

The Paycheck Protection Program: Progressivity and Tax Effects

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Abstract

The \$800 billion Paycheck Protection Program (PPP) provided pandemic relief to businesses retaining employees. Prior research has not directly estimated the PPP's distributional or tax effects. Linking PPP loans to tax records, we estimate progressive effects with respect to income for both workers and business owners. Bottom-quintile incomes increased 18 percent and top-quintile incomes increased 2 percent. About half of PPP relief benefitted workers. The PPP also increased taxes and decreased unemployment compensation, reducing net program costs by one-quarter. Net costs could have been even lower (and progressivity higher) without the tax exclusion of PPP forgiveness.

Keywords: PPP, Covid-19, employment protection, countercyclical policy, stimulus checks, unemployment insurance

JEL: E24, H22, H81

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During the pandemic nearly \$800 billion was spent to retain U.S. workers through the Paycheck Protection Program (PPP). The PPP funded small business loans that were fully forgiven if businesses spent a sufficient share on employee payroll costs. As with any business-level subsidy or tax, the distributional impact is opaque. We provide estimates of PPP incidence using administrative tax data, linking PPP loan data to business tax returns and then linking businesses to the tax returns of employees and owners.

Our estimates indicate the PPP was progressive with respect to income: bottom-quintile incomes increased 18 percent, middle-quintile incomes increased 4 percent, and top-quintile incomes increased 2 percent. Based on differences in the timing of PPP loan receipt, we estimate that about half of PPP loan forgiveness went to worker-retention costs. Our estimated progressivity of the PPP, however, is relatively *insensitive* to the worker share because owners and workers at PPP-receiving businesses occupied similar parts of the income distribution. Despite the progressivity of PPP loan forgiveness, we find that 44 percent accrued to the top quintile of income earners. This estimate is a smaller than previous estimates in the literature.¹ Our progressive PPP result is robust to alternative assumptions—even a counterfactual where we assign all the relief to business owners and none to affected workers. That said, we do find that the PPP was less progressive than other major relief programs during the pandemic, which is consistent with the PPP’s policy goal of retaining employees regardless of distributional consequences.

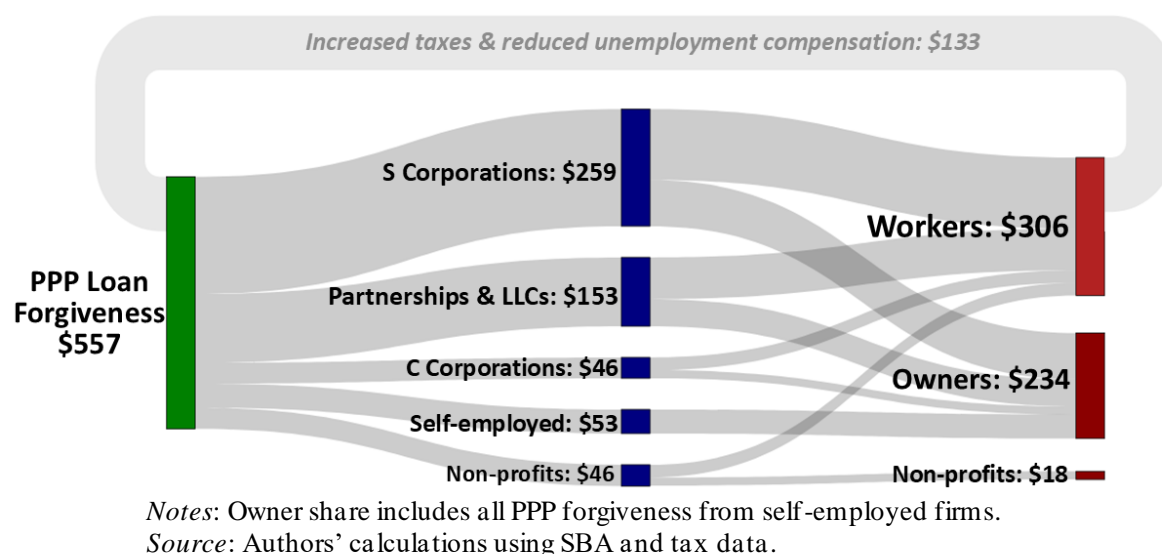
Analysis of the PPP’s economic incidence has been complicated by it being a subsidy targeted at workers but allocated through employers. Using links between PPP-receiving businesses and the tax returns of their workers and owners, we present a novel assessment of the PPP’s incidence. The current view is that the PPP was “highly regressive” and had a “substantial” cost per job-year retained (Autor et al. 2022a, p. 56). In contrast, we find the effects of the PPP on individual incomes were progressive.² More congruent with prior estimates, our estimated average gross cost per job-year retained by the PPP (\$133,000 to \$146,000) was about three times median annual earnings. This does seem substantial, but is similar to estimated costs for Great Recession job-retention programs (Cho 2018; Chodorow-Reich 2019).

We also find that PPP costs were partially offset by positive fiscal externalities. This results from retained workers paying higher income taxes and receiving less unemployment compensation compared to a counterfactual where their employer did not receive PPP loans. These fiscal externalities reduced the PPP’s net fiscal cost by one-quarter and had little effect on progressivity. Our baseline estimates show fiscal externalities reduced the cost per job-year retained from a gross cost of \$133,000 to a net cost of \$101,000.

¹ Autor et al. (2022a) estimated a top-quintile share of 72 percent.

² This paper applies a consistent framework to evaluate progressivity—PPP relief as a share of income—allowing comparisons of economically equivalent tax reductions and transfers.

Figure 1: Flow of first-draw PPP forgiveness to workers and owners (\$ billions)



Our paper contributes to the literature on PPP effects in several ways. Most importantly, we leverage tax data to provide a richer and more accurate picture of the PPP's distributional impacts. Second, by observing both taxes paid and unemployment compensation received by workers, we estimate the extent to which PPP's loan forgiveness costs were offset by higher taxes and lower unemployment payments. These positive fiscal externalities saved or returned an estimated \$133 billion of the program's first-draw costs to the government, as seen in Figure 1.³ Over \$80 billion in additional taxes could have offset program costs by making PPP forgiveness amounts taxable. Taxation of forgiven loans would have also further increased the PPP's net progressivity. Third, we account for non-wage payroll costs ignored in prior estimates, suggesting 55 percent of PPP loan forgiveness went to employee-retention costs. This estimated worker share is near the loan forgiveness requirement that 60 percent go to payroll costs.⁴ Fourth, we are the first to apply differences-in-differences methodologies to tax microdata, producing estimates of job-years saved that are consistent with prior work (Autor et al. 2022b; Dalton 2023). Finally, we use administrative data to provide the first consistent estimates of the distributional impacts of the three largest pandemic fiscal relief programs.

Thus, our estimates address two concerns of policymakers. First, they show the different distributional impacts of major recession programs—those helping workers keep their jobs (PPP), those helping workers who lost their jobs (unemployment compensation), and those supporting economy-wide demand (stimulus payments). The PPP had progressive impacts relative to income, but its progressivity was much less than that of

³ “First-draw” loans were the initial loans to eligible firms, mostly in 2020, accounting for the majority of PPP loans. Firms exhausting the first draw and suffering a 25 percent revenue reduction were eligible for a “second draw” in 2021.

⁴ Despite this similarity, our estimates are based on economic incidence, not statutory incidence. The original 75-percent forgiveness requirement was retroactively reduced to 60 percent on June 5th, 2020.

unemployment compensation or stimulus checks. Second, our estimates show the degree to which employee-retention programs can have direct fiscal feedback effects that offset deficit spending. As other pandemic programs were only partially taxable (unemployment compensation) or not taxable (stimulus checks), accounting for fiscal feedback effects provides a more apples-to-apples comparison of different policy options (Elmendorf, Hubbard, and Williams [forthcoming](#)).

Our analysis has a number of limitations. Accounting for indirect effects, such as consumption responses or employee scarring, would provide a more wholistic comparison but is beyond the scope of this paper.⁵ Considering longer-run and macroeconomic effects would also change the PPP's overall estimated impact. Benefits may be understated if the PPP prevented firm-specific human capital losses or costs from restarting businesses that would have closed. Further, if the PPP prevented a sustained recession and contributed to the post-Covid recovery, this could imply additional benefits. Conversely, benefits may be overstated if the PPP preserved less productive employment relationships and prevented the formation of more productive employment matches. Finally, our estimates are for a pandemic-driven recession and the applicability to standard downturns is unclear.

Section I discusses the policy background and reviews related studies. Section II describes the PPP loan data, the tax data, and the linking method. Section III presents the estimated effects of PPP loans on employment and wages. Sections IV and V show the PPP's distributional effects and fiscal externalities.

I. PPP Policy Background and Related Literature

Nearly \$1 trillion of relief supported U.S. employee retention during the pandemic. Most was from \$800 billion of PPP loans.⁶ The PPP was created by the Coronavirus Aid, Relief, and Economic Security Act in late March 2020 and was expanded and extended through March 2021. Eligible firms generally had no more than 500 employees, met Small Business Association size standards for their industry, or met an asset and net income test.⁷ For each eligible firm, original first-draw loan amounts were for up to 2.5 months of average payroll costs (after capping each employee's annualized pay at \$100,000) and a total loan of \$10 million. There were 8.6 million first-draw loans totaling \$589 billion, of which 95 percent was

⁵ Rose and Shem-Tov (2023) estimated cumulative earnings losses of \$40,000 among full-time workers losing jobs in firm-wide contractions. Davis and von Wachter (2011) estimated average male losses of 2.8 years of earnings in high-unemployment mass-layoff events. Gertler, Huckfeldt, and Trigari (2022) found the PPP increased recalls from temporary layoffs. Due to reduced scarring, Sledz (2025) found benefits from optimal PPP-like policies and Du (2024) found more permanent layoffs (fewer recalls) during Covid would have caused large and sustained GDP effects.

⁶ Additionally, over \$150 billion of employment retention tax credits targeted smaller businesses with gross receipts declines (Internal Revenue Service 2023) and nearly \$30 billion of grants went to businesses with the Restaurant Revitalization Fund. Other employer-based programs included sick and family leave tax credits and payroll tax deferral (Goodman 2021). Dilger, Lindsay, and Lowry (2021) provide PPP policy details.

⁷ See SBA's "[FAQs for Borrowers and Lenders](#)" for the PPP and small business standards at www.ecfr.gov/current/title-13/chapter-I/part-121.

forgiven. Second-draw loans totaling \$209 billion were distributed in 2021 to firms with no more than 300 employees and with revenue decreases.

In this paper, we focus on the \$557 billion of first-draw PPP loan forgiveness.⁸ Firms were generally eligible for loan forgiveness if they maintained pre-Covid employment and wages, had payroll and other eligible expenses—e.g., rent, mortgage, utilities—that reached the loan amount, and had payroll costs that were at least 60 percent of the loan amount. These requirements highlight the intent of retaining employees and preserving wage levels, as well as preventing business closures by allowing some non-payroll expenses.

A. Prior PPP Analysis

PPP research has focused on estimating the share of loans spent on employee wages. Using private payroll data, Autor et al. (2022a) estimated that up to 34 percent of the PPP went to wages. Using population-level administrative data, Dalton (2023) estimated that 43 percent of the PPP went to wages. Using similar methods, but with firm-level quarterly tax filings, we estimate that 46 percent went to employee wages. Including missing tax and health insurance payments for employees, suggests 55 percent of PPP forgiveness went to employee-retention costs.⁹

Estimating the cost of retaining employees is another major research question. We estimate a PPP cost per job-year retained of \$133,000 to \$146,000. This is similar to prior estimates: \$141,000 (Dalton 2023); \$169,000 when accounting for smaller firm effects (Autor et al. 2022b); and \$175,000 based on regional differences in loan timing (Granja et al. 2022). These estimates usually suggest the PPP retained about 3.5 million job-years, similar to our estimate of 3.8 to 4.2 million job-years retained. Switching the focus to saved jobs, which can be shorter duration than a year, suggests significantly lower costs of \$34,300 (Faulkender, Jackman, and Miran 2023) and \$32,000 to \$67,000 per job saved (Bartik et al. 2021).¹⁰ These estimates imply the PPP saved over 10 million jobs, a relevant estimate if the PPP's primary goal was to maintain employer-employee relationships. This large number of estimated jobs saved should be placed in context, as 45 million workers received unemployment insurance in 2020 (Larrimore, Mortenson, and Splinter 2023a).

Prior estimates sometimes relied on an alternative estimation strategy, specifically, comparing large firms just above and below the firm-size cutoffs for PPP eligibility, usually at 500 employees (e.g., Chetty et al. 2023). Although this approach theoretically provides a valid control group, it can have several sources of bias. First, because larger firms

⁸ Values are based on Small Business Association (2023). Some first-draw loans were in calendar year 2021. First-draw forgiveness is therefore prorated to \$500 billion in this paper's estimates of 2020 income distributions.

