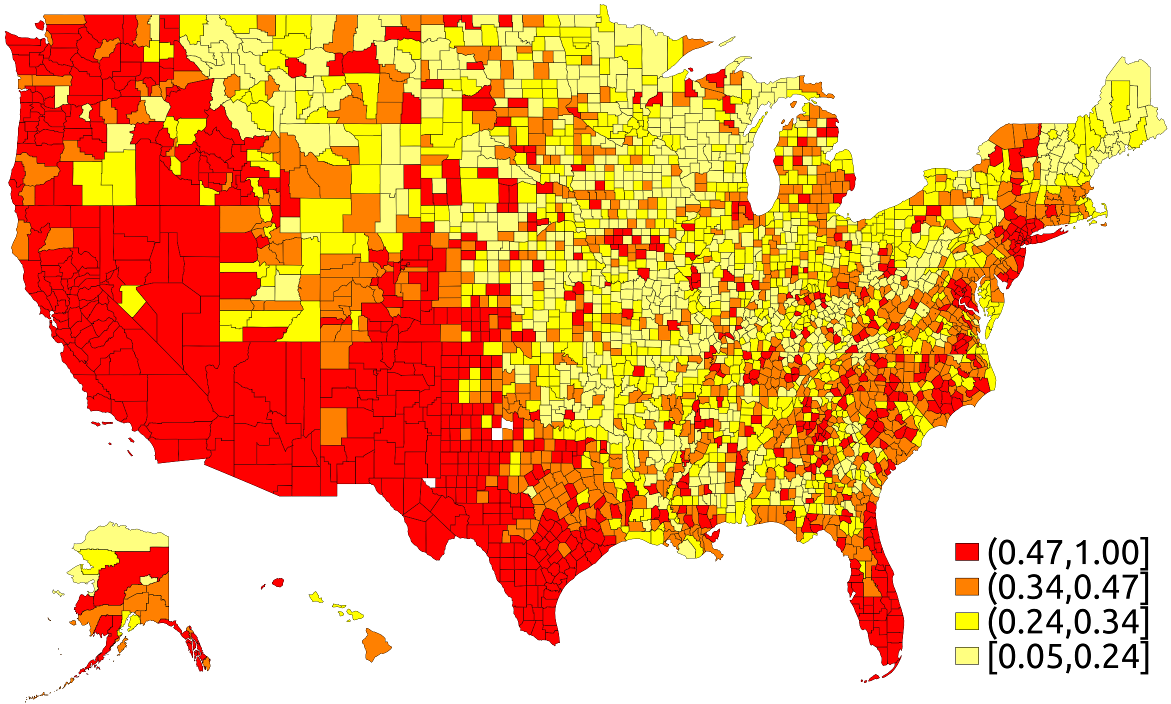
Note: The average potential worker lives in a CBSA where the share of delayed loans is 42%.
The standard deviation is 7.5%.

[a] *controls* contains month fixed effects.

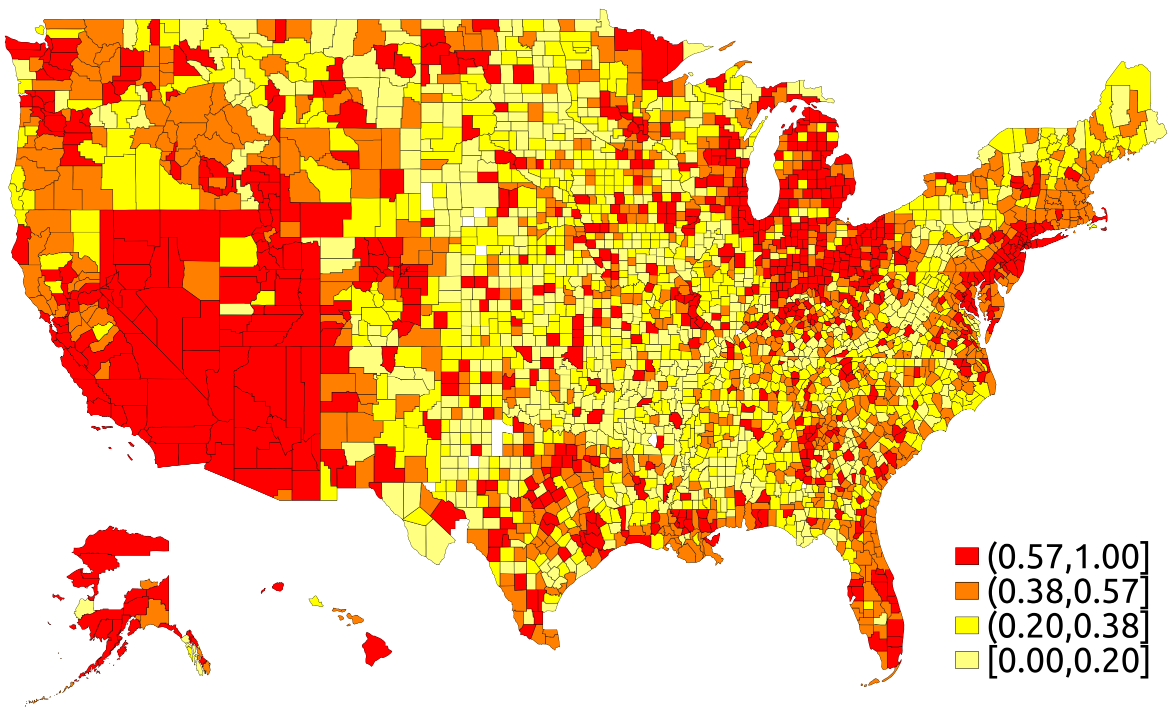
The baseline COVID-19 shock traces out the interaction between month fixed effects and year 2020.

