

Web Appendix: The \$800 Billion Paycheck Protection Program:
Where Did the Money Go and Why Did it Go There?*

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Web Appendix

This web appendix provides additional information on the analysis in the published text.

A Methodology for Take-up Rate and Other Elements of Table 1

We estimate PPP take-up—i.e. the number of employees at PPP recipient firms divided by the number of employees at eligible firms—by comparing data from the SBA on PPP loans by size bins to the total number of employees in firms of comparable sizes using data from the Census Bureau’s Statistics of U.S. Businesses (SUSB). We focus on take-up rates among employers with fewer than 500 employees for two reasons (There were 4,729 loans totaling \$18.6 billion to these firms and they are reflected in the memo line “Employers 500+” of Table 1). First, in all industries, firms with fewer than 500 employees were eligible for PPP loans, whereas in certain industries firms with more than 500 employees were eligible depending on firm revenue *or* PPP-specific carve-outs (i.e. in Accommodation and Food Services). Second, the SBA loan-level data censor the size of recipient employers at 500, making it difficult to accurately estimate take-up above the 500 threshold. For estimates of take-up rates among employers larger than 500, see [Autor et al. \(2020\)](#).

We make the following additional adjustments to the SBA loan-level data to remove some loans and to make it comparable to the SUSB data:

- We exclude loans to Puerto Rico, the Virgin Islands, and Guam (77,307 loans totaling \$3.3 billion across both 2020 and 2021).
- We exclude loans that are coded as “Active, Un-Disbursed” in the variable “lstat” (464,368 loans totaling \$10.3 billion)
- We exclude loans to businesses in the following NAICS industries as these are excluded from the SUSB universe: 111, 112, 482, 491, 525110, 525120, 525190, 541120, 814, 92.
- We exclude non-employers, defined here as loans to businesses of size equal to 1 *and* business type listed as self-employed, sole proprietors, independent contractors, or single-member LLCs.

– These loans are listed in the memo line “Non-employers” of Table 1.

– These businesses received 4.14 million loans totaling \$54.6 billion.

- Note: First-draw loans are defined in the SBA data as “procmm = PPP” and second draw loans are “procmm = PPS”.

In the text, we calculate the cost per job saved of the first two tranches of loans issued in 2020, totaling \$510 billion. Officially, however, the SBA reports that \$525 billion were issued in 2020. The discrepancy between these two figures is accounted for by removing 2020 loans to: Puerto Rico, the Virgin Islands, and Guam; Active, Un-Disbursed loans; and non-employers.

The latest release of Census SUSB data provide data for total employment by enterprise (i.e. firm) size as of March 2018. In order to compare the size of eligible employers on the eve of the COVID crisis in early 2020, we inflate employment by firm size using data from the BLS’s Business Employment Dynamics Table F which provides employment by firm size in March of 2018 and March of 2019 and 2020. Because employment had begun to decline in March 2020, we use the growth rate of employment by firm size from March 2018 to March 2019 and assume that same growth rate prevailed for an additional year. Because the firm size bins provided by the SUSB and BED do not correspond exactly, we use the closest comparison. For SUSB employment between 50-149, we use the BED data on employment at firms between 50-99; for SUSB employment between 150-299, we use 100-249 in the BED; for SUSB employment between 300-499, we use the 250-499 in the BED.

B Targeting of the PPP

The first two tranches of PPP funding released in 2020 were essentially untargeted other than for the size requirement (generally 500 or fewer employees). However, the third tranche of the PPP, released in 2021, was explicitly targeted at firms that experienced significant revenue losses over the course of the pandemic.¹ Targeting of this third tranche appears to have been relatively successful in directing loans to areas facing deeper economic shocks, as shown in Figure B.1. There is a pronounced, precise negative relationship between PPP loans issued in 2021—which were mostly second draw loans—and state-level employment changes occurring between February 2020 and June

