The Paycheck Protection Program: Progressivity and Tax Effects

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Abstract

The 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 the distribution of worker and owner portions of this relief. Over half of the PPP supported worker-retention costs and impacts were progressive. Bottom-quintile incomes increased 18 percent, while top-quintile incomes increased 2 percent. The PPP increased taxes and decreased unemployment compensation, reducing net program costs by one-quarter. Net costs could have been even lower without the tax exclusion of PPP forgiveness.

Keywords: Paycheck Protection Program, PPP, Covid-19, employment protection, countercyclical policy, redistribution, stimulus checks, unemployment insurance

JEL: D31, E24, H22, H23, H25, H32, H53, H81, J65

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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 a sufficient share was spent on payroll costs, among other requirements. 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 businesses and then linking businesses to the tax returns of employees and owners.

Our estimates indicate the PPP was weakly progressive, a large share of the PPP went to workers, and there were large reductions in net program costs from fiscal externalities (increased taxes and avoided unemployment benefits). Based on linked tax data, we estimate that the PPP especially benefitted owners and workers in the bottom quintile of the income distribution. Based on differences in the timing of PPP loan receipt, we estimate that over 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 firms occupied similar parts of the income distribution.

Overall, the PPP's impact on individual incomes was weakly progressive: bottom-quintile incomes increased 18 percent, middle-quintile incomes increased 4 percent, and top-quintile incomes increased 2 percent. Although we still find a large portion of overall PPP loan forgiveness (44 percent) accrued to the top quintile of income earners, this is a smaller share than previous estimates.[1] Additionally, we show that the PPP was less progressive than other major relief programs. This is consistent with the PPP's goal of retaining employees throughout the (unequal) wage distribution. Redistribution was not a clear goal of the PPP.

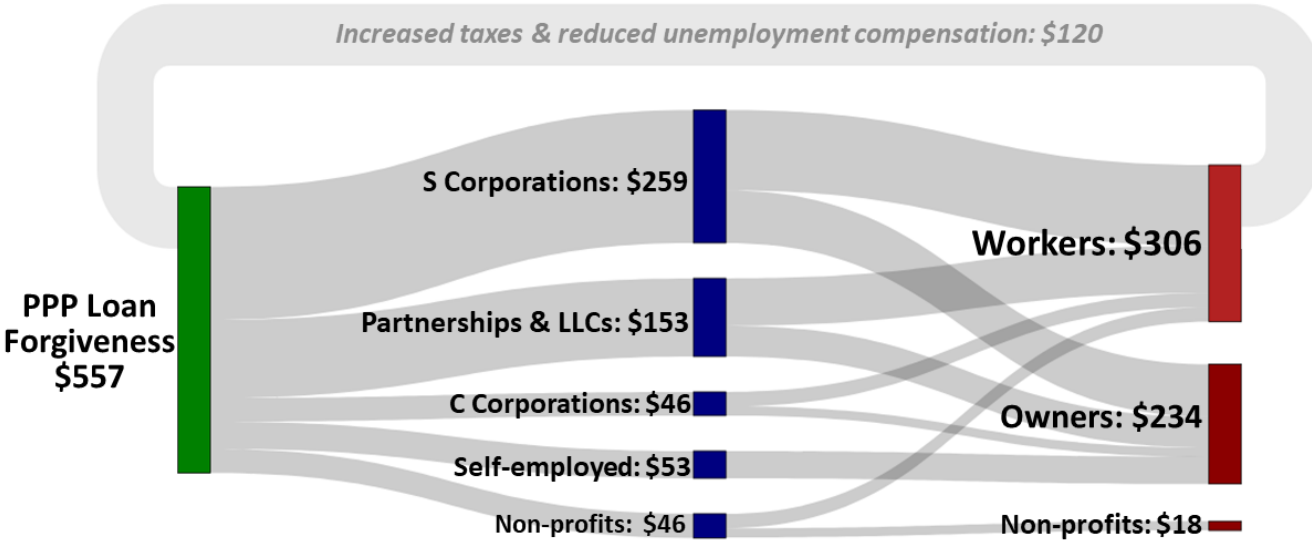
PPP costs were offset by fiscal externalities from increased taxes and avoided unemployment compensation. Leveraging our links to individual tax returns, we estimate the extent to which the PPP increased taxes via higher wages and reduced temporarily expanded unemployment compensation. These fiscal externalities reduce the PPP's estimated net fiscal cost by one-quarter and modestly reduce estimated progressivity. They also lower the estimated gross cost per job-year retained falls from $133,000 to a net cost of $101,000.

This paper extends prior PPP studies, making several empirical contributions. First, we match PPP loan data from the Small Business Association to businesses in tax data. Second, we estimate the PPP's employment and wage effects by building on prior work using dynamic difference-in-differences estimates (Autor et al. 2022b; Dalton 2023). We also account for non-wage payroll costs ignored in prior estimates. This suggests 55 percent of PPP loan forgiveness went to employee-retention costs (the worker share), just below the general forgiveness requirement that 60 percent go to payroll costs.[2] Figure 1 shows

[1] Autor et al. (2022a) estimated a top-quintile share of 72 percent using imputations that missed firm-specific impacts, see section I.A.

[2] Despite this similarity, our estimates are based on estimated average treatment effects (i.e., economic incidence and not statutory incidence). The original 75-percent forgiveness requirement was retroactively reduced to 60 percent on June 5th.

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



Notes: Owner share includes all PPP forgiveness from self-employed firms.
Source: Authors' calculations using SBA and tax data.

how PPP forgiveness amounts flow to different types of business and then to workers and owners. Third, to estimate distributional effects we link businesses to worker and owner incomes. In contrast to prior distributional assumptions, these matches allow for well-estimated distributional impacts and suggest the PPP benefited many low-income business owners. Our progressive PPP result is robust to alternative assumptions, even a zero-worker share (owner-only) assumption. This results from the similar income distributions of owners and workers. Fourth, we estimate how much the PPP increased tax receipts and reduced unemployment compensation, as seen with the estimated $120 billion returning to the federal government at the top of Figure 1.

Our analysis has limitations. While our estimates align with the national accounting cost-based valuation of government spending, welfare-based measures could capture higher benefits of the PPP from preventing long-term job scarring.[3] Considering longer-run and macroeconomic effects would also change the PPP's overall estimated impact. Higher estimates of benefits result from accounting for firm-specific human capital or frictions in restarting businesses that would have closed without the PPP. Conversely, benefits may be overstated if the PPP preserved less productive employment relationships and prevented the formation of more productive employment matches. Further, if the PPP prevented a sustained recession and contributed to the post-Covid recovery, this could imply additional benefits and even lower net fiscal costs. Finally, our estimates are for a pandemic-driven recession and the applicability to standard downturns is unclear.

[3] Rose and Shem-Tov (2023) estimated a cumulative earnings loss of $40,000 among full-time workers losing jobs in firm-wide contractions. Davis and von Wachter (2011) estimated that displaced men lose an average of 2.8 years of earnings in mass-layoff events with high unemployment. Considering the output of workers with PPP-retained jobs, however, may suggest more benefits accrued to owners and consumers.

