The Impact of the Paycheck Protection Program on (Really) Small Businesses

May 27, 2022

Abstract

This paper uses administrative data from a private payroll processor whose clients are primarily very small businesses (median 5 employees) to measure the effects of financial relief received through the Paycheck Protection Program (PPP). Firms that applied for PPP funds increased their average employment by 7.5% in the five months following the program's launch relative to otherwise similar firms that did not apply. The positive effects on employment occur primarily in industries in which firms were less affected by government shut-downs or higher levels of COVID-19, namely industries with more employees that are able to work remotely, those that have fewer hourly workers and essential businesses. Novel data on hiring also shows that the program worked as intended by preserving employment matches: positive employment effects occurred due to fewer layoffs, not through more hiring of new or former employees. My estimates imply a cost of approximately $270,000 per job per year at small firms.

The global economic crisis associated with the COVID-19 pandemic has put unprecedented strain on small businesses. Following the arrival of the virus in the United States and the declaration of a state of emergency on March, 13 2020, many small businesses experienced large declines in demand due to social distancing and the government mandated shut-downs that followed. In response, the federal government swiftly enacted the Coronavirus Aid, Relief and Economic Security (CARES) Act on March 27, 2020, a main component of which was the Paycheck Protection Program (PPP) designed to provide relief to small businesses. The PPP offered loans to small businesses, typically those with less than 500 employees, to cover up to eight weeks of payroll costs. The program was specifically designed to encourage small businesses to keep employees on their payroll in the face of the pandemic (SBA (2021a)). The loans are eligible to be forgiven if firms maintain most of their previous employment and wage payments. As of the end of the program on May 21, 2021, $793 billion in loans had been dispersed to nearly twelve million firms.[1] Did this spending achieve its goals of maintaining employment and wages of small businesses? Does it provide a reasonable model for future policy responses to the ongoing pandemic?

I use monthly administrative data from a private payroll processor in the American southwest to document several findings on both PPP take-up and efficacy for small and very small firms.[2] First, I show that take-up amongst small businesses is consistent with previous findings in the literature, which looked primarily at larger small businesses. Take-up is strongly associated with four factors: size, industry, average wages, and banking relationships. Next, I show that the PPP had a large and positive effect on employment amongst these small firms, and that the effect was much larger for firms that are in industries with a lower number of hourly workers, a high number of workers with remote working capabilities, and essential businesses. My estimates imply a much larger effect

[1] In comparison, the American Recovery and Reinvestment Act of 2009 and the Troubled Asset Relief Program (TARP) disbursed roughly $800 billion and $700 billion, respectively, across different areas of the economy.

[2] Previous work on the PPP has primarily compared outcomes for firms on either side of the 500 employee cutoff for program eligibility. In contrast, the median firm in my data set has five employees. Thus, rather than using variation in eligibility, I worked with the payroll processor to collect data on whether or not each firm applied for PPP.

on employment for small businesses than found by most previous studies on the PPP. My results are identified by using administrative data on PPP application status to compare firms that did apply for PPP versus those that did not, but are otherwise similar on observables. The identifying assumption is that PPP take-up is primarily driven by as-good-as random factors related to banking frictions and the complex roll-out of the program (Granja et al. (Forthcoming), Li and Strahan (2021), Joaquim and Netto (2021))

Before estimating the effects of the PPP, I validate previous findings on which firms are most likely to apply for relief through the PPP. I find four characteristics to be the strongest predictors of take-up. First, larger firms are more likely to take-up the PPP loans. My estimates imply that a firm that is one log-point larger has 1.8 times larger odds of applying, holding the other factors constant. In the context of my sample, this means that, all things equal, a firm at the 75th percentile of employment (12 employees) is 13% more likely to apply than a firm at the 25th percentile (3 employees).[3] Second, industry is a significant factor in determining take-up. Notably, the most affected industries were not necessarily the most likely to apply. For example, in my sample, Trade, Information, Professional Services and Healthcare were amongst the most likely applicants.[4] Third, firms that have higher average wages per worker are also more likely to apply. A firm at the 75th percentile of the average wage distribution is about 5.5% more likely to apply than a firm at the 25th percentile. Finally, firms that use a bank that does more PPP lending in their state are more likely to apply.[5]

In the next part of the paper, I measure the effect of the PPP on firm outcomes using a dynamic difference-in-differences event study (similar to Autor et al. (2022), Chetty et al. (2020), and Hubbard and Strain (2020)). My identification strategy relies on making comparisons among firms that are similar on observable characteristics and assuming that

[3]Previous evidence (Barrios et al. (2020), Bartik et al. (2020a), Balyuk et al. (2020)) has shown that large firms found it easier to access the loans, consistent with my findings.

[4]This is consistent with Papanikolaou and Schmidt (2021) and Granja et al. (Forthcoming), which found the most affected industries and areas received a disproportionately small number of loans.

[5]Granja et al. (Forthcoming) also shows that the bank at which the loan was processed is important for both the take-up decision and how quickly firms received funds. Joaquim and Netto (2021) theoretically show that banks are incentivized to allocate loans to firms that are less impacted by the pandemic. See also Li and Strahan (2021), Balyuk et al. (2020), Erel and Liebersohn (Forthcoming), Ben-David et al. (2021)

the remaining variation in PPP application is driven by as-good-as-random differences that are uncorrelated with the firms' outcomes. The observable factors I control for include indicator variables for firm size, NAICS sector by time fixed effects, and city by time fixed effects. In other words, I compare firms within the same city, 2-digit NAICS industry, and employment size group, tracing the effect of the PPP over time at a monthly frequency.

In this event study, I find that PPP increased average employment by 7.5% at treated firms in the five months following application to the PPP.[6,7] The estimate is significantly different from zero at the 1% level. I also find an insignificant effect on wages. Moreover, I find that that firms that received PPP did not hire more workers in general or rehire more former employees than non-PPP firms. Moreover, they were no more likely to rehire or hire employees on the intensive margin. My estimates also indicate that PPP applicants were slightly less likely to reduce employment in the entire post-PPP period, though the estimate is not significantly different from zero. Together, these results suggest that the program largely worked by preventing layoffs, as was its primary intent.

To the best of my knowledge, my findings are the first to show that the PPP worked as intended, largely by preventing layoffs, rather than inducing more hiring or rehiring of former employees, To confirm this hypothesis, I run cross-sectional regressions on employee turnover on firms characteristics. I separate the analysis by total hires, new hires, or rehires or former employees. A rehire of a previous employee implies that the the worker is known to be a good match to the firm and was likely laid off due to financial reasons, not for performance issues. Although the intention of the PPP was to preserve employee-employer relationships, I find no evidence that the PPP caused firms to rehire former employees at higher rates. However, I do find suggestive evidence that PPP firms were less likely to reduce employment, implying that more matches were preserved at PPP-applicants versus non-applicants.

Within these average effects there is significant heterogeneity by industry. The positive

[6] In the first month following PPP, my estimate is 13.7% which is quadruple the magnitude of that found in Autor et al. (2022) for the largest small businesses. The preferred estimate in that paper is a 3.25% increase in employment.

[7] On average, firms that applied for PPP and firms that did not both experienced employment declines over this period. The positive estimate implies that treated firms experienced a smaller decline.

effect of the PPP on employment was larger in industries with fewer hourly workers, more employees who can work remotely, and essential businesses. These results suggest that the effectiveness of the PPP depends on interactions with local economic conditions and restrictions on business activities. Firms were only able to reap the benefits of the program when they were able to continue operations and had sufficient consumer demand. There were no significant differences in hiring or rehiring trends between the different types of firms I examined.

There are several sources of variation that may drive differences in take-up, conditional on observables, but may or may not be related to firm outcomes outside of the PPP take-up channel. The primary source of such variation that I use for identification are the banking frictions associated with access to PPP. These frictions have been well documented in other papers (Granja et al. (Forthcoming), Erel and Liebersohn (Forthcoming), Barrios et al. (2020), Li and Strahan (2021), Ben-David et al. (2021)). Firms that had pre-existing relationships with more PPP-savvy banks were more likely to have quick access to a loan.[8] Thus the main identifying assumption is that PPP-savvy banks do not influence firm outcomes through other channels. This is consistent with the findings in Granja et al. (Forthcoming) that banks that did more PPP lending were actually smaller banks that typically did less SBA lending in general, thus suggesting that they are not necessarily more adept at small business financing. Moreover, the most commonly used banks (for payroll processing) in my sample were those that did proportionally small amounts of PPP lending, according to Granja et al. (Forthcoming). This suggests that banking frictions were likely to apply in this sample of firms and are thus a plausible source of exogenous variation.[9]

Of course there are other possible drivers of take-up. The complexity associated with the rapid roll-out of the program has also been well documented as a factor in the take-up decision. Three pieces of evidence support this. First, the initial wave of applications for PPP was rationed because the PPP ran out of money and stopped accepting applications

[8] While I observe which bank a firm uses to process their payroll, I do not observe which bank they used to process their PPP loan. This fact, combined with the small sample size, means that an instrumental variables approach is not possible in this setting.

[9] In section 4, I'll discuss in more detail the validity of the as-good-as random assumption when it comes to cross-bank variation.

for several weeks (Kimball (2020), Doniger and Kay (2021)). Being rationed in the first round may have discouraged some firms from applying later. Second, the requirements about how and when a loan would be forgiven were not only difficult to interpret, but also changed several times while the program was running, which presumably led to temporal variation in the attractiveness of the program across firms over time (Hayashi (2020), Brewer et al. (2020)). A number of businesses even took and subsequently returned loans. Third, given the confusion and complexity associated with the program, it is feasible that some firms chose not to apply simply to avoid navigating the process. Bartik et al. (2020b) find that over 10% of firms did not plan on applying for PPP because "it's a hassle". Another 10% did not think that they would get the money in time. Nearly one-third did not believe they were eligible, despite the survey only being administered to firms with less than 500 employees, most of which were presumably eligible. Balyuk et al. (2020) find hesitancy to take up PPP amongst small firms, due to concerns over government scrutiny. Together, these pieces of evidence support the idea that, controlling for observables, the take-up decision was driven by as-good-as-random differences, in which case my results are unbiased.

Two additional robustness checks address concerns about variation driven by private information or unobservable firm health prior to the decision to take on a PPP loan. I repeat all analyses using the balanced panel and again controlling for yearly growth in the first two months of 2020. The results are similar to the main sample. There is a positive effect on employment that declines over time and no statistically significant effect on wages for PPP-applicants. There is also no effect on hiring or rehiring in these subsamples. These results support the underlying identifying assumption that application was not selected on firm health.

0.1 Relation to Literature

This paper most directly relates to recent studies using differences-and-differences designs to evaluate the effectiveness of the PPP. There are two broad categories of paper that do so. First are those using firm-level data and second are those that use the SBA's loan-level PPP data combined with some measure of aggregate geographic outcomes. The first set of papers primarily uses the program's design for identification by comparing firms just above and below the 500-employee eligibility cutoff. Autor et al. (2022) finds a positive effect on both wages and employment, on the order of 2-4%. Chetty et al. (2020) finds a null effect on employment. Hubbard and Strain (2020) find a positive effect on employment of less than 1%. My work is complementary to these papers, but meaningfully expands upon their findings in three ways.

First, I study much smaller firms. Rather than focusing on firms around the 500-employee cutoff my data has a median size of five firms. As shown in Faulkender et al. (2020), 95% of PPP loans went to firms with 38 or fewer employees. Given the different operational structures and financing constraints (see Bartik et al. (2020a)) of firms of these different sizes, it is natural to think that their application decision as well as use of funds might differ. Indeed, given that I find much larger effects than the papers that use the cut-off strategy, financing constraints may be a significant factor in the effectiveness of the program at the firm level. Thus, it is essential to look at these very small firms when assessing the success of the program.

Second, this paper uses administrative data on PPP application status, rather than imputing application status based on size cutoffs. I know with certainty which firms in my sample applied for the program, thus I can measure treatment effects more precisely. In the papers that assume any firm with under 500 employees applied, there are inevitably some non-PPP firms in the treatment group. If the PPP truly had a positive impact on employment outcomes, the results using this that identification strategy could be downward biased. This is consistent with my finding of much larger employment effects in my sample.

Third, my data provides details not available in larger administrative payroll data sets. Namely, I have information on hiring and rehiring in the months following PPP. Thus, I observe not just the level of employment, but also whether the level changes were driven by hiring, re-hiring, or layoffs. As a result, I can speak directly to whether or not the program preserved jobs. Other papers that show only level changes cannot say if the PPP preserved pre-existing firm-employee matches.

Another paper that falls in this category is Bartik et al. (2020c). The data used in this paper is perhaps most similar to my data given the average number of employees: 7 as of January 2020. The main finding is that PPP increased firms' survival percentage significantly. However, this paper uses survey data and thus may be more subject to measurement error than my administrative data. It also lacks detailed information on wages and hiring. Another is Denes et al. (2021), which looks as firm financial fragility as an outcome, rather than employment outcomes.

The second set of papers, which do not have firm-level data, use identification strategies primarily related to the banking frictions and timing discontinuities associated with the PPP's roll out. Granja et al. (Forthcoming) uses geographical variation in banks, which had differing propensities to engage in PPP lending, and finds no significant effect of the PPP on employment or shutdowns at the local level.[10] Doniger and Kay (2021) uses the variation in timing due to funds running out for several weeks in April of 2020 to measure the effect of PPP, finding that it saved millions of jobs. Faulkender et al. (2020) also exploits variation in geographical banking structure and the timing of the loan roll-outs and find that a 10 percentage point increase in eligible payroll covered by PPP resulted in a 1 to 2 percentage point smaller jump in weekly initial unemployment insurance (UI) claims. Li and Strahan (2021) show that areas with better supply of PPP loans (due to presence of certain banks) have better employment outcomes. Bartik et al. (2020a) finds that states which received more PPP funding saw a faster employment recovery.

Compared to these papers, the focus of this paper is to assess detailed, firm-level outcomes, particularly for very small firms. Again, knowing application status provides the advantage of precise estimates that don't rely on being able to predict PPP-status. Firm-level data also allows me to look at the heterogeneous impact of the program across different types of firms. My data cannot speak as much to aggregate outcomes or random drivers of take-up. Both strategies provide unique information that is relevant for assessing

[10] Granja et al. (Forthcoming) has some firm-level data from Homebase. Homebase disproportionately covers small firms in food and beverage service and retail; therefore, it is not representative of aggregate employment.

the success of the policy.

My work is also related to other studies that assess the effectiveness of the PPP on various dimensions. Papanikolaou and Schmidt (2021) shows that PPP funds flowed to industries that potentially needed them less, as they were not as hard hit by the pandemic. Barrios et al. (2020) develops a framework for predicting demand for PPP loans and finds that, to date, disbursements have been quite similar to their model's predictions. Balyuk et al. (2020) finds that smaller firms were more hesitant to take up PPP and that banking relationships with small banks contributed to this. This paper substantiates these findings within a sample of very small firms.

This paper also contributes to the study of small business financing by providing another cost estimate for small business loan programs, particularly relief programs that occur during times of emergency. Brown and Earle (2017) studies the impact of SBA loans during normal times and finds that every million dollars in loans results in 3-3.5 new jobs created. Feyrer and Sacerdote (2011) examines the overall effectiveness of government stimulus during the 2008 financial crisis. The main finding is that the American Recovery and Reinvestment Act of 2009 created additional jobs at a cost of \$170,000 of stimulus per job. In contrast, I find a much higher cost-per-job estimate for the smallest firms, around \$270,000 per job per year.[11]

The paper proceeds as follows. Section 1 describes the details of the PPP. Section 2 describes the data. Section 3 substanties the stylized facts about PPP take-up in my sample. Section 4 details the research design for evaluating the effect of the PPP on firms outcomes. Section 5 describes my results on firm outcomes, using the differences-in-differences design. Section 6 briefly analyzes the aggregate effect and cost of the PPP that is implied by my results. Section 7 concludes.

[11] More generally, other papers have studied the importance of loan programs on business outcomes. Petersen and Rajan (1994) uses a survey administered by the SBA to study lending relationships. The main finding is that preexisting lending relationships can lead to easier access to funds; this is especially important in smaller firms where there are likely large information asymmetries between firms and lenders. Lelarge et al. (2010) finds that firms that received a loan guarantee from the French government had higher growth rates.

1 Policy Details

The Paycheck Protection Program (PPP) was a specific provision of the Coronavirus, Aid, Security, and Economic Relief (CARES) act intended to provide small business relief. Signed into law of March 27, 2020, the PPP program, implemented by the Small Business Administration (SBA) provides loans to small businesses, typically those with under 500 employees.[12]

Originally, $349 billion was allocated to the program, to be dispensed through the end of the year. However, the funding ran out less than two weeks after loans began to be dispensed. On April 24, 2020, an additional $320 billion was added to the PPP and the SBA began accepting applications again on April 27th, 2020. The first iteration of the program officially ended on August 8, 2020.[13] According to the SBA, the second round of loans dispensed smaller loans on average and the vetting process for approval was more stringent (Hare (2020)). For example, public companies were essentially excluded from the second round of loans.

The loans are low interest, with a rate of 1% and with a maturity of either two or five years.[14] Firms can apply to receive up to 10 weeks of payroll costs, based on their 2019 average payroll. Moreover, the loans are eligible to be forgiven in full if firms follow certain guidelines on how the funds are spent. The rules of the PPP have changed substantially since the program was first announced. In particular, the Paycheck Protection Program Flexibility Act (PPPFA), signed into law on June 5, 2020, changed several rules. Most of the changes related to forgiveness criteria applied retroactively to businesses that already had loans. I'll describe the rules in their form as of September 2020, which is the end of my sample period, but note that many businesses made the decision to apply with the original, more restrictive, rules in place. Appendix Table A.2 describes the changes that occurred after the PPPFA.

[12] There were some additional industry and tax based rules on eligibility. See SBA (2021b) for more details.

[13] The program was later extended, with extensive modifications, following the passage of the Consolidated Appropriations Act of 2021. That version of the program ran from January-May 2021.

[14] Originally, loans had a two year maturity. This was extended to five years for loans issued after June 5, 2020 with the Paycheck Protection Program Flexibility Act (see below).

In order to have the loan forgiven, at least 60% must be used to cover payroll costs. Additionally, the firm must maintain 75% of their full time equivalent (FTE) employment in order for the loan to be forgiven. The safe harbor rule gives firms until December 31, 2020 to satisfy this requirement.[15] The amount forgiven is "all or nothing": if a firm uses 60% of the funds for payroll, it is all forgiven while if they use less than 60% for payroll, none of it is forgiven.[16] This must occur over a 24-week covered period, meaning that firms must spend 60% of the loan, which is typically equivalent to 10-weeks of payroll costs, over a 24-week period.[17] Lastly, firms cannot reduce wages by more than 25% in order to qualify for loan forgiveness.

If the loan is not forgiven in full, payments can be deferred for six months plus the amount of time it takes for the SBA to give the lender the appropriate forgiveness amount. No personal collateral was required. However, despite the policy design, intended to make the loans more like a grant, business owners remained skeptical. A survey by Bartik et al. (2020b) found that over 30% of small business respondents said that they did not trust the government and/or the bank to forgive the loan. Moreover, business owners may have faced difficulties in spending even 60% of the funds on payroll and maintain 75% of their pre-pandemic FTE when many states had required businesses to remain closed and were offering generous unemployment insurance (UI) benefits during this time.[18,19]

The loans are financially attractive on several dimensions. Ignoring the forgiveness provisions, they are extremely low interest relative to typical SBA loans, which have an interest rate of 7-8%. Moreover, the deferral period, zero-collateral requirement, and the long maturity, especially for post-PPPFA loans, made the loans very low cost. The provisions enacted with the PPPFA also made forgiveness much more attainable. Since the

[15]If firms can document that they are unable to rehire individuals employed as of February 15, 2020 (or similarly qualified replacements) by December 31, 2020, this will not count against their forgiveness amount. This can either be due to individual choice on the part of the employees or because their business activity is limited by local governments.

[16]Prior to the PPPFA, the amount to be forgiven would be proportional to the amount that was spent on payroll. See Appendix Table A.2.

[17]This is a substantial relaxation of the original rules, in which the covered period was only eight weeks.

[18]Another source of confusion has been what will occur if a company goes bankrupt after receiving the loans, or is currently in bankruptcy proceedings (Moore (2020), Iacurci (2020))

[19]It is worth noting that Bartik et al. (2020a) and Altonji et al. (2020) find no effect of increased UI benefits on aggregate employment.

covered period is 24-weeks, firms have nearly 6 months to spend 60% of 10 weeks of payroll on wages. This means that firms only need to maintain a quarter of their pre-pandemic level of employment to qualify for forgiveness. The rules around wage reduction and FTE maintenance are somewhat alleviated by allowing provisions for businesses that face government shut-downs or employees that refused good faith offers of employment.

Consistent with these favorable terms, take-up of PPP loans was quite high. As of August 8, 2020, when the second round officially ended, $525 billion in loans had been disbursed to over five millions firms. Barrios et al. (2020) estimate that demand would be $750 billion if all eligible firms took up the loan. Appendix Figure A.1 shows the distribution by firm size of firms that received PPP versus all firms in the U.S. The calculated take-up rate is 75%. The SBA estimated that PPP loans had covered 72-96% of all payroll across states as of June 2020 (SBA (2020)). In this paper, I focus primarily on the first and second rounds of PPP, which stopped accepting applications on August 8, 2020. Following the passage of the Consolidated Appropriations Act of 2021, the PPP restarted in January 2021 with significant modifications, and continued through May 31, 2021.

