NBER WORKING PAPER SERIES

THE $800 BILLION PAYCHECK PROTECTION PROGRAM: WHERE DID THE MONEY GO AND WHY DID IT GO THERE?

David Autor
David Cho
Leland D. Crane
Mita Goldar
Byron Lutz
Joshua K. Montes
William B. Peterman
David D. Ratner
Daniel Villar Vallenas
Ahu Yildirmaz

Working Paper 29669
http://www.nber.org/papers/w29669

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 0213
January 2022

Key data for this paper were provided for research by ADP, LLP. ADP approved this paper's topic ex-ante and reviewed the paper prior to distribution to ensure it did not reveal confidential information about ADP's clients or business model. Coauthors Goldar and Yildirmaz were involved in making the data available to several research teams as employees of the ADP Research Institute. At the Federal Reserve Board, we thank Christopher Kurz and Norman Morin for support, Kendra Robbins and Eleanor Warren for excellent research assistance, and Kevin Moore for updating the analysis in Bhutta et al. (2020). We thank Tolga Tuncoglu of ADP for superb assistance with matching the SBA PPP loan data into the ADP data. We thank Michael Dalton, Andrew Goodman-Bacon, Erik Hurst, Katie Lim, Joseph Nichols, Ryan Nunn, Matthew Shapiro, Liyang Sun, and Eric Zwick for helpful discussion. Autor acknowledges financial support from the Smith Richardson Foundation (#20202252), Accenture LLP (#027843-0001), the Andrew Carnegie Fellowship (G-F-19-56882), and the Washington Center for Equitable Growth (APP-01666). The analysis and conclusions set forth here are those of the authors and do not indicate concurrence by other members of the Federal Reserve Board research staff, by the Board of Governors, or by ADP. ADP's data privacy policy can be found at https://www.adp.com/about-adp/data-privacy.aspx. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 29669
January 2022
JEL No. E65,H2,J38

ABSTRACT

The Paycheck Protection Program (PPP) provided small businesses with roughly $800 billion dollars in uncollateralized, low-interest loans during the pandemic, almost all of which will be forgiven. With 93 percent of small businesses ultimately receiving one or more loans, the PPP nearly saturated its market in just two months. We estimate that the program cumulatively preserved between 2 and 3 million job-years of employment over 14 months at a cost of $170K to $257K per job-year retained. These estimates imply that only 23 to 34 percent of PPP dollars went directly to workers who would otherwise have lost jobs; the balance flowed to business owners and shareholders, including creditors and suppliers of PPP-receiving firms. Program incidence was highly regressive, with about three-quarters of PPP funds accruing to the top quintile of households. This compares unfavorably to the other two major pandemic aid programs, enhanced UI benefits and Economic Impact Payments (i.e. stimulus checks). PPP's breakneck scale-up, its high cost per job saved, and its regressive incidence have a common origin: PPP was essentially untargeted because the United States lacked the administrative infrastructure to do otherwise. The more targeted pandemic business aid programs deployed by other high-income countries exemplify what is feasible with better administrative systems. Building similar capacity in the U.S. would enable greatly improved targeting of either employment subsidies or business liquidity when the next pandemic or other large-scale economic emergency occurs, as it surely will.

David Autor
Department of Economics, E52-438
Massachusetts Institute of Technology
77 Massachusetts Avenue Cambridge,
MA 02139
and NBER
dautor@mit.edu

David Cho
Board of Governors of the
Federal Reserve System
20th and C Streets, NW
Mail Stop 80
Washington, DC 20551
david.cho@frb.gov

Leland D. Crane
Federal Reserve Board
20th Street and C Street, NW
Washington, DC 20551
leland.d.crane@frb.gov

Mita Goldar
Independent Researcher
mitagoldar@yahoo.com

Byron Lutz Federal
Reserve Board of Governors Research
Division 20th and C Streets, NW
Washington, DC 20551-0001
Byron.F.Lutz@frb.gov

Joshua K. Montes
Federal Reserve's Board of Governors
20th & C St. NW
MS- 80
Washington, DC 20551
joshua.k.montes@frb.gov

William B. Peterman
Federal Reserve Board of Governors
20th & C St. NW
Washington, DC 20551
william.b.peterman@frb.gov

David D. Ratner
Board of Governors of the Federal Reserve System
20th Street and Constitution Avenue N.W.
Washington, D.C. 20551
david.d.ratner@frb.gov

Daniel Villar Vallenas
Federal Reserve Board
daniel.villar@frb.gov

Ahu Yildirmaz
The Coleridge Initiative
ahu.yildirmaz@coleridgeinitiative.org

In the early weeks of the COVID-19 pandemic, many small businesses in the United States were in precarious financial condition: revenues had plunged, access to credit was in many cases inadequate or absent, and large-scale layoffs and closures had already occurred (Bartik et al., 2020a,b). The potential consequences of widespread business failure were not confined to business owners. Since approximately 47 percent of US workers were employed by small businesses prior to the pandemic (SBA, 2019), these closures held the potential for vast job loss. Over the longer term, widespread firm closures could slow the subsequent economic recovery by destroying intangible firm capital, liquidating high quality worker-firm matches, and forcing the costly reallocation of physical capital.

To aid these distressed businesses, Congress enacted the Paycheck Protection Program (PPP), which provided uncollateralized, low-interest loans of up to $10 million to firms with fewer than 500 employees—loans that were forgivable on the condition that recipient firms maintained employment and wages at close to pre-crisis levels in the two to six months following loan receipt. The scale of the aid provided was extraordinary. By the time the program concluded in mid-2021, around $800 billion in loans had been extended. Despite facing initial capacity constraints, the Paycheck Protection Program was notably successful in distributing a vast number of loans in short order: the take-up rate among eligible firms was 94 percent. Crucial to this rapid rollout was the decision to enlist the private sector to oversee the origination of all PPP loans, with the Small Business Administration (SBA) serving as the guarantor.

The Paycheck Protection Program was ultimately comparable in size to the two other major federal transfer programs enacted in response to the pandemic: expenditures on household payments—i.e. stimulus checks—were around $800 billion; and expenditures on expanded unemployment benefits totalled roughly $680 billion under the Federal Pandemic Unemployment Compensation program (FPUC), Pandemic Unemployment Assistance program (PUA), and Pandemic Emergency Unemployment Compensation (PEUC) (CRFB, 2021). As another standard of comparison, each of these three programs was roughly comparable in size to the *entire* American Recovery and Reinvestment Act of 2009 (ARRA), the principal fiscal stimulus enacted in response to the Great Recession of 2007-2009.

This paper explores who ultimately benefited from those $800 billion in Paycheck Protection Program loans: concretely, where did the money go and why did it go there? We provide an answer in three steps. First, we consider how PPP funds flowed to three proximate sets of actors: workers who otherwise would have been laid off; creditors and suppliers of PPP-receiving businesses (e.g., landlords, utilities, etc.) who would otherwise not have received payments; and windfall transfers to PPP-recipient businesses (owners and shareholders) that would have maintained employment and met other financial obligations absent the PPP. Second, we calculate how these recipients were distributed across the household income distribution. Finally, we compare this allocation of funds to the household incidence of the two other major federal pandemic transfer programs: unemployment assistance and direct household payments. Our analysis combines lessons from existing research, including some of our own, and also presents new analysis using anonymized and aggregated payroll data from the private firm ADP, which processes payrolls for over 26 million individual workers in the United States per month.

PPP had measurable impacts. It meaningfully blunted pandemic job losses, preserving somewhere between 1.98 and 3.0 million job-years of employment during and after the pandemic at a substantial cost of $69K to $258K per job-year saved. PPP also reduced the rate of temporary closures among small firms, though it is less clear whether it reduced permanent closures. The majority of PPP loan dollars issued in 2020—66 to 77 percent—did *not* go to paychecks, however, but instead accrued to business owners and shareholders. And because business ownership and share-holding are concentrated among high-income households, the incidence of the program across the household income distribution was highly regressive. We estimate that about three-quarters of PPP benefits accrued to the top quintile of household income. By comparison, the incidence of federal pandemic unemployment insurance and household stimulus payments was far more equally distributed.

Ironically, the program feature that arguably made PPP's meteoric scale-up possible is also the feature that made it potentially the most problematic: the program was essentially untargeted, aside from excluding firms with more than 500 workers (a rule further relaxed for some sectors). Small firms merely needed to attest that they were "substantially affected by COVID-19" to qualify, and almost all did so. Evidence strongly suggests that the program did not ultimately differentiate among firms or geographic areas according to need. This near absence of targeting virtually guaranteed that a large fraction of the first two tranches of $525 billion in PPP loan dollars went to businesses that would have remained viable and retained their employees even absent PPP. Perhaps recognizing this program limitation, Congress explicitly targeted the final tranche ($285 billion) of PPP loans in 2021 toward firms that had experienced revenue losses.

The PPP's meteoric scale-up, its lack of targeting, and its highly regressive incidence reflect a key tradeoff that policymakers faced in March of 2020 when crafting an emergency pandemic business loan program under severe time constraints: a lack of existing administrative infrastructure for overseeing large-scale targeted federal support to US small businesses. Congress accordingly authorized the Small Business Administration (SBA) to harness the private sector to originate forgivable PPP loans and stipulated only a few coarse limitations on which firms could receive loans. These decisions rapidly opened the PPP floodgates to essentially all firms with fewer than 500 employees. Had policymakers instead insisted on better targeting, this would have likely substantially slowed aid delivery and reduced program efficacy. A key takeaway from the PPP experience is that building U.S. administrative capacity *prior to* the next pandemic or other large-scale economic emergency would enable greatly improved targeting of either employment subsidies or business liquidity when the need arises again.