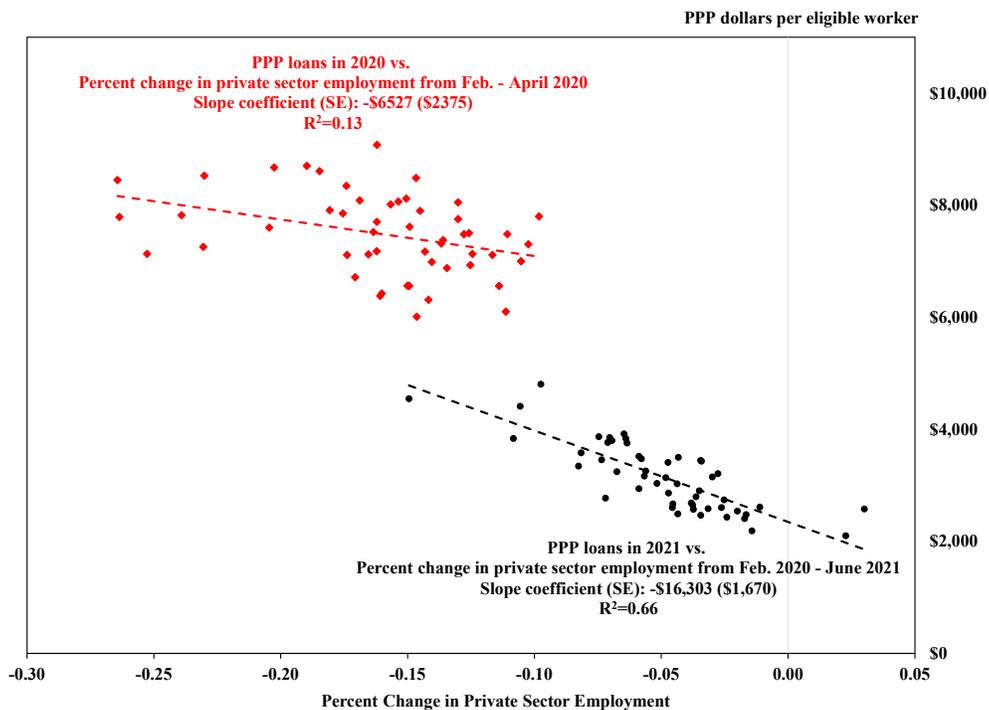
¹About 75% of the \$285 billion in third tranche funding went to second-draw loans for firms with under 300 employees that experienced significant revenue losses in 2020.

2021. The R-squared value of this bivariate regression is 0.66.

In contrast, there is little relationship between first and second tranche loan issues in 2020 and state-level employment changes occurring between February 2020 and April 2020. This finding is consistent with the lack of geographic correlation between the size of the initial COVID local economic shock, prior to PPP's passage, and subsequent PPP participation found in [Granja et al. \(2020\)](#).

Ironically, we find evidence that the poorly-targeted 2020 PPP loans moderately boosted employment. But we find no strong evidence that the relatively-better-targeted loans in 2021 positively affected employment.

Figure B.1: Targeting of PPP Relative to Employment Declines



Note. PPP loans per eligible worker at the state level are calculated by summing PPP loan amounts within each state and dividing by employment at firms with fewer than 500 employees. Loans in 2021 are either first or second draw loans. Source: Authors' analysis of Census Bureau SUBS, BLS CES, and SBA PPP data.

C Autor et al. (2020) Eligibility Threshold Difference-in-Difference Approach

In the paper we discuss employment results in [Autor et al. \(2020\)](#) based on a dynamic difference-in-difference (DD) model. We also present new employment estimates for second draw loans issued in 2021 which use this same DD model—see Figure 3. This appendix section discusses this research design.

The DD model estimates the effect of the PPP on various outcomes by comparing firms small enough to be eligible for the PPP to firms too large to be eligible. Specifically, the treatment group is comprised of firms in a range below the industry-specific employment size thresholds that define PPP eligibility. In most industries, the threshold is 500 employees. The control group is comprised of firms in a range above the threshold.

Formally, we estimate:

$$y_{ijst} = \alpha + \lambda PPP_i + \theta_{jt} + \theta_{st} + \sum_{t \in T} \beta_t (PPP_i \times \theta_t) + \varepsilon_{ijst} \quad (\text{A.1})$$

where y_{ijst} is the outcome being examined for firm i at week t indexed to equal 1 in February of 2020, PPP_i is an indicator variable equaling one if firm i is eligible for the PPP program based on the industry-specific size threshold, θ_{jt} is a vector of NAICS 3-digit industry j -by-week t fixed effects, θ_{st} is a set of state s -by-week t fixed effects, and θ_t is a vector of indicator variables for week t .

The β_t vector is the parameter of interest – it captures the time-varying treatment effect of PPP eligibility. The industry-by-week and state-by-week fixed effects control for the rapidly changing economic conditions across industries and states during the COVID crisis. The specification is weighted by firm size in February 2020; the results reflect the effect of the PPP on the average worker, as opposed to at the average firm. The sample is limited to firms within a given range above and below the industry-specific size threshold – e.g. within 250 employees of the threshold. Finally, we cluster standard errors at the NAICS 3-digit industry level.

See [Autor et al. \(2020\)](#) for more detailed information on the eligibility threshold DD approach, including a discussion of the identifying assumption required to interpret the results in a causal

sense and statistics demonstrating the comparability of the treatment and control groups.

