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July 2022

NBER WORKING PAPER SERIES

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Working Paper 29972
<http://www.nber.org/papers/w29972>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2022

We thank Christopher Kurz and Norman Morin from the Federal Reserve Board for their support, and Salome Baslandze, Simon Freyaldenhoven, Erik Hurst, Matthew Shapiro, Martin Sullivan, and Erick Zwick for helpful comments. We thank Kendra Robbins, Samantha Sharkoff, Eleanor Warren, and Lena Yemelyanov for excellent research assistance. David Autor thanks the following for financial support: Accenture LLP (#027843-0001), the Andrew Carnegie Fellowship (G-F-19-56882), the Smith Richardson Foundation (#20202252), and the Washington Center for Equitable Growth. The views expressed in the paper are the authors' and do not necessarily reflect the views of ADP or the National Bureau of Economic Research. Additionally, 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 or by the Board of Governors. The proprietary data for this paper were provided to the researchers at no cost by ADP, LLP, which approved the paper's topic ex-ante and reviewed the paper prior to distribution to ensure that the paper protected clients' confidentiality and did not reveal proprietary information about ADP's business model. Two co-authors, Goldar and Yildirmaz, were previously employed at the ADP Research Institute and were involved in making the data available to the outside researchers. ADP has shared the same data with researchers at Chicago, Princeton, and Cornell.

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NBER Working Paper No. 29972
April 2022
JEL No. E24,H25,H32,H81,J38

ABSTRACT

The Paycheck Protection Program (PPP), a principal element of the fiscal stimulus enacted by Congress during the COVID-19 pandemic, aimed to assist small businesses to maintain employment and wages during the crisis. We use high-frequency administrative payroll data from ADP--one of the world's largest payroll processing firms--to estimate the causal effect of the PPP on the evolution of employment at PPP-eligible firms relative to PPP-ineligible firms, where eligibility is determined by industry-specific firm-size cutoffs. We estimate that the PPP boosted employment at eligible firms by between 2 percent to 5 percent at its peak in mid-2020, with this effect waning to 0 to 3 percent throughout the remainder of the year. Employers retained an estimated additional 3.6 million jobs due to the PPP as of mid-May 2020, and 1.4 million jobs at the end of 2020. The implied cost per year of employment retained was \$169,000 to \$258,000, equal to 3.4 to 5.2 times median earnings.

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1 Introduction

The onset of the COVID-19 pandemic caused a dramatic plunge in U.S. economic activity, leading many small businesses to shut their doors and leaving many more in precarious financial condition (e.g. [Bartik et al., 2020a,b](#)). Anticipating further widespread hardship, Congress introduced the Paycheck Protection Program to provide forgivable loans to “small” businesses. Although the PPP had multiple goals, its primary aim was to support recipient firms to maintain employment at pre-pandemic levels. Hence Congress’s use of the word “paycheck” in the program name and its requirement that recipient firms spend the majority of PPP funds on wages to qualify for loan forgiveness. The program was economically large relative to the targeted sector: In its first year of operation, it issued forgivable loans totalling \$525 billion, roughly equal to the *entire* 10-week payroll of small businesses in the U.S.

This paper provides an assessment of the PPP’s efficacy in achieving its primary goal of sustaining small business employment. To provide a high-resolution picture of PPP’s effects, we analyze administrative data from ADP—one of the world’s largest providers of personnel management services, covering more than 25 million workers in the U.S. These data allow us to observe high-frequency, firm-level employment data at weekly intervals throughout the pandemic and to identify a set of firms that were eligible to receive PPP loans and a set that were not.

Our analysis uses a dynamic difference-in-difference framework to identify the effect of the PPP on employment. To form the treatment group, we focus on firms in a range below the industry-specific employment size thresholds that define eligibility for the program. The threshold is 500 employees for most industries, but not all. We compare these eligible firms to those in a range above the industry-specific threshold, which comprise the control group. To account for potential confounders stemming from rapidly evolving economic conditions across industries and states during the COVID crisis, our baseline results include a rich set of fixed effects, including three-digit NAICS industry-by-week and state-by-week fixed effects.

Our analysis finds that the PPP boosted employment at eligible firms, but that these

effects faded between the PPP’s implementation in the spring of 2020 and the end of the calendar year. Following the disbursement of the first tranche of PPP loans, employment at eligible firms began to rise relative to employment at ineligible firms. The peak effect on employment at eligible firms ranged between 2 and 5 percent around mid-May, depending on the specification, and waned gradually thereafter. By the end of our sample in the beginning of December 2020, the employment effect ranged from about 0 percent to about 3 percent. None of these December estimates, though, is precise enough to rule out that the PPP had no effect on employment at that time.

Additional steps are required to determine the aggregate employment effect of the PPP. We first translate the above intent-to-treat estimates—which contrast eligible vs. ineligible firms—into estimates of the effect of receiving a PPP loan. Doing so requires an estimate of the take-up rate of the PPP in the intervals around the eligibility threshold. Using data from the Small Business Administration (SBA) on PPP loans by firm size, as well as publicly-available data on the distribution of employment across firm size from the Census Bureau, we estimate that take-up for firms with between 300 to 499 workers was substantial—around 81%. We also find that there was non-trivial take up, approximately 27%, in the relevant range *above* the 500-worker threshold as some firms were eligible based on non-size criteria.

By scaling up our intent-to-treat estimates by the difference in take-up rates across the 500-worker threshold and applying them to the population of firms taking up PPP loans, we find that the PPP boosted aggregate U.S. employment by 3.6 million at its peak around mid-May and by 1.4 million at the beginning of December.

We estimate the PPP’s cost per worker retained under two different scenarios. In both scenarios, we extrapolate the trend decline in the estimated PPP treatment effect to the point where it reaches zero in mid-June 2021.

The first scenario relies on our baseline aggregate employment effect estimate. Integrating over treatment time—i.e. from early-April 2020 to mid-June 2021—we estimate that PPP expended approximately \$258,000 per full-year job retained, which is almost five times the

median full-time, full-year U.S. salary in 2020.

Most PPP loans were issued to smaller firms, however, and it is possible that the PPP boosted employment at these firms—which are more likely to be liquidity constrained—by more than it did at large firms. Since our estimates derive from firms in the vicinity of the eligibility thresholds of 500 workers, they may potentially understate these impacts on smaller firms. We take this caveat seriously under the second scenario by considering a hypothetical where the effect of the PPP for very small firms is double the local treatment effect we estimate here. In this more generous case, the estimated cost-per-job-saved by the PPP is \$169,000 (vs. \$258,000 above), or 3.4 times the median salary.

These high costs per job retained likely reflect the reality that the PPP program was designed to prioritize rapid aid disbursement over careful targeting ([Autor et al., forthcoming](#)). PPP was effectively available to all small businesses, and hence by nature did not target the firms most in need. One consequence was that a large share of PPP dollars appears to have gone to inframarginal firms that would have maintained employment in the absence of the PPP.¹

Drawing on the strengths of our data, our analysis focuses exclusively on the PPP’s effects on employment. We acknowledge however that a complete evaluation would include a broader set of outcomes, including business survival, loan delinquency, and potential general equilibrium effects on the broader macroeconomy. These broader consequences are discussed in [Hubbard and Strain \(2020\)](#) and [Autor et al. \(forthcoming\)](#).

Distinct from our threshold eligibility approach for identification, a number of recent papers have examined PPP employment effects by comparing firms receiving a PPP loan early in the program period to those receiving loans later, often exploiting variation in timing due to the varying tendency of local banks to quickly issue PPP loans. This timing approach is complementary to our threshold eligibility approach. The timing approach permits a direct analysis of the effect of the PPP on smaller firms. Conversely, our threshold

¹Corroborating this view, [Granja et al. \(2020\)](#) document that there was essentially no geographic correlation between the pre-PPP pandemic economic shock and PPP participation.

approach identifies the effect of the PPP using a well-defined, predetermined, pre-COVID firm characteristic: firm size. This is attractive relative to identification based on the timing of rollout, which arguably requires stronger identifying assumptions to interpret causally. The threshold approach is also well suited to examining the dynamic effect of the PPP over the full course of 2020. In contrast, the timing approach is best suited to examining the employment effects of the PPP in the early months of the program, after which point, most small businesses had taken up the PPP. From that point forward, the timing approach cannot provide a clean contrast between firms with and without a PPP loan.

Papers using the timing approach have come to a range of PPP employment effect estimates. [Autor et al. \(forthcoming\)](#), [Dalton \(2021\)](#), and [Granja et al. \(2020\)](#) estimate employment effects broadly similar in magnitude to those found here. In contrast, the results in [Li and Strahan \(2020\)](#) imply a much smaller boost to employment. The results in [Bartik et al. \(2021\)](#), [Doniger and Kay \(2021\)](#), [Faulkender et al. \(2020\)](#), and [Kurmann et al. \(2021\)](#) though, suggest a substantially larger employment effect than found in this paper.²

Our work is also related to the contemporaneous working paper by [Chetty et al. \(2020\)](#), who use the PPP’s eligibility size threshold to identify the effect of the program on employment, as we do here. Consistent with the results reported here, they find that employment was boosted by 2% at PPP-eligible firms through August of 2020, although their estimates are not statistically distinguishable from zero. [Hubbard and Strain \(2020\)](#) also assess the employment effects of the PPP using a variety of approaches, including the threshold eligibility design. Their preferred estimates indicate a peak employment effect of about $3\frac{1}{2}$ percent. Although these estimates are similar in magnitude to ours, we note that they rely on comparing extremely small firms to extremely large firms and therefore require rather stronger assumptions to be interpreted causally; moreover, in some instances these estimates achieve identification through the endogenous choice to take up a PPP loan.³

²These papers generally interpret their relatively larger employment effects as reflecting a more pronounced response among very small firms. That said, [Autor et al. \(forthcoming\)](#) and [Dalton \(2021\)](#) find only modestly larger employment effects for such firms.

³See their Table 4, columns (4) and (6), and Figures 3a and 3b. Their estimates most similar in spirit to

The paper proceeds as follows: Section 2 provides background on the PPP; Section 3 discusses the data and presents graphical analysis; Section 4 presents the intent-to-treat estimates; Section 5 presents the estimates of the aggregate effect of the PPP; and Section 6 concludes.

2 The Paycheck Protection Program

The PPP was established through the CARES Act, passed on March 27, 2020. The first PPP loan was approved on April 3, 2020 and funding was exhausted on April 16. Congress then provided a second tranche of funding and loan approval resumed on April 27. The second round of loans concluded in early August without exhausting the available funding, indicating the program was eventually able to meet available demand. A third tranche of funding enabled a resumption in PPP lending in early January of 2021. Unlike loans from the first two tranches, however, most third tranche PPP loans required businesses to demonstrate a significant revenue loss. Because our data lack information on firm revenue, we analyze only the first two tranches of PPP loans from 2020, and all subsequent discussion pertains to the first two tranches except where noted. The complex rules governing the program's eligibility and loan forgiveness were altered over time by Congress. Our discussion here focuses on the final rules applying to the first two tranches. See [Autor et al. \(forthcoming\)](#) and [Appendix A](#) for additional details on the PPP program rules and parameters.

PPP eligibility required a firm to meet the SBA's small business size standard, which is defined as 500 or fewer employees on average over a year for the large majority of industries, although the threshold was larger for some industries.⁴ Businesses were permitted to draw loans worth up to 10 weeks of payroll costs, with a maximum size of \$10 million dollars. Payroll costs include wage and salary compensation of all workers up to an annual rate of

those in this paper, which compare eligible firms sized 400-475 to ineligible firms sized 525-600, indicate the PPP had no effect on employment (see their Table 4, column 5).

⁴Businesses could also qualify for the PPP if their annual receipts or profits were lower than a given threshold. Lacking firm financial data, we are unable to leverage this alternative revenue cutoff.

\$100,000, as well as paid leave, health insurance costs, other benefit costs, and state and local taxes.

PPP loans were entirely forgiven if the loan-receiving firm met several criteria over the 24-weeks following loan disbursement: payroll expenses had to equal at least 60 percent of the loan amount; total qualifying expenses—which included payroll expenses, utilities, rent, and mortgage payments—had to at least equal the loan amount; and wages had to be maintained at not less than 75 percent of their pre-crisis level.⁵ If one or more of these criteria were not met, loans could still be partially forgiven. Ultimately, loan forgiveness was nearly universal, with 96% of 2020 PPP loans forgiven to date ([Small Business Administration, 2022](#)).⁶

The attractiveness of the PPP loans led to substantial take-up among eligible firms. About 5.2 million PPP loans were approved in 2020 worth around \$525 billion, which is about equal to 10 weeks of total payroll (the maximum permitted loan amount in most cases) for *all businesses* with fewer than 500 employees. See [Appendix A](#) for more details.

