# Putting the Paycheck Protection Program into Perspective: An Analysis Using Administrative and Survey Data

Michael Dalton
U.S. Bureau of Labor Statistics*

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**Abstract**

After matching over 3 million loans from the $669 billion Paycheck Protection Program to administrative wage records, I estimate a doubly robust dynamic difference-in-difference event study showing robust, causal impacts of the loans on employment, wages, and opening status of establishments 15 months after PPP approval. Doing back-of-the-envelope calculations, I find a range of $12,000 to $19,000 of PPP spent per employee-month retained 15 months post-approval, with about 43% of the PPP money going towards wage retention in the baseline model. The smallest employers show the largest impact from PPP, which explains disparate results from prior papers that focus on larger employers. Accounting for closures properly is also key to understanding both the short- and long-term impact of PPP.

*contact: dalton.michael@bls.gov

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# 1 Introduction

The Coronavirus Aid, Relief, and Economic Security Act (CARES Act) passed in March 2020 established the Paycheck Protection Program (PPP) administered by the Small Business Administration (SBA). Initially, $349 billion was allocated to the program, followed by an additional $320 billion authorized in April 2020. This $669 billion allocated for the PPP program alone amounted to approximately 85% of the initial estimated size of the entire American Recovery and Reinvestment Act of 2009 making it a remarkable program in both size and scope. The majority of employers in the United States were eligible for a loan through the program and the explicit objective of the program was to keep employers from terminating employees by allowing them to maintain the typical wages of their employees even when their businesses were adversely affected by the pandemic.

This paper provides a thorough analysis that answers the questions of how many jobs and how much in wages were protected by the PPP, the extent that businesses were able to remain open after PPP and for how long, and which employers benefited the most from the program. Linking the full set of administrative PPP loan microdata to the administrative Quarterly Census of Employment and Wages (QCEW) allows for observing monthly employment and quarterly wages before and after PPP approval. This paper is also able to contemporaneously observe employers who have not received PPP loans because the QCEW includes all employers that pay into the Unemployment Insurance system, covering 95% of all employment in the United States. The other benefit to this linkage is that the QCEW is the sampling frame for all other Bureau of Labor Statistics employer surveys. This allows further linking of unique information for each establishment from other surveys—monthly hours worked reported in the Current Employment Statistics survey (CES) and information to confirm the quality of record linking using the 2020 Business Response Survey (BRS) that asked employers questions related to the pandemic. Combined, this offers a rich collection of data on employers to fully understand the impact that PPP had on the labor market.

A number of papers have offered preliminary insight into the question of the impact of the PPP, and Section 5.2.1 and Table 5 consider these papers in detail. One common theme in these papers has been the use of the SBA employment cutoff for PPP eligibility,[1] and this has resulted in findings of null or smaller effects of PPP on employment. Another set of results using instrumental variables find larger local average treatment effect estimates[2]. These local average treatment effects can be identical to the average treatment effect if there is no heterogeneity in the effect of PPP across timing of PPP approval or employer characteristics. Furthermore, some of these papers have had to rely on geographically aggregating the data because they do not have access to both outcome data and PPP approval information for individual employers limiting the heterogeneity analysis that can be done on employer characteristics.

This paper expands on the previous research in some key ways. First, this is the first paper to rely on administrative wage records that covers more than 95% of employment in the United States. Second, this paper uses an econometric strategy based on Callaway and Sant'Anna (2020) and Sant'Anna and Zhao (2020) to estimate the effect of PPP via a doubly robust dynamic difference-in-difference routine, and only Autor et al. (2022a) have thus far made use of a similar econometric strategy. This is an improvement because it provides estimates that are consistent in the presence of heterogeneity in the effect of PPP on employers, allows for an event study style estimate of parameters, and it allows for controlling for static characteristics of the employer to make the estimation more robust to selection into PPP approval. Third, this paper is the first to combine estimates of the effect of PPP on employment, wages, and closure status simultaneously

[1] Autor et al. (2022b), Chetty et al. (2020), Hubbard and Strain (2020)

[2] Bartik et al. (2021), Faulkender et al. (2020), Doniger and Kay (2021)

to paint a complete picture of what has happened to employers over the pandemic recession and how PPP has impacted those employer outcomes. Fourth, this paper translates the PPP program into dollars spent per employee-month retained as well as dollars-of-wages retained to better understand the impact of PPP. Lastly, due to the rich information related to employer characteristics and geography contained in the BLS wage records and related surveys, this paper is able to control for details about employers that prior research has not had the opportunity to do.[3] The heterogeneity analysis across firm size groups puts the full SBA program into perspective by identifying where PPP had the biggest impact.

This paper also contributes to the flood of findings assessing the impact of the COVID-19 pandemic on the labor market. Most relevant to this paper are findings in Cajner et al. (2020), which shows small employers and low-wage workers suffered the largest employment losses early in the pandemic; Crane et al. (2021) find significant closures for small employers, though, by the latter half of 2020, not drastically different than previous years; Kurmann et al. (2022) and Dalton et al. (2020) which both find that the smallest employers had a deep dip in employment at the start of the pandemic but bounced back very quickly. This paper places these findings in the context of the large-scale PPP program to understand how some of these patterns may have been influenced by it. The results in this paper show that PPP had a larger effect for the smallest employers, particularly through reduction in both permanent and temporary closures, and these findings help explain some of the patterns found in prior research examining employment over the pandemic.

# 2 Data

## 2.1 Paycheck Protection Program Administrative Data

In December 2020, the Small Business Administration (SBA) began publishing data for all approved PPP loan applications including the loan amount, date that the loan was approved, business name, business type, address of business, reported industry, and reported number of jobs saved due to the loan. The key information used here is the date of the loan approval, the name and address of the business, and the loan amount. This analysis only considers loans approved in 2020 and future work will examine the loans approved in 2021.

## 2.2 Quarterly Census of Employment and Wages

The Quarterly Census of Employment and Wages (QCEW) is an administrative collection of monthly employment and quarterly wages for all establishments that pay into the unemployment insurance system, covering more than 95% of all employment. The data can be linked over time to follow any single establishment. Employment and wages can be tracked prior to the pandemic until December, 2021. In addition to employment levels, the QCEW also allows the tracking of closure status on a month-to-month basis. Closures are defined as an establishment having zero employment or returning an inactive code for a particular month, and the QCEW allows monthly tracking of an employer that, for example, moves from open to closed and back to open over the course of three months.

The QCEW also contains employer name and address information for every establishment, which allows for linking to the PPP. Lastly, since the QCEW is the sampling frame for all employer surveys conducted by the Bureau of Labor Statistics, responses to other surveys that contain valuable additional information

[3] Outside researchers can apply for access to the microdata here: `https://www.bls.gov/rda/home.htm`

can be also be linked to the PPP data. The QCEW is therefore well-suited for assessing the effects of PPP on private businesses.

### 2.3 Business Response Survey

The Business Response Survey (BRS) is an online survey conducted during the summer of 2020 that asked businesses a series of questions related to the pandemic. This survey collected 162,000 responses from a nationally representative sample from July through September 2020. One question in the survey asks the establishment whether it had received a loan or grant from the federal, state, or local government tied to the payroll. Since there were a number of such programs enacted at the time and the question was not specific to PPP, affirmative answers are not sufficient for denoting that PPP was received, but replying yes to the question should be a necessary condition if the establishment had received a PPP loan.[4] After linking the PPP data to the QCEW, the data was then linked to BRS survey responses to validate that a high proportion of BRS respondents who were approved for a PPP loan are also reporting having received a loan or grant from the government.

### 2.4 Current Employment Statistics survey

The Current Employment Statistics survey (CES) is a monthly survey of nearly 700,000 worksites used to construct monthly hours, wage and employment estimates. This survey is used as a robustness check against the results from the QCEW analysis and additional analysis on hours worked and pay per hour from CES add more nuance to the QCEW results.

## 3 Match Rate

Despite the PPP data including both employer names and addresses, it is still a nontrivial task to link the two datasets. The QCEW can contain multiple addresses for a particular business, and these addresses may not be the same as the one provided by the individual filing the PPP loan application with the bank. Furthermore, the QCEW has both trade and legal names of establishments, which may take a different format or be an entirely different name than what is used on the PPP loan application. For this reason, this paper employs a record-linking algorithm to identify the best match of an establishment in the QCEW to the information contained in the PPP loan application based on address, business name, and industry. Further details on the match are provided in Appendix Section A.1.

Table 1 provides match details from linking the loan data to the Fourth Quarter of 2019 QCEW. From the overall set of PPP loans approved in 2020, 65% of the PPP loans and 87% of the PPP loan money is successfully matched to the QCEW. However, a number of business types reported on the loan applications are typically out of scope for the QCEW, such as "self-employed individuals", "independent contractors", "sole proprietorship", and "non-profits" in the Religious Organization industry. The second row in the table removes these loans. From the set of the remaining 3.8 million PPP loans, 90% of the dollars distributed are matched, or 81% of the loans. There are about three-quarters of a million PPP loans that state that only one job was saved on the application, potentially suggesting this was an employee-owner business, which

[4] Approximately 5% of the sample responded that they did not know whether a loan or grant was received. For the purposes of this analysis, these respondents are treated as missing and the percentages are relative to the pool of respondents answering "yes" or "no". Additionally, it is possible some establishments were approved for a PPP loan after they completed the BRS survey.

means it may not be included in the QCEW because they would not pay taxes into the UI system. After removing these establishments, the match rate improves to 86% of PPP loans and 91% of the remaining PPP loan dollar amount.

Table 1: Match Rate of PPP to Wage Records

| Descriptor | Total Number of Loans (millions) | Total Dollar Amount ($billions) | % of Loans Matched | % of Loan $ Amount Matched |
|---|---|---|---|---|
| All Loans | 5.1 | 520 | 65.3 | 87.4 |
| After removing... | | | | |
| Out of Scope Business Types | 3.9 | 493 | 80.5 | 90.3 |
| Out of Scope + Reporting Only 1 Job Saved | 3.2 | 484 | 85.7 | 90.9 |

*Notes: Matching between PPP data and the QCEW using record linking techniques identifying similar addresses and employer names. Details in Appendix A.1.*

There are a number of reasons that a PPP loan may not match. In some cases, correct matches are removed because they do not meet the necessary threshold for a quality match by text of employer name and address; in cases where this was a mistake, this would be a standard type 2 (false negative) error. If the addresses do not match exactly between the PPP loan and the QCEW, and a business name provided in the PPP loan application is very different from either the legal or trade names in the QCEW, this will be rejected as a potential match. There may also be cases where the business that the PPP loan is intended for is simply not in the QCEW database, possibly because the establishment does not pay into the UI system. Since the unmatched PPP loans tend to skew towards smaller loan amounts and fewer reported jobs saved, this may imply that smaller establishments were more difficult to match. If smaller employers are more likely to have incomplete information on their loan application, then they may be less likely to match to the QCEW. Further, smaller employers may be less likely to pay into the UI system and therefore excluded from the QCEW.

Considering the overall high match rate in dollars disbursed, and the evidence that the unmatched loans may likely be out-of-scope for the QCEW, we move forward with the analysis and examine type 1 (false positive) errors in the matching.

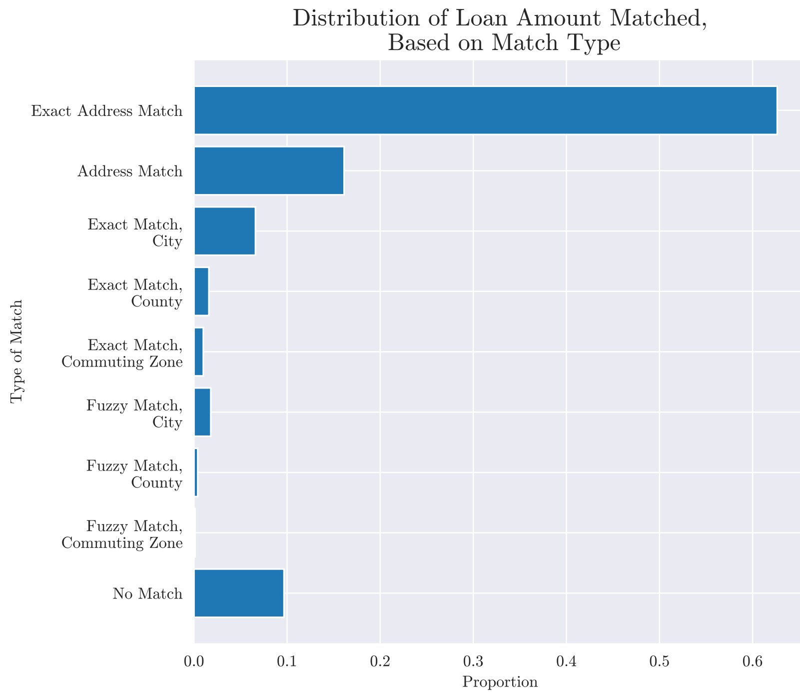
## 3.1 Match Quality in the PPP-to-QCEW Linkage

Figure 1 displays the distribution of PPP loans based on match type, weighted by loan amount, for those in-scope business types. The match type is defined by the geography level where a sufficient establishment match in the QCEW is found for the PPP loan. The second feature of the match type is whether it was an exact or fuzzy match on employer name[5]. Over 60% of the PPP loan amount falls into the highest quality match category - exact address and exact name match - and nearly 80% of the $493 billion of the in-scope PPP loan amount have at least an exact address match.