The Basics

The Paycheck Protection Program sought to issue forgivable loans to small firms facing financial distress.[1] Businesses were permitted to draw PPP loans worth up to 10 weeks of payroll costs—including wage and salary compensation not to exceed $100,000 per worker, as well as paid leave, health insurance costs, other benefit costs, and state and local taxes—with a maximum loan size of $10 million dollars. Although the Small Business Administration issued the loan guarantees and would ultimately determine whether loans would be forgiven, PPP loans were processed and delivered through the nation's banking system.

The program received three tranches of funding. The Coronavirus Aid, Relief, and Economic Security Act of 2020 (CARES) established the Paycheck Protection Program and provided $350 billion in appropriations on March 27, 2020. Subsequently, the Paycheck Protection Program and Health Care Enhancement Act, which passed on April 24$^{\text{th}}$, 2020, provided an additional $320

[1] The Paycheck Protection Program one was one of four large government direct-lending programs introduced during the pandemic; the other three programs were the Main Street Lending Program, Corporate Credit Facilities, and Municipal Liquidity Facility. These programs jointly covered a large swath of the US economy (Decker et al., 2021).

billion in appropriations. A third tranche of $285 billion was signed into law on December 27, 2020, as part of the Consolidated Appropriations Act of 2021. Finally, early on in the pandemic, the Federal Reserve introduced the Paycheck Protection Program Liquidity Facility (PPLF) to bolster the ability of the banking system to provide PPP loans (Anbil et al., 2021).

Loans from the first two tranches were issued in 2020 and available to firms meeting the PPP's definition of a small business. In most, but not all, industries this required having fewer than 500 employees. The third tranche provided loans to firms in 2021 that had not previously taken out a PPP loan. It also provided "second draw" loans for firms that had already taken out a PPP loan, had fewer than 300 employees, and had experienced a significant revenue loss in 2020. About 75 percent of the third tranche of funding went to second-draw loans.

While the moniker Paycheck Protection Program suggests that the program was focused solely on employment, the criteria for loan forgiveness reveal another complementary goal: providing firms with liquidity to meet non-compensation obligations to creditors (e.g., suppliers, banks, landlords, etc). Businesses had to do four things to qualify for PPP loan forgiveness: 1) spend at least 60 percent of the loan amount on payroll expenses; 2) spend (at least) the full loan amount on total qualifying expenses, including payroll, utilities, rent, and mortgage payments; 3) maintain average full-time equivalent employment at its pre-crisis level; and 4) maintain employee wages at at least 75 percent of their pre-crisis level. These criteria applied to a "covered period" that started on the date of loan disbursal and ran for 8 to 24 weeks, with the interval at the firm's discretion.

If these criteria were not met, SBA offered alternative routes to forgiveness. Businesses could exercise a "safe harbor" option to meet the employment and wage criteria by restoring their full-time equivalent employment and wage rates to their pre-COVID level by the end of 2020 (or by the end of the covered period for loans issued in 2021). This safe harbor provision made the employment criteria far less onerous. Moreover, if a firm did not meet all criteria, loan forgiveness could be partial. Finally, policymakers retroactively loosened the rules for forgiveness in June of 2020 (the discussion here pertains to the revised rules). The vast majority of firms were ultimately able to meet these criteria: as of late 2021, 94% of PPP loans issued in 2020 had applied for forgiveness and virtually all such applications had been approved by the SBA (Small Business Administration, 2021).

A Timeliness versus Targeting Tradeoff

Fiscal interventions during economic downturns are often judged based on whether they are targeted, timely, and temporary (Elmendorf and Furman, 2008). The Paycheck Protection Program was clearly *temporary*. How did it do on the other two T's?

Timeliness

The program deserves high marks for timeliness. When the pandemic began, no existing federal program had the scale to quickly distribute hundreds of billions of dollars to small businesses. The only other possible mechanism seemed to be state unemployment insurance systems (Bernstein and Rothstein, 2020), but these systems struggled to handle the flood of initial unemployment insurance claims, and struggled further when tasked with distributing the CARES Act's enhanced unemployment benefits. It seems unlikely that state UI systems could have handled an additional novel burden (Hubbard and Strain, 2020).

Despite these obstacles, the Paycheck Protection Program succeeded in delivering a staggering sum of money over a two-month period in the spring 2020. This can be seen in Table 1. As shown in column (3) of panel A, $505 billion in first draw loans were issued to firms with fewer than 500 employees (column 3) and all but seven percent of these were issued in 2020 (column 6). A very large share of these loans were issued in April and May (not shown). Finally, the memo lines show that non-employer businesses—e.g. the self-employed—received $44 billion in first draw loans and employers with more than 500 employees received a relatively small $18 billion.

One emblem of PPP's success is its market penetration, which we define as the employment-weighted share of firms that received PPP loans and will refer to as the takeup rate. We make use of loan-level data from the PPP on the size of each firm that received a PPP loan, along with publicly-available employment data from the Census Bureau's Statistics of U.S. Businesses (SUSB). SUSB data provide total employment for a number of categories of firm size which we use to form the rows of Table 1. For each size category, the takeup rate is the ratio of the total number of employees at PPP-receiving firms from the PPP loan data divided by total employment from SUSB. For example, in the PPP loan data, in the size bin 10-49, there were 1.3 million first-draw loans to firms with a total of 2.14 million employees over 2020 and 2021. In the aggregate, the

SUSB data from 2018 (the latest available) report that there were 2.14 million employees in firms with between 10-49 workers; accounting for the growth of employment between 2018 and before the pandemic, aggregate employment between 10-49 was 2.17 million. Thus, the takeup rate in this group is $\frac{2.14m}{2.17m} = 99$ percent, as given in column 5. We note that these estimated takeup rates are constrained by significant data limitations in determining the set of firms eligible for a PPP loan, inaccuracies in the reporting of firm size in PPP loan-receipt data, the possibility of fraudulent loans, and other measurement issues. (See the online appendix for further details on the methodology underlying Table 1, as well as additional information on the subsequent analysis in this paper.)

Overall, we estimate that 94 percent of employers with fewer than 500 employees took up a PPP loan; consistent with this high takeup rate, the distribution of loan dollars is tightly in line with employment shares—compare columns (2) and (4). Indeed, the fact that the second tranche of PPP funding concluded without exhausting all available funds suggests that the program had achieved something close to saturation in its first five months of operation. While near-universal participation in a government program is not altogether surprising since the program in most cases constituted a pure cash transfer, it is nevertheless a substantial administrative accomplishment: merely handing out $500 billion dollars in two months takes many hands. As noted above, this accomplishment would likely have been infeasible had Congress not authorized the Small Business Administration to enlist the private banking sector to issue PPP loans.

The early rollout of the Paycheck Protection Program in April and May 2020 did, however, stumble on two hurdles. First, initial demand for loans significantly exceeded the ability of banks to deliver them. In the face of these capacity constraints, banks appear to have prioritized firms with which they had a pre-existing relationship (Amiram and Rabetti, 2020; Cororaton and Rosen, 2021; Joaquim and Netto, 2021; Granja et al., 2020; Li and Strahan, 2020). Larger firms, which tend to have ongoing banking relationships, accessed PPP funds sooner than smaller firms on average. Moreover, as most small business lending is sourced from local banks (Brevoort et al., 2010), the aptitude and willingness of local banks to process loan applications generated significant geographic heterogeneity in the initial distribution of loans (Bartik et al., 2021; Li and Strahan, 2020).

The second hurdle was the significant uncertainty and confusion among businesses and banks over the specifics of the program, particularly over whether the loans would be forgiven. For exam-

Table 1: PPP Loans by Employer Size

Employer size	Employment Share	Loan $ (billions)	Share of $	Takeup rate	% of $ received in 2020
(1)	(2)	(3)	(4)	(5)	(6)
		A. First Draw Loans			
1-4	10%	44	9%	81%	84%
5-9	11%	54	11%	98%	93%
10-49	35%	182	36%	99%	96%
50-149	23%	122	24%	97%	98%
150-299	13%	64	13%	91%	98%
300-499	9%	40	8%	87%	97%
1-499	100%	505	100%	94%	93%
Memo:					
Non-employers	--	43	--	--	25%
Employers 500+	--	18	--	--	93%
		B. Second Draw Loans			
1-4	10%	17	9%	30%	--
5-9	11%	25	13%	43%	--
10-49	35%	87	46%	45%	--
50-149	23%	45	24%	34%	--
150-299	13%	14	8%	29%	--
300-499	9%	1	0%	3%	--
1-499	100%	189	100%	34%	--
Memo:					
Non-employers	--	11	--	--	--
Employers 500+	--	0.2	--	--	--

Note. Panels A and B reflect data on employer businesses. The main panels exclude loans to the self-employed, sole proprietors, independent contractors, and single-member LLCs with only one reported job because non-employers are excluded from the SUSB data used to calculate the denominator of the takeup rates displayed in column (5). The roughly 4.6 million non-employer loans (constituting about 8 percent of total loan dollars) are reported in the first memo lines of each panel. As PPP loan-level data censor firm size at 500, in the main panels of the table we restrict attention to loans to businesses smaller than 500; loans to businesses reported as having 500 employees in the PPP loan-level data are reported in the second memo line of each panel. Loans to businesses in Guam, Puerto Rico, and the Virgin Islands are excluded. Loans to businesses in the following NAICS industries are excluded as they are out of scope for the SUSB data used in columns (2) and (5): 111, 112, 482, 491, 525110, 525120, 525190, 525920, 541120, 814, and 92.
Source. Authors' analysis of Census Bureau Statistics of U.S. Businesses (SUSB) 2018, BLS BED, and SBA PPP data.

ple, in April 2020, the Small Business Administration announced that publicly traded companies were unlikely to satisfy the required good faith certification of need for a loan from the Paycheck Protection Program and stipulated a time window in which firms could return loans. Simultaneously, the Treasury Department announced that loans in excess of $2 million would be subject to review and warned of possible criminal charges for those who failed the review. These issues were resolved over the course of several months. By the second round of funding, confusion about eligibility and forgiveness terms had abated. Meanwhile, initially under-performing banks upped their loan tempo, and non-banks stepped in to fill gaps in local loan provision (Granja et al., 2020; Erel and Liebersohn, 2020). By July 2020, virtually all firms that would access a PPP loan in 2020 had done so.