The blue bars of [Figure 1](#) show the number of employees at firms receiving PPP loans by firm size bracket as measured using PPP loan-level data from the Small Business Administration. The red bars show total employment in the same size bins from the Census Bureau’s *Statistics of U.S. Businesses* (SUSB) data for 2017. Employment-weighted take-up—defined as the ratio of the blue bars to the red bars—was high across the size distribution, averaging a bit more than 90%. [Appendix B](#) provides additional information.

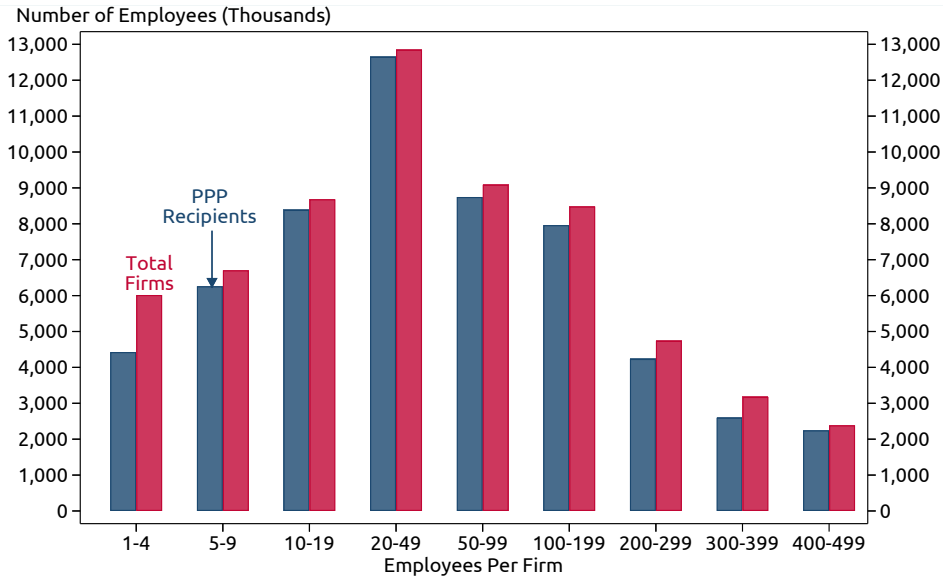
3 A Preliminary Look at the Data

Our analysis harnesses anonymized and aggregated payroll data, organized as a panel of firm-week observations, from the private-sector firm ADP, which processes payrolls for over 26 million individual workers in the United States per month. Workers at each firm are

⁵There was also a maintenance of employment requirement, but a number of “safe harbor” provisions significantly loosened or eliminated this requirement for many firms.

⁶Despite some initial confusion about these criteria, it is likely that firms anticipated a high degree of loan forgiveness. For example, even firms with significant staffing reductions could potentially spend 60 percent of the loan amount over the 24 week window because the loan size was equal to only 10 weeks of payroll.

Figure 1: Distribution of PPP Loans by Firm Size, 1-499



Note: Excludes about 850,000 loans to the self-employed, sole proprietors, independent contractors, single-member LLCs with only one job reported as well as loans to businesses in Puerto Rico, Virgin Islands, and Guam as those types of employers and areas are excluded from the SUBS universe.

Source: Authors' analysis of SBA loan-level PPP data and Census Bureau *Statistics of U.S. Businesses*.

considered to be employed for the duration of the employer-specific pay period as long as they received any payment.⁷ If a firm stops appearing in the ADP payroll data, this could mean that the firm has permanently shut down, that it has temporarily suspended operations, or that it has discontinued operations with ADP's payroll services. We treat these sample exits as closures, meaning that we set employment to zero for firms that exit the sample for any reason. Though there is some turnover in ADP's clientele (leading to false closures), we do not expect customer turnover to be correlated with PPP treatment eligibility except through the effect of PPP on firm shutdowns.

The representativeness of the ADP data has been carefully documented in earlier work by [Cajner et al. \(2018\)](#), [Grigsby et al. \(2019\)](#), and [Cajner et al. \(2020a\)](#). Particularly relevant for this paper, [Cajner et al. \(2020b\)](#) show that employment indexes derived from the ADP data closely matched the dynamics of the Bureau of Labor Statistics monthly CES data in the early stages of the pandemic. See appendix C for additional discussion.

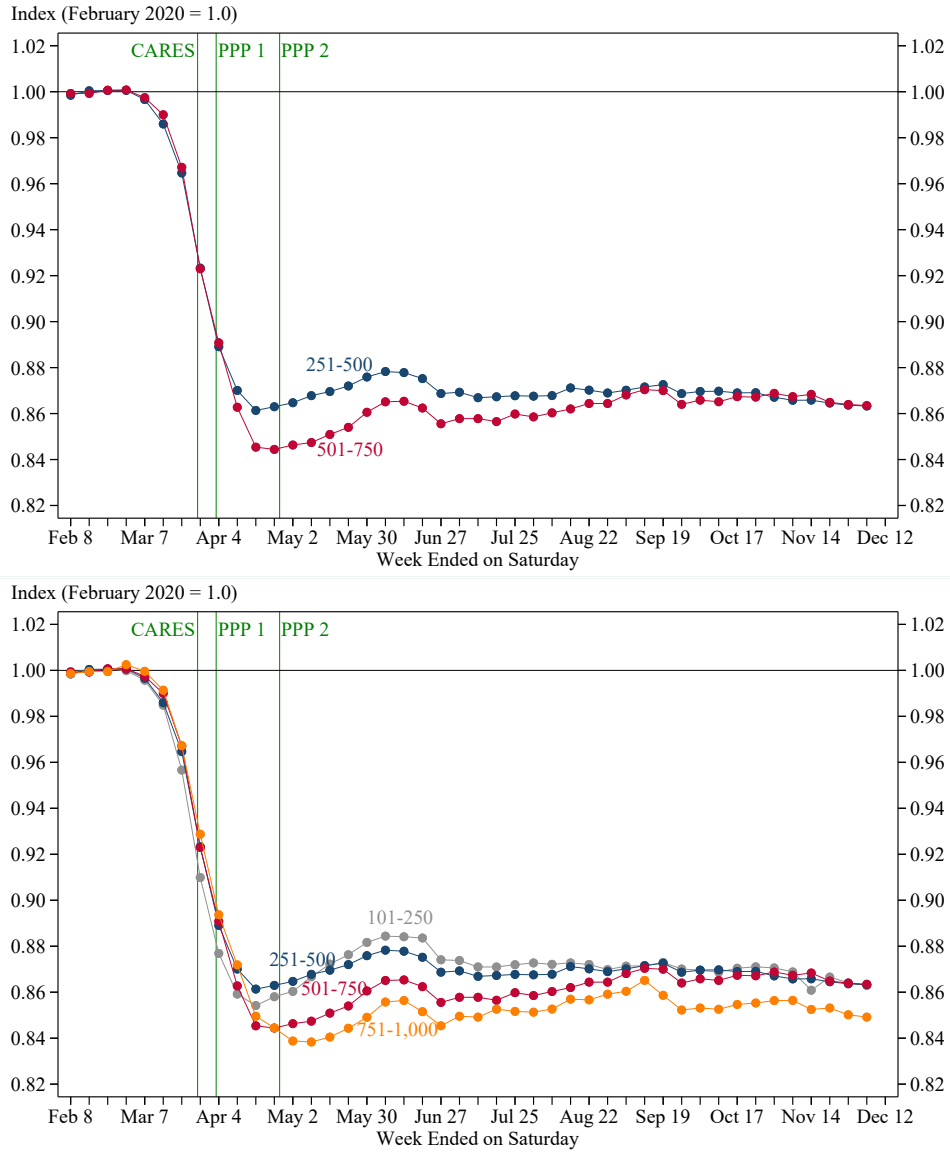
⁷This is the same employment concept used by the Bureau of Labor Statistics Current Employment Statistics (CES) data.

Firms are eligible for PPP loans if their employment is either below 500 workers or less than an SBA-specific size threshold (exceeding 500). We exploit this threshold rule to contrast employment outcomes at firms that are above versus below the SBA’s employment thresholds. Our analysis accordingly focuses on the subset of relatively larger firms among small businesses, all of which have at least 250 employees. Only 14% of the PPP’s 2020 loan volume went to firms with 250 or more employees, meaning that our analysis sample focuses on firms that are substantially larger than the typical PPP-recipient firm. Nevertheless, as shown in Appendix C, our sample of large firms has a sectoral mix broadly similar to that of all PPP-recipient firms. Because virtually all firms in accommodation and food service (NAICS 72) were likely eligible for PPP loans (meaning that there is no natural comparison group), we omit that sector in all analysis.

Prior to the formal analysis, Figure 2 provides a preliminary look at the evolution of employment among likely-eligible and likely-ineligible firms from early February of 2020, prior to the pandemic’s U.S. onset, to late December of the same year.⁸ The top panel plots employment indexed to a firm’s average level of employment in February 2020 for two size classes: 251-500 (likely eligible, in blue) and 501-750 (likely ineligible, in red). Employment declines symmetrically across these groups through the beginning of the crisis, falling by about 11 percent in both size classes by the beginning of April. Once the PPP is in operation, however, the trajectories of these groups diverges, with employment stabilizing more quickly in firms with 251 to 500 employees. Around two months after the launch of the PPP, employment is approximately 2 percent higher relative to baseline at firms that are likely eligible for PPP loans than at those that are not. From the end of May forward, employment relative to baseline among firms in these two coarse size bins gradually converges, with the difference falling to about 1 percent by the beginning of July and disappearing by the beginning of September.

⁸Firms in industries with higher thresholds than 500 are excluded from the graphical exercise in Figure 2. These firms are used in the regression analysis below, where we apply the SBA’s industry-specific thresholds to define treatment status.

Figure 2: 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 in both 2019 and 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: Authors' analysis of ADP data.

The bottom panel of Figure 2 provides further detail by additionally plotting the evolution of employment at firms further away from the PPP eligibility threshold: those with pre-pandemic employment of either 101-250 workers or 751-1,000 workers. Employment trends in these additional size categories broadly reinforce the pattern seen in the first panel. Employment at firms with 101-250 workers closely tracks those with 251-500 workers, while

employment at firms with 751-1,000 workers tracks that of firms with 501-750 workers. Thus, relative to firms with 501–750 employees, employment at firms with 101-250 employees rises by roughly 2 percent from the time of PPP enactment to the end of June 2020, after which point this employment gap gradually closes. These plots suggest that the PPP may have temporarily boosted employment at firms that were eligible to receive loans compared to those that were primarily ineligible. Our subsequent analyses formally explores these relationships.

4 Identification Approach and Primary Estimates

Our empirical strategy exploits the PPP eligibility size thresholds to identify the effect of the PPP loan receipt on employment. In the spirit of Figure 2, we compare the outcomes of firms above and below the industry-specific eligibility threshold using a dynamic, difference-in-difference (DD) approach.

One practical challenge in implementing our research design is accurately assigning firms to PPP eligibility status. The PPP allows firms flexibility in choosing a window over which to define average employment for the purposes of meeting the threshold, including calendar year 2019, the trailing 12-month average prior to application, or various 12-week periods for seasonal firms. We do not observe the precise data or rule chosen by firms to establish their eligibility. In order to limit the potential for spurious eligibility assignment, we define eligibility based on *both* average 2019 employment and February 2020 employment and omit from the estimation sample firms whose PPP eligibility status differs across these two firm size measures.⁹ In Appendix E, we apply alternative windows for calculating eligibility and obtain results broadly similar to our baseline results.

⁹One issue that could lead to spurious inference is mean reversion in firm size. For example, short-term fluctuations in employment around the eligibility-threshold could be inversely correlated with employment growth over the estimation period, and thereby produce upward bias in our estimated treatment effects of the PPP. By defining firm size based on 2019 average employment *and* February 2020 employment we reduce the likelihood of this pitfall as short-term employment fluctuations will tend to average out over longer periods of time.

We use the following dynamic difference-in-difference specification to estimate the relationship between PPP eligibility and employment:

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

where y_{ijst} is total employment for firm i in industry j in state s 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, θ_t is a vector of indicator variables for weeks t , and PPP_i is an indicator variable equaling one if firm i is eligible for the PPP program based on the industry-specific size threshold. Week t spans the period from the week starting January 5, 2020 through the week starting November 29, 2020 (ending December 5, 2020)—covering the period prior to the crisis, the passage of the CARES Act (March 27th), and through most of the ensuing year.¹⁰ Standard errors are conservatively clustered at the NAICS 3-digit industry level. Finally, we weight the regressions by firm size in February 2020 so that the results can be interpreted as the estimated effect of the PPP on the employment of the average worker employed at the set of firms operating in 2020.