I. PPP Policy Background and Related Literature

Nearly $1 trillion of relief supported U.S. employee retention during the pandemic. The majority was from $800 billion of PPP loans. Additionally, over $150 billion of employment retention tax credits targeted smaller firms with gross receipts declines (Internal Revenue Service 2023). The distribution of these credits appears consistent with our estimated PPP distributions (Goodman 2023). Nearly $30 billion of grants were allocated to businesses with the Restaurant Revitalization Fund. Other employer-based programs included sick and family leave tax credits and payroll tax deferral (Goodman 2021).

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.[4] For each eligible firm, original first-draw loan amounts were for up to 2.5 months of average payroll costs (calculated 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 forgiven. Second-draw loans totaling $209 billion were distributed in 2021 to firms with no more than 300 employees and revenue decreases. Dilger, Lindsay, and Lowry (2021) provide details.

In this paper, we focus on the $557 billion of first-draw PPP loan forgiveness.[5] 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 loan amounts. These requirements highlight the intent of retaining employees and preserving their wage levels and employer-sponsored health insurance, as well as preventing some business closures by allowing some non-payroll expenses.

A. Prior Analysis of PPP

PPP research has focused on estimating the share of PPP loans spent on retaining employees, the cost per job-year retained, and distributional effects. The share of PPP loans going to employee wages was estimated to be up to 34 percent by Autor et al. (2022a) using private payroll data. 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

[4] 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.

[5] 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 for estimates in this paper based on 2020 income.

insurance payments for employees, suggests 55 percent of PPP forgiveness went to employee-retention costs.

Estimating the cost of retaining employees is a major research question. We estimate a PPP cost per job-year retained of $133,000 before considering fiscal externalities. This is just below 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 4.2 million job-years retained. In contrast to these estimates of job-years, a focus on possibly shorter-duration jobs saved suggests costs of $34,300 (Faulkender, Jackman, and Miran 2023) and $32,000 to $67,000 per job saved (Bartik et al. 2021).[6] These estimates suggest 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).

Higher cost estimates in Autor et al. (2022a, 2022b) and Chetty et al. (2023) likely resulted their choice of estimation strategy, specifically, comparing large firms just above and below the original firm-size cutoffs for PPP eligibility, usually at 500 employees. Although this approach theoretically provides a valid control group, it suffers bias in several ways. First, because larger firms were less negatively affected by the pandemic than smaller firms (Faulkender, Jackman, and Miran 2023), using larger firms as a control group systematically biases down estimates of jobs saved. That is, by focusing solely on large firms just above and below the threshold, the methodology overlooks the stronger responses by smaller firms.[7] Additionally, our data suggest there was no actual treatment discontinuity at 500 employees (see appendix Figure A1). Firms with more than 500 employees frequently received first-draw PPP loans, likely due to various exemptions and inconsistent enforcement.[8] As a result, the control group for this methodology is contaminated with treated firms, further biasing down estimates of jobs saved. Finally, the $10 million cap on PPP loans was a binding constraint for many larger firms, which potentially attenuates efficacy for larger firms.

[6] Doniger and Kay (2023) focus on spring 2020, when jobs were likely more sensitive to PPP funding. Their estimates suggest a marginal cost per job saved of $17,000.

[7] For firms with more than one hundred employees, Faulkender, Jackman, and Miran (2023) estimated they had less than one-fifth the effect on job preservation as smaller firms. Dalton (2023) estimated that large firms had one-quarter the average effect on wages.

[8] Beggs and Harvison (2023) found evidence of employee number misreporting, as the PPP application process essentially used the "honor system." Autor et al. (2022b) directly accounted for industry-based size exemptions and made additional adjustments, in part, due to exemptions based on gross receipts or assets and net income. Faulkender, Jackman, and Miran (2023) found many large firms without exceptions still received PPP loans and discussed size-threshold approach issues.

Our analysis also offers several additional differences from prior work to enable a more accurate distributional analysis. First, Autor et al. (2022a) estimated 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 significant PPP loans going to low-income owners, including many self-employed individuals. Second, while Autor et al. estimated worker distributional effects by allocating wage declines to match employment losses across the income distribution, 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. Third, they defined income as after taxes and transfers, which deviates from standard approaches.[9]

This paper expands upon prior work using tax data to match businesses to owner incomes by Love (2021) and Goodman (2023). Using tax data to distribute only the PPP's owner portion, Larrimore, Mortenson, and Splinter (2023b) concluded that the PPP's owner portion had a U-shape over the wage. These estimates, however, were based on estimated rather than linked PPP loans, a limitation motivating this study.

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 because of the PPP. This firm-preservation effect contributes to preserved wages. Granja et al. (2022) showed that banks influenced PPP take up and that firms used PPP loans for buffer savings and non-wage expenses. Hubbard and Strain's (2020) estimates suggest the PPP helped firms pay bills on time. Beggs and Harvison (2023) found that PPP application misreporting is associated with firms having employment growth. The PPP 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). As compared to inflationary effects from relief to households, the PPP resulted in little short-term inflationary effects (Hale, Leer, and Nechio 2023). Smart et al. (2023) studied the Canadian pandemic wage subsidy. Strain and Veuger (2023) and Hong and Lucas (forthcoming) compared Covid-era credit programs across developed countries.

[9] This shift from the standard redistribution rate denominator (income before taxes and transfers) results in a downward bias for transfer progressivity, partly from re-ranking individuals. Splinter (2020) discussed standard redistribution measures.

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 the firm's workers and owners to allocate the respective portions of the PPP. Population-wide incomes are 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 include quarterly payroll tax filings for employers (Form 941) and annual entity-level tax returns (e.g., Forms 1120-S and 1065). 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 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 matches capture 74 percent of first-draw PPP forgiveness amounts in the SBA data.

B. Linking Firms to Workers and Owners

We match each firm to its workers' and owners' individual tax returns to allocate PPP loan forgiveness over the income distribution. 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 sum unemployment compensation on Form 1099-G and aggregate them to firm-level recipient counts and amounts.

Owner portions of PPP loan forgiveness are allocated proportional to each owner's share of the firm. Sole-proprietorship owners, including 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. Among firms for which we match to an EIN, these links account for 68 percent of PPP forgiveness. Nearly one-third of the incomplete matches is due to non-profits, which cannot be matched to owners. 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 de-identified taxpayer identification numbers of workers and owners to match them to their respective individual tax returns and to other information returns. This allows us to assign each individual worker and owner to an income group as described below. This step also accounts for sole-proprietorship ownership because Schedule C is part of individual tax returns.

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 expansions and corrections: add adjustments, nontaxable interest, and nontaxable Social Security benefits; remove taxable unemployment compensation (most was already excluded from taxation 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.

Following a common approach in 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 U.S. Census' equivalized-income distributions and estimates using tax data by the Congressional Budget Office (2022) and Auten and Splinter (2023), as seen in appendix Table A3.