D Event-Study Estimates

In the paper we present event-study estimates of the effect of the PPP on employment and firm closure for firms with fewer than 50 employees—see Figures D.1 and D.2, respectively. These estimates rely on first matching SBA PPP loan-level data to the ADP payroll data and then utilizing the methodology of Sun and Abraham (2020). This appendix section provides additional information on the estimates and also presents additional event-study estimates.

D.1 Merging PPP Loans to ADP Payroll Records

This appendix subsection describes the procedure that was adopted in order to identify which companies within our sample of ADP’s clients may have participated in the Paycheck Protection Program (PPP).

First, ADP cleaned each company name from both its client base and the database of PPP loan recipients that was disclosed by the Small Business Administration. This process initially entailed the removal of any prefixes, suffixes, stop words, and non-alphanumeric characters from a company name. Then, the remaining stem of each company name was converted into a Soundex code in order to allow for phonetic comparisons across both datasets. Next, for each PPP loan recipient, ADP compared the Soundex codes for every client that was physically located within a 0.1 mile radius of a given address. Specifically, a token set ratio was estimated for the comparison of each PPP borrower to an ADP client, and all approximate string matches with scores of at least 40 (on a scale of 0 to 100) were retained. It is worth noting that this approach explicitly allowed for the possibility of multiple ADP clients being matched to a single PPP recipient. Finally, in order to reduce the likelihood of false positives, these results were further restricted to string matches with a score of at least 80 for which the first characters of the names of each PPP loan recipient and a potential ADP client were also identical.

In order to preserve the confidentiality of ADP’s clients, we are unable to disclose the precise number of firms within our sample of employers that were matched to a PPP loan recipient. However, this string matching exercise suggested that only about half of the companies within our

sample of ADP clients participated in the Paycheck Protection Program. Given that PPP take-up is believed to have been nearly universal among employers with fewer than 500 employees (as shown in Table 1), it seems likely that this approach failed to identify a sizable number of ADP clients that actually received a loan.

D.2 Sun and Abraham (2020) Methodology

A burgeoning recent literature on event studies with differential timing of treatment highlights that the canonical two-way fixed effects regression suffers from the flaw that the composition of the ‘control’ group evolves dynamically as the set of treated firms grows (see [Goodman-Bacon, 2021](#); [Callaway and Sant’Anna, 2020](#); [Sun and Abraham, 2020](#)). This can cause bias when the magnitude of the effect of treatment is correlated with the timing of treatment.

To overcome this confound, we rely on the estimator developed by [Sun and Abraham \(2020\)](#) (SA hereafter), which estimates “cohort-specific” average treatment on the treated parameters and then averages those estimates using weights defined by the relative size of the cohorts. SA’s estimator can accommodate treatment effect heterogeneity across cohorts of treatment timing—in the case of the PPP, the week of loan approval—as well as time-varying treatment effects.

Because effectively all small firms are eventually treated over the sixteen weeks of the program in 2020, we obtain identification by contrasting firms that received PPP loans in the first eleven weeks of the program to firms that (subsequently) received loans in the final seven weeks. We are therefore assuming that employment in the control group firms would have evolved similarly to earlier (treatment) recipients in the absence of the PPP. We relegate firms receiving loans in the last seven weeks of PPP to the control group to ensure a sufficient sample size of comparison firms. Using only those firms receiving a PPP loan in the final week of the program as a comparison sample gives qualitatively similar results, however.

We bring the [Sun and Abraham \(2020\)](#) approach to the data with the following specification:

$$y_{it} = \alpha + \sum_{c \in T} \sum_{g=-8}^{11} (\beta_{c,g} * PPP_{g,it}) * D_c + \theta_{jt} + \theta_{st} + \epsilon_{it} \quad (\text{A.2})$$

where y_{it} is the outcome for firm i at week t , θ_{jt} is a vector of NAICS 3-digit industry j -by-week t fixed effects, θ_{st} is a set of state s -by-week t fixed effects, and $PPP_{g,it}$ is a dummy variable equaling

one if firm i at time t was approved for a PPP loan g weeks ago; $g = 0$ denotes the week of approval and the week prior to approval ($g = -1$) is the omitted category. D_c is a dummy variable denoting the week of PPP receipt for each cohort in the treatment set T (the first week through the eleventh week of the program).

We implement SA’s estimator using the authors’ Stata package “eventstudyinteract.” Standard errors are clustered at the NAICS 3-digit industry level. Estimates are weighted by firm size in February 2020 such that the results can be roughly interpreted as the effect of the PPP on the outcome variable for the average worker (rather than for the average firm).

D.3 Additional Event-Study Results

Figure D.1 presents the estimates of the SA event-study estimates including all firms in the ADP sample, as opposed to only firms with 1-49 workers as shown in Figure 2. Similarly, Figure D.2 presents the estimates of the effect of the PPP on firm exit using the event-study design estimated with all firms in the ADP sample, as opposed to only firms with 1-49 workers as shown in Figure 5. Note, though, that there are few larger firms receiving PPP loans late in the sample period that can serve as controls in the Sun and Abraham (2020) methodology. As a result, we have relatively more confidence in the event-study results for smaller firms sized 1-49 as compared to the results presented here for all firms.