⁹ Autor et al.'s results average across four ranges around employee firm-size discontinuities and the narrow range, often preferred for regression discontinuity estimates, yields an estimate similar to our estimated employment effects (see the appendix).

¹⁰ Doniger and Kay (2023) focus on spring 2020, when jobs were likely most sensitive to PPP funding. Their estimates suggest a marginal cost-per-job-saved of only \$17,000.

were less negatively affected by the pandemic than smaller firms, using larger firms as a control group can systematically bias down the estimated number of jobs saved.¹¹ Moreover, the \$10 million cap on PPP loans was binding for many large firms, attenuating job-retention efficacy for larger firms. Additionally, tax data suggest there was no treatment discontinuity at 500 employees and firm sizes were strategically misreported in SBA applications, bunching just below the eligibility threshold (appendix Figure A9). Thus, firms with more than 500 employees frequently received first-draw PPP loans, likely due to various exemptions and inconsistent enforcement.¹² Autor et al. (2022a, 2022b) used firm-size cutoffs, but their job-year retained cost estimates are similar to our estimates. This is because Autor et al. directly accounted for industry-based size exemptions, made adjustments for other exemptions, and applied a correction to address the treatment contamination issue around firm-size cutoffs. To address concerns about small firms, Autor et al. (2022a) estimated an event-study analysis like that used here and found small-firm employment effects of about 6%, as compared to our estimates of 7%.

Our analysis also enables a more accurate distributional analysis than prior work. Autor et al. (2022a) estimated business owner distributional effects assuming a similar distribution to that of capital income. But capital income largely results from dividends and capital gains of large firms that were ineligible for PPP loans. In contrast, our direct links between businesses and their owners reveal a significant number of PPP loans going to low-income owners, including many self-employed individuals. Also, Autor et al. estimated worker distributional effects by allocating counterfactual wage declines across the income distribution to match observed pandemic-year employment losses. In contrast, we use employer-employee linked wage data to produce more targeted estimates across firm size and worker income distributions. Our results suggest a higher share of retained wages went to low-income workers at smaller firms. This is consistent with the PPP's targeting of smaller firms. Finally, we use standard pre-tax (rather than after-tax) incomes to estimate distributional effects.

Other research considered PPP's impact on firm closures, additional uses of PPP loans, and the need to access these loans through financial institutions. Hubbard and Strain (2020), Bartik et al. (2021), Kurmann et al. (2022), Autor et al. (2022b), and Dalton (2023) found evidence of reduced business closures from the PPP. This firm-preservation effect contributes to preserved wages. Granja et al. (2022) showed that banks influenced PPP take

¹¹ Faulkender, Jackman, and Miran (2023) found firms with more than one hundred employees had less than one-fifth the effect on job preservation relative to smaller firms. Dalton (2023) found large firms had one-quarter the average effect on wages. Cole (2024) estimated that medium to large firms had one-quarter to one-half the employment effect of small firms.

¹² Beggs and Harvison (2023) found evidence of employee number misreporting, as the PPP application process essentially used the "honor system." Faulkender, Jackman, and Miran (2023) found many large firms without exceptions still received PPP loans and discussed size-threshold approach issues. This paper includes two indirect indications of fraud. First, the inability to match seven percent of PPP loan forgiveness by the name and location of the firms gives a possible amount going to fabricated firms. Second, the appendix shows bunching of self-reported firm sizes at exactly 500 or just below in the PPP data, which suggests fraud along this margin.

up and that firms used PPP loans for buffer savings and non-wage expenses. Hubbard and Strain (2020) showed the PPP helped firms pay bills on time. Gorbachev, Luengo-Prado, and Wang (2024b) found the improved financial condition of PPP receiving firms aided continued operation but not later expansion. The PPP also helped small businesses access other credit (Karakaplan 2021), but this increased credit access was temporary (Mueller and Spiegel 2023), was sensitive to prior banking relationships and firm size (Neilson, Humphries, and Ulyssea 2020; Li and Strahan 2021; Duchin et al. 2022), had higher likely misreporting among fintech lenders (Griffin, Kruger, and Mahajan 2023), and when controlling for other characteristics, black-owned firms were less likely to apply and be approved for PPP loans (Chernenko et al. 2023). Hamilton (2020) called for implementing worker-retention programs through the IRS, which could disperse funds more directly and assess loan forgiveness eligibility. Smart et al. (2023) studied the Canadian pandemic wage subsidy. Strain and Veuger (2023) and Hong and Lucas (2023) compared Covid-era credit programs across developed countries.

II. Data and Linking PPP Loans to Employers and Owners

PPP loan amounts and forgiveness amounts for each firm come from the Small Business Administration (SBA 2023). We link these SBA data to Employer Identification Numbers (EINs) in the tax data using firm names and addresses. Each firm’s EIN is linked to their workers and owners to allocate the respective portions of the PPP. Population-wide income data is used to estimate the PPP’s distributional effects on both workers and owners.

A. Linking PPP Loans to Firms’ Tax Filings

In the tax data, addresses, names, and firm EINs are reported on various forms depending on the nature of the entity. These forms include quarterly payroll tax filings from employers (Form 941) and annual entity-level tax returns (e.g., Forms 1120-S and 1065 for S corporations and partnerships). For sole proprietorships, name and address information are reported on Schedule C of individual tax returns. For SBA and tax data, the same procedure cleans names and addresses. We link the SBA and tax data sequentially, beginning with exact matches on address and name. Next, fuzzy matches use a similarity measure of the combination of address and name. These matches begin at the zip-code level and sequentially expand to the city, county, and state levels (appendix Table A1). To benchmark the validity of these matches, we compare matched PPP loan forgiveness with certain PPP amounts captured in tax data and find similar results (appendix Figure A2).

For first-draw loans, 93 percent of PPP forgiveness amounts are matched to a firm’s EIN. The SBA data includes self-reported entity types and match rates are highest for C and S corporations (97 percent), which represent about half of PPP forgiveness amounts. Match rates are lowest for sole proprietorships, self-employed, and independent contractors (67 percent) but these represent only about one-tenth of PPP forgiveness amounts. To adjust

for differential match rates, estimates are reweighted by the inverse match rate for each entity type. For our regressions in the next section, firms with matched EINs are linked to Forms 941, which report firms' quarterly employee counts and wages. Overall, these firm-level matches capture 74 percent of first-draw PPP forgiveness amounts in the SBA data, as they exclude self-employed PPP recipients with no employees.

B. Linking Firms to Workers and Owners

We match each firm to its employees and owners. Employees are matched to all their employers based on the EINs of Forms W-2 reporting wages of at least \$250. Among firms for which we match to an EIN, firms linked to Form W-2 wages represent 72 percent of PPP forgiveness amounts. For employees of a given firm, we aggregate unemployment compensation recipient counts and amounts observed on Form 1099-G to firm-level.

Owner portions of PPP loan forgiveness are allocated proportional to each owner's share of the firm. Sole-proprietorship owners, including the self-employed and contractors, are identified with Schedule C of individual tax returns. C-corporation owners are identified with Schedule G of Form 1120, which reports the individuals or entities owning 20 percent or more of the corporation's voting stock, along with their ownership shares. S-corporation and partnership owners and ownership interests are identified with Schedules K. Tiered ownership of partnerships are based on Love (2021). Among firms for which we match to an EIN, these links account for 68 percent of PPP forgiveness. Non-profits cannot be linked to owners and explain nearly one-third of the incomplete matches. Some incomplete matches are also due to owners without individual tax returns, such as trusts and foreigners, or C-corporation owners with small shares of the firm. To account for incomplete links, we gross up the matched distributions for each SBA-based entity type to fully account for the PPP's worker and owner portions. Finally, we use taxpayer identification numbers of workers and owners to match them to their respective individual tax returns and information returns. This allows us to assign each worker and owner to an income group.

C. Income Groups

Workers and business owners are placed into fiscal income groups. *Fiscal income* is essentially market income observed in tax data plus Social Security benefits. This income definition parallels other studies using tax data (Larrimore, Mortenson, and Splinter 2021; Congressional Budget Office 2022). For tax return filers, fiscal income is adjusted gross income with the following changes: add adjustments, nontaxable interest, and nontaxable Social Security benefits; remove taxable unemployment compensation (most was already excluded from taxable income in 2020) and negative other income; and replace taxable retirement income with total distributions less rollovers. For non-filers, fiscal income includes wages, dividends, interest, some miscellaneous income, private retirement income, Social Security benefits, and partnership income. These amounts come from various third-party reported information returns. Following a common approach in income distribution

studies, we estimate income-group thresholds and totals after bottom-coding incomes at zero, size-adjusting incomes for ranking, and creating groups based on the number of individuals. This resembles the U.S. Census’ equivalized-income distributions and estimates using tax data (see appendix Table A3).

III. Effects on Employment and Wages: Dynamic Difference-in-Differences Analysis

We first estimate the effects of the PPP on employment and wages using the dynamic difference-in-differences estimator developed by Callaway and Sant’Anna (2021). This estimator is used to estimate job and wage changes in similar settings (Miller 2023). Our approach and sample restrictions, broadly follow Dalton (2023), and are described in detail in the appendix.¹³ We estimate an average treatment effect on the firms receiving PPP loans by comparing firms receiving the treatment against firms yet to receive the treatment. More precisely, we compare proportional changes in employment or wages:

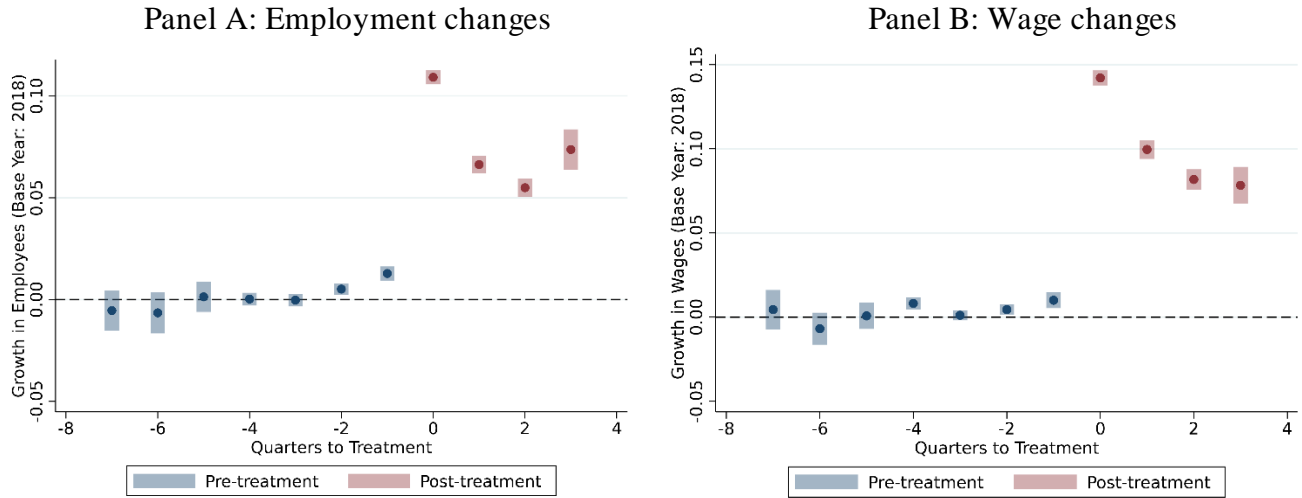
$$Y_{i,q,t,s,j,f} = \frac{y_{i,q,t,s,j,f}}{(y_{i,q,t,s,j,f} | t=2018)} \quad (1)$$

In Equation 1, $y_{i,q,t,s,j,f}$ is the employment or wages in firm i , quarter q , year t , state s , two-digit industry code j , and firm-size f . To estimate proportional changes, each firm’s employment or wages is divided by its value in the same quarter in 2018. We further “demean” the dependent variable by the average employment or wage growth for state, two-digit industry code, and firm-size groups (s,j,f) to control for geographic variation in Covid-related employment shocks and state-level policy. Firm sizes are based on the minimum employment in any quarter in 2018.

To measure employment and wages for outcome variables, we use Form 941 quarterly payroll filings from 2018 to 2021. Form 941 data presents an advantage over self-reported PPP data, which is subject to misreporting (appendix Figure A9). The regression sample is limited to firms with Form 941 quarterly observations that were matched to first-draw PPP loans in the SBA data. This limits the identifying variation to differences in the timing of first-draw PPP receipt, with treatment mostly occurring in the second and third quarters of 2020 but sometimes in the first and second quarters of 2021. While this approach’s identifying variation could be problematic to the degree timing of PPP receipt is endogenous to wage and employment growth, this approach avoids the limitations of the size-threshold approach discussed in section I.A. Moreover, this approach shows clear effects in the quarter of treatment, small pre-trend effects, and similar results to prior methods using various methods.