2 Administrative Payroll Data

My data is from a private payroll processor that is headquartered in the American southwest. The company services nearly 400 firms, representing approximately 6,000 employees prior to the pandemic's start. The firm has clients located in nine U.S. states, but the majority of the clients (and employees) are located in one U.S. state in the Southwest. The company has provided me access to firm level data at the monthly frequency from January 2019-September 2020. The data contains information about industry, number of employees, hiring, total wage bill and whether or not the firm applied for a PPP loan during the pandemic.[20,21]

[20] I assume that if a firm applied, they received a loan. This is likely to be the case for most firms, as the acceptance rate was high (Horan (2020)).

[21] Note that I observe if a firm applied, regardless of which bank they applied through. Thus, I know application status even if they applied via a non-traditional lender, like FinTech. This may be particularly important for these very small firms (Erel and Liebersohn (Forthcoming)).

The firms represented in the data set are quite representative of all U.S. small businesses, both in terms of industry composition and size. The top panel of Figure 1 shows the breakdown of firms by industries, measured at the 2-digit NAICS code level. The bottom panel shows the distribution of employment at these firms prior to the pandemic (February 2020). Despite the small sample size, the firm has broad coverage of many different industries. Moreover, it matches the distribution of firms in the U.S. and the states in which it has clients reasonably well. It is over-represented in the healthcare sector and somewhat underrepresented in retail and wholesale trade, relative to the U.S. total.

Table 1 shows summary statistics for the sample versus all businesses in the U.S. with under 500 employees. The average firm had a mean of 14 employees from February 2019-February 2020, versus 9 in the 2016 SUSB data. Average monthly wages are slightly higher than in the US, but the mean wage is slightly lower, perhaps due to the differing sector compositions.[22] In sum, the sample is quite comparable to the average small firm in the U.S. and has a good representation across sectors.

The sample provides a snapshot of how effective the PPP was for some small and very small businesses, particularly those in states that did not have a severe case load during the first wave (March and April 2020) of COVID-19 the United States. During the first wave, which coincided with the start of the PPP, there was a much lower case load in the states where my firms operate compared to the U.S.[23] Around mid-June, the states represented in my data see an increase in case-load. Regarding the timeline of events affecting business activity, there was a shutdown that took place from early April to mid-May. However, the requirements of the shut-down were relatively relaxed compared to states that had more severe outbreaks. Moreover, many businesses were allowed to start reopening before the shutdown technically ended.

The sample is clearly selected in several ways. First, the firms are geographically concentrated in the American southwest, where the first wave of the virus was less severe.

[22] The sample over-represents the health service sectors, in which the average wage is below the national average (BLS (2020b)).

[23] Appendix Figure A.2 shows the number of cases per person in the states in which the firm has clients versus the U.S. total.

(a) Number of Firms

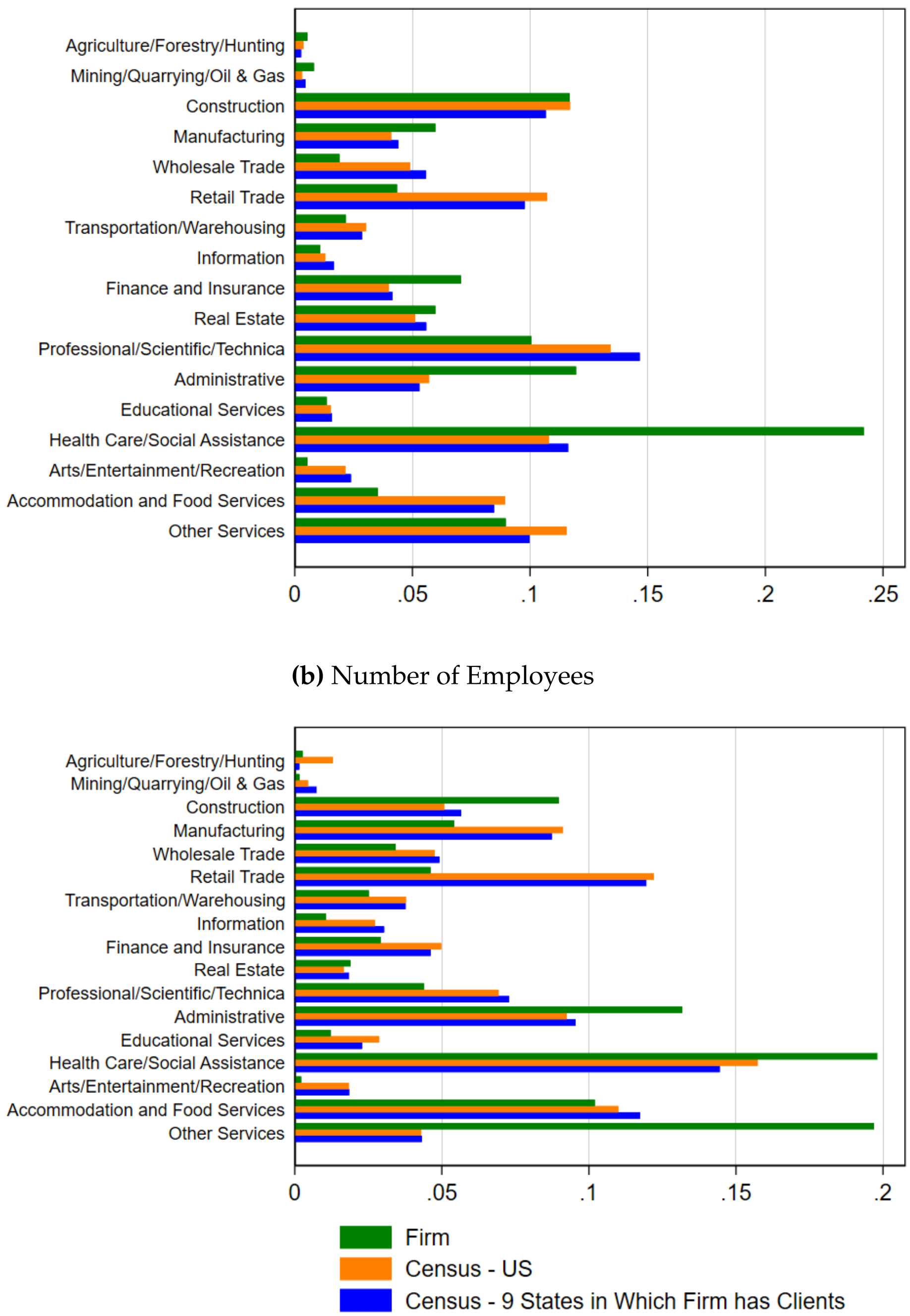


Figure 1: Distribution by NAICS Industries

Notes: These figures show the distribution of the number (top) and employment (bottom) of firms in my sample by 2-digit NAICS code compared to the total U.S. and the nine states in which the firm has clients. Appendix Table A.1 shows the top panel in table format, with sample firms split out by PPP application status.

Second, these firms are primarily small enterprises, with the median firm having only 5 employees (prior to the pandemic). The largest client has just over 300 employees.[24] Thus, the sample does not cover large firms, but rather captures a sample of small to medium sized enterprises. Third, the firm's clients are concentrated in specific industries (see Figure 1). In particular, the healthcare industry is over-represented relative to the U.S. population. Lastly, the firms are those which would hire a payroll processor, rather than do their payroll in house. Despite these caveats, the summary statistics compared to the U.S. total are reassuring in the sense that these firms are, at least based on size, fairly representative of the average small business in the U.S.

Table 1: Summary Statistics of Sample Firms

	Applied for PPP?			
	No	Yes	All	All Small Businesses < 500 Employees
Employment	8.90 (2.67)	17.82 (8.15)	13.95 (5.31)	9.32
Monthly Wage Bill ($)	33,276.77 (6,868.77)	49,518.42 (26,817.52)	42,477.50 (16,229.22)	36,737.91
Average monthly wages per worker	3,200.01 (2,244.35)	3,364.12 (2,983.48)	3,292.98 (2,641.47)	3,639.12
Observations	163	214	377	

Notes: This table shows summary statistics (means) for the firms in my sample. Medians are in parentheses. Employment and wage figures are based on the firms' averages from February 2019-February 2020 (for one year prior to the pandemic in the U.S.). The first and second columns show the statistics for firms that did not apply and applied for PPP, respectively. The third column shows the full sample. One firm in the sample has over 500 employees and therefore is not eligible for the PPP, so it is dropped from all analyses. The final column shows a comparison for all firms with under 500 employees in U.S., which are calculated from the 2016 Statistics for U.S. Businesses (Census Bureau).

The data have been provided to me on a monthly basis. I observe time varying data on wage bills, number of employees, and the composition of new hires (i.e. whether a new hire is a rehire of a previous employee, or a completely new person).[25] I also observe several fixed characteristics of the firm: zip code, 6-digit NAICS code, and the bank where

[24] The firm has one client with just over 500 employees, but it is dropped from all analyses as it is ineligible for the PPP.

[25] Wage bills are normalized to the monthly level based on the firm's pay frequency and how many paydays they have in a given month. Most firms in the data pay at a biweekly frequency, so the wage bills must be adjusted to account for the fact that some months have five Fridays while some have four.

the firm processes payroll. In most of the analysis, I keep firms that were present in the data beginning in February 2020 and remained until September 2020.[26,27]

3 PPP Loan Take-up

In this section, I show that take-up in my sample is consistent with the stylized facts on PPP take-up that have been documented elsewhere in the literature. The advantage of my data is that I have more details than are provided in the Treasury data release and most other administrative datasets. In particular, I know exact employment and wage bill and the firms' histories over the year and half prior to the pandemic. Within my sample, I can explore the factors which contribute to a firm not applying for the PPP. As discussed in Section 1, these loans have extremely favorable terms, have the chance of turning into a grant, and are being offered in the middle of a large economic crisis that has impacted firms across the board. Hence, not applying is a puzzle.[28]

To estimate the effects of variables found to be associated with take-up in previous analyses, I run a logistic regression of the form[29]:

$$PPP_i = \alpha + \gamma_j + \gamma_s + \gamma_b + X_i + \epsilon_i \tag{1}$$

where γ_j is an industry fixed effect, γ_s is a state fixed effect, γ_b is a bank-PPP lending tercile fixed effect and X_i is a vector of variables of interest: baseline size, average salary, and a measure of the size of the firm's shock in April of 2020, either at the individual firm level or the industry-wide level.

[26] Unreported robustness checks impute zero for firms that drop out, rather than dropping them from the analysis. This occurs only for a small number of firms (< 10). Results are similar.

[27] Note that the Treasury has released detailed information on all firms, including name and the dollar value of the loan received. However, due to a privacy agreement with my data provider, I am not able to match my data to the Treasury data.

[28] An important agenda for future research is to analyze why 25% of firms in total did not apply. Given the sample selection, my results here may or may not generalize to the whole population.

[29] The variables of interest are also motivated by summary statistics in my own sample. Appendix Figure A.3 shows several factors that seem to be important for take-up based on the previous research, split by PPP-applying and non-applying firms. The figures confirm that larger firms with more highly paid employees, and firms that are in certain industries are more likely to apply.

The results are shown in Table 2. First, I'll discuss the results on size. Size has been well documented as a factor in take-up in other studies, with larger firms being more likely to apply for and receive PPP loans (Barrios et al. (2020), Bartik et al. (2020a), Balyuk et al. (2020), Erel and Liebersohn (Forthcoming)). This is true in my sample as well. Even with the full set of controls in column (5), the odds ratio is nearly 1.8, implying that a firm that is one log-point larger, in terms of employment, has 1.8 times the odds of applying. In the context of this sample, the interpretation is that a firm at the 75th percentile of the employment distribution (12 employees) is 13% more likely to apply than a firm at the 25th percentile (3 employees), all other factors constant.[30]

Second, industry is a significant factor in take-up. However, this relationship is not a one-to-one positive correlation with pandemic exposure. That is, not all firms that were most exposed to the effects of the pandemic were most likely to apply. Figure 2 shows the odds ratios on the industry fixed effects from the regression in the fifth column of Table 2. The reference category is the Food and Accommodation Services sector. Almost all industries were less likely to apply than those in Food and Accommodation Services, with the exception of Trade, Information, Professional Services, Educational Services, and Healthcare and Social Assistance. Somewhat surprisingly, these are actually not the industries that had the largest employment declines during the early pandemic, as measured by Cajner et al. (2020). They are however, some of the most exposed to the pandemic, as measured both by the percentage of workers that are able to work from home, as in Papanikolaou and Schmidt (2021) and the amount of face-to-face interaction required with customers, as in Leibovici et al. (2020).

The coefficient on average wage is also significant and greater than one across each specification. This implies that firms within the same industry, the same state, that use a similar bank and have similar employment levels and similar employment growth in April of 2020 are more likely to apply if they have a higher average wage per worker. A firm at the 75th percentile of the average wage distribution is about 5.5% more likely to apply

[30] I also find significant differences between those firms that applied early, during the first round of PPP that took place from April 3, 2020-April, 17, 2020, versus those that applied during the second round, in May or June. Appendix Tables A.3-A.5 show the results.

Table 2: Take-up Regressions: Full Sample

	(1)	(2)	(3)	(4)	(5)
			Odds Ratios		
Log Base Employment	1.7912	1.7654	1.8254	1.7903	1.7847
	(0.2620)	(0.2964)	(0.2391)	(0.2677)	(0.4841)
Log Average Salary (Base Period)	1.4498	1.4290	1.4654	1.4514	1.3958
	(0.2349)	(0.2285)	(0.1904)	(0.1755)	(0.1345)
Monthly Employment Growth (April 2020)		0.9643		0.9204	0.8478
		(0.3443)		(0.2785)	(0.3526)
Average industry employment change during pandemic			0.1994	0.2027	
			(0.1325)	(0.1438)	
Bank in 2nd Tercile of Overall PPP Lending					1.2385
					(0.5479)
Bank in 3rd Tercile of Overall PPP Lending					1.5148
					(0.2865)
Industry FE?	Y	Y	N	N	Y
State FE?	Y	Y	Y	Y	Y
N	345	342	342	339	291
pseudo R^2	0.126	0.120	0.112	0.107	0.121

Notes: This table shows the results of a logistic regression of an indicator equal to one if the firm applied for PPP on a set of controls for firm characteristics (equation (1)). The odds ratios are reported. Log Base Employment is the log of the firm's average employment from February 2019-February 2020 (or the months in which the firm appears in that data set over that time period). Log Base Average Salary is the firm's average monthly wage per worker over the same time period. Monthly Employment Growth (April 2020) is the firm's log employment growth from March 2020 to April 2020. Average industry employment change during pandemic is the percentage decline in paid employment for the firm's two-digit NAICS code from February 15, 2020-April 25, 2020 (taken from Cajner et al. (2020)). 2nd and 3rd tercile of overall PPP lending are dummies equal to one if the firm's primary bank is in the second or third (highest) tercile, respectively, of share of PPP lending less than $150,000 in the firm's state. Standard errors, clustered at the 2-digits NAICS code level, in parentheses.

than a firm at the 25th percentile. This, combined with the industry results, is consistent with the evidence documented in Granja et al. (Forthcoming), Papanikolaou and Schmidt

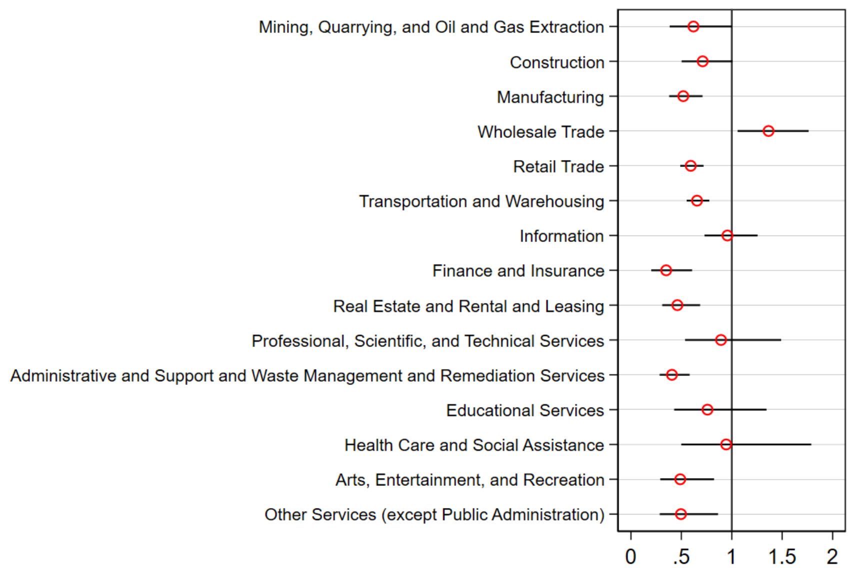


Figure 2: Odds Ratios of NAICS fixed effects

Notes: This figure shows the odds ratios for the NAICS industry fixed effects from the logistic regression of take-up on firms characteristics (equation (1)). The reference category is NAICS 72, Food and Accommodation Services. Standard errors are clustered at the 2-digit NAICS level. The coefficients correspond to the final column in Table 2.

(2021) and Joaquim and Netto (2021) that funds flowed disproportionately toward firms that were less in need.

Third, column (5) shows that firms that use a primary bank that did more PPP lending in their state overall are more likely to apply. This is consistent with the findings in Granja et al. (Forthcoming) and Faulkender et al. (2020). Specifically, a firm with a bank in the top tercile of PPP lending in the state has 1.5 times the odds of applying compared to a firm in the bottom tercile.[31]

Finally, there is no significant effect on the monthly employment growth variables in any specification, indicating that there is not adverse selection into PPP application after controlling for industry, size, and bank.[32,33]

[31]See also Li and Strahan (2021), Balyuk et al. (2020), Erel and Liebersohn (Forthcoming).

[32]Hence, the difference observed in Appendix Figure A.3b is likely a function of size and/or industry composition amongst appliers versus non-appliers. In other words, industries that had larger drops in employment in April were also more likely to apply.

[33]The results also hold if I control for firms' long-term (yearly) growth prior to the pandemic (Appendix Table A.6) The results are also qualitatively similar looking only at the balanced panel of firms that have

The results are robust to using an industry fixed effect (columns (1), (2), and (5)), or a control for the industry's employment decline in the early pandemic (from February 15, 2020-April 11, 2020, as measured in Cajner et al. (2020) (columns (3) and (4)). The odds ratio of less than one on this variable implies that firms in industries with higher employment growth during the early pandemic were less likely to apply, as expected.

Consistent with studies to date that have analyzed PPP loan take-up, I find significant effects of size, industry, bank, and average wage per worker. The results confirm that firms in industries more exposed to the pandemic, but not necessarily firms that were individually more adversely affected, were more likely (and quicker) to apply. Moreover, larger firms and firms with higher wages per worker were more likely to apply. Lastly, firms with banking relationships with banks that did more PPP lending overall were more likely to apply.

4 Identification of PPP Treatment Effects

My methodology to assess the effectiveness of the PPP is a dynamic difference-in-differences event study, similar to Autor et al. (2022). In order to assess the PPP's effectiveness, we need a valid counterfactual. In other words, what would have happened to firms if they hadn't applied? While no such counterfactual exists, I can estimate it by looking at firms that are otherwise similar on observables but did not apply for the PPP. Because I observe whether or not a firm applied for the PPP, I can directly compare the outcomes of firms that applied to those that did not. The specification is:

$$y_{it} = \alpha + \theta PPP_i + \sum_{t \in T} \beta_t (\theta_t \times PPP_i) + \gamma_e + \gamma_{jt} + \gamma_{st} + \epsilon_{it} \tag{2}$$

where PPP_i is an indicator equal to one if the firm applied for the PPP, θ_t is a time dummy that corresponds to months relative to when the firm applied for PPP[34], γ_e is a dummy for

been in the data set since January of 2019 (Appendix Table A.7). These robustness checks serve as additional evidence that firms likely didn't change their application decision based on private information.

[34]Firms in my sample applied in either April, May or June. θ_t indexes months since application, with $t = 0$ corresponding to the month of application, thus this varies across firms. I set $t = 0$ to April for all control

the firm's employment size[35], γ_{jt} is a 2-digit NAICS code by month fixed effect, and γ_{st} is a month by city fixed effect. y_{it} is an outcome variable, employment, total wage bill, or a measure of hiring divided by the value of the variable in February 2020. The large set of controls ensures that I compare outcomes of treated firms to similar untreated firm, hence I can estimate the average effect of treatment on the treated (ATT).

The assumption underlying the estimation is that, in the absence of the PPP, control and treated firms would have had similar outcomes. In other words, differences in take-up are driven by as-good-as random variation, primarily related to the complexity, speed, and subsequent banking frictions with which the program was introduced. The regression itself takes several steps to ensure that this is the case. First, the employment size fixed effects ensure that I compare firms of similar size. Second, the time by industry fixed effects control for time varying shocks within a given industry that are common to all firms in that industry. Given the varied exposure to the pandemic across industries (Papanikolaou and Schmidt (2021)) this is quite important. Lastly, the city by time fixed effects control for time varying shocks within a city that are common to all firms in that city. This is especially important, given that the pandemic affected all areas differently and local governments had heterogeneous responses to contain/prevent outbreaks.