Figure 6: Geographic Distribution of Loan Timing within other Windows.

Panel A: Relative Share of Second Round



Panel B: Relative Lateness within Second Round



Source: U.S. Treasury (2020) and authors' Calculations

A Additional Details about the Design and Implementation of the PPP

While not directly pertinent to the design of this study, we hope that these details will serve as a useful reference for other researchers.

The PPP program was run by the SBA in consultation with the US Department of the Treasury. Borrowers had to meet a multi-part test to be eligible for a PPP loan.[34][35] However, unlike virtually all other forms of credit, these were not tested for collateral or ability to repay and the paper work requirements were modest (estimated by the SBA at about two hours).[36] Once a borrower had their PPP application approved, their lender was supposed to send the funds to the borrower within 10 calendar days.[37][38] The qualifying loan amount per employee was 2.5 times the average total monthly per-employee payments for payroll costs for the year prior to the loan date (or, at the option of the borrower, for 2019) including up to $100,000 in annualized cash compensation (wages, salaries, and cash tips) plus group insurance premiums (including health care benefits).[39] The qualifying loan amount is the total over all employees for covered firms, up to a limit of $10 million.[40]

[34] First, borrowers had to employ 500 or fewer employees in most industries. Second, firms are subject to net worth and income limitations. Firms had to have tangible net worth of less than $15 million and an average net income after federal taxes of less than $5 million. Third, borrowers had to independently owned and operated and located within the United States. Fourth, firms have to be engaged in a legal activity and not be a household employer nor a hedge or private equity fund. Fifth, they could not be delinquent or in default on an existing SBA loan, nor could they be bankrupt.

[35] The SBA publishes North American Industry Classification System Codes (NAICS codes) based definitions for small businesses based on their headcounts or annual revenues (SBA (2020)). For about 310 NAICS code these limits are larger 500 employees and in these cases, the PPP is available for firms with more than 500 employees. However, there are no instances in the PPP data of any firms getting loans to support more than 500 employees, suggesting that perhaps firms meeting the less restrictive headcount requirements in these industries were unable to satisfy the other requirements or got multi-establishment loans like those available to hospitality firms.

[36] Despite the lack of traditional loan underwriting, Bartik et al. (2020b) find that 12-25 percent of small businesses had their PPP denied or wanted to apply for PPP loans either did not qualify or were told that they did not.

[37] This changed somewhat over the program. Initial program documentation said that lenders had 10 days to make their distributions, but was unclear on what this actually required and when the clock started ticking. Eventually the Treasury and the SBA issued a rule that for loans approved on or before April 28, 2020, lenders had 10 calendar days from April 28, 2020 to fund the loan. For loans after this, the loan had to fund in 10 calendar days.

[38] To our knowledge, there are no published statistics on how long it took banks to pay out PPP loans once approved. Though payment from approval to distribution is not recorded, Select Subcommittee on the Coronavirus Crisis (2020) reports time for application to distribution at the largest banks. These varied enormously, as fast as 3 days at JP Morgan for their largest customers to 35 days for Truist for its smallest customers.

