ONLINE APPENDIX

The Paycheck Protection Program:
Progressivity and Tax Effects

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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**

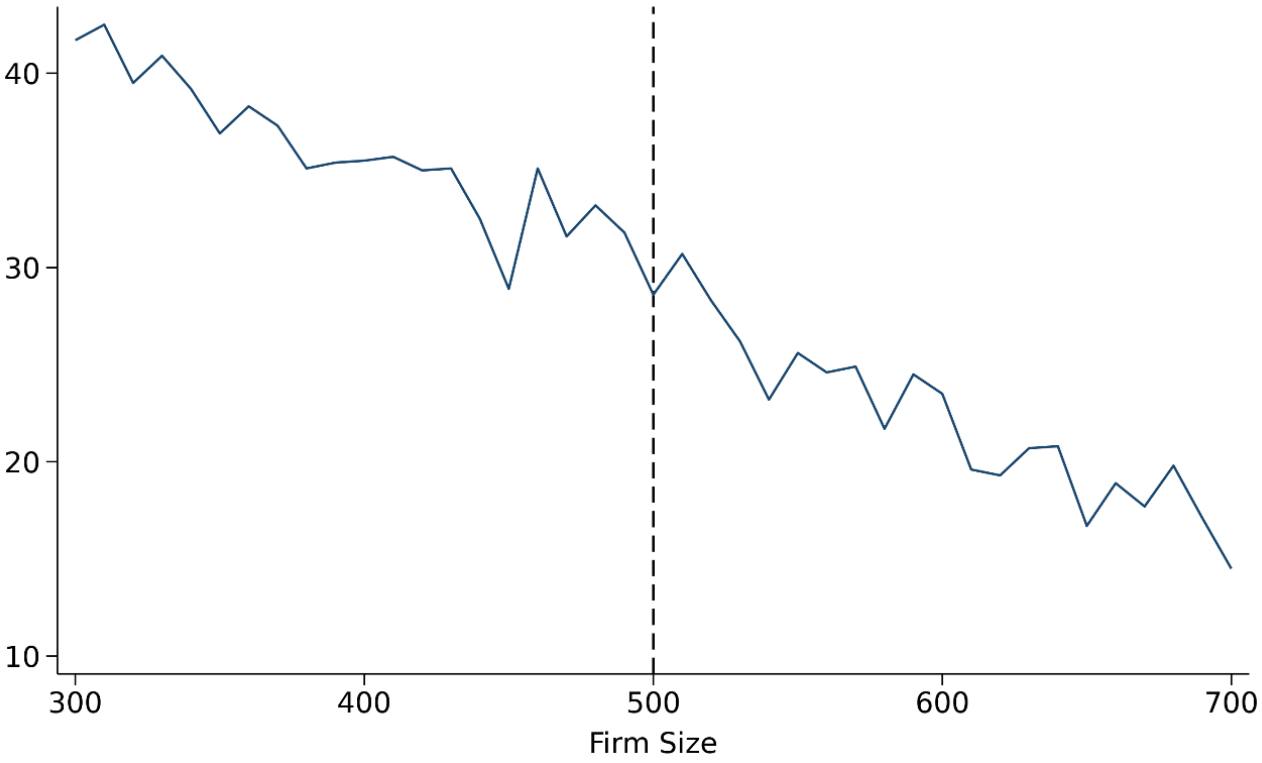
Sources: Authors' calculations using SBA and tax data.

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 (%)