¹³ Firms are dropped if they: (1) had zero or one employee in any quarter of 2018 or 2019, (2) had no valid industry classification code, (3) had no observed state, (4) had an industry classification of public administration or utilities due to ineligibility, (5) are restaurants because of simultaneous industry-specific relief, or (6) never received first-draw PPP loans.

Figure 2: PPP effects on firm-level employment and wages



Notes: Only first-draw PPP loans are considered. Dots are average treatment on treated coefficient estimates and ranges are 95 percent confidence intervals associated with quarterly employment growth pre- and post-treatment. Wages are from Form 941 indexed to 2020 dollars. *Source:* Authors' calculations using SBA and tax data.

Table 1: Total PPP effects on employment and wages

<i>Panel A: Retained Employees from PPP</i>					
Firm-Size Group	Avg. treatment #employees	Base Employees (millions)	Retained Job Years (millions)	PPP Forgiven (\$billions)	\$ of PPP per Job-Year Retained
Self-empl.	---	---	---	53	---
1-9	0.08	14.4	1.2	135	116,000
10-49	0.07	18.2	1.3	164	130,000
50-99	0.07	7.4	0.6	70	127,000
100-249	0.07	8.2	0.5	78	143,000
250+	0.06	10.6	0.7	57	86,000
Total	0.07	58.9	4.2	557	133,000
<i>Panel B: PPP worker share (costs to retain employees)</i>					
Firm-Size Group	Avg. treatment for wages	Base employee costs (\$billions)	PPP employee retention costs (\$billions)	PPP Forgiven (\$billions)	PPP employee retentions costs (% of PPP)
Self-empl.	---	---	---	53	---
1-9	0.10	697	71	135	53
10-49	0.10	1,162	111	164	68
50-99	0.09	408	38	70	55
100-249	0.10	453	46	78	59
250+	0.07	529	39	57	69
Total	0.09	3,249	306	557	55

Notes: Average treatment effects are averaged across four quarters and totals are base-weighted averages. PPP are forgiven amounts of first-draw loans scaled to reflect a 72 percent match rate. Firm sizes and base wages are from the 2018 quarter corresponding to quarter of treatment in 2020. Base employee costs are from Form 941 wages, indexed to 2020 dollars, scaled by the match rate, and increased 20 percent to account for non-wage employee costs. Firms with one employee are excluded from the regression and the size 2–9 coefficients are applied above. *Source:* Authors' calculations using SBA and tax data.

Base-year wages are adjusted to account for non-wage costs of employee retention. This is because many direct payroll costs from retaining employees are missing from our measure of quarterly wages (the maximum of Form 941 total compensation or Medicare wages capped at \$100,000 average per employee). Base-year wages are scaled up 20 percent to account for employer-paid federal payroll taxes, health insurance premiums, and retirement contributions (appendix Table A4). This ignores other employer costs from retaining employees, suggesting we may modestly underestimate the PPP’s worker share.

Figure 2A displays coefficient estimates and confidence intervals associated with quarterly employment growth pre- and post-treatment. The coefficient in the period of initial treatment is around 0.11, which can be interpreted as firms receiving first-draw PPP in that quarter resulted in 11 percentage points higher employment growth relative to firms that received first-draw PPP in a future quarter. Figure 2B displays analogous estimates for total wage growth, and the coefficient during the quarter of treatment is around 14 percent. Attenuated effects persist into the following three quarters. The small anticipatory effects are likely due to widespread knowledge of the program a few weeks before enactment (Autor et al. 2022b).

The overall impact on employment and wages is estimated by multiplying firm-size regression coefficients by real base-year levels. Table 1A suggests the first-draw PPP forgiveness retained 4.2 million job-years at a cost of \$133,000 per job-year retained. When adjusting for quarter-before-treatment levels across firm sizes, first-draw PPP forgiveness retained 3.8 million job-years at a cost of \$146,000 per job-year retained. Table 1B shows that first-draw PPP loans that were forgiven induced \$306 billion of additional employee-retention costs. This represents 55 percent of PPP loan forgiveness.¹⁴ Excluding PPP amounts going to self-employed firms suggests 61 percent of forgiveness supported employee payroll costs. Further robustness checks are discussed in the appendix.

IV. Distribution of PPP Effects

To allocate PPP loan forgiveness over the income distribution, we divide forgiveness into worker and owner portions. The worker portion accounts for employee-retention costs. The owner portion is the remaining amount (less non-profits) and allocated by firm ownership. Allocations are across eight annual 2020 income groups: the bottom four quintiles, P80–90, P90–95, P95–99, and the top one percent.

The worker portion’s distribution varies by allocation method, as seen in Figure 3A. Allocating lump sum, where each worker within a firm is allocated the same amount (up to their observed wage), gives a more progressive result. Allocating proportional to wages gives a less progressive result. The lump-sum allocation would be more appropriate when the PPP helped firms retain all employees—but for additional time among low-wage

¹⁴ When adjusting for quarter-before-treatment levels across firm sizes, these are \$258 billion and 46 percent of PPP loan forgiveness.

employees more likely to become unemployed or have longer unemployment spells without PPP loans. The proportional allocation would apply when the PPP prevented firm closure or reduced unpaid furloughs of equal time for all workers at a firm. Estimates suggest that low-wage workers had disproportionately large wage declines in 2020 (Larrimore, Mortenson, and Splinter 2023b), which supports a lump-sum allocation. Given the small differences between the allocations, we simplify the main exposition by using the average of lump-sum and proportional allocations.

The owner portion represents the 43 percent of PPP forgiveness left after deducting the estimated worker portion and the implied amount going to non-profit owners (\$18 billion). Overall, the owner portion of PPP tends to decline with income (see appendix Figure A3) in a similar pattern to that seen for the worker portion in Figure 3A.

A progressive effect results from the combined worker and owner portions. Figure 3B shows the shares of each PPP amount by income quintile: the bottom quintile receives 10 percent while the top quintile receives 44 percent.¹⁵ Due to the lower incomes of the bottom quintile, their smaller share of PPP still represents a higher share of income than for other income groups. Figure 3C shows that PPP amounts represent 18 percent of bottom-quintile income, 4 percent of middle-quintile income, and 2 percent of top-quintile incomes. While we estimate that the PPP had progressive impacts on income, we also find it was less progressive than other major pandemic relief with unemployment compensation and stimulus checks. Figure 3D shows this differential progressivity with redistribution rates.

PPP progressivity may be underestimated in our analysis for several reasons. First, we only consider wage effects for one year after PPP receipt. However, the share of the PPP going to wages increases as one considers a longer post-period (Dalton 2023; Autor et al. 2022b). Second, non-payroll costs supported by the PPP, such as from rents and utilities, are not captured in our analysis and imply too much is allocated to owners. Third, our income definition used to rank individuals includes PPP-retained wages. While this allows for conventional distribution measures using observed wages, it understates the PPP's progressivity because increased wages moved some individuals up the income distribution.

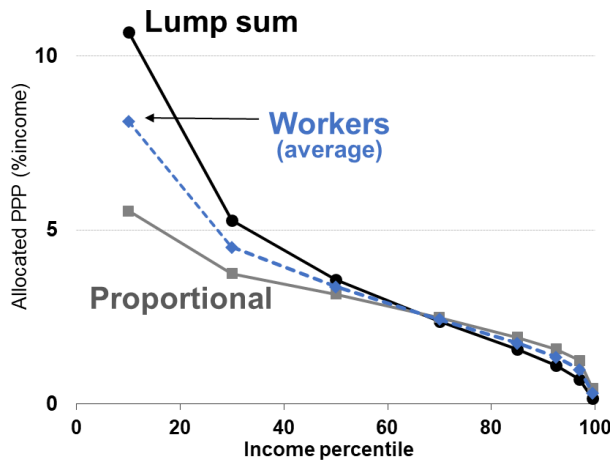
Our progressive PPP result is robust to alternative assumptions. To simplify, we present distribution-wide progressivity summary measures.¹⁶ A positive value means the PPP was progressive and our baseline progressivity is 0.38. Worker portions allocated proportionally or lump-sum produce progressivities of 0.31 and 0.46. Worker shares of zero and 100 percent lead to progressivities of 0.22 and 0.63. Allocating the seven percent of PPP forgiveness among unmatched firms to the top or bottom quintiles gives progressivities of 0.31 to 0.46.

¹⁵ These estimates resemble those using linked employer-employee tax data for the Employee Retention Tax Credit. Using an equal-owner/employee assumption and a lump-sum approach, about 12 percent of these credits went to incomes below \$10,000 and 39 percent to incomes above \$100,000 (Goodman 2023).

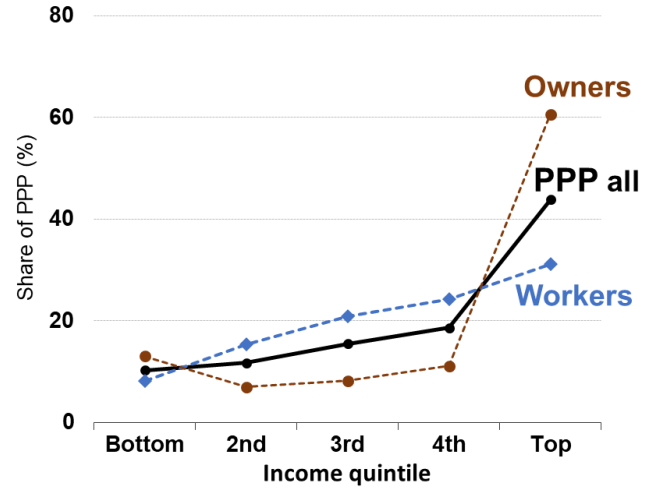
¹⁶ Elasticity-based progressivity is one minus the slope from regressing the natural log of relief on the natural log of income. Progressivity is zero when relief is proportional to income across the distribution. See the appendix.

Figure 3: PPP Distributions, 2020

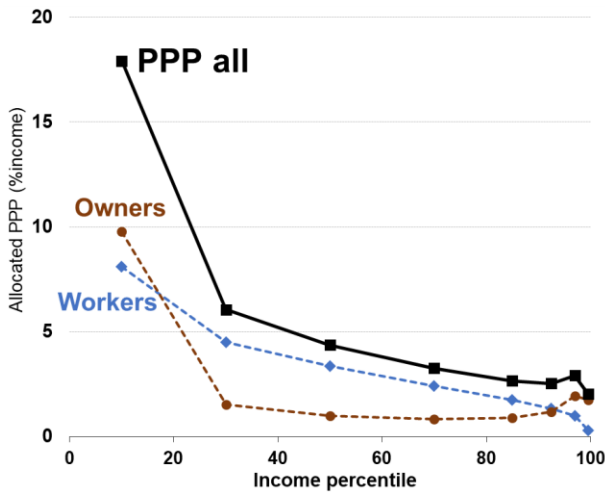
Panel A: Worker portion by allocation method



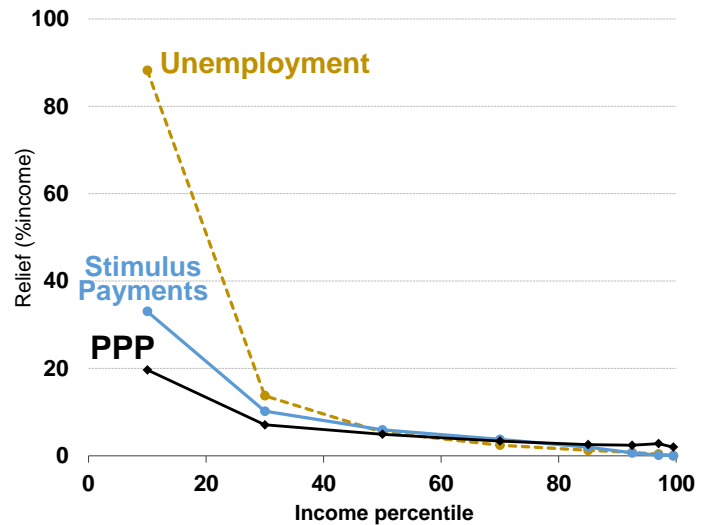
Panel B: Shares by income quintile



Panel C: Redistribution rates



Panel D: Redistribution rates by relief program



Notes: Income is essentially market income plus Social Security benefits reported in tax data (fiscal income) for both filers and non-filers, as described in the text. Quintiles have the same number of individuals ranked by size-adjusted income. Panel D: Total 2020 relief amounts were \$577 billion for unemployment compensation, \$445 billion for stimulus payments (including true-ups claimed on tax returns), and \$500 billion of PPP first-draw forgiveness. *Source:* Authors' calculations using SBA and tax data.

