Online Appendix

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

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

David R. to fill in. INCLUDE DISCUSSION OF $525 versus $510

B Targeting of the PPP

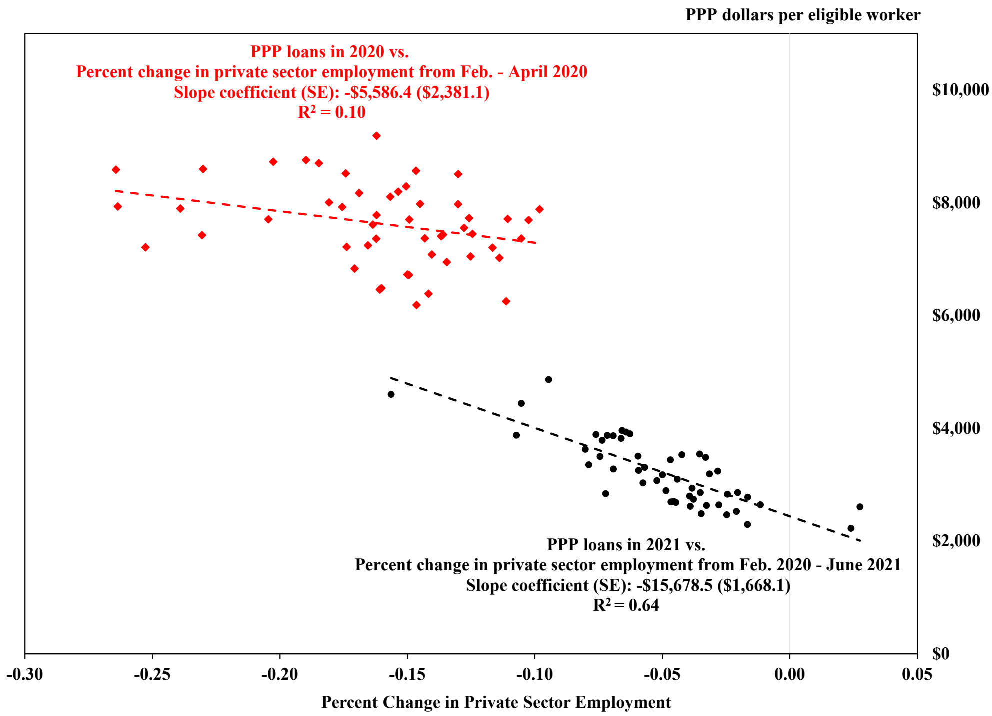
Unlike the first two tranches of funding released in 2020, the third tranche of the PPP, released in 2021, was explicitly targeted at firms that experienced significant revenue losses over the course of the pandemic.[1] Targeting of this third tranche appears to have been relatively successful in directing loans to areas facing relatively 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 third tranche loans—and state-level employment changes occurring between February 2020 and June 2021. The R-squared value of this bivariate regression exceeds 0.60.

In contrast, there is little relationship between first and second tranche loans 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. We find no strong evidence, however, that the relatively-better-targeted loans in 2021 positively affected employment.

[1] About 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.

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 SUSB, BLS CES, and SBA PPP data.

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

In the paper we present results based on the dynamic difference-in-difference (DD) model employed in Autor et al. (2020). 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:

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

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. t spans the week of February 2$^{\text{nd}}$ through February 8$^{\text{th}}$ 2020 to the week of November 29$^{\text{th}}$ through December 5$^{\text{th}}$ 2020.