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

Administrative tax data suggest the PPP helped retain 4.2 million job-years and nearly two-thirds of PPP loan forgiveness supported employee-retention costs. These effects on employment and wages are estimated using a dynamic difference-in-differences estimator developed by Callaway and Sant'Anna (2021). Event-study models are often used to estimate job and wage losses (Miller 2023)—a symmetric application to our estimates of job and wage retention. The underlying data are 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. 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.

$$ATT_{p,q} = \mathrm{E}[\, Y_q(\mathrm{PPP}) - Y_q(\mathrm{No\ PPP})\,] \qquad (1)$$

The coefficient estimated using the dynamic difference-in-differences estimator is an average treatment effect on the treated (ATT). In Equation 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).

$$Y_{i,q,t,s,j,f} = \frac{y_{i,q,t,s,j,f}}{(y_{i,q,t,s,j,f} \mid t{=}2018)} \qquad (2)$$

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

In Equation 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 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.

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

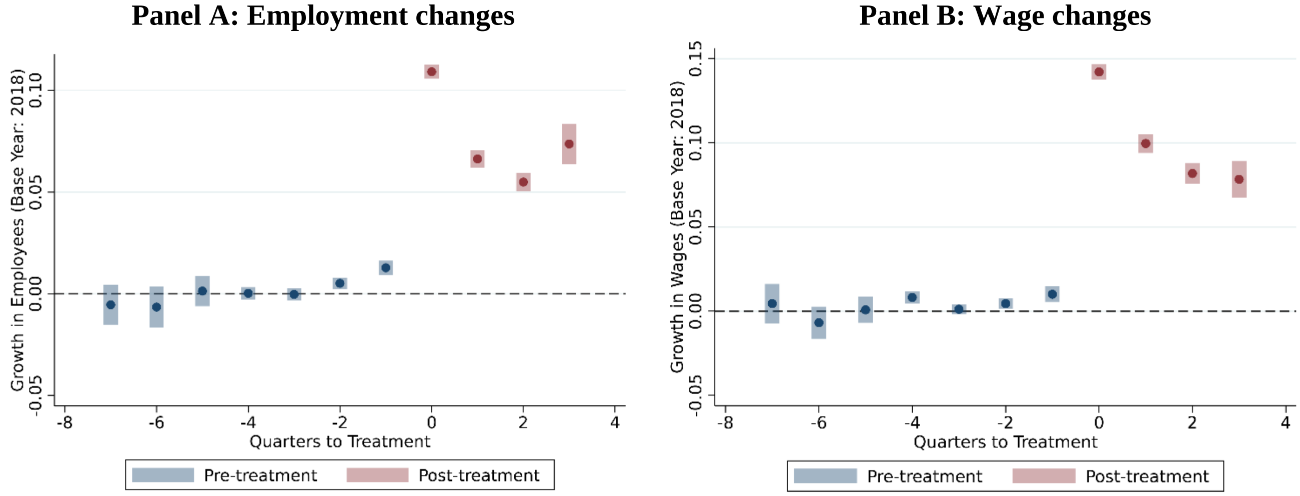
Figure 2A displays coefficient estimates and confidence intervals associated with quarterly employment growth pre- and post-treatment. The coefficient in the period of initial treatment is around 0.11, which can be interpreted as firms receiving first-draw PPP in that quarter resulted in 11 percentage point 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 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. Results in Table 1A suggest the first-draw PPP forgiveness retained 4.2 million job-years at a cost of $133,000 per job-year retained. Table 1B shows that first-draw PPP loans that were forgiven induced $338 billion of additional employee-retention costs. This represents 55 percent of PPP loan forgiveness.[11] As discussed in the appendix, this result is robust to alternative approaches. For example, restaurants had nearly identical first-quarter and second-quarter effects.

[10] 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.

[11] Using population-level estimates—as opposed to firm-size specific regressions results—suggest higher estimates of 4.5 million job-years retained and 59 percent of loan forgiveness supporting employee payroll costs. Excluding PPP amounts going to self-employed firms suggests 61 percent of forgiveness supported employee payroll costs.

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

Panel A: Retained Employees from PPP

Firm-Size Group	ATT_e	Base Employees (millions)	Retained Job Years (millions)	PPP ($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**

Panel B: PPP worker share (costs to retain employees)

Firm-Size Group	ATT_w	Base employee costs ($billions)	PPP employee retention costs ($billions)	PPP ($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: ATT is averaged across four quarters and total ATTs 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 or 2019 quarter corresponding to quarter of treatment in 2020. Base wages are from Form 941, indexed to 2020 dollars, scaled by the match rate, and increased 20 percent to account for non-wage costs of retaining employees. 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.

IV. Distribution of PPP Effects

To allocate PPP loan forgiveness over the income distribution, we first divide forgiveness into worker and owner portions. The worker portion accounts for the costs of retaining employees and represents 55 percent of first-draw PPP forgiveness. The owner portion is the remaining amount and is allocated based on firm ownership. All allocations are done at the level of eight annual 2020 income groups: the bottom four quintiles, P80–90, P90–95, P95–99, and the top one percent.

The worker portion 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 wages) gives a progressive result. Allocating proportional to wages gives a less progressive result. The lump-sum allocation would be more appropriate when the PPP helped firms to retain all employees—but for more time among low-wage employees who may have been more likely to become unemployed or to have longer unemployment durations. The proportional allocation would apply when the PPP prevented firm closure or reduced unpaid furloughs of equal time for all workers at a firm. As discussed in footnote 9, estimates suggest that low-wage workers had disproportionately large wage declines, which points towards a lump-sum allocation. Given this evidence and the small differences between the allocations, we simplify the main exposition by using the average of lump-sum and proportional allocations and consider both in the robustness checks.