While we estimate that the PPP had progressive impacts on income, we also find it was less progressive than other major relief. In 2020, stimulus payment progressivity was 1.6, unemployment compensation progressivity was 1.4, and PPP progressivity was 0.4 (see appendix). Stimulus payments were more progressive than the PPP because stimulus was essentially a universal lump-sum transfer (Splinter 2023). Unemployment compensation targets individuals with job losses and was especially progressive in the pandemic (Cortes and Forsythe 2023; Larrimore, Mortenson, and Splinter 2023a). As each relief program was about half a trillion dollars in 2020, these progressivity differences translate into

redistribution differences.¹⁷ Unemployment compensation lowered the Gini coefficient by 4 points, stimulus payments by 2 points, and the PPP by only 1 point.

V. Net Fiscal Cost of PPP: Accounting for Fiscal Externalities

The gross amount spent on PPP loan forgiveness overstates the net fiscal cost. Leveraging our links to individual tax returns, we estimate taxes on PPP-retained wages. In addition, we estimate the amount of reduced unemployment compensation resulting from PPP loans. When considering these positive fiscal externalities, the estimated net fiscal cost of the 2020 first-draw PPP relative to the gross amount falls by 24 percent.

PPP-induced taxes are estimated by multiplying PPP-retained wages by applicable average marginal tax rates (AMTRs). These are the Table 1B employee costs reduced by the non-wage costs excluded from taxation. For individual income taxes, a simple federal payroll and income tax calculator based on taxable income from workers' tax returns provides AMTRs of wage decreases of \$10,000. Refundable credit phase-ins result in bottom-quintile negative AMTRs. Payroll tax rates include both the employee and employer shares, for a total of 12.4 percent up to the taxable maximum of \$137,700 and 2.9 percent on all wages for Medicare taxes. We assume only the latter applies to the top five percent. Estimated taxes are aggregated to the firm-level for the eight income groups and weighted by wages. For 2020, we estimate positive fiscal externalities from income taxes of \$46 billion and from payroll taxes of \$42 billion, for a total of 18 percent of gross PPP forgiveness.

PPP loans that successfully preserved employment possibly lowered the amount of unemployment compensation paid. Using annual unemployment compensation data, for each income group we compare unemployment compensation as a share of wages of similar-sized firms receiving first-draw PPP in the second quarter of 2020 to those receiving it later (results are similar when comparing firms receiving and never receiving the PPP).¹⁸ See the appendix for details. For 2020, we estimate positive fiscal externalities from reduced unemployment compensation of \$32 billion, or 6 percent of the gross PPP costs. This suggests \$8,000 in avoided unemployment compensation per retained job-year, although this estimated reduction has significant uncertainty.

These estimated externalities from taxes and unemployment compensation reduce the net cost to about 76 percent of the gross cost, suggesting the cost per job-year retained falls from \$133,000 to \$101,000 (from \$146,000 to \$111,000 with a pre-treatment adjustment). The latter estimates are about twice median annual worker earnings. These externalities have some distributional effects (Figures 4A and 4B) but little overall impact on progressivity (Figure 4C).

The PPP had other effects beyond the two externalities considered here. Our analysis excludes additional program costs from overhead and bank fees, although most loans only had a one-percent fee. It also ignores small offsetting effects from the taxation on unemployment compensation because in 2020 up to \$10,200 per spouse was excluded from income taxation for those with modified adjusted gross incomes under \$150,000. It also

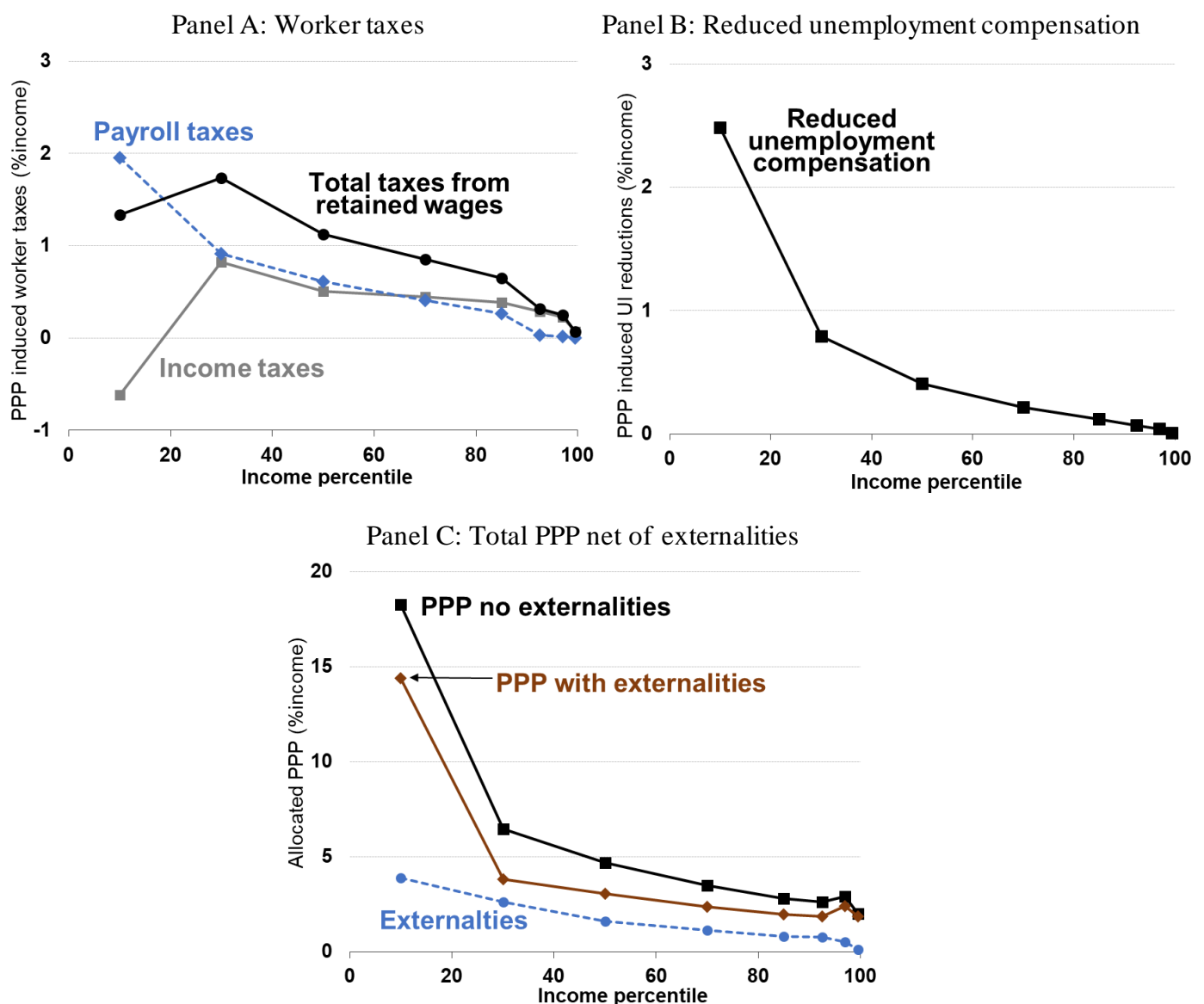
¹⁷ Progressivity measures are independent of program size, but redistribution measures are size dependent (Splinter 2020).

¹⁸ These unemployment compensation amounts in tax data are comprehensive, include expansions, and are over twice the amount in the CPS (Larrimore, Mortenson, and Splinter 2023a). Many firms receiving PPP laid off some workers for limited spells, allowing them to claim unemployment compensation.

excludes some sources of lower net costs: (1) additional taxes or reduced unemployment compensation in later years, (2) additional state taxes from increased wages, (3) additional taxes from induced taxable business profits, and (4) reductions in spending on other government programs such as Medicaid, disability, and SNAP.

An embedded net fiscal cost is that forgiven PPP loans were excluded from firms' taxable income, even though the expenses they funded were deductible for tax purposes. This "double" benefit implies a tax expenditure. Multiplying 2020 loan forgiveness by owner AMTRs, we estimate a tax-exclusion tax expenditure of \$84 billion, or nearly one-fifth of the PPP's gross cost. About 90 percent of this tax expenditure accrued to the top quintile (appendix Figure A10). This suggests an alternative PPP policy in which forgiven loans were taxed would eliminate much of top-quintile net relief and further reduce net costs.

Figure 4: Net Fiscal Impact of the PPP, 2020 redistribution rates



Notes: See Figure 3 notes. Source: Authors' calculations using SBA and tax data.

VI. Conclusion

It is unclear who bears the burden of business-level taxes. Business-level subsidies present a similar puzzle—the PPP was a subsidy targeted at workers but allocated through employers. Using links between PPP-receiving businesses and the tax returns of their workers and owners, we estimate the distributional impacts of the PPP.

We present a novel assessment of the PPP’s incidence. Autor et al. (2022a, p. 56) summarize the current view—the “incidence of the program across the household income distribution was highly regressive.” Using administrative tax data, we estimate that the overall distribution of the PPP was less concentrated than previous assumptions suggested, and that the PPP’s effects on individual incomes was progressive. The PPP had a less progressive impact than targeted relief like unemployment compensation, but this is consistent with the PPP’s objective of retaining employees across the distribution.

The cost of retaining jobs with the PPP appears similar to other programs. Using an event-study approach, we estimate an average gross cost per job-year retained of \$133,000 to \$146,000. For comparison, certain Great Recession programs in the Recovery Act had an estimated cost per job-year of about \$196,000 when using firm-level matched data (Cho 2018), which is similar to our approach, and usually between \$50,000 and \$200,000 when using local and state-level employment changes that capture macroeconomic effects (see review in Chodorow-Reich 2019).¹⁹ Meanwhile, Covid aid to state and local governments had an estimated \$850,000 cost per job-year (Clemens, Hoxie, and Veuger 2022). Finally, we account for positive fiscal externalities ignored in prior analysis. When considering PPP-induced decreases in unemployment compensation and increases in taxes paid, progressivity is unchanged and the net cost of the PPP declines by one quarter. An alternative policy making forgiven PPP loans taxable would increase net progressivity and further reduce net costs.

During the Covid pandemic, a broad group of workers and firms faced shocks, with 45 million workers receiving unemployment insurance. In mild recessions, however, shocks are smaller and likely more concentrated among marginal workers and businesses. Therefore, rather than business-level subsidies like the PPP, countercyclical fiscal policy typically relies on individual-level transfers, like unemployment compensation. These transfers can target those with job or income losses but do not impede job changes or firm exits like the PPP. The unique pandemic circumstances motivated the less-targeted approach of the PPP, but means it is unclear to what degree this paper’s distributional estimates would apply to another PPP-like program. Still, the \$800 billion PPP can provide some lessons about the effects of job-retention programs during deep economic shocks.

¹⁹ When considering macroeconomic effects, unemployment compensation and stimulus payments were associated more increase in county-level employment than the PPP (Gorbachev, Luengo-Prado, and Wang 2024a).

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Online Appendix

1. Linking Data

The SBA data are linked to tax data by name and address. Addresses, names, and Employer Identification Numbers (EINs) are reported on a variety of tax filings, depending on the nature of the entity. These include quarterly payroll tax filings for employers (Form 941) and annual entity-level tax returns (Forms 1120, 1120-S, 1065, and 990). For sole proprietorships, name and address information are reported on individual tax returns (Form 1040) filing a Schedule C and account for either business or individual names, including both spouses' names on joint returns. For both SBA and tax data, we clean all names and addresses using the same cleaning procedure, removing special characters and spaces and standardizing address and business name endings.

We link the SBA and tax data sequentially, with exact matches on address and name. Next, fuzzy matches use a similarity measure of the combination of address and name. These matches start at a small geographic level (zip codes) and sequentially expand to consider matches at the city, county, and state levels. As the geographic level expands, the match criteria become stricter. Table A1 summarizes the proportion of loans matched at each step. Table A2 shows match rates by firm size, and Figure A1 shows first-draw PPP take-up rates by firm size.

For first-draw loans, 93 percent of PPP forgiveness amounts, 93 percent of employees, and 79 percent of loans are matched to a firm's EIN. The SBA data includes self-reported entity types (e.g., partnership, C corporation, etc.) that help show which entities are matched well, even though these self-reported entity types do not completely align with tax entity types. Match rates are highest for corporations (97 percent), and these represent over half of PPP forgiveness amounts. About half of loans counts are to certain single-person firms (sole proprietorships, self-employed, and independent contractors). This group's low match rate (67 percent) pulls down the percent of loans matched from 93 percent without these small firms to 79 percent with them. However, as these firms receive only about one-tenth of total PPP forgiveness amounts, they only push the forgiveness match rate down from 96 percent to 93 percent.