The results in Figures D.1 and D.2 for all firms are similar to those displayed in Figures 2 and 5 for firms sized 1-49 employees, but are smaller in magnitude.

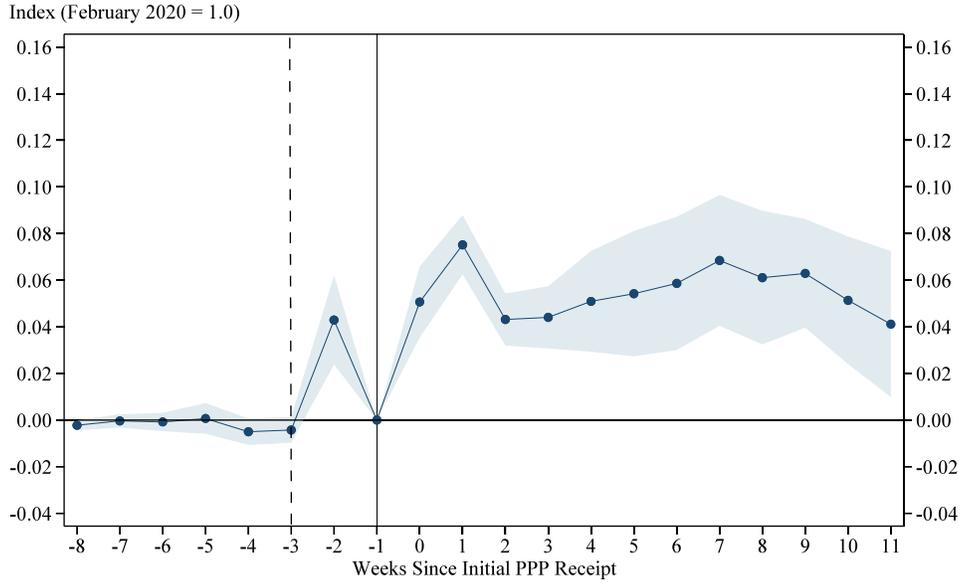
D.4 Event-study Timing

In Figures 2, 5, D.1, and D.2, the coefficient estimates for week $t - 2$ is typically non-zero and sometimes significant, in stark contrast to the estimates in earlier pre-treatment periods in each figure. This could indicate that our PPP treatment effects spuriously reflect factors other than the effect of the PPP. Or, the significant treatment effect in period $t - 2$ could reflect an anticipation effect: firms expecting to get PPP loans in the near future might be particularly unlikely to close down in advance of loan approval or may begin reopening.

However, there is an alternative potential explanation for these seemingly anomalous estimates.²

²Note that Dalton (2021), using a similar methodology, finds comparable treatment effects but no evidence of

Figure D.1: Event-Study Employment Effects at All Firms



Note. Estimates from Sun and Abraham (2020) event-study interaction estimator on the sample of loan-matched ADP firms. The outcome variable is firm-level employment indexed to equal 1 in February 2020. The estimates are weighted by each firm’s employment as of February 2020 and include controls for 3-digit industry-by-week and state-by-week fixed effects. Standard errors are clustered at the 3-digit industry.

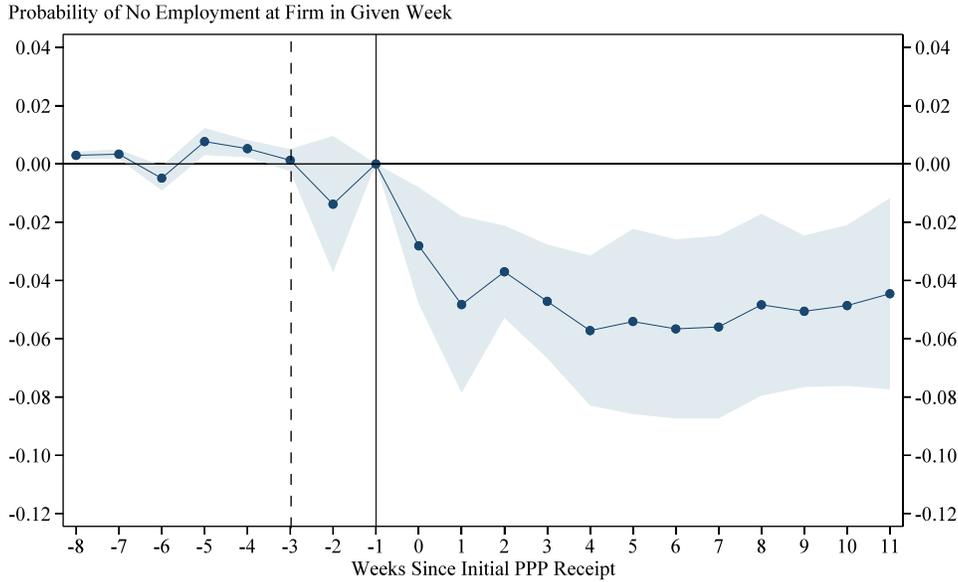
**All points to the right of the solid line represent post-treatment periods. Alternatively, accounting for the biweekly pay schedule of most ADP employers, and the back-filling used to establish start dates, all periods to the right of the dashed line can be viewed as post-treatment. See Appendix Section D.4 for more details.