The delay in delivering funds in April and May 2020 had real consequences. Doniger and Kay (2021) and Kurmann et al. (2021) find that loans received even a little earlier had a more pronounced effect on employment than those issued a bit later. Meanwhile, as we show below, the third tranche of loans, which did not go out until 2021, had no discernible effect on employment, perhaps because this tranche was issued when the labor market was already rapidly recovering.

Targeting

The rapid, near-universal takeup of Paycheck Protection Program loans in 2020 is inseparable from the reality that the program was essentially untargeted. That takeup was around 94 percent of *all* small businesses means that loans reached the most and least distressed firms—and all those in between—in nearly equal proportions. This observation helps to explain why there is little geographic correlation between the size of the initial COVID local economic shock, prior to PPP's passage, and subsequent PPP participation (Granja et al., 2020).

Around $200 billion in so-called second draw loans were issued in 2021—see column (3) of Table 1, Panel B. Unlike the first two tranches of PPP funds, these loans were explicitly targeted at firms that had experienced significant revenue losses over the course of the pandemic (and had already received a first PPP loan). We find a much higher correlation between PPP loan volumes and state-level employment declines for loans issued in 2021 than those issued in 2020 (see online appendix Figure B.1), suggesting that this targeting was more than nominal. Nevertheless these loans do not appear to have boosted employment, as we show below.

What Did the Paycheck Protection Program Accomplish?

Supporting Employment

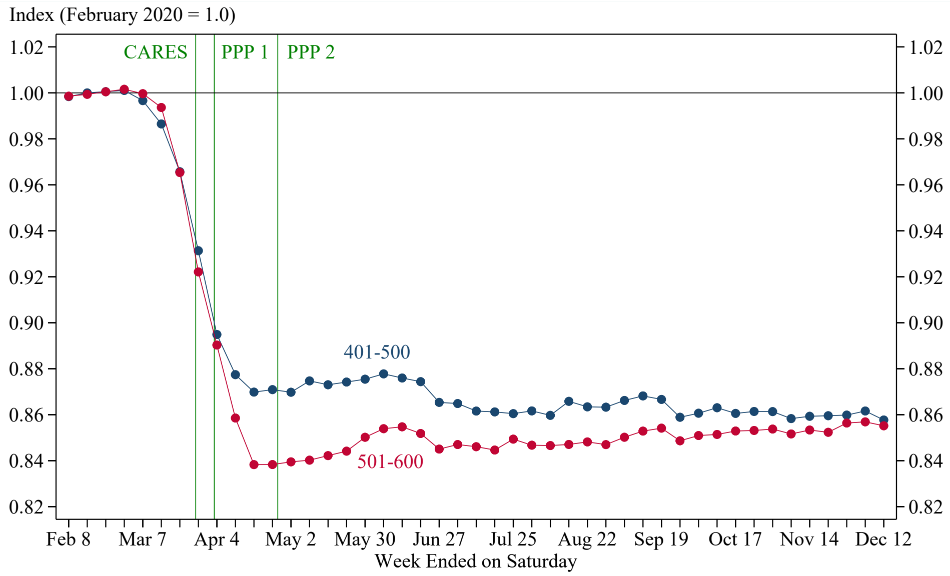
A first step in calculating where the PPP money went is to determine what fraction of Paycheck Protection Program funding went to paychecks that would otherwise not have been paid. Because PPP was ultimately taken up by almost all small businesses, we lack an ideal control group for making experimental comparisons. Nevertheless, a burgeoning literature, our own analysis included, indicates that PPP substantially boosted payroll employment.

The simplest and arguably most credible—though not necessarily most complete—method to assess the employment effects of the Paycheck Protection Plan is to compare the trajectory of employment at firms below the 500-employee initial-eligibility threshold to employment at ineligible firms above this threshold during the course of the pandemic. Figure 1—which is similar to our analysis in Autor et al. (2020)—presents this comparison using ADP payroll data. Employment is indexed to each firm's average level of employment in February 2020 (immediately before the pandemic) for two employment size classes: 401-500 employees (in blue) and 501-600 employees (in red). Employment declines in parallel for these groups of firms at the start of the crisis. Following the launch of PPP, these trends diverge, with employment at firms that are likely eligible for PPP loans (401-500 employees) falling by substantially less than employment at firms that are likely ineligible (501-600). Approximately a month after the start of the PPP, employment had fallen by approximately 4 percent less at likely-eligible firms than at likely-ineligible firms. In the months thereafter, employment levels relative to baseline at likely-eligible and likely-ineligible firms gradually converged, with the difference falling to less than 2 percent by the start of July 2020. It disappeared altogether by September of 2020.

Our formal econometric analysis of the employment effects of the Paycheck Protection Program in Autor et al. (2020) exploits this comparison of firms above versus below the size eligibility threshold, while additionally controlling for the differential impact of the pandemic across industries and states. After accounting for the fact that not all eligible firms received a loan, particularly in the initial months of the program, we estimate that taking out a PPP loan boosted firm employment by between 4 and 10 percent in mid-May and by 0 to 6 percent by the end of the year.[2] Our best evidence is that about 2.97 million jobs per week were preserved by the Paycheck Protection Program in the second quarter of 2020, and 1.75 million jobs per week were preserved in the fourth quarter. Chetty et al. (2020) and Hubbard and Strain (2020) conduct similar analysis exploiting the eligibility size threshold, using non-ADP data sources, and reach broadly similar conclusions. Assuming that the employment effect declines linearly from its peak in May 2020 to zero by June

[2] Adjusting for incomplete takeup means rescaling our Intent-to-Treat (ITT) estimates by the takeup rate to obtain Treatment-on-the-Treated estimates (TOT).

Figure 1: Employment by Firm Size for Industries With PPP Eligibility at 500 Workers



Note: Each series represents average employment for firms with that particular range of workers during 2019 (on average) and in February 2020. Data are weighted by each firm's employment as of February 2020. Sample reflects firms that were present in the ADP data for all 12 months of 2019.
Source: Author's calculations using ADP data.

2021 implies that PPP saved 1.98 million worker years of employment at the very substantial cost of $258,000 per worker-year retained.

These estimates based on eligibility thresholds are subject to an important caveat: because they focus on firms just above and below the 500 employee size-eligibility threshold for PPP loans, they may not capture the effect of such loans on smaller firms. If smaller firms are more liquidity constrained and hence more likely to shrink or shut down during the pandemic (Chodorow-Reich et al., 2021), the threshold-based estimates will likely underestimate the effects of PPP at these firms and, by implication, understate the full effect of PPP.

To develop causal effect estimates that cover a broader set of treated firms, a number of papers exploit an event-study approach that compares employment at firms receiving a loan early in the program period to employment at firms receiving a loan later. This approach potentially captures the effect of PPP loans on small firms that are well below the eligibility threshold, though it comes at a cost of focusing only on the early months of the program, before most firms had taken loans.[3]

[3]Papers using the event-study approach obtain a range of employment effect estimates. The first to employ this approach, Granja et al. (2020), finds aggregate employment effects that are comparable to those found by the eligibility threshold papers. Estimates in Li and Strahan (2020) imply a much smaller boost to employment, however, while those in Bartik et al. (2021), Doniger and Kay (2021), Faulkender et al. (2020), and Kurmann et al. (2021) point toward a larger employment effect.

We complement existing event-study estimates using the vast ADP database, which offers substantial precision and a sample frame identical to that used for the size-threshold analysis above. To implement the event-study using timing of loan takeup, we merge PPP loan-level data from the Small Business Administration into our sample of employers from ADP. This provides the precise date of PPP loan approval for each matched firm within a sizeable sample of firms with fewer than 50 employees.

Figure 2 presents our timing-based estimates which trace out the effect of receipt of a Paycheck Protection Program loan on employment at firms with fewer than 50 employees.[4] The employment trend prior to loan approval is roughly flat and about equal to zero, but begins rising on loan approval. Five weeks later, employment is roughly 12 percent higher, where it remains through the close of the outcome window. The relatively flat pre-trend centered around zero, and the sharp upward break after approval, are consistent with the interpretation that we are detecting a causal effect of PPP loans on small firm employment. We emphasize that these results indicate that small firms shrank *relatively* less after receiving a PPP loan as compared to firms not yet receiving a loan—not that their employment rose during the pandemic. The fact that the estimated effect on small firm employment is roughly *twice* as large as what we estimate for larger firms supports the view that smaller firms received a bigger employment boost from PPP.