Figure 2 gives a box plot for each of the match types of the ratio of jobs reported on the PPP application to the largest monthly employment reported in the QCEW for 2019 by the establishment. The dotted light blue line represents the expected median ratio of 1. Although the expected ratio is 1, the reporting of

[5]"Fuzzy" match refers to the identification of employer names with similar text. Details are in Appendix A.1

Figure 1: Proportion of Loans Matched



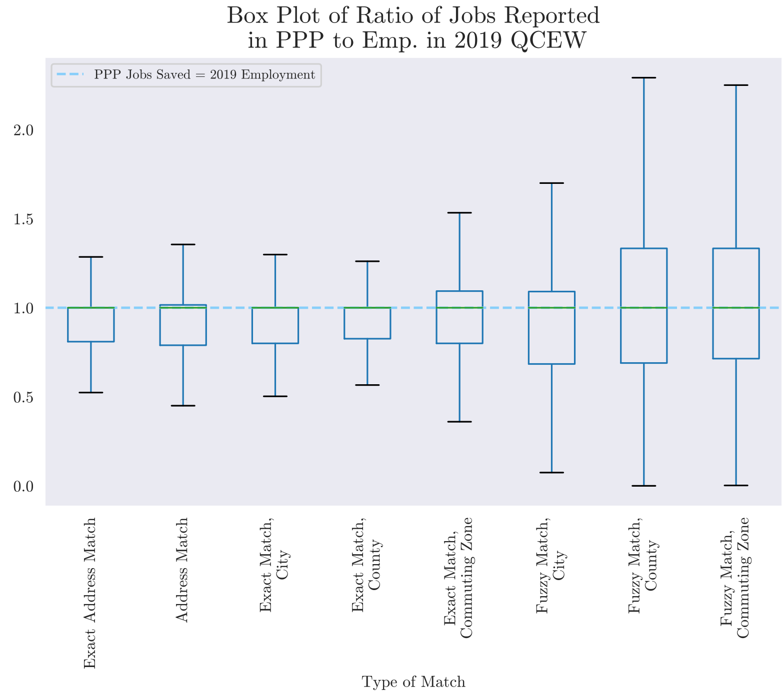

*Notes*: The record-linking algorithm matching the PPP to the QCEW moves in successive steps in the order displayed in the graph. If a sufficient employer match is found at one stage of the match type, then that PPP observation is removed.

jobs saved on the PPP application was an imperfect measure, as it was left up to the interpretation of the applicant. Additionally, the employment may have changed from 2019 to the time of application submission in 2020. For these reasons, it should be expected that there is some variance around the median ratio. It is worth noting that as the match quality decreases, going left to right, the interquartile ranges get wider, which is further evidence of somewhat lower quality matches. Though, importantly, the box plots for the first three match types are fairly similar, and make up approximately 97% of the total PPP matches.

A measure that is less prone to mismeasurement on the application is the loan amount itself. One restriction on the PPP loan amount was that it could not exceed 10 weeks of wages for establishments prior to the pandemic. This should give a ratio of approximately 0.19 for the PPP loan amount to four times the maximum 2019 reported quarterly wages reported by the establishment. Figure 4 shows the box plots for this measure. The light blue dotted line represents a ratio of 0.19, or a loan equal to 10 weeks of year's wages. Each of the match types have a median very close to the 19% mark, with interquartile ranges around 15% to 23% at every geography level. This provides further evidence that many of the matches are of high quality.

As a final test for quality of matches, Table 2 displays a test for false positives using data obtained from the Business Response Survey (BRS). Each row represents a different match type in the record-linking process, based on type of employer name match (column 1), geography where the match was identified (column 2), and the quality of text match between employer names (column 3). Column 4 reports the number of BRS respondents for each row, and the final column reports the percentage of respondents reporting in the BRS that they had received a loan or grant from the government as of the time of the survey, which was collected from end of July 2020 through September 2020. The most relevant comparison is the last column in each row relative to the bottom row - the percent of establishments not matched to a PPP loan that reported having

Figure 2: Quality of Matches




*Notes*: Dotted blue line represents equal maximum monthly employment in 2019 for the establishment and number of jobs reported on PPP application. The green line is the median and the blue box shows the interquartile range.

received a loan or grant from the government in the BRS. Overall, 95% of those respondents matched to a PPP loan report having received a loan or grant, compared to 27% for the unmatched portion of respondents. As is clear from the table, having been matched to a PPP loan correlates very strongly to a much higher percentage reporting having received a loan or grant.

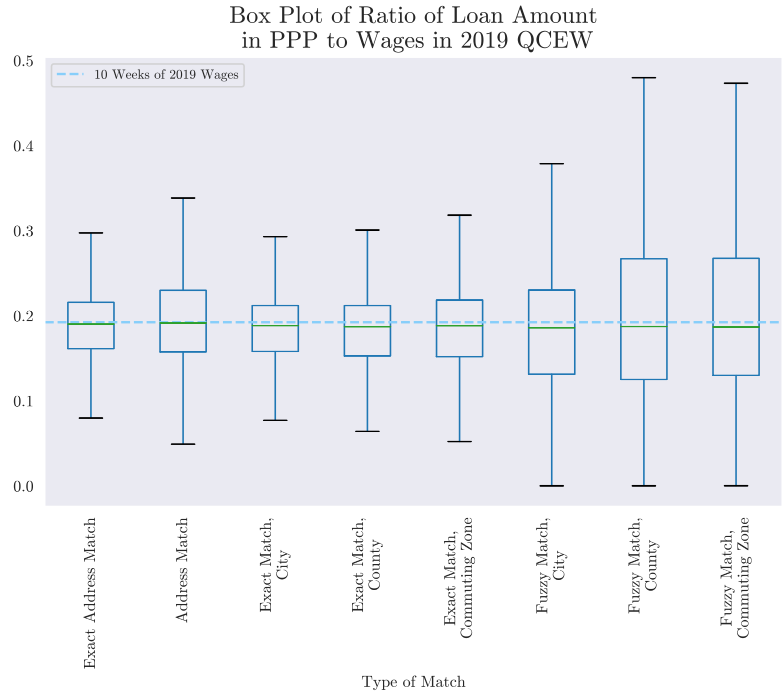
Combined, this evidence suggests that the accepted matches are of very high quality, and justifies continuing forward with identifying the effect of PPP on employment and closures.

# 4 PPP Take-up Rate

The take-up rate is the set of establishments that received PPP loans divided by the eligible set of establishments. For the denominator, the number of eligible establishments in the QCEW, where eligibility is based on industry, establishment employment, and firm-level links between establishments. Typically, the SBA defines small businesses according to industry-specific employment cutoffs, where employment levels are based on all affiliated establishments. One caveat to this definition is that firms that are officially designated as franchises have their employment determined at the establishment level.[6] Employers in the QCEW are matched to the franchise database to identify which EINs (employer identification numbers) fall under the franchise designation. This is an imperfect measure as there is no geography that allows for identifying quality matches - only employer name. This means there is both a higher likelihood of false positives, as well as a higher proportion of franchises that are not matched to the QCEW because of employer names that are

[6] `https://www.sba.gov/sba-franchise-directory`

Figure 3: Match Rate by Number of Reported Jobs




*Notes*: The dotted blue line represents 10 weeks of wages relative to a full year's wage bill for the establishment. The green line is the median and the blue box shows the interquartile range.

too different to find quality matches.

One significant change that the SBA made to the eligibility criteria for PPP is allowing all employers with NAICS code 72 (the Accommodation and Food Services sector) to be eligible if the establishment-level employment is below 500, regardless of the firm-level employment or franchise status.[7]

The last key point for eligibility determination is that EIN is an imperfect link of establishment affiliates. The SBA defines entities as affiliated "if one has the power to control the other or a third party has the power to control both" establishments.[8] For a variety of reasons, establishments that are in the same firm are not guaranteed to have the same EIN. This makes determining firm-level employment, or affiliate-summed employment, and eligibility an imperfect process. This would most likely lead to incorrectly assigning eligibility status to an establishment because the EIN employment is undercounting the true firm-level employment. This would increase the denominator and may result in undershooting the true take-up rate.

For the numerator, PPP receipt is defined similarly as eligibility: if a PPP loan is matched to an EIN that is not a franchise or NAICS 72 (the Accommodation or Food Services sector), all establishments with that same EIN are determined to have received the PPP loan. For franchisees and NAICS 72 establishments, PPP receipt is determined at the establishment level. One ad hoc imposed additional criteria: if the average loan amount per employee at the EIN-level is less than $500, then that loan is instead treated as an establishment-level loan. This somewhat arbitrary cutoff is an attempt to identify loans that may have been misidentified as applying to an entire firm when they were more likely applied for by the single establishment. 9% of those matched to an EIN-level PPP loan are instead treated as an establishment-specific PPP loan.

[7]The intention of this eligibility change was to be less restrictive on qualifications for the hardest-hit sector.

[8]`https://www.sba.gov/sites/default/files/bank_eligibility_questionnaire_0.pdf`

Table 2: Confirming Successful Data Linkage Using the Business Response Survey (BRS)

| Match Type | Geography | Fuzzy Match Score | Number of BRS Respondents | Percent Reporting Received Loan/Grant in BRS of Any Type |
|---|---|---|---|---|
| Exact Match | Address | Exact | 61085 | 95.4% |
| Fuzzy Match | Address | - | 15596 | 91.9% |
| Exact Match | City | Exact | 6573 | 96.1% |
| Fuzzy Match | City | High | 331 | 97.3% |
| Fuzzy Match | City | Medium | 471 | 91.7% |
| Fuzzy Match | City | Low | 421 | 81.5% |
| Fuzzy Match | City | Lowest | 227 | 70.0% |
| Exact Match | County | Exact | 1203 | 96.5% |
| Fuzzy Match | County | High | 82 | 91.5% |
| Fuzzy Match | County | Medium | 104 | 74.0% |
| Fuzzy Match | County | Low | 76 | 76.3% |
| Exact Match | Commuting Zone | Exact | 676 | 91.3% |
| Fuzzy Match | Commuting Zone | High | 45 | 80.0% |
| Fuzzy Match | Commuting Zone | Medium | 20 | 80.0% |
| BRS Respondents with no PPP Match | | | 60370 | 27.2% |

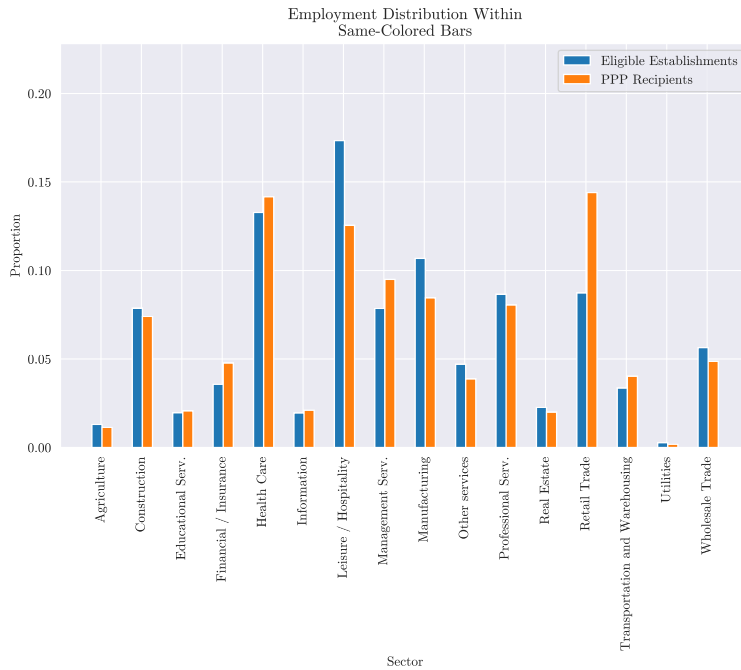
*Notes: Record linking between PPP administrative data and the QCEW using matching techniques identifying similar addresses and employer names. Business Response Survey was an online survey of employers fielded between July and September 2020. Details in Appendix.*

Overall, the take-up rate among eligible establishments is 48%, and 52% when weighted by employment. As 19% of in-scope loans are not matched, this take-up rate is an underestimate. Though, as described in the previous section, a number of these unmatched loans are likely out of scope for the QCEW. Specifically, there are about 300,000 of the unmatched loans where their business is in-scope but report having an employment of 1 on the PPP application, so many of these PPP applicants are likely not in the QCEW frame because they will not report wages to unemployment insurance offices.

Another reason why this may be an underestimate of the true take-up rate is the imperfect, under-counted measure of affiliate-summed employment, which increases the denominator by identifying ineligible establishments as eligible. However, when the sample is focused only on NAICS 72 establishments where PPP eligibility is determined by employment at the establishment level thus sidestepping the affiliate-linked issue, a 49% take-up rate is observed, only a slight increase above the overall take-up rate.

This estimate is lower than some other reports of PPP take-up, though these differences can partially be explained. Autor et al. (2022a) present a take-up rate of 94% based on total reported jobs on PPP loan applications and firm-level employment estimates from the Census Bureau's Statistics of U.S. Businesses (SUSB). This number can be re-created by taking the sum of the reported jobs on applications for in-scope PPP loans (58.2 million) and dividing by the estimated employment for enterprises with employment less than 500 from the 2019 SBUS (61.7 million). The key issue with this estimate is that it relies strictly on the firm-level eligibility requirement, where the SBA made specific carve outs for individual establishments in the restaurant industry (NAICS 72), in addition to the typical eligibility for single establishments that are

Figure 4: Take-up Rate by Sector




part of a franchise. Using the QCEW data industry codes and identifying franchises, there are 8.8 million employees in establishments with firm-wide employment greater than 500 but establishment employment less than the SBA cutoff. Including this eligible employment in the denominator gives a take-up rate of 83%.

In between the take-up rate estimates presented here and in Autor et al. (2022a) is the take-up rate from the Census Pulse Small Business Survey, which consistently found that approximately 72% of eligible establishments reported having received PPP.[9] One notable difference is that the Census Pulse survey targeted single-unit establishments with employment less than 500, and a number of industries are out-of-scope for the Census Pulse. When the QCEW sample is restricted to a comparable definition, the take-up rate is 55% (and 65% when employment-weighted). Adding to the numerator the 457,000 loans that are a) in scope, based on reported business type, b) reported jobs saved greater than 1 on the PPP application, and c) remain unmatched to the QCEW, this would increase the take-up rate to 65% (79% when employment-weighted) for the subset of establishments that meet the Census Pulse sample definition.

Even conditional on an underestimate of the true take-up rate, it is still of interest to examine how the take-up rate varies across different employer characteristics. Figure 4 displays eligibility and PPP approval by sector, where the blue bar is the overall proportion of employment among eligible establishments and the orange line is the proportion of employment among establishments receiving PPP. In cases where the orange bar exceeds the blue bar, this shows that the group has a higher take-up rate than average. Retail trade has a higher take-up rate than average, and leisure and hospitality has a lower take-up rate than average. The other sectors all manage to stay approximately near the average.

Figure 5 breaks establishments down by their average wage in 2019. The very lowest wage establishments had a slightly lower-than-average take-up rate, and the 2nd lowest wage establishments (where the average employee makes between $20,000 and $40,000 a year) had a somewhat higher-than-average take-up rate.