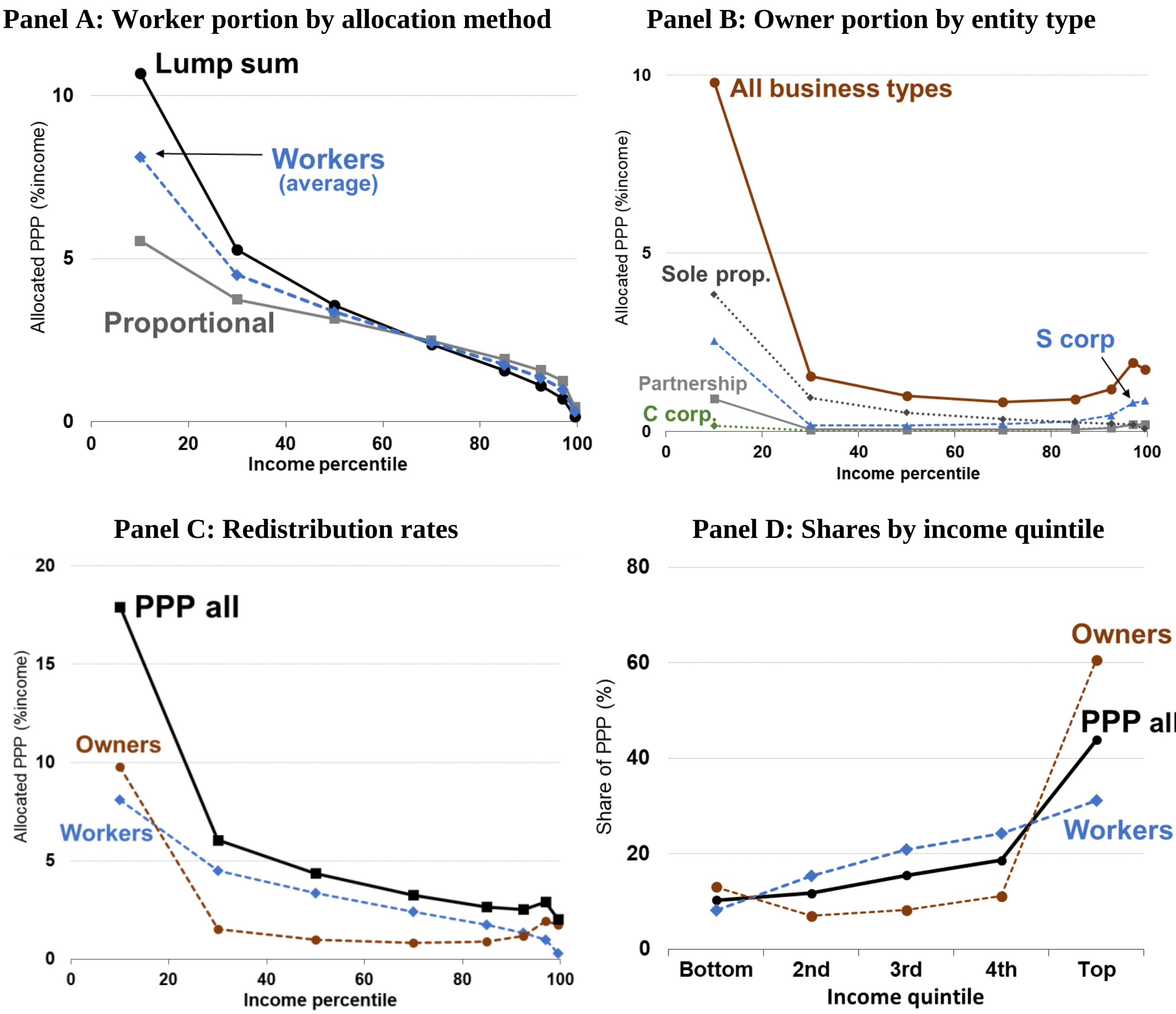
The owner portion represents the 43 percent of PPP forgiveness left after deducting the estimated worker portion and the implied amount going to non-profit owners ($18 billion). Figure 3B shows redistribution rates of the owner portion by tax-data entity type. The levels of these redistribution rates are determined by the share of loans going to each type of business. 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 and (to a lesser degree) top 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 of the income distribution.

The combined effects of the worker and owner portions are weakly progressive. 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. Figure 3D shows the shares of each PPP amount by income quintile: the bottom quintile receives 10 percent while the top quintile receives 44 percent.[12] Due to the lower incomes of the bottom quintile, that smaller share of PPP represents a higher share of income, which is the basis of progressivity measures.

[12] 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 $100,0000 (Goodman 2023).

Also, we caution that PPP progressivity may be underestimated in our analysis. 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, as noted by Dalton (2023) and 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 mean we may allocate too much to owners. Third, our income definition used to rank individuals includes PPP-retained wages. While this allows our estimates to be incorporated into conventional distribution measures using observed wages, they understate the PPP's progressivity because increased wages moved some individuals up the income distribution.

Figure 3: PPP Distributions, 2020



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 B is grouped by tax-data based entity types where unmatched types are not shown. *Source*: Authors' calculations using SBA and tax data.

Our progressive PPP result is robust to alternative assumptions. To simplify, we present distribution-wide progressivity summary measures.[13] A positive value means the PPP was progressive and our baseline progressivity is 0.38. First, worker portions allocated proportional or lump-sum gives progressivities of 0.31 and 0.46. Second, worker shares of zero and 100 percent gives progressivities of 0.22 and 0.63. Third, allocating the seven percent of PPP forgiveness among unmatched firms to the top or bottom quintiles gives progressivities of 0.31 to 0.46. Finally, combining the zero-wage-share and top-quintile unmatched assumptions gives a lower-bound progressivity of 0.17, which is still progressive.

While we estimate that the PPP had progressive impacts on income, we also find it was less progressive than other major relief (appendix Figure 4). In 2020, unemployment compensation progressivity was 1.4, stimulus payment progressivity was 1.6, and PPP progressivity was 0.4. Unemployment compensation targets individuals with job losses and was especially progressive during the pandemic (Cortes and Forsythe 2023; Larrimore, Mortenson, and Splinter 2023a). Stimulus payments were more progressive than the PPP because they were essentially universal lump-sum transfers (Splinter 2023).

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 amounts reduced by non-wage costs that are 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 (because counterfactual wages without the PPP would have been lower). 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 ($137,700 in 2020) 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 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 costs in 2020.

[13] 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.

PPP loans that successfully preserve 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).[14] 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 per retained job-year. Note that this estimated reduction has significant uncertainty.

Figure 4 shows the distribution of these estimated effects from worker taxes. These estimated externalities from taxes and unemployment compensation reduce the net cost to about 76 percent of the gross cost, suggesting a reduction of the cost per job-year retained from $133,000 to $101,000. The latter estimate is about twice median annual worker earnings in 2020. Also, recall that the PPP's targeted smaller businesses that had below-average wages. Externalities impact the entire distribution with little impact on progressivity.