For our regressions, firms with matched EINs are then linked to Forms 941, which have quarterly employee counts and wage amounts. Overall, these matches capture 74 percent of first-draw PPP forgiveness amounts. For comparison, Dalton (2023) matched 87 percent of certain 2020 PPP loans amounts to quarterly wages, but this excluded entities that tend to have lower match rates (i.e., sole proprietorships, self-employed, independent contractors, and non-profit firms).

Table A1: Linking Method: SBA and tax data, first-draw loans

Match Type	Share of loan counts (%)	Share of loan dollars (%)	Share of employees (%)
Exact	22	39	38
Zip code	15	26	26
City	2	3	3
County	2	2	2
State	4	2	3
Schedule K	0	0	0
Schedule C	35	21	21
Total	79	93	93

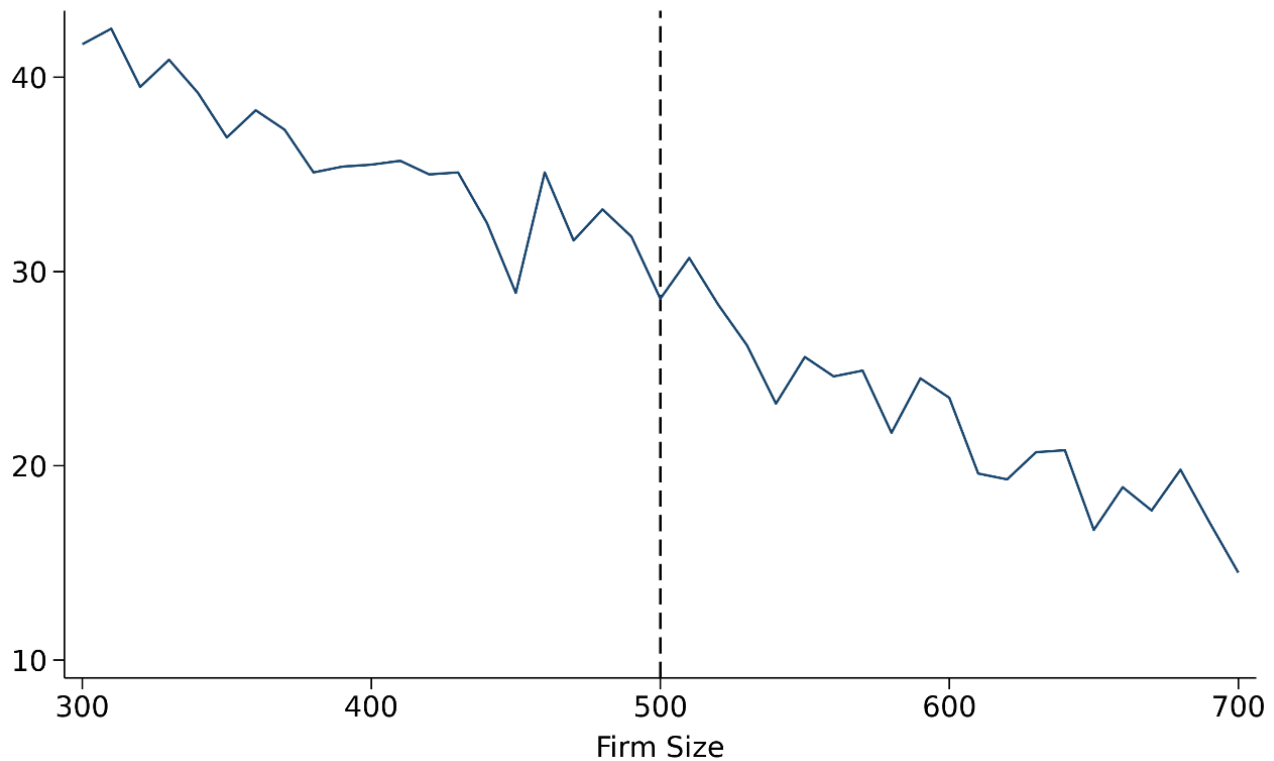
Sources: Authors' calculations using SBA and tax data.

Table A2: SBA to tax EIN match rates: First-draw PPP loans by firm size

Firm Size	Forgiveness Amount		Number of Loans	
	\$Billions	Match Rate (%)	Thousands	Match Rate (%)
1-9	138	84	7,281	76
10-49	180	96	1,103	95
50-99	78	96	132	95
100-249	89	96	71	95
250+	72	97	25	95
Total	557	93	8,613	79

Sources: Authors' calculations using SBA and tax data.

Figure A1: PPP take-up rate of firms by firm size (%)

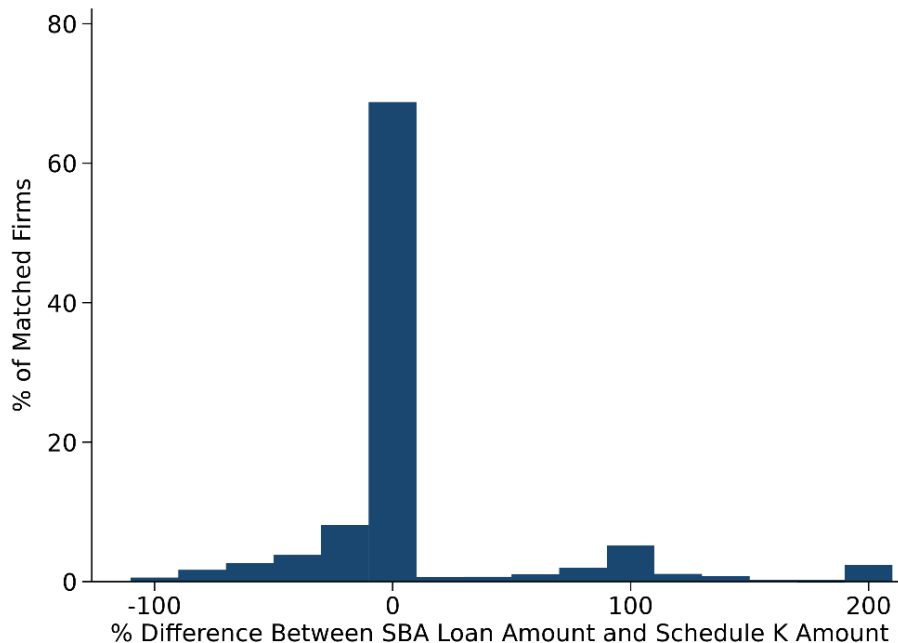


Notes: Within each firm-size bin of 10 employees, take-up rate is the number of SBA-matched firms with first-draw PPP loans divided by the total number of firms with Form 941. Firm sizes are based on the average number of employees reported on quarterly filed 2019 Forms 941. *Source:* Authors' calculations using SBA and tax data.

2. Comparison with PPP amounts in tax data

Comparisons of the SBA loan data with forgiven amounts reported on schedule K for S corporations and partnerships (Forms 1120-S and 1065) suggest that our procedure results in a high rate of correct matches. Despite being excluded from taxation, S corporations and partnerships were instructed to report this forgiveness as other tax-exempt income (schedule K lines 18b and 16b, respectively). While other types of income could be reported here, total other tax-exempt income reported by S corporations and partnerships increased from \$14 billion (0.1 million records) in 2019, to \$148 billion (1.2 million records) in 2020, and in 2021 to \$298 billion (1.5 million records) in 2021. This suggests the vast majority of this income reported on these lines was forgiven PPP loans. Of the 2.1 million firms with relevant schedule K's and reporting other tax-exempt income, we match 75 percent to a PPP loan. Aggregating across loans and across tax years 2020 and 2021, we find that 69 percent of the matched had PPP loan amounts within 10 percent (most others were just below this range, as seen in Figure A2).

Figure A2: Ratio of all PPP forgiveness in SBA and tax data, matched firms



Notes: Ratio of first- and second-draw PPP loan forgiveness in SBA data to other tax-exempt income reported on Schedule K in tax data for 2020 and 2021, only among matched firms. Bins are for 20-percent intervals. Firms with twice as much SBA amounts than tax amounts suggests that some EINs were linked to two different PPP-receiving firms. *Source:* Authors' calculations using SBA and tax data.

3. Differences-in-Differences Estimation

We estimate the effects of the PPP on employment and wages using a dynamic difference-in-differences estimator developed by Callaway and Sant'Anna (2021). We use Form 941 quarterly payroll filings from 2018 to 2021, which include counts of employees and wages paid in that quarter.

We make several sample restrictions. Firms are dropped if they have: (1) zero or one employee in any quarter of 2018 or 2019, (2) no valid industry classification code in any year, (3) no observed state in any year, (4) industry classifications of public administration or utilities due

to ineligibility, (5) are restaurants because of simultaneous industry-specific relief paralleling the PPP, or (6) never received first-draw PPP loans.²⁰ Finally, the panel is balanced by setting the number of employees and wages to zero in quarters for which a firm was inactive. Wages are adjusted to 2020 price levels.

$$ATT_{p,q} = E [Y_q (\text{PPP}) - Y_q (\text{No PPP})] \quad (\text{A.1})$$

The coefficient estimated using the dynamic difference-in-differences estimator is an average treatment effect on the treated (ATT). In Equation A.1, for firms receiving PPP in quarter p , $ATT_{p,q}$ is the average difference in quarter q of the dependent variable Y (employment or wages) between firms receiving and not receiving first-draw PPP loans. This approach, after demeaning to control for time-invariant covariates, accounts for different treatment timing and heterogeneous treatment effects to estimate an uncontaminated ATT (Dalton 2023).

$$Y_{i,q,t,s,j,f} = \frac{y_{i,q,t,s,j,f}}{(y_{i,q,t,s,j,f} | t=2018)} \quad (\text{A.2})$$

$$\tilde{Y}_{i,q,t,s,j,f} = Y_{i,q,t,s,j,f} - \bar{Y}_{s,j,f} \quad (\text{A.3})$$

In Equation A.2, $y_{i,q,t,s,j,f}$ is the employment or wages in firm i , quarter q , year t , state s , two-digit industry code j , and firm-size f . To estimate proportional changes, each firm's employment or wages is divided by its value in the same quarter in 2018. Following Dalton (2023), in Equation A.3, the dependent variable is “demeaned” by the average employment or wage growth for state, two-digit industry code, and firm-size groups (s,j,f). This is similar to using fixed effects, although it allows for faster processing with the full population. Demeaning by state helps control for geographic variation in Covid-related employment shocks and state-level policy. The firm-size bins are defined based on the minimum employment observed in any quarter in 2018. The regression sample is limited to firms with Form 941 quarterly observations that were matched to first-draw PPP loans in the SBA data. This limits the identifying variation to differences in the timing of first-draw PPP receipt, with treatment mostly occurring in the second and third quarters of 2020 but sometimes in the first and second quarters of 2021. While this identifying variation could be problematic to the degree timing of PPP receipt is endogenous to wage and employment growth, this approach avoids the limitations of the size-threshold approach discussed in section I.A of the main paper and section 8 of this appendix.

Base-year wages are adjusted to account for non-wage costs of employee retention. This is because many direct payroll costs from retaining employees are missing from our measure of quarterly wages using Form 941 (the maximum of total compensation or Medicare wages and then capped at \$100,000 average per employee). Base-year wages are scaled up to account for employer-paid federal payroll taxes, health insurance premiums, and retirement contributions.²¹

²⁰ The restrictions result in the following changes: (1) dropping zero- or one-employee firms reduces PPP amounts by about one-fifth (including from many Sch. C sole proprietors), (2) removing firms without industry codes reduces the firm count and PPP amounts by 3% (codes come from firm tax returns), (3) removing firms with no reported state has negligible effects, (4 and 5) removing public admin., utilities, or restaurants reduces the firm count and PPP amounts by 8%, and (6) removing firms never receiving first-draw PPP loans reduces the firm count by about one-third.

²¹ Average costs with respect to wages are based on national accounts: 8 percent for employer-paid payroll taxes, 8 percent for health insurance, and 4 percent for retirement contributions. See appendix Table A4.

This approach ignores other employer costs from retaining employees, suggesting we may modestly underestimate the PPP's worker share.

