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The Job-Preservation Effects of Paycheck Protection Program Loans

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Abstract

The Paycheck Protection Program (PPP) covered in excess of 80% of eligible U.S. small business employment, supporting 51 million American jobs through the program's close on August 8^{th}, 2020. Of those supported jobs, how many would have been lost in the absence of PPP loans? To answer this question, we execute an empirical strategy to identify the effects of PPP loans on county-level unemployment insurance claims. Specifically, we exploit variation in the timing of loan receipt caused by differences in local banking market structure across US counties. On the margin, we estimate that a 10 percentage point increase in eligible payroll covered by PPP resulted in a 1 to 2 percentage point smaller jump in initial weekly unemployment insurance (UI) claims, as a share of employment covered by UI. That same 10 percentage point increase in PPP coverage resulted in an estimated 5 percentage point smaller increase in the insured unemployment rate. In order to compare our estimates with related studies, we calculate an aggregate employment effect of PPP loans. Moving from the 25th percentile to 75th percentile of counties by early PPP coverage causes an improvement in the insured unemployment rate of over 12 percentage points, or, extrapolated nationally, 18.6 million jobs, at an average cost of roughly $28,000. We note meaningful caveats to interpreting this paper's aggregate number; the same caveats that apply to other papers evaluating PPP loans. This paper's estimates are an order of magnitude larger than previous evaluations of PPP, which have tended to find small employment effects or none at all.

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I. Introduction

When most American small businesses faced mandatory Covid-19 related closures and a drastic reduction in revenue, the Paycheck Protection Program (PPP) offered a financial lifeline. With the economy contracting at an annualized 31.4% rate in the second quarter of 2020, widespread and permanent small business closures and mass layoffs seemed likely. PPP was implemented to temporarily sustain America's small businesses and the jobs they provide. PPP coverage was broad: over 80% of eligible small business employment was supported, totaling 51 million jobs when the second tranche closed. However, calculating the number of jobs supported by the PPP is relatively simple; calculating the number of jobs preserved poses a harder task. This paper addresses that thornier question: how many more workers would have been on UI, in the absence of PPP? In other words, how many paychecks did the Paycheck Protection Program protect?

To identify the employment effects of PPP loans, we simultaneously exploit geographic and timing variation in loans. Variation in the date of loan approval should govern the timing of the treatment effects. Specifically, we study how the cumulative share of county-level employment supported by PPP as of April 11th –the date of maximum dispersion in PPP coverage—affects the evolution of unemployment claims across counties over time.

However, small business finances and local employment are not independent, so we cannot directly identify on timing. To address the endogeneity of the loan process, we add geographic variation based on the local market structure of banks. Differences in banking market structure help to isolate an exogenous component of loan timing: community banks were markedly quicker to approve and disburse first round PPP funds than national banks or non-bank lenders. Under the condition that the composition of the local banking market has no effect on early pandemic changes in local employment – save through PPP loans – we can use this variation to identify the effects of PPP loans on employment. We hypothesize that areas which received PPP money early should have better labor market outcomes at the pandemic's outset, but that as PPP money reached near-full saturation, labor market outcomes should converge. This dynamic convergence is the crux of our hypothesis, and an important test of our identification strategy. Since our hypothesis is dynamic, any potential bias in the instrument must also be dynamic, relevant early in the pandemic but not later.

Further sharpening our identification of the supply effect of PPP, the granularity of our data allows us to condition on numerous potential cofounding variables. Critically, we include state-week fixed effects in all our empirical specifications. As a result, our identifying variation is entirely intrastate, meaning we avoid complications introduced by the different timing of state-level economic restrictions and lockdowns, as well as state-level policies toward unemployment insurance or banks. Since direct Covid-induced variation remains within each state, we also control for weekly and monthly Covid cases and deaths at the county-level. Finally, we control for county-level pre-pandemic unemployment rates and population density. In sum, our empirical results are driven by within-state variation in community bank penetration – prior to the pandemic – mitigating core endogeneity concerns.

Estimates based on county-level data suggest that, at the margin, a 10 percentage point increase in PPP coverage of eligible payroll resulted in a smaller jump in initial Unemployment Insurance (UI) claims by between 1 and 2 percentage points of covered employment.[1] With a lag in time, the same increase in PPP

[1] Technically, we are referring to an initial claims analog to the Insured Unemployment Rate (IUR). In other words, the underlying percentage is (number of initial UI claims)/(Covered Employment).

coverage led to a smaller increase in continuing UI claims (the IUR) by 5 percentage points. Consistent with our expectations, the comparative benefits of early loan receipt disappear over time as lagging states catch up on PPP penetration.

Extrapolating these findings to aggregate employment, our results imply that moving from the 25th percentile to 75th percentile of counties by PPP coverage causes an improvement in the unemployment rate of over 12 percentage points, or, extrapolated nationally, 18.6 million jobs. At an average cost of approximately $28,000 per job saved, this makes PPP an extremely cost-effective means of job preservation in the 2020 recession. This calculation assumes that the estimated marginal effects equal the average effect within the interquartile range. While this strong assumption is unlikely to hold literally, we make it in order to compare our estimates with other studies on the topic, which require the same assumption to compute aggregate employment effects.