The time-varying β_t vector is the parameter of interest; under our identifying assumptions, discussed below, it traces out the treatment effect of PPP eligibility on employment. The treatment effect is likely to vary over time for several reasons: receipt of PPP loans gradually ramps up over the period we examine; it may take time for firms to bring workers back onto payroll; and ineligible firms may rebound even absent PPP support as the recovery takes hold. The 3-digit industry-week fixed effects absorb time-varying shocks common to firms within a given industry, while state-week fixed effects absorb time-varying shocks common to all firms in a state. Both sets of fixed effects are important because industries were affected differently by the pandemic and because states imposed different social distancing

¹⁰Because our weekly ADP data begin in 2020, we commence our analysis of pre-pandemic outcomes at the beginning of that year. We believe that the most informative period for assessing common pre-trends among PPP-eligible and PPP-ineligible firms is the weeks immediately after the pandemic’s U.S. onset but prior to PPP’s enactment.

rules, did so at different times, and may have experienced different degrees of voluntary social distancing.

The identifying assumption of the empirical model is that, absent the PPP, firms below the size-eligibility threshold would have experienced comparable employment growth or contraction to firms above the threshold, conditional on the covariates. Underlying trends in firm employment not due to the PPP, particularly those induced by social distancing and the broader economic downturn, are the most likely violation of this assumption. We address these potential violations of the identifying assumption in three principal ways. First, the pre-CARES Act portion of the β_t vector provides a partial check against differential employment trends correlated with PPP eligibility. If PPP eligibility is not confounded with underlying trends, there should be no trend in the β_t vector in the pre-CARES Act period. Second, as discussed above, we include industry-week and state-week fixed effects controls for time-varying shocks associated with COVID-19 at both the industry and state level. Third, in order to render the treatment and control groups as comparable as possible, we limit the estimation sample to firms in various windows around the threshold, from between 50 to 250 workers.¹¹

As an initial check on the comparability of firms above and below the eligibility threshold, Table 1 displays firm summary statistics, including gender composition, industry affiliation, and average hourly wages, weekly hours, and weekly earnings. These comparisons show that, apart from size, firms above and below the eligibility threshold appear quite comparable prior to the crisis. For example, average weekly earnings at firms 0 to 249 workers below the threshold, equal to \$1,272, are barely distinguishable from those at firms with 1 to 250 workers above the threshold, equal to \$1,277.

Figure 3 reports our main estimates of equation (1). Each panel presents estimates of the β_t vector for a different firm size window. The shaded region in each panel corresponds

¹¹The Main Street Lending Facility was potentially available to firms in our control group (Decker et al., 2021). Appendix F discusses why it is unlikely that this significantly affects our estimates of the effect of the PPP program.

Table 1: Summary Statistics as of February 2020

	PPP Threshold ± 250		PPP Threshold ± 100	
	0-249 Below	1-250 Above	0-99 Below	1-100 Above
Employment	389.8	653.4	472.9	579.1
% Female	46.2	46.4	46.1	48.5
% Hourly	62.5	64.1	63.0	63.0
Weekly Hours Per Worker	36.8	37.4	37.3	37.2
Weekly Earnings Per Worker (\$)	1,271.8	1,277.3	1,278.6	1,278.8
Hourly Wage Per Worker (\$)	37.8	36.9	37.7	37.5
Sectors (%):				
Manufacturing	7.8	9.0	8.7	8.2
Wholesale Trade	8.2	9.0	8.1	10.4
Retail Trade	6.4	8.1	6.2	8.4
Financial Activities	9.1	9.1	9.3	8.0
Professional & Business	17.4	17.0	17.2	15.9
Education & Health	18.9	17.9	20.2	18.3
Leisure & Hospitality	6.6	6.9	6.4	6.7
Other	25.7	22.9	24.0	24.2

Note: Employment, weekly hours, weekly earnings, and hourly wage represent firm-level means for each column. Data are weighted by each firm's employment as of February 2020. Samples reflect firms that were present in the ADP data for all 12 months of 2019.

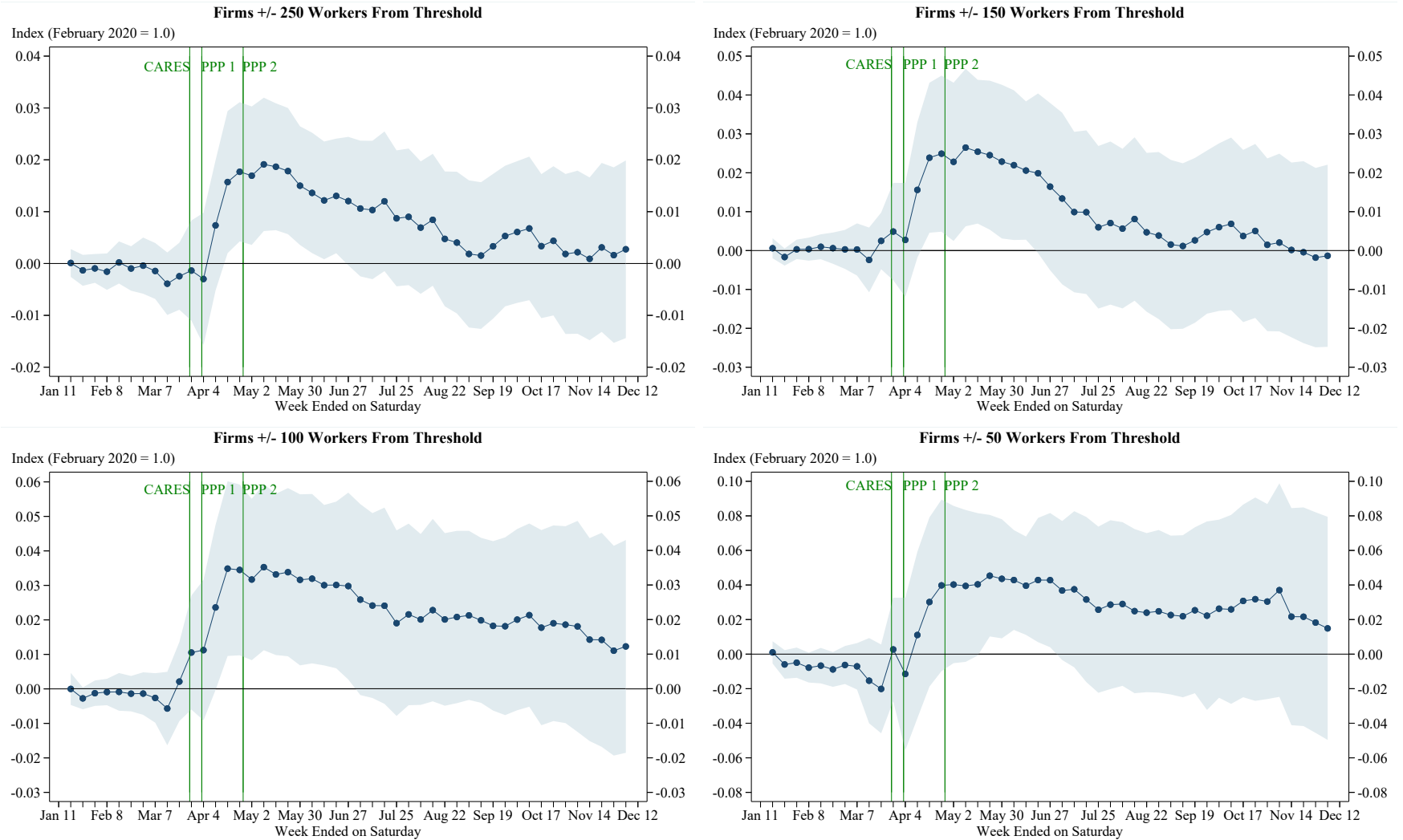
Source: Authors' analysis of ADP data.

to the 95 percent confidence interval around the point estimates. These estimates uniformly find a positive treatment effect of PPP eligibility on firm employment. In the top-left panel, employment at firms with up to 250 employees below the eligibility threshold trends in parallel with employment at firms with up to 250 employees above the eligibility threshold prior to PPP, with pre-trend point estimates consistently around zero. Once the PPP commences in the first week of April 2020, employment rises at eligible relative to ineligible firms, increasing by about 2 percent through May, after which the gap attenuates. This contrast is no longer statistically significant from early July forward, though the point estimates suggest that employment at eligible firms was about 1 percent higher than at ineligible firms in July and roughly 0.5 percent higher on average thereafter.

The subsequent panels of Figure 3 present estimates for different size windows around the eligibility threshold. These estimates are in all cases qualitatively similar to those in the first panel, though the magnitude of the the point estimates at peak PPP efficacy (around May 2020) grows somewhat larger as we shrink the firm size window around the eligibility

threshold. When including firms within 150 employees of the eligibility threshold (top-right panel), the estimated peak employment effect is roughly 2.5 percent. This estimate rises to 3.5 percent and 5 percent, respectively, for firms that are within 100 and within 50 employees of the eligibility criteria (bottom-left and bottom-right of the figure). Averaging across all four specifications, the peak effect registers at about 3 percent in mid-May of 2020. After mid-May, the point estimate declines throughout 2020. At the end of the year, the point estimates range from no effect (for the ± 150 window) to about 3 percent (for the ± 50 window), neither of which is statistically significant. Across the four specifications, the point estimates average about 1.2 percent at the end of 2020.

Figure 3: Effect of PPP Eligibility on 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 includes firms that were present in the ADP data for all 12 months of 2019.

Source: Authors' analysis of ADP data.

Employment in treatment and control groups was trending in parallel in the pre-PPP period but not thereafter, as shown in Figure 3, consistent with a causal interpretation of the treatment effect estimates.¹² One anomaly is visible when focusing on firms within 100 employees of the eligibility threshold (bottom-left panel): the treatment effect appears to commence *during* the week of the passage of the CARES Act, which was passed by the Senate on March 25, 2020, and passed by the House and signed into law two days later. In the week prior to the act’s passage, there was widespread reporting on an SBA loan program for small businesses with under 500 employees.¹³ It is therefore possible that business owners below the threshold held off paring back on payrolls in anticipation of the loan program. There is also a clear jump upward in the treatment effect vector after PPP loans commence. This pre-treatment jump using the ± 100 employee size window is the one anomalous finding in our analysis, and we flag it for the sake of caution.

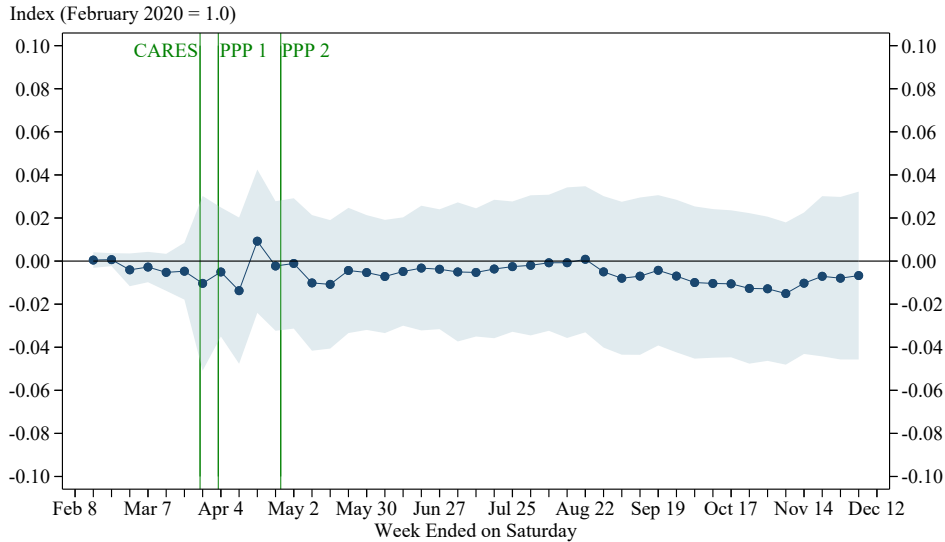
Figure 4 offers a reality check on our identification strategy. Although in most sectors PPP eligibility was limited to firms with 500 or fewer employers, the size cap was higher in specific sectors. We would accordingly not expect to find a “treatment effect” at the 500 threshold in these sectors. To test this implication, we estimate equation (1) for firms in high-threshold industries, using firms of size 251 to 500 employees as the placebo treatment group and firms of size 501 to 750 serve as the comparison group. (The minimum actual PPP-eligibility threshold for firms with a non-500 threshold is 750.) Figure 4 confirms that the placebo treatment effect is near zero in both the pre- and post-PPP period. Appendix G presents the actual PPP treatment effect estimates for the same industries used in the placebo test; the point estimates are broadly similar to our primary results in Figure 3.

Appendix H discusses results for additional outcomes using the DD research design. We

¹²While the estimates in the pre-PPP periods in Figure 3 are nearly all statistically insignificant, in some cases the estimates appear to be declining prior to the PPP. This raises the possibility that our estimates might understate the employment effect in the post-PPP period. To assess this possibility, we account for these pre-trends using the procedure developed in Freyaldenhoven et al. (forthcoming) and Dobkin et al. (2018). The results are quite similar to our baseline estimates, as discussed in Appendix I.