The banking frictions, from which my results are partially identified, warrant additional discussion. Granja et al. (Forthcoming) and others have documented that there was significant heterogeneity in the administration of PPP loans across banks. Consequently, firms in locations with access to those banks were more likely to receive loans. Some firms that wanted PPP may not have applied simply because they didn't have a banking relationship with the right bank. However, one may worry that a banking relationship is correlated with firm outcomes, independent of its impact on PPP status (Petersen and Rajan (1994) and Brown and Earle (2017)). Granja et al. (Forthcoming) find that the banks that did the most PPP lending were actually banks that typically did less SBA lending overall. This suggests that PPP-heavy banks are not necessarily more adept at small business financing

firms. The industry by time and city by time fixed effects refer to calendar time, thus these controls account for differences in economic conditions from month-to-month.

[35]I group firms into buckets with between one and five employees, between five and 25 employees and greater than 25 employees. This roughly corresponds to terciles.

in general. Indeed, the three largest banks by deposit share in my sample are JP Morgan Chase, Bank of America, and Wells Fargo, which did a disproportionately small amount of PPP lending (relative to their usual small business lending). This alleviates some concern about how the bank may influence the firm's outcome, outside of PPP lending.

Which additional sources of variation might drive differences in take-up, conditional on observables, but are not related to firm outcomes (other than through PPP take-up)? There are two such sources that may call into question the main assumption underlying my estimation. In what follows, I'll discuss each and argue that they are not significant sources of bias in my sample. First, is a CEO or manager's beliefs about the severity and duration of the pandemic. Bartik et al. (2020b) found in an April 2020 survey that about half of firms believed that the crisis would be over by July of 2020; given this belief, some firms may not think it worth it to apply. If this is the case, these businesses would likely appear better off in the short-run, but then would be more negatively impacted as the pandemic continued. This is the opposite of what I observe: I see untreated firms becoming more similar to treated firms over time. This indicates that this is not a primary driver of take-up in my sample.[36]

Second, the manager may have some private information about the firm's prospects.[37] That is, a CEO/manager may have known that his firm was likely to fail with or without the PPP. This is obviously correlated with the ex-post outcomes. While I can't observe this directly, I can control for it with several measures available in my data. First of all, I can observe how long the firm has been with the payroll processor. Given that younger firms are more likely to fail, limiting the analysis to firms who have been with the payroll

[36] A likely alternative story is that if a manager believes that the pandemic is not serious and hence wouldn't expect it to impact their business much, this makes the PPP essentially "free money". Especially given that they have a payroll processor dedicated to helping them apply, these firms may even be more likely to apply given their optimism (see Landier and Thesmar (2009)). Hence, if many firms that took the loans did not direly need the assistance, my estimates might overstate the effect of the PPP. This is consistent with Joaquim and Netto (2021), which shows that firm-level regressions are likely to overestimate the effect of the PPP, due the the banking system's distorted incentives.

[37] Another possible potential source of variation is the firm's financial sophistication. Firms with a dedicated CFO or accountant, or better financial records may be more likely to apply. Then, one might believe that such firms may be more or less successful in navigating the pandemic. I rule this out as primary explanation for the take-up decision, because I am dealing with a sample of firms that already use a payroll processor. By definition, these firms have someone dedicated to maintaining their wage and employment records.

processor since the beginning of my data set (January 2019) somewhat alleviates this concern. In robustness checks, I repeat all analyses using the balanced panel and again controlling for yearly growth in the first two months of 2020. The results show a positive, significant, and decreasing of the PPP on employment in the five months following PPP application, similar in magnitude to the effect observed in the full sample. Hence excluding firms that are statistically most likely to fail, based on age, maintains the results. However, the general direction of the bias due to this source of variation is ambiguous. It could be that firms with this private information do not apply, thus overestimating the results, or it could be that they do apply, thus attenuating the results. The fact that I don't see large differences in employment or wage levels between the control and treatment group five months after application suggests that this is not a significant source of bias in my sample. Moreover, as of September 2020, very few firms had left the sample, suggesting there had been minimal business failures.[38]

A second version of the analysis collapses all observations by either pre or post-PPP:

$$y_{it} = \alpha + \theta PPP_i + \beta_t(\theta_{t=POST} \times PPP_i) + \gamma_e + \gamma_{jt\in\{Pre,Post\}} + \gamma_{st\in\{Pre,Post\}} + \gamma_t + \epsilon_{it} \quad (3)$$

This measures the average effect of the PPP in all months following application for treated firms, relative to controls after April. The same control variables are used in order to compare firms in the same industry and in the same city. An additional control for calendar time fixed effects, γ_t is added in order to account for the differences in economic conditions each month.

The coefficient of interest in both cases is the β_t vector. This traces the effect of the PPP over time (or pre versus post-PPP in the case of equation (3)). I begin the regressions in February 2020, using all firms that were present as of January 2020.[39] If a firm drops

[38] Note that business failures are difficult to measure. First, some firms did undertake furloughs at various points in the sample, only to reenter at more normal wage and employment levels later. Thus a month of zero wages does not necessarily mean failure. Second, just because a firms leaves my sample, doesn't mean they have failed; they may simply have ended their relationship with the payroll processor.

[39] I exclude January due to seasonality: firms often make adjustments to employment and compensation during this month. I exclude the data from 2019 to preserve sample size.

out at any point after February 2020, I drop them from the sample entirely.[40] Examining the coefficient prior to the start of the pandemic serves as an additional check on the comparability of firms. If PPP application is not correlated with unobservables, there should be no trend prior to the start of the pandemic.

5 Results

Using the difference-in-differences analysis, I first show that the PPP had a positive but transient effect on employment for treated firms in the full sample, on the order of nearly 14% in the first month following application to the program. The effect decreases over time and is statistically equivalent to zero five months after application. Then, I show that there was no significant effect on total wage bill. Next, I show that the positive employment effects occur primarily in industries that are less exposed to the pandemic: industries with fewer hourly workers, more remote workers, and essential businesses. Hence, the program was only effective for firms least affected by government shut-downs and social distancing. Lastly, I show that there is no evidence that firms receiving PPP funds were more likely to rehire former employees or hire more employees in general; the increase in employment at PPP firms is driven primarily by fewer layoffs.

5.1 Employment and Wage Effects

The PPP had an initial positive effect on employment at treated firms that is significantly larger than most other studies have found. The effect declines to zero over time, but is positive and significantly different from zero in the entire post-PPP period that I study. The first panel of Figure 3, shows the results from equation (2) with employment as the dependent variable. The point estimate for one and two months prior to the onset of the pandemic and the month of the onset are all statistically zero. This indicates that firms

[40]Unreported obustness checks impute zero for firms that drop out, rather than dropping them from the analysis, as in Autor et al. (2022). This paper says that to do so is "conservative", because firms that drop out of their sample may not have failed, but just left the payroll processor. However, whether or not this is "conservative" depends on if the firm is in the treatment or control group, thus I elect to just drop firms that leave the sample entirely. This occurs only for a small number of firms (< 10). Results are similar.

are ex-ante similar, holding the covariates constant, supporting a causal interpretation of the PPP on employment. One month following PPP application, average employment increased by 13.7% at treated firms. The effect is significant at the 1% level. Two to four months after the PPP, the estimate is again positive, significant and around 12-14%. The effect declines to zero five months after application. The estimate for one month after substantially larger in magnitude than that of Autor et al. (2022), which estimates an effect between 2-4.5% as of the end of May on much larger firms (around 500 employees). Figure 3c shows the results from equation (3), which aggregates each month after application to one "post-PPP" period. The aggregate effect on employment is 7.5% and is statistically different from zero at the 1% level.

I also find that the PPP had no corresponding impact on the firm's total wage bill (Figure 3b).[41] Again, the point estimate one and two months prior to PPP application is close to zero, implying that firms are similar on the observables, ex-ante. Total wage bill decreased by 12.8% the month of application, though this estimate is not statistically different from zero. In the following months, total wage bill increased relative to controls in each month after application. However, none of these estimates are significantly different from zero. The aggregate result in 3c suggests a near zero effect on wages, on average.

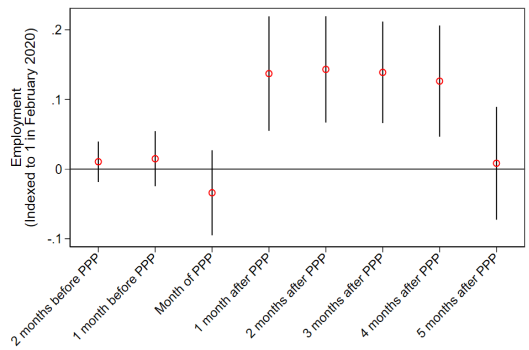
These results are not driven by positive selection into PPP application. I have already presented several pieces of evidence to show that firms that were not as affected by the pandemic (within an industry and city) were not more likely to apply. First, the results on take-up suggested that firms that applied were in fact in industries more exposed to the pandemic. Thus, it seems unlikely that firms more affected on some unobservable dimension would be more likely to apply. Second, the point estimates of the difference-and-difference specification in the month of application are statistically zero for both employment and wage bills.

I address remaining concerns about selection in three ways.[42] First, I repeat the analysis

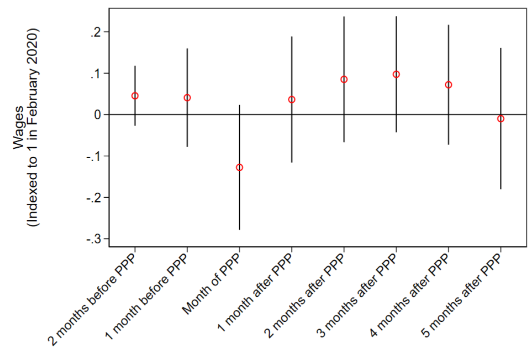
[41] In the following subsection, I will show that this ambiguous result is partially driven by heterogeneity across types of firms.

[42] As discussed in Section 4, firms may have private information about their firm's prospects and apply for the PPP even if that private information is negative. This would lead to attenuation of my results, as

(a) Employment



(b) Wage Bill



(c) Employment & Wage Bill, Average Effect

Figure 3: Effect of PPP on Employment, Wage Bill, and Wage per Worker

Notes: These figures show the results from the difference-in-differences specifications. Panels (a)-(b) correspond to equation (2) and panel (c) corresponds to equation (3). I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The dependent variables are normalized to one in February 2020. Black lines represent 90% confidence intervals.

only on the balanced panel of firms that have been in the data set from January 2019-June 2020. This provides some reassurance that more established firms are not being compared to fledgling firms in the analysis. The results, shown in Appendix Figure A.6 suggests a similar effect in this sample of firms. Second, I repeat the analysis adding a control for average yearly growth in January and February of 2020.[43] The results, shown in Appendix Figure A.7, also show a similar effect for both employment and wages. These results suggest that the PPP is similarly effective for firms that are more established, suggesting

such firms would pollute the treatment groups with poorer outcomes. Moreover, if only firms that were very negatively affected by the pandemic early on apply, the results are also attenuated.

[43] Note that this includes only firms in the balanced panel starting in February 2020, by construction.

that adverse selection is not present.

Third, I present suggestive evidence that the results are not driven by unobservable differences between firms. Appendix Figures A.8-A.9 show the results for each outcome variable, respectively, but with the controls added progressively. Appendix Figure A.8 shows that, even with no controls, the difference between treated and controls is nearly zero before the pandemic and the PPP. Even adding the size control barely changes the results, which is intuitive given that the dependent variables are measured relative to February for all firms. However, the effect on employment is zero before adding industry controls (Appendix Figure A.8c). It is not until we compare firms within the same industry that we see the PPP have a positive effect on employment ex-post. Then adding city by time fixed effects (Appendix Figure A.8d) results in firms that are more comparable ex-ante and a slightly larger effect. The fact that all of the point estimates are close to zero prior to PPP application, is reassuring that there are likely not unobservables that would significantly alter the firms' comparability. The results on wage bill in Appendix Figure A.9 shows a similar pattern.

5.2 Heterogeneity by Firm Type

In this subsection, I show that the PPP is much more effective for firms in industries with a smaller number of hourly workers, more workers that are able to work from home (remotely), and for essential businesses. The results suggest that the PPP in isolation is insufficient to buffer firms from the effects of the pandemic. However, if firms are able to remain open, or have employees who can work from home, then the PPP has positive effects on both employment and wages.

First, I establish that firms in industries with a low amount of hourly workers are positively impacted by the PPP, while firms in industries with a high number of hourly workers are not. I do this by first classifying industries based on the prevalence of hourly workers. Using data from the BLS, I calculate the percentage of workers that are hourly in each two-digit NAICS code (BLS (2020a), BLS (2016)). I then repeat the analysis on the firms that are in industries in the top tercile of the distribution (have the most hourly workers) versus those that are in the bottom tercile, (have the fewest hourly workers).[44]

As shown in Figure 4a, the employment gains observed in the full sample take place entirely in industries with a low percentage of hourly workers. Average employment in these industries increased by 8.5% in the five months following application to the PPP. As in the full sample the effect is initially higher and decreases over time (see Appendix Figure A.10). On the other hand, firms in industries with many hourly workers experienced no effect on employment.

There is no effect of PPP on wages for firms with many or few hourly workers (Figures 4a, A.10c, A.10d). The positive effect on employment and zero effect on wages for firms with few hourly workers suggests that these firms may have maintained employment but reduced wages.

In sum, industries with many hourly workers benefited less from the PPP than firms with more salaried workers. It is also true that industries in which most workers are not hourly are those that are less affected by stay-at-home orders. For example, many professional services firms, such as finance, insurance and tech, continued operating during the pandemic with their employees working remotely. On the other hand, restaurants, hotels, and retail stores, where many workers are hourly, were not able to do so (Papanikolaou and Schmidt (2021)). Hence, even firms in the latter industries that received PPP may not have been able to put the money to full use with shut down orders in place.

To test this hypothesis, I break out firms based on the percentage of workers in an industry that are typically able to work from home, as measured in Papanikolaou and Schmidt (2021).[45] I repeat the difference-in-difference analysis for firms in the top versus bottom half of the distribution for workers that can work from home.

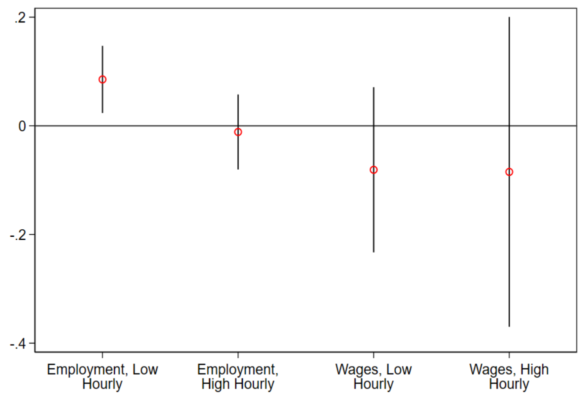
The positive effects of the PPP on employment are much larger in industries in which many workers can work from home (Figure 4b). Average employment at the most remote capable firms increased by 19% in the five months following PPP. Average employment at the least remote capable firms, also increase, but by less than half as much, 9.9%. Both

[44] Appendix Table A.8 shows the percentages for each industry.

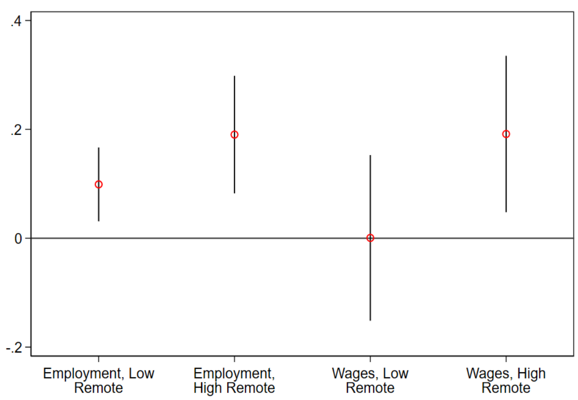
[45] Note that the remote measure and the hourly measure are highly correlated. See Appendix Table A.9.

Figure 4: Average Effect of PPP, by Firm Type

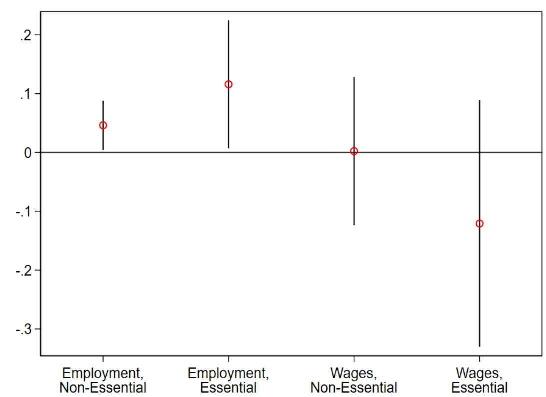
(a) By Prevalence of Hourly Workers



(b) By Remote Capability



(c) By Essential Businesses



Notes: These figures show the results from the difference-in-differences specification in equation (3), split across different types of firms. I plot the coefficient on β_t, which estimates the difference between firms that applied for PPP and firms that did not after application to the PPP. The dependent variables are normalized to one in February 2020.

effects are statistically significantly different from zero. Appendix Figures A.11a and A.11b show that the employment effects declined to zero over time for the highly remote firm, but remained positive throughout the sample period for the low-remote firms.

The findings on wages in this subsample also reveal significant heterogeneity, which partially drives the ambiguous result in the full sample. Figure 4b shows that the PPP had a positive effect on wages for the highly remote workers of 19.1%, but no effect on wages for the low-remote workers. A.11c and A.11d show the results on a monthly basis. As with employment, the effect of PPP is initially positive but declines over time for the highly-remote firms.

The fact that PPP raised employment primarily in firms where workers are not paid hourly and in industries where they can work from home suggests that PPP's effectiveness was limited by stay at home orders and decreased demand/foot traffic due to social distancing. To test this hypothesis, I split out the results by essential versus non-essential businesses.[46]

Indeed, the positive effects of the PPP on employment and wages occurred mostly within essential businesses (Figure 4c). Essential businesses increased employment by 11.6% in the five months following PPP. Non-essential businesses increased average employment by only 4.6% over the same period. The month by month results (Appendix Figures A.12b and A.12a) show that the effect declined over time for both types of firm. The effects on wage bills and indicate that PPP had no effect for both essential and non-essential businesses

PPP-treated firms that were not allowed to remain physically open (non-essential businesses) did not benefit from the program, while essential businesses did. At the same time, only firms with with many remote-capable workers and fewer hourly workers benefited from the program. This suggests that the PPP was only effective for these small businesses if they were able to remain operational in some capacity, either due to being

[46] I categorize firms using the list provided in Papanikolaou and Schmidt (2021). This list is based on a conservative definition intended be applied for the whole U.S. The definition of essential in the state where most firms in my sample are located is certainly more generous. Hence, I am likely classifying some businesses that were considered "essential" as non-essential.

considered essential or from being able to conduct business remotely.[47]

5.3 Employee Turnover

In this subsection, I show new evidence that differences in employment at PPP versus non-PPP firms were driven by fewer layoffs, not increased hiring or rehiring of former employees. This holds in the full sample as well as the subsamples comparing more hourly versus less hourly, remote versus non-remote, and essential versus non-essential industries. To show this, I utilize the data on employee turnover. For every new employee that a firm hires, I am given data on whether the employee is a new hire or a rehire of a previous employee.[48]

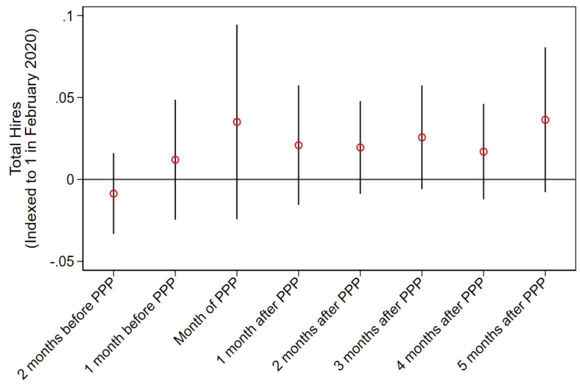
I first repeat the difference-in-differences analysis to show that hiring behavior was not significantly different between PPP and non-PPP firms in the months following application. The results using the number of total hires, new hires, and rehires as a percentage of employment in February 2020 as dependent variables are shown in Figure 5. For total hires (Figure A.22a), there is a positive effect of the PPP in the month of application, though it is not significantly different from zero. The effect decreases one and two months following application and then increases again, but its never significantly different form zero. The pattern is similar for number of new hires (Figure A.22b). Figure A.22c shows that PPP-treated firms did not re-hire more former employees in the months following treatment either.