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; as a result, 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. These estimates rely on matching SBA PPP loan-level data into the ADP payroll data and utlize 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 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 as well as 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 techniques 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:

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

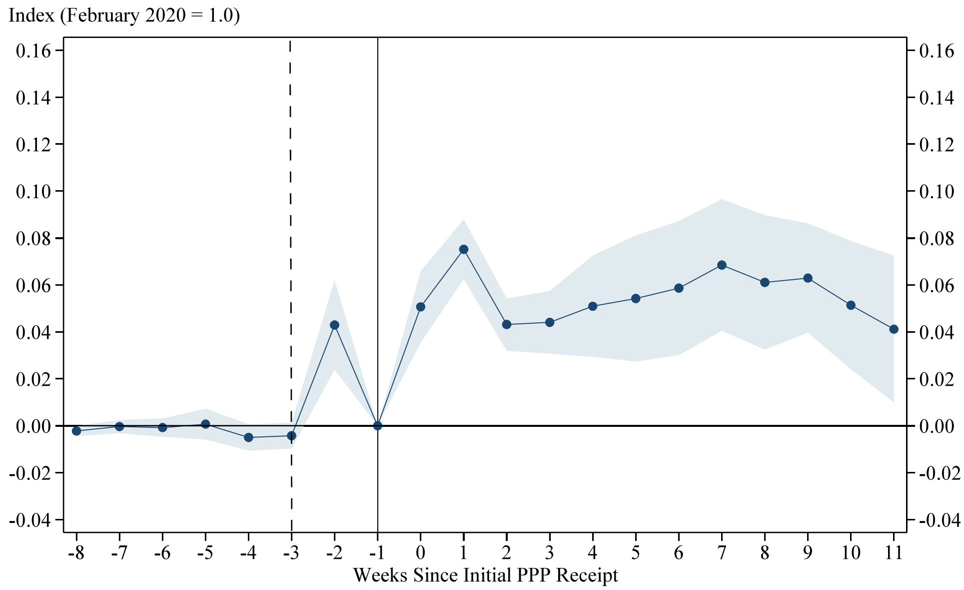
where y_{it} is total employment for firm i at week t indexed to equal 1 in February of 2020, θ_{jt} is a vector of NAICS 3-digit industry j–by–week t fixed effects, θ_{st} is a set of state s–by–week t fixed effects, and $PPP_{g,it}$ is a dummy variable equaling one if firm i at time t was approved 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 average worker employed (rather than employment at the average firm). Using firms which received a PPP loan in the final seven weeks of the PPP as our control group, we estimate the regression using ADP employment data through the week of June 27$^{\text{th}}$.

D.3 Additional Event-Study Results

Figure D.1 presents the estimates of the Sun and Abraham 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

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



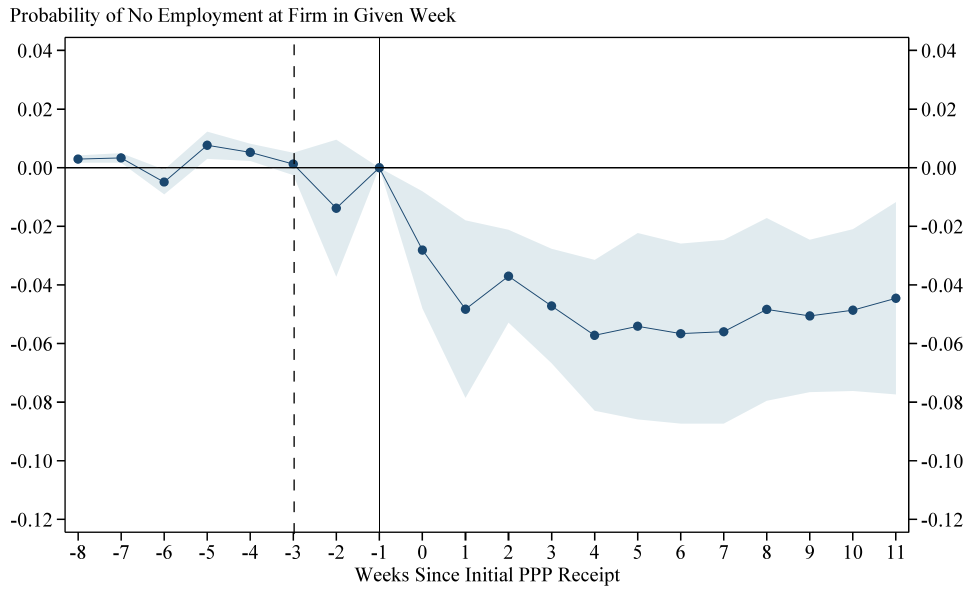
Note. Estimates from Sun and Abraham (2020) event-study interaction estimator on the sample of loan-matched ADP firms. The outcome variable in firm-level employment indexed to equal 1 in February 2020. The estimates are weighted by each firm's employment as of February 2020 and include controls for 3-digit 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.