Source: Authors’ analysis of SBA and ADP data using Sun and Abraham (2020) “eventstudyinteract” STATA implementation.

We believe they are possibly driven by the timing of hires within bi-weekly pay periods which are used by the vast majority of firms in the ADP data. While we observe the pay period in which a worker earns compensation in the ADP data, we do not observe the specific days on which they worked. The convention we follow is to assume that workers begin employment at the start of pay periods, e.g. if a worker is hired on the last day of the pay period, we assume she worked both weeks of the pay period. Thus, our “back-filling” procedure might artificially inflate employment two weeks prior to what happened in actuality. Indeed, the pre-PPP treatment estimates in the β_t vector prior to $t = -2$ are small and hover around zero. For this reason, we include a dashed vertical line at $t = -3$, two weeks prior to the standard vertical line at $t = -1$, and interpret all points to the right of $t = -3$ as plausibly post-treatment.

pre-treatment anticipation or non-zero pre-trends.

Figure D.2: Employment Change Due to Firm Closure at All Firms



Note. Estimates from Sun and Abraham (2020) event-study interaction estimator on the sample of loan-matched ADP firms. The outcome variable is an indicator variable equal to one if the firm has zero employment in a given week and zero if it has positive employment. The estimates are weighted by each firm’s employment as of February 2020 and include controls for 3-digit industry-by-week and state-by-week fixed effects. Standard errors are clustered at the 3-digit industry.

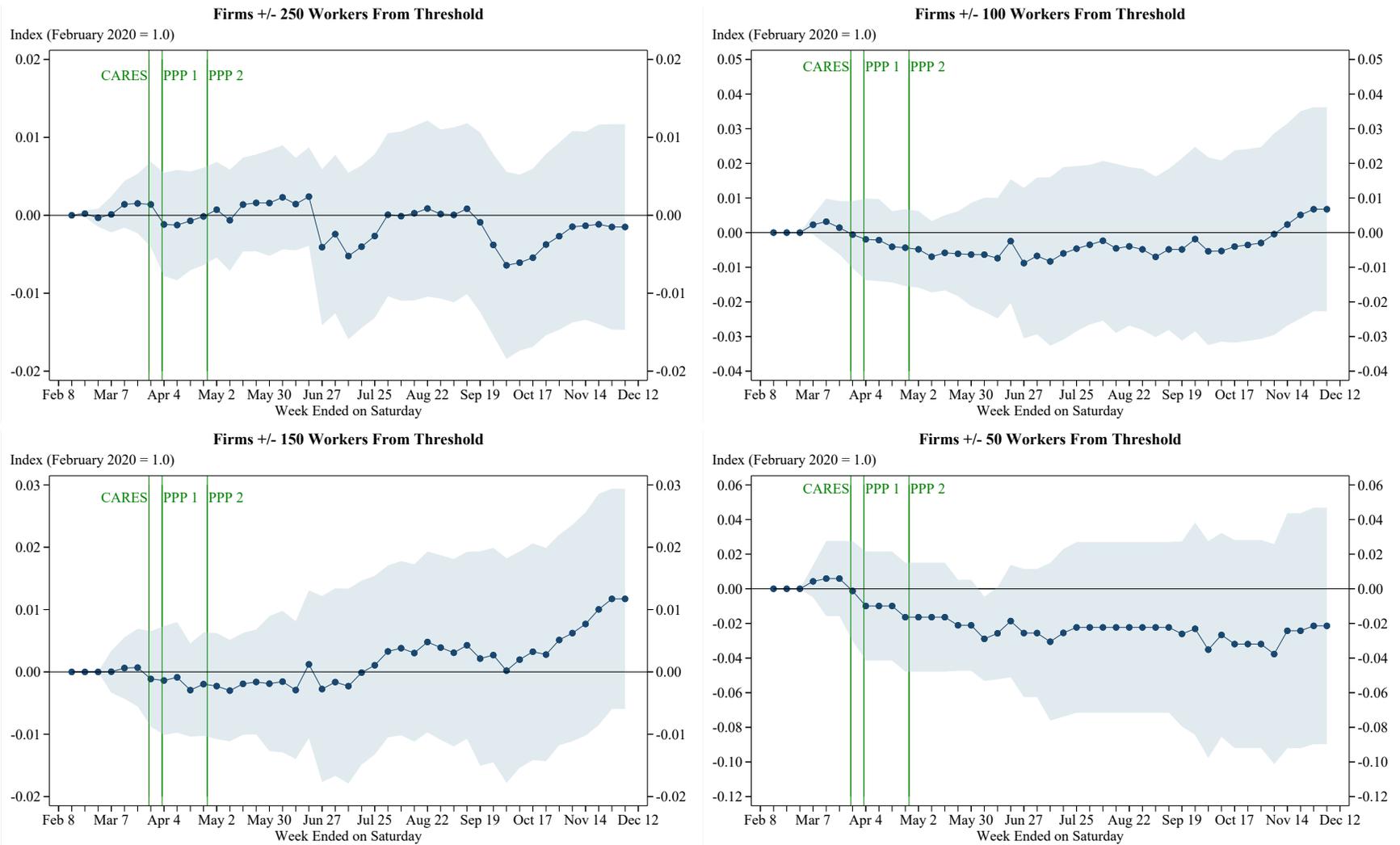
**All points to the right of the solid line represent post-treatment periods. Alternatively, accounting for the biweekly pay schedule of most ADP employers, and the back-filling used to establish start dates, all periods to the right of the dashed line can be viewed as post-treatment. See Appendix Section D.4 for more details.