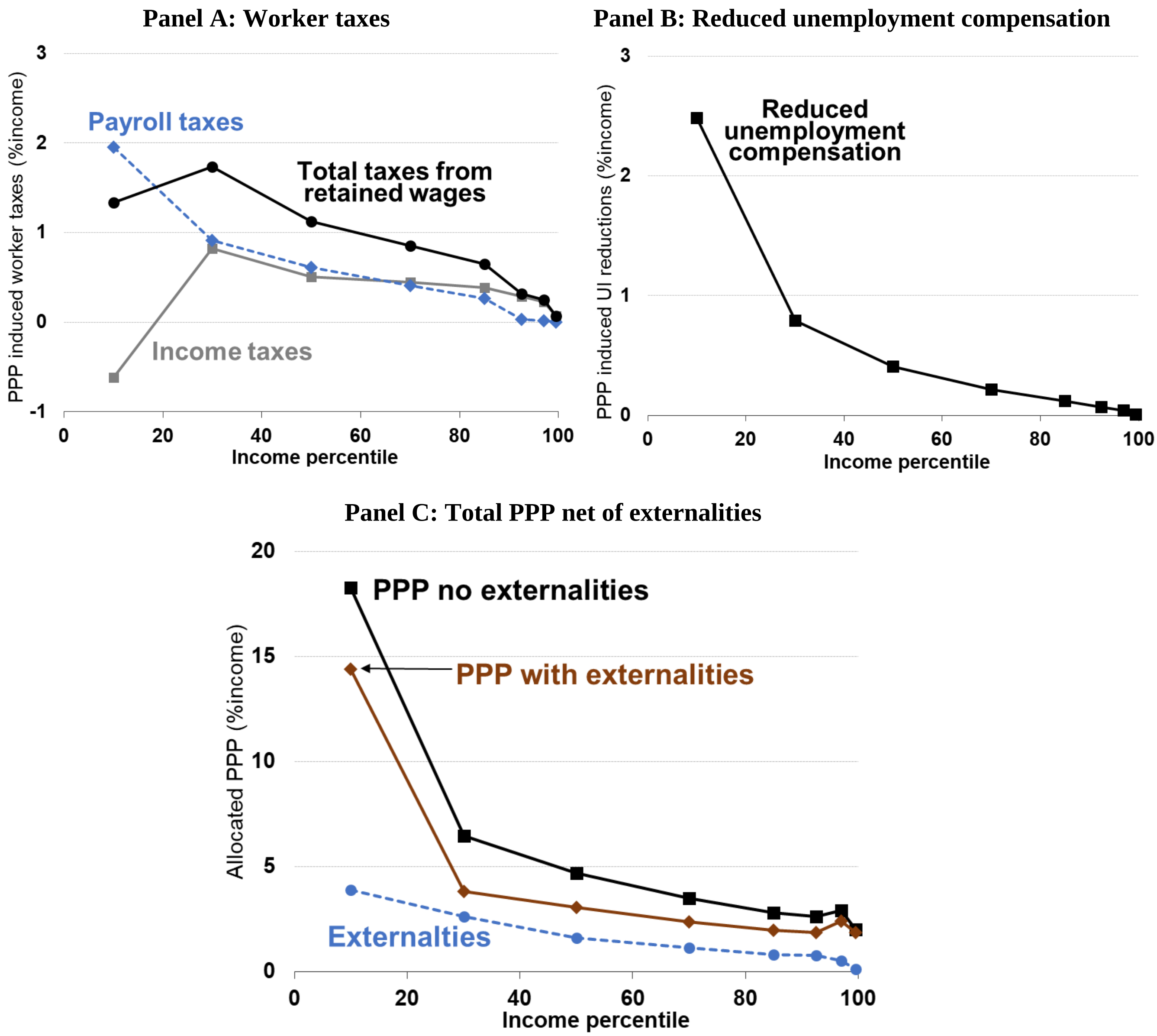
The PPP had other effects beyond the two externalities considered here. Our analysis excludes additional program costs from overhead and bank fees, although most loan amounts only had a one percent bank fee. It also excludes some sources of lower net costs: (1) additional taxes or reduced unemployment compensation in later years, (2) additional state taxes from increased wages, (3) additional taxes from induced taxable business profits, and (4) reductions in other government programs such as Medicaid, disability, and SNAP.

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 asymmetric treatment implies a tax expenditure. Multiplying 2020 loan forgiveness by average marginal tax rates of owners, we estimate a tax-exclusion tax expenditure of $84 billion, or nearly one-fifth of the PPP's gross cost.[15] About 90 percent of this tax expenditure accrues to the top quintile. This suggests an alternative PPP policy in which forgiven loans were taxed would eliminate much of top-quintile net relief and further reduce the net cost.

[14] These unemployment compensation amounts in tax data are comprehensive, including all federal expansions, and include over twice the amount in the CPS (Larrimore, Mortenson, and Splinter 2023a). Note that many firms receiving PPP still laid some workers off for spells, allowing them to claim unemployment benefits.

[15] AMTRs of owner profit increases of $25,000 are estimated 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 loan-forgiveness underreporting.

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



Notes: See Figure 1 notes. *Source*: Authors' calculations using SBA and tax data.

VI. Conclusion

It is unclear who bears the burden of business-level taxes. Hence, a significant literature explores the distributional incidence of corporate 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 direct and net distributional impacts of the PPP.

We present a novel assessment of the incidence of the PPP. Autor et al. (2022a, p. 56) summarize the current view—the "incidence of the program across the household income distribution was highly regressive" and the PPP had a "substantial" cost per job-year retained. Using administrative tax data, which was not used in prior studies, we estimate that the overall distribution of the PPP was less unequal than previous assumptions suggested, and that the effects of the PPP on individual incomes was progressive. While the PPP had a less progressive impact than targeted relief like unemployment benefits, this is consistent with one of the PPP's objectives: retaining employees across the distribution. Moreover, when considering PPP-induced decreases in unemployment benefits and increases in taxes paid, progressivity is unchanged and the average cost per job-year retained falls from $133,000 to $101,000. 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 additional macroeconomic effects (see review in Chodorow-Reich 2019), while Covid aid to state and local governments had an estimated $850,000 cost per job-year (Clemens, Hoxie, and Veuger 2022).

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. Given the unique pandemic circumstances during which the PPP was implemented, it is unclear to what degree this paper's distributional estimates would apply to another PPP-like program. Despite these limitations, the $800 billion PPP can provide some lessons about effects of job-retention programs during deep economic shocks. As more years of data become available, we may also learn more about PPP's prevention of long-term job scarring effects.

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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**

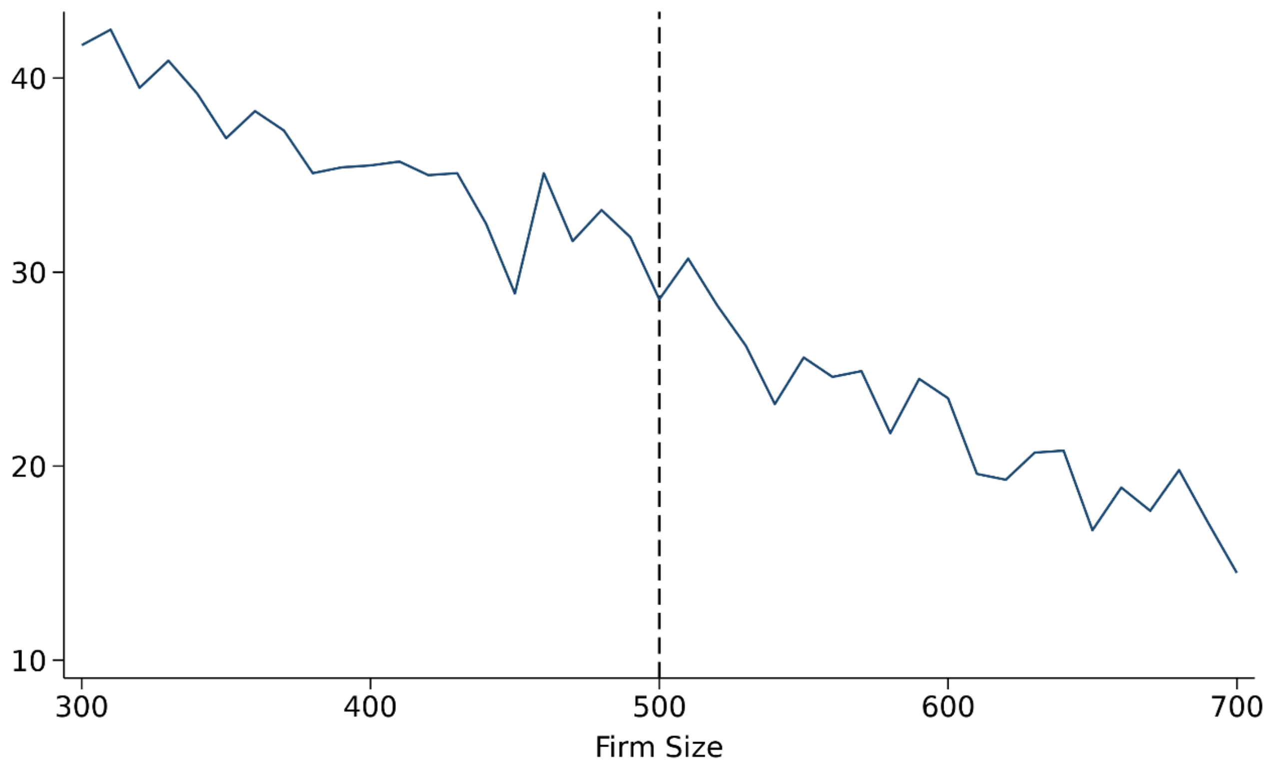
Sources: Authors' calculations using SBA and tax data.