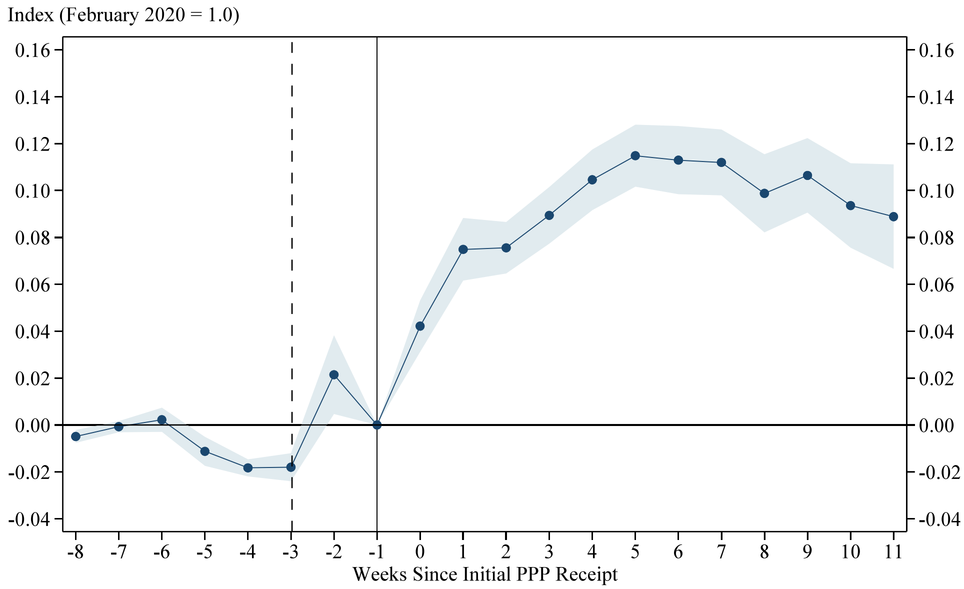
Combining the results from Autor et al. (2020) for larger firms with the smaller firm results in Figure 2, we estimate that PPP loans originated in 2020 preserved about 3.0 million job-years at an average cost of $169.3K per job-year saved. We use this result below when calculating the share of PPP funds that accrued to paychecks.

[4] A rapidly growing literature—e.g. Goodman-Bacon (2021); Callaway and Sant'Anna (2020); Sun and Abraham (2020)—highlights the problems that arise in event-study estimates when the magnitude of the treatment effect is correlated with the timing of treatment. We resolve this issue using the approach proposed by Sun and Abraham (2020): we estimate and then average separate treatment effects for each of the first eleven cohorts of borrowers, where cohort refers to week of loan issuance, while using the final seven cohorts as a comparison group. We choose the final seven cohorts to ensure a sufficient sample size. Using only those firms receiving a PPP loan in the final week of the program yields qualitatively similar results. We estimate the following specification:

$$y_{it} = \alpha + \sum_{c \in T} \sum_{g=-8}^{11} (\beta_{c,g} * PPP_{g,it}) * D_c + \theta_{jt} + \theta_{st} + \epsilon_{it} \tag{1}$$

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). For additional details of how we implement the equation, see the online appendix.

Figure 2: Event-Study Employment Effects at Firms Sized 1 - 49

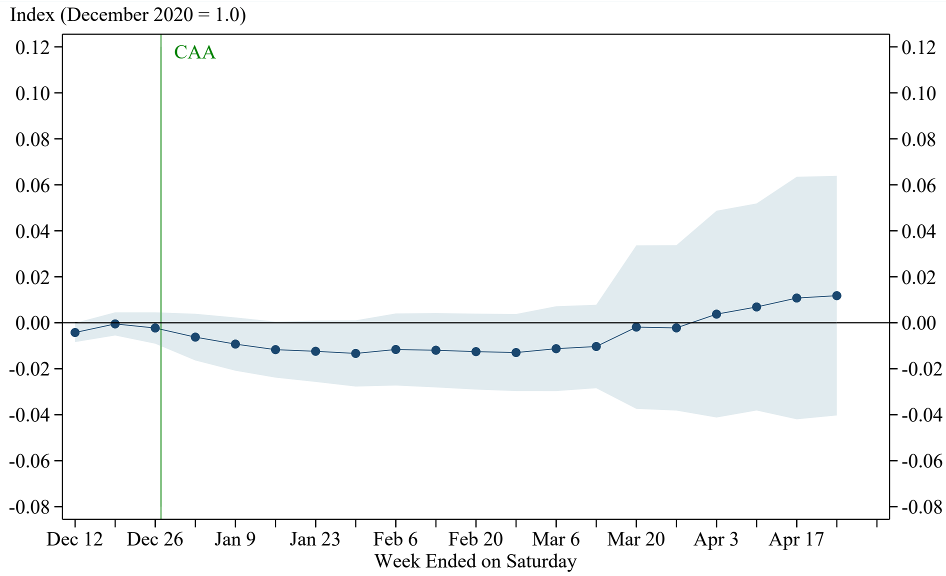


Note. Estimates from Sun and Abraham (2020) event-study interaction estimator on the sample of loan-matched ADP firms with between 1-49 employees where firm size is defined using the average size in February 2020. The outcome variable—firm-level employment—is indexed to equal 1 in February 2020. The 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 online appendix section D.4 for more details.
Source: Authors' analysis of SBA and ADP data using Sun and Abraham (2020) "eventstudyinteract" STATA implementation.

While our findings in Figures 1 and 2 capture the employment effects of loans issued in 2020 from the first two tranches of PPP funding, we know of no similar evidence on the consequences of the third major tranche of $278 billion in PPP loans issued in 2021. To complete this picture, we estimate difference-in-difference threshold eligibility results analogous to those in Autor et al. (2020) for the second draw PPP loans which constituted the majority of third tranche loans issued in 2021 (comparing employment at firms above and below the 300 worker eligibility threshold for second draw loans).

Despite seemingly better targeting than the 2020 loans, we find no evidence in Figure 3 that the 2021 second-draw loans boosted employment, perhaps because they were issued too late to be relevant, after the economic recovery was well underway. If this interpretation is correct, it affirms that Congress was wise to prioritize speed over precision in dispatching the initial two tranches of PPP loans.

Figure 3: Diff-in-Diff Eligibility Threshold Employment Effect, Second Draw Loans in 2021



Note: Each firm's size is determined using average employment in 2019 and employment in February 2020, the same size measure as used in Autor et al. (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. The specification is analgous to the difference-in-difference eligibility threshold specification in Autor et al. (2020); treatment and control groups are based on being within a range of 100 below and above, respectively, the 300 eligibility threshold for second draw loans. See the online appendix for additional discussion.
Source: Authors' analysis of ADP data.

Preventing Firm Exits

The spike in business closings during the COVID pandemic was historic. The Business Employment Dynamics database collected by the Bureau of Labor Statistics finds that employment at closing firms, which hovered at about 1 million worker per quarter for the last three decades, spiked to 2.1 million workers in the second quarter of 2020. (Evidence in Crane et al. (2020) and Kurmann et al. (2021) corroborates these trends using a number of alternative indicators, including data from ADP.)

A key justification for the Paycheck Protection Program was to prevent a contagion of business closures that would cause longer-term economic damage (Hubbard and Strain, 2020). Business deaths—as distinct from business contractions and temporary closures—may potentially produce lasting economic harm not only by forcing the costly reallocation of physical capital, but also by permanently destroying worker-firm relationships and the associated match-specific capital (Farooq et al., 2020). Indeed, the prevalence of recall hires—as opposed to new hires—when firms rebound from contractions underscores the importance of match-specific capital to both employers and employees (Fujita and Moscarini, 2017, e.g.).

We can observe the importance of firm closures for employment losses during the pandemic in the ADP data. Figure 4 groups firms into size classes based on their February 2020 (pre-pandemic) employment and reports the share of their original employment that is lost due to firm shutdowns in each week between February and December of 2020. Shutdowns are heavily concentrated among small firms: fully 10 percent of employment at firms that had 1-50 employees in February of 2020 was lost due to shutdowns by early April of 2020. For firms with more than 50 workers, these losses were only one-tenth to one-third as large. (We note that the general upward slope of the series in this figure is expected since some fraction of firms inevitably closes each year.[5])

Figure 4 also offers tantalizing evidence that PPP may have inhibited firm closures or spurred reopenings. Among firms sized 1-50 and 51-100, firm shutdowns peaked shortly after PPP loans began flowing and rapidly reversed course thereafter. By June of 2020, the fraction of employment at small firms lost due to closure was only half as large as in April—meaning that many had reopened.

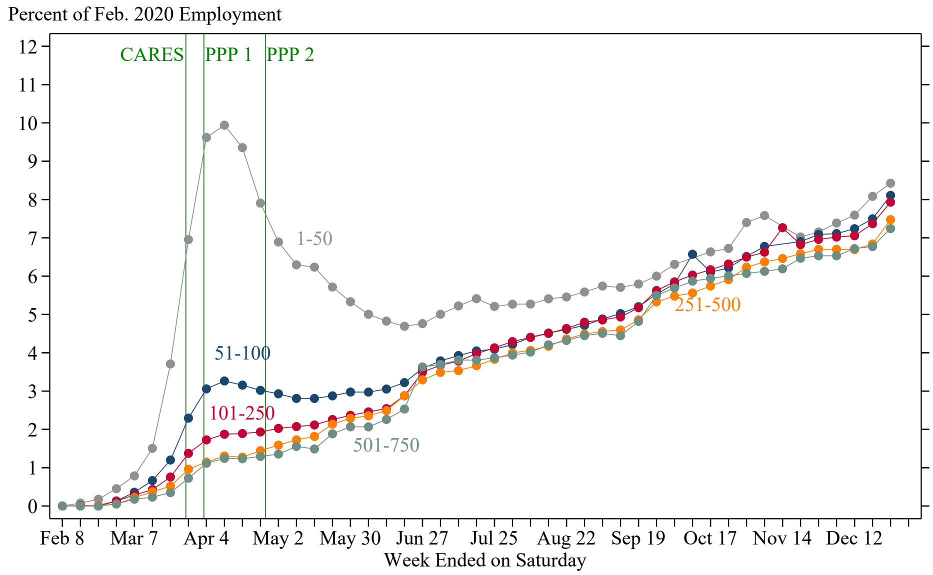
Following recent work by Dalton (2021), we test whether the receipt of a PPP loan affects the probability that firms with fewer than 50 employees remain open (or reopen after closure). Using event study estimates akin to those above for small-firm employment, we find in Figure 5 that PPP loans reduced employment losses due to small-firm closures by about eight percentage points five weeks after loan receipt. Since our earlier results in Figure 2 found a peak PPP effect on small firm employment of 12 percentage points at week five, we infer that about two-thirds of the employment-preserving effect of PPP loans on very small firm employment was due to PPP keeping the lights on at establishments that would have otherwise shuttered—at least temporarily.[6]

Ultimately, permanent business closure proved less pervasive than many had anticipated at the

[5]We define a firm as shutdown if it has no paid employment in a given week. Although we cannot definitively determine whether firms that appear to be shutdown in the ADP data have shuttered business or rather stopped utilizing ADP's payroll services, we expect that the *spike* in apparent shutdowns during the pandemic primarily reflects firms dropping to zero employment rather than discontinuing ADP's services.