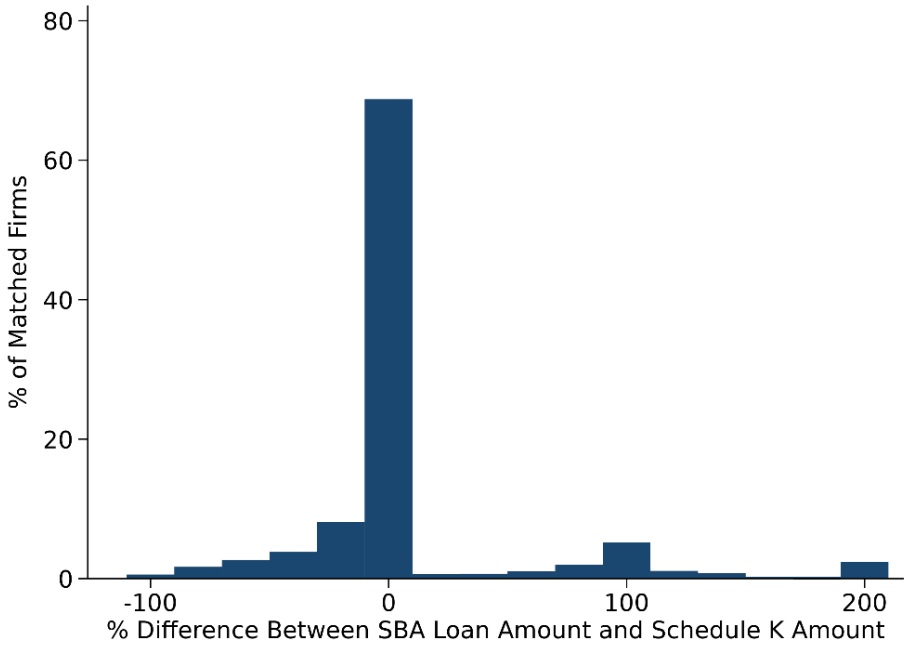
Notes: Within each firm-size bin of 10 employees, take-up rate is the number of SBA-matched firms with first-draw PPP loans divided by the total number of firms with Form 941. Firm sizes are based on the average number of employees reported on quarterly filed 2019 Forms 941. *Source*: Authors' calculations 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.[1] Finally, the panel is balanced by setting the

[1] 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

number of employees and wages to zero in quarters for which a firm was inactive. Wages are adjusted to 2020 price levels.

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

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).

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

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

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.[2] 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

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.

[2] 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.

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).[3]

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.[4]

In the 2020 tax data, we observe 332.7 million domestic individuals, which matches the U.S. Census resident population.[5] 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 number of individuals on a tax return (filers and dependents).[6] Income groups are created based

[3] 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.

[4] 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).

[5] 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.

[6] 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.

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 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:

$$r_o = \frac{u_0}{w_0} \qquad \text{(A.4)}$$

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,

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

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:

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

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.

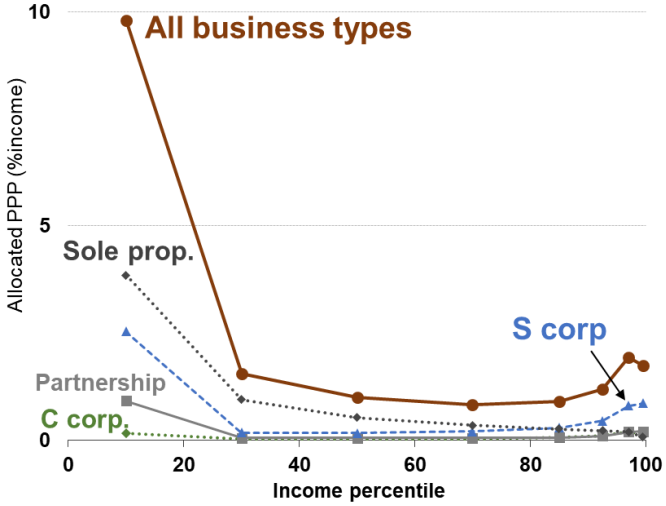
7. Elasticity-Based Progressivity

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.