Source: Authors’ analysis of SBA and ADP data using Sun and Abraham (2020) “eventstudyinteract” STATA implementation.

E Firm Closure Estimates for Larger Firms

Figure E.1 presents estimates of the effect of the PPP on firm closure for larger firms than considered in the estimates displayed in Figure 5. The estimates are based on the difference-in-difference approach of Autor et al. (2020)—discussed in appendix section C—which achieves identification by comparing firms below the employee eligibility threshold to firms above the eligibility threshold. Thus, the sample contains firms somewhat below and somewhat above the employee eligibility threshold—generally 500 workers. The estimating equation is appendix equation (A.1). The dependent variable is an indicator variable for a firm being closed, defined as having no employment in that week. We find no evidence that the PPP averted shutdowns for the larger sized firms considered.

Figure E.1: Effect of PPP Eligibility on Probability of Firm No Employment



Note: Each firm's size is determined using employment in both 2019 and February 2020. Regressions are weighted by firm size as of February 2020 and include controls for state-by-week and industry-by-week effects. Standard errors are clustered at the 3-digit NAICS industry level. Sample reflects firms that were present in the ADP data for all 12 months of 2019.

Source: Authors' analysis of ADP data.

F Distributionsal Incidence Calculations

This appendix section discusses the distributionsal incidence estimates presented in the paper for the PPP, expanded UI, and stimulus checks. The first subsection discusses the methodology behind these estimates and the second subsection presents alternative estimates based on assumptions which differ from those used in the paper.

F.1 Distributionsal Incidence Methodology

F.1.1 Method to Impute PPP compensation across the income distribution

1. Imputing PPP dollars that flowed to workers

- Let T denote our estimate of the PPP funds that flow from recipient businesses to the workers whose jobs were saved by the PPP.
- This is calculated as $T = CJ$
- C is compensation per worker whose job was saved by the PPP, calculated as average weekly wages in the CPS ORG microdata multiplied by the ratio of total compensation to total private industry wages and salaries from the BLS ECEC data from 2020Q1,
$$C = W \times \alpha_{ECEC}$$
 - $\alpha_{ECEC} = 1.42$, i.e., total compensation is 42% higher than wages and salaries.
 - $W = 52 * wk$, where wk is the employment-loss weighted average weekly wage calculated from the February 2020 CPS ORG, following the Center for Economic Policy Research’s methodology (CEPR, 2020). The employment loss weights are discussed below. We truncate the weekly wage at an annual rate of \$100,000 since the PPP did not support more than \$100,000 in worker compensation. We calculate $wk = \$786$.
 - $C = 52 \times \$786 \times 1.42 = \$58,185$
- J is the estimate of job-years saved by the PPP.
 - We start with the jobs saved estimates from Autor et al. (2020) which end in December 2020. We then extend the estimates through June 2021 (when they hit

zero) by linear extrapolation of the trend from the peak effect in May 2020 through December 2020.