Figure 3A of the main paper displays coefficient estimates and confidence intervals associated with quarterly employment growth pre- and post-treatment. The coefficient in the period of initial treatment is around 0.11, which can be interpreted as firms receiving first-draw PPP in that quarter resulted in 11 percentage point higher employment growth relative to firms that received first-draw PPP in a future quarter. Figure 3B of the main paper displays analogous estimates for total wage growth, and the coefficient during the quarter of treatment is around 14 percent. Attenuated effects persist into the following three quarters. Although we observe a slight pre-trend in the two quarters prior, the magnitude is tiny in comparison to the size of the estimate effect, and a portion is attributable to anticipatory effects (Autor et al. 2022).²²

4. Expanded Fiscal Income

Workers and business owners are placed into 2020 fiscal income groups, which is essentially market income observed in tax data and Social Security benefits. This income definition parallels other studies using tax data: Piketty and Saez (2003); Larrimore, Mortenson, and Splinter (2021, 2022); and Congressional Budget Office (2022). For tax return filers, *fiscal income* is adjusted gross income reported on individual tax returns adjusted as follows: add nontaxable interest, adjustments, and non-taxable taxable Social Security; remove taxable unemployment compensation (most was excluded from taxation in 2020) and negative other income to account for net operating loss carryovers from prior-year losses; and replace taxable private retirement income with gross private retirement income (total distributions less rollovers). For non-filers, fiscal income includes wages, dividends, interest, miscellaneous income (half to account for missing deductions), gross private retirement income, Social Security benefits, and partnership income from Forms W-2, 1099-DIV, 1099-INT, 1099-NEC, 1099-MISC, 1099-R, 5498, SSA-1099, and 1065 and 1120-S Sch. K-1s. In 2020, our measure of fiscal income totals \$14.0 trillion, or 79 percent of national income. Fiscal income is usually about 60 percent of national income (Auten and Splinter 2024), but our measure includes capital gains and certain Social Security benefits.²³

In the 2020 tax data, we observe 332.7 million domestic individuals, which matches the U.S. Census resident population.²⁴ This includes filers and dependents on domestic tax returns and non-filers with at least one domestic information return. This fits with prior analysis, where 2010 tax data included 99.8 percent of the U.S. Census resident population (Larrimore, Mortenson, and Splinter 2021).

Following the approach in prominent income distribution studies, we estimate income-group thresholds and totals after bottom coding incomes at zero, size-adjusting incomes, and creating groups based on the number of individuals. This resembles the equivalized-income distributional estimates presented by the U.S. Census and estimates using tax data by Auten and Splinter (2024) and the Congressional Budget Office. Our income shares resemble prior estimates (see Table A3). When ranking tax units across the distribution, income is size adjusted to account for economies of scale and sharing. This adjustment divides income by the square-root of the

²² Along with anticipation before overall enactment, some of the treatment firms in later periods likely anticipated receipt or adapted behavior in months prior to receipt anticipating funds would be available.

²³ Our 2020 income is 95 percent of the 2019 Congressional Budget Office (2022) measure of expanded fiscal income, which includes additional income sources (e.g., corporate taxes and Medicare benefits).

²⁴ U.S. Census July 1st population estimates for 2020 and 2021 are 332.0 and 333.3 million and average to 332.7 million, the more comparable end-of-year level for IRS data. See www.census.gov/quickfacts/fact/table/US/PST045222.

number of individuals on a tax return (filers and dependents).²⁵ Income groups are created based on the number of individuals such that each quintile has the same number of individuals (as compared to the same number of tax filing units).

Our income rankings include PPP-induced wages, although redistribution measures should use pre-policy income. However, assuming the estimated 4.6 million affected workers fall two quintiles without PPP would only increase elasticity-based progressivity from 0.40 to 0.41.

Table A3: Fiscal Income Share Comparisons

	Bottom Quintile	2nd Quintile	3rd Quintile	4th Quintile	Top Quintile
AS: no cap gains, 2019	2%	7%	13%	21%	57%
CBO: cap gains, 2019	4%	9%	14%	20%	55%
Fiscal Income: cap gains, 2020	2%	7%	12%	19%	60%

Notes: Our measure is fiscal income, adjusted gross income plus nontaxable interest, Social Security benefits, and non-rollover retirement income minus taxable unemployment compensation and net operating loss carryovers. Congressional Budget Office (CBO) income is “income before taxes and transfers” (includes realized capital gains, Social Security, unemployment compensation, and Medicare benefits) and has a larger sharing unit for size adjusting than other measures (household vs. tax units), which increases bottom-quintile incomes. Auten and Splinter (AS) fiscal income is from the step just after grouping by size-adjusted income and number of individuals (excludes realized capital gains and transfers). All measures define groups using the number of individuals and size-adjusted income. *Sources:* CBO (2022), Auten and Splinter (2024), and authors’ calculations.

Table A4: Non-wage employee cost adjustments using 2020 values in NIPA tables 7.8 and 2.2B

Type	Amount	Base Wages	Percent
Payroll taxes	717	9,457	8%
Health insurance	797	9,457	8%
Retirement contributions	295	7,963	4%
Total			20%

Notes: Payroll taxes include \$454 billion of OASDI Social Security taxes, \$131 billion of Medicare taxes, \$87 billion of workers’ compensation, and \$45 billion of unemployment taxes. Wages and salaries are \$9,457 billion and for retirement, private wages and salaries are \$7,963 billion. *Source:* Bureau of Economic Analysis (NIPA Tables 7.8 and 2.2B).

5. Income Variability: Redistribution or Stabilization

PPP distributional estimates appear robust to one-year income changes. Redistribution rates are based on annual income, but income variability can push individuals into different income groups in surrounding years. This means fiscal relief from PPP and other programs can represent “redistribution” for individuals with persistently low incomes, but “stabilization” for those with short-term losses (Larrimore, Mortenson, and Splinter 2016). To assess low-income dynamics, we consider 2020 tax returns with less than \$10,000 of size-adjusted income. In both the preceding and subsequent years, about 30 percent of these filers have higher incomes but only 0.5 percent have surrounding-year incomes above \$100,000. Splinter (2022) estimated similar low-income dynamics. When limiting to returns with business income or losses on Schedule E, which accounts for S

²⁵ As non-filing tax units are not defined in the tax data and only about one-tenth of non-filing tax units are married (Auten and Splinter 2024), we do not combine non-filers into synthetic tax units. Linking non-filers, however, would re-rank few non-filers across our broad income groups.

corporations and partnerships, about 45 percent have higher incomes and 9 percent have surrounding-year incomes above \$100,000. The bottom quintile also appears insensitive to removing certain business losses. Among the 2020 tax returns with zero or negative income, removing Schedule E losses, leaves 96 percent with incomes below \$10,000 and moves less than one percent above \$100,000.

6. Estimating Avoided Unemployment Compensation from the PPP

If workers are retained as a result of the PPP, they do not need to apply for unemployment compensation. Because we estimate a sizeable number of workers were retained due to PPP, we also estimate the amount of unemployment compensation avoided because of the PPP. It is not possible, however, to run differences-in-differences regressions to estimate avoided unemployment compensation in the same manner as estimating retained employees or wages, which are reported quarterly in tax data. Unemployment compensation reported in tax data is only available annually, and thus we cannot exploit quarterly variation. Instead, we use a simple procedure that relies on the following assumption: had the PPP not happened, workers in the same portion of the income distribution at similar-sized firms would have had the same aggregate unemployment-compensation-to-wage ratios whether the firm received the PPP in the second quarter of 2020 (treated) or after this quarter (untreated).

The following equations more precisely state our procedure. Take the aggregate wages of the untreated workers (w_0) and the aggregate unemployment compensation of the untreated workers (u_0) in a single “bin,” where bins are by firm size and by the placement of the worker across our eight income groups. For each bin, these can be represented as an unemployment-compensation-to-wage ratio r_0 :

$$r_0 = \frac{u_0}{w_0} \quad (\text{A.4})$$

We assume that $r_0 = r_t^n$, where r_t^n is the same ratio for the treated firms *had the PPP not happened*. That is,

$$r_0 = \frac{u_0}{w_0} = \frac{u_t^n}{w_t^n} = r_t^n \quad (\text{A.5})$$

where u_t^n and w_t^n represent the unemployment compensation and wages of the treated workers had the PPP not occurred (thus the superscript n).

The challenge is that we cannot observe u_t^n or w_t^n , so we must infer them. However, we can observe u_t^a and w_t^a , the *actual* unemployment compensation and wages of the treated workers. We find in our empirical estimates that the PPP retained workers on net, thus increasing wages and reducing unemployment compensation. We write the actual observed ratio for treated workers r_t^a as:

$$r_t^a = \frac{u_t^n - u_t^s}{w_t^n + w_t^s} \quad (\text{A.6})$$

where u_t^s and w_t^s represent the unemployment compensation and wages of the treated workers that were *saved* due to the PPP (i.e., the unemployment compensation was avoided and the wages were saved).

Using the three equations above, we can solve for the desired term, unemployment compensation avoided (u_t^s), as a function of observable aggregate parameters within each bin:

$$u_t^s = r_o \cdot w_t^n - u_t^a \quad (\text{A.7})$$

In other words, the avoided unemployment compensation (u_t^s) equals the amount of unemployment compensation that would have been paid for the treated workers without PPP (because $u_t^n = r_t^n \cdot w_t^n$ by definition and $r_t^n = r_o$ by assumption, thus $r_o \cdot w_t^n = u_t^n$) minus the actual observed unemployment compensation paid to the treated workers (u_t^a). We can estimate w_t^n using the results of our regression analysis on saved wages, because $w_t^n = w_t^a - w_t^s$ by definition). Practically speaking, we take the results of our regression analysis of estimated saved wages by firm size, apply those saved wages proportionally across the income distribution categories, and then estimate the unemployment compensation avoided by each firm size and worker income distribution bin. Note that this excludes self-employed workers and may therefore present an underestimate of the PPP's effect on unemployment compensation.

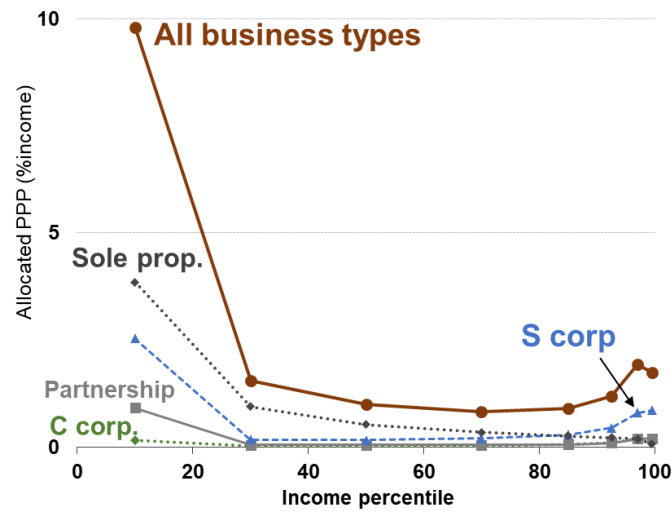
7. Elasticity-Based Progressivity

The elasticity-based progressivity is one minus the slope from regressing the natural log of relief on the natural log of income (average per capita amounts for eight income groups). Splinter (2020) discusses this measure as applied to taxes. A progressivity of zero would result from relief that was proportional to income. Positive progressivity results from relief that decreases with income (negative slope) or increases more slowly than income (positive slope of less than one). Note that progressivity measures control for the size of the relief, making different relief programs more comparable (in contrast, redistribution measures are sensitive to the total amount of relief; see Lambert 1993 and Kakwani 1977).

Figure A3 shows redistribution rates (PPP forgiveness as a share of income) of the owner portion by entity type. The PPP targeted smaller firms and therefore less went to C corporations and partnerships than to S corporations and sole proprietorships. Over the income distribution, the C corporation portion is relatively proportional. The partnership portion and S corporation portions have inverse J-shapes with larger benefits for the bottom of the distribution. The sole proprietorship share is strongly progressive. Overall, we observe an inverse J-shape pattern of the owner portion of PPP over the income distribution: 10 percent of income for the bottom quintile, about 1 percent for middle quintiles, and 2 percent for the top percentile of the income distribution.