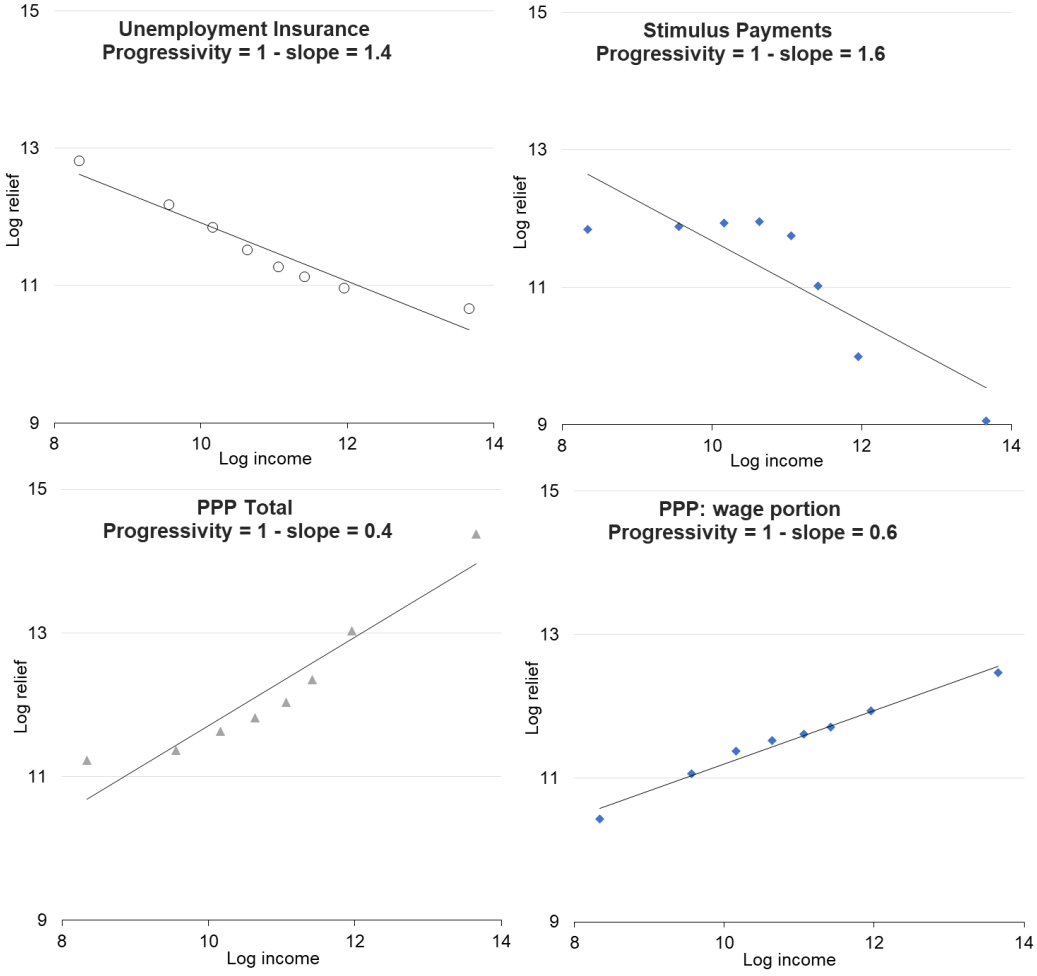
Figure A4 shows the average log relief and log incomes for the eight income groups in 2020. The figure shows a negative slope, meaning absolute amounts of unemployment insurance decrease as one moves up the income distribution, hence the large progressivity of 1.4. The top-right figure shows the same 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.[7] 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.[8] 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.

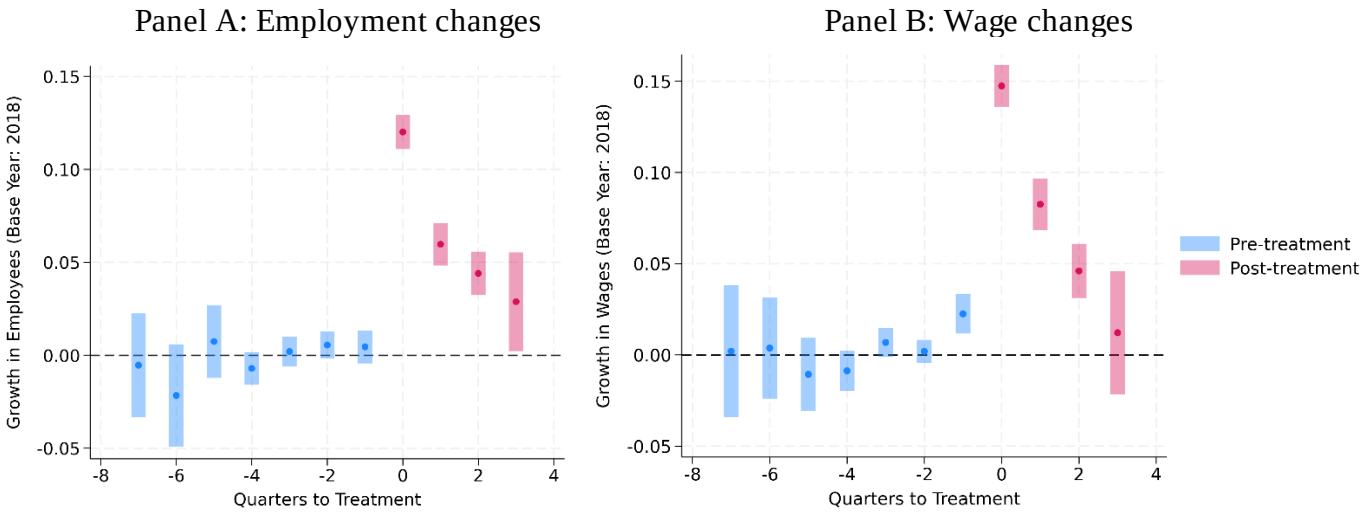
Table A4: Firm characteristics by quarter of treatment