[9]Based on percentages from September through November 2020. `https://portal.census.gov/pulse/data/`

Figure 5: Takeup Rate by Avg. Wage at Establishment in 2019

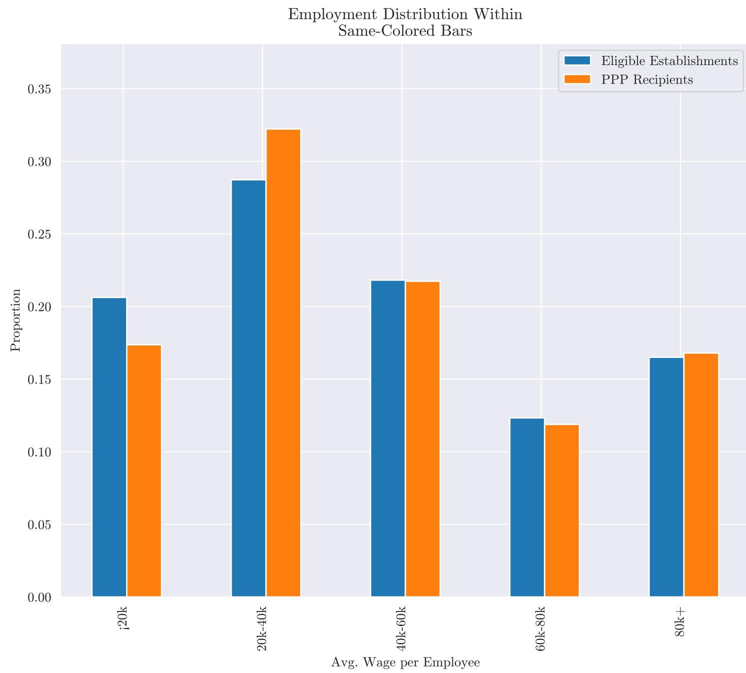



Table 6 examines take-up by establishment size, determined as the average monthly employment in 2019. The majority of eligible establishments are very small - slightly more than 50% had an average of fewer than 3 employees in 2019. However, there is also a big gap in the take-up rate, as only about one-third of PPP receiving establishments are in the smallest size class. This can be interpreted in two competing ways: either it is evidence of poorer linking between the PPP and QCEW of the smallest establishments (demonstrated in Table 1), or it may simply be that the smallest UI-paying employers had the lowest take-up rate. All other size classes show a larger-than-average take-up rate.

Table 7 shows take-up by occupation employment in establishments. This sample is conditional on also being surveyed in the 2017-2019 OEWS. Establishments employing sales occupations had the highest relative take-up rate, though food preparation occupations also had a larger-than-average take-up rate. Production and construction occupations seemed to be underrepresented in PPP take-up relative to their share of employment in eligible establishments.

Overall, take-up rates are fairly similar across a variety of employer characteristics, but the lower-than-average take-up rate among the smallest employers is notable, which may be an artifact of higher false negative matches to PPP loans among these employers. This homogeneity in take-up likely speaks to the expansive scope of the PPP, which allowed a variety of employers access to the program.

# 5 The Effect of PPP on Employer Outcomes

## 5.1 Estimation Strategy

One reason this paper is unique is because of its ability to rely on an analytical sample that represents the entire universe of UI-paying establishments through the QCEW, which covers more than 95% of employment in the United States. This is an improvement over other datasets that rely on subsamples of establishments in

Figure 6: Takeup Rate by Establishment Size in 2019

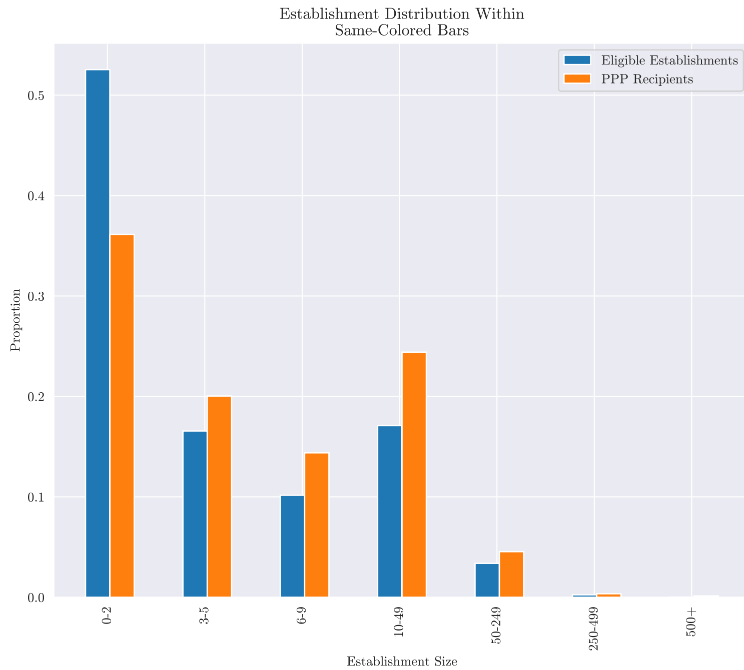
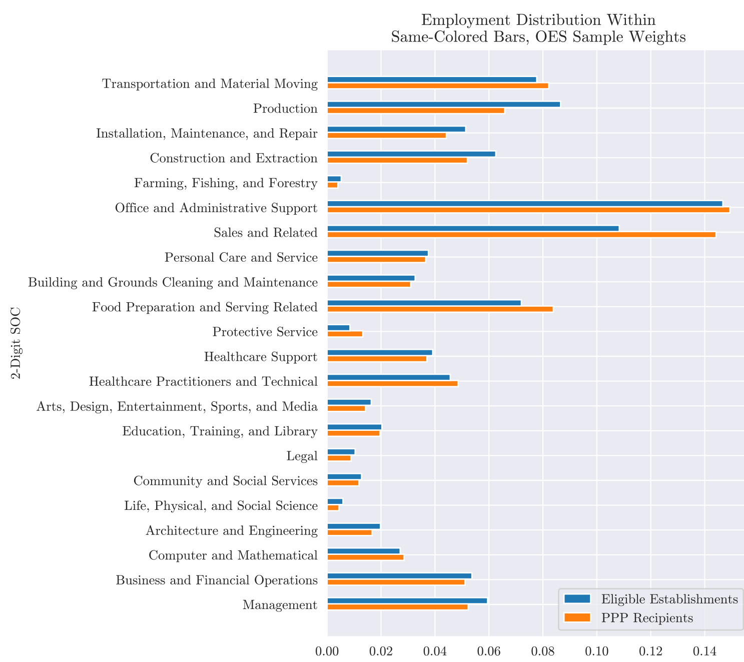



Figure 7: Takeup Rate by Occupational Employment




*Notes*: Distribution based on 2019 employment in QCEW, where occupational composition for the establishments comes from the 2017-2019 OEWS.

particular industries or size classes, or where a selection bias into those samples may confound any estimates.

Another improvement is relying on a dynamic difference-in-difference (DDID) estimation strategy based on Callaway and Sant'Anna (2020), referred to as CS from this point forward. This estimation routine has many useful features that are valuable to this context. First, it allows for estimating an average treatment on the treated effect (ATT). The treatment effect of interest is the impact of receiving a PPP loan on establishment outcomes, such as employment, the probability of closure, and wages. The ATT is the causal parameter of primary interest with regards to the impact of PPP.

Second, DDID allows for estimating dynamic treatment effects. This is important for constructing an event study analysis that follows the path of employer outcomes of an establishment from prior to the start of the pandemic through March 2021. The nature of the pandemic shifted throughout 2020, on both a local and global scale, and the PPP has the ability to influence employer behavior over time. The PPP loans could be large enough to cover 10 weeks of wages, so the effects of PPP may reasonably be observed for three months, or more. Additionally, businesses may have used the PPP loan as a supplement for maintaining wages as businesses were only partially closed. This may extend the impact of PPP to many months beyond 10 weeks. Lastly, the PPP can have longer-term impact if it explicitly allowed a business to remain open. All of these impacts should be studied in a dynamic framework, which DDID allows for.

Estimates of ATT using two-way fixed effects (TWFE) are very common in the scientific literature. Recently, researchers have identified issues with TWFE that may give misleading ATT estimates[10]. When there are heterogeneous treatment effects between the treatment groups, TWFE can give coefficient estimates that are different from the true ATT. In the PPP context, the month of PPP receipt defines discrete treatment groups.[11] Given the timing of the first round of PPP loans, there is one treatment group for each month from April 2020 through August 2020. Since the labor market was very fluid through 2020 as the pandemic spread across the country, there is reason to think receiving a PPP loan in August may have had a different impact on employment than receiving the loan in the first tranche of loans in April. Estimating separate dynamic treatment effects allows for an event study that considers ATT effects that differ based on when the loan was approved.

The following notation closely follows the one used by CS. The key benefit of this strategy is that it allows for different treatment timing, heterogeneous treatment effects, and controlling for time invariant covariates to estimate an uncontaminated ATT from the regression.

$$ATT_{p,t} = E[Y_t(1) - Y_t(0)|X, PPP_p = 1] \tag{1}$$

where the $ATT_{p,t}$ is the ATT effect of PPP on employment $Y_t$ in month $t$ for establishments receiving PPP in month $p$. There is a unique $ATT_{p,t}$ for each calendar month $t$ from February 2020 through March 2021 and for each treatment group (defined by month of PPP approval) $p$ from April 2020 through August 2020.[12]

One implication is the assumption that $ATT_{p,t} = 0$ for all $t < p - \delta$, conditional on the time invariant covariates $X$ and $\delta$ is the number of periods of anticipation of treatment. This simply states that there should be no effect of PPP on employment in months *before* the establishment anticipates receiving PPP.

[10] See Goodman-Bacon (2021).

[11] The reported monthly employment level in the QCEW is for the pay period including the 12th of the month. The establishment is determined to have received a PPP loan in month $t$ if the date of the PPP approval was before or on the 12th of the month. If it comes after the 12th, then the treatment group will be $t + 1$.

[12] To avoid collinearity, January 2020 is the reference group in the estimates.

$Y_t(1)$ is the outcome when PPP is received, compared to $Y_t(0)$, the counterfactual outcome when PPP is not received. In this context, contemporaneous counterfactual outcomes remain unobserved. As an alternative, assuming that the time trend for untreated establishments is comparable to what the counterfactual time trend would have been for treated establishments, then untreated establishments are a sufficient control group. Testing for $ATT_{p,t} = 0$ where $t < p - \delta$ becomes an informative test of the conditional parallel trends assumption needed for unbiased estimates of $ATT_{p,t}$ where $t \geq p$.

$$E[Y_t(0) - Y_{t-1}(0)|X, PPP_p = 1] = E[Y_t(0) - Y_{t-1}(0)|X, C = 1] \quad \forall\, t > p - \delta \tag{2}$$

Equation 2 represents the conditional parallel trends assumption, which says that conditional on a set of time invariant covariates $X$, the employment change from month $t-1$ to $t$ for establishments receiving PPP in month $p$ in the counterfactual world where they never received PPP equals the same change in employment for the control establishments who have not received PPP, $C = 1$. Note that the control group can both be establishments that never receive PPP and the group of establishments that eventually receive PPP but have not as of time $t$. In order words, establishments treated at month $p^{'} > t$. Equation 2 is the key assumption necessary for estimating unbiased ATT.[13]

The initial rush to obtain PPP by businesses led to a quick depletion of the initial funding. There was a gap of 11 days in April where no loans were approved, as the second round of funding was approved for the program. Businesses that submitted applications during the first tranche but were not approved until the second round of funding was approved, may have made payroll decisions in anticipation of receiving PPP a number of days before PPP approval. Furthermore, the dates that are used for this analysis are the date of PPP approval - which may come a number of days after application submission. In both scenarios, business behavior may change prior to actually getting approval. Lastly, because QCEW employment is reported as a pay period including the 12th of the month, it is possible for PPP approval to occur after the 12th but employment in that pay period is still directly impacted by PPP approval.[14] For these reasons, anticipation, represented by $\delta$, needs to be considered in this analysis. I assume one month of anticipation, or $\delta = 1$.

CS show that the $ATT_{p,t}$ for each period can be semi-parametrically estimated from the product of a propensity score matching weight predicting the probability an establishment is in treatment group $p$ and the difference-in-difference estimate for each period $t$ and each PPP receipt month $p$:

[13] Additional assumptions are i.i.d. data and common support among the control variables and receipt of PPP.

[14] Mapping the timing of PPP approval to reported employment in the QCEW and the CES can be tricky. The QCEW employment measure is for the pay period containing the 12th of the month - a pay period could be a week, 2 weeks (ending in the week of the 12th or the week after) or even a monthly pay period. For this reason, it is possible that the month classification of assigning establishment $i$ to treatment group $p = t + 1$ if the PPP loan was approved after the 12th of month $t$ may not reflect reported employment prior to PPP approval in month $t$. For this reason, I allow for one month of anticipation of PPP receipt. Returning to Equation 3, this sets $\delta = 1$, or one period of anticipation. This may also reflect the reality that there is a lag between when the PPP application is submitted and when it is actually approved. If the establishment has taken action with regards to employment decisions in anticipation of an approval, then there may be a treatment effect "seeping" into the period prior. For the purposes of estimation, all this does is change the comparison pre-treatment period to be $p - 2$ instead of $p - 1$ when calculating each $ATT_{p,t}$.

$$ATT_{p,t} = \mathbb{E}\left[\overbrace{\left(\frac{PPP_p}{\mathbb{E}(PPP_p)} - \frac{\frac{Prob_{p,t+\delta}(X)(1-D_{t+\delta})}{1-Prob_{p,t+\delta}(X)}}{\mathbb{E}[\frac{Prob_{p,t+\delta}(X)(1-D_{t+\delta})}{1-Prob_{p,t+\delta}(X)}]}\right)}^{\text{Inverse Probability Weight}} \overbrace{(Y_t - Y_{p-\delta-1} - c_{p,t,\delta}(X))}^{\text{Outcome Regression}}\right], \tag{3}$$

$$\text{where } c_{p,t,\delta}(X) = \mathbb{E}[Y_t - Y_{p-\delta-1}|X, D_{t+\delta} + PPP_p = 0]$$

$$Prob_{p,t+\delta}(X) = \mathbb{E}(PPP_p|X, PPP_p + (1 - D_{t+\delta}) = 1)$$

The third line of Equation 3 is the propensity score prediction for receiving PPP in month $p$, predicted on the group of establishments receiving PPP in month $p$ (or, $PPP_p = 1$) and those that either never receive PPP or receive PPP no earlier than period $t + \delta + 1$ (or, $D_{t+\delta} = 0$). This is also referred to as the "inverse probability weight" (see Abadie (2005)). The second line is the observed change from $p - \delta - 1$ to $t$ in outcome $Y_t$ for the group never receiving PPP or receiving PPP no earlier than period $t + \delta + 1$, conditional on covariates $X$. The change in outcome $Y_t$ over the same period for the establishments receiving PPP in month $p$ minus the second line, and times the inverse proportion of establishment-months observed for establishments receiving PPP in month $p$ (this is $\frac{PPP_p}{\mathbb{E}(PPP_p)}$) would be the "outcome regression" (Heckman et al. (1998)). Combining the inverse probability weighting with the outcome regression gives an estimate that is "doubly robust" (Sant'Anna and Zhao (2020) and Sun and Abraham (2020)) in the sense that only either the propensity score function needs to be properly specified *or* the outcome regression for the control observations needs to be properly specified for the $ATT_{p,t}$ estimate to be valid (Sant'Anna and Zhao (2020)).