I supplement the analysis by running cross-sectional regressions, looking specifically at the month of, one, and two months following application as well as the entire-post PPP period to compare treated versus control firms. Specifically, I regress the number of total, new, and rehires as a percentage of February 2020 employment on an indicator variable for PPP application, a control for lagged monthly employment growth (in order to compare firms that would need to hire a similar number of employees to reach their previous level

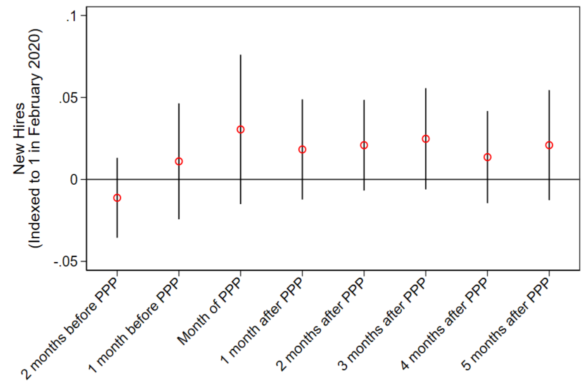
[47] I also test for differences between tradable and non-tradable industries, but do not find significant differences. Results available upon request.

[48] Appendix Figure A.13 plots the number of total hires, new hires, and rehires as a percentage of the firm's employment in February 2020, split by PPP and non-PPP firms.

(a) Total Number of Hires



(b) Number of New Hires



(c) Number of Rehires

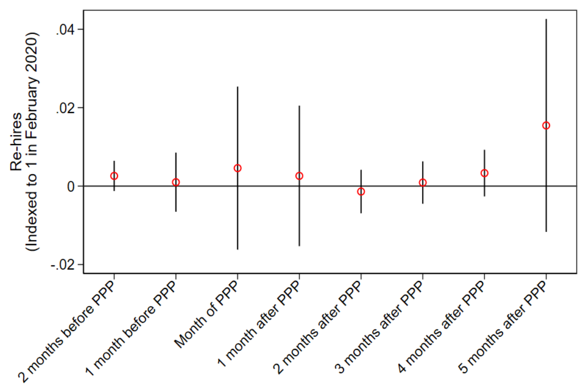


Figure 5: Effect of PPP on Employee Turnover

Notes: These figures show the results from the difference-in-differences specification in equation (2). I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The dependent variables are measured as fraction of the firm's base employment (average February 2019-February 2020). The black lines represent 90% confidence intervals.

of employment), two-digit industry fixed effects, size fixed effects, and state fixed effects. In the monthly regressions, I also include a month fixed effect to account for the fact that different firms applied in different months.

I find that there is no effect of the PPP on total or new hires in any month following application, or in the entire post period. These results are shown in the top and middle panels of Table 3. In fact, the point estimates on total hires and new hires are almost all negative. Moreover, the only significant effects on rehiring are a negative effect two months after application. These point estimates suggest that PPP firms re-hired .59% fewer employees (as a percentage of their February employment) in the entire post-PPP period and did no more hiring overall.

Table 3: Cross-sectional Regressions on Employee Turnover

	Month of PPP Application	One Month After PPP Application	Two Months After PPP Application	Entire post-PPP period
		# of Total Hires		
Applied for PPP Loan	-0.6209	0.1600	-1.2754	0.0281
	(4.6565)	(4.4492)	(2.2023)	(0.0894)
N	355	348	344	334
R^2	0.024	0.047	0.120	0.053
		# of New Hires		
Applied for PPP Loan	-1.3991	-0.0057	-0.4816	-0.4580
	(3.5959)	(4.2099)	(2.1833)	(1.6480)
N	355	348	344	338
R^2	0.031	0.033	0.115	0.265
		# of Re-hires		
Applied for PPP Loan	0.7782	0.1658	-0.7938***	-0.5918
	(1.3467)	(1.4251)	(0.2819)	(0.5529)
N	355	348	344	338
R^2	0.055	0.125	0.172	0.351
Industry FE?	Y	Y	Y	Y
State FE?	Y	Y	Y	Y
Employment Size Controls?	Y	Y	Y	Y
Date Controls?	Y	Y	Y	N
Control for lagged employment growth?	Y	Y	Y	Y

Notes: This table shows the results of cross-sectional regressions of percentage of total, new and rehires on a PPP indicator. Coefficients are shown as percentages of the firm's employment in February 2020. The regressions also control for one-month lagged monthly employment growth, industry fixed effects, state fixed effects, employment size fixed effects and month fixed effects. The first column shows results for the month of PPP application (April for controls firms, April-June for treated firms). The second column shows results for one month following PPP application (May for control firms, May-July for treated firms). The third column shows results for two months following PPP application (June for control firms, June-August for treated firms). The fourth column is for the entire post-PPP period from April-September. Standard errors in parentheses. $p < 0.10$, $p < 0.05$, $p < .01$.

I also find that there is no effect of PPP on the intensive margin of rehiring, hiring, or reducing employment. That is, PPP firms are not more likely to rehire former employees, hire anyone, or reduce employment in general. To show this, I run a logistic regression

Table 4: Cross-sectional Regressions on the Effect of PPP on the Likelihood of Rehiring, Hiring, and Reducing Employment

	Month of PPP Application	One Month After PPP Application	Two Months After PPP Application	Entire post-PPP period
		Rehired a Former Employee		
Applied for PPP Loan	1.6672	0.9246	0.0658***	0.5565
	(1.1116)	(0.4916)	(0.0627)	(0.2267)
N	309	321	273	326
pseudo R^2	0.388	0.371	0.584	0.403
		Hired Any Employee		
Applied for PPP Loan	1.0153	1.5492	1.0583	1.1839
	(0.4130)	(0.6224)	(0.4703)	(0.3772)
N	337	329	330	328
pseudo R^2	0.237	0.307	0.400	0.359
		Reduced Employment		
Applied for PPP Loan	1.0653	0.6291	1.5359	0.9230
	(0.4091)	(0.2612)	(0.7653)	(0.2705)
N	342	336	300	325
pseudo R^2	0.252	0.178	0.179	0.204
Industry FE?	Y	Y	Y	Y
State FE?	Y	Y	Y	Y
Employment Size Controls?	Y	Y	Y	Y
Date Controls?	Y	Y	Y	N
Control for lagged employment growth?	Y	Y	Y	Y

Notes: This table shows the results of a logistic regression on the extensive margin of employment outcomes. The dependent variable in the to panel is an indicator equal to one if the firm rehired former employees in a given month and zero otherwise. The dependent variable in the second panel is an indicator equal to one if the firm hired any employees in a given month and zero otherwise. The dependent variable in the bottom panel is an indicator equal to one if the firm reduced employment from February 2020 to September 2020 and zero otherwise. The regressions also control for one-month lagged employment growth, industry fixed effects, state fixed effects, employment size, and month fixed effects. Odds-ratios are reported. Standard errors in parentheses.

of the extensive margin of rehiring, hiring and reducing employment on the same set of controls. I set an indicator equal to one if the firm did the respective adjustment in a given month and zero otherwise. The results are shown in Table 4, with the coefficients reported as odds ratios. Beginning with rehiring in the top panel, the month of PPP application, treated firms are not significantly more likely to rehire former employees, though the odds ratio is greater than one, implying a positive, but statistically insignificant effect of the PPP. One month after PPP, there is also no significant effect and the odds ratio is nearly one. Two months after PPP application, treated firms are less likely to rehire former employees; the odds ratio is meaningful in magnitude and highly significant.[49] In the entire post-PPP period, treated firms are less likely to rehire former employees, but not significantly so.

Turning to hiring, reported in in the second panel of Table 4, PPP firms are not significantly more likely to hire in the months following PPP. Though the odds ratios are larger than one, they are not significantly different from zero.

Lastly, PPP firms were not more (or less) likely to reduce employment in the months following PPP. The odds ratio for the entire post-PPP period is less than one, indicating that they were slightly less likely, on average, to reduce employment than non-PPP firms, However the estimate is not significantly different from zero.

The results imply that the positive effect on employment observed in the difference-in-differences results (Figure 3) is largely driven by PPP firms laying off fewer workers than controls, rather than hiring more employees. This indicates that the program worked as intended, by preserving employer-employee matches. Moreover, I find no evidence that PPP firms were more likely to bring back previously laid off employees than the controls.

Splitting up the firms by business-type, as in the previous section, I find no significant effects on hiring or rehiring for any type of business on a monthly basis (Appendix Figures A.19-A.21). The average estimates for the entire post-PPP period suggest that some non-essential businesses and low-remote businesses hired more workers in the months following PPP (Appendix Figure A.22). This is consistent with the narrative that these firms did not have positive employment effects from the PPP. Thus they likely laid off more workers early on and needed to do more hiring than the businesses for which the PPP helped to prevent layoffs. Overall, these results confirm that fewer layoffs drive the positive employment results across the board.

[49] This may be because they have already done so, given the odds-ratio greater than one in the month of application.

6 Aggregate Effects

How effective was the PPP, on a per dollar basis? In this section, I calculate the aggregate effect of the PPP through September of 2020 based on my estimates in the previous section. I estimate that the program maintained employment at an expense of between $88,000-157,000 per job as of September 2020. The effect is estimated using:

$$\text{Total payroll effect}_t = \beta_t \times \gamma \times N \tag{4}$$

where β_t is my average effect of treatment on the treated (ATT) effect from equation (3), γ is the percentage of the eligible population that applied, and N is the number of employees at eligible firms. For this section, I use unreported results in which the regressions are weighted by the firm's size employment share in the BLS. In the primary analysis, regressions are unweighted as I am intending to estimate causal effects. However, when estimating population descriptive statistics, weighting to correct for representativeness is needed (see Solon et al. (2015)).

First, I limit the analysis to the local effect in my sample. In this case, $N = 5,525$ (as of January 2020) and $\gamma = .57$. Using my primary estimate of β_t=8.0% for the entire post-PPP period from April-September, this implies 253 jobs preserved in the sample through September 2020. Noting that the PPP disbursed an estimated $39.6 million to firms in my sample, each job costs $157,000.[50] The results using the estimates from my main specification are summarized in Table 5, in the "In-sample - main results" column.

Next, I assume that the estimate applies to all small firms (<300 employees) to be consistent with the firms represented in my sample.[51] This results in an estimate of N of 57 million.[52] I use an estimated γ of 75%, consistent with my calculation in Section 3.

[50] The estimate of funds distributed to firms in my sample is based on their total wage bill. The PPP allows firms to apply for 2.5 times their average monthly wage bill, based on the average in the year prior to the start of the pandemic.

[51] The largest firm in my sample, besides the firm with 500+ employees that was excluded from the analysis, has just over 300 employees.

[52] The Statistics of U.S. Businesses (SUSB) estimates that there are approximately 55 million employees at firms with less than 300 employees as of 2017. Following Autor et al. (2022), I scale up this number by an additional 3 percent, corresponding to the growth in private payrolls between December 2017 (the last year of the SUSB data) and December 2019 in the BLS's Current Employment Statistics data.

Using again my primary (weighted) estimate of β_t=8.0% for the entire post-PPP period from April-September, this implies 3.43 million jobs preserved through September 2020. In aggregate the PPP disbursed $465 billion to firm with fewer than 300 employees.[53] This implies a cost-per-job of $135,000 as of September 2020. The results for this sub-sample are shown in the second column of Table 5.

Table 5: Estimate of aggregate costs

	In-sample - main results	Aggregate - Firms with < 300 Employees	In-sample - most remote-capable industries
β_t	8.0%	8.0%	13.1
γ	57%	75%	58%
N	5,525	58 million	3,533
Total jobs preserved	253	3.43 million	268
Total funds disbursed	$39.6 million	$465 billion	$23.7 million
Cost per job			
April-September 2020	$157,000	$135,000	$88,000
With UI Benefits	$143,000	$121,000	$74,000
Per year	$313,000	$271,000	$177,000
% of total funds that goes to wages of preserved jobs	12.6%	19.3%	19.4%

Notes: This table shows the estimates for the aggregate cost and cost-per-job of the PPP, quantified in equation (4). Cost per job April-September is the total funds disbursed divided by the total jobs preserved. Cost per job April-September 2020 (accounting for UI benefits) is the total cost per-job less the estimated amount than an individual would have received in UI benefits from both their state and the federal government, had they remained unemployed from April-September. This estimate is based on an average of state UI benefits corresponding to the sample in each column and the $600 per week provided by the Federal Government via the CARES Act through July 25, 2020. Cost-per job year is a scaled up estimate of Cost per-job April-September 2020, assuming the cost is a monthly flow. Percentage of total funds that goes to wages of preserved jobs is the number of jobs preserved multiplied by the average dollar value of wages a worker in each sample would have earned from April-September 2020, divided by total funds disbursed.

[53] To calculate this, I use the microdata provided by the Treasury. I calculate the total value of loans disbursed for each firm that indicated that the number of jobs retained was less than 300. Firms with jobs retained of less than 300 represent 99.8% of all firms in the data.

Thus, my in-sample estimates imply a slightly higher estimate than in the aggregate calculation. All in all, these estimates correspond to a cost of $271,000-313,000 per job-year, assuming the cost is a monthly flow. Further, the estimates suggests that only 10-20% of the total funds disbursed went to wages at preserved jobs.[54]

Recall however that there is significant heterogeneity in treatment effects; only firms that were less affected by the pandemic, namely firms with remote capable workers, truly benefited from the programs. Aggregate estimates for this subsample on which the program was most effective provide an upper bound for the true effect of the program.

Consider the aggregate estimate for only the firms that benefited the most in my sample, those in industries with many employees who can work remotely, where $\beta = 13.1$ in the main specification (when weighted by firm size). In my sample, these firms represent $N = 3,533$ and had an estimated total PPP loan value of $23.7 million. This implies a job-year cost of $88,000 as of September 2020 for the firms that benefited most from the program. In this case, approximately 19% of the total funds disbursed went directly to wages for preserved jobs. The results for this subsample are shown in the final column of Table 5,

These estimates of course do not consider the cost of the alternative, had these employees been laid off. As an extreme example, suppose that each employee had been unable to find another job and instead received unemployment insurance (UI) for the six months from April to September.[55] Such an individual in my sample would have received approximately $4,284 over this period in state UI and approximately $9,600 in federal UI, for a total of $13,884.[56] Hence, even considering the cost of UI, the per-person estimate is reduced only by approximately $14,000. This is likely an upper-bound given most people did not remain continuously unemployed for six months.

[54]To calculate this, I multiply the number of jobs preserved by the average wage that would have been earned from April-September 2020 by employees in each sample, then divide by total funds disbursed.

[55]This is an extreme, but still reasonable assumption given the high rate of unemployment over this time period. The BLS estimates that the majority of workers who lost their jobs during the pandemic had returned to work as of September 2020, though the labor force had also shrunk.

[56]The state UI estimate is based on the average amount per week given in the state in which most of the employees in my sample are located, and assuming they would be eligible for benefits for all six months of unemployment. The federal estimate is based on the additional $600 per week that was specified in CARES act from 3/29/2020-7/25/2020.

My estimates imply a smaller per job-year cost than found in Autor et al. (2022)'s, where the preferred estimate is $224,000 per job as of the end of May 2020.[57] In comparison, Brown and Earle (2017) estimate that typical SBA loans cost only $21,580 to $25,450 per job per year. Feyrer and Sacerdote (2011) estimate a cost of $170,000 per job-year due to the American Recovery and Reinvestment Act of 2009. Thus the PPP appears to be more expensive than typical SBA loans, but similar to measures of other relief programs, though the PPP intends to *preserve* rather than create jobs. Moreover, the jobs are quite expensive even when focusing on the subsamples that the program appears to have targeted most effectively. While my estimates imply quite a high cost per job of the PPP, even for the most affected firms, it would be preliminary to offer a complete welfare analysis of the PPP from this small sample. The overall effects of the program must be considered along with other aid provided as part of the CARES act, such as the individual stimulus checks and increased unemployment benefits as well as the loss of healthcare benefits associated with losing a job. It would also be remiss to ignore the potential longer-run effects that the program might have on job preservation and business survival. Future work should seek to address this.

7 Discussion and Conclusion

Using administrative data from a payroll processor, I find that the PPP was largely successful in increasing employment at treated firms. Average employment at treated firms increased by 14% the month after application and 7.5% cumulatively over the five month period following PPP. On the other hand, there is no significant effect on wage bills. My results suggest a high per-job cost of the PPP, as much as $157,000 as of September 2020. At best, less than 20% of all funds disbursed went to wages at preserved jobs.

I also find that there is substantial heterogeneity in the effect of the PPP across types of firms. The employment preservation occurs almost entirely in industries that have a low percentage of hourly workers and more employees who can work remotely. The effect

[57]This implies a per job-year cost of $1,2942,222.

is also larger in essential businesses. This implies that the PPP does not act alone, but interacts with local economic conditions and restrictions in business activities.

This paper also provides insights into firm take-up of PPP loans. Take-up is estimated at 75% nationally, thus a non-significant number of eligible firms opted not to take these very financially attractive loans. My results suggest that smaller firms with lower average salaries are less likely to take-up the loans. This suggests that the least organizationally complex firms elected not to take the loans.

The results come with several caveats. First, the sample is not geographically diverse, with most firms being in one state. Moreover, this state was not particularly hard hit by the pandemic in the first wave in April and May. Second, the sample is small relative to all firms in the U.S. However, the sample is quite comparable to all small businesses in the U.S. in terms of its distribution by industries and average size.

There are several avenues for future work. First, after the end of my sample period, the PPP was adjusted in several ways to target smaller, more affected businesses. My results suggest that exactly these firms were less likely to take the loans. It remains an open question if those adjustments, like increasing assurances of the loans being forgivable and increasing the range of expenditures that the loans could be used for, alleviated this. Second, we should seek to understand why some firms did not apply. Though my results suggest that smaller, less complex firms were less likely to apply, more detailed data, including surveys of small business owners would help to understand better why this is the case. Lastly, examining the long-run outcomes of the firms that received the PPP versus those that did not will speak to the effectiveness and overall welfare effect of the program.

References

Altonji, Joseph, Zara Contractor, Lucas Finamor, Ryan Haygood, Ilse Lindenlaub, Costas Meghir, Cormac O'Dea, Dana Scott, Liana Wang, and Ebonya Washington, "Employment Effects of Unemployment Insurance Generosity During the Pandemic," Working Paper, Tobin Center for Economic Policy at Yale University 2020.

Autor, David, David Cho, Leland D. Crane, Mita Goldar, Byron Lutz, Joshua Montes, William B. Peterman, David Ratner, Daniel Villar, and Ahu Yildirmaz, "An evaluation of the Paycheck Protection Program using administrative payroll microdata," Journal of Public Economics, 2022, 211, 104664.

Balyuk, Tetyana, Nagpurnanand R Prabhala, and Manju Puri, "Indirect Costs of Government Aid and Intermediary Supply Effects: Lessons From the Paycheck Protection Program," Working Paper 28114, National Bureau of Economic Research November 2020.

Barrios, John Manuel, Michael Minnis, William C. Minnis, and Joost Sijthoff, "Assessing the Payroll Protection Program: A Framework and Preliminary Results (May 13, 2020)," Working Paper 2020.

Bartik, Alexander W., Marianne Bertrand, Feng Lin, Jesse Rothstein, and Matthew Unrath, "Measuring the Labor Market at the Onset of the COVID-19 Crisis," Brookings Papers on Economic Activity, BPEA Conference Drafts, 2020.

Bartik, Alexander W, Marianne Bertrand, Zoë B Cullen, Edward L Glaeser, Michael Luca, and Christopher T Stanton, "How Are Small Businesses Adjusting to COVID-19? Early Evidence from a Survey," Working Paper 26989, National Bureau of Economic Research April 2020.

_ , Zoe B Cullen, Edward L Glaeser, Michael Luca, Christopher T Stanton, and Adi Sunderam, "The Targeting and Impact of Paycheck Protection Program Loans to Small Businesses," Working Paper 27623, National Bureau of Economic Research July 2020.

Ben-David, Itzhak, Mark J Johnson, and René M Stulz, "Why Did Small Business Fintech Lending Dry Up During March 2020?," Working Paper 29205, National Bureau of Economic Research September 2021.

BLS, https://www.bls.gov/opub/reports/minimum-wage/2015/home.htm 2016. Accessed: 2021-08-31.

_, "Employment by major industry sector," https://www.bls.gov/emp/tables/employment-by-major-industry-sector.htm 2020. Accessed: 2021-08-31.

_, "Occupational Employment and Wages Summary," https://www.bls.gov/news.release/ocwage.nr0.htm#modal-box 2020. Accessed: 2021-08-31.

Brewer, William L., Douglas R. Brown, and S. Graham Simmons III, "Government Provides Relief in the Paycheck Protection Program Flexibility Act," National Law Review, 2020, X (162).

Brown, J. David and John S. Earle, "Finance and Growth at the Firm Level: Evidence from SBA Loans," Journal of Finance, 2017, 72, 1039–1080.

Cajner, Tomaz, Leland D Crane, Ryan A Decker, John Grigsby, Adrian Hamins-Puertolas, Erik Hurst, Christopher Kurz, and Ahu Yildirmaz, "The U.S. Labor Market during the Beginning of the Pandemic Recession," Working Paper 27159, National Bureau of Economic Research May 2020.

Chetty, Raj, John N Friedman, Nathaniel Hendren, Michael Stepner, and The Opportunity Insights Team, "How Did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real-Time Economic Tracker Based on Private Sector Data," Working Paper 27431, National Bureau of Economic Research June 2020.

Denes, Matthew, Spyridon Lagaras, and Margarita Tsoutsoura, "First Come, First Served: The Timing of Government Support and Its Impact on Firms," Working Paper 2021.