II. Previous Estimates

Three significant studies, Chetty et al. (2020), Autor et al. (2020), and Hubbard and Strain (2020) use innovative high frequency, large datasets to study the effects of PPP. In most industries, only firms with fewer than 500 employees were eligible to receive PPP money, and these studies leverage this eligibility cutoff, comparing employment at firms just above and below that threshold. Autor et al. (2020) estimate that PPP preserved 1.4 to 3.2 million jobs, while Chetty et al. (2020) find "no evidence" of a significant impact of the Program, and Hubbard and Strain (2020) find statistically significant evidence of small employment effects.

In contrast, many market participants have argued that the PPP was pivotal in mitigating the potential economic devastation the pandemic could have caused. Jamie Dimon of JPMorgan Chase stated that he estimates the program to have saved "30 to 35 million jobs" (Ruhle, Miranda, and Capetta (2020)) while the chief economist at Standard & Poors (Fox et al. (2020)) has publicly stated that 13.6 million jobs were saved. Goldman Sachs' chief US economist stated that PPP was a prime factor preventing what "really seemed like it had the potential to be a huge collapse...[the lack of bankruptcies] has come as a pleasant surprise". How do we reconcile the significant differences in the estimated impact of the program between recent academic studies and market participants?

While the empirical design of Autor et al. (2020) and Hubbard and Strain (2020) may be relevant in the neighborhood of the eligibility cutoff, we question the external validity of these kinds of estimates. Using reports from PPP loans themselves, we display in Figure I the cumulative density of PPP loan recipients based on firm size, as measured by self-reported employment on loan applications. Where not reported (6.8% of all loans), we leave firm size at 0, to keep with the Small Business Administration (SBA) standard. However, imputation would not make a noticeable difference to our numbers. Figure I shows that 57.6% of loans went to firms with three or fewer employees, 78.8% of loans went to firms with nine or fewer employees, and 95% of loans went to firms with 38 or fewer employees.[2] Table 1 details the fraction of PPP loans that went to mid-sized firms and establishments, by number of loans, number of workers covered, and total PPP dollars. The cutoffs were chosen to facilitate comparison with the findings of Autor et al. (2020). For that paper's largest treatment group, it captures 13% of all workers covered by PPP loans, representing only

[2] If we instead exclude loans with an unrecorded number of jobs, 54.7% of loans would be to firms with 3 or fewer employees, 77.4% to firms with fewer than 10, and the 95th percentile of firm size would be 40 employees.

11.1% of the total dollars lent and a mere 0.34% of total loans.

In order to credibly evaluate the aggregate employment effect of the PPP, we need an empirical strategy to identify the effect across the size spectrum of firms and their number of workers. Estimates based on much larger firms, which constitute a small share of PPP recipients, apply to smaller firms only if we believe large and small firms were equally vulnerable to the pandemic's economic shock. If, however, the assumption of equal vulnerability is violated, these studies only tell us what happened in the neighborhood of the eligibility cutoff, for firms roughly 500 employees in size. Finding modest effects in large firms does not entail the same modest impact at firms with 5 or 10 employees. Since small firms are more financially fragile, these firms stood to gain the most from a PPP loan. As such, evaluations using the employee size cutoff potentially underestimate the true impact of PPP by a significant amount.

TABLE I

PPP Loans to Mid-Sized Firms, by Three Measures

Firm Size	No. of Loans	Total Workers	Total Dollars
> 450	5,160 (0.10%)	2.5 million (5.0%)	\$21.7 billion (4.1%)
> 400	7,103 (0.14%)	3.36 million. (6.6%)	\$29.0 billion (5.5%)
> 350	9,551 (0.18%)	4.28 million (8.4%)	\$37.0 billion (7.1%)
> 250	17,508 (0.34%)	6.63 million (13.0%)	\$58.1 billion (11.1%)

Source: SBA and author's calculations. Data as of 8 August, 2020. Firm Size is as reported by the SBA, without any additional imputation. "Mid-sized firms" as measured by employee head-count. 13% of Program dollars went to firms with more than 250 employees

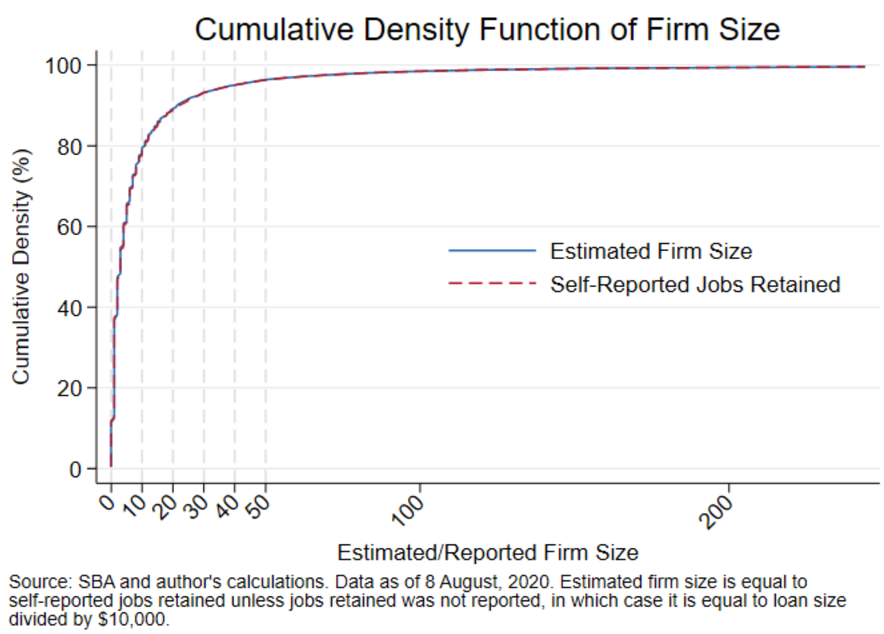


Figure I

Cumulative density of firm sizes by number of employees. Inferences drawn from firms with hundreds of employees tell us little about the bulk of loan recipients. These lines are on top of each other, which tells us our estimation for firms with missing worker counts matters little.