¹³For example, both a [Washington Post](#) article on March 18th and a [tweet from Senator Marco Rubio](#) on March 17th discuss the 500 firm size threshold.

Figure 4: Placebo Effect of Having 251-500 Workers on Employment for Firms With PPP Eligibility Above 500



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. The sample is restricted to firms with a PPP eligibility threshold above 500 and with 251 to 750 employees; firms with 251 to 500 workers form the placebo treatment group and those with 501 to 750 workers form the control group. The sample contains firms that were present in the ADP data for all 12 months of 2019. Source: Authors’ analysis of ADP data.

find no evidence that the PPP influenced either the intensive margin of employment (i.e., hours) or the propensity of firms to remain open. Hence, the employment results in Figure 3 likely reflect the extensive margin adjustment of the number of workers at firms which remained open.

5 Estimating Treatment-on-the-Treated

Our primary results shown in Figure 3 correspond to intent-to-treat (ITT) estimates, reflecting the effect of loan eligibility rather than take-up on employment. To estimate the effect of *receiving* a PPP loan (i.e. the average effect of treatment-on-the-treated, ATT), we re-scale the ITT estimates, β_t , using the standard Wald estimator:¹⁴

$$\delta_t = \frac{\beta_t}{\underline{\gamma} - \bar{\gamma}}. \quad (2)$$

¹⁴For the sake of simplicity, we use terminal take-up rates; hence the γ ’s are time-invariant.

where $\underline{\gamma}$ is employment-weighted PPP take-up among those firms below the SBA size threshold and $\bar{\gamma}$ is employment-weighted take-up among firms above the threshold. The take-up above the threshold reflects, at least in part, that firms with sufficiently small revenues or profits were entitled to PPP loans, despite potentially having more than 500 workers.

Since our primary data source does not record PPP loan receipt, we estimate take-up using SBA loan-level PPP records. Unfortunately, because the size of recipient firms reported in the SBA loan data is truncated at 500 workers, we cannot estimate take-up *below* the industry-specific threshold, $\underline{\gamma}$, for industries with eligibility thresholds above 500 employees. For the same reason, across *all* industries, we cannot directly estimate the take-up rate *above* the threshold, $\bar{\gamma}$.

We address these limitations as follows. To estimate take-up below the threshold, we restrict attention to industries with a 500 worker threshold and assume the estimated take-up rates from this subset of industries holds across all industries. Using publicly-available Census SUSB data reporting firm size by industry paired with SBA PPP loan-level data, we estimate that $\underline{\gamma} \approx 81\%$ within a firm size window of 300-499 employees. Next, to estimate take-up above the eligibility threshold, $\bar{\gamma}$, we again restrict attention to industries with a 500 worker threshold and assume that firms coded (i.e., truncated) at size 500 in the PPP loan-level data are of the same average size as firms from the 500-999 size bin in the SUSB data. This approach yields an estimate of $\bar{\gamma} \approx 27\%$.

Adjusting for take-up above and below the threshold yields an ATT estimate of $\delta_t = \frac{1}{\underline{\gamma} - \bar{\gamma}} \times \beta_t = \frac{1}{0.81 - 0.27} \times \beta_t = 1.85 \times \beta_t$. In practice, different firm size bins above and below the eligibility threshold produce slightly different scaling factors, $\frac{1}{\underline{\gamma} - \bar{\gamma}}$. In the aggregate employment effect calculations below, we set $\frac{1}{\underline{\gamma} - \bar{\gamma}}$ equal to its average value of 2 across a set of such estimates (Appendix Table B.2). See Appendix B for additional information on our ATT estimates, including Figure B.1 which presents estimates of the ATT, a comparison to similar estimates in Chetty et al. (2020), and a discussion of how fraud would influence our ATT estimates.

Applying this scaling factor, we estimate the implied effect of the PPP on total U.S. payroll employment as

$$E_t = \delta_t \times T, \tag{3}$$

where δ_t is the ATT estimate and T is the number of employees at PPP-recipient firms. We estimate $T = 59.2$ million using our estimated take-up rates multiplied by the count of employment below industry-specific eligibility thresholds, *plus* PPP take-up above 500, which we again assume is drawn from the 500-999 firm-size bin. See Appendix D for additional details.

At its peak around mid-May 2020, averaging across the same specifications as shown in Figure 3, PPP loan receipt raised recipient employment by about 6% (3% average intent-to-treat estimate times the scaling factor of 2), yielding an estimated employment gain of about 3.6 million workers in total (6% \times 59.2 million). By the beginning of December, the ATT estimates are uniformly smaller, averaging 2.4%, implying an employment boost of about 1.4 million.

These calculations extrapolate from treatment effects that are estimated from firms in the vicinity of the eligibility thresholds. We noted above that the PPP may have had different effects on smaller firms, which are farther away from the eligibility threshold. If smaller firms were relatively more cash constrained during the crisis, PPP funds may have resulted in a larger share of jobs retained at these firms. Approximately 52% of small business employment is at firms with 1-49 employees, which is plausibly the group of firms that may have been particularly vulnerable *and* which do not contribute to the identification of our causal effect estimates. If we assume that the peak effect of loan receipt is twice as large in this group of firms (12%)—consistent with the evidence in Autor et al. (forthcoming)—this increases our estimated peak employment effect from 3.6 million to 5.5 million.

To put these employment numbers in dollar terms, we calculate the cost per year of employment retained by the PPP. We calculate this cost as: $52 \times \frac{PPP_{volume}}{\sum_{t \in T} E_t}$, where $\sum_{t \in T} E_t$ is the sum of additional weekly employment attributable to the PPP from the beginning of

the PPP program through the end of our sample, and PPP_{volume} is the total dollar volume of PPP loans from the first two tranches of the program. This calculation yields a cost of \$317,000 per full-year job preserved by the PPP from the program’s inception to the start of December of 2020 (the end point of our data set).

A limitation to this calculation is that it implicitly assumes that there is no effect of the PPP on employment after early December. Our point estimates in Figure 3, however, suggest that the impact remains positive in that month, although these estimates are statistically insignificant. We conservatively adjust for the effects of PPP on employment after early December 2020 by extrapolating the treatment effect of the PPP (E_t) after our estimation ends using the trend decline observed from the peak effect in mid-May through December 2020. This yields a linearly-declining path of PPP treatment effects that reaches zero in June 2021, shown in Appendix Figure D.1. Under this assumption, the PPP preserved 1.6 million jobs per week on average from April 2020 through June 2021, implying a program expenditure of \$258,000 per full-year-equivalent job preserved, or roughly 5.2 times the median worker’s salary.¹⁵ Alternatively, using the same extrapolation but assuming that the treatment effect was double for smaller firms, the PPP is estimated to have saved 2.4 million jobs per week at a cost of \$169,000, or about 3.4 times the median salary.

6 Conclusion

Utilizing high-resolution administrative microdata on firm-level employment from ADP, we provide an assessment of the PPP’s effect on U.S. employment, focusing on the \$525 billion in forgivable PPP loans made during 2020, prior to a substantial change in program targeting in 2021. Using a dynamic difference-in-difference framework, we estimate that the PPP increased the level of employment at eligible firms by 2 to 5 percent at its peak in mid-May, an effect that slowly declined thereafter. These estimates imply that the PPP preserved

¹⁵Equal to about \$50,000, or 52 times median weekly earnings in the first quarter of 2020 of \$949 as measured in the Bureau of Labor Statistics Usual Weekly Earnings series (BLS, 2020).

approximately 3.6 million jobs in mid-May of 2020, and about 1.4 million jobs at the end of 2020. The estimated dollar amount of PPP expenditure per year of employment retained is equal to 5.2 times the median full-time full-year U.S. salary in 2020. These estimates are identified by PPP-induced changes in employment at firms a good bit larger than the typical PPP-receiving firm. Assuming that small-firm employment was boosted by the PPP by twice as much as large firm employment yields a cost per year of employment preserved of 3.4 times the median salary. Thus PPP outlays very substantially exceeded the salary costs of jobs supported by the program.

A full cost-benefit analysis of the PPP would include several additional margins of potential efficacy not evaluated here. By preventing bankruptcies, the PPP may have preserved valuable intangible firm capital, which could have positive long-run economic effects. Additionally, the PPP may also have reduced loan defaults, which would benefit creditors throughout the economy (e.g. suppliers to businesses and commercial landlords) and would also possibly reduce strain on the financial system. Finally, the PPP may have reduced other public outlays that workers would have received had the PPP not preserved their employment, including unemployment compensation, rental assistance, Supplemental Nutrition Assistance Program (SNAP) aid, and other safety-net benefits. A full accounting of these indirect avenues of potential PPP program efficacy, including both their partial and general equilibrium effects, merits significant additional research.

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Web Appendix

A Details of PPP Program

The first tranche of PPP funding was included in the Coronavirus Aid, Relief, and Economic Security (CARES) Act passed on March 27, 2020, and the second tranche was established in the Paycheck Protection Program and Health Care Enhancement Act, which passed on April 24. The third tranche of PPP funding—which we do not analyze in this paper—was provided by the Consolidated Appropriations Act of 2021, passed on December 27, 2020.

The first applications for PPP loans were accepted on April 3rd with funds disbursed within 10 days of final SBA approval.¹ There was intense demand for loans at the beginning of the program, with the first tranche of \$350 billion exhausted by April 16th and 85% of all loans from the first two tranches approved by the end of the first week of May.

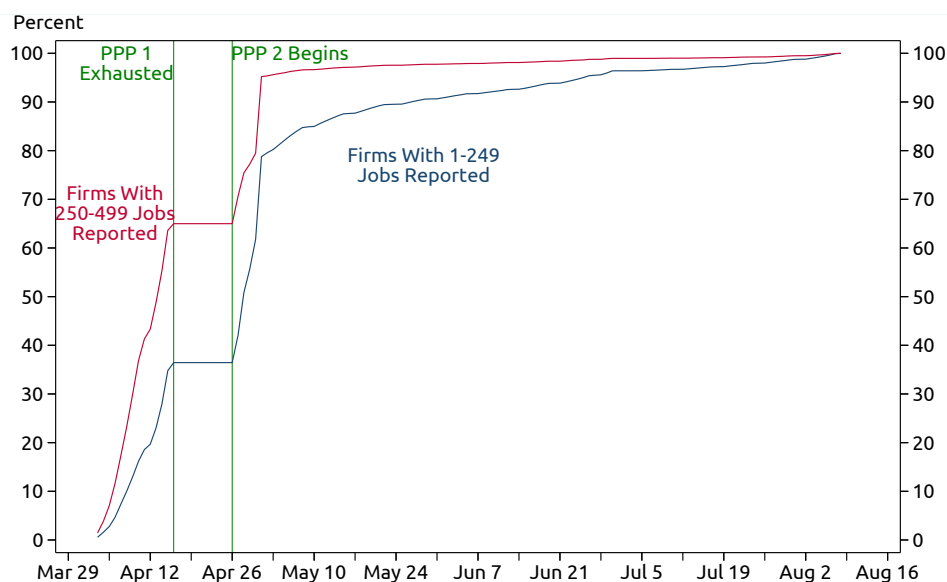
The speed at which loans were granted varied with the size of businesses. Figure [A.1](#) examines the timing of the approvals of these first and second tranche loans by size of the firm. By the middle of April, the SBA had already approved 70 percent of the eventual total number of loans granted to firms between 250 and 499 employees. In contrast, loans to smaller businesses did not reach 70 percent of their eventual total until early May.

There was initially significant confusion among businesses and analysts over the specifics of the PPP rules for loan forgiveness and these rules evolved considerably after the passage of the CARES Act. Most notably, the Paycheck Protection Program Flexibility Act, passed on June 4th, 2020 but applied retroactively to previously-approved loans, extends the window over which loan proceeds can be spent to qualify for forgiveness from 8 weeks to 24 weeks and reduces the required share of the loan spent on payroll from 75 percent to 60 percent.²

¹Participating lenders were responsible for verifying the applications and passing them onto the SBA for final approval. Initially, some lenders made initial partial disbursements within 10 days of loan approval but did not making full disbursements until later in order to delay the forgiveness criteria reference period. At the end of April, guidance was issued that the funds must be disbursed within 10 days of the loan being approved.

²If borrowers are required to repay a PPP loan, the terms are relatively favorable: The first installment

Figure A.1: Cumulative Distribution of PPP Loan Approvals by Date



Note: Data reflect first and second tranche PPP loans split by bins of jobs reported. Data exclude PPP loans to non-employers as well as loans to businesses in Puerto Rico, Virgin Islands, and Guam. Source: Authors' analysis of SBA loan-level data release.