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 (%)

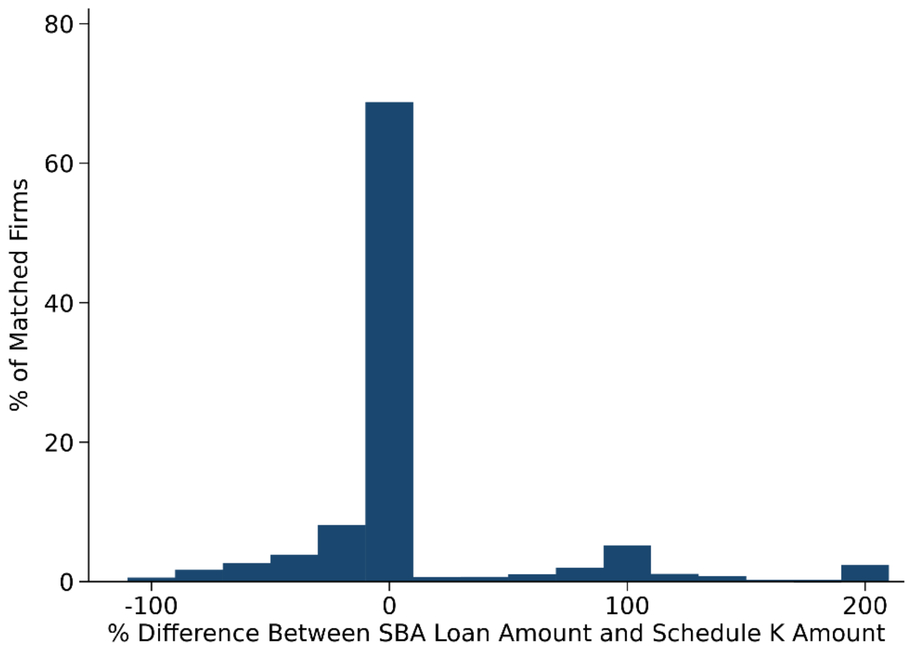


Notes: Within each firm-size bin of 10 employees, take-up rate is the number of SBA-matched firms with first-draw PPP loans divided by the total number of firms with Form 941. Firm sizes are based on the average number of employees reported on filed 2019 Forms 941. *Source*: Authors' calculations using 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. 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 2023), but our measure includes capital gains and certain Social Security benefits.[16]

In the 2020 tax data, we observe 332.7 million domestic individuals, which matches the U.S. Census resident population.[17] 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 (2023) 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 number of individuals on a tax return (filers and dependents).[18] 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 (2023), and authors' calculations.

[16] 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).

[17] 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.

[18] 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 2023), 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.

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	9457	8%
Health insurance	797	9457	8%
Retirement contributions	295	7963	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).

4. Income Variability: Redistribution or Stabilization

These 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 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.

5. 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:

$$r_o = \frac{u_0}{w_0} \qquad \text{(A.1)}$$

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,

$$r_o = \frac{u_0}{w_0} = \frac{u_t^n}{w_t^n} = r_t^n \qquad \text{(A.2)}$$

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. But 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:

$$r_t^a = \frac{u_t^n - u_t^s}{w_t^n + w_t^s} \qquad \text{(A.3)}$$

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:

$$u_t^s = r_o \cdot w_t^n - u_t^a \quad \text{(A.4)}$$

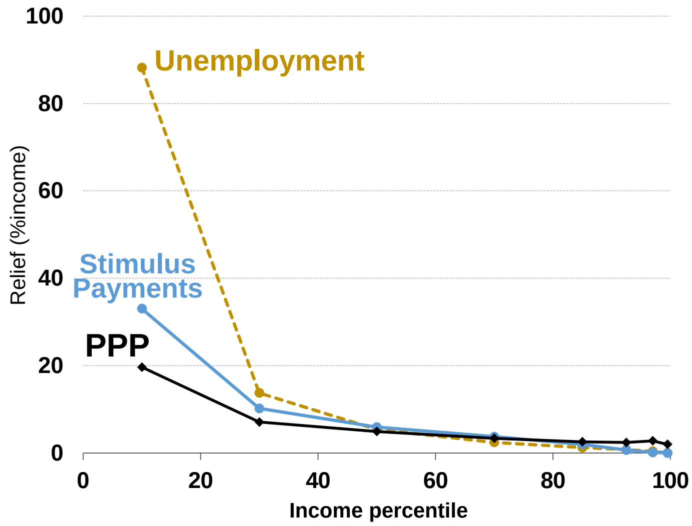
In other words, the avoided unemployment compensation (u_t^s) equals the amount of unemployment compensation that would have been paid for the treated workers without PPP (because $u_t^n = r_t^n \cdot w_t^n$ by definition and $r_t^n = r_o$ by assumption, thus $r_o \cdot w_t^n = u_t^n$) minus the actual 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.

6. 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 progressivities result from relief that decrease with income (negative slopes) or increase more slowly than income (positive slopes 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).