Once each $ATT_{g,t}$ is estimated, a weighted average of each $ATT_{p,t}$ is constructed to create a set of results reflecting an event study - $ATT_e$ - identifying the effect of PPP for each period $e$ relative to the period of PPP approval $p$, providing a set of parameters for:

$$ATT_e = \sum_{e=0}^{\tilde{\mathrm{T}}} ATT_{p,p+e}\,\mathbb{E}(PPP_p|p + e \leq \mathrm{T}, C \neq 1) \tag{4}$$

where $\mathbb{E}(PPP_p|p + e \leq \mathrm{T}, C \neq 1)$ is the fraction of all PPP-approved establishments-months that were approved for a PPP loan in month $p$, and $\tilde{\mathrm{T}}$ is 15 months, the last date for which data is available for each month of PPP receipt.[15] Although balancing the analytical sample removes establishment-observations from the estimation (for instance, all months after July 2021 are dropped for establishments receiving PPP in April 2020), there is an upside: when comparing $ATT_e$ of different time periods, there is no concern of a compositional effect. For example, for the estimate $ATT_{e=19}$, the only set of establishments where there are observations 11 months post-PPP approval are those loans approved in April 2020. Thus, the sample composition used to estimate $ATT_{e=19}$ would be different than the sample composition for estimating $ATT_{e=1}$. For this reason, the panel is balanced such that $ATT_{e=15}$ is the farthest out estimate from PPP approval that will be estimated on the full sample. Lastly, likely due to reporting errors, there are a fraction of observations reporting extreme increases in employment. For this reason, .01% of the observations are removed for having employment or wage changes exceeding 100 times the baseline.[16]

[15] This would be July 2021 for PPP approval in April 2020, August 2021 for PPP approval in May 2020,...,November 2021 for PPP approval in August 2020.

[16] Cutoffs of 5 and 10 times the baseline yield nearly identical results.

The R package publicly provided by CS is used for all displayed estimates[17]. These estimates result from using the doubly robust method (which relies on the propensity score match estimates for weighting), with bootstrapped standard errors that are clustered at the establishment level. All reported confidence intervals are the 95% simultaneous confidence band, which are more robust to multiple hypothesis testing. Lastly, to reduce computational burden, a 5% random sample of all establishments with 15 months of non-missing reporting in the QCEW and employment of 2 or more for at least 1 month in 2019 make up the analytical sample.

## 5.2 Effects of PPP on Employment

$$\boldsymbol{E^*_{imyjc}} = \frac{e_{imyjc}}{\frac{\sum_{t=2017}^{2019} e_{imtjc}}{3}} - \frac{\sum_{k \neq i \in j,c} e_{kmyjc}}{\sum_{k \neq i \in j,z} e_{km2019jc}} \tag{5}$$

Table 3 presents the ATT effects along with 95% simultaneous confidence bands for the full event study going from five months prior to PPP approval to 15 months after. The dependent variable is shown in equation 5. $e_{imyjc}$ is the employment in establishment $i$, month $m$, year $y$, 4-digit NAICS $j$ and county $c$ is relative to the relative to average employment in the same calendar month from 2017-2019 for that establishment, so that the estimates can be viewed as a proportion change in employment.[18] Using the same calendar month as the baseline helps avoid issues with seasonality. Controlling for seasonality is important in this context because Accommodation and Food Services establishments, which were targeted for PPP, are more likely to be affected by seasonality. Additionally, bonuses are often paid at the end of the year, which are also important to control for when looking at wages.[19] The second term in equation 5 is the total employment in county $c$ and industry $j$ in month $m$ and year $y$ excluding the employment of establishment $i$ relative to the same calendar month in 2019. This term gives a time-varying control for what is happening in the local geography and industry. This covers evolving local COVID policies, spread of COVID, local bank access, as well as COVID-specific impacts to particular industries, such as restaurants. This dependent variable formula also applies to analysis using wages later in the paper.

The first column of Table 3 displays the DDID estimates with no control variables and the second column includes the control variables.[20] Comparing the columns without controls to the columns with controls, there is an attenuation towards zero for each month where $e \geq 5$ in the event study for almost every month. Additionally, the change in estimates after adding in the controls gets larger the further from PPP approval, potentially suggesting that long-term effects observed may be due to some selection that is captured by the time invariant controls. For this reason, the estimates with controls are the preferred specification.

Focusing on estimates with controls, within the first month of being approved for a PPP loan, there is an increase of approximately 8.8 percent in employment absent PPP loan approval. The effect slowly tapers as the months progress, though have statistically significant and positive effects through fifteen months after PPP loan approval. This is a particularly interesting result as it provides some evidence of the longer-term impact of being approved for a PPP loan. As observed in Figure 3 and according to the rules governing

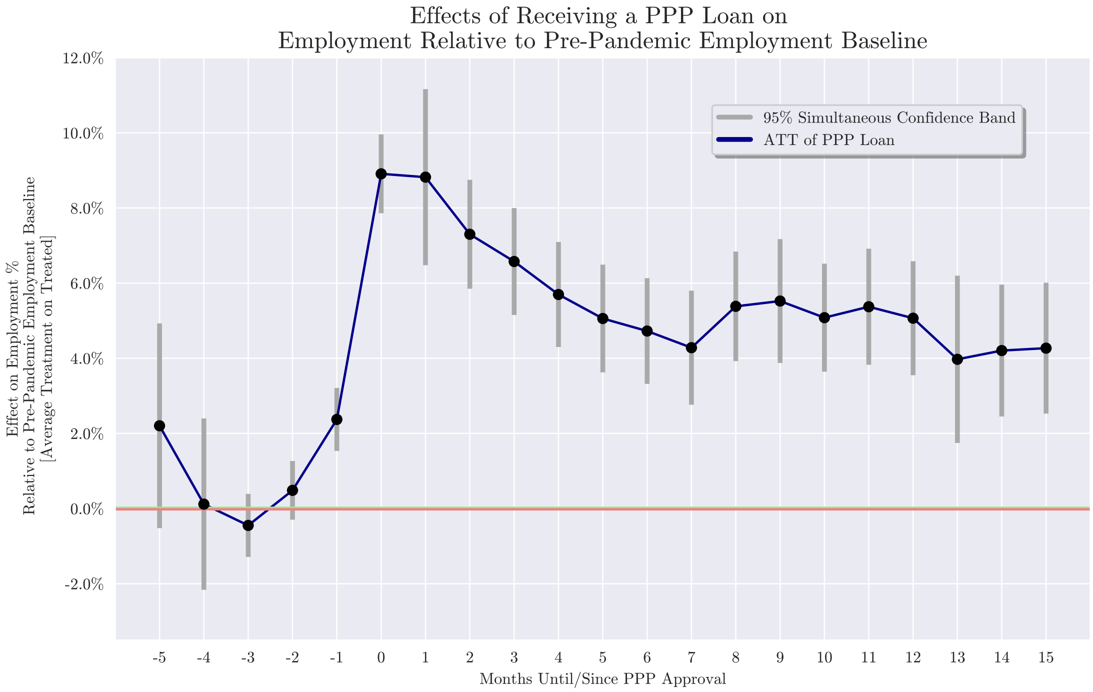
[17] `https://www.rdocumentation.org/packages/did/versions/2.1.2`

[18] Only years where the establishment exists are included in the average. For months in establishments that have an average of zero employment in prior years, the employment percentage is imputed as 100. The number of observations where this is imputed is 2%. The results are unaffected whether these observations are included or excluded.

[19] Results using average employment for the establishment across all 2019 calendar months as the baseline are slightly larger but follow the same patterns and statistical significance as the results shown here.

[20] Appendix A.2 gives detail about the control variables chosen. Since the sample is sufficiently large, the choice of control variables is not particularly parsimonious, which is another benefit of using the QCEW.

Figure 8: Average Treatment on the Treated (ATT) of PPP Loan Approval on Employment Outcomes




*Notes*: Based on a random sample of 1% of establishments in the QCEW. Visualization of Column 2 of Table 3.

maximum PPP loan amount, the loan amounts were intended to be up to 10 weeks of wages. Ten weeks of wages implies an employment effect that should completely dwindle after three months in the event study if all wages are being covered using PPP. Many businesses simply had reduced demand without fully closing, suggesting that PPP may cover a portion of wages over a longer time period than just 10 weeks. Also, the effects may be longer-lasting because one of the stipulations for converting the loan to a grant are that the employment and compensation levels pre-pandemic must be maintained for 24 weeks following PPP receipt, through the end of the calendar year of 2020. Most importantly to this study, this may also reflect the fact many businesses were able to avoid permanent closures as a result of PPP receipt. I go into more detail on this point later in the paper.

Figure 8 visualizes the ATT effects displayed in Column 2 of Table 3. The months prior to being approved for a PPP loan all have estimates close to zero and statistically indistinguishable from zero. A key test of the primary assumption of parallel trends displayed in Equation 2 is that the ATT in months prior to being approved for PPP should be zero. In all months prior to $e = 0$, the estimated ATT is indistinguishable from zero. Overall, there is no evidence of pre-treatment selection after controlling for time invariant regressors.

One concern is that there were a number of programs that were enacted in response to the pandemic that employers could apply for. Some examples are the Economic Injury Disaster Loan and Grant (EIDL) programs and an additional round of PPP in 2021. Though these programs were significantly smaller than the PPP 2020 round of loans, there was still an overlap in eligibility. Since the SBA also publishes information about recipients for these programs, a similar match is done between the QCEW and the participants in these other programs. To avoid the possibility of contamination from participating in multiple programs, Column 3 in Table 3 shows the results removing all employers that participate in one of these other programs. The first two months after PPP approval show some attenuation going from Column 2 to Column 3, however, the coefficients are more precisely measured and larger in magnitude for later months when removing the contaminated observations. Overall, both columns tell a story of statistically significant impact of the program immediately upon approval and then a sustained impact even 15 months after approval.

As an additional robustness check that offers additional nuance to the results, Table 4 shows results from an analogous analysis using monthly microdata from the CES. The CES has a panel structure that allows the use of survey responses from prior months to be the baseline outcome to compare the month $t$ outcome. Furthermore, CES contains information about hours worked that can help understand the effect of PPP on employment. The sample is restricted to respondents that give a valid response for the entire 2020 calendar year. To avoid any compositional bias in the estimates, the ATT estimates are produced only for 4 months after PPP approval - the last value of $e$ where all groups $p$ can be estimated.

For Table 4, the dependent variables are month $t$ values relative to the reported January 2020 values. Column 1 shows results very similar to Column 2 in Table 3. Each month's 95% confidence bands overlap despite being statistically different from zero, suggesting that the CES - an entirely different source of establishment employment information - corroborates the results found using the QCEW. Interestingly, the hours estimates in column 2 suggest that employers were not simply paying employees while they did not work. To the extent that PPP may have been used to keep businesses afloat while employees stayed home without working during the pandemic, this does not seem to be common enough to show in the average estimates. The effect of PPP on hours is again comparable to the effect on employment. The last column looks at the effect on the ratio of hours per employee each month relative to the reported value in January 2020. One month after PPP approval shows a statistically significant 1.3% positive effect, but that dissipates quickly int he following months. Compared to the employment estimates, this suggests that the effect of PPP seem to only hinge on the extensive margin of keeping employees on payrolls, and not the intensive margin of how many hours those workers were paid for.

One thing to note about the CES results is that they are subject to nonresponse bias in a way that the QCEW results are not. In particular, the CES is more subject to a nonresponse being the result of a closure[21]. For this reason, it may bias the results from the CES downward as some closed establishments are not included in the analytical sample.

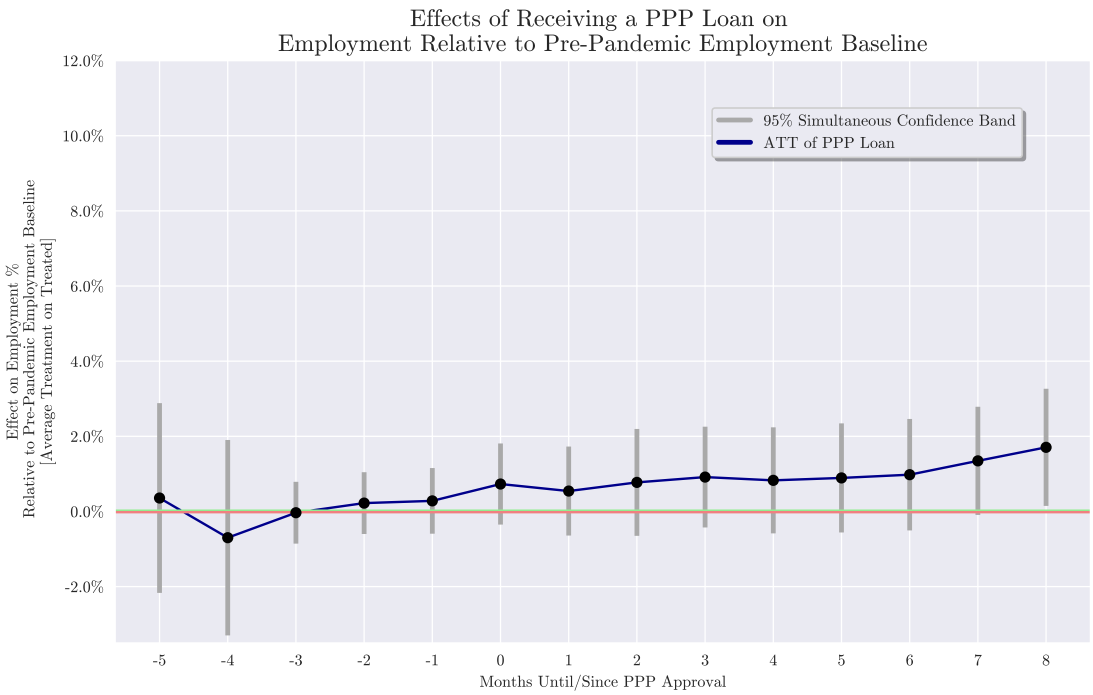
One final robustness check is to do a placebo test, doing the exact same analysis but for years prior to the pandemic. Specifically, looking at establishments in existence in 2017 and estimating the employment trajectory through March 2019 for establishments approved for PPP in 2020. The sample is restricted to establishments that also continue to exist in 2020, so as to not bias the estimates on PPP approval upward. Month of PPP approval is the same calendar month as approval the establishment received in 2020.