[39] Therefore, a PPP loan for an employee with no benefits and an annual salary of $48,000 would be $10,000 ($\$48,000 \times \frac{2.5}{12}$), but for an no-benefits employee making $100,000 or more, the loan would be for $20,834.

[40] The maximum loan amount from compensation expenses for employees was $46,154 per employee plus health and retirement benefits. For sole proprietors, independent contractor, or the self-employed, the maximum of compensation component of the loan was $20,833 and health care and retirement benefits are not covered. That said, most PPP loans were much smaller: the average loan was for $9,600 per employee and the average loan size was for $114,000, while the median loan was only $25,000.

PPP loans were forgivable if they were used for qualified expenses during a specified period. For the loans we focus on, borrowers were told they had 8 weeks after fund distribution to spend the proceeds on qualified expenses and at least 75% of funds had to be spent on qualified payroll expenses.[41] And of the proceeds on these PPP loans that did not qualify for forgiveness were due in two years at the cost of one percent per year interest.

[41] The vast majority (92%) of PPP loans are made before June 5, 2020. Before that, the US Treasury and the SBA guidance was firms had 8 weeks to spend their funds to qualify for forgiveness and 75% of expenses had to be for payroll. On June 5, 2020, the Paycheck Protection Program Flexibility Act of 2020 passed and the US Treasury and the SBA rules under that act that relaxed the requirement to allow 24 weeks to spend the PPP funds and 60% of expenses had to be for payroll to qualify for loan forgiveness.)

B Data Appendix

B.1 Outbreak Severity

Because local economic conditions and the resulting employment effects of the PPP are affected by the severity of the local Covid-19 outbreak and the non-pharmaceutical interventions (NPIs) used to fight it, we use Covid-19 cases, deaths, and NPIs as controls in some of our specifications. The Covid-19 confirmed cases and deaths are county level data from JHU CSSE (2020), which are the most prominent and frequently used source of case and death data. We use only their US data, which are cleaned and organized data provided by the US Centers for Disease Control and Prevention (CDC).

B.2 Non-pharmaceutical Interventions

NPI data are from the Oxford COVID-19 Government Response Tracker Hale et al. (2020). Their repository contains NPIs at the State level for all 50 states and the District of Columbia. We assess the county NPIs during the week that contains the 12th of the month to align with the monthly timing of our labor market data.[42]

B.3 Occupational Exposure to the Covid-19 Shock

We rank occupations according to their exposure to the Covid-19 shock on three dimensions, need for proximity to others, essentialness, and ability to work from home. To do so we follow the metrics generated by Leibovici et al. (2020) and Jackson (2020).

B.4 Social Safety Net

The CARES Act and subsequent executive action significantly increased eligibility for and assistance from the social safety net, in particular unemployment insurance. The CARES Act mandated an expansion of unemployment insurance eligibility–Pandemic Unemployment Assistance (PUA)–and an additional $600 per week, through the end of July, in unemployment insurance for those whose claims were approved–Pandemic Unemployment Compensation (PUC). States took variable amounts of time to implement the new policies resulting in one to two month differences in the start date of payments under the two programs (Nunn et al. (2020)). After these PUC lapsed, President Trump signed an executive order mandating $300 to $400 extra per week in unemployment insurance under a program called "Lost Wages Assistance" (LWA). These funds were supported by grants from the Federal Emergency Management Agency (FEMA). Due to variable time to approval for FEMA funds states began paying LWA and exhausted Federal funding or the program at different times (Singh (2020)).

[42] As noted in section 3.2 the Current Population Survey queries about labor market activities during the week of the month that contains the 12^{th}.

B.5 Additional details about the Current Population Survey

In the fourth and the eighth monthly surveys, respondents report their weekly earnings if they are public or private employees. In addition, each March respondents also report their wage and salary income in the past year regardless of their current employment status or class of worker. Thus, for each respondent, we can calculate one to two proxies for the unemployment insurance that she would receive were she to claim and be eligible for unemployment insurance, given state level unemployment insurance laws, state level variation in the PUC, PUA, and LWA, and her past reported labor income.of Labor (2020) In our baseline specification we control for the state-level timing of PUC, PUA, and LWA participation interacted with class of worker (self-employed, private, public). Results are robust to controlling for unemployment insurance replacement rates under these programs inferred from the Earner Study or March Supplement income data and to replacement rates estimated using imputed earnings based on either of these sources of earnings data.