Doniger, Cynthia L. and Benjamin S. Kay, "Ten Days Late and Billions of Dollars Short: The Employment Effects of Delays in Paycheck Protection Program Financing," Finance and Economics Discussion Series 2021-003, Board of Governors of the Federal Reserve System (U.S.) January 2021.

Erel, Isil and Jack Liebersohn, "Does FinTech Substitute for Banks? Evidence from the Paycheck Protection Program," Journal of Financial Economics, Forthcoming.

Faulkender, Michael, Robert Jackman, and Stephen Miran, "The Job Preservation Effects of Paycheck Protection Program Loans," Working Paper January 2020.

Feyrer, James and Bruce Sacerdote, "Did the Stimulus Stimulate? Real Time Estimates of the Effects of the American Recovery and Reinvestment Act," Working Paper 16759, National Bureau of Economic Research February 2011.

Granja, João, Christos Makridis, Constantine Yannelis, and Eric Zwick, "Did the Paycheck Protection Program Hit the Target?," Journal of Financial Economics, Forthcoming.

Hare, Neil, "Loan Forgiveness Under The PPP And SBA EIDL Programs: 10 Things Small Businesses Need To Know," Forbes, 2020.

Hayashi, Yuka, "Demand for Small-Business Loans Cools," The Wall Street Journal, 2020.

Horan, Stephanie, "PPP Loan Acceptance Rates in the First and Second Rounds of Funding – 2020 Study," yahoo!, 2020.

Hubbard, Glenn and Michael R. Strain, "Has the Paycheck Protection Program Succeeded?," Conference Draft, Brookings Paper on Economic Activity Fall 2020.

Iacurci, Greg, "Small businesses expect to fail. Here's what will happen to their PPP loans.," CNBC, 2020.

Joaquim, Gustavo and Felipe Netto, "Bank incentives and the effect of the paycheck protection program," Working Papers 21-15, Boston, MA 2021.

Kimball, Spencer, "Congress approved $370 billion more in small business loans, but the money could run out in days — again," CNBC, 2020.

Landier, Augustin and David Thesmar, "Financial Contracting with Optimistic Entrepreneurs," The Review of Financial Studies, 2009, 22 (1), 117–150.

Leibovici, Fernando, Ana Maria Santacreu, and Matthew Famiglietti, "How the Impact of Social Distancing Ripples through the Economy," On The Economy Blog, Federal Reserve Bank of St. Louis, April 2020.

Lelarge, Claire, David Sraer, and David Thesmar, Entrepreneurship and Credit Constraints: Evidence from a French Loan Guarantee Program, University of Chicago Press, May

Li, Lei and Philip E. Strahan, "Who Supplies PPP Loans (and Does It Matter)? Banks, Relationships, and the COVID Crisis," Journal of Financial and Quantitative Analysis, 2021, 56 (7), 2411–2438.

Moore, Brett, "PPP Loans Sparking Disputes and Differing Results in Bankruptcy Proceedings," New York Law Journal, 2020.

Papanikolaou, Dimitris and Lawrence D W Schmidt, "Working Remotely and the Supply-Side Impact of COVID-19," The Review of Asset Pricing Studies, 11 2021, 12 (1), 53–111.

Petersen, Mitchell A. and Raghuram G. Rajan, "The Benefits of Lending Relationships: Evidence from Small Business Data," The Journal of Finance, 1994, 49 (1), 3–37.

SBA, "Paycheck Protection Program Report through June 30, 2020," https://www.sba.gov/document/report-paycheck-protection-program-report-through-june-30-2020 2020. Accessed: 2021-08-31.

_ , "First Draw PPP loan," https://www.sba.gov/funding-programs/loans/covid-19-relief-options/paycheck-protection-program/first-draw-ppp-loan#section-header-2 2021. Accessed: 2021-08-31.

_ , "Paycheck Protection Program," https://www.sba.gov/funding-programs/loans/covid-19-relief-options/paycheck-protection-program 2021. Accessed: 2021-08-31.

Solon, Gary, Steven J. Haider, and Jeffrey M. Wooldridge, “What Are We Weighting For?,” The Journal of Human Resources, 2015, 50 (2), 301–316.

A Appendix Figures and Tables

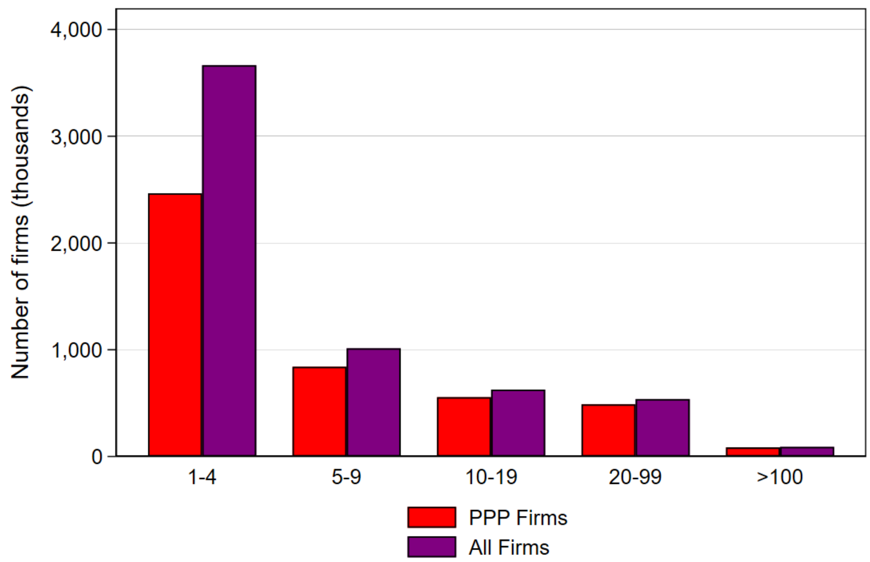
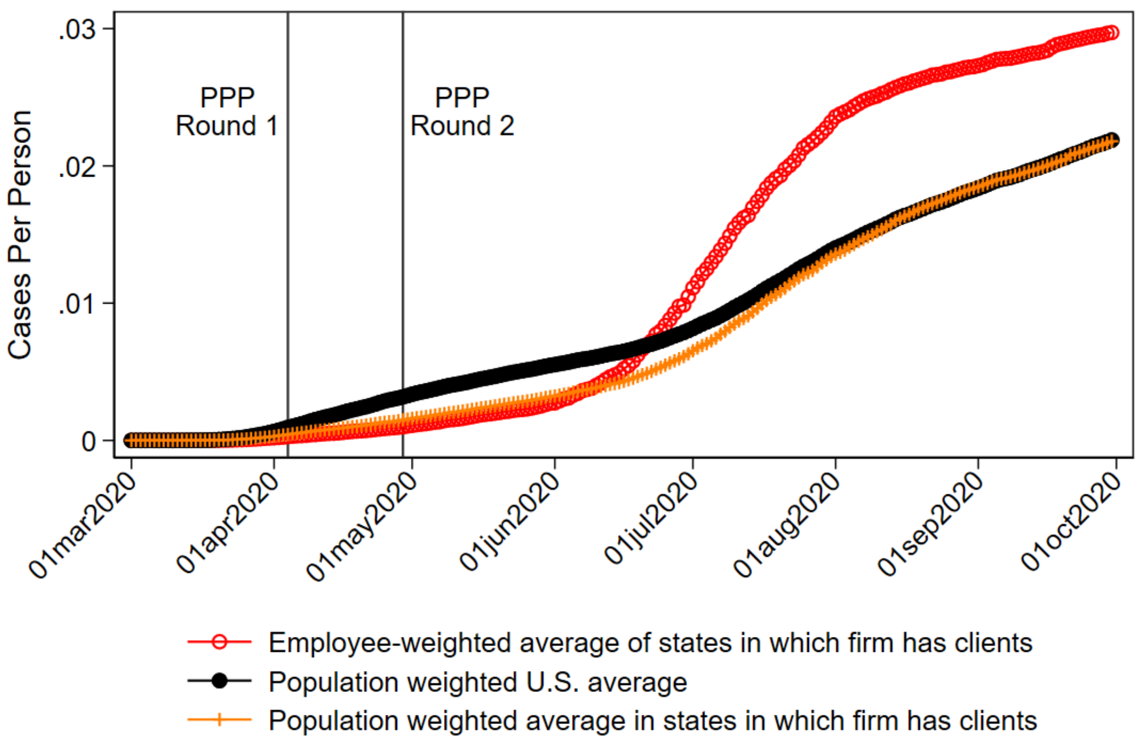


Figure A.1: Distribution of PPP Loan Recipients and All Firms, by Firm Employment

Notes: This figure shows the distribution of firms that received the PPP (red bars) and all firms in the U.S. (purple bars). Based on the author's calculations from the Treasury micro data on SBA loans and the 2016 Statistics of U.S. Businesses from the Census Bureau.

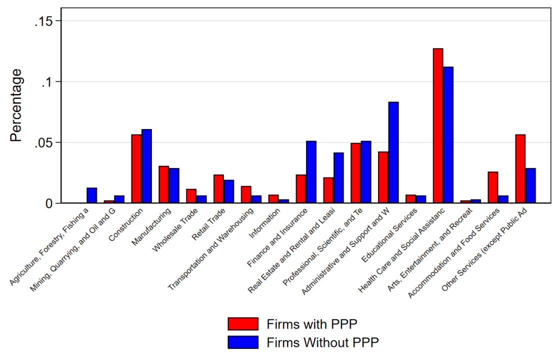
Figure A.2: Cases per Person, Sample versus U.S.



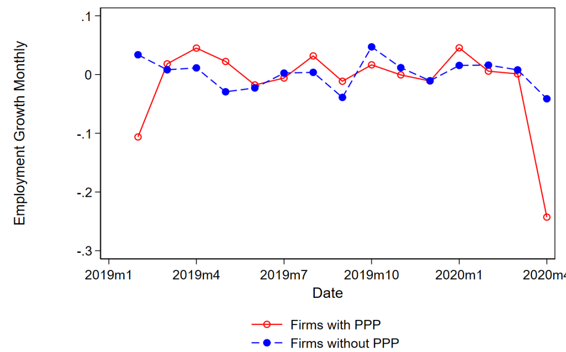
Notes: This figure shows the daily number of cases per person. First, I show the the average, weighted by number of employees in each state in the sample, for the states in which the firm has clients. Second, I show the population-weighted U.S. average in all 50 states. Lastly, I show the population-weighted average in the states in which the firm has clients. Case count data are from the Johns Hopkins Center for Systems Science and Engineering. Population data are from the Census.

Figure A.3: NAICS distribution and time series of selected variables, PPP versus non-PPP firms

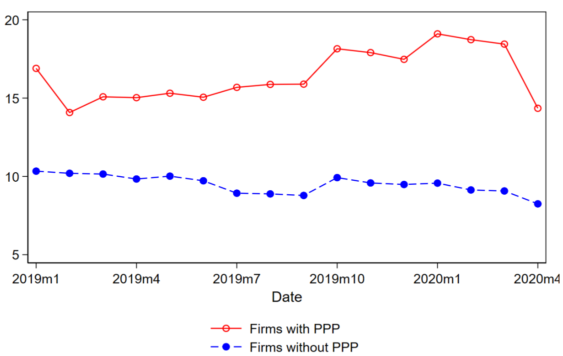
(a) NAICS Distribution



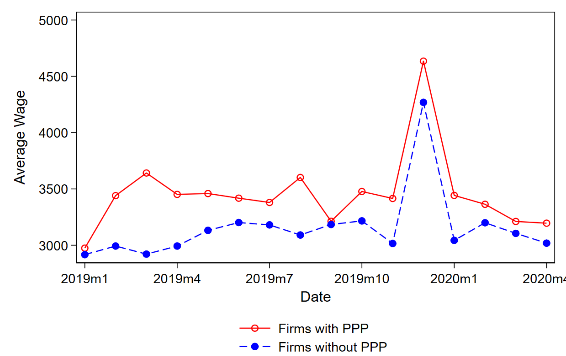
(b) Monthly employment growth



(c) Employment

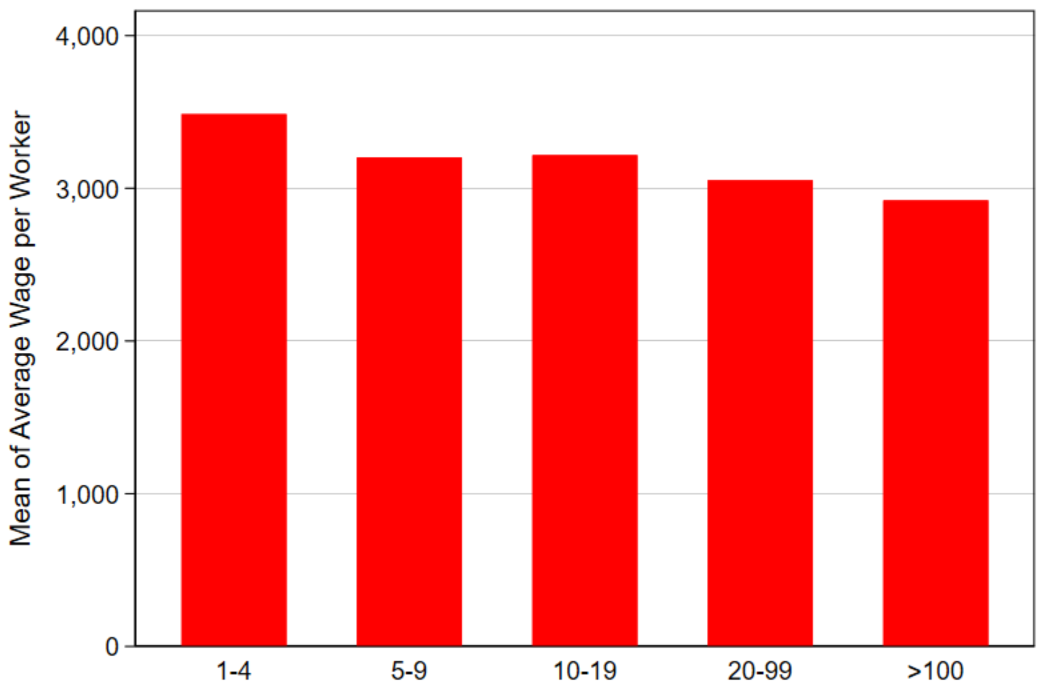


(d) Average wage per worker



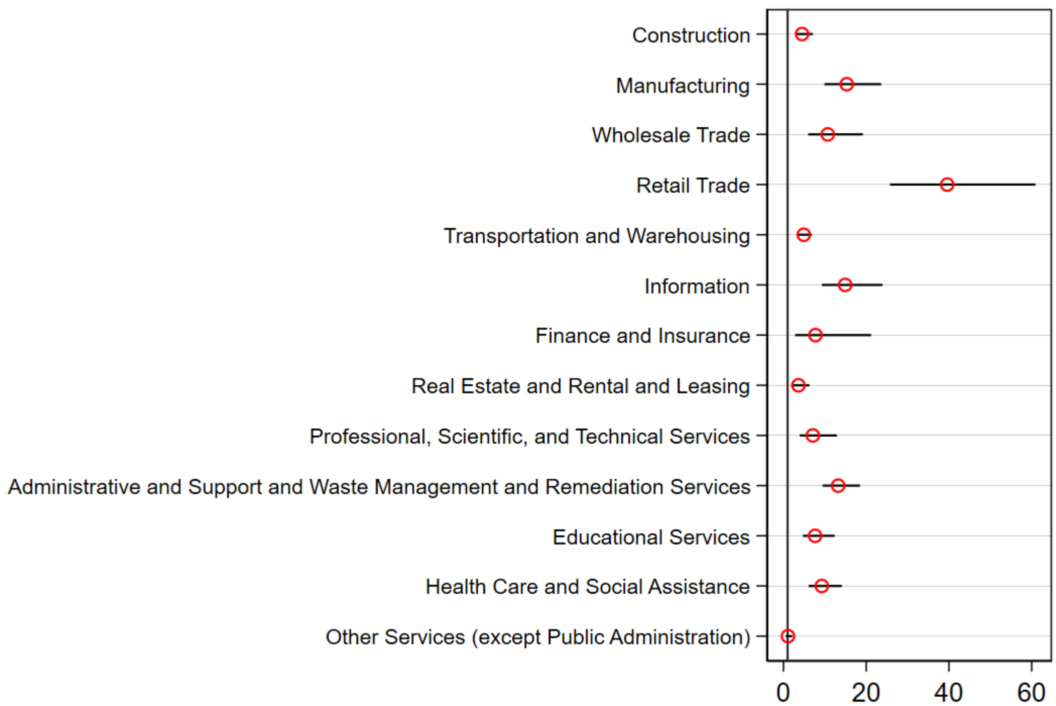
Notes: These figures show characteristics of each variable for the firms that received PPP (red) versus the firms that did not receive PPP (blue). The spike in average wage per worker at the end of 2019 is due to end-of-year/holiday bonus compensation.

Figure A.4: Average wage per worker, by firm size



Notes: This figure shows the average monthly wages per worker at firms within each bucket of number of employees, as of February 2020.

Figure A.5: Odds Ratios of NAICS fixed effects - Early versus Late Applicants



Notes: This figure shows the odds ratios for the NAICS industry fixed effects from the logistic regression of take-up on firm characteristics. This version compares late applicants versus early applicants. The reference category is NAICS 72, Food and Accommodation Services. Standard errors are clustered at the 2-digit NAICS level. The coefficients correspond to the final column in Table A.3.

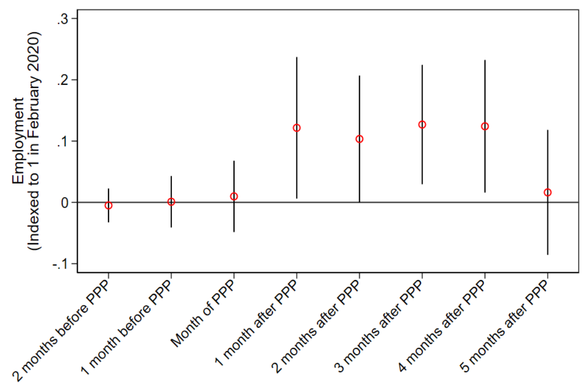
Table A.1: NAICS Distributions of Sample Firms

	Applied for PPP?			
	No	Yes	All	All Small Businesses < 500 Employees
NAICS sectors (%)				
Construction	11.7	11.3	11.4	11.7
Manufacturing	5.5	6.1	5.9	4.1
Wholesale Trade	1.2	2.3	1.9	4.9
Retail Trade	3.7	4.7	4.3	10.7
Transportation/Warehousing	1.2	2.8	2.1	3.1
Finance/Insurance	9.8	4.7	6.9	4.0
Real Estate	8.0	4.2	5.9	5.2
Professional/Scientific/Tech	9.8	9.9	9.9	13.5
Administrative/Support/Waste	16.0	8.5	11.7	5.8
Health Care	21.5	25.4	23.7	10.9
Accommodation/Food	1.2	5.2	3.5	9.0
Other Services	5.5	11.3	8.8	11.6
Observations	163	214	377	

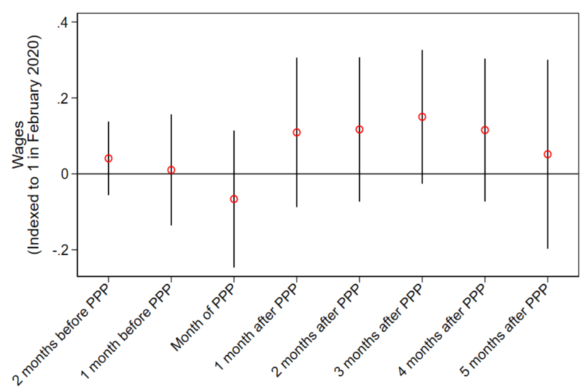
Notes: This table shows the 2-digit NAICS industry distribution for my sample firms. The first and second columns show the statistics for firms that did not apply and applied for PPP, respectively. The third column shows the full sample. One firm in the sample has over 500 employees and therefore is not eligible for the PPP, so it is dropped from all analyses. The final column shows a comparison for all firms with under 500 employees in U.S., which are calculated from the 2016 Statistics for U.S. Businesses (Census Bureau).

Figure A.6: Effect of PPP on Employment and Wage Bill, Balanced Panel

(a) Employment



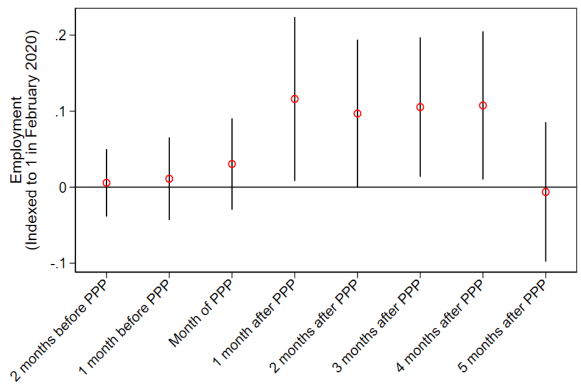
(b) Wage Bill



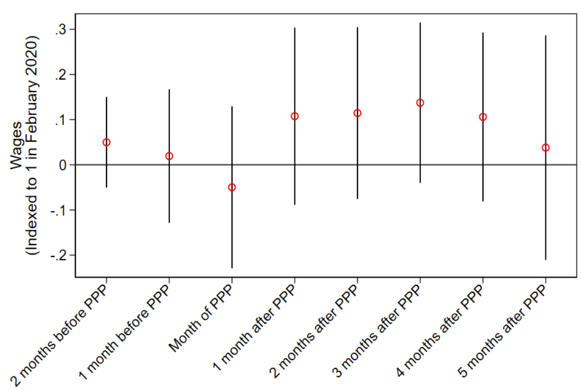
Notes: These figures show the results from the difference-in-differences specification in equation (2) for the balanced panel of firms. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The dependent variable is normalized to one in February 2020.