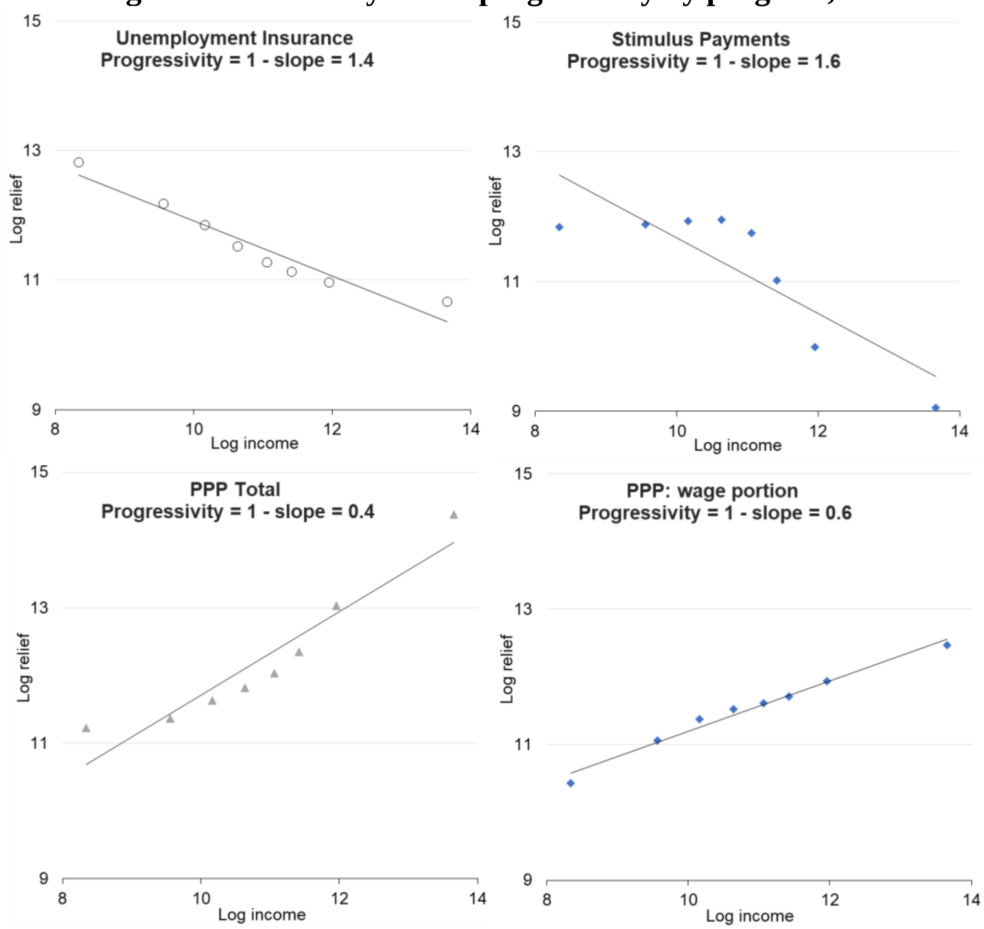
While we estimate that the PPP had progressive impacts on income, we also find it was less progressive than other major relief. Figure A3 shows this differential progressivity with redistribution rates only. Figure A4 shows plots of the average log relief and log incomes for the eight income groups in 2020. The top-left 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. The bottom-left figure shows a positive slope of less than one, meaning absolute amounts of unemployment insurance 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: Redistribution rates by relief program, 2020



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. 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.

Figure A4: Elasticity-based progressivity by program, 2020



7. 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 4 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.

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 ATTs. Their peak 6 percent ATT falls to 2.4 percent by the end of 2020.[19] 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 2A.

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 estimate average treatment on the treated (ATT) rather than intent-to-treat and then adjust those to get ATTs.[20] 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

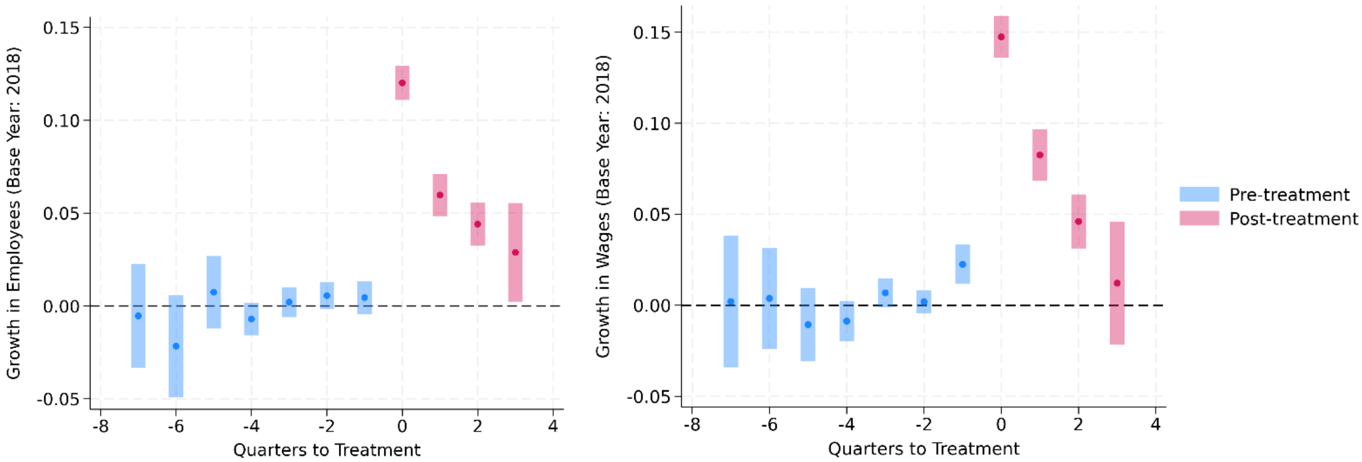
Quarter of Treatment	Mean Employee Count	Mean Wage
Q2 2020	19	13,779
Q3 2020	12	11,390
Q1 2021	15	12,070
Q2 2021	32	11,572

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

[20] 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

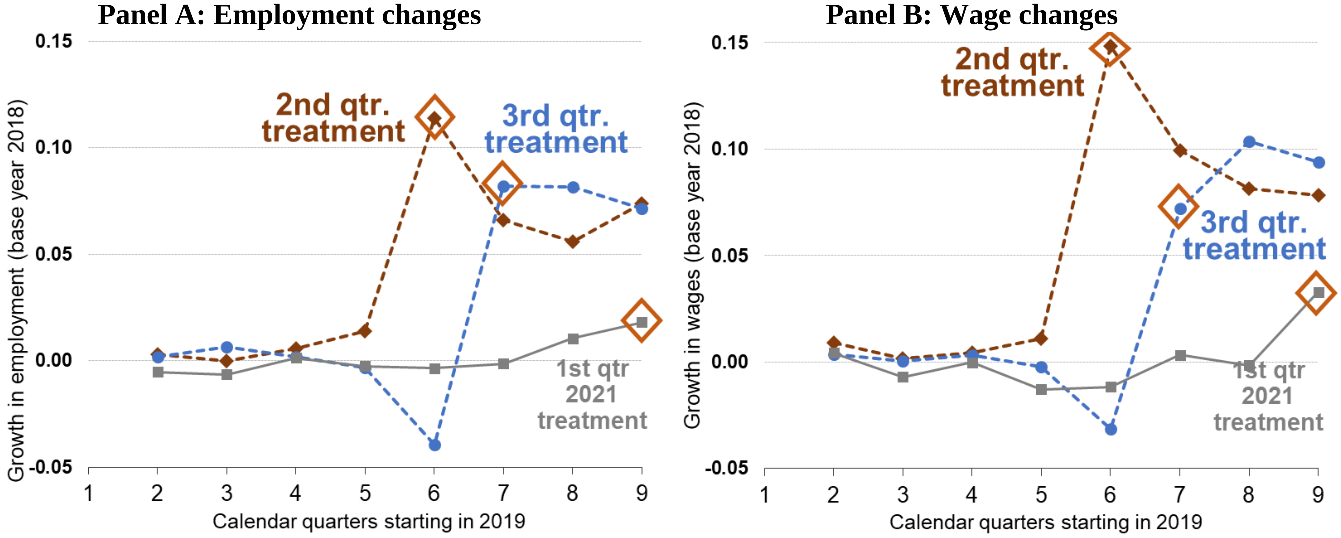
Panel A: Employment changes **Panel B: Wage changes**



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.