The COVID-19 outbreak complicated the collection of CPS data in 2020, as it complicated virtually all other government, business, and household activities. The CPS is typically collected via a mixture of telephone and in-person interviews with in-person interviews primarily being used for the first and fifth wave of data collection. In March 2020, CPS suspended in-person interviews to protect its employees. In person interviews resumed in some parts of the country in July. These changes in operating protocol resulted in large increases in non-response and there is evidence that non-response is non-random. There is some evidence that the COVID-19 pandemic and related policy interventions had a non-neutral impact on the CPS' representation of demographic subgroups (Ward and Edwards, 2020). To ameliorate the effects of these survey challenges, we control for interview type in all of our specifications.

C Online Appendix Tables

Table 12: Unemployment Effects.

Share Delayed X 2020 X										
January	-0.0126	-0.0135	-0.0142	-0.0125	-0.0126	-0.0126	-0.0126	-0.0123	0.0139	-0.0140
	(0.0154)	(0.0153)	(0.0153)	(0.0154)	(0.0154)	(0.0154)	(0.0154)	(0.0155)	(0.0137)	(0.0154)
February	-0.0201	-0.0233	-0.0231	-0.0200	-0.0201	-0.0202	-0.0202	-0.0187	0.00631	-0.0231
	(0.0192)	(0.0189)	(0.0189)	(0.0192)	(0.0192)	(0.0192)	(0.0192)	(0.0193)	(0.0169)	(0.0190)
March	0.00418	-0.000179	-0.000518	0.00459	0.00358	0.00453	0.00420	0.00415	0.0318	-0.000854
	(0.0232)	(0.0225)	(0.0225)	(0.0232)	(0.0232)	(0.0232)	(0.0232)	(0.0232)	(0.0222)	(0.0225)
April	-0.00199	-0.0130	-0.0127	-0.0191	-0.00208	0.000293	0.000248	-0.00121	-0.0341	-0.0381
	(0.0543)	(0.0434)	(0.0433)	(0.0504)	(0.0543)	(0.0542)	(0.0542)	(0.0542)	(0.0508)	(0.0430)
May	0.117**	0.106***	0.109***	0.0995**	0.115**	0.119**	0.118**	0.118**	0.101**	0.0998**
	(0.0507)	(0.0376)	(0.0379)	(0.0473)	(0.0507)	(0.0505)	(0.0506)	(0.0506)	(0.0475)	(0.0389)
June	0.117**	0.103***	0.103***	0.113**	0.112**	0.119**	0.116**	0.118**	0.118**	0.0935**
	(0.0504)	(0.0393)	(0.0394)	(0.0458)	(0.0506)	(0.0502)	(0.0506)	(0.0505)	(0.0484)	(0.0378)
July	0.116***	0.100***	0.0997***	0.113***	0.112**	0.118***	0.116***	0.117***	0.117***	0.0925**
	(0.0447)	(0.0362)	(0.0362)	(0.0432)	(0.0449)	(0.0445)	(0.0449)	(0.0446)	(0.0433)	(0.0360)
August	0.113***	0.0971***	0.0976***	0.109***	0.108***	0.115***	0.112***	0.114***	0.0990**	0.0926***
	(0.0397)	(0.0326)	(0.0325)	(0.0400)	(0.0400)	(0.0396)	(0.0401)	(0.0397)	(0.0413)	(0.0334)
September	0.133***	0.121***	0.121***	0.127***	0.128***	0.135***	0.132***	0.133***	0.105***	0.113***
	(0.0380)	(0.0325)	(0.0325)	(0.0379)	(0.0385)	(0.0378)	(0.0385)	(0.0380)	(0.0373)	(0.0335)
label Observations	756465	756465	756465	756465	756465	756465	756465	756465	500408	756465
R-squared	0.667	0.676	0.675	0.667	0.667	0.667	0.667	0.667	0.469	0.677
Fixed Effects										
Individual	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month-in-2020	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls										
Industry and Occupation		A	B							B
Cases and Deaths				C						C
NPIs					D	E	D,E			D,E
Unemployment Insurance								F		F
Measurement Changes									G	G

Standard errors, clustered at the CBSA X Industry, in parentheseise. * p<0.10, ** p<0.05, *** p<0.01.