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

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

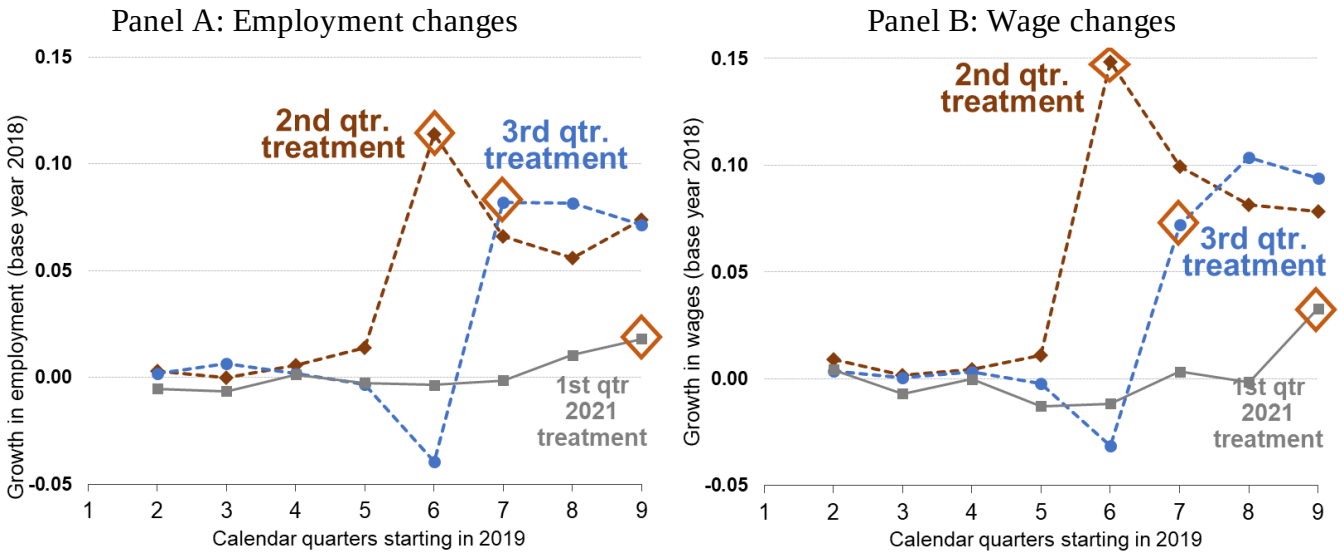
[8] Our regression is: employment growth relative to 2019 average with respect to an indicator for receiving first-draw PPP (at all), state-by-quarter fixed effect, industry-by-quarter fixed effect, and quarter-by-PPP fixed effect. Autor et al. account for higher threshold in select industries. Our results 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