Moreover, Autor et al. (2020) note that neither of these studies observes firm-level loan data, which clouds interpretation. Because the studies mingle eligible firms which received PPP loans with eligible firms which did not receive PPP loans, the estimates themselves are biased downward; each estimate is a mixture of no effect for eligible firms near the cut-off which did not receive a loan, and—potentially small—effects for eligible firms near the cut-off which did receive a loan.

More problematic, as these studies move further away from the eligibility cutoff in order to increase sample size, their identification strategy grows murkier. The larger the difference in employee size, the weaker the comparison. Larger firms tend to have more sophisticated managers, larger cash buffers, more ready access to established lines of credit, fixed contracts with customers, and more bargaining power with suppliers. This puts larger firms in a stronger position to weather economic shocks than smaller businesses. Because of their superior ability to access lending and financial markets, larger firms were able to benefit from other treatments not available to smaller firms, like the Federal Reserve's interventions. Distance from the eligibility cutoff is particularly a problem in Chetty et al. (2020), who use firms with hundreds or thousands of employees as counterfactuals for firms with 50 employees; it is unclear what to make of such a comparison.

If businesses with fewer than ten employees had similar Covid-era employment outcomes as businesses with more than 250 employees (as suggested by Chetty et al. (2020)), this speaks to the success of PPP, not its failure. Without PPP's safety net to buffer against this unprecedented aggregate shock, economic theory and historical experience strongly suggest widespread small business permanent shutdowns and layoffs would have occurred; larger businesses are better able to survive shocks. We review evidence for these differences in fragility between small and mid-sized firms below.

A fourth study, conducted by Granja et al. (2020), finds precisely estimated tiny effects of PPP on hours worked, business shutdowns and UI claims. With respect to hours worked and business shutdowns, we note that the goal of PPP was to facilitate employees being paid while not necessarily physically showing up to workplaces, essentially working zero hours, and for businesses to shut down without laying off workers, closing permanently or filing for bankruptcy. Finding widespread business shutdowns and reductions in hours worked without corresponding bankruptcies doesn't imply failure of the Program. Indeed if those employees went back to work as the economy reopens, such outcomes will speak to the Program having worked as intended. In normal recessions, hours worked and business shutdowns are a good indicator of severity. However, a public-health recession is unusual in many ways. Policy was not traditionally Keynesian, and instead it was explicitly aimed at reducing business activity and face-to-face interaction by providing household liquidity that facilitated compliance with stay-at-home orders.

Further, Granja et al. (2020) study employment outcomes as a function of ultimate PPP penetration. However, since final PPP penetration is very high everywhere—in excess of 80% of eligible employment is covered, nationally—there might not be enough variation in the underlying independent variable of interest to identify the key effects. Granja et al. (2020) instead use an alternative independent variable, which is a function of bank market shares like ours below, and which likely induces more variation than ultimate PPP penetration itself. Our approach is distinct, precisely because it exploits the dynamic variation in PPP receipt.

Using survey data, Bartik et al. (2020c) estimates a larger employment effect for the PPP. However, their core employment estimates are not statistically significant. Bartik et al. (2020c) uses pre-existing relationships with banks to instrument for loan receipt, marking a similarity with this paper's use of banking market structure. The two banking-relationship instruments yield point estimates of 3.31 and 4.99 jobs saved per loan, though the associated standard errors are also large. If we multiply the point estimates by the roughly five million loans, this would imply between 16.6 and 25 million jobs preserved by the PPP, consistent with our main results below considering the size of the standard errors.

The work which comes closest to our own approach is that in Doniger and Kay (2021), which exploits the discontinuity in loan receipt around a ten-day window in which the Program had run out of funds from the first Congressional appropriation until the Program received the second Congressional appropriation; between April 16th and 27th, no new loans were approved. Doniger and Kay (2021) compares geographies which had large portions of loans approved between April 14th to 16th to geographies which had large portions of loans approved on April 27th and 28th, and, like our own work, finds large and significant job preservation effects of early PPP receipt. In this study, the cost per job saved is \$43,000, much closer to our own estimate of about \$28,000 than to the estimates in the papers which use the 500-employee threshold, which range from $173,000to$396,000.

However, while the principle identification concern with our approach is whether there are non-PPP cross-county differences correlated with both community-bank shares and employment outcomes, Doniger and Kay (2021)'s principal identification concern is whether there are differences in loans that occurred before and after the 10-day application window closure. For the reasons discussed below which caused some banks to be very fast to issue loans and other banks to be very slow, there may be some important differences interfering with random assignment. In practice, this meant that right before the window closure, commercial banks were heavily prioritizing their most valuable clients, and the lion's share of their PPP issuance went to important clients with their own personal banking representatives; less important clients were not worth the regulatory risk. During the 10-day closure while waiting on Congress, Treasury and SBA finalized key guidance for banks while the banks made significant progress on their automated processes used to qualify large volumes of smaller loans; after the closure, commercial banks pursued a much more egalitarian distribution of loan issuance.

We therefore see Doniger and Kay (2021) as complementary to our approach; while both exploit variation in the timing of loan receipt, they encounter different empirical obstacles. That they take a different route to exploit loan timing yet reach broadly similar conclusions is reassuring.