A: Fixed effects for 2-Digit NAICS and 22 Broad Industries fully interacted with Month-in-2020 fixed effects. B: Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020).. C: New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Data: JHU CSSE (2020). D: Percent of counties in the CBSA with mandated business closures of any kind or strict social distancing in the reference week. Data: Keystone Strategy (2020). E: Percent of counties in the CBSA with school closures fully interacted with the number of children in the household. Data: Keystone Strategy (2020). F: Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). G: Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

Table 13: Nonemployment Effects.

Share Delayed X 2020 X										
January	-0.0172	-0.0194	-0.0198	-0.0170	-0.0172	-0.0126	-0.0126	-0.0154	0.00726	-0.0178
	(0.0218)	(0.0217)	(0.0218)	(0.0218)	(0.0218)	(0.0154)	(0.0154)	(0.0216)	(0.0192)	(0.0216)
February	-0.0160	-0.0191	-0.0194	-0.0157	-0.0160	-0.0202	-0.0202	-0.0143	-0.00963	-0.0183
	(0.0251)	(0.0246)	(0.0246)	(0.0251)	(0.0251)	(0.0192)	(0.0192)	(0.0251)	(0.0210)	(0.0245)
March	0.0229	0.0180	0.0170	0.0235	0.0220	0.00453	0.00420	0.0236	0.0609**	0.0182
	(0.0287)	(0.0280)	(0.0278)	(0.0287)	(0.0287)	(0.0232)	(0.0232)	(0.0287)	(0.0277)	(0.0278)
April	0.0122	0.00313	0.00000118	-0.0214	0.0121	0.000293	0.000248	0.0126	-0.0175	-0.0419
	(0.0606)	(0.0480)	(0.0481)	(0.0548)	(0.0606)	(0.0542)	(0.0542)	(0.0605)	(0.0571)	(0.0461)
May	0.159***	0.149***	0.150***	0.135***	0.156***	0.119**	0.118**	0.160***	0.127**	0.135***
	(0.0558)	(0.0414)	(0.0415)	(0.0517)	(0.0559)	(0.0505)	(0.0506)	(0.0557)	(0.0532)	(0.0422)
June	0.127**	0.115***	0.115***	0.127**	0.121**	0.119**	0.116**	0.128**	0.133**	0.109***
	(0.0554)	(0.0429)	(0.0431)	(0.0502)	(0.0556)	(0.0502)	(0.0506)	(0.0553)	(0.0522)	(0.0412)
July	0.123**	0.108***	0.108***	0.119**	0.117**	0.118***	0.116***	0.122**	0.104**	0.0992**
	(0.0498)	(0.0409)	(0.0410)	(0.0482)	(0.0500)	(0.0445)	(0.0449)	(0.0499)	(0.0496)	(0.0407)
August	0.149***	0.134***	0.135***	0.141***	0.143***	0.115***	0.112***	0.146***	0.143***	0.122***
	(0.0452)	(0.0377)	(0.0375)	(0.0456)	(0.0455)	(0.0396)	(0.0401)	(0.0453)	(0.0466)	(0.0387)
September	0.149***	0.132***	0.135***	0.140***	0.143***	0.135***	0.132***	0.147***	0.136***	0.124***
	(0.0466)	(0.0384)	(0.0384)	(0.0466)	(0.0472)	(0.0378)	(0.0385)	(0.0465)	(0.0429)	(0.0401)
label Observations	794173	794173	794173	794173	794173	756465	756465	794173	508918	794173
R-squared	0.640	0.647	0.646	0.641	0.640	0.667	0.667	0.644	0.478	0.651
Fixed Effects										
Individual	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month-in-2020	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls										
Industry and Occupation		A	B							B
Cases and Deaths				C						C
NPIs					D	E	D,E			D,E
Unemployment Insurance								F		F
Measurement Changes									G	G

Standard errors, clustered at the CBSA X Industry, in parentheseise. * p<0.10, ** p<0.05, *** p<0.01.

A: Fixed effects for 2-Digit NAICS and 22 Broad Industries fully interacted with Month-in-2020 fixed effects. B: Fixed effects for 2-Digit NAICS and controls for occupational exposure to the Covid shock fully interacted with month-in-2020 fixed effects. Occupational exposure is measured by ability to work from home, required proximity to others, and essentialness. Coding follows Leibovici et al. (2020) and Jackson (2020). C: New cases and new deaths in the CBSA. New York City is an outlier, as a result we allow for the coefficient on cases and deaths to differ for this CBSA. Data: JHU CSSE (2020). D: Percent of counties in the CBSA with mandated business closures of any kind or strict social distancing in the reference week. Data: Keystone Strategy (2020). E: Percent of counties in the CBSA with school closures fully interacted with the number of children in the household. Data: Keystone Strategy (2020). F: Indicator for whether PUC, PUA, and/or LWA were made in the respondent's state in the reference week interacted with class of worker. Data: Nunn et al. (2020) and Singh (2020). G: Fixed effects for month in which the respondent was first interviewed by the CPS and for interview type fully interacted with month-in-2020 fixed effects.