Figure 9 shows the same estimates for a 5% sample of establishment-months from January 2018-March 2019, where the employment baseline is the same calendar month employment average from 2015-2017, removing the county-industry employment changes over the same time period. The results show imprecisely estimated coefficients with point estimates less than 1 and indistinguishable through zero up to eight months after the calendar month that they received PPP. Based on the estimates from the placebo, any potential bias is small relative to estimates presented above, as all confidence bands exist outside of the estimates shown in Column 2 of Table 3. This is strong evidence that the baseline results are not an artifact of seasonality or some other patterns of recent growth correlated to PPP application and approval.

One concern with the results could be that there are about 457,000 in-scope PPP loans that went unmatched, many of which could just not be matched because address or name information differs from what is in the wage records data. This issue of treatment misclassification can make the estimated ATT

[21] Documented in Dalton et al. (2021).

Figure 9: Average Treatment on the Treated (ATT) of PPP Loan Approval on Employment Outcomes in Years Prior to Pandemic (Placebo Effect)




*Notes*: Based on a random sample of 5% of establishments from the year 2018. Same estimation as Table 3.

different from the true $ATT^*$:

$$ATT^* = \frac{\hat{ATT}}{P(PPP^* = 0|PPP = 0) + P(PPP^* = 1|PPP = 1) - 1}$$

$ATT^*$ is the true ATT and $\hat{ATT}$ is the estimated ATT. $P(PPP^* = 1|PPP = 1)$ is the probability a business actually participated in the PPP program if the match determines that they did. This can be calculated from the exercise shown in Table 2 where 95% of respondents in the BRS that were matched to a PPP loan reported that they had received a loan or grant. Granted, there will be noise in responses to surveys, so falling short of 100% may not represent an issue of treatment misclassification but simply reflecting respondent error. Nevertheless, .95 is used for this probability.

$P(PPP^* = 0|PPP = 0)$ is the probability a business truly did not actually participate in the PPP program if no PPP loan match is found. Taking the 457,000 in-scope, unmatched loans as an estimate for number of establishments unmatched to a PPP loan but identified as not receiving PPP, this gives a probability of .92. Thus, the true treatment effect estimated here is potentially underestimated by about 15%.

An alternative concern is the simple criticism of selection into treatment - businesses that were struggling and decided to close in response to the pandemic had no intention to apply for the PPP loan program. This would imply an overstated treatment effect. First, more than half of the PPP loans were for less than $25,000, which means there were no collateral requirements for the loan, thus these loans would likely be forgiven in the event of a business closure. Furthermore, the loans were intentionally forgivable if the establishment met certain payroll criteria. Even if the criteria could not be met, the loans had a 1% interest rate with maturity of either 2 or 5 years, making the cost of taking on the loan to be very minimal. For these reasons, it is difficult to imagine a struggling business intentionally choosing to pass on participating in the program. Lastly, there is also the opposite selection story at play - establishments most in need were going to be the ones to seek out the loans first. Bartik et al. (2020) find that those businesses with less cash-on-hand and more impacted by COVID were more likely to apply; however, they also find that having less cash-on-hand meant the business was less likely to get approval, so it is a mixed interpretation. Overall, there is not strong evidence of selection into PPP driving the results observed here.

In the next section, these estimates are put into context compared to estimates from other research projects.

### 5.2.1 Comparison to Previous Results

Table 5 shows estimates from previous research estimating the effects of PPP on employment and business outcomes. The results presented in previous research so far have been varied, both in terms of results and methodology. Nearly all of the papers have found at least some positive impact of PPP on employment, though a number of them amount to an economically small effect. Chetty et al. (2020) and Hubbard and Strain (2020), and Granja et al. (2022) find effects on employment that do not exceed 2%. Autor et al. (2022b) finds a central effect of a little more than 3%. These results are all notably smaller than the effects identified in the baseline results presented thus far.

In a more recent paper, Autor et al. (2022a) employ a similar methodology to this paper estimating cohort-specific average treatment effects but using payroll processor data. The baseline estimates between the papers are comparable: they find an overall employment effect of 6% across the size distribution, which is very similar to the overall effect estimated here of at least 5% across the first 5 months since PPP approval. More comparisons will be made to this paper in later sections.

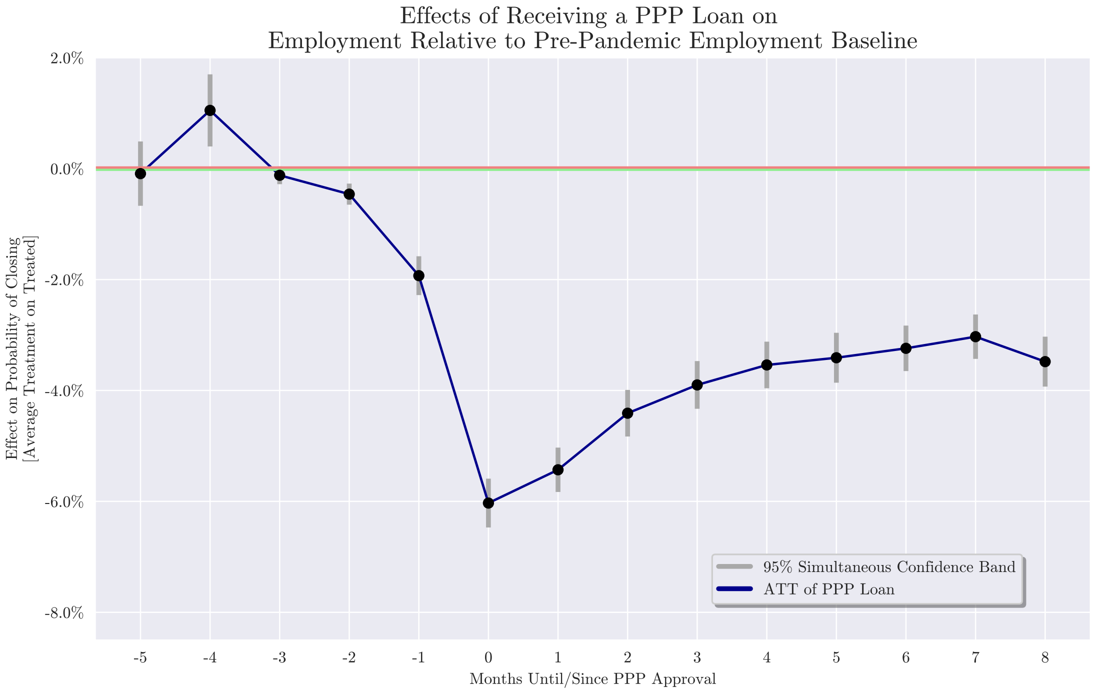
Faulkender et al. (2020) find a large 12% effect on employment of increased PPP access, though that number is not an ATT effect and therefore not an apples-to-apples comparison to the results presented. Bartik et al. (2021) also find a large increase on employment, where their focus is on the delay in funding caused in the few weeks between the first tranche of PPP in April 2020 running out due to demand and the second tranche opening up again. The effect that they estimate is limited to a specific subset of the establishments—those that were unable to access PPP loans in the first tranche because of the inability of nearby banks to deal with an influx of PPP applications.

Doniger and Kay (2021) find positive effects of PPP going through September - up to five months after the first PPP loans were disbursed, consistent with this paper's findings on the seven post-PPP approval months of effects as shown in Table 3.

Kurmann et al. (2022) take on the difficult task of managing the inherent data issues pertaining to the microdata from the work-hours processor Homebase. In particular, they address the fact that sample churn in this source of private data masks both temporary and permanent closures. For any paper assessing the impacts of PPP and using this data, this is a key issue that needs to be addressed. Kurmann et al. (2022) find reduced business closures at the county-level through the beginning of 2021 as a result of PPP.

As seen in Column 4 of Table 3, when removing businesses that close (whether temporarily or permanently) from the sample, the estimates attenuate to zero across all post-approval months, with many of the later months post-approval losing statistical significance. This makes clear the importance that closures play in understanding the impact of PPP. Figure 10 shows the estimated impact of PPP receipt on closures using a similar econometric design presented in 3. A closure in this context is when an establishment reports 0

Figure 10: Average Treatment on the Treated (ATT) of PPP Loan Approval on Business Operating Status




employment for the month or receives an inactive status in the QCEW. As the graph clearly shows, the likelihood of closure is greatly reduced for recipients of PPP and the effect persists even 15 months after PPP approval. This is strong evidence of a reduction in permanent closures as a result of the PPP program. To put these results into context, approximately 16% of establishments that existed at the start of 2020 had closed by December 2021. The PPP effect by the 15th post-approval month is a 3.5-percentage point reduction in the probability of closure. With about 48% of establishments receiving PPP, the percent of closures to be expected had PPP not been in place (and ignoring general equilibrium effects), would be about 17.7%, or a 10.7% increase in closures over what actually was observed through December 2021.

Kurmann et al. (2022) combine multiple sources of information with the Homebase data in order to identify whether an establishment left the Homebase sample due to a closure versus leaving for reasons such as no longer doing business with Homebase. Kurmann et al. (2022) find positive impacts from PPP whereas Granja et al. (2022) find essentially null effects using the same Homebase data but not correcting for the sample churn versus closures issue. As the results in this paper confirm, having quality information about closure status is necessary in order to have an accurate assessment of both the short- and longer-term impacts of PPP. The QCEW merged with the PPP data allows for a comprehensive assessment of closure status in response to PPP.

A different set of papers, Chetty et al. (2020), Autor et al. (2022b), and Hubbard and Strain (2020) all rely on the variation for the largest establishments, or largest loans (which is a proxy for establishment size), when analyzing the PPP impacts. For Chetty et al. (2020) and Autor et al. (2022b), they explicitly rely on a discontinuity design based around the employment cutoff SBA sets for determining whether an employer fits the definition of a "small business" for the purposes of PPP. The benefit of this restriction is that the eligibility criteria is set exogenously, but this necessarily means estimates are derived from the group of establishments getting PPP just below the employment threshold compared to the group of establishments immediately above the employment threshold. To the extent that the effect of PPP is the same for larger employers (those near a size of 500, for instance) as it is for small employers, then this strategy is valid. Similarly, Hubbard and Strain (2020) rely on loans greater than \$150,000, which is a small subset of all of the PPP loans more likely to have been received by larger employers as it is for small employers, then these strategies are valid.

However, if the effects vary for the smallest and largest of eligible "small" employers, then applying the results from the large employers to all eligible establishments may be a misleading interpretation of the impact of PPP on employment.

To better understand these results, the last column of Table 3 shows the $ATT_e$ estimates after reducing the sample to only employers where the employment is between .5 and 1.5 times the SBA industry-specific employment cutoff. This sample is close in spirit to the samples used for estimates in Chetty et al. (2020), Autor et al. (2022b), and Hubbard and Strain (2020). When the QCEW sample is limited in this way, the effects on employment are notably similar to the results in those papers - between a 1 and 2% effect in the first months after PPP receipt, with the magnitude tapering towards zero in later months and with large enough standard errors to not achieve any standard statistical significance. This provides an average effect smaller than the 2% estimate in Chetty et al. (2020) and the 3.3% estimate in Autor et al. (2022b) though in the same ballpark as the .9% estimate in Hubbard and Strain (2020).

Overall, properly accounting for closures and sample differences help explain the inconsistent results among a number of papers in assessing PPP. Understanding these limitations emphasizes the importance of doing a more detailed heterogeneity analysis on size.

Figure 11 shows the employment results for firm-size groups across 6 different classifications: firms with only one employee, 2 to 5, 6 to 10, 11 to 25, 25 to 100, and 100 to 500 employees. The biggest impact on employment across all 15 months is for the size group 2 to 5, though the results follow a fairly monotonic path, ending with the largest employers showing a null or even negative impact 15 months after PPP approval. The heterogeneity shines through and makes it even more clear how focusing on employers around the SBA cutoff to understand the impact of PPP will miss the bigger picture. Figure 12 presents the same groupings but examining the probability of closure. The monotonic pattern is clearer here, with some notable differences. First, the firms of size 1 also had the biggest decline in closures initially, with a 12% decline one month after PPP approval, but a steeper attenuation of this effect in the following months than other size groups. Another interesting fact is that by month 15, all of the size groups except for the largest had approximately the same reduction in closures - between 3 and 4%. So there is heterogeneity in the immediate impact on remaining open from receiving PPP, but the long-term impacts are pretty similar across the 5 smallest size classes.