[6]One anomaly in our results in Figure 5 is that the estimated employment effects of PPP receipt at small firms appear to start a week too early relative to loan receipt. A possible explanation is that a large fraction of ADP paycheck recipients are paid biweekly, and this payment scheme blurs the observable timing of any discrete event over the prior 13 days. Concretely, imagine that a firm's two-week pay period begins July 17 and ends July 31. After receiving a PPP loan, that firm reopens its doors on July 30th. In our estimation, this firm will show an employment jump on July 17, even though all of its hires occurred 13 days later, when the loan was issued.

Figure 4: Share of February 2020 Employment at a Firm with Zero Employment



Note: Each series represents the size-weighted share of firms with zero employment in a given week, using February 2020 employment as the weight. Sample reflects firms that were present in the ADP data for all 12 months of 2019, and excludes firms in NAICS 72.
Source: Authors' analysis of ADP data.

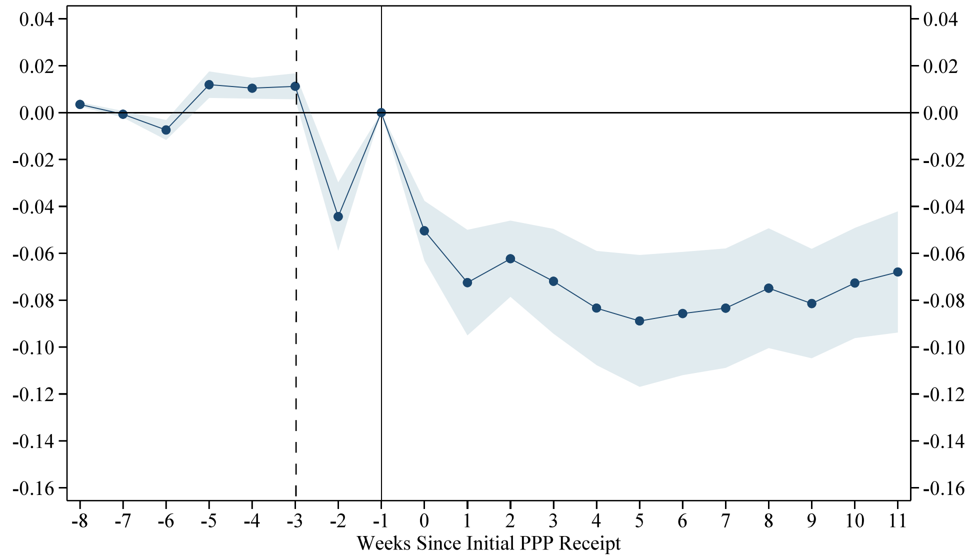
pandemic's onset. The Paycheck Protection Program may be part of the reason. Because our methodology permits examining firm closures only over the short run, we cannot assess whether PPP averted permanent firm exits or mainly temporary closures. Using a related methodology, Dalton (2021) finds that the PPP effect on small-firm closures waned somewhat over the ensuing seven months, indicating that some of the PPP effect on closure was temporary, not permanent, in nature. For larger firms around the 500 employee eligibility threshold, we find no consistent evidence that the PPP influenced shutdowns, either over the short or longer-term (see online appendix figure E.1). Despite bolstering jobs during the pandemic, PPP may not have had a pronounced effect on preserving intangible business capital. More work is needed to definitively assess the effect of the PPP on permanent business closure.[7]

Reducing Commercial Delinquency

Alongside preserving jobs and keeping firms open during the pandemic, PPP may have indirectly benefited creditors of small businesses—landlords, banks, holders of mortgage-backed securities,

[7] Other recent work provides mixed evidence on these outcomes. Granja et al. (2020) find little evidence of a PPP effect on firm shutdown. Bartik et al. (2021) and Kurmann et al. (2021), though, find that PPP mitigated business shutdowns.

Figure 5: Employment Change Due to Firm Closure, Firms Sized 1 - 49



Note. Estimates from Sun and Abraham (2020) event-study estimator on the sample of loan-matched ADP firms with between 1-49 employees where firm size is defined using the average size in February 2020. The analysis is implemented by setting y_{it} in the equation in footnote 4 equal to one if firm i has zero employment in week t and zero if it has positive employment. The specification includes controls for 3-digit industry-by-week and state-by-week fixed effects. Standard errors are clustered at the 3-digit industry. The β_t vector is displayed in the figure and can be interpreted as the percentage point effect of the PPP on employment due to firm closure as the specification is weighted by pre-pandemic firm employment. 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 online appendix Section D.4 for more details.
Source: Authors' analysis of SBA and ADP data using Sun and Abraham (2020) "eventstudyinteract" STATA implementation.

suppliers, etc.—by keeping payments flowing. There is limited evidence on the effect of PPP on loan recipients' ability to pay creditors, but the evidence that exists suggests the impact was positive. Exploiting differences in the tendency of small versus large commercial properties to have PPP-eligible tenants, Agarwal et al. (2021) find that PPP significantly blunted the rise in commercial mortgage delinquency rates during the pandemic, particularly in the retail sector. Using survey data and a variant of the event-study strategy discussed above, Granja et al. (2020) also find that PPP decreased delinquency on mortgages and other payments.

Where Did the Money Go?

The estimates above provide a key input for answering our motivating question, i.e., where did the money go? Using the employment effects estimated above, along with many other data sources, we estimate the incidence of the $510 billion in Paycheck Protection Program loans issued in 2020 across the household income distribution. We further compare this incidence to other pandemic economic assistance programs. Additional information on these calculations can be found in section F of the online appendix.

Proximate Recipients: Workers vs. Non-Workers

PPP funds were paid to businesses. In turn, businesses used these funds to pay three proximate groups of beneficiaries: workers who otherwise would have been laid off; creditors and suppliers who otherwise would not have been paid; and owners and shareholders of PPP-receiving firms as residual claimants in cases where businesses would have met some or all of their payroll and other financial obligations absent PPP (AKA, windfall profits). The distribution of PPP funds among these groups—workers versus non-workers, in particular—matters for our accounting exercise because different groups represent different parts of the household income distribution.

We focus first on payments to workers. As documented above, PPP loans issued in 2020 modestly raised employment at recipient firms. To convert these employment effects into payroll expenditures, we use the main estimates reported above from Autor et al. (2020), who find that PPP boosted employment by about six percent in mid-May 2020, with effects tapering off gradually thereafter. These numbers imply that PPP preserved about 2.97 million jobs per week in the second quarter of 2020 and about 1.75 million jobs per week by the fourth quarter of 2020. Assuming a linear trend decline in this program effect, PPP would have had zero employment effects by June 2021. Converting these weekly job numbers into job-years (that is, one worker for one year), implies that PPP preserved about 1.98 million job-years of employment at a cost of \$258K per job-year saved (i.e., \$510B/1.98M). We assume that actual employee compensation for each saved job averaged \$58,200 since the average weekly wage from the Current Population Survey in February 2020 is \$786 (truncating at an annual wage of \$100,000 above which the PPP did not provide additional support per worker) and, on average, total compensation is 42% larger than wages according to BLS Employer Costs for Employee Compensation data (\$786 $\times$ 52 $\times$ 1.42). The 1.98 million job-years saved then imply that \$115 billion in PPP loans (\$58,200 $\times$ 1.98m) accrued to employee paychecks.

We also produce an alternative estimate of the amount of PPP loans accruing to compensation based on our 3.0 million job-years saved estimate which combines the results from Autor et al. (2020) and the larger effects for smaller firms in Figure 2. Continuing to assume that compensation at retained jobs averaged \$58,200 implies that \$175 billion in PPP compensation went to paychecks. It is likely that this \$175 billion estimate is an upper bound on the share of PPP funds flowing to worker compensation. Some event-study estimates for the *entire* size distribution of PPP-eligible firms, including small firms, find an overall peak PPP employment effect of approximately six to eight percent (see online Appendix Figure D.1 and those of Dalton (2021)). These estimates are more in line with our smaller \$115 billion estimate. Additionally, our assumption of a smooth trend decline in PPP's impact through June of 2021 is generous.[8] Moreover, we are not accounting for loans issued in 2021 where our evidence suggests the PPP failed to boost employment; doing so would further lower the estimated share of PPP loans that flowed to workers relative to non-workers.

These bounds of \$115 to \$175 billion in PPP funds accruing directly to paychecks imply that between 23% and 34% of the first two tranches of PPP dollars totalling \$510 billion supported jobs that would otherwise have been lost. By implication, the remaining \$335 to \$395 billion (66 to 77 percent) accrued to owners of business and corporate stakeholders, including creditors and suppliers, etc.

The Household Distributional Incidence of PPP

To trace the flow of PPP payments from their proximate recipients to their household incidence requires information on the income distributions of both worker and non-worker (i.e., owner) beneficiaries. Starting with the worker beneficiaries, we estimated above that, at the high end, \$175 billion in PPP money flowed to workers whose jobs were saved. We assume that the distributional incidence of those funds followed the distribution of job loss in 2020 by household income quintile.