D Detailed Timeline of the PPP in the Popular Press

Detailed Timeline of PPP Events in Popular Press

	Government Action:		Reporting:		
Week Of	Action	Date	Topic	Summary	Cite
March 30th to April 6th	PPP Passed Into Law	3/27/2020	PPP Opening	Coronavirus relief bill appropriates $350 Billion for PPP. Loan applications open Friday, April 3rd.	[5]
	Private Directives from Treasury	3/28/2020	Loan Allocation	Treasury Department privately encourages lenders to prioritize their existing customer base over new applicants in call with American Bankers Association.	[36]
	Sec. Mnuchin Press Conference Error	4/2/2020	Program Administration	Treasury Secretary Mnuchin gives wrong address for SBA application portal in a Coronavirus Task Force briefing, instructing applicants to go to SBA.com instead of SBA.gov, the application portal.	[21]
			Loan Applications	Applications handled through SBA approved lenders. Lenders were not ready to take loan applications. Treasury was late to issue program rules.	[42]
			Loan Requirements	Loans made for 8 weeks at 1% interest to help businesses make payroll and rent. The expectation of loan forgiveness was widely reported.	[24]
			Loan Allocation	Banks often prioritize larger businesses with established banking relationships.	[41]
	Congress Announces Expectations about Funding.	4/4/2020	Funding Expectations	Senator Marco Rubio tweets funding for PPP expected to run out in late May and will require more appropriations.	[38]
April 6th to 13th	Fed and Treasury Announce Program to Back PPP Loans	4/6/2020	PPP Financing	The Federal Reserve Board, in cooperation with Treasury, announces a new 13(3) facility to offer term financing backed by PPP loans.	[6]
			Loan Recipients	Large restaurants, franchises, and hotel chains, notably the New York based Shake Shack, gain exemption from employment limits and announce their intent to seek funding. There is a growing concern over these businesses competing with smaller firms for funds.	[7]
	Congress Announces Intentions about Funding.	4/7/2020	Funding Expectations	Congress expresses intent to appropriate $250 Billion in additional PPP funding by end of week. Congress also expresses the intent to approve new lenders.	[40]

Tabular Summary of PPP Timeline *(continued)*

	Government Action:		Reporting:		
Week Of	Action	Date	Topic	Summary	Cite
			Loan Application	Processing applications costly for banks and borrowers. Banks take steps to limit applications to firms with existing accounts. Banks frequently change application rules responding to Treasury guidance, public criticism, and program capacity. Borrowers uncertain about application rules and loan availability. SBA loan infrastruture cannot process volume, experiencing notable crashes.	[52]
			Loan Allocation	Banks limit loans primarily to firms with existing credit relationships due to availability of funding and program interest. Unbanked and smaller firms at a significant disadvantage when seeking loans, as banks have a large incentive to only make loans to firms with existing credit lines whose ability to repay is better known.	[52]
			Loan Receipt	Small businesses report significant delays receiving funds. Firms attempt to substitute towards other lending out of desperation for funds.	[11], [16]
			Loan Availability	Wells Fargo reports reaching its capacity and stops making PPP loans. However, the bank still accepts applications expecting fund replenishment.	[26], [48]
April 13th to 20th			Loan Receipt	Businesses still report significant delays in receiving PPP funds.	[43], [47]
	Cease and Desist Issued to SBA.com	4/14/2020	Program Administration	Cease and Desist issued to SBA.com by New York Attorney General Letitia James.	[21]
	Congressional Negotiations Resume	4/15/2020	Funding Expectations	With the expected depletion of program funds, Congressional negotiations over PPP appropriations resume. Dem. Caucus wants appropriations for hospitals and state and local governments, with the GOP against.	[50]
	PPP Funding Officially Depleted	4/16/2020	Loan Availability	Some banks stop taking new applications in anticipation of funding depletion with as many as 700,000 businesses awaiting funding. Others continue receiving applications but stop making new loans.	[37] [39]
			Loan Availability	National Federation of Independent Businesses (NFIB) reports only 4% of surveyed members have been approved for a loan, while none of those approved have yet to receive funding.	[26]
			Loan Availability	SBA excludes tiered franchises from receiving loans.	[4]