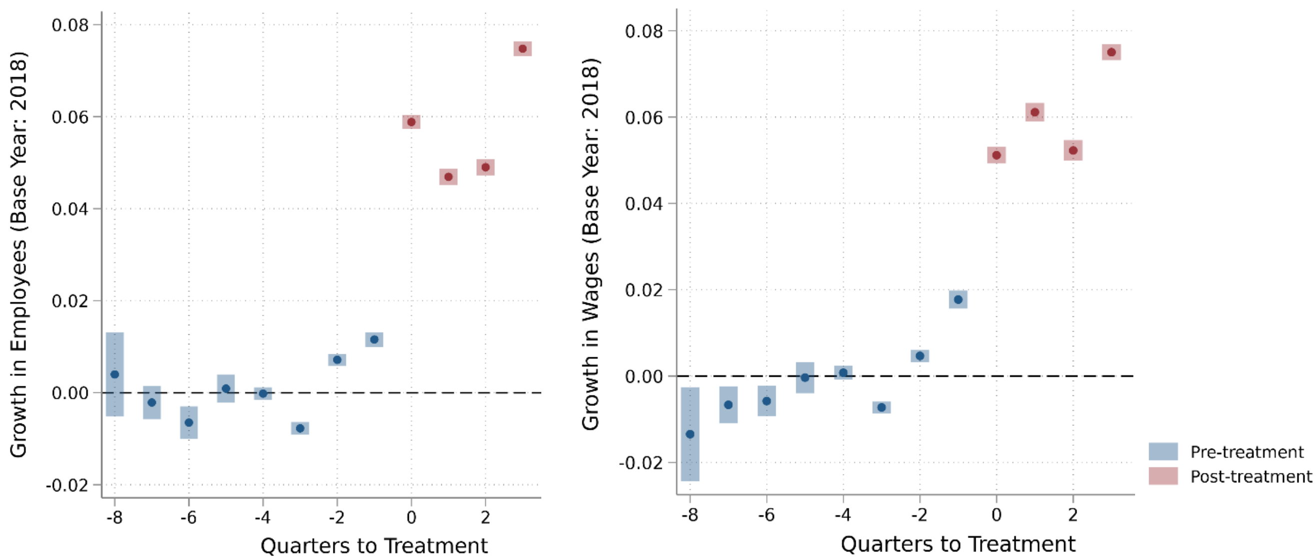
Figure A6: PPP effects on firm-level employment and wages, by calendar quarter

Panel A: Employment changes **Panel B: Wage changes**



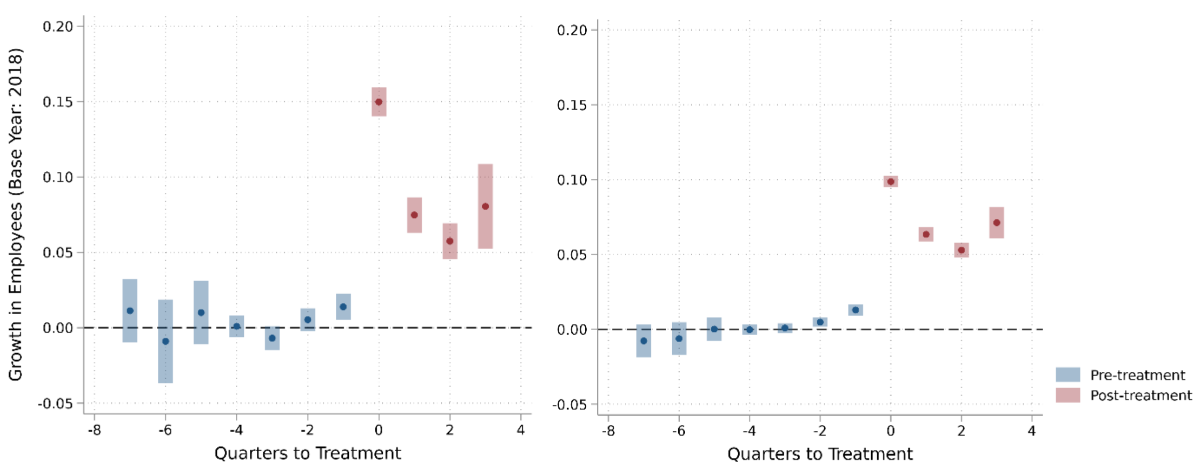
Notes: See above. *Source*: Authors' calculations using SBA and tax data.

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



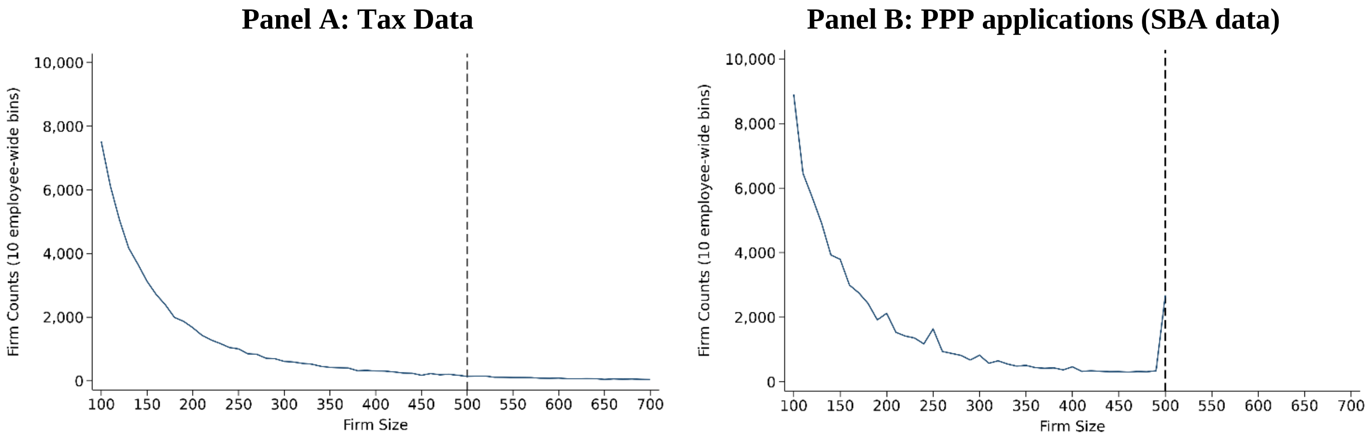
Notes: See above. *Source*: Authors' calculations using SBA and tax data.

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.

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