- The estimates in [Autor et al. \(2020\)](#) are based on the analysis of relatively large firms. In the main text of this paper we find that for smaller firms with between 1 to 49 employees, the PPP jobs-saved effect is roughly double that estimated in [Autor et al. \(2020\)](#). Accordingly, we assume that the job-saved effect for these small firms is double that calculated immediately above based on the estimates in [Autor et al. \(2020\)](#). Since firms between 1-49 workers comprise about 52% of small business employment according to the BLS’s BED data, our jobs-saved estimate is $2 \times \beta \times 0.52 + \beta \times (1 - 0.52) = 1.52 \times \beta$, where β are the job-year estimates based on [Autor et al. \(2020\)](#) for each quarter from 2020Q2 through 2021Q2 (using the interpolation described immediately above).
- [Autor et al. \(2020\)](#) estimated that the PPP raised employment by $J^{Autor} = 1.98$ million job years.
- Using the larger effect on small firms, we estimate that the PPP raised employment by $J^{boost} = 3$ million job years.
- $T^{Autor} = C J^{Autor} = \$58,185 \times 1.98m = \$115$ billion.
- $T^{boost} = C J^{boost} = \$58,185 \times 3.0m = \$175$ billion.

2. Imputing PPP compensation to weekly wage quintiles

- *Assumption:* Workers whose jobs were saved by the PPP (and therefore who received PPP compensation) came from the same wage distribution as workers who did ultimately lose their jobs during 2020.
- We use the Current Population Survey ORG data on weekly wages in February 2020 to split workers into quintiles of the weekly wage distribution in that month prior to COVID, again following [CEPR \(2020\)](#).
- For each quintile, we calculate total employment for each month from February 2020 to December 2020. We then calculate the average decline in employment for each quintile-month, taking the log difference relative to February 2020. Let this employment decline

be denoted by d_q for quintile q .

- For each quintile, we calculate the average loss in weekly wages per month from March through December: $wk_q \times d_q$, where wk_q is the quintile-specific average wage from February 2020.
 - We truncate the weekly wage at an annual rate of \$100,000 due to the PPP’s cap on compensation per worker.
 - As an example, for the lowest quintile, $wk_1 = \$283$ and $d_1 = 17.8\%$, so $wk_1 \times d_1 = \$50.30$ per week on average over March through December 2020.
 - Now the share of compensation loss due to job loss can be calculated for each quintile:
$$s_q = \frac{wk_q d_q}{\sum_q wk_q d_q}.$$
 - Total PPP compensation for each quintile is simply $T \times s_q$.

3. Imputing PPP compensation to household income quintile

- We use data from the March 2020 Current Population Survey downloaded from IPUMS (Flood et al., 2021) to map the weekly wage distribution to the household income distribution.
- Using total household income for calendar year 2019 from the March 2020 CPS, we can compare the weekly wage distribution to the household income distribution. We do this as follows:
 - Define weekly wages as total wage and salary income divided by weeks worked.
 - Winsorize at the 1st and 99th percentiles.
 - Truncate at \$100,000 in wages and salaries.
 - Split individuals into their weekly wage quintiles.
 - Also split individuals into their household income quintiles.
 - Define the 5-by-5 probability matrix P where each entry is p_{wh} , the probability that an individual with weekly wages in the w th quintile is in a household in the h household income quintile.
- We can then map from weekly wage PPP compensation shares, defined above as s_q as follows.

- Define the 1-by-5 vector $S = [s_1, \dots, s_5]$.
- Then $S \times P$ gives a vector of the imputed shares of compensation lost by household income.
- Note that if P was the identity matrix, it would amount to assuming that the weekly wage distribution maps directly to the household income distribution.

F.1.2 Method to Impute PPP capital income across the income distribution

- Total PPP funds that flowed to non-workers, or capital, is \$510 billion minus the PPP funds that flowed to compensation, described in the previous section.
- The PPP went to both business owners and shareholders of businesses, and the BEA estimates a split between the PPP subsidies to corporations (64.6%) and to sole proprietors and partnerships (35.4%).
- PPP funds that flowed to corporations are a one-time windfall profit and so the incidence is assumed to fall entirely on capital. We follow the Congressional Budget Office assumption that the distribution of capital income follows that of the distribution of income from capital gains, interest, rent, and dividends.
- PPP funds that flowed to sole proprietors and partnerships are assumed to follow the distribution of business income in the CBO distributional tables.

F.1.3 Method to Impute Unemployment Insurance across the income distribution

- We impute shares of UI benefits using the same data on job loss and weekly wages as we described in section F.1.1. Recall that we denote the percent change in employment by quintile d_q and the weekly wage in February 2020 wk_d , and define their product, l_q , to be the average weekly wage lost by quintile.
- Unemployment insurance benefits are progressive in normal times in the sense that they replace a lower share of wages the higher the wages are. This is mediated through UI benefit schedules, which vary by state and replace wages subject to minimum and maximum weekly benefits, and imply a replacement rate rr_q which varies with wage quintile.