Tabular Summary of PPP Timeline *(continued)*

	Government Action:		Reporting:		
Week Of	Action	Date	Topic	Summary	Cite
			Program Administration	There is a growing concern that banks will "authorization hoard," collecting applications that they do not intend to fulfill which they can chose among when issued more funds.	[48]
	Pres. Trump Signals Support for Hospital Funding	4/18/2020	Funding Expectations	President Trump signals support for appropriating funds for hospitals, with the GOP Caucus still unsupportive.	[49], [19]
April 20th to 27th	Democrats Offer More Appropriations for Swing States	4/20/2020	Funding Expectations	Democratic Caucus outlines compromise offer appropriating funds to local governments, with favorable apportionments to swing states.	[9]
	White House Outlines Negotiating Position	4/20/2020	Funding Expectations	White House and Sec. Mnuchin report a willingness to appropriate $300 Billion for the PPP.	[10]
	SEC 8-K Filings Released	4/20/2020	Loan Recipients	SEC 8-K filings reveal 70 publically traded companies received PPP loans. The White House and the GOP Caucus face sustained public criticism over recipients.	[28]
			Loan Recepients	Shake Shack announces they will return their $10 Million PPP loan to the SBA.	[31]
			Loan Receipt	NFIB reports only about 20% of their surveyed members who applied for a PPP loan had received money by April 17. Further, about 75% had applied with 26% in the process of applying when funding ran out.	[25]
	PPP Funding Passes Senate	4/21/2020	Funding Expectations	$484 Billion in relief funding passes Senate, with $310 Billion for PPP loans and $11 Billion for related fees.	[51]
			Loan Applications	Regional and community lenders are more successful than large banks at making modest loans to a large number of small businesses. Loan seekers report subsituting towards applying for PPP loans at smaller banks.	[22]
			Loan Allocation	JP Morgan Chase faces criticism after disclosing data that reveals high rejection rates among small business clients and high acceptance rates among large clients. Only 6% of JP Morgan's 300,000 small business customers received loans.	[23]
	Congress Passes New PPP Funding	4/23/2020	Funding Expectations	$321 Billion for PPP funding and related fees	

Tabular Summary of PPP Timeline *(continued)*

	Government Action:		Reporting:		
Week Of	Action	Date	Topic	Summary	Cite
	SBA Bars Firms with Other Cash Sources	4/23/2020	Loan Recipients	The SBA issues new guidance suggesting companies with other sources of cash would not qualify for PPP loans, and that the firms who have already borrowed under this standard should return their loan by May 7th.	[32]
			Loan Recipients	Kura Sushi and Sweetgreen agree to return millions in PPP loans.	[30]
			Program Administration	Small business associations express dismay over new funding numbers. Each expects that replenishment will be exhausted quickly, and criticizes the program's rollout as insufficient.	[3]
			Loan Recipients	Autonation, a national network of auto dealerships, returns more than $77 Million in PPP funds after two employees whistleblow to the Washington Post.	[29]
			Loan Allocation	There is a growing concern that minority owned businesses will be shut out of PPP funds, as funding sources more readily available to them, like Community Development Financial Institutions, are not PPP approved lenders.	[20]
			Program Administration	Small business owners who were denied PPP loans are becoming increasingly dissatisfied with the program. There is significant confusion among applicants and potential applicants over applications, approvals, where to get funds, and if they were accepted, whether they would get funds. There is a growing impression that large businesses had sizable advantages in securing loans combined with a waning belief in the good faith of the program. The self employed also feel shut out of the progam's rollout.	[2]
	President Signs New PPP Funding into Law	4/24/2020	Funding Expectations	$321 Billion for PPP funding and related fees	
April 26th to May 4th	SBA Institutes Cap on Per-Bank Applications	4/26/2020	Program Administration	The SBA sets a cap on the number of relief loans a single bank can process. Banks can issue at maximum 10% of the funding authority of PPP.	[27]
	Applications for New PPP Funds Open	4/27/2020	Loan Availability	Small Businesses encouraged to apply as soon as possible to access funds.	[8]