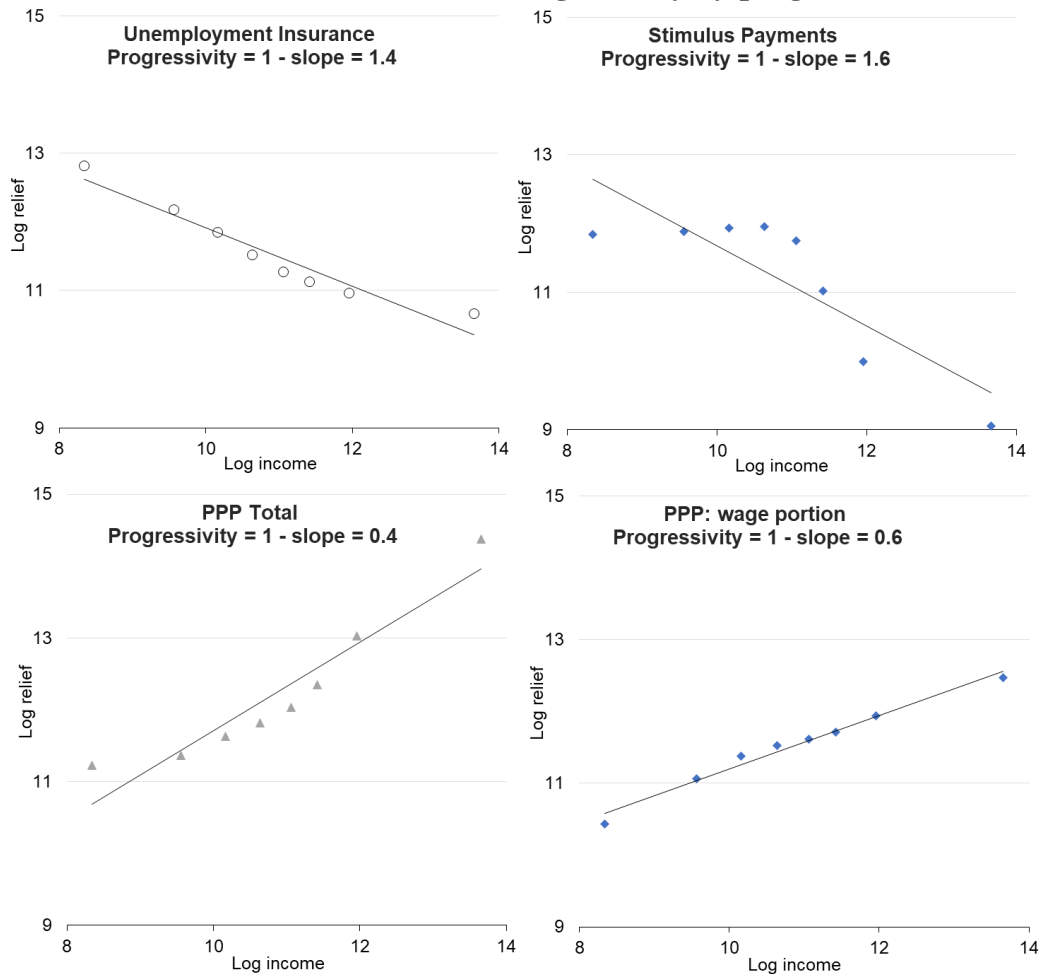
Figure A4 shows the average log relief and log incomes for the eight income groups in 2020. The figure shows a negative slope, meaning absolute amounts of unemployment insurance decrease as one moves up the income distribution, hence the large progressivity of 1.4. The top-right figure shows the same for stimulus payments and a large progressivity of 1.6 (although the fit is poor because this was a flat benefit amount with an income phase out). The bottom-left figure shows a positive slope of less than one, meaning absolute amounts of unemployment insurance increase but increase more slowly than income as one moves up the income distribution, hence the small progressivity of 0.4 for the overall PPP. The bottom-right figure shows a flatter positive slope for a moderate progressivity of 0.7 for the wage portion of PPP. Reynolds–Smolensky redistribution indexes show similar results: unemployment compensation lowers the Gini coefficient by 4 points, stimulus payments by 2 points, and the PPP by 1 point.

Figure A3: PPP owner portion by entity type, 2020 (redistribution rates)



Notes: Income is essentially market income plus Social Security benefits reported in tax data (fiscal income) for both filers and non-filers, as described in the text. Quintiles have the same number of individuals ranked by size-adjusted income. Businesses are grouped by tax-data based entity types where unmatched types are not shown. *Source:* Authors' calculations using SBA and tax data.

Figure A4: Elasticity-based progressivity by program, 2020



8. Robustness Checks

The main regression compares firms that received a first-draw PPP loan in a given quarter with firms that receive first-draw PPP loans in subsequent quarters. In this section, we present robustness checks after describing our main analysis. Our main analysis is based on variation in treatment over time. This has some strengths and weaknesses due to likely reasons firms delay applying for or receiving PPP loans. Some firms may have delayed loan applications due to incomplete information, the belief that they did not initially qualify because they were not affected by the pandemic (and later they thought they were affected), or they delayed their application because they initially had no banking relationship. As owners learned of the widespread take up and billions spent on PPP loans, all three of these frictions should decrease over time. The latter two reasons would be problematic if application timing was correlated with real economic shocks or if those with no banking relationships are different. Our summary statistics in Table A4 suggest the firms receiving the PPP in different calendar quarter have similar average wages, although sizes vary over time. Note that low average wages in Table A4 are because tax data do not allow us to separate part-time and full-time workers or remove workers joining or leaving a firm during that quarter.

The first robustness check we consider includes separate estimates for restaurants, which are excluded from the main analyses due to simultaneous relief from a separate program. Relative to our main results, Figure A5 shows nearly identical first and second quarter effects and lower third and fourth quarter effects. Second, we show the results by calendar year time, as the main figures pool results of treatments starting in various quarters. Figure A6 shows that initial-quarter treatment effects were positive for all three timing groups but dissipated for later groups, consistent with the waning of the pandemic's economic shock over time. Third, we include firms that have 600 employees or less in the base year and never received PPP loans, which is closer to the Dalton (2023) approach. Compared to our baseline estimates, Figure A7 shows that including firms never receiving PPP results in lower one-quarter-after-treatment effects and higher four-quarter-after-treatment effects. These results seem less reasonable given the expectation of declining effects over time and the pre-trends also look problematic.

Some papers exploit the nearly two-week period during which PPP loans were not approved during mid-April 2020 due to allocated funds running out. Appropriations were added soon afterwards, but this provides an alternative identification of the effect of PPP loans on employment. The SBA data show that approximately no applications were approved between April 16 and April 26, 2020. We limit our sample to two groups of firms: those approved for PPP loans the week before the approval pause (April 9 to April 16) and those approved for PPP loans the week after the pause (April 27 to May 3). Using a standard difference-in-difference regression, we compare the employment trends in subsequent quarters for those treated the week before the pause with those treated the week after the pause. This suggests firms approved for PPP loans in the week just before the approval pause had average employment growth 1.5 percentage points higher than firms approved in the week just after the pause (p-value less than 0.1%). This is about one-fifth the estimated 7 percentage points higher employment effect with our baseline approach, which captures much more heterogeneity across time than this one-week approach. Therefore, we view the findings from the approval-pause discontinuity approach as being broadly consistent with our baseline findings.

Covid intensity early in the pandemic may be associated with the effectiveness of PPP loans at mitigating employment loss. To highlight this mechanism, we divide our sample between firms located in New York and New Jersey, states that were impacted early in the pandemic, and all other states. Figure A8 shows that first-quarter employment effects of PPP loans are larger in

New York and New Jersey, the states affected earlier and more intensely by Covid. Later-quarter effects are similar when comparing these states and other states.

Finally, we replicate the threshold method from Autor et al. (2022, hereafter Autor et al.) and find no statistically significant effect on wages with this method when using the matched tax data. The threshold method compares firms just above and below the original firm-size cutoffs for PPP eligibility. This approach theoretically provides a valid control group. However, many firms with more than 500 employees received first-draw PPP loans and there was no take-up discontinuity at this size threshold, as seen above in Figure A1. This resulted from various exemptions and likely inconsistent enforcement of the threshold, meaning some firms in the control group received treatment and this may have attenuated results. We also find evidence of misreported firm size in the self-reported SBA data. Figure A9 (right side) shows clear evidence of firm size bunching just below the 500 employee eligibility threshold that is not observed in the matched tax data (left side). Specifically, for first-draw PPP applications in the SBA data, over 2,000 firms self-reported having 491 to 500 employees, while fewer than 500 firms reported having 481 to 490 employees.

Autor et al. estimated intent-to-treat effects for four employee ranges, comparing firms within 50, 100, 150, and 250 employees of the eligibility threshold. They then averaged results across the four employee ranges, resulting in their mid-May average intent-to-treat effect on employment of 3 percent. Next, to account for take-up, they multiplied this intent-to-treat estimate by two to yield average treatment on the treated (ATT). Their peak 6 percent ATT falls to 2.4 percent by the end of 2020.²⁶ The 6 percent estimate is essentially an average of ATTs across the four employee ranges (50, 100, 150, and 250 employees) of 10, 7, 4, and 2 percent. Hence, the effects differ by a factor of five. Note that the 50-employee range peak ATT of 10 percent is similar to our peak ATT of 11 percent in Figure 3A.

We run the same regression except for three changes: we use quarters rather than weeks, we use a base period of 2019 rather than Feb 2020, and we directly estimate ATTs rather than intent-to-treat that require adjustments to get ATTs.²⁷ Using the threshold method and linked tax data, our average estimated peak effect is essentially zero. The lack of discontinuity at the 500-employee threshold may contribute to the sensitivity of estimates to different employee ranges, as well as attenuate results. To address this concern, we replicate the 50-employee range estimate but add a 50-employee donut around the 500-employee threshold. This had no effect on our zero-estimate result using the threshold method.

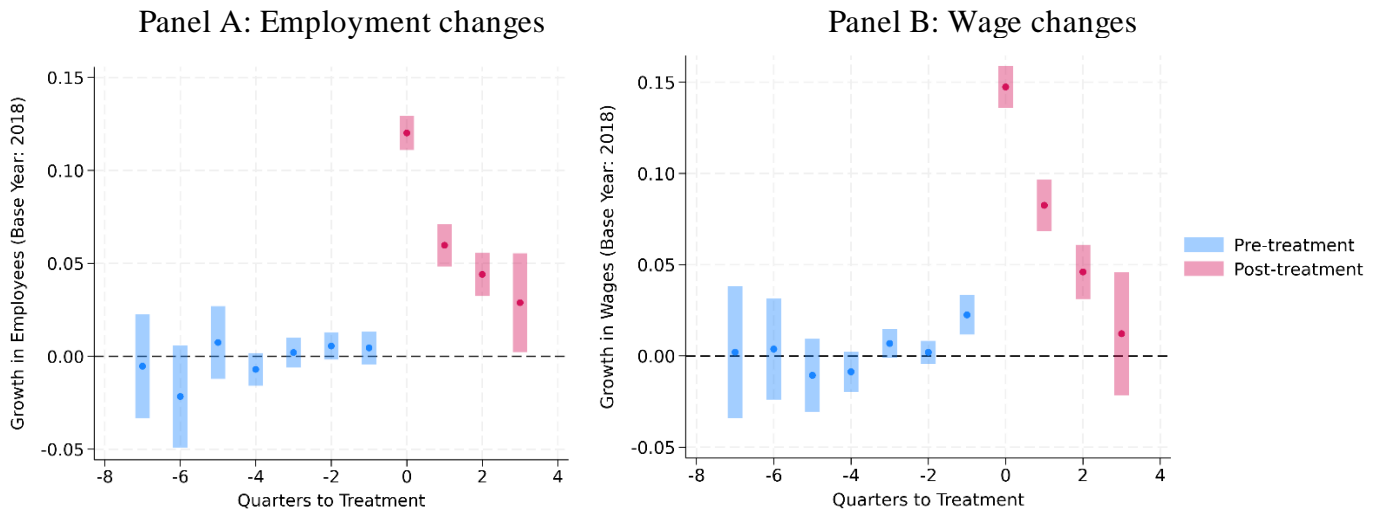
Table A4: Firm characteristics by quarter of treatment

Quarter of Treatment	Mean Employee Count	Mean Wage
Q2 2020	19	13,779
Q3 2020	12	11,390
Q1 2021	15	12,070
Q2 2021	32	11,572

²⁶ Note that Autor et al. extend these ATTs to account for small-firm and post-2020 effects.

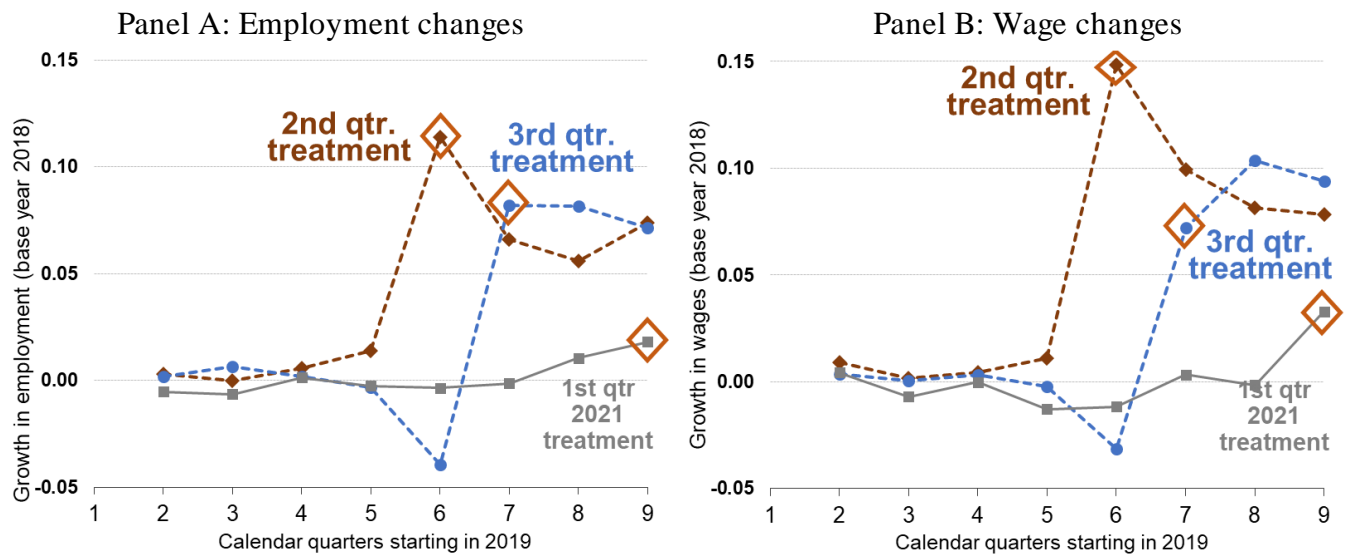
²⁷ Our regression is: employment growth relative to 2019 average with respect to an indicator for receiving first-draw PPP (at all), state-by-quarter fixed effect, industry-by-quarter fixed effect, and quarter-by-PPP fixed effect. Autor et al. account for higher threshold in select industries. Our results may be a bit attenuated because we do not account for higher thresholds in select industries.