Many firms received loans well before the Flexibility Act was passed and may have made decisions under the original rules. Alternatively, firms may have used the more flexible rules to spend additional funds on fixed obligations rather than payrolls, thus reducing the likely employment impact of the PPP.

The scale of the approximately \$525 billion in first and second round PPP loans issued in 2020 was about equal to the total payroll of the targeted set of small businesses. Specifically, according to Census Bureau's Statistics of U.S. Business (SUSB) data, 10 weeks of payroll—the metric used to determine PPP loan size in most cases—for *all private-sector businesses with fewer than 500 employees* was about \$520 billion in 2017. This figure, though, likely underestimates potential payrolls eligible for the PPP, since in some industries, businesses with more than 500 employees could qualify for PPP loans.

B Take-up and Treatment-on-the-Treated Estimates

The take-up scaling adjustment for the treatment-on-the-treated estimates as defined in [of the loan is deferred for six months and the interest rate is only 1 percent.](#)

Table A.1: Firms With Fewer Than 500 Employees, 2017

	Firms	Employment	2 $\frac{1}{2}$ Months Payroll (\$)
Total Private Sector	5,976,761	60,556,081	521,449,419
Agriculture, Forestry & Fishing	22,535	136,591	1,124,746
Mining & Oil & Gas Extraction	18,720	244,367	3,707,711
Construction	700,393	5,373,702	59,522,179
Manufacturing	244,098	5,039,772	47,835,647
Trade, Transportation & Utilities	1,129,034	10,736,588	91,535,076
Information	78,430	984,379	14,433,836
Financial Activities	544,763	3,361,539	45,126,926
Professional & Business	1,170,857	9,368,738	108,232,178
Education & Health	742,837	10,630,121	81,539,312
Leisure & Hospitality	666,730	9,971,192	40,272,986
Other Services	695,268	4,697,878	28,058,288

Source: Census Bureau, *Statistics of U.S. Businesses*.

equation (2), is equal to $\frac{1}{\underline{\gamma}-\bar{\gamma}}$. This adjustment requires estimates of the PPP take-up rate both below ($\underline{\gamma}$) and above ($\bar{\gamma}$) the eligibility threshold. We calculate employment-weighted PPP take-up rates (i.e., number of employees at PPP-receiving firms relative to total number of employees at all firms) using two data sources. The number of employees at PPP-recipient firms—the numerator in the take-up rate—is obtained from the “jobs reported” variable in the PPP loan-level data maintained by the SBA. The number of jobs—the denominator in the calculation—is taken from the Census Bureau’s SUSB data. We utilize SUSB data cut by firm size bins and six-digit NAICS industries as of 2017Q1. We extrapolate SUSB employment to 2019Q4 using a growth rate calculated from the BLS’s *Business Employment Dynamics* (BED) data for the closest relevant firm-size bin.

We restrict the sample to six-digit NAICS industries with PPP eligibility size thresholds of 500 due to the truncation of firm size to 500 in the PPP loan data.³ We collapse the number of jobs reported in the PPP loan data by the following size bins (determined by the bins available in the SUSB data): 1-4, 5-9, 10-19, 20-49, 50-99, 100-199, 200-299, 300-399, 400-499, and 500. We eliminate loans to businesses in Puerto Rico, the Virgin Islands,

³The take-up rates estimated here are not strictly comparable to those reported in [Autor et al. \(forthcoming\)](#) primarily because, in this paper, we restrict attention to six-digit industries with 500-employee thresholds.

and Guam as those are outside of the scope of the SUSB data. We further drop loans to non-employers, defined as sole proprietors, independent contractors, single-member LLCs, and the self-employed with one reported job as they are also out of scope for the SUSB. In addition, we drop loans that are reported as un-disbursed. Finally, we trim (or drop) PPP loans at the bottom and top 1 percentile of the loan-amount-per-job distribution to address outliers.

We calculate employment-weighted PPP take-up rates using SUSB data on the count of employment by six-digit industry by firm-size bin. We compute total employment counts by size bin across all six-digit industries with PPP eligibility thresholds of 500 to form the denominators of the take-up rates. Some cells at the six-digit level are suppressed in the SUSB data to ensure data confidentiality. We adjust for data suppression by multiplying total employment estimated from aggregated six-digit-industry-by-size cells for industries with 500-worker thresholds by the ratio of published total employment by size bin across all industries to total employment aggregated from the six-digit-industry-by-size cells.

For size bins smaller than 500, take-up is calculated simply as the ratio of the number of jobs in the PPP loan data divided by number of employees from SUSB data by firm size bin. To estimate take-up of firms larger than 500, we assume that firms are drawn from size bins 500-749 or, alternatively, 500-999 and that the average size of PPP recipients are the same as the average size of firms in those bins at the national level. For example, for firms at the truncated size of 500 in the PPP loan data and in industries with size thresholds of 500, we impute that firm size is 633 within the 500-749 bin and 742 in the 500-999 bin. (Because we cannot obtain firm-size counts by detailed industry, we cannot calculate average size at the disaggregated industry level.)

Table [B.1](#) reports take-up rates calculated with our methodology. The first column is for industries with size thresholds of 500, which shows an employment-weighted average of 89.9% below 500, and around 81% between 200-499 or 300-499. Take-up is either 38.5% using the 500-749 window or 26.6% using the window 500-999. The second column shows take-up

rates omitting our trimming of the PPP loans at the top and bottom percentile of the loan-amount-per-job distribution. This shows similar patterns although somewhat higher take-up rates. Finally, for comparison, the last column shows take-up rates estimated in industries with higher than 500 PPP eligibility thresholds. These take-up rates do not fall off above 500 as they do in industries with a eligibility threshold of 500. This pattern supports the design of our placebo falsification exercise in Figure 4. If, conversely, the take-up rate fell off above size 500 in industries with an eligibility threshold above 500, we'd expect to estimate a PPP eligibility effect in the placebo test.

Table B.1: PPP Take-up by Firm Size (%)

Firm Size	500 Threshold Inds	500 Threshold Inds Untrimmed	> 500 Threshold Inds
1-4	72.5	73.7	70.0
5-9	92.9	94.1	87.0
10-19	96.9	98.4	89.0
20-49	99.0	101.4	89.3
50-99	94.7	98.9	85.5
100-199	88.1	94.6	86.0
200-299	81.2	87.6	89.6
300-399	75.4	81.7	75.2
400-499	87.8	94.3	87.2
500-749	38.5	50.2	109.0
500-999	26.6	34.6	74.9
Additional Statistics:			
1-499	89.9	93.4	86.5
200-499	80.9	87.4	84.4
300-499	80.7	87.1	80.4

Note: Estimates in the first and third columns drop loans that are below the first percentile or above the 99th percentile of the loan-to-size distribution. We exclude NAICS 72 from the first two columns, but include it in the third as virtually all NAICS 72 firms were eligible for the PPP. See Appendix B for further details on the calculations.

Source: Census Bureau, *Statistics of U.S. Businesses*, SBA PPP, and BLS BED.

Table B.2 shows the resulting scaling factors from the take-up rates estimated in Table B.1. The different size bins below the eligibility threshold form the rows of the table and the different size bins above the eligibility threshold form the columns. The inflation factors range from 1.6 to 2.4, averaging about 2, the estimate we implement in Section 5 to calculate the aggregate employment effects of the PPP.

Figure B.1 presents our treatment-on-the-treated estimates (ATT) based on averaging

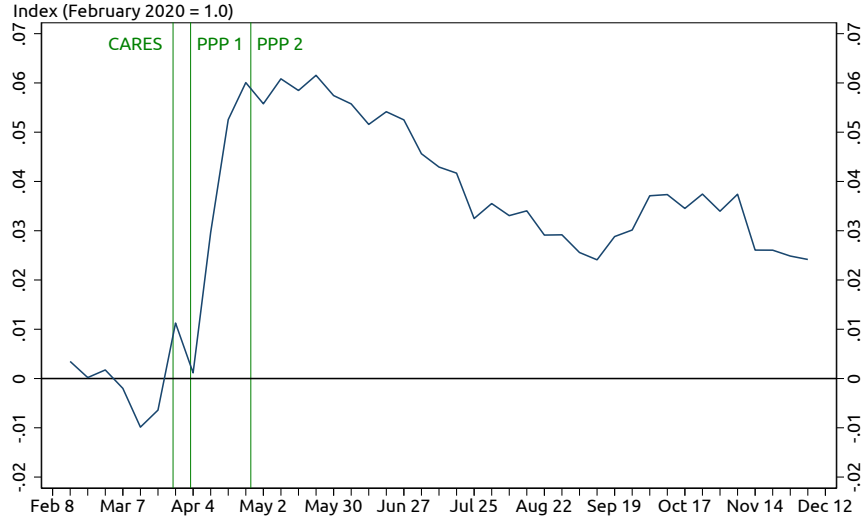
Table B.2: Scaling Factors

		$\bar{\gamma}$	
		500-749	500-999
$\underline{\gamma}$	200-499	2.4	1.8
	300-499	2.4	1.8
	400-499	2.0	1.6

Note: Scaling factor equals $1/(\underline{\gamma} - \bar{\gamma})$, where $\bar{\gamma}$ is the take-up rate in the size bin denoted by the columns and $\underline{\gamma}$ is the take-up rate in the size bin denoted by the rows.

Note: Census Bureau, *Statistics of U.S. Businesses*, SBA PPP, and BLS BED.

Figure B.1: Average Treatment-on-the-Treated Coefficients



Note: The coefficients shown here are averaged across the four panels of Figure 3 for each week of the ADP sample and then multiplied by an inflation factor of 2. See the notes to Figure 3 for details on the specification.

Source: Authors' analysis of ADP data.

across the intent-to-treat estimates on the four panels of Figure 3 for each week of the ADP sample and then multiplying by the average scaling factor of 2.

Chetty et al. (2020) make calculations similar to those discussed in this appendix section. They use a loan-dollar-per-job differential, however, instead of inflating their PPP intent-to-treat estimates by an employment-weighted take-up rate differential as we do here. Their resulting inflation factor is equal to 1.35. Calculating the equivalent inflation factor in our data yields an inflation factor of about 1.55, somewhat lower than the estimates implied by the employment-weighted take-up rates presented here.

There have been widespread reports of fraud within the PPP program (e.g. Tracy, 2020). In general, we do not view fraud as a significant threat to the validity of our estimate of the PPP's cost per job saved. If there was widespread fraud—e.g. loans taken out which

were not used for the approved purposes, including maintaining employment—then this will *properly* lower our estimate of the the employment effects of the program.

That said, there is a specific form of fraud that might bias downward our estimates of the average effect of treatment-on-the-treated (ATT). If firms above the eligibility size threshold fraudulently claimed to be beneath the threshold, this would bias downward our treatment-on-the-treated adjustment factor, $\frac{1}{\gamma-\bar{\gamma}}$. In turn, this would cause us to *underestimate* the ATT.

Although we do not directly observe fraud, we doubt it has a large influence on our ATT estimates. It appears that fraud within the PPP program was heavily concentrated in loans extended by fintechs (Griffin et al., 2021). However, larger firms—including those near the eligibility thresholds with the potential to fraudulently lower employee counts to obtain eligibility—overwhelmingly received loans from well-established commercial banks rather than fintechs (e.g. Granja et al., 2020). Such banks have long-standing compliance programs and established reputations; accordingly, they have more incentive to avoid fraud (Griffin et al., 2021). Moreover, media and government scrutiny of large firms which took out PPP loans likely tended to inhibit such firms from fraudulent behavior. For instance, in the early stages of the program the Treasury announced that a review of all PPP loans in excess of \$2 million would take place (see Hubbard and Strain, 2020).

C Representativeness of the ADP data

The ADP data used in this paper begin as a linked employer-employee panel. For analysis purposes in this paper, the data is converted into a panel of firm-week observations. Grigsby et al. (2019) show that the ADP employer-employee data are broadly representative with respect to firm size, average wage level, demographics of workers, hourly versus salaried status, and frequency of pay. Cajner et al. (2020a) and Cajner et al. (2018) show that a closely-related firm-level dataset from ADP is also broadly representative with respect

Table C.1: Summary Statistics as of February 2020

	All Sizes	250-500 Employees	
	PPP	PPP	ADP
Manufacturing	9.7	10.8	6.3
Wholesale Trade	4.9	5.0	9.6
Retail Trade	9.9	6.3	7.5
Financial Activities	5.0	4.0	10.5
Professional & Business	17.2	18.1	20.2
Education & Health	19.1	27.4	22.1
Leisure & Hospitality	2.8	3.2	2.1
Other	31.5	25.3	21.7

Note: Sample reflects firms that were present in the ADP data for all 12 months of 2019 and February 2020. NAICS 72 firms are excluded from both ADP and PPP loan-level data, and non-employer firms are excluded from the PPP loan-level data. Source: Authors' analysis of ADP data.

to industry composition, firm size, and geography, and, additionally, that the aggregate employment dynamics in the ADP data mirror the business cycle-frequency dynamics in the official data over the Great Recession and the subsequent recovery. [Cajner et al. \(2020b\)](#) show that indexes derived from the ADP data closely matched the dynamics of the Bureau of Labor Statistics monthly CES data following the onset of the pandemic-induced recession.