Tabular Summary of PPP Timeline *(continued)*

	Government Action:		Reporting:		
Week Of	Action	Date	Topic	Summary	Cite
			Loan Requirements	PPP loans mandate that 75% of the funds go towards salaries and 25% go towards rents to receive forgiveness. Businesses in high rent metros feel the squeeze, and either shut down or worry their loans will not be forgiven for spending more on rent than the loans require.	[13]
May 4th to 11th			Program Administration	Demand for PPP loans falls, but not demand for loans in general. Borrowers look elsewhere for funds, as they worry they will not get PPP loans forgiven and are unconvinced they can apply for and get funds quickly. Of $310 Billion appropriated, 40% remains available as of May 8th. Banks finding about 10% of their loan applications are duplicates. An illustrative quote from a business owner: "It's basically a large loan that we are going to be stuck with."	[12]
			Program Administration	The SBA inspector general finds that the SBA did not issue guidance to prioritize rural and underserved communities following congressional guidance on PPP administration.	[35]
May 11th to 18th	Census Bureau Small Business Survey	5/14/2020	Program Administration	Census Bureau survey finds that nationally, about 75% of small businesses sought some sort of Federal aid, with 38% of respondents reporting they had gotten the loan money. Of those surveyed, 30% sought EIDL loans, with 10% having received payment. The respondents believe it will take a significant period of time to return to pre-COVID-19 levels of demand.	[33]
May 18th to 25th	Congress Plans Changes to PPP Loan Requirements	5/17/2020	Loan Requirements	Congress expresses intent to change loan requirements to give businesses both more time and more flexibility as to how to disburse PPP funds.	[17]
	Senate Adjourns Without PPP Deal	5/21/2020	Loan Requirements	Senate adjourns for recess, failing to pass legislation to extend the term of PPP loans. Adjournment delays resolution of the problem until at least next month.	[18]
			Program Administration	SBA's administration of the program increasingly perceived as opaque and ignoring legislative priorities.	[34]

Tabular Summary of PPP Timeline *(continued)*

	Government Action:		Reporting:		
Week Of	Action	Date	Topic	Summary	Cite
	Treasury Rule Change	5/23/2020	Program Administration	Treasury Department revises PPP loan rules allowing the SBA to review program loans "of any size at any time of SBA's discretion" signaling intent to monitor how businesses spend funds. Borrowers must retain documentation for 6 years after initial loan and alert state unemployment offices if workers refuse requests to return to work.	[15]
			Program Administration	The Treasury guidance is seen as partly insufficient, as it leaves business's uncertaint about eligibility for forgiveness and loan terms untouched. However, the guidance does provide clairification on how to calculate payroll and non-payroll expenses.	[15]
May 25th to June 1st					
June 1st to 8th	Senate Passes PPP Extension	6/3/2020	Loan Requirements	Senate passes bill extending loan forgiveness timeline from eight to twenty four weeks. The bill also reduces payroll requirements to allow 60% of the loan to go towards payroll, compared to the previous requirement of 75%.	[1]
	PPP Extension Signed into Law	6/5/2020	Loan Requirements	Senate bill is signed into law, largely unchanged. Deadline to apply for new PPP loans Aug. 8th.	[14]
June 8th to 15th			Loan Forgiveness	Changes to program complicate the process of obtaining loan forgiveness. Uncertainty over the loan spending requirements and the prospect of increased fraud enforcement scares some businesses from applying for forgiveness. Lenders determine whether a loan satisfies forgiveness guidelines and fear being stuck with unprofitable loans, but the SBA has not yet said how lenders should submit forgiveness applications. The NFIB reports nearly one-third of businesses who received PPP loans passed the original eight week deadline by June 14. Multiple interviewees express that the loan could end their business if not forgiven instead of saving it, believing they would be unable to service a debt even if economic conditions returned to normal.	[45]

Tabular Summary of PPP Timeline *(continued)*

	Government Action:		Reporting:		
Week Of	Action	Date	Topic	Summary	Cite
June 15th to 22nd			Program Administration	The PPP widely seen as not living up to expectations. Only 4.6 million loans were allocated to 31.7 million US small businesses. The application process is widely seen as confusing and onerous in retrospect, with many small businesses report either returning money or not applying because they did not think they met the requirements.	[46]
June 22nd to 29th			Loan Forgiveness	Firms who received PPP loans early quickly spent the money to comply with the original rules, but now, cannot reapply to receive more PPP funding under the PPP's "one business, one loan" policy.	[44]

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