Figure A5: PPP effects on firm-level employment and wages, only restaurants



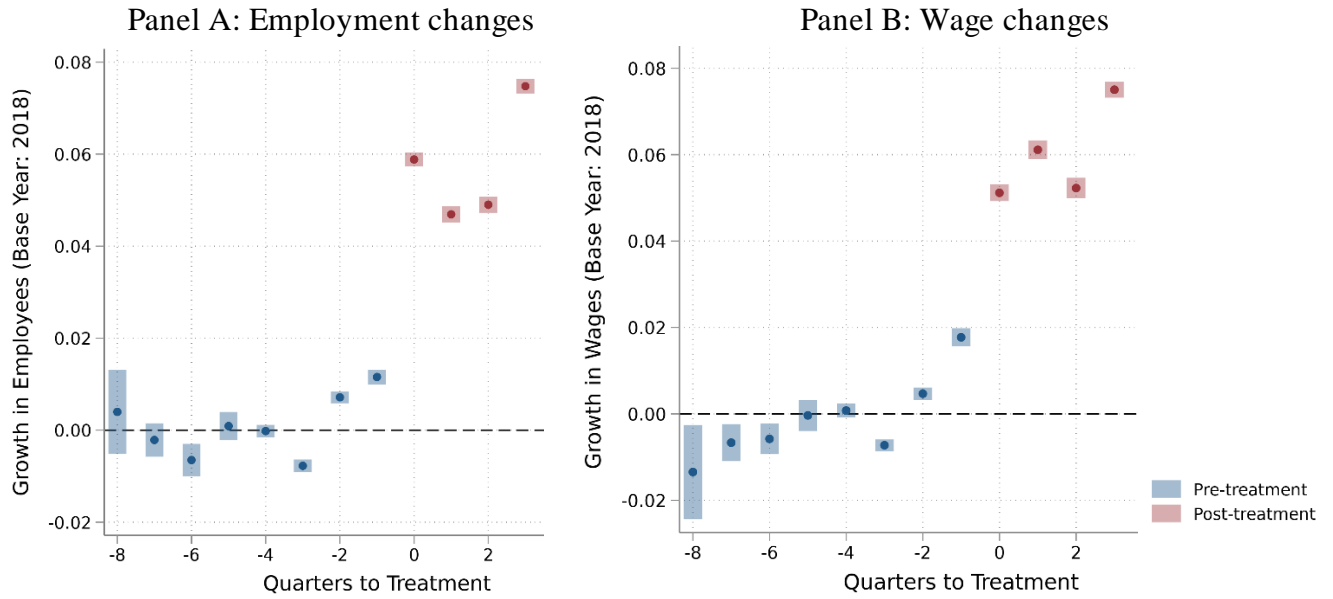
Notes: Only first-draw PPP loans are considered. Dots are average treatment on treated coefficient estimates and ranges are 95 percent confidence intervals associated with quarterly employment growth pre- and post-treatment. Wages are from Form 941 indexed to 2020 dollars. *Source:* Authors' calculations using SBA and tax data.

Figure A6: PPP effects on firm-level employment and wages, by calendar quarter



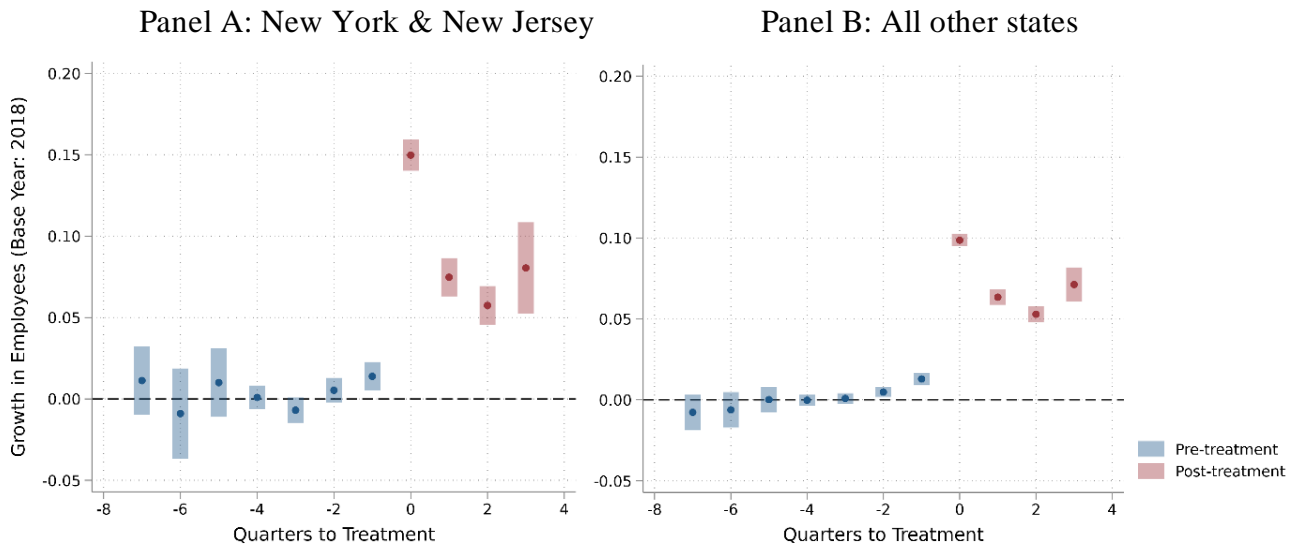
Notes: See above. *Source:* Authors' calculations using SBA and tax data.

Figure A7: PPP effects, including firms never receiving first-draw loans as controls



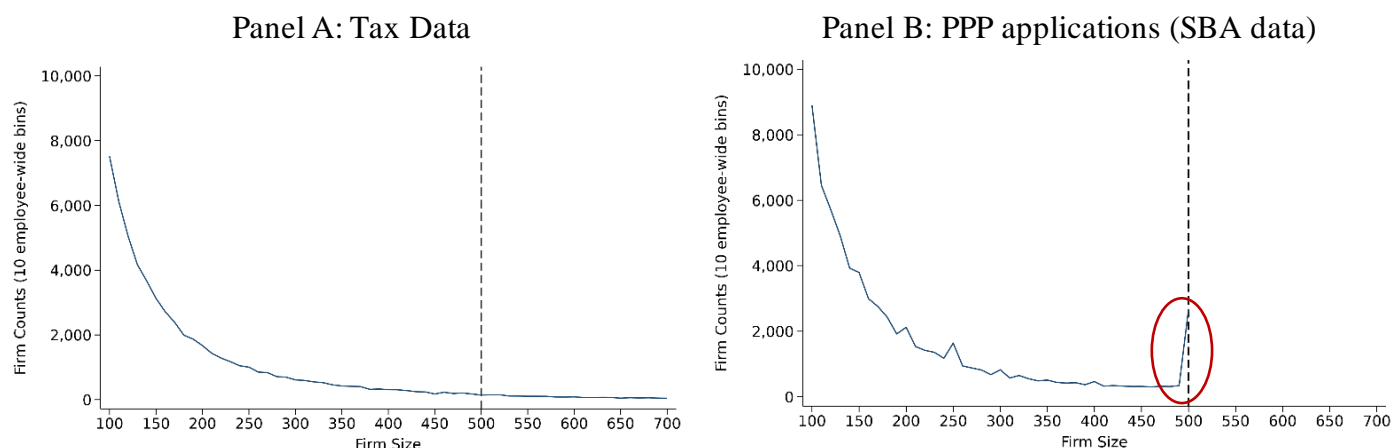
Notes: See above. *Source:* Authors' calculations using SBA and tax data.

Figure A8: PPP effects on firm-level employment, by NY-NJ and other states



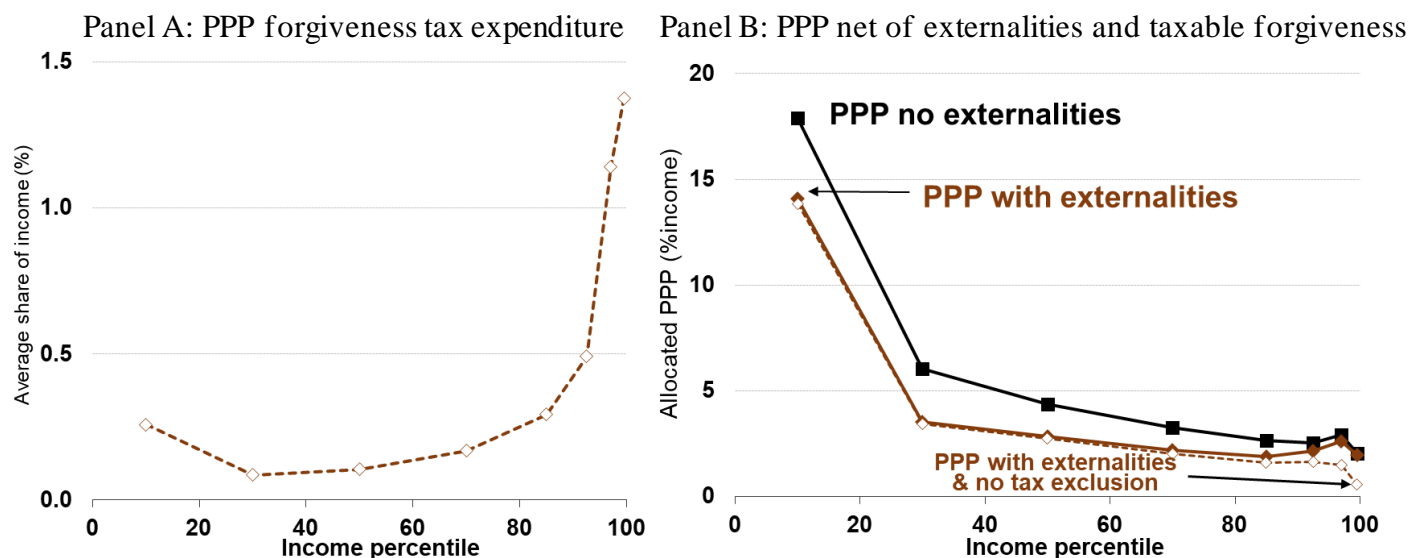
Notes: See above. *Source:* Authors' calculations using SBA and tax data.

Figure A9: Firm counts by reported firm size in PPP loan application



Notes: Firm counts are for ten-employee wide bins. In first-draw PPP applications (SBA data) on the right, over 2,000 firms report being in the 10-employee range just below 500 employees (i.e., 491 to 500 employees), while fewer than 500 firms report being in the 10-employee range just below that (i.e., 481 to 490 employees). Source: SBA data and authors' calculations merging both SBA and tax data.

Figure A10: PPP forgiveness exclusion from taxation, 2020



Notes: An embedded net fiscal cost of the PPP is that forgiven PPP loans were excluded from firms' taxable income, even though the expenses that the loans funded were still deductible for tax purposes. This implies a tax expenditure. We multiply 2020 first-draw loan forgiveness by average marginal tax rates of business owners to estimate a tax-exclusion tax expenditure of \$84 billion that is distributed as in Panel A. Average marginal tax rates are estimated for owner profit increases of \$25,000 using the simple tax calculator and weighting by ownership. A 21 percent tax rate is applied to C corporations. Tax expenditures ignore responses that would decrease the estimate, such as any expected loan-forgiveness underreporting if forgiven amounts were taxable. For income definitions, see Figure 4 notes. Source: Authors' calculations using SBA and tax data.

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