## 5.3 Wages

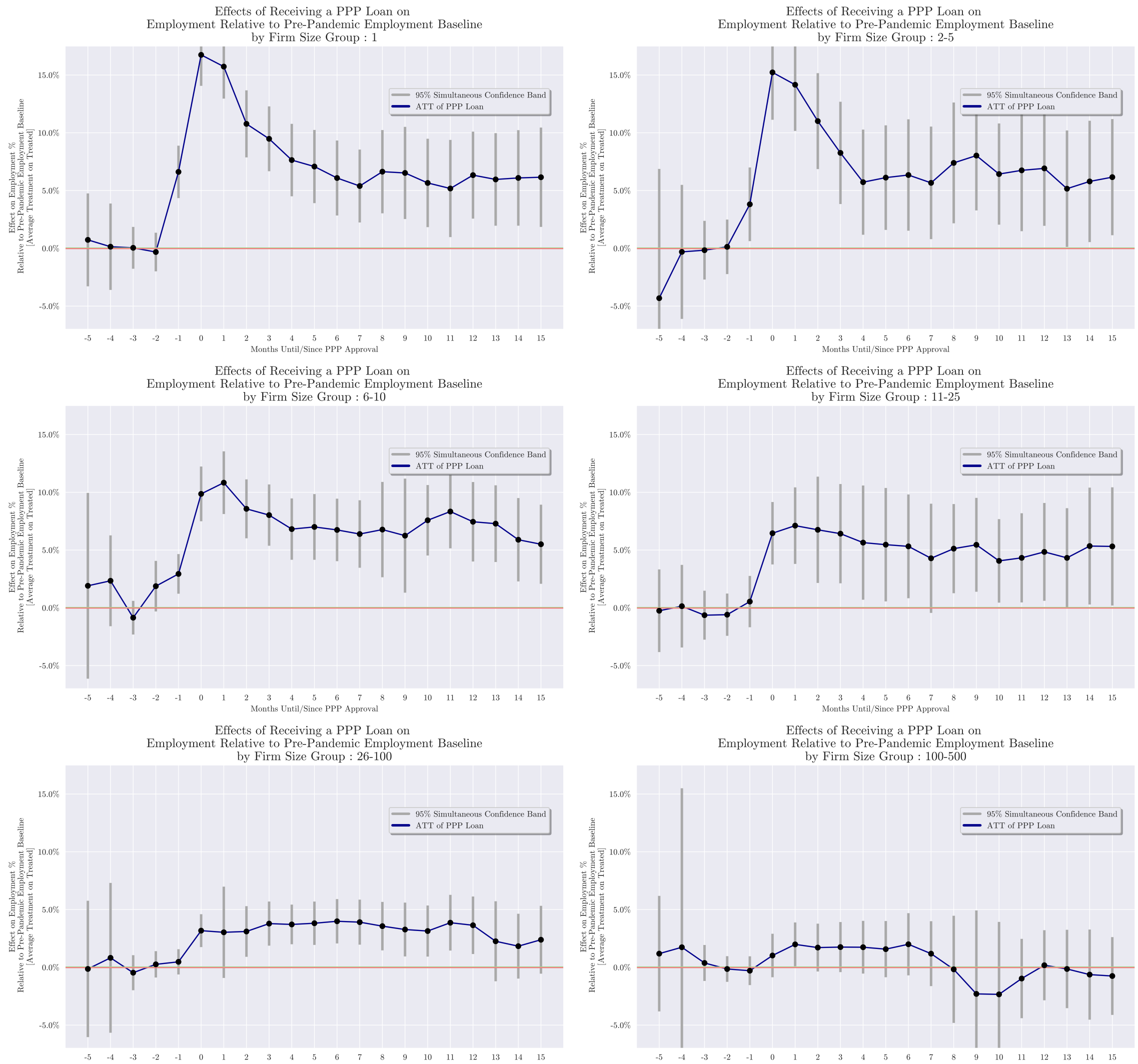
Another key insight of this paper is to make use of actual administrative wage data to assess the pass through from PPP receipt.

The following formula translates reported quarterly wages in the QCEW into monthly wages:

$$wage^y_{i,t} = wage^y_{i,q} * \frac{emp^y_{i,t}}{\sum_{t' \in q} emp^y_{i,t'}}$$

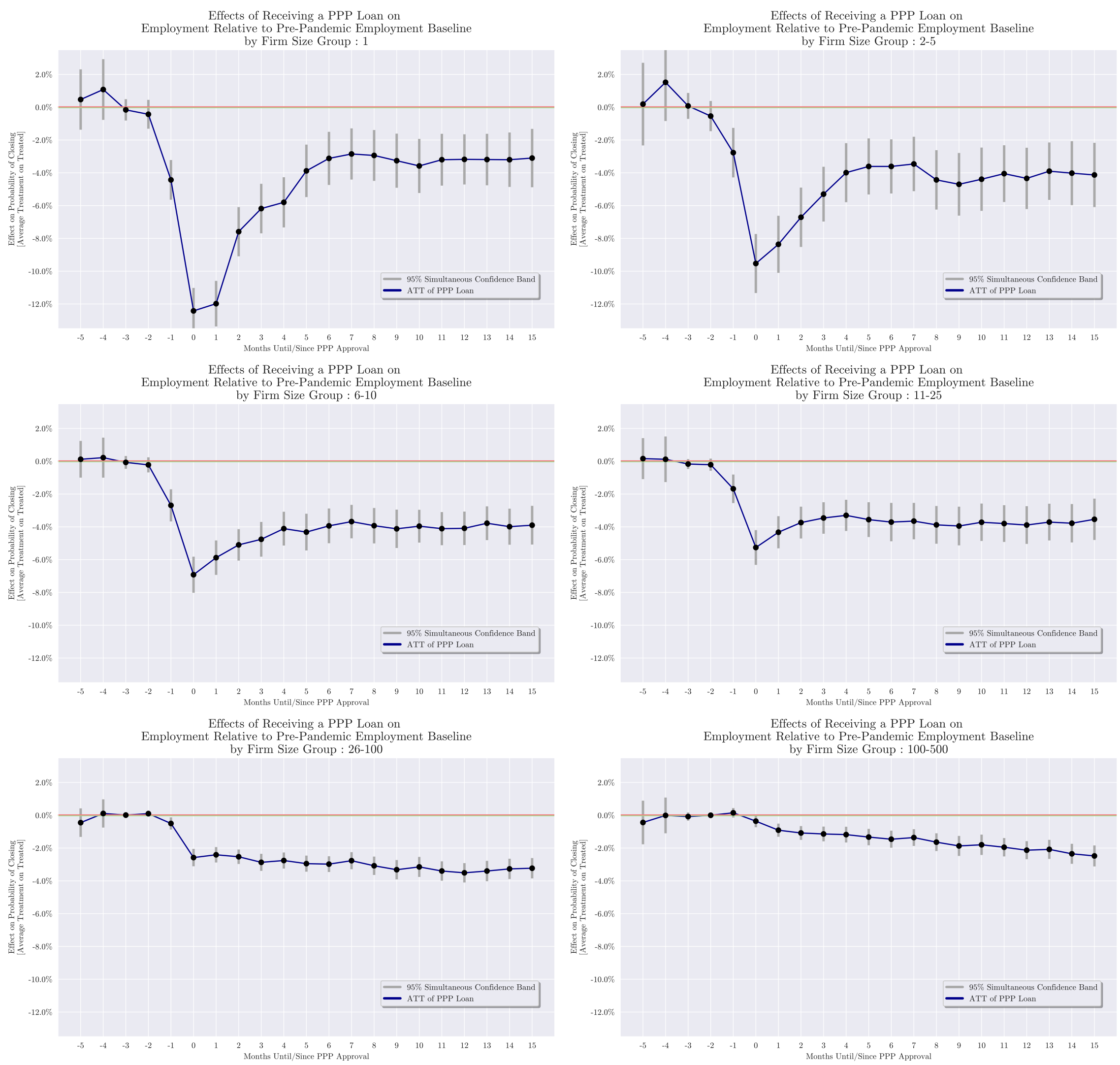
where $wage^y_{i,q}$ are the wages reported in the quarter $q$ in year $y$ for establishment $i$, proportioned to each

Figure 11: Average Treatment on the Treated (ATT) of PPP Loan Approval on Employment, by Firm Size



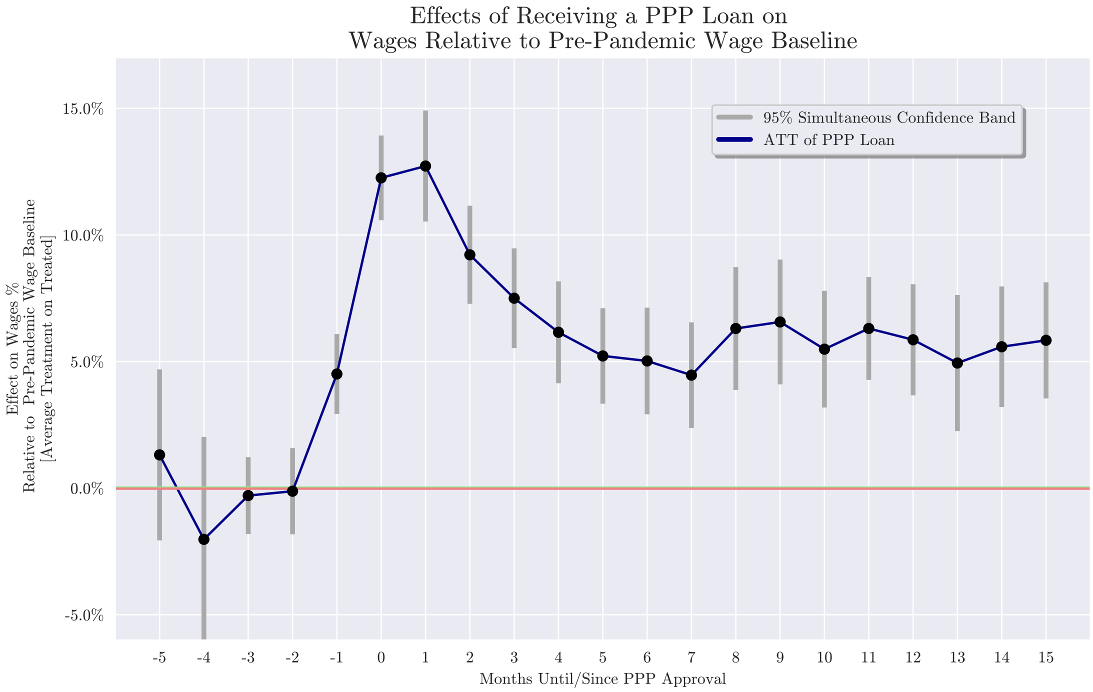

*Notes*: These are the $ATT_e$ estimates used in Table 8. They are analogous to the DDID estimates constructed for Table 3 but estimated separately for each subset of the sample.

Figure 12: Average Treatment on the Treated (ATT) of PPP Loan Approval on Wages, by Firm Size




*Notes*: These are the $ATT_e$ estimates used in Table 8. They are analogous to the DDID estimates constructed for Table 3 but estimated separately for each subset of the sample.

Figure 13: Average Treatment on the Treated (ATT) of PPP Loan Approval on Business Wages




month in that quarter, weighted by the proportion of total employment across each of the three months of that quarter in month $t$.

Figure 13 presents results examining the effect of PPP on establishment wages, following an analogous econometric strategy to equation 3.

Similar patterns observed in Figure 8, which examines the effects of PPP loans on employment, emerge for PPP loans' effects on wages. The ATT shows a statistically significant jump at PPP approval of 12% - a few percentage points higher than the employment effect. Furthermore, the effect on wages maintained a statistically significant 5.8% increase of wages 15 months after PPP approval. There are a few possible interpretations of these results.

One key caveat to this analysis on wages is that by using reported wages for the entire establishment, this does not control for compositional changes to employment. In other words, the translation from quarterly wages to monthly wages is assuming each employee-month is earning the same salary in that quarter. For instance, if an establishment temporarily reduces staffing of low-wage employees, and then the following month in the same quarter they are all hired back, the estimated wages for the final month of the quarter may be an overestimate because the below-average employees are returning, though they are treated as "average wage" employees within the establishment. The CES, which is a monthly report of wages, as opposed to the QCEW, is not subject to this specific form of error. Table 6 shows the effects of PPP on closures and wages using the CES monthly reported data on wages paid. Column 1 in Table 6 shows similar patterns to Figure 10 and also each of the post-PPP $ATT_e$ estimates have overlapping 95% confidence bands, suggesting no statistically significant difference. This helps rule out the theory that mismeasurement of monthly wages in the QCEW is driving the results.

Another potential issue is that this pattern might actually be a result of how employers receiving PPP loans avoided cutting wages for their employees. The 2020 BRS showed that 11% of employers reported decreasing wages for at least some employees in response to the pandemic, which highlights the importance of considering the PPP's effects on wages in addition to employment.[22] Column 2 of Table 6 shows the effect on pay per hour worked. All post-PPP estimates are positive, though of small size, and with only statistically significant effects within the first month after PPP approval. A 1.5% increase in pay per hour is observed for employers in the month following PPP approval, which suggests that there is modest evidence that employers receiving PPP loans actually paid higher wages for retained workers.

However, it is important to note that these findings could be a result of employers explicitly retaining or hiring back higher-wage employees and not hiring back lower-wage employees. If true, this would give the appearance of an increase in wages even though workers are simply receiving the same wage they were receiving before. There is no information contained in any employer survey about which employees were hired or retained. However, one of the stipulations for being eligible for converting the PPP loan to a grant is for **all employees** to be hired back over the 8 or 24-week period following receipt of the PPP loan.[23] There is a caveat in the forgiveness rules that states that if an employee quits or refuses to return after an offer is made by the employer, then the employee count to determine eligibility for loan forgiveness will not include that worker. If low-wage workers are more likely to turn down an offer from their employer to return to work, then this may explain the pattern of a slight increase in wages per hour.

One way to test this last hypothesis is to do a heterogeneity analysis where the employers are broken into two groups - states with high median replacement unemployment insurance rates and low median replacement unemployment insurance states. The replacement rate varies from state-to-state in normal times, and, even with the federally implemented Pandemic Unemployment Compensation (PUC), there remains some variation across states in terms of the midpoint of replacement rates due to unemployment insurance among the claimants. Ganong et al. (2020) provide estimates for the median replacement rate across states. Figure 14 shows estimates from the same baseline estimation but separately estimated for the 15 states with the highest replacement rates and then again for the 15 lowest rates. The test in this case is to see if high replacement states saw a statistically significant lower impact of PPP on employment and wages than in low replacement states. If unemployment insurance is keeping employees from accepting offers to return to their previous jobs, there should be a divergence between the ATT effect on employment compared to wages for the high replacement states versus the low replacement states. Although the point estimates for the high UI replacement rate states (the second column of graphs) are consistently smaller than the low UI replacement rate states (the first column), the confidence bands all overlap, suggesting no statistically different estimate of effects of PPP based on state-specific UI replacement rates. This result is consistent with results showing only a small impact of expanded UI on job finding rates of workers (Greig et al. (2021)).