As a concluding comment, none of the current studies on PPP and employment (our own included) can capture general equilibrium effects. Given the historic scale of the crisis in March and April 2020, any successful large-scale intervention would have considerable general equilibrium effects. If layoffs at treated businesses reduce aggregate demand and cause layoffs at other businesses, then outcomes in any control group are caused in part by outcomes at the treatment group. This would lead to a classic violation of the Stable Unit Treatment Value Assumption (SUTVA) in a Difference-in-Differences design, and the observed differences between treatment and control groups will understate the true effects. If the study includes time fixed effects to measure overall employment outcomes at a moment in time, the time fixed effects will capture many of these general equilibrium effects. In that case, the true variable of interest will be some combination of the estimated treatment effect and the time fixed effect. However, since the time fixed effects will also capture the effects of other policies and shocks—for instance, Federal Reserve interventions, transfers to households, medical developments—identification of the full treatment effect will be difficult. Since PPP ultimately covered over 80% of national eligible employment, these general equilibrium effects will be nontrivial, and estimated treatment effects likely understate the true effects of PPP, though there may be biases in the other direction as well. Since our approach below uses counties as observational units with state-week fixed effects, this problem is likely to be less pronounced, though we will still miss cross-county economic linkages.

III. Back-of-the-Envelope Calculations

While most academic estimates of PPP's effects are very low, perceptions in markets and industry are much more optimistic. Standard and Poors (Fox et al. (2020)) estimates that PPP saved 13.6 million jobs, while JP Morgan's Jamie Dimon believes that PPP saved 35 million jobs (Ruhle, Miranda, and Capetta (2020)). In March, there were near-universal market expectations for a world of widespread, permanent small business closures and the mass layoffs that would result. For example, in February and March, many stock market indices declined by over 40%, including the Russell Microcap Index. The subsequent recovery in broad equity prices, including microcap stocks—which are only about 6% off their February high as of August 19th, 2020—is prima facie incompatible with miniscule effects of PPP loans and other government interventions. There is a wide disconnect between academic and market opinions.

At the outset of the pandemic, small businesses suddenly found themselves facing significant revenue declines that would likely cause insolvency. Smaller businesses are less likely to have access to an established credit line; and indeed if all small businesses sought credit simultaneously, the ability of the banking system to respond would likely increase the price and decrease the availability of credit.[3] A study by Farrell and Wheat (2016) found that the median small business had cash buffers to last only 27 days without revenue, and only 25% of small businesses could last more than 62 days without income. Their sample comprised roughly 600,000 small businesses; of these, 70% had five or fewer employees, which closely matches the universe of PPP loans wherein 70% of recipients had seven or fewer employees.

Against this backdrop, economic theory and experience from past recessions would indicate a surge in small business bankruptcies given the magnitude of the economic shock, as has occurred in previous recessions. Nevertheless, increases in small business bankruptcies have been modest, despite the largest economic shock in nearly a century, according to data from the Justice Department and analysis by the Council of Economic Advisers (2020). Indeed, contrary to expectations that small business bankruptcies should surge in this economic environment, Figure II shows that after spiking in February and March due to regulatory changes around Chapter 11 filings,[4] increases in small business bankruptcies in April through June were lower than they were before pandemic struck, and that small business bankruptcies increased by less than larger-sized business bankruptcies.

Moreover, we observe that Chapter 11 bankruptcies picked up in July and remained elevated, consistent with

[3] Although the Federal Reserve eased conditions in financial markets, it did not provide regulatory relief that would have explicitly eased the ability or willingness of banks to directly make loans to the number of small businesses under stress.

[4] For a discussion of the rules changes, see Ekvall and Evanston (2020). Small businesses took advantage of the easing criteria for Chapter 11 reorganizations, in the weeks before the pandemic hit. The Small Business Reorganization Act was signed in August 2019, and thus forward-looking firms had plenty of time to plan for the implementation in February.

the timing of when PPP funds began depleting.[5] Figure III shows that even with the spike in the summer, overall Chapter 11 bankruptcy filings are well within historical norms and far from levels seen in the last recession. Figure IV shows that Chapter 7 bankruptcies for the entirety of FY 2020 are below any level since 2008; unfortunately, the Department of Justice does not provide CEA with separate Chapter 7 filings data by business size, or data at frequencies higher than fiscal years. While many small businesses may simply shut down instead of filing for Chapter 7 or Chapter 11, one might expect a jump in simple shut-downs to be correlated with jumps in filings, and we haven't yet observed any historic jump in filings to correspond to the historic economic shock.

Indeed, industry economists have expected economy-wide bankruptcies throughout the recession. According to David Mericle, head of US economics at Goldman Sachs, "This really seemed like it had the potential to be a huge collapse. For most people, and I would include myself, [the lack of bankruptcies] has come as a pleasant surprise." For Mericle, the Paycheck Protection Program was the top item explaining the lower-than-expected number of bankruptcy filings (Coy (2020)).

Economists also anticipated higher job losses for extended periods of time. According to the BLS Establishment Survey, 20.7 million jobs were lost in April 2020 after a loss of 1.7 million jobs in March 2020. Markets expected that job losses would worsen into the summer, with concerns that the unemployment rate would exceed 20%. For May 2020, the Bloomberg median forecast projected more than seven million additional jobs lost. The most optimistic forecast in Bloomberg projected a loss of approximately two million jobs. The forecast error was pervasive and historic: the economy added more than 2.8 million in May, a surprise of more than 10 million jobs. While PPP was just one part of the CARES Act, it was the largest component by dollars appropriated and was fully implemented by the May 12 timeframe of the May employment surveys. Given the astonishing reversal in the employment situation – immediately following the hundreds of billions of dollars of PPP lending – the claim that PPP had minimal impact on the unexpected job rebound requires an alternate explanation with exceptionally strong evidence.