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 on Figures D.1 and D.2 for all firms are similar to those displayed on 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.

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.

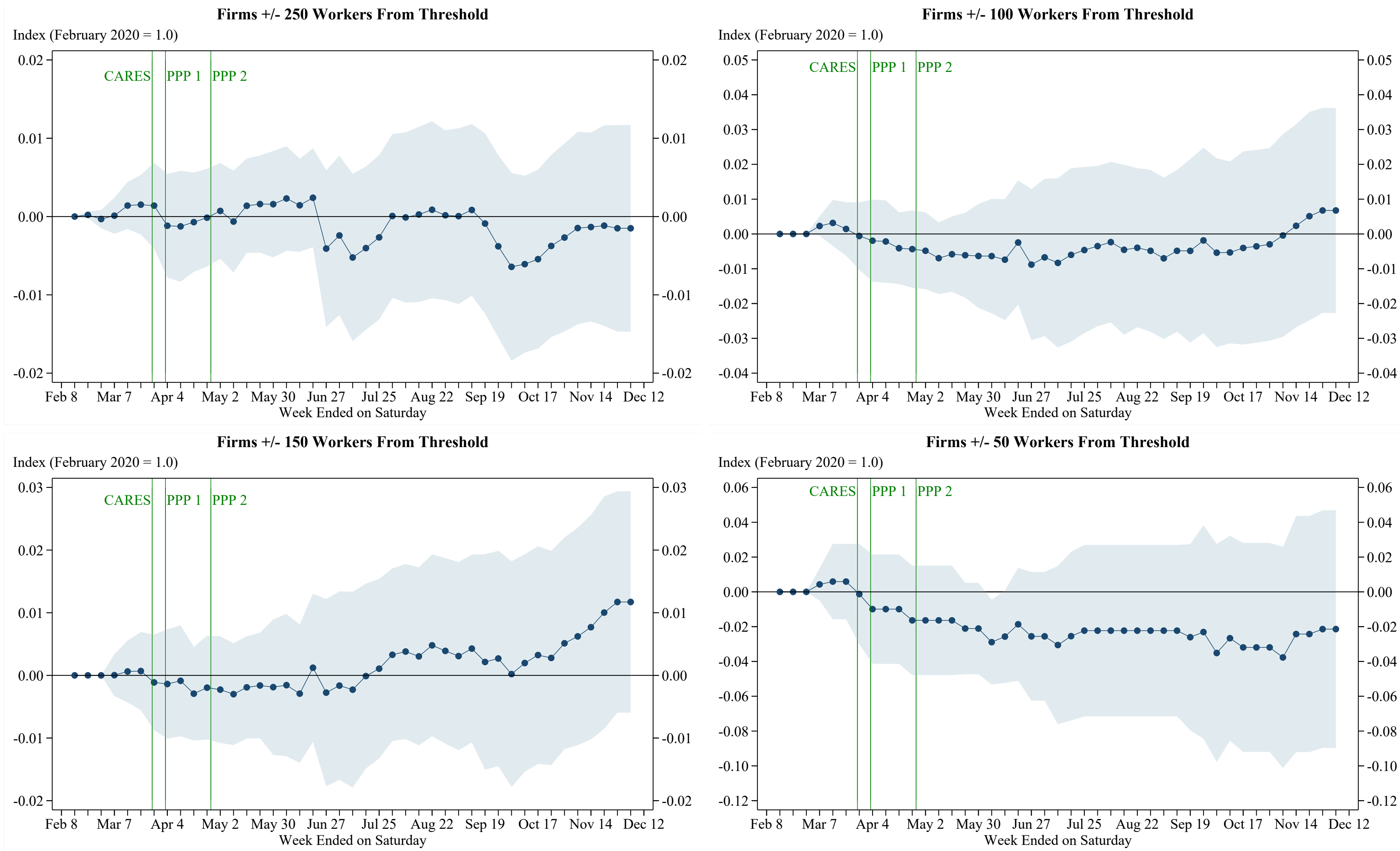
However, there is a potential explanation for these seemingly anomalous estimates.[2] We believe they are possibly driven by the timing of hires within bi-weekly pay periods which are used by the vast majority of firms in the ADP data. While we observe the pay period in which a worker earns compensation 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 bounce 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.