To perform this calculation, we first measure employment declines across the weekly wage distribution using the Current Population Survey Outgoing Rotation Group (CPS ORG) files. Pandemic job losses were largest for low-paid workers: total employment fell by 17.8 percent from March 2020 through the end of 2020 among the lowest-paid (1st) quintile of workers; by 10.6 percent in the second quintile; by 6.0 and 2.2 percent in the third and fourth quintiles, respectively; and by a substantial 8.7 percent among the highest quintile of earners. We convert these job loss percentages into average weekly wage losses by multiplying them each by the February 2020 pre-COVID average

[8] In Autor et al. (2020), we detect no statistically significant impact of PPP on employment after July 2020. But because the point estimates remain non-zero through December 2020, we extrapolate the entire series out until it is numerically zero in June of 2021.

weekly wage within quintile. From there, it is straightforward to calculate the share of compensation lost by weekly wage quintile, which we impute to the household income distribution using March CPS data on the joint distribution of weekly wages and household income.[9]

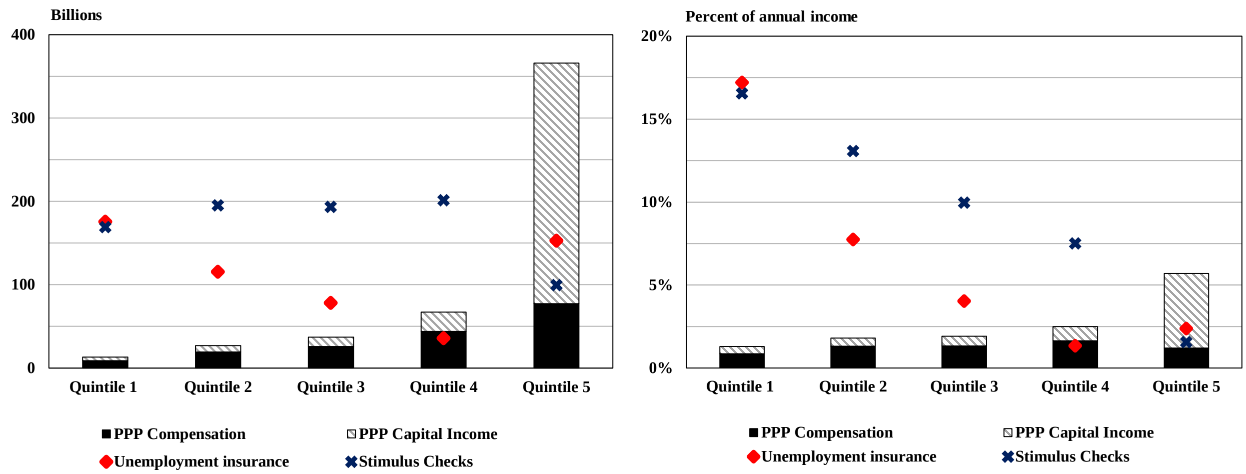
We make an analogous (but simpler) imputation for the household incidence of the $335 billion in PPP fund payments that flowed to non-worker beneficiaries, i.e. creditors and suppliers who otherwise would not have been paid and owners and shareholders of PPP-receiving firms. Specifically, we use the Congressional Budget Office's most recent estimates on the distribution of capital incomes by type (CBO, 2020a) to distribute the funds across households.We do not attempt to account for the flow of PPP funds from proximate and subsequent recipients, e.g., a PPP-receiving firm's supplier pays its workers; a worker at a PPP-receiving firm pays her landlord. Thus, our exercise is in the spirit of the static distributional incidence analyses performed for tax policies by the Joint Committee on Taxation.

Unlike PPP, which went to businesses, transfer payments made by the two other major federal pandemic emergency assistance programs—pandemic unemployment insurance payments and household stimulus payments—went directly to households and workers. The size of these payments rivaled those of PPP, as noted above. To facilitate comparison with PPP, we calculate the distributional incidence of these as well.

For household payments, we use incidence data from Bhutta et al. (2020), who analyzed the effect of stimulus payments on household finances using the Survey of Consumer Finances. For UI, we calculate approximate shares of benefits paid during the pandemic—including regular state programs and the pandemic enhancements to UI—using, as a starting point, our estimates of average wages lost during the pandemic, and applying the methodology above for apportioning the PPP funds to paychecks. We combine these data on wages lost by quintile with simple estimates of the UI replacement rate by quintile, which we estimate using CPS ORG data. This calculation accounts for both pandemic supplements to weekly UI benefits and the portion of Pandemic Unemployment Assistance payments that went to the self-employed, as estimated by Boesch et al. (2021).

[9] Specifically, we calculate $S_q \equiv T \times \frac{U_q \times W_q}{\sum_{q=1}^{5} U_q \times W_q}$, where T is total PPP dollars that support employment, U_q is quintile $q's$ share of job losses during the pandemic, and W_q is quintile $q's$ wage in February 2020 prior to the pandemic. We can then map from the *weekly wage* distribution to the *household income* distribution using March CPS supplement data on the probability that a worker in a given weekly wage quintile is in each household income quintile.

Figure 6: Distribution of PPP, UI, and Stimulus Check Payments



Note. See online appendix for details of calculations. Calculations based on our upper-end (most generous) estimate of the wage compensation supported by PPP. Online appendix Figure reports the same calculations using our lower-end estimate of the wage compensation supported by PPP.
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).

Panel A of Figure 6 reports the distribution of PPP, UI, and household payments in billions of dollars across household quintiles. The distribution of Paycheck Protection Program loans overwhelmingly accrued to high-income households. Of the $510 billion in PPP loans provided in 2020, we estimate that only $13.2 billion ultimately flowed to households in the bottom fifth of the income distribution, and that $130.8 billion to the second through fourth quintiles. The remaining $365.9 billion (72 percent) flowed to the top-fifth of household income. This skew reflects two features of PPP. First, high-wage earners are found in high-income households. Though the PPP only offered loans to support up to $100,000 in annual earnings, even with this truncation, the top fifth of households account for about 35 percent of wage and salary earnings. Second, the distribution of capital ownership is even more right-skewed than the distribution of wage earnings—with the top fifth of households commanding 86.2 percent of capital income—meaning that subsidies to businesses are ultimately subsidies to high-income households.

In comparison, both household stimulus payments and pandemic unemployment insurance payments were far less regressive than PPP. The incidence of household stimulus checks in dollar terms was close to uniform across the lower four income quintiles. Moreover, due to the income caps that Congress set on household payments, the incidence of these payments was much smaller for the highest quintile of households.

Meanwhile, the incidence of unemployment insurance during the pandemic was weighted towards both the upper and lower tails of the household income distribution. We estimate that 31.5 percent ($175.6 billion) and 20.6 percent ($115.5 billion) in pandemic unemployment insurance payments went to the bottom fifth and second-to-bottom fifth of households, respectively (red diamonds in panel A of Figure 6). Surprisingly, the top fifth of households received a bit more than one-quarter of unemployment insurance benefits. This occurred both because the highest income quintile of wage and salary workers sustained substantial employment losses during the pandemic (as documented above), and because the Pandemic Unemployment Assistance (PUA) program allowed self-employed business owners—who tend to have high incomes—to collect unemployment insurance benefits, with estimates from Boesch et al. (2021) suggesting that self-employed business owners received about 40 percent of Pandemic Unemployment Assistance insurance benefits.

Panel B of Figure 6 recasts these distributional incidence figures into household annual income replacement rates rather than dollar transfers. Both stimulus checks and unemployment insurance payments replaced about 17% of the incomes of the lowest quintile of households, with much lower shares at higher quintiles. Thus, although the combination of these three programs is highly regressive in dollar terms, it is roughly progressive in replacement rate terms due to the highly skewed distribution of U.S. household incomes.

Macroeconomic Benefits

An additional benefit of these transfers programs is that they provided stimulus during a time of rapid economic contraction. The short-term macroeconomic boost of a program during a recessionary period is conventionally linked to the marginal propensity to consume (MPC) of those who receive benefits from the program. Cashin et al. (2018) provide estimates of the marginal propensity to consume for different types of fiscal shocks based on characteristics such as: the type of policy change (say, tax versus transfer payment); who is receiving the benefit (say, low-income households versus corporations); and whether the flow of benefits is temporary or permanent. These MPC estimates are informed by a publicly available macroeconomic model, FRB/US, used by the Federal Reserve Board staff (described in Brayton et al. (2014)), and by the relevant empirical literature.

Using these MPC estimates, we offer a back-of-the-envelope comparison of the degree of stimulus provided by the three main programs mentioned above: the Payment Protection Program, stimulus payments, and pandemic-enhanced unemployment insurance. This calculation relies on the following assumptions:

1. Since unemployment insurance payments are generally made to households that are highly liquidity-constrained, the marginal propensity to consume out of unemployment insurance payments is one (Cashin et al., 2018).

2. Because stimulus payments are made to a broad mixture of households across the income distribution, we use the estimate from Cashin et al. (2018) for the MPC of general, temporary transfers to households of 0.5.

3. The part of the Paycheck Protection Program that flows through to wages is similar to unemployment insurance, and thus has a plausible marginal propensity to consume of one.

4. For the part of PPP that flows to non-workers, we use the estimates in Cashin et al. (2018) for the MPC of temporary corporate tax cuts of 0.2; this relatively low marginal propensity to consume is consistent with these funds flowing disproportionately to the upper quintile of the income distribution.

Weighting these components together, we obtain an overall marginal propensity to consume out of PPP loans of about 0.5, which is comparable to stimulus checks (where we have an imputed MPC of 0.5) and much lower than unemployment insurance payments (where we have imputed an MPC of one).[10] This illustrative calculation thus suggests the PPP loans and stimulus checks were roughly equally effective at boosting spending, and both were much less effective on this margin than pandemic unemployment insurance.