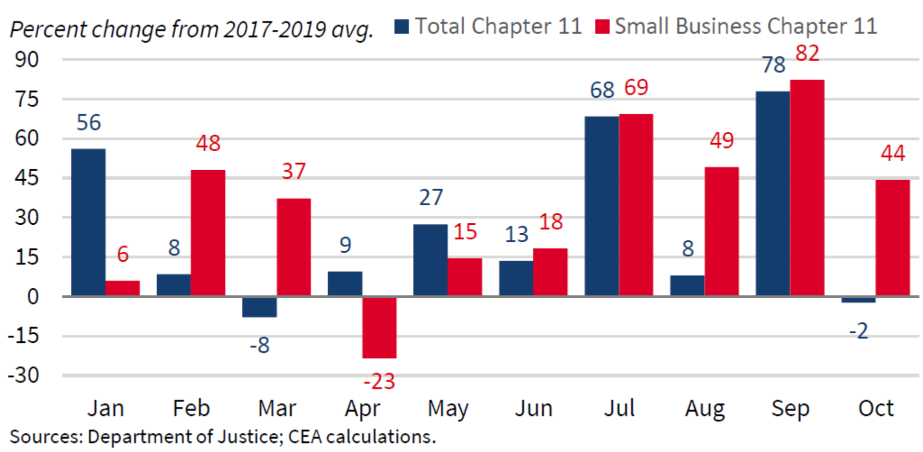


Figure II

US Total and Small Business Chapter 11 Bankruptcy Filings. Source: Council of Economic Advisers (2020).

[5] It is possible the uptick in bankruptcies corresponds to a conversion of liquidity problems to solvency problems; any firms which would be adversely affected by *permanent* changes in economic behavior due to the pandemic would find the viability of their business models degraded.

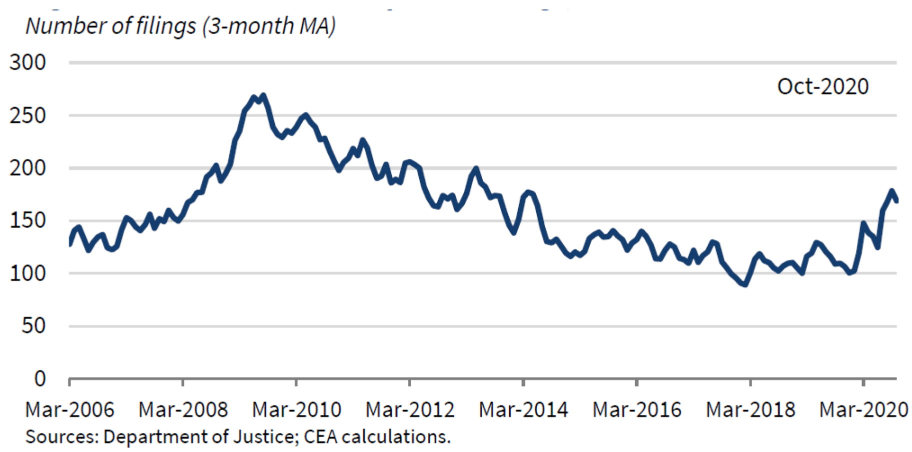


Figure III

Small business Chapter 11 bankruptcy filings, March 2006 - Oct 2020. Source: Council of Economic Advisers (2020).

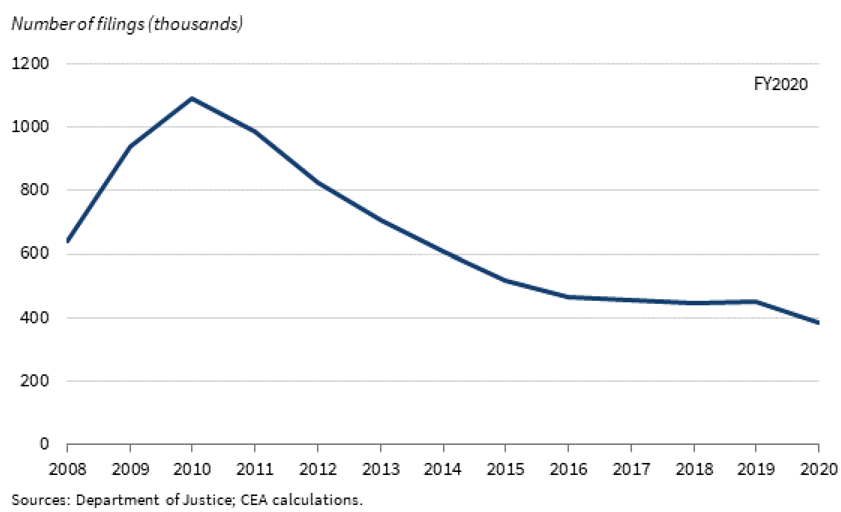


Figure IV

Total Chapter 7 bankruptcy filings, FY 2008 - FY 2020. Source: Council of Economic Advisers (2020).