[2]Note that Dalton (2021), using similar methodology, finds comparable treatment effects but no evidence of pre-treatment anticipation or non-zero pre-trends.

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) which achieves identification by comparing firms below the employee eligibility threshold to firms above the eligibility thresholds. 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 Having 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

F.1 Distributional 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 flows 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 average weekly wage in the CPS ORG data from February 2020, calculated using the imputation method from the Center for Economic Policy Research CITE.
     - wk is calculated as the employment-loss weighted average wage, truncated at an annual rate of $100,000 since the PPP did not support more than $100,000 in worker compensation. We get $wk = 786$.
     - $C = 52 \times \$786 \times 1.42 = \$58,185$
   - J is the estimate of job-years saved by the PPP.
     - We use the estimates from Autor et al. (2020), where we extend our estimates from December 2020 through June 2021 (when they hit zero) by linear extrapolation of the trend from the peak effect in May 2020 through December 2020.
     - To accommodate the larger effect on employment for firms between 1-49 as we find above, we assume that these effects are double those 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 estimate is $2 \times \beta \times 0.52 + \beta \times (1 - 0.52) =$

$1.52 \times \beta$, where β are the job-year estimates from Autor et al. (2020) for each quarter from 2020Q2 through 2021Q2.

  – 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.

- $T^{Autor} = CJ^{Autor} = \$58,185 \times 1.98m = \$115$ billion.
- $T^{boost} = CJ^{boost} = \$58,185 \times 3.0m = \$175$ 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 fro 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. ADD NOTE FOR DETAILS ON WEEKLY WAGE CODING USING CEPR.
- For each quintile, we calculate total employment for each month from March 2020 to December 2020 and calculate the average decline in employment for each quintile. 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 to map fro 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 wth 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 $S = [s1, ..., s5]$.
     - 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 map 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 business owners 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 let their product l_q be 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 that 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: $rr_q = E\left[\frac{\min\{\overline{UI}_s, \max\{\underline{UI}_s, 50\% \times wk\}\}}{wk}\right]$, where $\underline{UI}_s$ and $\overline{UI}_s$, where $\underline{UI}_s$ and $\overline{UI}_s$ are the state-specific minimum and maximum UI benefits reported by the DOL. CITE

- 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: $rr_q = E\left[\frac{\min\{\overline{UI}_s, \max\{\underline{UI}_s, 50\% \times wk\}\}}{wk} + \frac{s}{wk}\right]$, where s is the weekly supplement.

- Finally, we take the simple average from March through December of the replacement rates by quintile, $\overline{rr}_q$.