These estimates have the virtue of transparency. They also have shortcomings. The pandemic environment surely generated non-normal household and business behavior. Extraordinarily high replacement rates delivered by enhanced unemployment benefits may have diminished the MPC of recipients. The substantial share of PPP payroll income received by the top quintile also suggests that treating this income as similar to unemployment insurance probably overstates the MPC.

[10] We noted in the previous section that a substantial share of PUA recipients were likely high income self-employed business owners who might be expected to have lower MPCs than the one we assume here for UI recipients. Nonetheless, even if we assumed an MPC of *zero* for self-employed PUA recipients, the overall MPC out of UI would be about 0.7 (since non-PUA benefits were about 70% of total UI), which is still higher than our estimates of the MPC out of PPP and stimulus checks.

Finally, these estimates quantify only the transfer's initial boost to aggregate demand; they do not capture aggregate supply effects, such as that arising from preventing firm bankruptcies, or subsequent general equilibrium effects. Fortunately, the Congressional Budget Office has also estimated the boost to GDP per dollar for these same pandemic programs, carefully accounting for the pandemic environment and the specifics of each program. CBO also strives to capture the full, general equilibrium effect of each program, including its potential impact on business closure. CBO concludes that the enhanced unemployment and stimulus checks were far more effective at boosting GDP than was PPP (CBO, 2020b). Specifically, the CBO estimates a per dollar boost to GDP of 0.36 for the PPP and 0.60 and 0.67 for stimulus checks and enhanced UI benefits, respectively. Taking account of the highly distributionally-skewed incidence of PPP payments, we concur that PPP was likely the least effective of the three programs in boosting the macroeconomy.

Lessons Learned from the Paycheck Protection Program Experience

The U.S. small business sector appeared at risk of collapse at the outset of the pandemic. To avert this collapse, Congress enacted the Paycheck Protection Program, which successfully distributed vast amounts of aid to the near-universe of eligible small businesses in the space of a few months. Our best evidence to date indicates that PPP's economic impacts were less than hoped: it preserved only a moderate number of jobs at a high cost per job-year retained and transferred resources overwhelmingly to the highest quintile of households.

These outcomes should not however be viewed first and foremost as programmatic failures. PPP's regressive distributional incidence and its limited efficacy as economic stimulus stem from the program's absence of targeting. This absence, in turn, reflected necessity. Given the time constraints and, more profoundly, the lack of existing administrative infrastructure for overseeing targeted federal support to the entire population of US small businesses at the onset of the pandemic, we strongly suspect that Congress could not have better targeted the Paycheck Protection Program without substantially slowing its delivery. We thus concur with Bartik et al. (2021) that policymakers made a defensible trade-off between speed and targeting in the PPP's design.

If however the PPP was a logical answer to a highly constrained question, a forward-looking lesson from the PPP experience is that the United States should invest now to relax those constraints. We have emphasized that the PPP had dual goals: preserving jobs and providing liquidity. These goals could be better served with the administrative capacity to address these issues directly and separately, thus enabling better targeting and a more progressive incidence. The primary job retention goal of the Paycheck Protection Program could in the future be better achieved through an expanded program to encourage "work-sharing," which refers to a policy in which employers, when faced with an economic downturn, are encouraged to reduce hours worked more broadly across the workforce rather than laying off a narrower group outright. In effect, the government program ends up paying partial unemployment to many, rather than full unemployment to some.

Currently, 26 US states have a work-sharing program through their unemployment insurance systems, but these were not well-subscribed where available during the COVID recession. A number of proposals over the last decade that advocate for expanded work-sharing suggest that to reach broader coverage, such programs should be simplified and automated (Abraham and Houseman, 2014; Strain, 2020; Dube, 2021). A work-sharing program can target firms of all sizes that are cutting hours or employment, not just small firms. Additionally, with sufficient administrative capacity developed in normal times, the progressivity of the program could be altered as policymakers deem appropriate.

A separate liquidity provision program could then be targeted primarily at small firms, which are more likely to be liquidity-constrained. Moreover, with better information systems operational, liquidity could be provided in proportion to firms' decline in revenues as well as firms' actual fixed obligations.

Distinct from the U.S., many other high-income countries responded to the pandemic with a mixture of job retention incentives, including: 1) work-sharing programs that allowed either partial or complete furloughs; 2) newly-introduced *wage subsidy* programs, similar in many ways to the Paycheck Protection Program, that provided businesses with direct support for at least some fraction of their wage bill (OECD, 2021). Both work-sharing and wage subsidy programs were targeted. Wage subsidy programs were explicitly targeted to firms that had experienced declines in revenue: for example, Canada's Employer Wage Subsidy was available to firms that experienced a year-over-year revenue drop of 30 percent (reduced to 10 percent later). In some countries, firms were entitled to wage subsidies on a sliding scale in proportion to their declines in revenues. By contrast, work-sharing programs were not *explicitly* targeted to distressed firms. But the requirement that firms reduce workers' hours to obtain assistance generally makes firm participation unattractive absent a negative shock (see Giupponi et al. (forthcoming) in this symposium). The details of these programs, such as the length of benefits and the extent to which non-payroll expenses are covered, as well as their efficacy, varies across countries, of course.

A key lesson from these cross-national comparisons is that targeted business support systems were feasible and rapidly scalable in other high-income countries because administrative systems for monitoring worker hours and topping up paychecks were already in place, prior to the pandemic. Lacking such systems, the United States chose to administer emergency aid using a fire hose rather than a fire extinguisher, with the predictable consequence that virtually the entire small business sector was doused with money. This approach may have been necessary, but it was desirable only because the U.S. lacked viable alternatives. By building administrative capacity in the years ahead, the United States could more deftly target, calibrate, and deploy its emergency business response systems when most needed.

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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

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 "procm = PPP" and second draw loans are "procm = 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 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 second draw loans—and state-level employment changes occurring between February

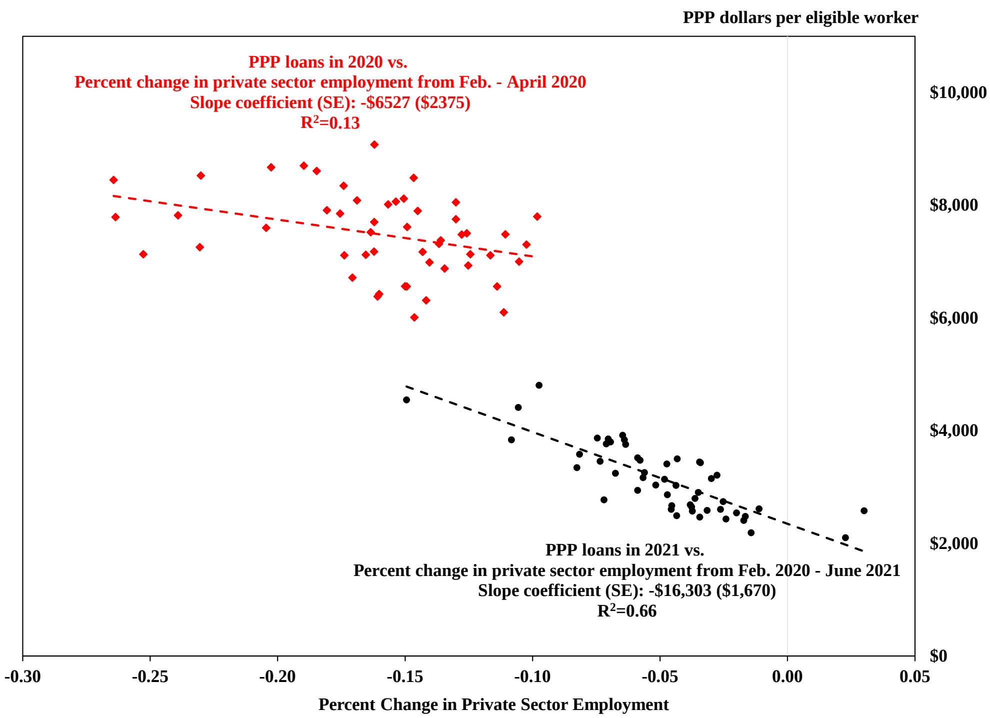
[1] About 75% of the $285 billion in third tranche funding went to second-draw loans for firms with under 300 employees that experienced significant revenue losses in 2020.

2020 and June 2021. The R-squared value of this bivariate regression is 0.66.

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. 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 SUSB, 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:

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

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.

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—see Figures D.1 and D.2, respectively. These estimates rely on first matching SBA PPP loan-level data into 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 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 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 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 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 on Figures 2 and 5 for firms sized 1-49 employees, but are smaller in magnitude.

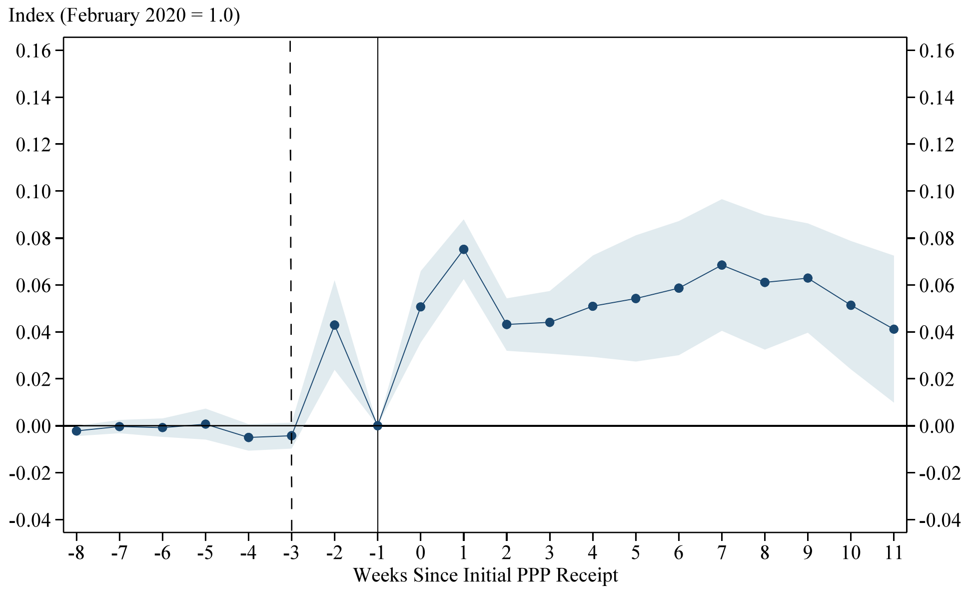
D.4 Event-study Timing

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.