We propose two simple back-of-the-envelope calculations to guide thinking about job preservation through the PPP. The first takes the threat of large-scale, permanent small business closure seriously, and estimates the job losses that might arise from those permanent closures. The second uses the small business survey responses from Bartik et al. (2020b) and estimates potential job losses based on those data. While these back-of-the-envelope calculations are not rigorous empirical work, they do serve to provide a ballpark of where one might expect empirical results to land, and thus a credibility check on empirical studies including the previous work we discussed above and our own empirical work below.

First, we consider the consequences of permanent, large-scale small business closure. Although we cannot directly observe the cash flows of small firms, we draw on findings in Farrell and Wheat (2016) that fewer than 75% of small firms hold cash buffers to cover more than 62 days of expenses. The PPP has given these financially vulnerable small businesses a vital cash flow lifeline to fill in for the drastic reduction in revenues caused by the pandemic and economic shutdown, thereby facilitating their survival and shoring up millions of jobs. The fragility of small businesses found in Farrell and Wheat (2016) is corroborated in work by Bartik et al. (2020a), which finds in an independent survey that fewer than 30% of firms had cash on hand to cover more than two months' expenses. Additionally, the Census Bureau's Small Business Pulse Survey found in the week ending May 2 that only 16.7% of small businesses had enough cash on hand to cover three or more months of business operations.

We start the first back-of-the-envelope calculation by making the assumption that 75% of small businesses covered by PPP would permanently shut down and layoff their workers in the Program's absence. This assumption is strong, but the evidence cited on small business fragility provides some justification. While it is possible small businesses could secure liquidity from private sources to help them manage shocks, evidence from the Joint Small Business Credit Survey in Federal Reserve Banks of New York, Atlanta, Cleveland, and Philadelphia (2014) suggests otherwise. According to the Fed Banks' report, only 32% of firms with 1-9 employees (which correspond to 75% of PPP recipients) received any credit, and a majority of firms with less than $1 million in revenue were unable to secure any credit whatsoever. Moreover, 40% of firms seeking credit said the primary purpose was for expansion, which suggests that fewer than 13% of firms with 1-9 employees had a line of credit which could be used to buffet revenue shocks.

The Fed report further finds that the primary means of financing of firms with less than $250,000 in revenues is personal savings; that the average time it takes a small business to fill out a credit application is 24 hours; and that typical wait times for approval are on the order of months. Credit has since the report become harder to get: according to the Federal Reserve's Senior Loan Officer Survey, the net percentage of banks tightening lending conditions for commercial and industrial loans to small firms reached 70% in the third quarter, only a few percentage points away from the previous peak in the series in the fourth quarter of 2008. All this suggests that most small businesses rely on their cash buffers to smooth expenditures.

In recognizing that some firms will have access to alternate lines of credit, and that revenues did not fall to zero for many businesses, we make a more conservative assumption regarding which firms go out of business. We assume that only the smallest firms who received PPP close: as argued, the smallest firms are the most likely to close. Further, insofar as this particular assumption misses the mark, it leads us to understate the true number of jobs preserved. Therefore, this back-of-the-envelope calculation assumes that the smallest 75% of PPP recipients would have had to lay off their workers without PPP, because their cash buffer stocks wouldn't allow them to meet expenses for longer than 62 days. Implementation of this assumption suggests that 13.4 million workers had their jobs preserved due to PPP.

To facilitate comparison with the estimates in Autor et al. (2020), consider that if all jobs lost were as a result of shutdowns at the smallest businesses, the loss of 1.4 to 3.2 million jobs is approximately consistent with the employment of the smallest 1.1 to 1.9 million firms in the sample of PPP loans. In contrast, the smallest 75% of firms we use for our back-of-the-envelope calculation corresponds to approximately 3.5 million small business closures.

An alternative back-of-the-envelope calculation can be derived from surveys in Bartik et al. (2020b). These surveys indicated that small firms in early April expected to have employment levels relative to January to fall 40% by year-end. However, when told about the forgiveness provisions in the CARES Act loans, the survey results reduced that forecast to a 6% reduction (firms told about loans but not forgiveness expected reductions of 14%). Extrapolated over the 51 million small business employees supported by PPP, this implicit reduction in unemployment is equal to 17.3 million workers.

One final point of reference is results in Barlett and Morse (2020), who find in a survey of 278 small businesses in Oakland, CA, that PPP receipt reduced the subjective risk of medium-term small business closure by an average 20.5 percentage points; if 20.5% of firms with fewer than 500 employees were forced to shut down due to the recession, that would destroy approximately 12.4 million jobs. Because the sample is small and localized, the results from this survey may generalize less readily than those of our other calculations above.

While these back-of-the-envelope calculations are useful for providing context, they are not careful empirical work. We now turn to an empirical strategy to identify and estimate the employment effects of PPP.

IV. Data

In our empirical strategy discussed below, our primary variation is PPP Loan data courtesy of the Small Business Administration. We observe the universe of approved SBA loans through August 8th, when the Program closed to new applicants. These data include loan recipient, address, exact loan amount and date, as well as a self-reported number of jobs covered.[6] Further, we observe the lender for each loan, which we match up to FDIC data in order to identify community banks.