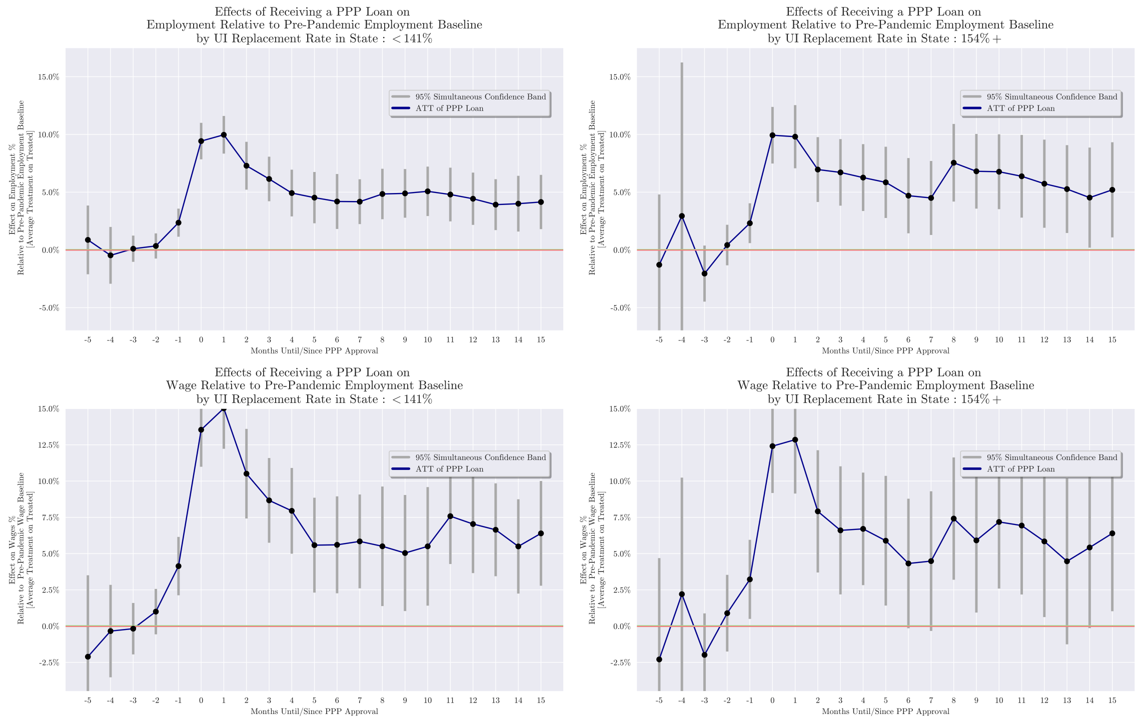
# 6 Estimating Employment and Wages Retained per PPP Dollars Spent

This analysis can also be expanded to include matched loan amounts to each establishment in order to identify how much PPP money went to each of the outcomes estimated. Table 7 provides a back-of-the-envelope calculation for the amount of PPP money spent per job saved. Columns (1) and (2) are the $ATT_e$ estimates displayed in Tables 3 and 13. Column (3) takes the number of employees at establishments that

[22] `https://www.bls.gov/brs/`, Table 6

[23] The PPP loan forgiveness terms were altered which extended loan forgiveness timeline to 24 weeks from 8 weeks.

Figure 14: Average Treatment on the Treated (ATT) of PPP Loan Approval Employment and Wages, by State UI Replacement Rates




*Notes*: Visualization of estimates and 95% confidence bands from samples restricted to UI generosity, based on reported numbers in Ganong et al. (2020).

matched to a PPP loan (44.9 million) and multiplies that number by the estimates in column (1). Column (4) does the same but for total wages of the establishments matched to a PPP loan ($189 billion) times column (2). Columns (5) and (6) are then the sum of columns 3 and 4, respectively - for a total of 40 million employee-months and $207 billion in monthly wages retained fifteen months after PPP approval. Lastly, columns 7 and 8 are the amount of PPP money in the matched sample ($491 billion) divided by columns 5 and 6, respectively. This gives an overall estimate of $11,737 of PPP loans per employee-month retained and $2.3 of PPP loans per dollar-wage retained. The $2.3 estimate can also be inverted to say how much of the PPP money went to wage retention: 43% after fifteen months. After 7 months, the estimate is about 25% of PPP money being saved in retained wages. These estimates are close to the bounds shown by Autor et al. (2022a) of 25-40% of PPP dollars went to workers' wages. One key factor in the result depicted in this paper is that because of the reduced permanent closures due to PPP, the proportion passing through to wages will continue to increase as more months are added to the analysis.

To put the 43% into a broader economic context, it is first worth noting that, in order to be eligible for forgiveness, only 60% of the loan amount needs to be spent on payroll costs; the rest can be spent on recurring business expenses. Another aspect to consider is the amount that a firm saves by not dealing with the churn associated with letting a worker go and then hiring a new worker to fill their place. Boushey and Glynn (2012) estimate that the cost to an employer of turnover is about 20% of the salary of the lost employee. This 20% is due to costs of recruitment, training, and loss of production from sub-optimal employment. To the extent that this estimate holds in the context of the pandemic, this would translate to $42 billion of the $207 billion in retained wages 15 months after PPP approval would have otherwise been a cost to the employer. Another point is the financial cost of employees losing their jobs during times of high unemployment. Davis and von Wachter (2011) find workers lose an average of $78,000 in lifetime earnings as a result of unemployment during a mass layoff, and that amount increases with high unemployment, like seen during 2020. Also, there are other benefits that are harder to put a dollar amount on - the benefit to communities from keeping businesses from permanently closing, the societal costs avoided by keeping workers from potentially experiencing long-term unemployment, and the government-cost savings from the employees not moving to unemployment insurance.

Lastly, since there is an anticipation effect estimated for establishments prior to PPP approval, observed in the $ATT_{-1}$ estimate in Figure 13, there are potentially some retained wages not included in this analysis, suggesting an even smaller number in column 8 of Table 7 if these retained wages, plus those unobserved in the data occurring after December 2021, were added to the aggregated retention effects. For that reason, the benefits per PPP loan may be even higher.

Prior research has estimated the following— Bartik et al. (2021) estimates between $32,000 and $67,000 per job retained, Doniger and Kay (2021) estimates $100,000 per job retained, Autor et al. (2022b) estimates $224,000 per job retained, and Chetty et al. (2020) estimates $377,000 per job retained. Autor et al. (2022a) use a similar methodology to estimate a cost between $170,000 and $258,000 per job-year saved, but the estimate presented here of $11,737 per employee-month saved or $141,000 per job-year saved is below their upper bound. Table 7 aggregates multiple months so that these are not specifically jobs retained but employee-months retained, which will be a larger number as it double counts jobs.

## 6.1 Heterogeneous Effects of PPP by Firm Size

Using the QCEW as the basis of analysis allows us to look at employer characteristics like firm size. The analogous results to Columns 5 through 8 in Table 7 are provided for each subset of the sample. The full set of $ATT_e$ for each grouping can be provided upon request.

One caveat with the following analysis is that the numbers reported are average effects and not marginal effects. While using the average as an approximation for marginal effects is still useful to understand heterogeneity across this variety of employer characteristics, it is useful to keep that distinction in mind when interpreting these results.

### 6.1.1 Firm Size

Another notable pattern that emerged over the pandemic is that small establishments struggled more than larger establishments through 2020 (Cajner et al. (2020), Kurmann et al. (2022), Dalton et al. (2020)). An important caution to this is that the smallest establishments (fewer than 10 employees) had a steep drop in employment in April 2020 but then bounced back rather quickly compared to the other size classes.[24] One potential explanation is that PPP was particularly effective for the smallest employers. In fact, that is exactly what we see in Table 8, which shows a low ratio of PPP dollars spent on the smaller establishments relative to the number of employee-months retained by employers. This holds more clearly in the dollar-wage retained measure, too. This offers one explanation for why the smallest size class rebounded so much quicker than the other size classifications - PPP had a drastic and immediate impact for small businesses, with a 14% increase in employment within a month of PPP approval. This is in contrast to the much smaller effects on wages for the 100+ size group. These results help better understand some of the unique patterns

[24]See Kurmann et al. (2022), Dalton et al. (2020).

observed early in the pandemic: small businesses suffered but PPP had an important impact for the smallest employers that allowed them to maintain employment and bounce back quicker. Combining the estimates gives an estimate of about $19,200 in PPP loans per employee-month retained, a higher estimate than the baseline estimate in Table 7, which is driven by the fact that 29% of the disbursed PPP loan amount went to this largest firm size, despite there being minimal positive impact for this group.

# 7 Conclusion

Estimating impacts of PPP on employers via a dynamic difference-in-difference regression using administrative and survey data from the Bureau of Labor Statistics linking microdata directly to over 3 million PPP loans, this paper is able to more thoroughly establish the positive effects of the PPP on employment, closures, and wages while reconciling the results from prior research. I show an 9% increase in employment, 12% increase in wages, and a 6% decline in the likelihood of closure within one month of PPP approval. 15 months after PPP approval, I find a remaining 4% impact on employment, a 6% effect on wages, resulting from the observed decline in probability of permanent closure of 3.4 percentage points. Doing back-of-the-envelope calculations, fifteen months after PPP approval, I find a range of $12,000 to $19,000 of PPP spent per employee-month retained, with about 43% of the PPP money going towards wage retention in the baseline model. Fifteen months after PPP approval, establishments are estimated to have retained $207 billion in wages as of December, 2021.

I am also able to separate out the effects for larger employers relative to the full sample, which explains a number of the conflicting results shown in the literature. Using the QCEW allows for analysis of the effects of PPP across the most important employer characteristic in this context, employer size. I find that the smallest establishments had the most employee-months and dollars of wages retained per PPP dollar spent.

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# A Appendix

## A.1 Record Linking

Linking the QCEW to other surveys is trivial, since the QCEW is the sample frame for BLS establishment surveys. However, there is no direct way to link with other data sources like the PPP loan microdata. However, in the absence of unique identifiers or other administrative codes, common data between the two databases can be used for matching. The PPP Data includes employer name, address, reported NAICS code.

All of the PPP information comes directly from the loan application. For a variety of reasons, there may be inconsistencies between what is input on the loan application for an establishment and what would show up in the QCEW data. For instance, the person filling out the application may use an abbreviation for the employer that is not used in the trade or legal name in the QCEW. The applicant may provide a local contact address instead of the physical location of the establishment, which may be more common during the pandemic as people were less likely to come directly into the office. For these reasons, it is not immediately straight-forward to match the PPP application data to the QCEW. For this, more advanced record-linking techniques are necessary.

All addresses in the QCEW and PPP are standardized using the Postal Service's geocoding tool that takes address inputs and returns the nearest address in the full index of current addresses. Next, the employer name provided on the PPP application and both the legal and trade names are standardized by removing punctuation and capitalization, and replacing all references to states with the appropriate state abbreviation.

Also uninformative words are removed, such as "INC" or "CORP" and city names repeated in the business name.

After these standardization rules are applied, a fuzzy text matching between QCEW and PPP business names are applied. The TF-IDF, or Term Frequency (TF) – Inverse Document Frequency (IDF), text matching method is used on business name matching between PPP and the QCEW. TF-IDF is both faster than other common methods, which is especially important in a Big Data matching space, and more flexible. To apply TF-IDF, business names were broken up into n-grams, or n-letter strings, where n=3. These n-grams are also referred to as tokens and are useful in breaking up text strings into component pieces that can be compared between records. TF-IDF calculates how common or unique each of these n-grams are. Then TF-IDF scores are computed such that term frequency (TF) is the fraction of n-grams in employer name that are in specific 3-letter string, IDF (inverse document frequency) is the natural log of the inverse fraction of employer names containing the specific string, and the final score is TF time IDF.

The TF-IDF score is for each n-gram, and thus creates a vector of scores across the universe of n-grams in the name space. Every employer name has its own vector, and can be compared against the vector of another employer name by calculating the cosine of the angle between the two vectors. This number is the cosine similarity index and allows for a simple pair-wise comparison across all potential employer names. A cosine similarity index of 1 means an exact match, with 0 being the lowest score.

To minimize Type I errors and also reduce the state space in order to make the matching process more efficient computationally, the match is done in 3 stages. The three stages follow concentric circles of geography - city, county, and then commuting zone within the same state. At each stage, the state space is only employers that are a) within the same geography and b) have not previously been matched to a PPP loan. When a sufficient match is found, both the PPP loan and the QCEW establishment are removed from the state space. As the linking proceeds to larger geographies, the minimum threshold for a sufficient match increases. One other piece is do the match for matching sector - if the PPP establishment and the QCEW establishment have the same sector, a small "bonus" match is added to the cosine similarity score to indicate a higher quality match.

## A.2 Time invariant Control Variables

The doubly robust difference-in-difference estimates allow for controlling time invariant characteristics that may predict an establishment choosing to participate in the PPP. These controls are sets of dummy variables for sector, urban classification, whether the establishment is part of a franchise, bins of age of establishment, bins for average wage of establishment, bins for EIN size, bins for establishment size, whether the establishment was determined to be eligible for PPP, whether the establishment grew or declined between 2018 and 2019, 1 dummy for each month of 2019 for if the establishment was closed, and 1 dummy each for if the establishment was matched to an Economic Injury Disaster Loan (EIDL), grant from the EIDL program, or PPP 2021 loan. Lastly, a continuous measure of 2018 to 2019 growth for the establishment is included as a control as well.