- We calculate rr_q using the same CPR ORG data as we describe above. The replacement rate is estimated using a simplified formula: $rr_q = E \left[\frac{\min\{\overline{UI}_s, \max\{UI_s, 50\% \times wk\}\}}{wk} \right]$, where UI_s and \overline{UI}_s are the state-specific minimum and maximum UI benefits reported in Chapter, Table 3-5 in the Department of Labor’s 2019 Comparison of State Unemployment Laws.
- We assume that this normal benefit formula applied in March and then October through December after the supplements to weekly benefits lapsed. From April through July, the CARES Act provided \$600 per week for each beneficiary and in August and September the Lost Wage Assistance program provided an additional \$300 per week.
 - We can augment the estimated replacement rates in those months in a straightforward way: $rr_q = E \left[\frac{\min\{\overline{UI}_s, \max\{UI_s, 50\% \times wk\}\}}{wk} + \frac{s}{wk} \right]$, where s is the weekly supplement.
- Finally, we take the simple average from March through December of the replacement rates by quintile, \overline{rr}_q .
- We can now apply the replacement rate to the wage loss by quintile to estimate the share of UI benefits that flow to each wage quintile: $s^{UI} = \frac{\overline{rr}_q \times l_q}{\sum_q \overline{rr}_q \times l_q}$.³
- Multiplying the share of UI benefits by quintile by the total amount of UI paid in 2020 (above what would have been paid if the 2019Q4 amount continued into 2020), \$557 billion (BEA), gives our estimate of UI benefits by quintile.

F.1.4 Method to Calculate Annual Household Income by Quintile

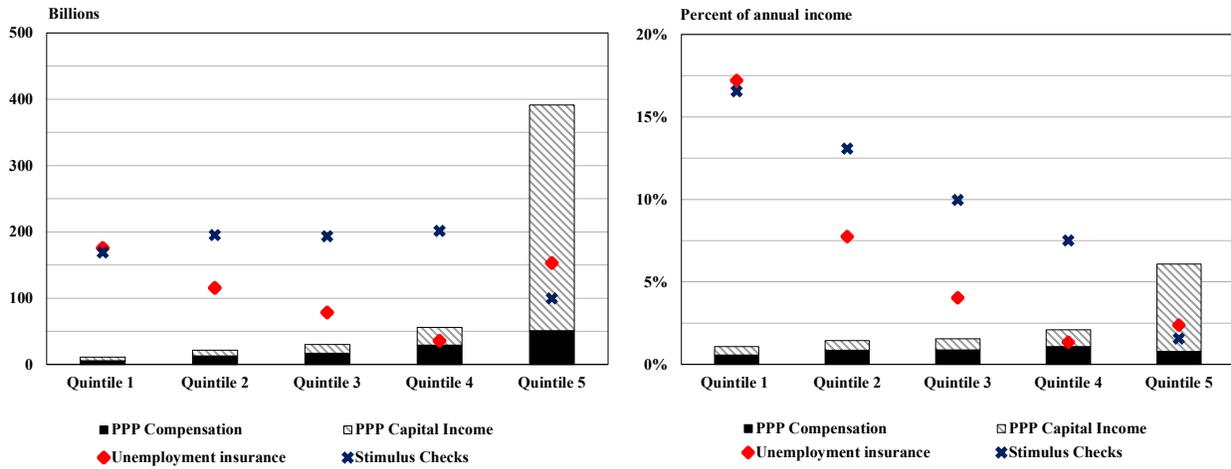
- The right-hand panel of Figures 6 and F.1 utilizes annual household income by quintile. To calculate this we take CBO data from 2017 (the last year available) on average after-tax and transfer household income by quintile and multiply by the number of households.
- To inflate these figures to pre-COVID levels, we use Census average household income growth by quintile from 2017-2019.

³We map these to the household income distribution using the method in section F.1.1.

F.2 Alternative Distributional Incidence

Figure 6 in the published text displays our incidence calculations based on relative generous assumptions for the magnitude by which the PPP supported employee compensation; specifically, the estimates in Figure 6 use the assumption that \$175B of employee compensation was supported by the PPP. Figure F.1 offers the same distributional breakdown of PPP funds as shown in Figure 6, but under the alternative, smaller assumption that the PPP supported \$115B of compensation. Appendix subsection F.1.1 discusses the calculation of the \$175B and \$115B PPP-supported compensation estimates.

Figure F.1: Alternative Distributional Analysis of PPP



Note. See online appendix text for details of calculations.

Source: Authors' analysis of CBO, Census Bureau, BEA, BLS ECEC, Current Population Survey microdata, and estimates from Autor et al. (2020), Bhutta et al. (2020), and Boesch et al. (2021).

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