County-level Unemployment Insurance (UI) claims weekly data are furnished by the Bureau of Labor Statistics. Initial claims data reflect the number of initial claims filed and eventually accepted by the state. Note that this is a different measure of initial UI claims than the widely-used weekly state-level UI release, which reports the number of claims filed, regardless of whether or not they are ultimately accepted.[7] Continuing UI claims data are also furnished by the BLS. Unlike the data on initial UI claims, we observe these data monthly, not weekly. We impute the data for the interim weeks using both accepted initial claims and the attrition rate implied by the monthly difference in continuing claims. These data reliably cover 40 states and DC. The remaining 10 states either do not report county-level statistics to the BLS, or their county-level data suffer from inconsistent reporting.[8] A state-level analysis in Appendix B, which draws on all 50 states and DC, corroborates our county-level findings, albeit it cannot make use of cross-sectional fixed effects.

We construct data for community bank penetration from the Federal Deposit Insurance Company's Summary of Deposits data, and we defined community banks following the FDIC's Institution Directory. Data on county population size and land area are from the US Census Bureau. Additionally, measures of eligible county-level payroll for firms with fewer than 500 employees are estimated based on Census' 2017 Statistics of U.S. Business. The reported payrolls have been adjusted for three years of estimated growth. These estimates are further adjusted to account for the fact that larger firms in the Accommodation and Food

[6] We exclude loans that were subsequently canceled or never disbursed.

[7] Given that our data include only claims which are accepted, they avoid some of the double-counting issues involved in applications for ordinary state programs and Pandemic Unemployment Assistance funds, discussed in Cajner et al. (2020) and elsewhere. Our metric of initial UI claims is arguably cleaner than the state-level releases made available to the public.

[8] The omitted states are California, Florida, Hawaii, Iowa, Kansas, Michigan, Minnesota, North Carolina, Oregon, and South Dakota.

Services industry were also permitted loans. With the exception of Nevada, this adjustment was relatively small. County-level data on Covid-19 cases and deaths are from the New York Times, based on reports from state and local health agencies.

Summary statistics for all our key variables can be found in Appendix A.

V. Identification from variation in timing of loan receipt

To estimate the employment effects of PPP loans, we propose to exploit variation in the timing of PPP receipt. This contrasts with a strategy that examines firms near the eligibility cutoff (Chetty et al. (2020), Autor et al. (2020) or ultimate PPP receipt Granja et al. (2020)). Since PPP penetration was ultimately very high throughout the country, firms which received money earlier should have better employment outcomes earlier, with other firms catching up as PPP approached saturation everywhere.[9]

However, the timing of loan receipt cannot, by itself, identify the employment effect of these loans; potential confounders abound. Due to this potential endogeneity, an OLS specification is likely biased. For instance, better management may have led to quicker loans, or loans flowed to firms which were more (or less) effected by Covid-19 and its associated demand effects. It could be that banks had more confidence in larger clients, or clients with a long-term banking relationship, and hence favored these firms to receive first tranche loans. To address these endogeneity concerns, we need to instrument for PPP penetration before we can leverage the variation in timing of loan receipt.

We propose community bank shares as our instrument. Some banks rapidly submitted loan applications to the SBA, while other banks chose to move slowly. Liu and Volker (2020) documented that community banks were among the fastest; this is due in part to differences in pre-existing banking relationships, but also to the regulatory complications of larger banks, which slowed their PPP lending. Mike Faulkender, one of this paper's authors, was the Treasury principal responsible for policy implementation of PPP. Several large banks told him directly that they refused to make PPP loans until SBA and Treasury issued further regulatory clarification via interim final rules and frequently asked questions. These banks had suffered years of legal disputes following TARP, after they had acted quickly and without comprehensive guidance. The consequence of these disputes was an aversion to regulatory risk – larger banks wanted explicit official guidance to serve as legal protection from large fines. By contrast, smaller and community banks did not believe they would be an attractive political target:no Administration would eagerly sue them. Smaller banks were therefore willing to act quickly to fill this market opening, taking regulatory risk before all the technical details were entered into the Federal Register. This difference in regulatory risk aversion was the key contributor to speed of PPP loan disbursement. Importantly, variation in regulatory risk aversion should be orthogonal to the spread of the virus or customer demand for PPP loans.

Early in the Program, therefore, there were significant differences across markets in PPP penetration, based in part on significant variation in community bank market share. In figure V, we illustrate that 56.6% of all PPP loans extended in the program's first 9 days came from community banks, as well as 46.8% of all

[9]We are implicitly assuming that anticipation of funds from PPP was not enough for small firms to delay layoffs, either because firms didn't have enough cash to allow them to do so, because they were not certain the PPP funds would arrive, or because small firms are less sophisticated and less forward-looking in their behavior.

loans approved during the first tranche of PPP funding. In contrast, by August 8th, this cumulative share fell to 30.6%, as other lenders more fully participated in the second round of funding. With clear evidence that community banks were quicker to disburse PPP loans, we use geographic variation in community bank market share to instrument for early loan receipt.

While some endogeneity concerns remain—community bank market share may itself be correlated with other relevant variables—we note that our hypothesis is about the timing of such outcomes. This means that any potential confounder would not merely have to correlate with community bank market shares, but would have to be dynamically correlated with employment outcomes. In other words, endogeneity issues would have to introduce bias in April and May, and then reverse the sign of this bias in July and August. In OLS results in Appendix A, community bank shares show a similar pattern to our IV estimates: any alternative explanation of these effects must discuss why these community bank shares are relevant in some weeks but not others. In addition, our empirical specification (discussed below) includes a number of controls, including county-level COVID case counts and state-week fixed effects, to further mitigate the potential impact of confounding effects.