Figure A.7: Effect of PPP on Employment and Wage Bill, Controlling for Yearly Growth

(a) Employment

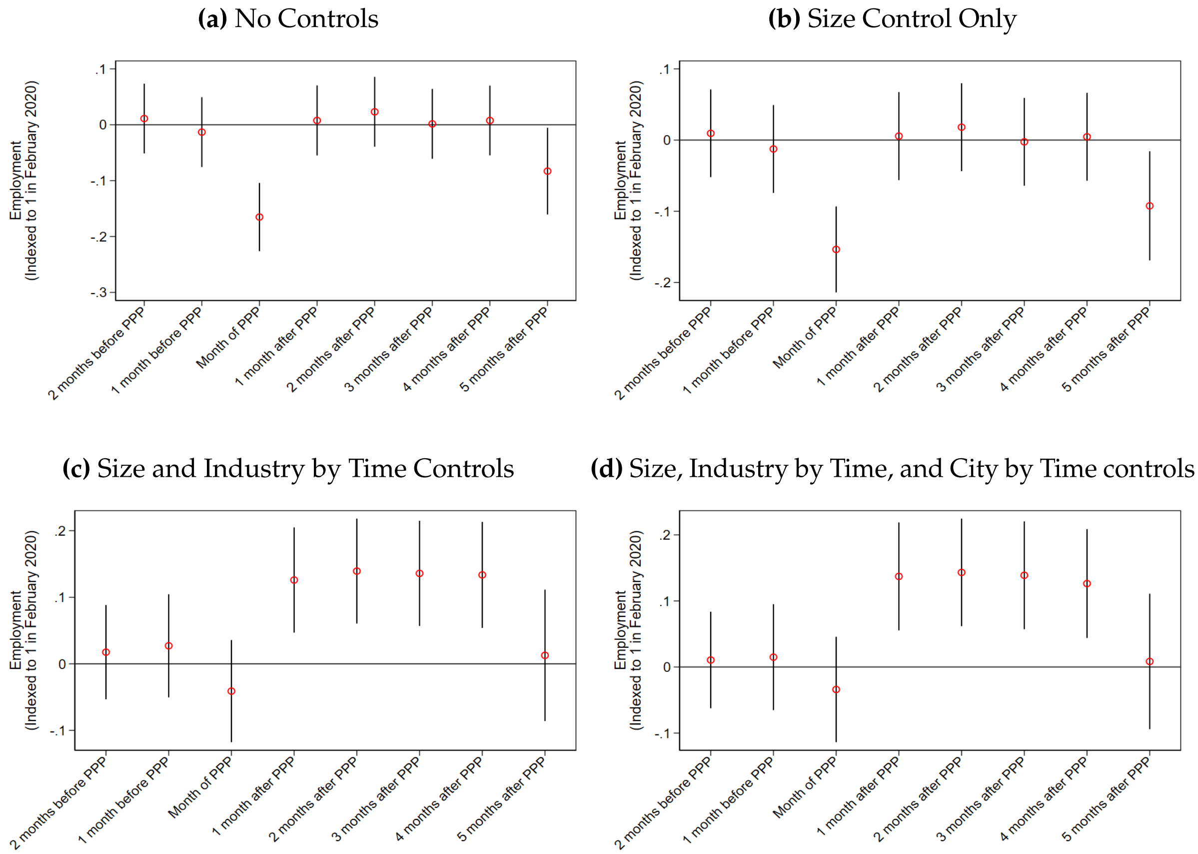


(b) Wage Bill



Notes: These figures show the results from the difference-in-differences specification in equation (2) controlling for the firm's yearly growth in the first two months of 2020. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The dependent variable is normalized to one in February 2020.

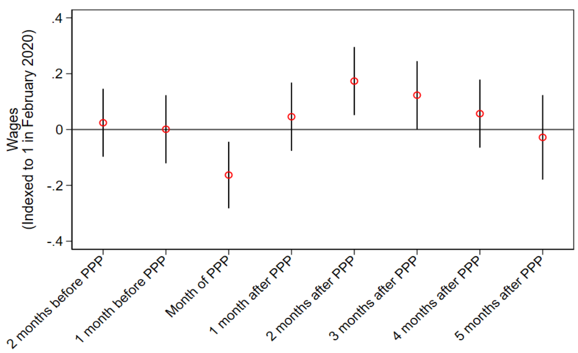
Figure A.8: Effect of PPP on Employment, Difference-in-Differences Results, Progressively Adding Controls



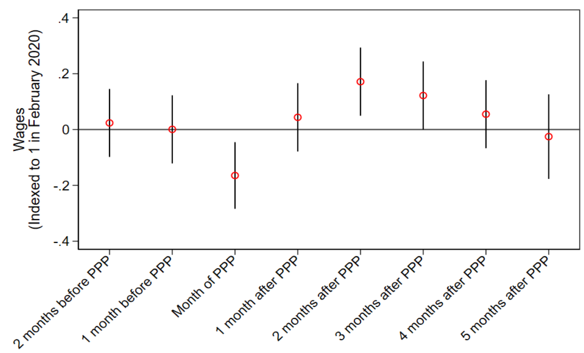
Notes: These figures show the results from the difference-in-differences specification in equation (2) with the controls progressively added. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. Panel a shows the regression with no controls. Panel b shows the regression with only the employment size fixed effects. Panel c shows the results with the size control and the industry by time fixed effects. Panel d shows the results with the full set of results, as shown in Figure 3. The dependent variable is normalized to one in February 2020.

Figure A.9: Effect of PPP on Wage Bill, Difference-in-Differences Results, Progressively Adding Controls

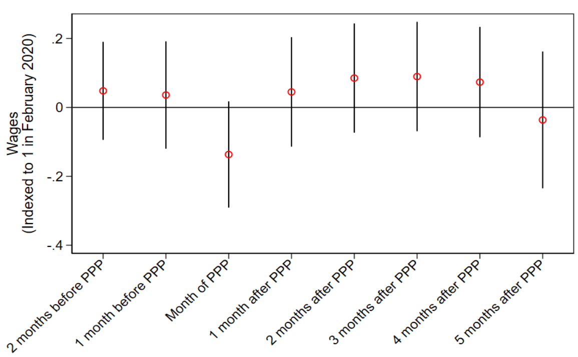
(a) No Controls



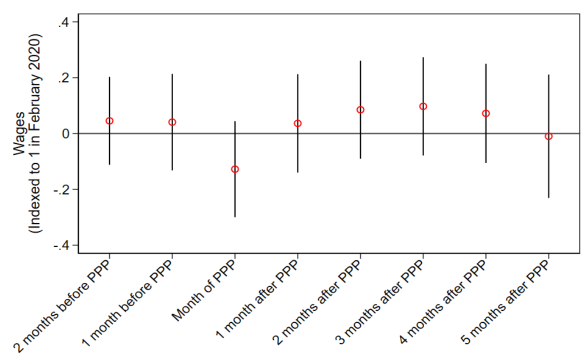
(b) Size Control Only



(c) Size and Industry by Time Controls

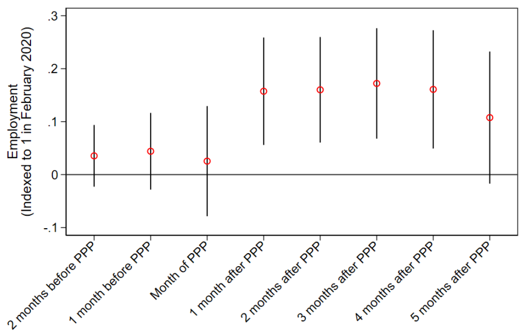


(d) Size, Industry by Time, and City by Time controls

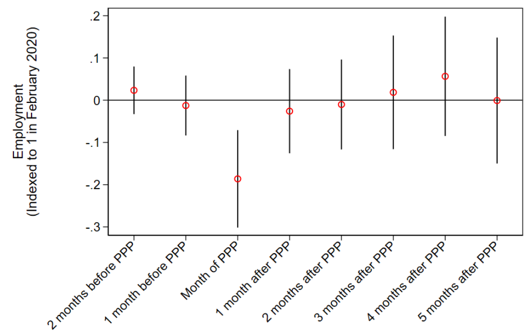


Notes: These figures show the results from the difference-in-differences specification in equation (2) with the controls progressively added. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. Panel a shows the regression with no controls. Panel b shows the regression with only the employment size fixed effects. Panel c shows the results with the size control and the industry by time fixed effects. Panel d shows the results with the full set of results, as shown in Figure 3. The dependent variable is normalized to one in February 2020.

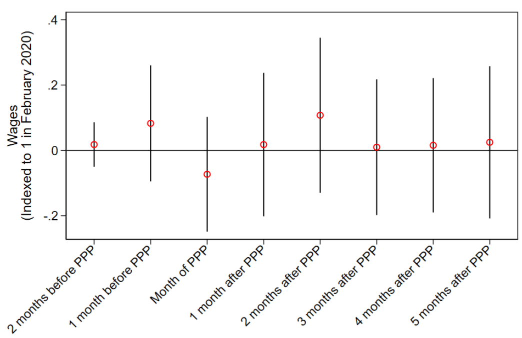
(a) Employment: bottom tercile of hourly workers



(b) Employment: top tercile of hourly workers



(c) Wage Bill: bottom tercile of hourly workers



(d) Wage Bill: top tercile of hourly workers

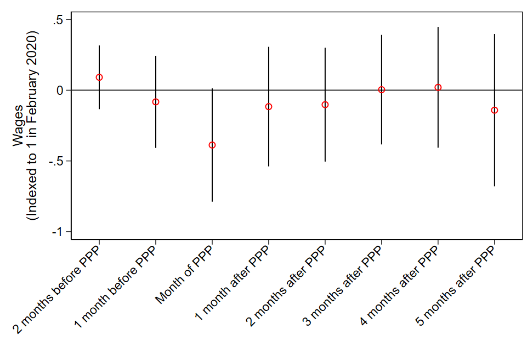
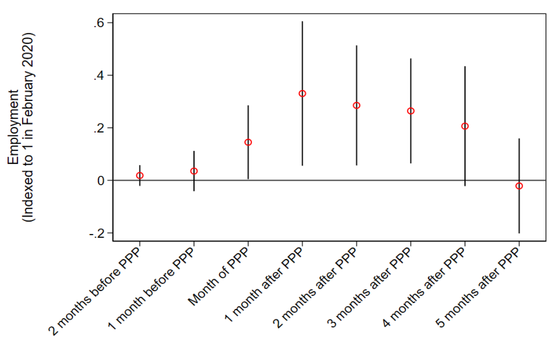


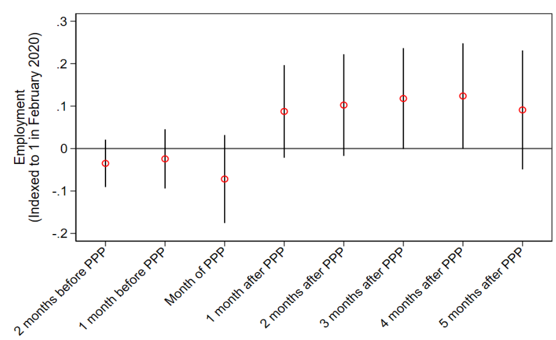
Figure A.10: Effect of PPP, by Prevalence of Hourly Workers in the Industry

Notes: These figures show the results from the difference-in-differences specification in equation (2). I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The figures on the left show the results for firms that are in industries which are in the lowest tercile of percentage of hourly workers (i.e. have few hourly workers). The figures on the right show the results for firms that are industries that are in the top tercile of hourly workers (i.e. have many hourly workers). The dependent variables are normalized to one in February 2020. Black lines represent 90% confidence intervals.

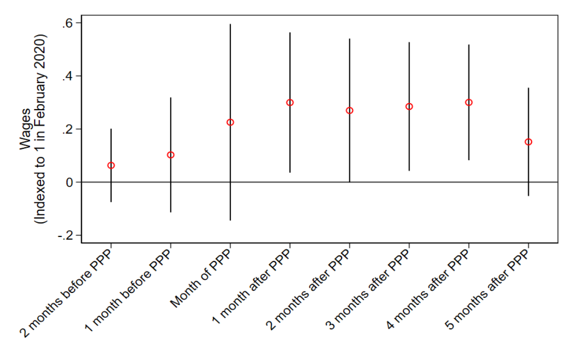
(a) Employment: top half of remote workers



(b) Employment: bottom half of remote workers



(c) Wage Bill: top half of remote workers



(d) Wage Bill: bottom half of remote workers

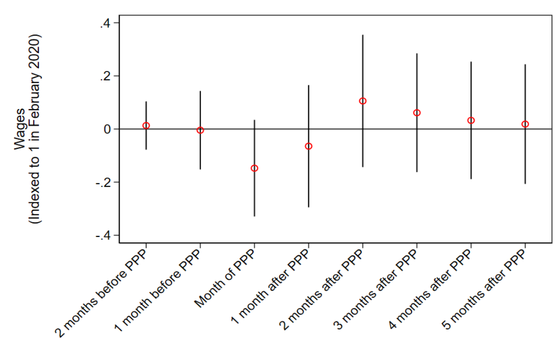
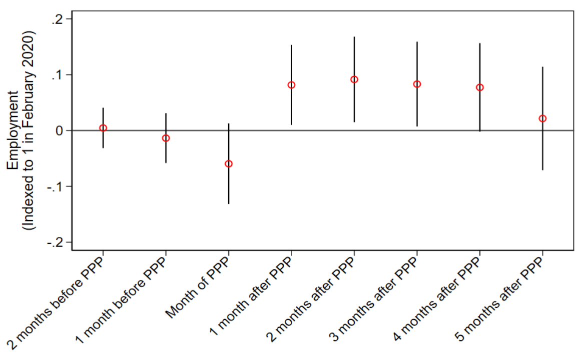


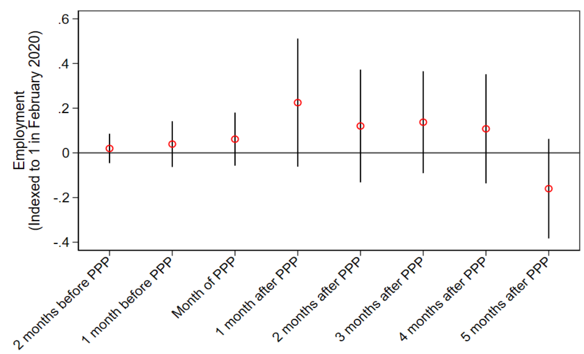
Figure A.11: Effect of PPP, by Percentage of Workers that can Work Remotely

Notes: These figures show the results from the difference-in-differences specification in equation (2). I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The figures on the left show the results for firms that are in industries which are in the top half of percentage of workers that can work remotely (i.e. have more remote-capable workers). The figures on the right show the results for firms that are industries that are in the bottom tercile of remote workers (i.e. have fewer remote-capable workers). The dependent variables are normalized to one in February 2020. Black lines represent 90% confidence intervals.

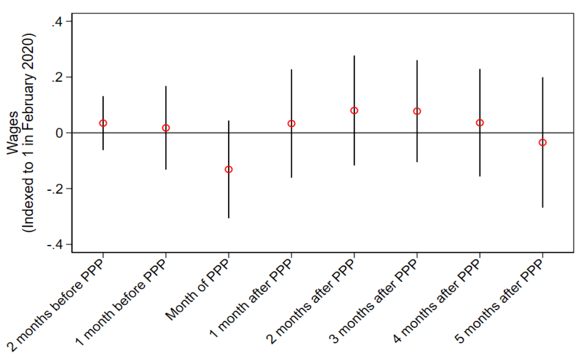
(a) Employment: non-essential businesses



(b) Employment: essential businesses



(c) Wage Bill: non-essential businesses



(d) Wage Bill: essential businesses

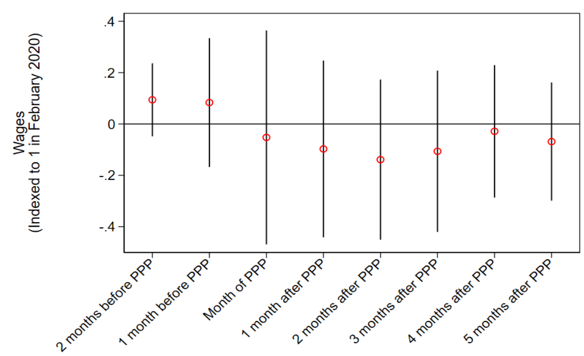
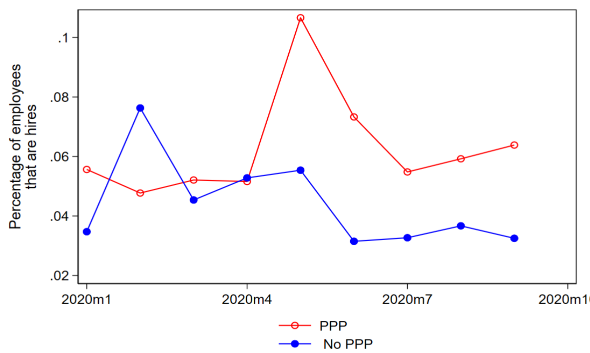


Figure A.12: Effect of PPP, by Essential versus Non-Essential Businesses

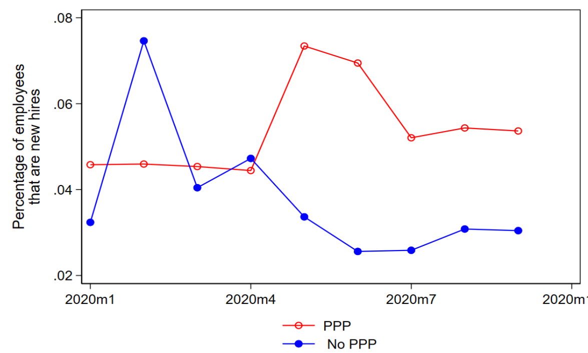
Notes: These figures show the results from the difference-in-differences specification in equation (2), I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The figures on the left show the results for firms that are considered essential businesses. The figures on the right show the results for non-essential businesses. The dependent variables are normalized to one in February 2020. Black lines represent 90% confidence intervals.

Figure A.13: Employee turnover, PPP versus non-PPP firms

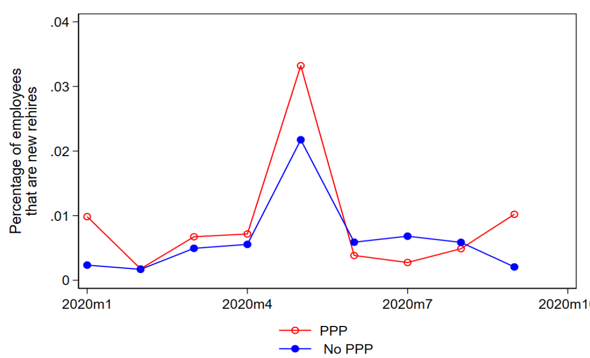
(a) Total hires



(b) New hires



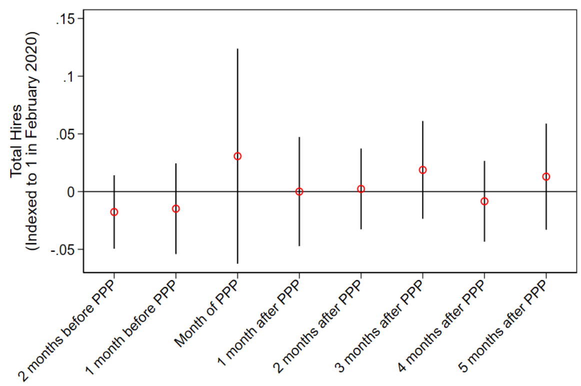
(c) Rehires



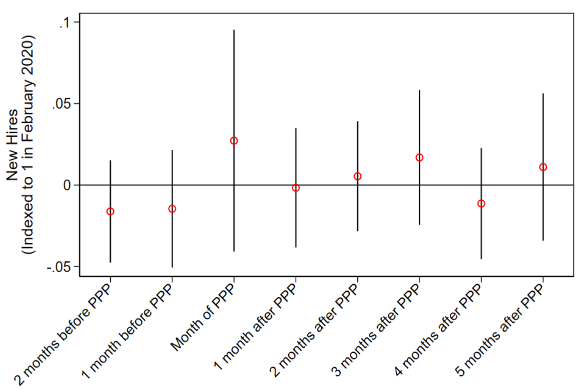
Notes: This figures shows the average number of total hires (a), new hires (b) and rehires (c), as a percentage of base employment in PPP versus non-PPP firms. Base employment is the average number of employees from February 2019-February 2020.