As a further indication of the representativeness of the ADP sample with respect to PPP loans, [Table C.1](#) gives the sectoral shares of employment in the universe of PPP loans as well as for firms with 250-500 employees, the universe we focus on in our paper. Within firms with 250-500 employees (the 3rd and 4th columns), the sectoral shares of employment are largely comparable, with a few exceptions, the largest of which is in the Financial Activities sector. It is also the case that the industry mix of ADP firms with 250-500 employees is broadly similar to that of all PPP-receiving firms (compare the 1st and 3rd columns). Overall, [Table C.1](#) suggests that the ADP data have broad coverage across sectors to a similar degree that PPP loans flowed across the economy.

Our empirical estimation sample is, by necessity, unrepresentative of PPP recipient firms on one key dimension: firm size. Because our empirical methodology compares firms in a region below the industry-specific PPP size eligibility threshold of 500 workers to a set of firms above this threshold, we by necessity focus on larger firms. The smallest firm in

any of our estimation samples has 250 employees. Most PPP-recipient firms were much smaller, however, as only 14% of the PPP’s 2020 loan volume went to firms with 250 or more employees.

As noted earlier, because virtually all firms in accommodation and food service (NAICS 72) were likely eligible for PPP loans—and we therefore lack an eligibility threshold with which to estimate an effect of the program—we omit that sector in all analysis. However, because most NAICS 72 firms are smaller than 250 employees, this restriction has only limited effect on our sample. For example, only 1.7% of PPP-recipient firms in 2020 were both in NAICS 72 and had 250 or more employees.

D Estimating Total Employment Effects

Calculating the aggregate employment effect of the PPP, E_t , requires an estimate of the number of employees at PPP-recipient firms, T ; see equation 3. Due to the variation in PPP-eligibility thresholds across industries, as well as take-up above 500, the calculation requires several data points from the SUSB employment data (corrected to account for employment growth from 2017Q1-2019Q4 and for data suppression as discussed above in Appendix B).

First, we calculate total employment across all industries below their respective PPP-eligibility thresholds. Because the SUSB data do not provide employment in the size bin 1,000–1,250 but rather for the bin 1,000–1,499, for industries with 1,250 worker thresholds we must add up all employment below 1,000 plus half of employment between 1,000-1,499. Additionally, we include all employment in NAICS 72, Accommodation and Food Services, since virtually all such employment was PPP eligible. We estimate that 64.1 million employees were at firms under PPP eligibility thresholds, which we multiply by our estimate of take-up of 89.9% for firms sized 1-499—see Appendix Table B.1—to find 57.6 million employees were at PPP-recipient firms under the industry-specific PPP eligibility thresholds.

Finally, we include employees at firms above the size threshold of 500 using our assumption that these firms were drawn from the bin 500-999. In total, this represents 5.7 million employees. Multiplying this by the take-up rate for firms between 500-999 of 26.6%, we find there were 1.5 million employees at PPP-receiving firms above the size threshold of 500.

Putting these pieces together, we estimate that there were 59.2 million workers at firms that received PPP loans. As a sanity check for this number, we calculate the total number of workers reported in the PPP loan-level data, assuming that those firms reporting 500 workers are on average the size of firms with 500-999 workers. This yields 61.6 million workers at PPP receiving firms, which is within 4% of our estimate using the SUSB data.

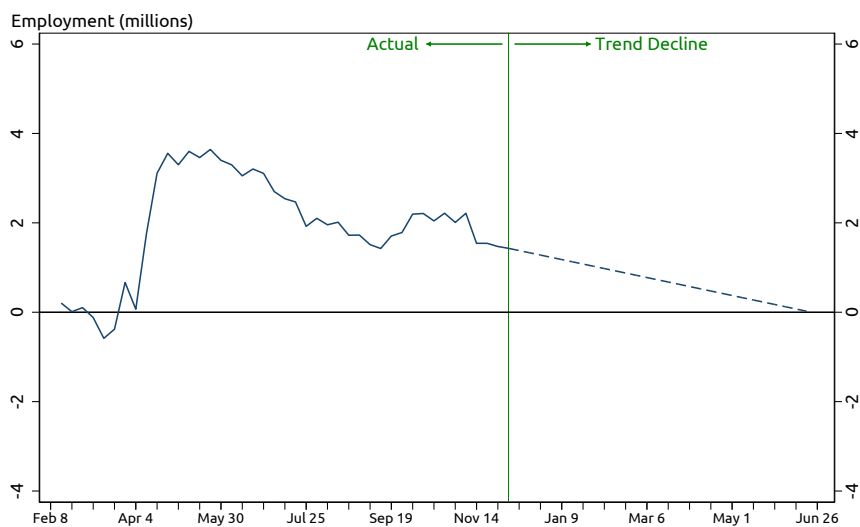
Figure D.1 displays the estimated aggregate employment effect of the PPP according to the methodology explained in Section 5. The blue dashed line presents the scenario in which the employment effect continues to decline according to its estimated trend from mid-May to the end of the sample period in early December. The trend decline scenario implies that the aggregate employment effect of the PPP reached zero by June of 2021.

This extrapolation appears plausible for two reasons. First, the trend decline from mid-May through December is nearly identical to the trend decline seen in the last several weeks of estimates. Second, the most recent CBO economic forecast estimates that the economy reached its potential in the middle of 2021, coinciding with when our trend decline scenario reaches zero. That this timing lines up is sensible since there is little scope for the PPP to boost employment with the economy at its potential.

E Eligibility Definition Robustness Analysis

In our main estimates, displayed in Figure 3, we determine PPP eligibility using *both* average 2019 employment and February 2020 employment, discarding firms whose PPP eligibility differs across these two measures. We choose this approach because we do not observe the actual data or rule used by firms to establish PPP eligibility. Requiring eligibility to be

Figure D.1: Aggregate Employment Effect



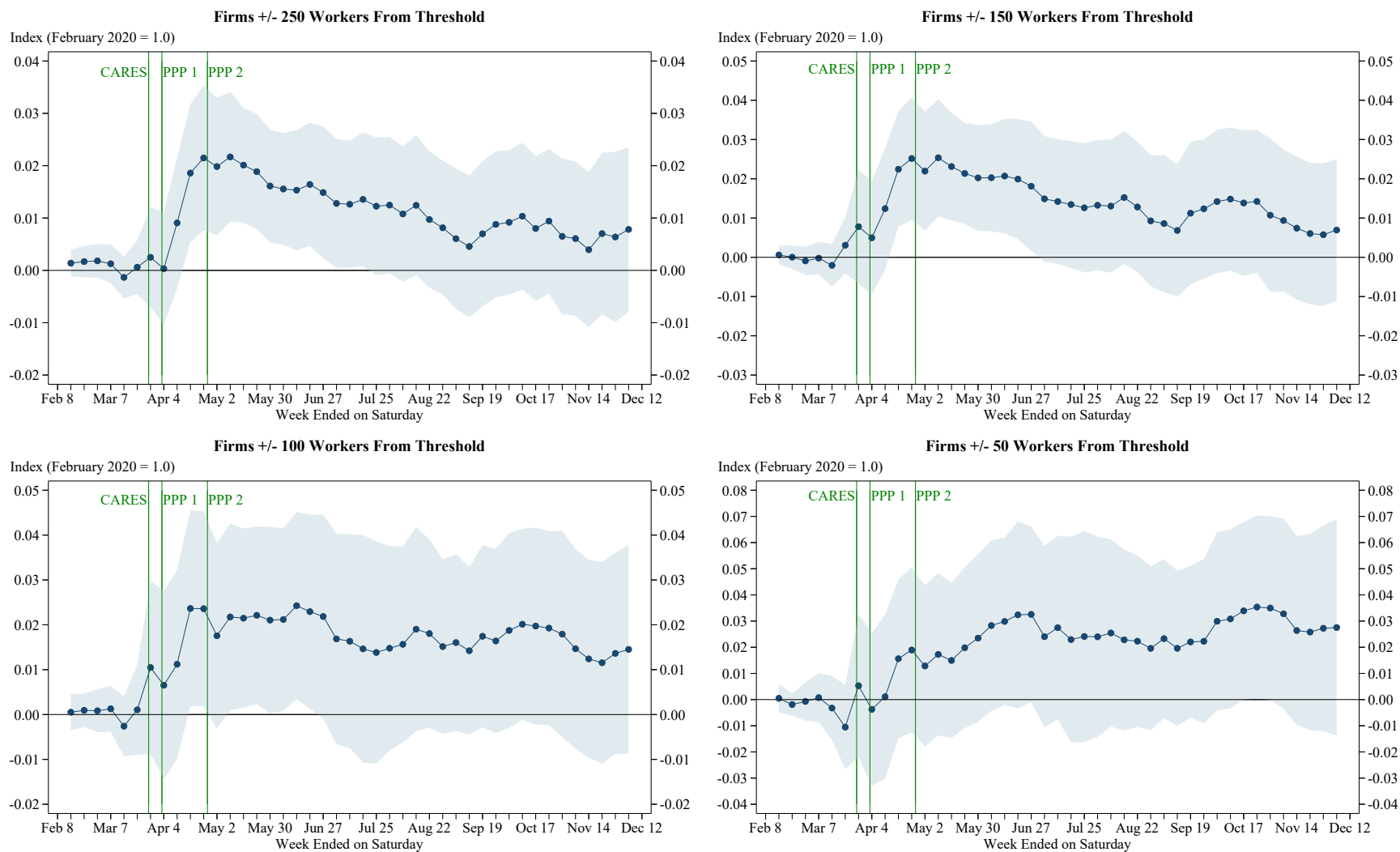
Note: The estimates shown here are calculated according to the methodology explained in Section 5.
Source: Authors' analysis of ADP data.

satisfied in both 2019 and February 2020 reduces the odds of spurious eligibility assignment.

We provide robustness checks for our PPP eligibility definition here. Figures E.1 and E.2 present the results of estimating equation (1) with PPP eligibility determined, respectively, by firm average employment in 2019 and by firm employment in February 2020. These estimates are similar to our main findings, with the caveat that those based on February 2020 employment show faster program fadeout. We suspect that assigning PPP eligibility using employment immediately prior to the pandemic provides a less reliable measure of true PPP eligibility, which may lead to downward attenuation bias.

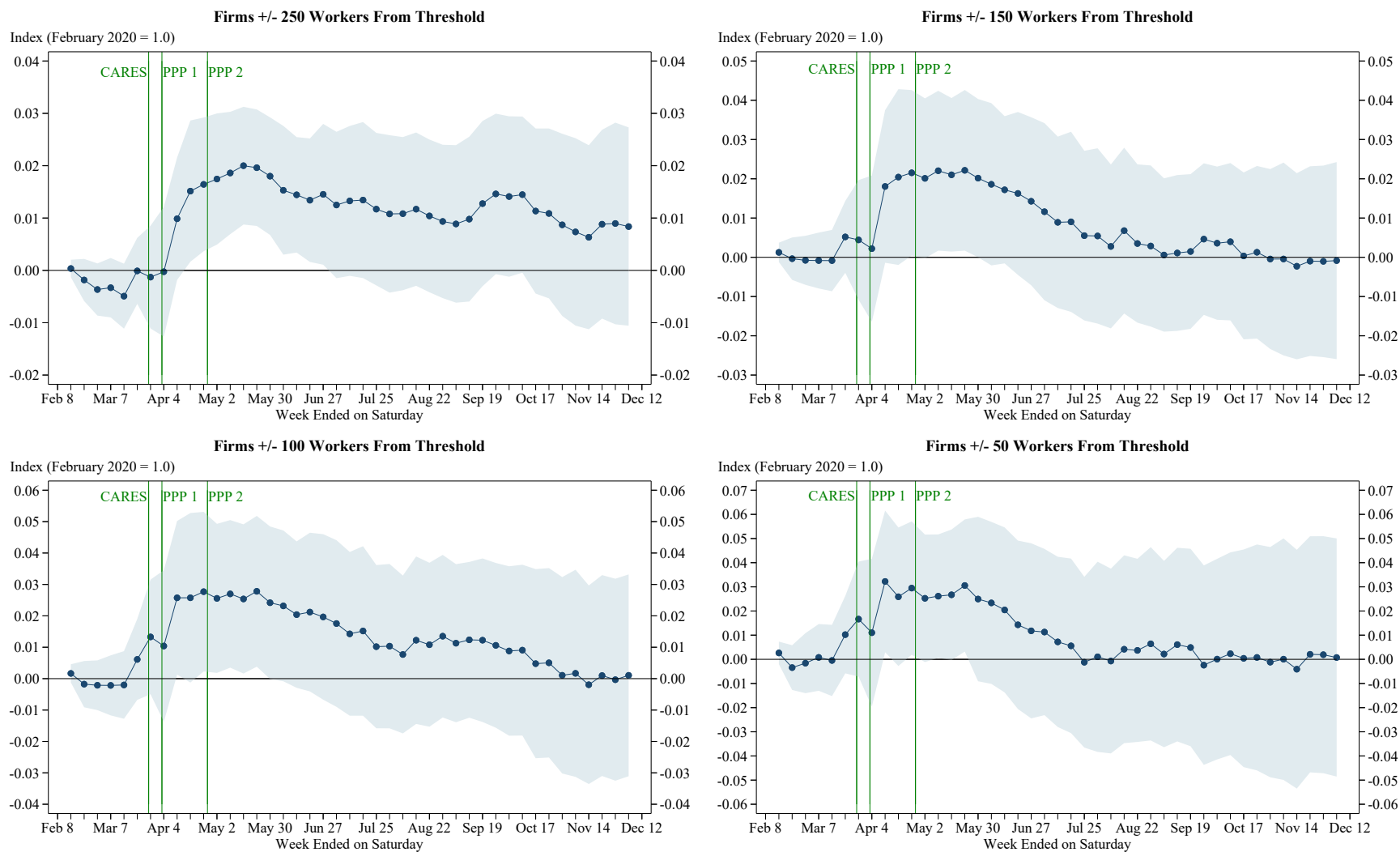
Figure E.3 defines firm size as the minimum of average firm size in 2019 and average firm size in the 12 months ending in March 2020. This is a rough approximation to the firm size specified in the PPP eligibility rules for non-seasonal firms. The results are again similar to our main findings.