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

[2]Note 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

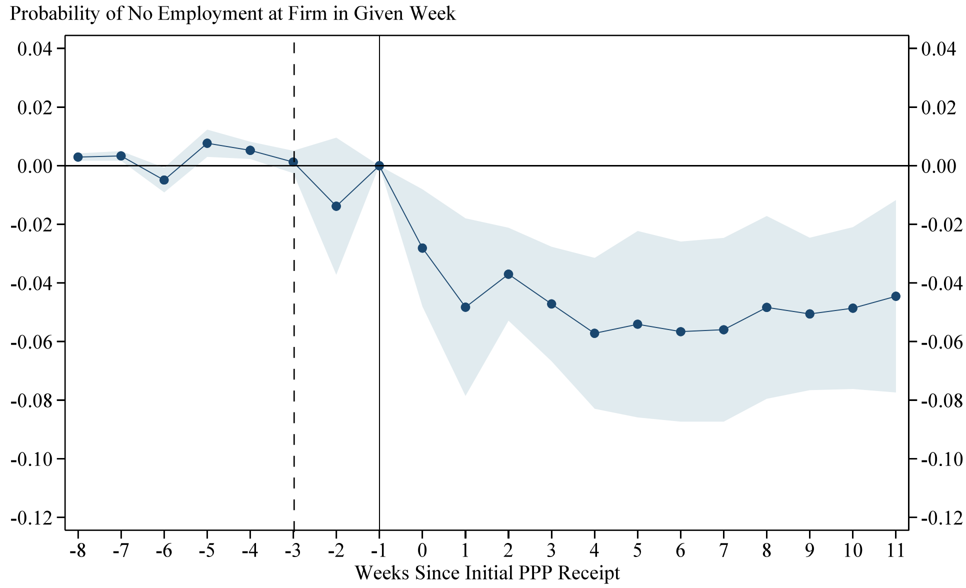


Note. Estimates from Sun and Abraham (2020) event-study interaction estimator on the sample of loan-matched ADP firms. The outcome variable is firm-level employment indexed to equal 1 in February 2020. The estimates are weighted by each firm's employment as of February 2020 and include controls for 3-digit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

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

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

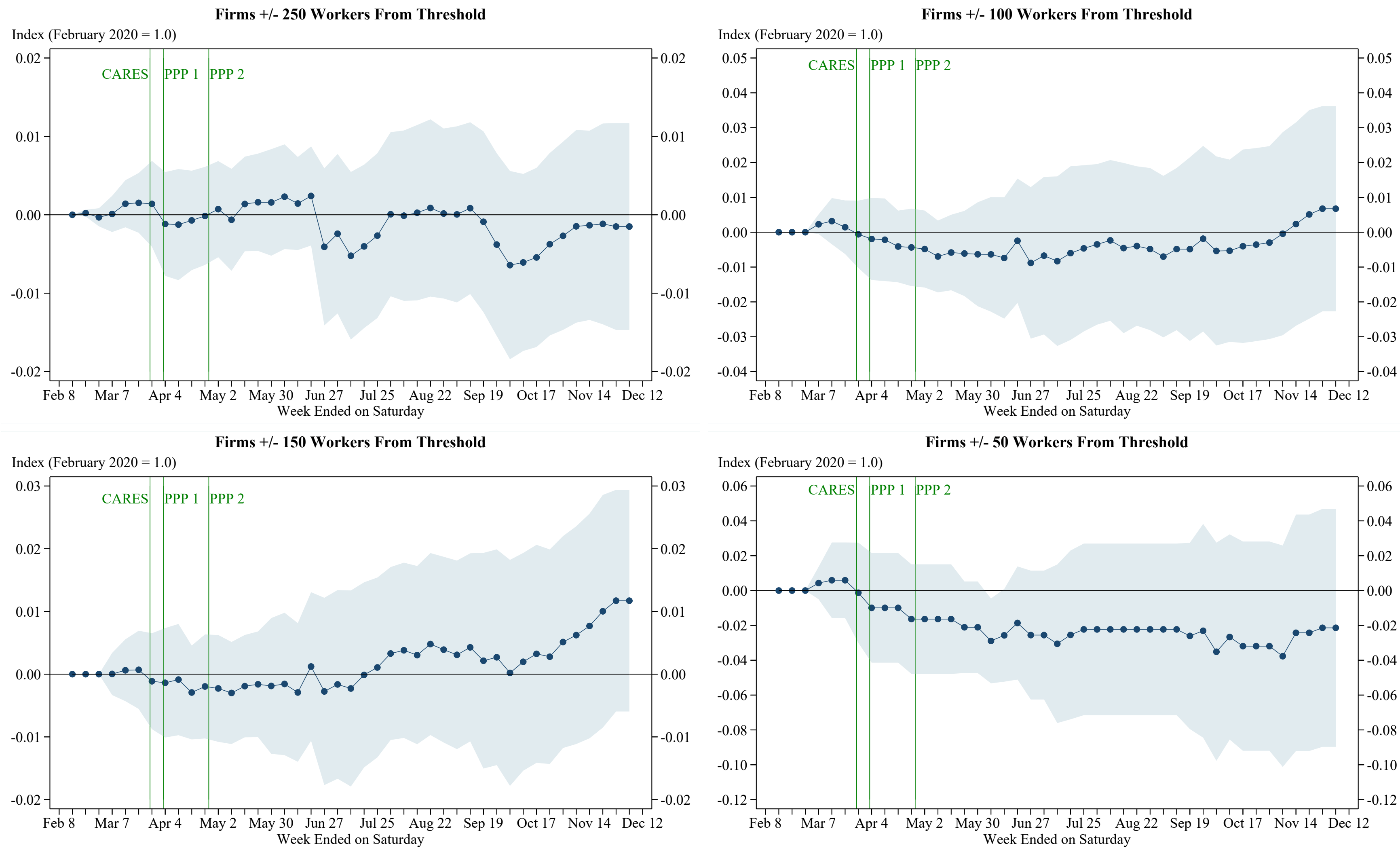


Note. Estimates from Sun and Abraham (2020) event-study interaction estimator on the sample of loan-matched ADP firms. The outcome variable is an indicator variable equal to one if the firm has zero employment in a given week and zero if it has positive employment. The estimates are weighted by each 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 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

This appendix section discusses the distributional 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 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 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.

- $T^{Autor} = CJ^{Autor} = \$58,185 \times 1.98m = \$115$ 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, 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 from 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 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: $rr_q = E\left[\frac{\min\{\overline{UI}_s, \max\{\underline{UI}_s, 50\% \times wk\}\}}{wk}\right]$, where $\underline{UI}_s$ 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: $rr_q = E\left[\frac{\min\{\overline{UI}_s, \max\{\underline{UI}_s, 50\% \times wk\}\}}{wk} + \frac{s}{wk}\right]$, 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 (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.

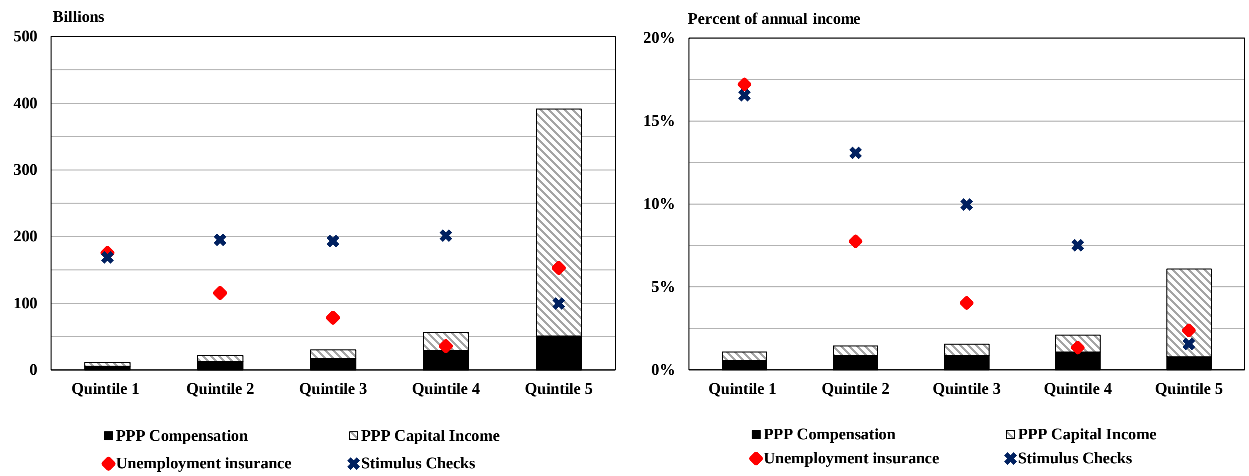
- To inflate these figures to pre-COVID levels, we use Census average household income growth by quintile from 2017-2019.

[3]We 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).