Figure A.14: Effect of PPP on Employee Turnover, Balanced Panel

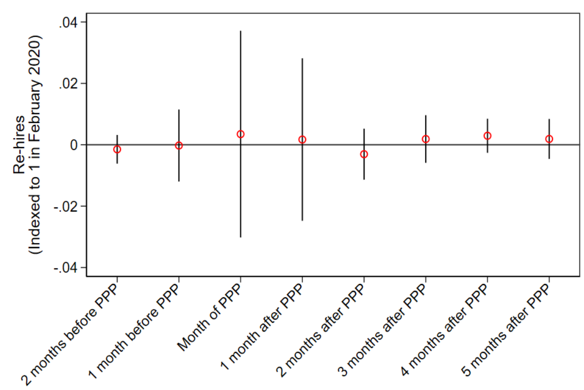
(a) # of Total Hires



(b) # of New Hires



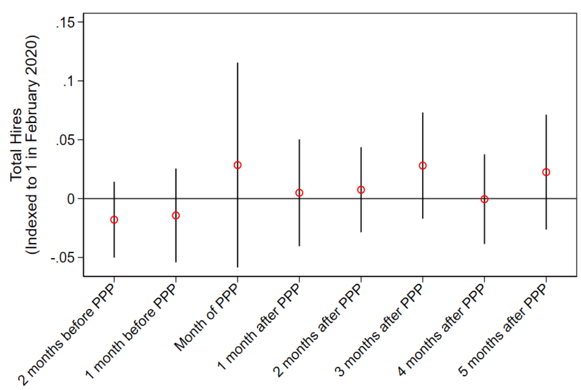
(c) # of Rehires



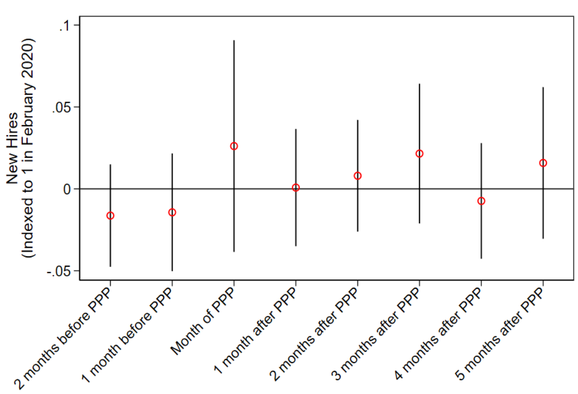
Notes: These figures show the results from the difference-in-differences specification in equation (2) for the balanced panel of firms. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The dependent variable is normalized to one in February 2020.

Figure A.15: Effect of PPP on Employee Turnover, Controlling for Yearly Growth

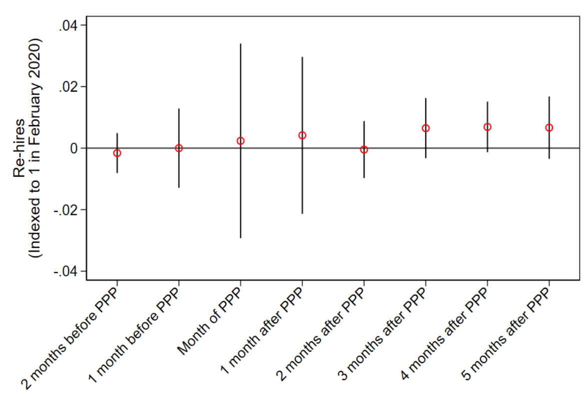
(a) # of Total Hires



(b) # of New Hires

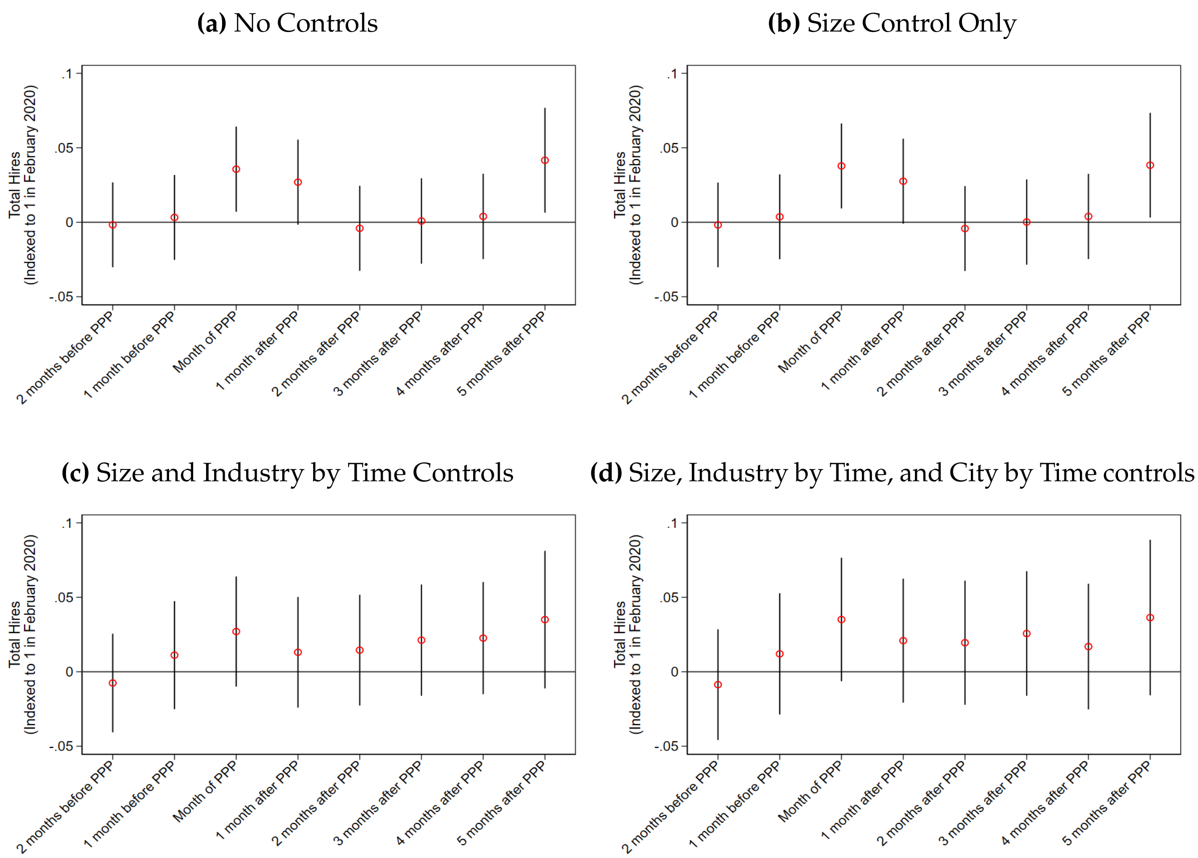


(c) # of Rehires



Notes: These figures show the results from the difference-in-differences specification in equation (2) controlling for the firm's yearly growth in the first two months of 2020. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The dependent variable is normalized to one in February 2020.

Figure A.16: Effect of PPP on Total Hires, Difference-in-Differences Results, Progressively Adding Controls



(a) No Controls

(b) Size Control Only

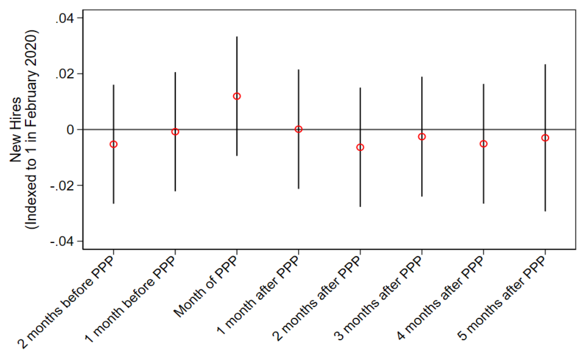
(c) Size and Industry by Time Controls

(d) Size, Industry by Time, and City by Time controls

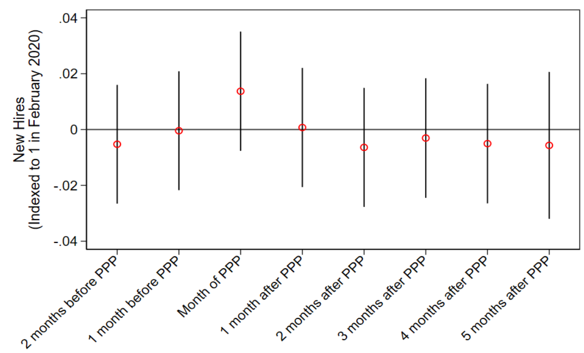
Notes: These figures show the results from the difference-in-differences specification in equation (2) with the controls progressively added. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. Panel a shows the regression with no controls. Panel b shows the regression with only the employment size fixed effects. Panel c shows the results with the size control and the industry by time fixed effects. Panel d shows the results with the full set of results, as shown in Figure 5. The dependent variable is normalized to one in February 2020.

Figure A.17: Effect of PPP on New Hires, Difference-in-Differences Results, Progressively Adding Controls

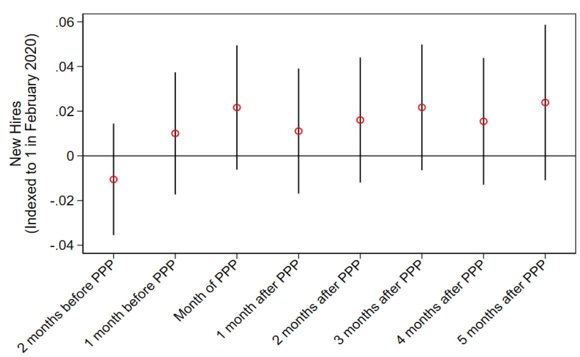
(a) No Controls



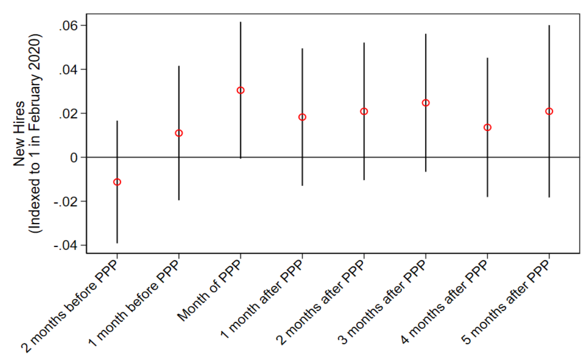
(b) Size Control Only



(c) Size and Industry by Time Controls



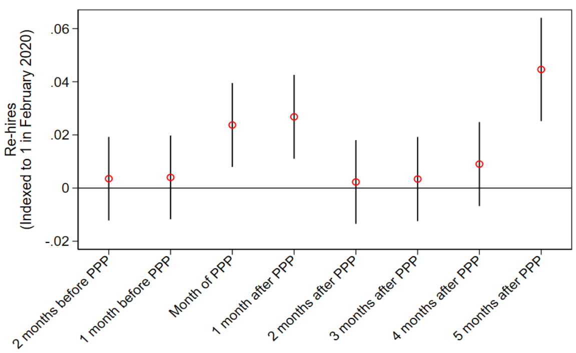
(d) Size, Industry by Time, and City by Time controls



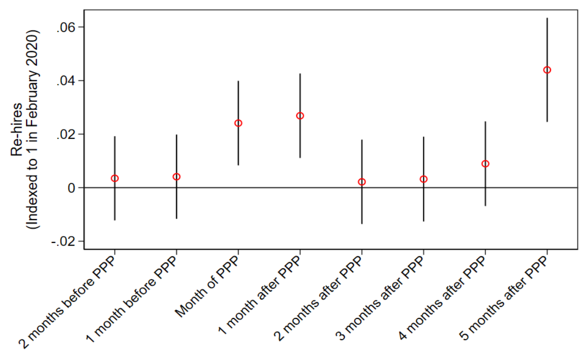
Notes: These figures show the results from the difference-in-differences specification in equation (2) with the controls progressively added. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. Panel a shows the regression with no controls. Panel b shows the regression with only the employment size fixed effects. Panel c shows the results with the size control and the industry by time fixed effects. Panel d shows the results with the full set of results, as shown in Figure 5. The dependent variable is normalized to one in February 2020.

Figure A.18: Effect of PPP on Re-hires, Difference-in-Differences Results, Progressively Adding Controls

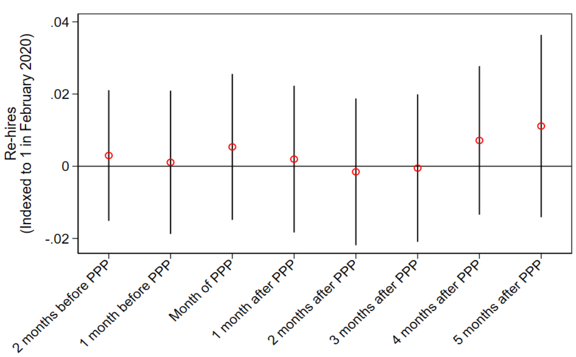
(a) No Controls



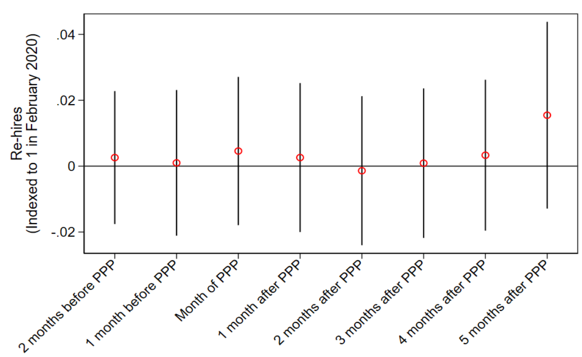
(b) Size Control Only



(c) Size and Industry by Time Controls



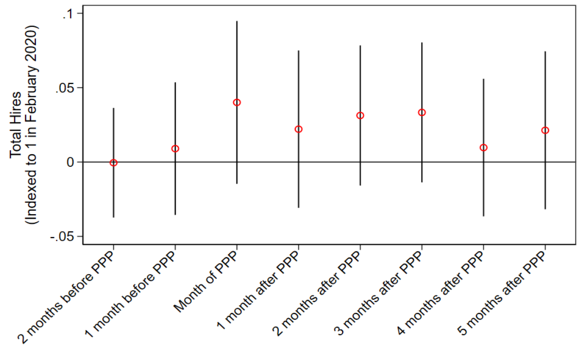
(d) Size, Industry by Time, and City by Time controls



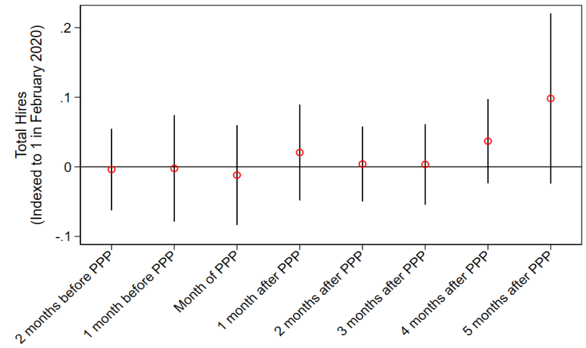
Notes: These figures show the results from the difference-in-differences specification in equation (2) with the controls progressively added. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. Panel a shows the regression with no controls. Panel b shows the regression with only the employment size fixed effects. Panel c shows the results with the size control and the industry by time fixed effects. Panel d shows the results with the full set of results, as shown in Figure 5. The dependent variable is normalized to one in February 2020.

Figure A.19: Effect of PPP on Employee Turnover, by Prevalence of Hourly Workers in the Industry

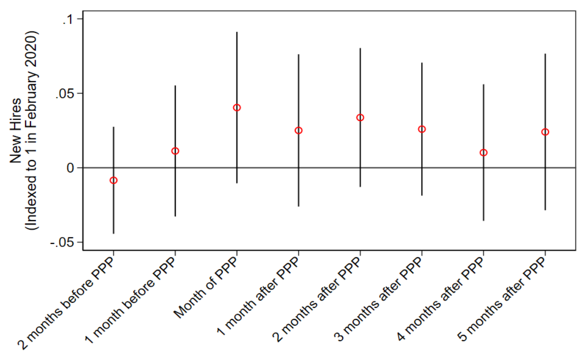
(a) Total Hires: bottom tercile of hourly workers



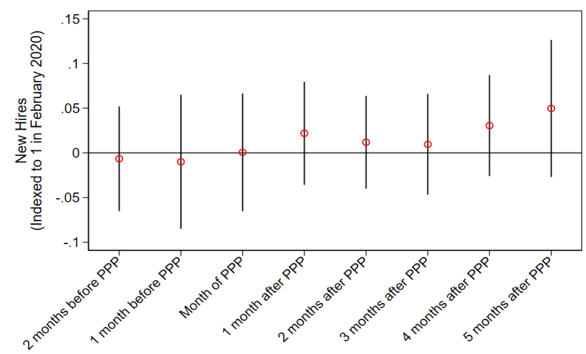
(b) Total Hires: top tercile of hourly workers



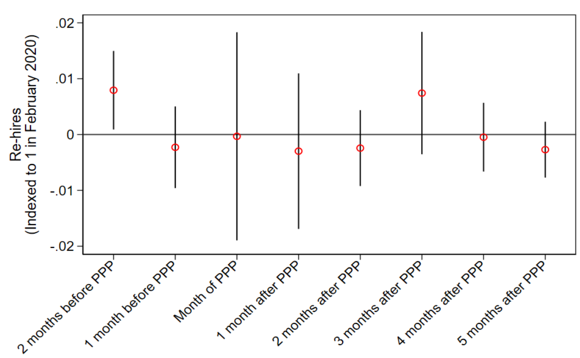
(c) New Hires: bottom tercile of hourly workers



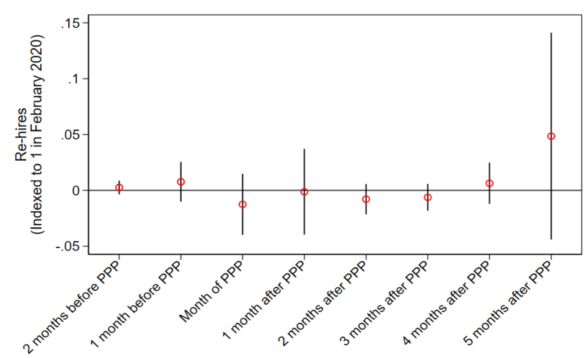
(d) New Hires: top tercile of hourly workers



(e) Rehires: bottom tercile of hourly workers



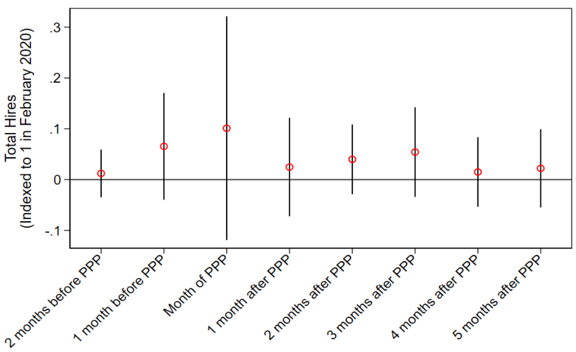
(f) Rehires: top tercile of hourly workers



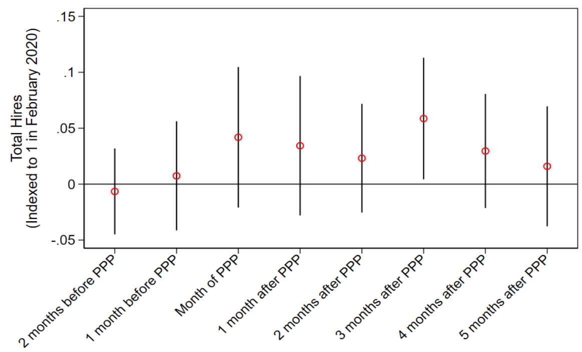
Notes: These figures show the results from the difference-in-differences specification in equation (2), I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The figures on the left show the results for firms in industries with the fewest hourly workers. The figures on the right show the results for firms in industries that have the most hourly workers. The dependent variables are normalized to one in February 2020. Black lines represent 90% confidence intervals

Figure A.20: Effect of PPP on Employee Turnover, by Remote Capability of Employees