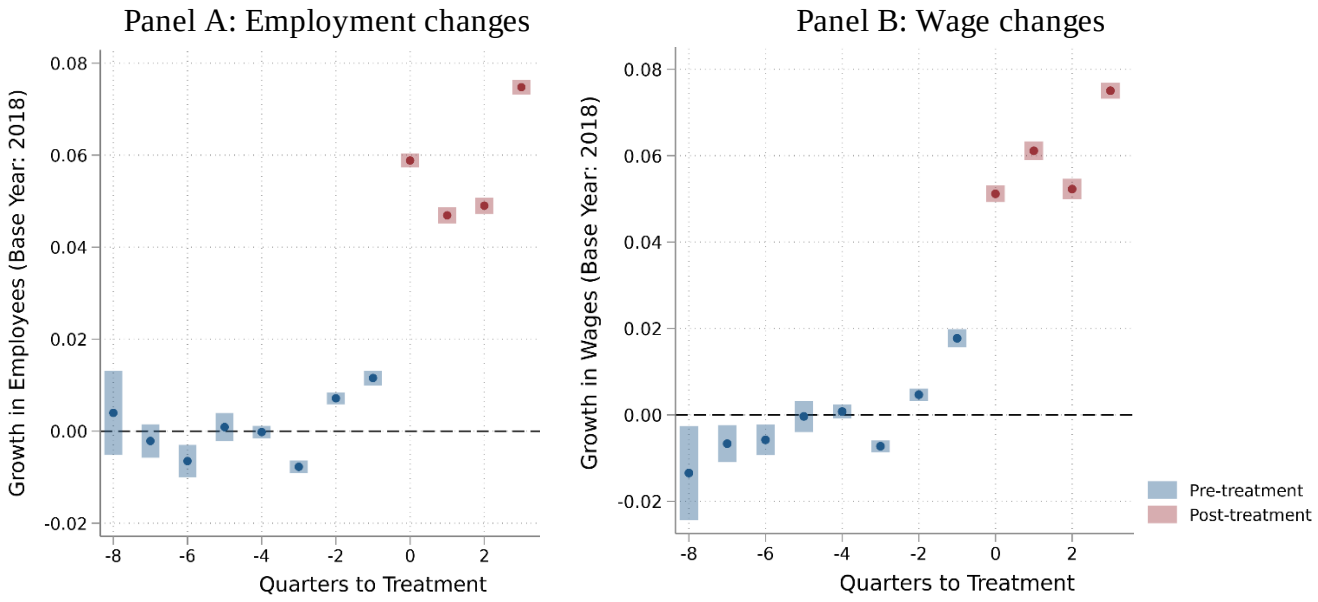
Notes: Only first-draw PPP loans are considered. Dots are average treatment on treated coefficient estimates and ranges are 95 percent confidence intervals associated with quarterly employment growth pre- and post-treatment. Wages are from Form 941 indexed to 2020 dollars. *Source*: Authors' calculations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

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



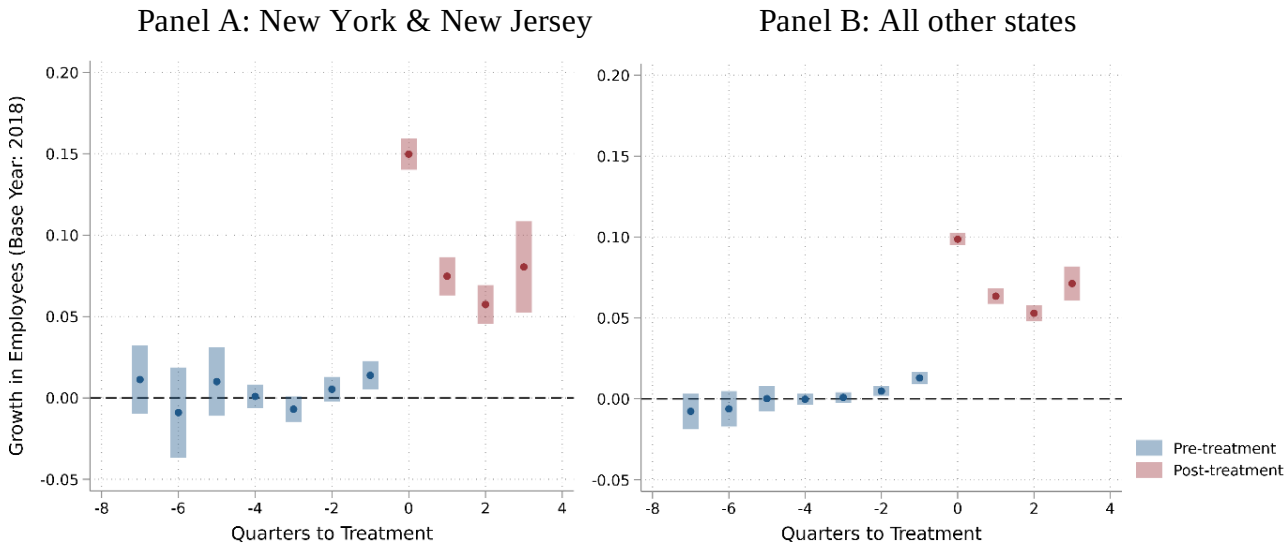
Notes: See above. *Source*: Authors' calculations using SBA and tax data.