Table 3: Estimates of the Effect of PPP Approval on Employment

| Months Until / After PPP Loan Approval | Estimate | Without Controls (1) | With Controls [Primary Specification] (2) | Remove Multiple Program Participants (3) | Removing Businesses with Closures (4) | Only Employers Around Eligibility Cutoff (5) |
|---|---|---|---|---|---|---|
| | | Dependent Variable: % of Baseline Employment minus County-Industry Employment Change | | | | |
| -5 | ATT | 1.74 | 2.2 | 2.98 | 2.01 | -2.01 |
| | [95% C.I.] | [-0.8, 4.3] | [-0.5, 4.9] | [-0.6, 6.6] | [-1.5, 5.6] | [-11.3, 7.2] |
| -4 | ATT | -2.09 | 0.12 | 1.12 | 1.5 | -7.15 |
| | [95% C.I.] | [-4.4, 0.3] | [-2.2, 2.4] | [-2.3, 4.5] | [-1.0, 4.0] | [-22.6, 8.3] |
| -3 | ATT | -0.87 | -0.45 | 0.11 | -0.67 | 1.05 |
| | [95% C.I.] | [-1.6, -0.1] | [-1.3, 0.4] | [-0.8, 1.1] | [-1.6, 0.2] | [-2.7, 4.8] |
| -2 | ATT | -0.56 | 0.48 | 0.38 | 0.33 | -0.38 |
| | [95% C.I.] | [-1.2, 0.0] | [-0.3, 1.3] | [-0.4, 1.2] | [-0.6, 1.2] | [-2.6, 1.9] |
| -1 | ATT | -1.26 | 2.37 | 2.2 | 1.01 | -0.54 |
| | [95% C.I.] | [-2.0, -0.5] | [1.5, 3.2] | [1.3, 3.1] | [0.1, 1.9] | [-2.6, 1.5] |
| 0 | ATT | 5.88 | 8.91 | 6.8 | 4.1 | -0.17 |
| | [95% C.I.] | [5.0, 6.7] | [7.9, 10.0] | [5.8, 7.8] | [3.0, 5.2] | [-3.2, 2.8] |
| 1 | ATT | 8.0 | 8.82 | 7.4 | 3.58 | 0.89 |
| | [95% C.I.] | [7.1, 8.9] | [6.5, 11.2] | [6.3, 8.5] | [0.4, 6.8] | [-2.0, 3.7] |
| 2 | ATT | 6.58 | 7.3 | 6.79 | 2.77 | 2.0 |
| | [95% C.I.] | [5.6, 7.5] | [5.9, 8.7] | [5.5, 8.1] | [1.1, 4.5] | [-1.3, 5.3] |
| 3 | ATT | 6.01 | 6.58 | 6.47 | 2.61 | 0.79 |
| | [95% C.I.] | [5.0, 7.0] | [5.2, 8.0] | [5.3, 7.6] | [1.1, 4.1] | [-2.6, 4.1] |
| 4 | ATT | 5.66 | 5.7 | 6.05 | 2.28 | 0.69 |
| | [95% C.I.] | [4.7, 6.7] | [4.3, 7.1] | [4.8, 7.3] | [0.8, 3.7] | [-2.7, 4.1] |
| 5 | ATT | 5.29 | 5.06 | 5.73 | 1.79 | 0.48 |
| | [95% C.I.] | [4.3, 6.3] | [3.6, 6.5] | [4.5, 7.0] | [0.3, 3.3] | [-3.0, 3.9] |
| 6 | ATT | 5.13 | 4.72 | 5.52 | 1.71 | 0.75 |
| | [95% C.I.] | [4.1, 6.2] | [3.3, 6.1] | [4.2, 6.8] | [0.1, 3.3] | [-2.8, 4.3] |
| 7 | ATT | 4.73 | 4.28 | 5.36 | 1.62 | -0.63 |
| | [95% C.I.] | [3.7, 5.7] | [2.8, 5.8] | [4.0, 6.7] | [0.0, 3.2] | [-6.3, 5.1] |
| 8 | ATT | 6.09 | 5.38 | 6.3 | 1.94 | -0.3 |
| | [95% C.I.] | [4.9, 7.2] | [3.9, 6.8] | [4.9, 7.7] | [0.3, 3.6] | [-5.8, 5.2] |
| 9 | ATT | 6.74 | 5.52 | 6.18 | 1.57 | 0.32 |
| | [95% C.I.] | [5.7, 7.8] | [3.9, 7.2] | [4.7, 7.7] | [-0.2, 3.3] | [-3.7, 4.3] |
| 10 | ATT | 6.76 | 5.08 | 5.53 | 1.34 | 0.37 |
| | [95% C.I.] | [5.8, 7.7] | [3.6, 6.5] | [4.2, 6.9] | [0.1, 2.6] | [-3.3, 4.0] |
| 11 | ATT | 7.49 | 5.37 | 5.92 | 1.93 | 1.37 |
| | [95% C.I.] | [6.4, 8.6] | [3.8, 6.9] | [4.3, 7.5] | [0.3, 3.6] | [-2.8, 5.6] |
| 12 | ATT | 6.97 | 5.07 | 5.55 | 1.58 | 1.02 |
| | [95% C.I.] | [5.8, 8.2] | [3.5, 6.6] | [4.0, 7.1] | [-0.1, 3.3] | [-3.2, 5.2] |
| 13 | ATT | 6.52 | 3.97 | 5.07 | 0.5 | 0.86 |
| | [95% C.I.] | [5.3, 7.7] | [1.7, 6.2] | [3.5, 6.6] | [-2.0, 3.0] | [-3.4, 5.2] |
| 14 | ATT | 6.7 | 4.21 | 5.35 | 0.6 | 0.43 |
| | [95% C.I.] | [5.4, 8.0] | [2.5, 6.0] | [3.7, 7.0] | [-1.4, 2.6] | [-3.7, 4.6] |
| 15 | ATT | 6.28 | 4.27 | 5.29 | 0.93 | -0.22 |
| | [95% C.I.] | [5.1, 7.5] | [2.5, 6.0] | [3.6, 7.0] | [-1.0, 2.9] | [-4.5, 4.0] |

*Notes: These are estimates from a dynamic difference-in-difference semi-parametric estimation, based on Callaway and Sant'anna (2020). Results displayed aggregate group-time average treatment on the treated (ATT) effects, showing simultaneous 95% confidence bands via bootstrapping over 1000 iterations. The standard errors are clustered at the establishment level. Controls included are described in Appendix A.2. The unit of observation is an establishment-month in the QCEW. This was estimated on 7,250,727 observations, or a 5% random sample of private establishment-months in the QCEW with employment greater than 1 in 2019. The pre-pandemic employment baseline is the average employment for that establishment in the same calendar month from the years 2017-2019.*

Table 4: Estimates of the Effect of PPP Approval on Employment in the CES

| Months Until / After PPP Loan Approval | Estimate | Dependent Variable: % of January 2020 Employment (1) | % of January 2020 Hours (2) | % of Hours per Employee Relative to January 2020 (3) |
|---|---|---|---|---|
| -5 | ATT | -2.7 | -2.43 | -1.1 |
| | [95% C.I.] | [-7.3, 1.9] | [-13.2, 8.3] | [-7.9, 5.7] |
| -4 | ATT | -3.21 | -5.54 | -0.52 |
| | [95% C.I.] | [-6.8, 0.4] | [-13.0, 1.9] | [-4.6, 3.6] |
| -3 | ATT | -2.14 | -1.06 | 0.35 |
| | [95% C.I.] | [-4.8, 0.5] | [-6.4, 4.3] | [-2.2, 2.9] |
| -2 | ATT | 0.73 | 0.17 | 0.04 |
| | [95% C.I.] | [-0.2, 1.7] | [-1.2, 1.6] | [-0.9, 1.0] |
| -1 | ATT | 0.84 | 1.41 | 0.11 |
| | [95% C.I.] | [-0.3, 2.0] | [-0.3, 3.1] | [-0.9, 1.1] |
| 0 | ATT | 5.87 | 6.98 | 0.88 |
| | [95% C.I.] | [4.7, 7.1] | [5.1, 8.9] | [-0.1, 1.9] |
| 1 | ATT | 8.11 | 9.85 | 1.31 |
| | [95% C.I.] | [6.7, 9.5] | [7.6, 12.1] | [0.1, 2.5] |
| 2 | ATT | 6.34 | 6.97 | 0.19 |
| | [95% C.I.] | [4.9, 7.8] | [4.6, 9.3] | [-1.5, 1.9] |
| 3 | ATT | 5.2 | 5.55 | -0.24 |
| | [95% C.I.] | [3.7, 6.7] | [3.2, 7.9] | [-1.7, 1.2] |
| 4 | ATT | 4.52 | 5.14 | 0.41 |
| | [95% C.I.] | [3.1, 5.9] | [2.7, 7.6] | [-1.0, 1.8] |

*Notes: These are estimates from a dynamic difference-in-difference regression, based on Callaway and Sant'anna (2020). Results displayed are aggregate group-time average treatment on the treated (ATT) effects, showing simultaneous 95% confidence bands via bootstrapping over 1000 iterations. The standard errors are clustered at the establishment level. Controls included are described in Appendix A.2. The unit of observation is an establishment month in the Current Employment Statistics survey (CES). The dependent variable is the establishment's reported value in the month relative to their report in January 2020.*

Table 5: Estimates of the Effect of PPP on Employment from Prior Research

| Paper | Effect of PPP on Employment | Identification Strategy | Limitations |
|---|---|---|---|
| Chetty et al. (2020) | 2% | Diff-in-Diff around cutoff | Focuses only on employers with 100+ employees; Specific Industries |
| Autor et al. (2022b) | 3.25% | Diff-in-Diff around cutoff | Focuses only on employers with 100+ employees |
| Autor et al. (2022a) | 6% | Robust Diff-in-Diff | No wage analysis and Limited heterogeneity analysis with ADP data |
| Hubbard and Strain (2020) | .9% | Diff-in-Diff | Only identifies establishments with PPP loan of $150k+ |
| Granja et al. (2022) | Null effect on county-wide closures | Instrumental Variables | Aggregated local effects; Relies on Homebase |
| Bartik et al. (2021) | 16-35% | Instrumental Variables | Uses first tranche of loans from April; small sample size so harder to identify heterogeneity |
| Doniger and Kay (2021) | + Effects on Employment through September | Instrument using delay in first tranche | Aggregated to location; no heterogeneity analysis besides size |
| Faulkender et al. (2020) | 12% | Instrumental Variables | Not an ATT effect |
| Bartlett and Morse (2020) | 4.7% increase in survival for size 3 | Control for application success | One metropolitan area; small sample; not using observed closures |
| Kurmann et al. (2022) | Reduction in closings into 2021 | Local measure of delayed PPP | Aggregated to the geography |

Table 6: Estimates of the Effect of PPP Approval on Business Operating Status and Wages

| Months Until / After PPP Loan Approval | Estimate | Dependent Variable: % of January 2020 Pay (1) | Dependent Variable: % of Pay per Hour Relative to January 2020 (2) |
|---|---|---|---|
| -5 | ATT | -5.57 | 0.76 |
| | [95% C.I.] | [-15.8, 4.7] | [-3.0, 4.5] |
| -4 | ATT | -5.4 | -0.89 |
| | [95% C.I.] | [-12.5, 1.7] | [-4.4, 2.6] |
| -3 | ATT | -1.14 | 1.01 |
| | [95% C.I.] | [-6.9, 4.7] | [-1.0, 3.0] |
| -2 | ATT | 0.03 | -0.64 |
| | [95% C.I.] | [-1.5, 1.5] | [-1.4, 0.1] |
| -1 | ATT | 1.28 | 0.25 |
| | [95% C.I.] | [-0.8, 3.4] | [-0.8, 1.3] |
| 0 | ATT | 7.87 | 1.35 |
| | [95% C.I.] | [5.7, 10.0] | [0.4, 2.3] |
| 1 | ATT | 11.47 | 1.48 |
| | [95% C.I.] | [8.9, 14.1] | [0.5, 2.5] |
| 2 | ATT | 7.98 | 0.96 |
| | [95% C.I.] | [5.5, 10.5] | [-0.2, 2.1] |
| 3 | ATT | 6.02 | 0.41 |
| | [95% C.I.] | [3.5, 8.6] | [-0.7, 1.5] |
| 4 | ATT | 5.55 | 0.8 |
| | [95% C.I.] | [2.9, 8.2] | [-0.1, 1.7] |

*Notes: These are estimates from a dynamic difference-in-difference regression, based on Callaway and Sant'anna (2020). Results displayed are aggregate group-time average treatment on the treated (ATT) effects, showing simultaneous 95% confidence bands via bootstrapping over 1000 iterations. The standard errors are clustered at the establishment level. Controls included are described in Appendix A.2. The unit of observation is an establishment-month in the Current Employment Statistics survey (CES). The dependent variable is the establishment's reported value in the month relative to their report in January 2020.*

Table 7: Value of PPP Loans per Employee Retained

| $e$ Months after PPP Approval | $ATT_e$ Employment % | $ATT_e$ Wages % | Retained due to PPP: Employee Months | Retained due to PPP: Monthly Wages ($) |
|---|---|---|---|---|
| | From Column (2) in Table 3 | From Figure 13 | (3) | (4) |
| $ATT_0$ | 8.91 | 12.26 | 3,999,792 | 23,188,544,212 |
| $ATT_1$ | 8.82 | 12.72 | 3,959,836 | 24,069,531,408 |
| $ATT_2$ | 7.3 | 9.22 | 3,277,438 | 17,436,961,782 |
| $ATT_3$ | 6.58 | 7.5 | 2,952,087 | 14,192,862,668 |
| $ATT_4$ | 5.7 | 6.16 | 2,557,868 | 11,646,968,613 |
| $ATT_5$ | 5.06 | 5.22 | 2,270,902 | 9,880,453,049 |
| $ATT_6$ | 4.72 | 5.02 | 2,121,224 | 9,503,914,278 |
| $ATT_7$ | 4.28 | 4.46 | 1,922,161 | 8,443,740,039 |
| $ATT_8$ | 5.38 | 6.3 | 2,416,271 | 11,929,467,299 |
| $ATT_9$ | 5.52 | 6.56 | 2,479,527 | 12,418,021,624 |
| $ATT_{10}$ | 5.08 | 5.49 | 2,281,003 | 10,383,766,181 |
| $ATT_{11}$ | 5.37 | 6.31 | 2,411,647 | 11,932,305,531 |
| $ATT_{12}$ | 5.07 | 5.86 | 2,274,494 | 11,087,458,488 |
| $ATT_{13}$ | 3.97 | 4.94 | 1,783,437 | 9,348,757,598 |
| $ATT_{14}$ | 4.21 | 5.58 | 1,887,951 | 10,565,602,241 |
| $ATT_{15}$ | 4.27 | 5.84 | 1,916,549 | 11,046,398,733 |

| | Total Retained due to PPP: Employee Months | Total Retained due to PPP: Monthly Wages ($) | $ of PPP Loans per...: Employee-Month Retained | $ of PPP Loans per...: Dollar-Wage Retained |
|---|---|---|---|---|
| | (5) | (6) | (7) | (8) |
| | 40,512,187 | 207,074,753,744 | $11,737 | $2.3 |

*Notes: $ATT_e$ come from Table 3 and Figure 13. The employee months saved is the product of the number of employees as of 2019 in establishments that received PPP approval (47.6 million) times the $ATT_e$ for employment %. The monthly wages retained is the product of the total monthly wages in 2019 at establishments that received PPP approval ($196 billion) times the the $ATT_e$ for wages %.*

Table 8: Value of PPP Loans per Employee Retained

| Firm Size Class Employment | $ of PPP Loans per... Employee-Month Retained (1) | Dollar-Wage Retained (2) | Percent of PPP Going to Group (3) | Percent of Employment in Group Receiving PPP (4) |
|---|---|---|---|---|
| 1 | $18,790 | $1.61 | 1.89% | 25% |
| 2-5 | $10,360 | $1.83 | 8.7% | 51% |
| 6-10 | $9,344 | $1.62 | 10% | 66% |
| 11-25 | $12,737 | $2.86 | 19% | 71% |
| 26-100 | $20,856 | $4.12 | 30% | 65% |
| 100-500 | $164,790 | $13.49 | 29% | 14% |

| $ of PPP Loans per... Employee-Month Retained | Dollar-Wage Retained |
|---|---|
| (5) | (6) |
| $19,186 | $3.49 |

*Notes: $ATT_e$ come from estimates analogous to Tables 3 but on subsets of the sample based on firm size. The employee months saved is the product of the number of employees as of 2019 in establishments that received PPP approval for each group times the $ATT_e$ for employment %. The monthly wages retained is the product of the total monthly wages in 2019 at establishments that received PPP approval for each group times the the $ATT_e$ for wages %. This analysis is based on a 10% random sample of private establishments in the QCEW with positive employment in 2019.*