Figure E.1: Effect of PPP Eligibility on Employment Based on Firm Size as of 2019



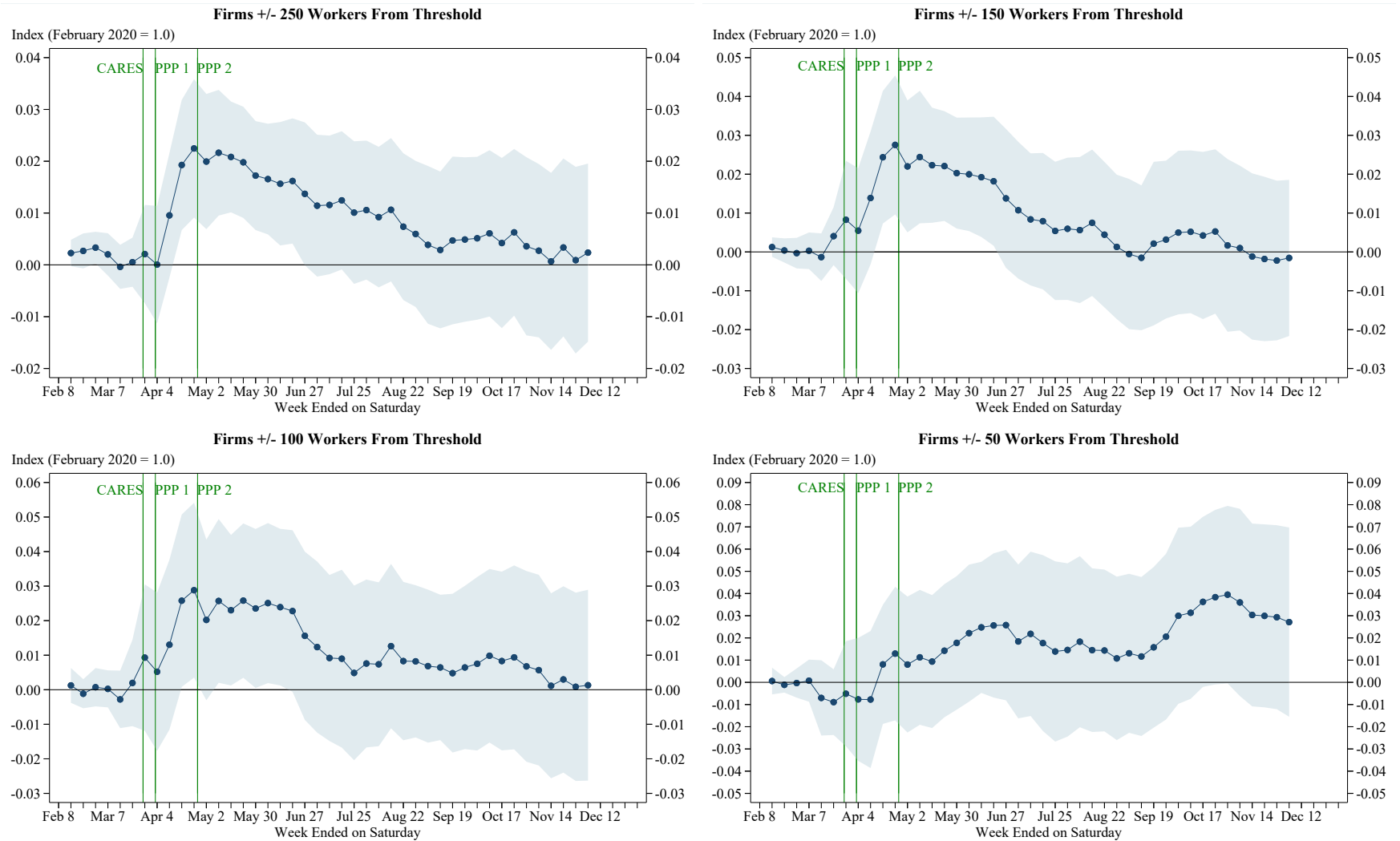
Note: Each firm's size is determined using average employment in 2019. Regressions are weighted by firm size as of February 2020 and include controls for state-by-week and industry-by-week effects. Standard errors are clustered at the 3-digit NAICS industry level. Sample reflects firms that were present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

Figure E.2: Effect of PPP Eligibility on Employment Based on Firm Size as of February 2020



Note: Each firm's size is determined using employment in 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.

Figure E.3: Effect of PPP Eligibility on Employment Based on Minimum of Firm Size as of February 2019 and Firm Size the 12 Months Ending in March 2020



Note: Each firm's size is determined using the smaller of employment in 2019 and employment in the 12 months ending in March 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.

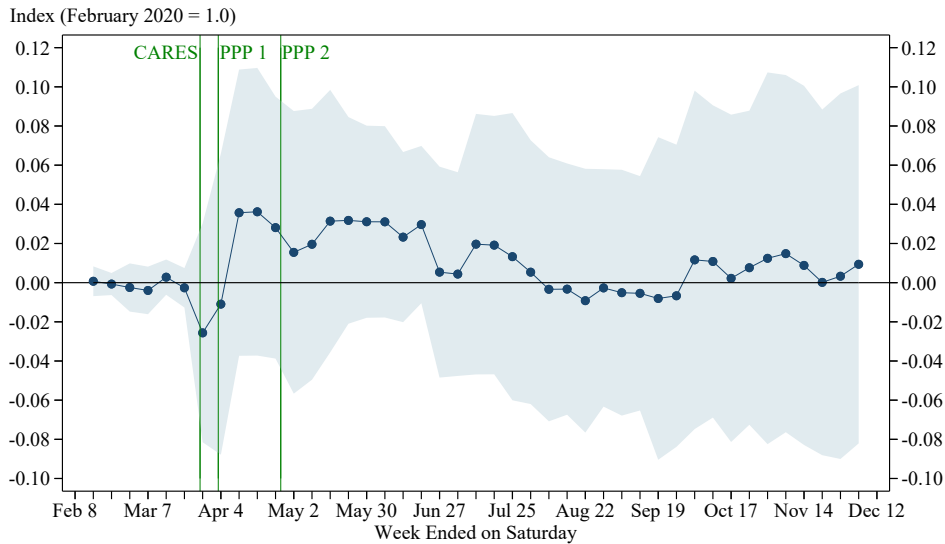
F The Main Street Lending Facility

The PPP was part of a suite of pandemic-era lending programs that collectively covered a large swath of the U.S. economy (Decker et al., 2021). Relevant to our analysis, firms somewhat too large to receive a PPP loan were potentially eligible instead for a loan from the Federal Reserve’s Main Street Lending Facility. Unlike PPP loans, Main Street loans were not forgivable. Since these firms comprise our control group, this could complicate the interpretation of our estimates of the PPP’s effects on employment. Because utilization of the Main Street program was extremely low relative to the PPP, it is unlikely that lending under this program had much influence on our results. The Main Street facility extended only 1,830 loans valued at a total of \$17.5 billion; moreover, half of the dollar volume of Main Street loans were made in December of 2000—the final month Main Street operated and the tail end of our sample period (Bräuning and Paligorova, 2021). In contrast, the PPP extended 5.2 million loans valued at around \$525 billion in 2020.

G Treatment Effect Estimates for Placebo Industries

This appendix section presents treatment effect estimates for the sample of industries used to estimate the placebo test in Figure 4—that is, industries with above-500 eligibility thresholds. In contrast to the placebo test, where placebo PPP eligibility is determined based on a 500 threshold, here PPP eligibility is defined based on the actual industry-specific threshold. The results, displayed in Figure G.1, are broadly similar to our primary results in Figure 3, although the PPP treatment effect fades out somewhat faster than seen in our primary estimates. These estimates are quite imprecise, however, likely reflecting a very small sample size. (The placebo test—which employs the same industries but smaller firms—has a considerably larger sample size.) We view the similarity between the point estimates in Figure G.1 and our primary results in Figure 3 as suggesting the very small placebo treatment effects in Figure 4 are informative and supportive of our empirical model’s identifying assumption.

Figure G.1: Effect of PPP Eligibility on Employment for Firms With PPP Eligibility Above 500



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. The sample is restricted to firms with a PPP industry-specific eligibility threshold above 500 and within +/- 250 employees of the threshold; those firms below the industry-specific threshold form the treatment group and those above the threshold form the control group. The sample contains only firms that were present in the ADP data for all 12 months of 2019.

Source: Authors’ analysis of ADP data.

H Additional Outcomes: Total Hours and Firm Closure

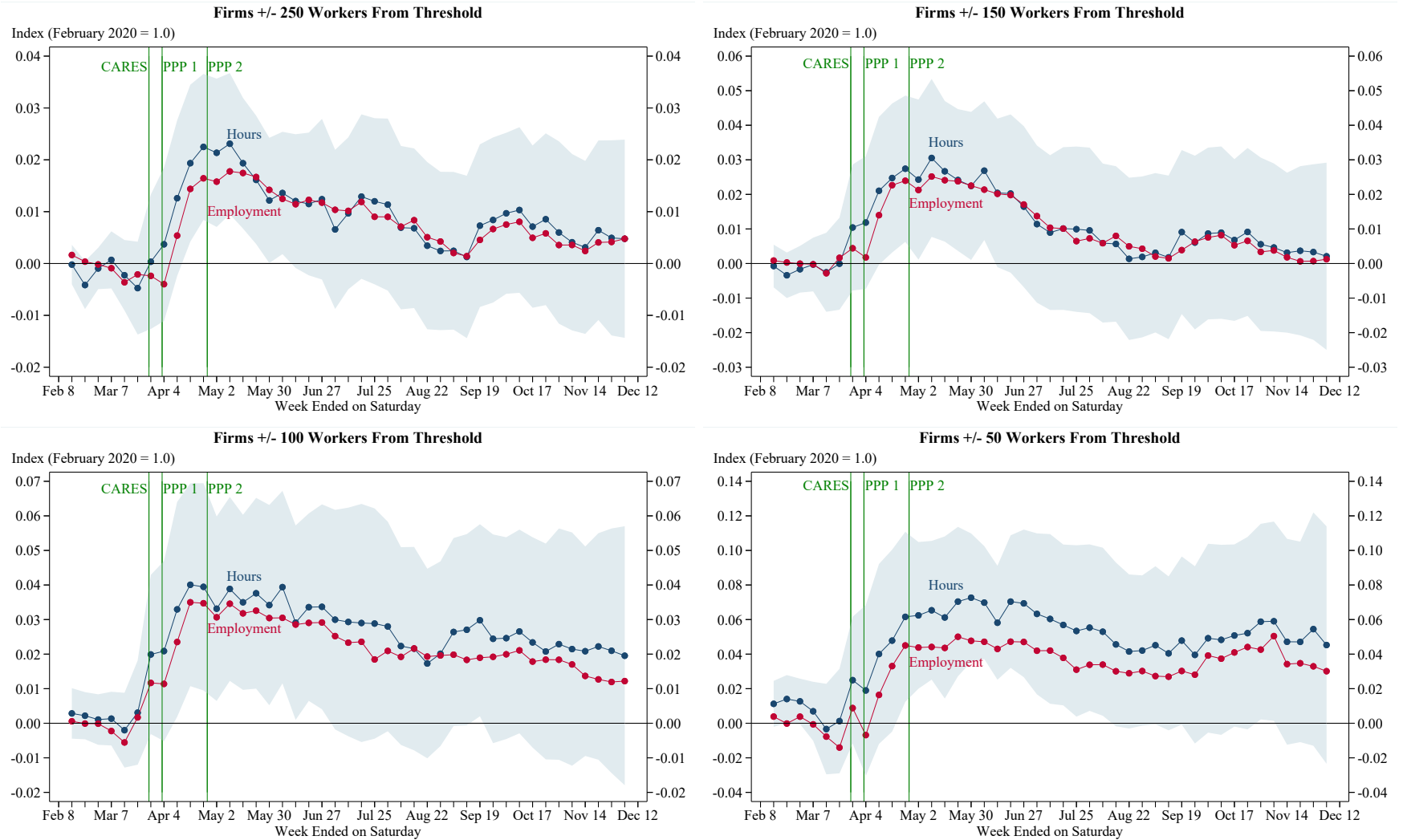
This appendix section discusses results for additional outcomes beyond employment—specifically, total hours and firm closure—that allow for a more nuanced interpretation of the employment results in Figure 3. Figure H.1 presents estimates of equation (1) with total hours as the dependent variable. The estimates show roughly the same pattern and magnitude as the treatment effect for employment from Figure 3.⁴ This similarity suggests that the effect of PPP on employment was largely on the extensive margin rather than the intensive margin of hours adjustment. This extensive margin adjustment could reflect either firm closures or employment adjustments (e.g. layoffs) among open firms. Utilizing the same ADP sample of firms and research design used here, Autor et al. (forthcoming) find little evidence of a PPP