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



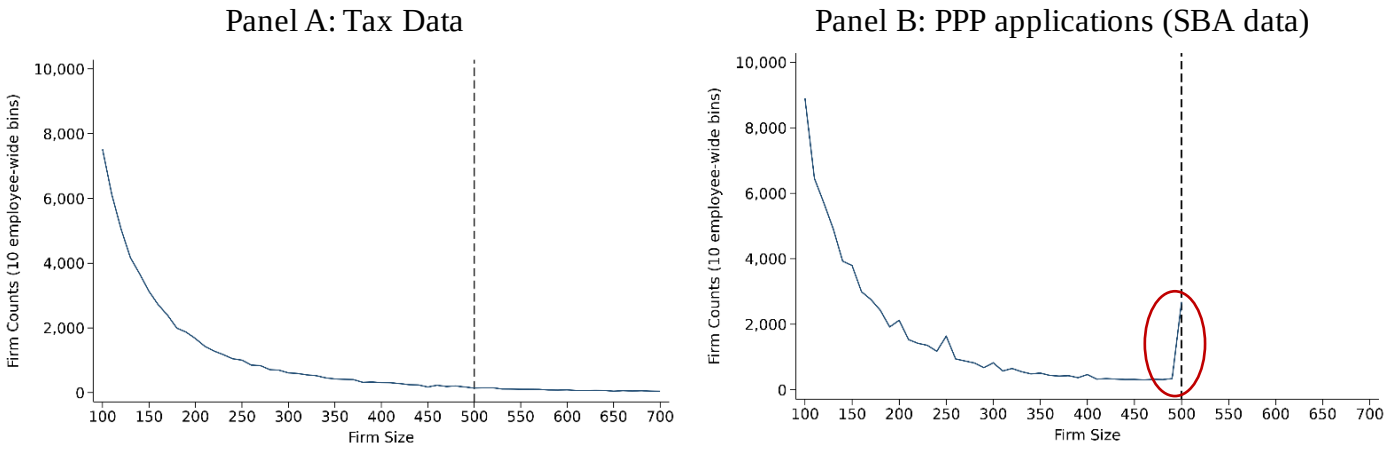
Notes: See above. *Source*: Authors' calculations using SBA and tax data.

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



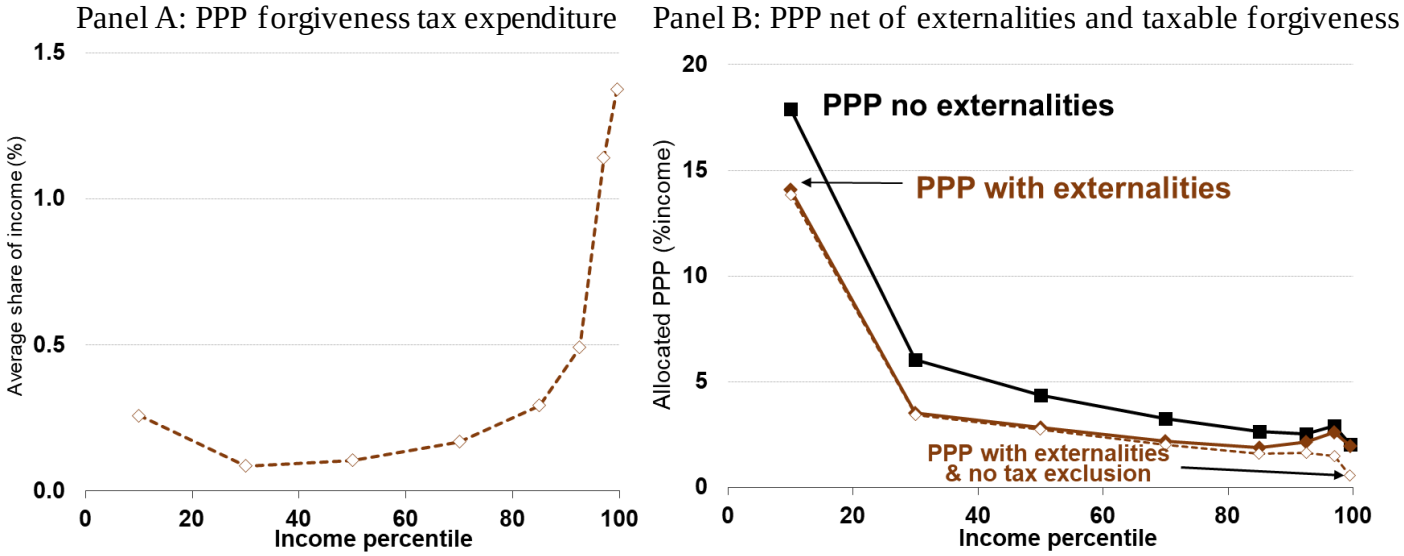
Notes: See above. *Source*: Authors' calculations using SBA and tax data.

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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