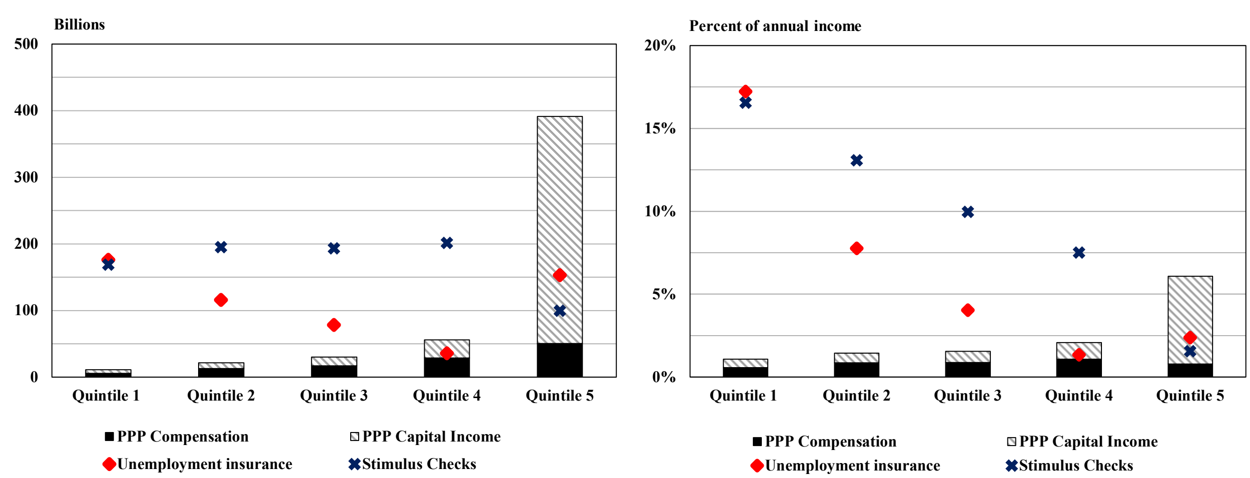
- We can now apply the replacement rate to the wage loss by quintile to estimate the share of UI beneifts that flow to each wage quintile: $s^{UI} = \frac{\overline{rr}_q \times l_q}{\sum_q \overline{rr}_q \times l_q}$.[3]

- Multiplying the share of UI benefits by quintile by the total amount of UI paid in 2020, \$557 billion cite, gives our estimate of UI dollars by quintile.

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.

Figure F.1: Alternative Distributional Analysis of PPP



Note. See online appendix for details of calculations.
Source: Authors' analysis of CBO, BEA, BLS's ECEC, and CPS data, and estimates from Autor et al. (2020) and Boesch et al. (2021).

[3]As above, we map these to the household income distribution using the method in section F.1.1.

10195 (errors:1) words

53685 (errors:1) characters (not including spaces)

```
File: ADP-JEP-20211210-2nd-Submission.tex
Encoding: utf8
Sum count: 10195
Words in text: 8950
Words in headers: 105
Words outside text (captions, etc.): 1064
Number of headers: 21
Number of floats/tables/figures: 8
Number of math inlines: 75
Number of math displayed: 1
Subcounts:
  text+headers+captions (#headers/#floats/#inlines/#displayed)
  1220+18+246 (2/0/0/0) _top_
  576+2+45 (1/0/0/0) Section: The Basics
  33+5+0 (1/0/0/0) Section: A Timeliness versus Targeting Tradeoff
  942+1+5 (1/1/1/0) Subsection: Timeliness
  185+1+0 (1/0/0/0) Subsection: Targeting
  0+7+0 (1/0/0/0) Section: What Did the Paycheck Protection Program Accomplish?
  1076+2+317 (1/3/17/1) Subsection: Supporting Employment
  618+3+231 (1/2/0/0) Subsection: Preventing Firm Exits
  115+3+0 (1/0/0/0) Subsection: Reducing Commercial Delinquency
  76+5+0 (1/0/0/0) Section: Where Did the Money Go?
  571+5+39 (1/0/3/0) Subsection: Proximate Recipients: Workers vs. Non-Workers
  978+6+76 (1/1/6/0) Subsection: The Household Distributional Incidence of PPP
  592+2+85 (1/0/0/0) Subsection: Macroeconomic Benefits}\label{subsec:macro-benefits
  814+8+0 (1/0/0/0) Section: Lessons Learned from the Paycheck Protection Program Experience
  0+3+0 (1/0/0/0) Section: Distributionsal Incidence Calculations
```

1083+31+15 (4/0/48/0) Subsection: Distributional Incidence Methodology

71+3+5 (1/1/0/0) Subsection: Alternative Distributional Incidence

(errors:1)