⁴The ADP measure of hours refers to hours paid rather than hours worked. It is therefore possible that firms reported that they were paying workers for the same hours, but, potentially for reasons related to shutdown orders, did not require work schedules of the same length.

effect on firm closure as measured by the number of firms with no paid employees.⁵ Thus, the evidence here suggests that the employment effects in Figure 3 likely reflect extensive margin employment adjustments at firms that remained open.

⁵Using an alternative methodology, [Autor et al. \(forthcoming\)](#) find that the PPP prevented the closure of firms smaller than those examined in this paper.

Figure H.1: Effect of PPP Eligibility on Employment and Total Hours



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. Displayed confidence intervals are for the total hours outcome. Sample reflects firms that were present in the ADP data for all 12 months of 2019. Source: Authors' analysis of ADP data.

I Pre-Trend Robustness

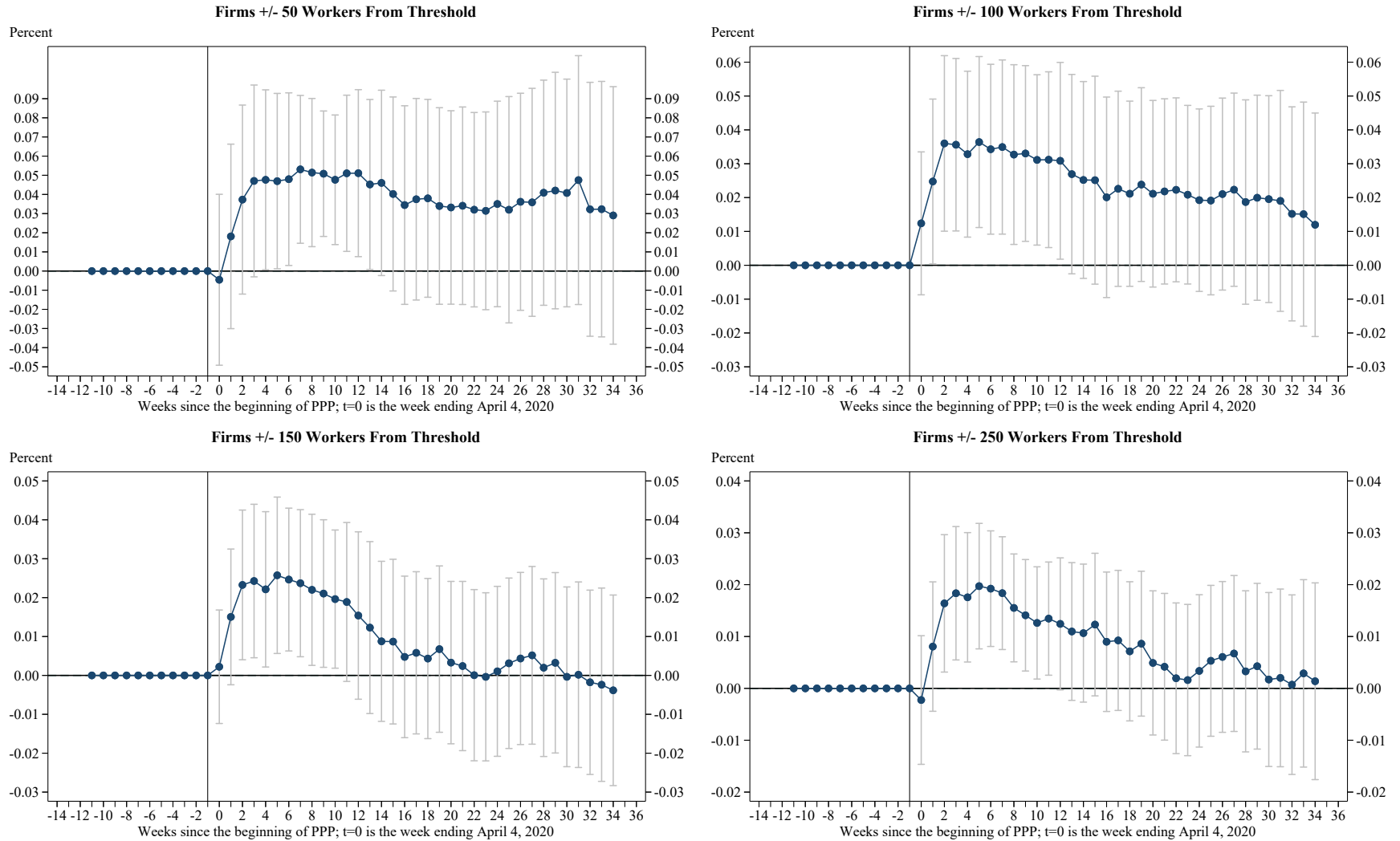
While the estimates in the pre-PPP period in Figure 3 are nearly all statistically insignificant, in the panels other than the top-right the estimates show some evidence of a downward trend prior to the PPP. This downward trend in the pre-treatment estimates suggests that the PPP might have had a somewhat larger effect on employment than indicated by a straight read of our post-treatment coefficients.

In this section, we adopt a robustness check following Freyaldenhoven et al. (forthcoming) and Dobkin et al. (2018). We first estimate a linear pre-trend, corresponding to the trend difference in employment between PPP-eligible and ineligible firms in the pre-PPP period. We then subtract the extrapolated pre-trend from the post-PPP treatment effects to generate estimates purged of the pre-trend. The resulting treatment effect estimates, shown in Figures I.1 and I.2, correspond to *deviations* from the extrapolated time trend in the post-PPP period. In Figure I.1 we use the period from the beginning of our sample through April 4 as the pre-period, while in Figure I.2, we define the pre-period as ending on March 28; the former date was at the time the PPP began and the latter was at the time the CARES Act was passed and thus both are reasonable proxies for the beginning of the PPP treatment period. We implement this approach using the Stata package provided by Freyaldenhoven et al. (forthcoming) “xtevent”, defining event time as zero in March 28 or April 4 for all PPP-eligible firms and defining event time as equal to zero for ineligible firms in all periods. We include the same set of sample restrictions and fixed effects as in our baseline specification.

Accounting for the estimated pre-trends does little to change the nature of our results. First, regardless of the start date of treatment, March 28 or April 4, the results are quite similar. Second, the point estimates of the PPP treatment effect are broadly similar to those in our baseline results shown in Figure 3. At their peak, the results in Figures I.1 and I.2 suggest a PPP effect on eligible firms of between roughly 2 and 5 percent, comparable to our baseline estimates. Finally, similar to our baseline estimates, the effect of PPP is estimated to attenuate over the course of 2020, and ends our sample with an effect between 0 and 3

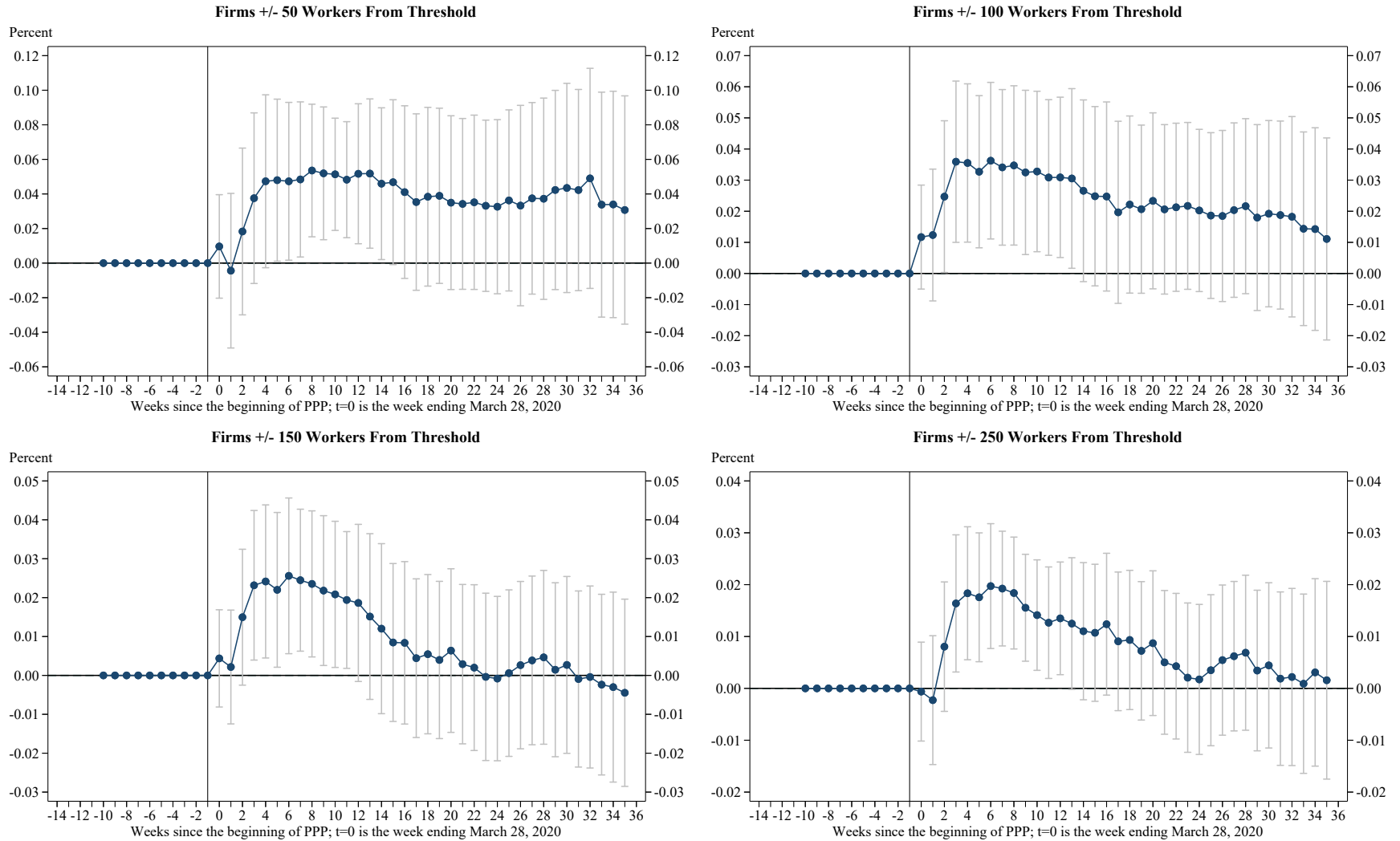
percent.

Figure I.1: Freyaldenhoven et al. Linear Pre-Trend Estimates through April 4



Note: Estimated using “xtevent” provided by [Freyaldenhoven et al. \(forthcoming\)](#) with a linear pre-trend estimated from January 11 through April 4. 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 includes only firms that were present in the ADP data for all 12 months of 2019. Source: Authors’ analysis of ADP data.

Figure I.2: Freyaldenhoven et al. Linear Pre-Trend Estimates through March 28



Note: Estimated using “xtevent” provided by [Freyaldenhoven et al. \(forthcoming\)](#) with a linear pre-trend estimated from January 11 through March 28. 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.