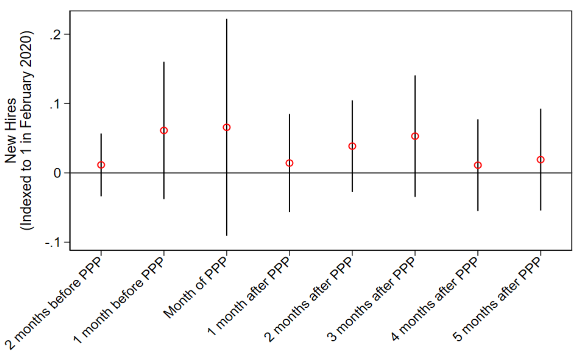
(a) Total Hires: high remote capability



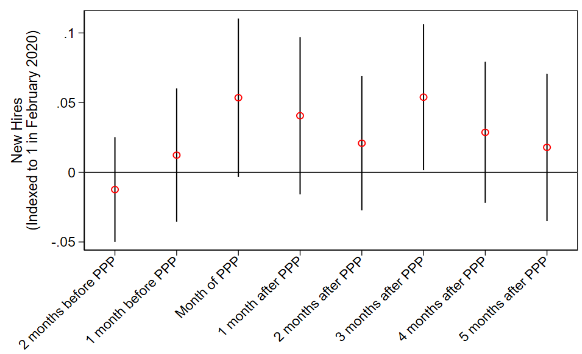
(b) Total Hires: low remote capability



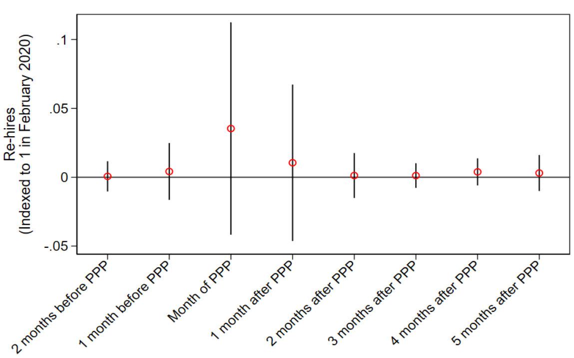
(c) New Hires: high remote capability



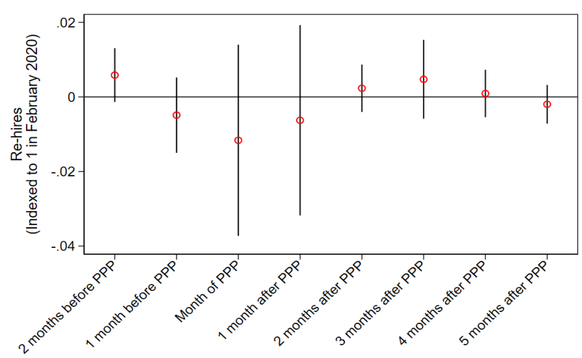
(d) New Hires: low remote capability



(e) Rehires: high remote capability



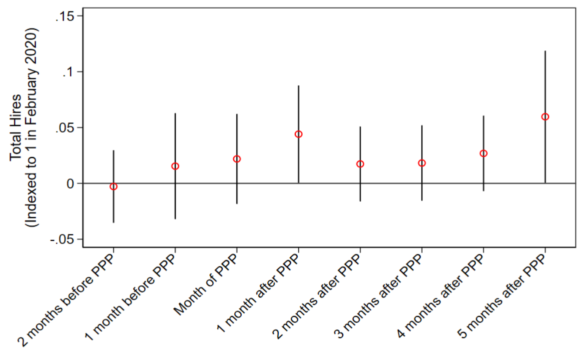
(f) Rehires: low remote capability



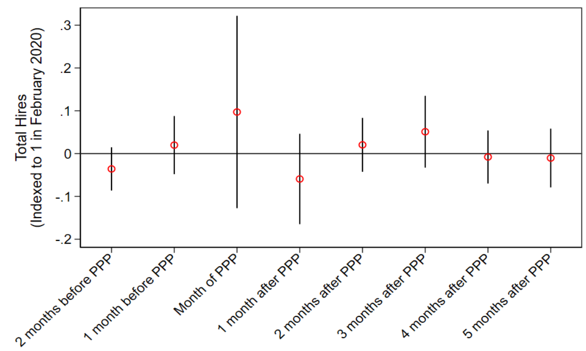
Notes: These figures show the results from the difference-in-differences specification in equation (2), I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The figures on the left show the results for firms which are in industries in which employees are less likely to be able to work from home. The figures on the right show the results for firms which are in industries in which employees are more likely to be able to work from home.The dependent variables are normalized to one in February 2020. Black lines represent 90% confidence intervals

Figure A.21: Effect of PPP on Employee Turnover, by Essential versus Non-Essential Businesses

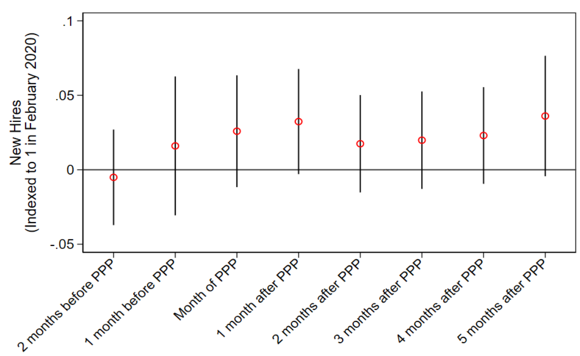
(a) Total Hires: non-essential businesses



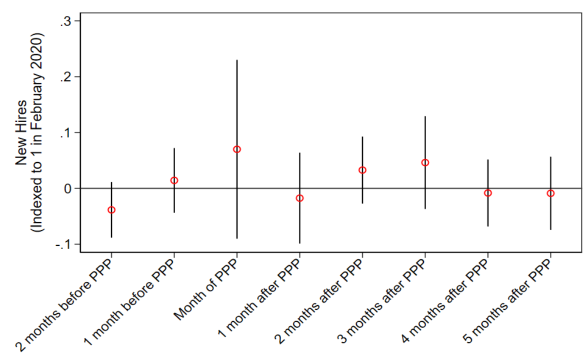
(b) Total Hires: essential businesses



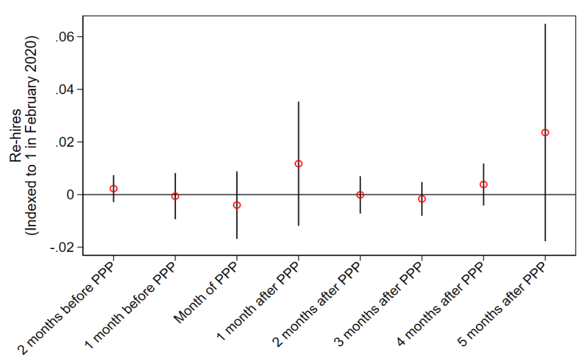
(c) New Hires: non-essential businesses



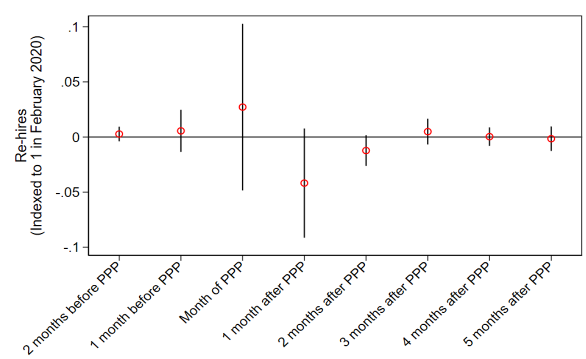
(d) New Hires: essential businesses



(e) Rehires: non-essential businesses

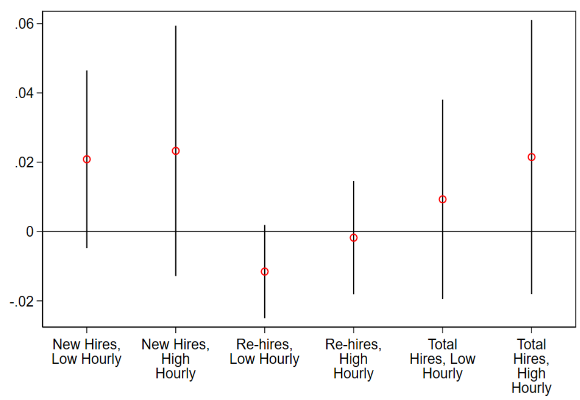


(f) Rehires: essential businesses

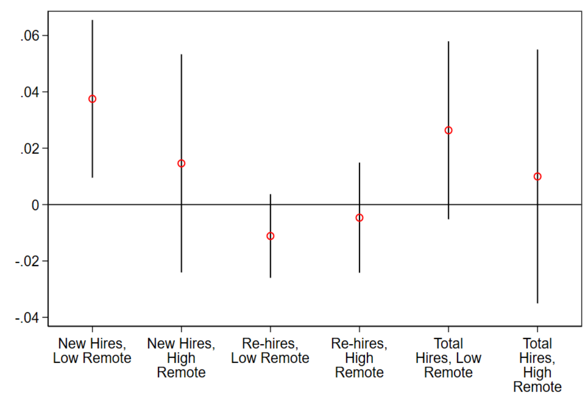


Notes: These figures show the results from the difference-in-differences specification in equation (2), I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The figures on the left show the results for firms which are typically classified as non-essential. The figures on the right show the results for firms which are typically classified as essential. The dependent variables are normalized to one in February 2020. Black lines represent 90% confidence intervals

(a) By Prevalence of Hourly Workers



(b) By Remote Capability



(c) By Essential Businesses

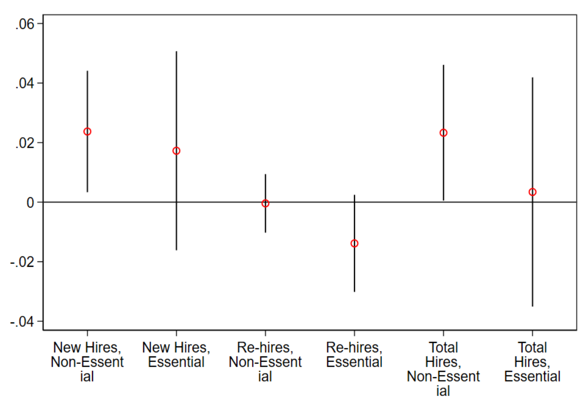


Figure A.22: Effect of PPP on Employee Turnover, Average Effects

Notes: These figures show the results from the difference-in-differences specification in equation (2, split by different types of firms. I plot the coefficient on β_t, which traces the differences between firms that applied for PPP and firms that did not over time. The dependent variables are measured as fraction of the firm's base employment (average February 2019-February 2020). The black lines represent 90% confidence intervals.

Table A.2: PPP Loan Terms and Forgiveness Rules, Pre-PPPFA and Post-PPPFA

	Loans issued:	
	March 27, 2020-June 4, 2020	June 5, 2020-August 8, 2020
Loan term	2 years	5 years
Interest Rate	1%	1%
% that must be used for payroll	75%	60%
Prorated forgiveness?	Yes	No
Covered period	8 weeks	24 weeks
FTE restoration deadline	June 30, 2020	December 31, 2020
Payment deferral period	6 months	6 months + administrative lag time

Notes: This table describes the rules on loan terms and forgiveness for the PPP prior to the Paycheck Protection Flexibility Act (June 5, 2020) and afterward. Note that the new rules on forgiveness apply retroactively to firms that applied before the PPPFA was enacted.

Table A.3: Take-up Regressions: Late versus Early appliers

	(1)	(2)	(3)	(4)	(5)
			Odds Ratios		
Log Base Employment	0.6387***	0.6471***	0.6588***	0.6916***	0.7390***
	(0.0664)	(0.0734)	(0.0614)	(0.0651)	(0.0784)
Log Average Salary (Base Period)	0.9621	0.9613	0.9147	0.8935	0.9928
	(0.4412)	(0.4370)	(0.3436)	(0.3402)	(0.4805)
Monthly Employment Growth (April 2020)		0.9670		1.4155*	0.9320
		(0.1846)		(0.2913)	(0.2033)
Average industry employment change during pandemic			11.1326	8.5239	
			(30.8182)	(23.6085)	
Bank in 2nd Tercile of Overall PPP Lending					1.3178
					(0.7744)
Bank in 3rd Tercile of Overall PPP Lending					1.5910
					(1.0898)
Industry FE?	Y	Y	N	N	Y
State FE?	Y	Y	Y	Y	Y
N	194	193	195	194	168
pseudo R^2	0.135	0.134	0.053	0.058	0.119

Notes: This table shows the results of a logistic regression of an indicator equal to one if the firm applied for PPP in the second round on a set of controls for firm characteristics (equation (1)). I drop all firms that did not apply, so this regression compares those that applied late to those that applied early. The odds ratios are reported. Log Base Employment is the log of the firm's average employment from February 2019-February 2020 (or the months in which the firm appears in that data set over that time period). Log Base Average Salary is the firm's average monthly wage per worker over the same time period. Monthly Employment Growth (April 2020) is the firm's log employment growth from March 2020 to April 2020. Average industry employment change during pandemic is the percentage decline in paid employment for the firm's two-digit NAICS code from February 15, 2020- April 25, 2020 (taken from Cajner et al. (2020)). 2nd and 3rd tercile of overall PPP lending are dummies equal to one if the firm's primary bank is in the second or third (highest) tercile, respectively, of share of PPP lending less than $150,000 in the firm's state. Standard errors, clustered at the 2-digits NAICS code level, in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < .01$

Table A.4: Take-up Regressions: Early Applicants

	(1)	(2)	(3)	(4)	(5)
			Odds Ratios		
Log Base Employment	2.1305***	2.0410***	2.1098***	1.9962***	1.8450***
	(0.1571)	(0.1344)	(0.1430)	(0.1234)	(0.1684)
Log Average Salary (Base Period)	1.1429	1.1918	1.1738	1.2445	1.1754
	(0.4427)	(0.4754)	(0.3685)	(0.3979)	(0.4392)
Monthly Employment Growth (April 2020)		0.7634		0.6276*	0.6942
		(0.2258)		(0.1644)	(0.2246)
Average industry employment change during pandemic			0.0897	0.1170	
			(0.2109)	(0.2798)	
Bank in 2nd Tercile of Overall PPP Lending					0.8779
					(0.6142)
Bank in 3rd Tercile of Overall PPP Lending					0.8552
					(0.5358)
Industry FE?	Y	Y	N	N	Y
State FE?	Y	Y	Y	Y	Y
N	327	324	342	339	276
pseudo R^2	0.196	0.195	0.149	0.153	0.160

Notes: This table shows the results of a logistic regression of an indicator equal to one if the firm applied during the first round for the PPP on a set of controls for firm characteristics (equation (1)). The odds ratios are reported. Log Base Employment is the log of the firm's average employment from February 2019-February 2020 (or the months in which the firm appears in that data set over that time period). Log Base Average Salary is the firm's average monthly wage per worker over the same time period. Monthly Employment Growth (April 2020) is the firm's log employment growth from March 2020 to April 2020. Average industry employment change during pandemic is the percentage decline in paid employment for the firm's two-digit NAICS code from February 15, 2020- April 25, 2020 (taken from Cajner et al. (2020)). 2nd and 3rd tercile of overall PPP lending are dummies equal to one if the firm's primary bank is in the second or third (highest) tercile, respectively, of share of PPP lending less than $150,000 in the firm's state. Standard errors, clustered at the 2-digits NAICS code level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A.5: Take-up Regressions: Late Applicants

	(1)	(2)	(3)	(4)	(5)
			Odds Ratios		
Log Base Employment	1.4949**	1.5108**	1.5551***	1.5578***	1.6742**
	(0.2432)	(0.2511)	(0.2034)	(0.2040)	(0.4349)
Log Average Salary (Base Period)	1.5826**	1.5386**	1.4354*	1.3990	1.4662
	(0.2976)	(0.3004)	(0.2918)	(0.2890)	(0.3576)
Monthly Employment Growth (April 2020)		1.3190		1.1279	1.1754
		(0.5279)		(0.3403)	(0.4801)
Average industry employment change during pandemic			0.5441	0.5562	
			(0.8138)	(0.8322)	
Bank in 2nd Tercile of Overall PPP Lending					1.0090
					(0.3385)
Bank in 3rd Tercile of Overall PPP Lending					1.7539**
					(0.4559)
Industry FE?	Y	Y	N	N	Y
State FE?	Y	Y	Y	Y	Y
N	262	260	259	257	223
pseudo R^2	0.096	0.094	0.064	0.061	0.116

Notes: This table shows the results of a logistic regression of an indicator equal to one if the firm applied during the second round of the PPP (excluding firms that applied during the first round) for the PPP on a set of controls for firm characteristics (equation (1)). The odds ratios are reported. Log Base Employment is the log of the firm's average employment from February 2019-February 2020 (or the months in which the firm appears in that data set over that time period). Log Base Average Salary is the firm's average monthly wage per worker over the same time period. Monthly Employment Growth (April 2020) is the firm's log employment growth from March 2020 to April 2020. Average industry employment change during pandemic is the percentage decline in paid employment for the firm's two-digit NAICS code from February 15, 2020- April 25, 2020 (taken from Cajner et al. (2020)). 2nd and 3rd tercile of overall PPP lending are dummies equal to one if the firm's primary bank is in the second or third (highest) tercile, respectively, of share of PPP lending less than $150,000 in the firm's state. Standard errors, clustered at the 2-digits NAICS code level, in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < .01$

Table A.6: Take up regressions: controlling for average yearly growth

	(1)	(2)
		Odds Ratios
	All Applicants	Late versus early applicants
Log Base Employment	1.9531**	0.8089
	(0.5515)	(0.1176)
Log Average Salary (Base Period)	1.1728	0.9748
	(0.2301)	(0.5211)
Monthly Employment Growth (April 2020)	0.9001	2.8864**
	(0.4133)	(1.4994)
Average Yearly Employment Growth (2020, pre-pandemic)	1.1299	0.6612
	(0.2314)	(0.4832)
Bank in 2nd Tercile of Overall PPP Lending	0.9442	1.9074
	(0.6246)	(1.3279)
Bank in 3rd Tercile of Overall PPP Lending	1.3629	1.6017
	(0.4872)	(1.5099)
Industry FE?	Y	Y
State FE?	Y	Y
N	194	115
pseudo R^2	0.118	0.142

Notes: This table shows the results of a logistic regression of an indicator equal to one if the firm applied for the PPP on a set of controls for firm characteristics (equation (1)). The first column shows the full sample. The second column shows the late applicants, relative to the early applicants only. The odds ratios are reported. Log Base Employment is the log of the firm's average employment from February 2019-February 2020 (or the months in which the firm appears in that data set over that time period). Log Base Average Salary is the firm's average monthly wage per worker over the same time period. Monthly Employment Growth (April 2020) is the firm's log employment growth from March 2020 to April 2020. Average industry employment change during pandemic is the percentage decline in paid employment for the firm's two-digit NAICS code from February 15, 2020- April 25, 2020 (taken from Cajner et al. (2020)). 2nd and 3rd tercile of overall PPP lending are dummies equal to one if the firm's primary bank is in the second or third (highest) tercile, respectively, of share of PPP lending less than $150,000 in the firm's state. Standard errors, clustered at the 2-digits NAICS code level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A.7: Take-up Regressions: Balanced Sample

	(1)	(2)	(3)	(4)	(5)
			Odds Ratios		
Log Base Employment	1.7639***	1.6562**	1.8510***	1.7571***	1.9297*
	(0.3246)	(0.3456)	(0.2861)	(0.3014)	(0.7037)
Log Average Salary (Base Period)	1.2850	1.3785	1.3863	1.4535	1.1279
	(0.4757)	(0.4166)	(0.4817)	(0.4360)	(0.3284)
Monthly Employment Growth (April 2020)		0.5862*		0.5865	0.4206
		(0.1768)		(0.1909)	(0.2383)
Average industry employment change during pandemic			0.6416	1.0688	
			(0.9174)	(1.6979)	
Bank in 2nd Tercile of Overall PPP Lending					0.8017
					(0.5201)
Bank in 3rd Tercile of Overall PPP Lending					1.2698
					(0.4382)
Industry FE?	Y	Y	N	N	Y
State FE?	Y	Y	Y	Y	Y
N	206	206	211	211	175
pseudo R^2	0.110	0.116	0.081	0.086	0.135

Notes: This table shows the results of a logistic regression of an indicator equal to one if the firm applied for PPP on a set of controls for firm characteristics (equation (1)). This robustness check includes only the balanced panel of firms that have been in the data since January 2019. The odds ratios are reported. Log Base Employment is the log of the firm's average employment from February 2019-February 2020 (or the months in which the firm appears in that data set over that time period). Log Base Average Salary is the firm's average monthly wage per worker over the same time period. Monthly Employment Growth (April 2020) is the firm's log employment growth from March 2020 to April 2020. Average industry employment change during pandemic is the percentage decline in paid employment for the firm's two-digit NAICS code from February 15, 2020- April 25, 2020 (taken from Cajner et al. (2020)). 2nd and 3rd tercile of overall PPP lending are dummies equal to one if the firm's primary bank is in the second or third (highest) tercile, respectively, of share of PPP lending less than $150,000 in the firm's state. Standard errors, clustered at the 2-digits NAICS code level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A.8: Hourly Workers by Industry

	Number of Hourly Employees	Number of Employees	Percentage of hourly employees
Manufacturing	9,094	12,688.7	71.7
Retail Trade	11,346	15,833.1	71.7
Mining/Quarrying/Oil and Gas Extraction	475	683.3	69.5
Construction	4,807	7,289.3	65.9
Transportation/Warehousing	3,395	5,419.1	62.6
Accommodation/Food	9,828	6,348.5	60.1
Educational and Health Services	13,283	23,666.5	56.2
Other Services	3,254	6,622.4	49.1
Finance/Insurance	3,499	8,568.8	40.8
Agriculture/Forestry/Fishing /Hunting	825	2,310	35.7
Professional Services	6,336	20,999.5	30.2
Wholesale Trade	1,675	5,852.5	28.6

Notes: This table show the number and percentage of employees in a given industry that are paid on an hourly basis. Employment numbers are in thousands. Data are from the Bureau of Labor Statistics.
https://www.bls.gov/emp/tables/employment-by-major-industry-sector.htm
https://www.bls.gov/opub/reports/minimum-wage/2015/home.htm

Table A.9: Correlations between remote, hourly, and essential workers

	Highly remote	Highly hourly	Essential
Highly remote	1.00	-0.77	0.08
Highly hourly	-0.77	1.00	-0.16
Essential	0.08	-0.16	1.00