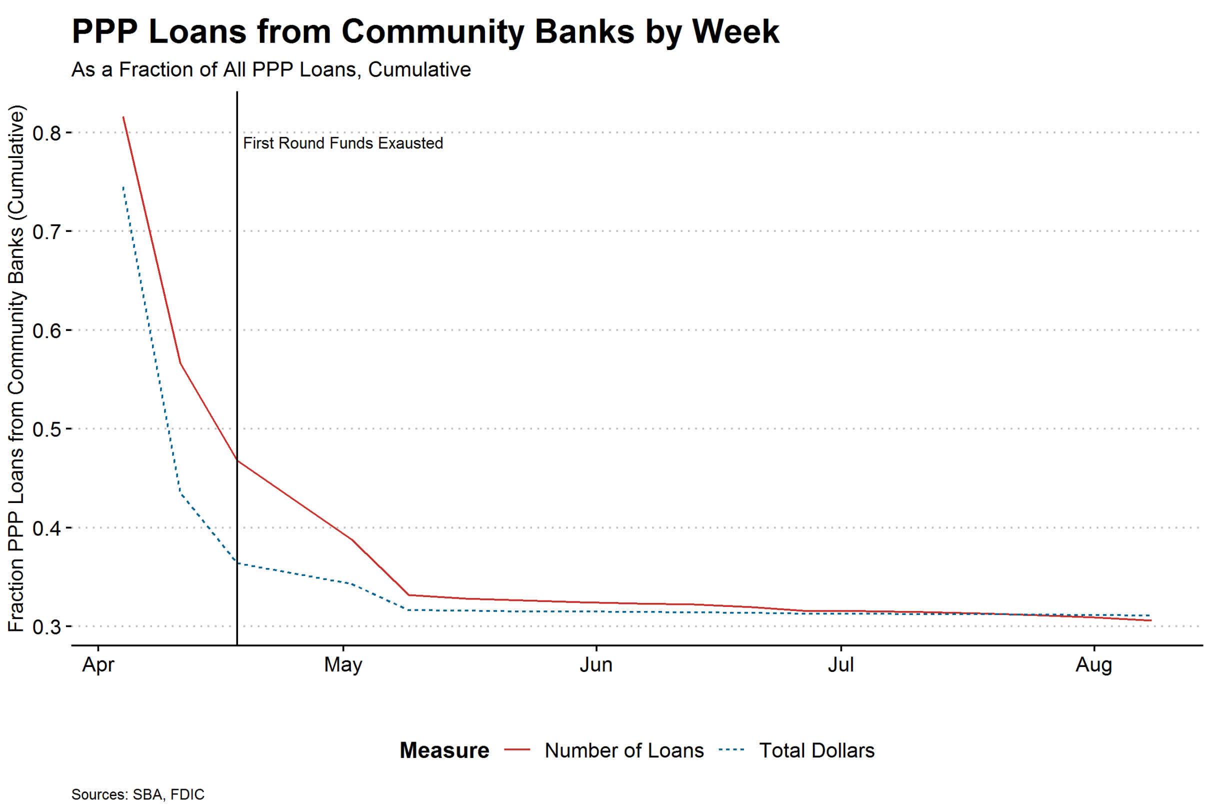
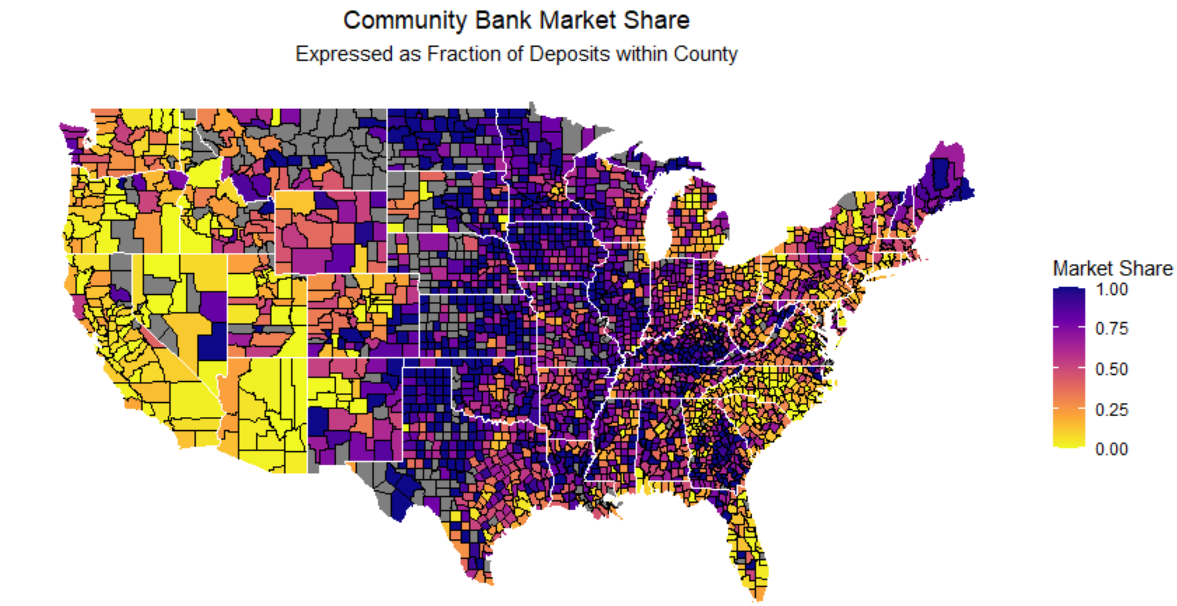


Figure V



Source: FDIC, Author's Calculations.

Figure VI

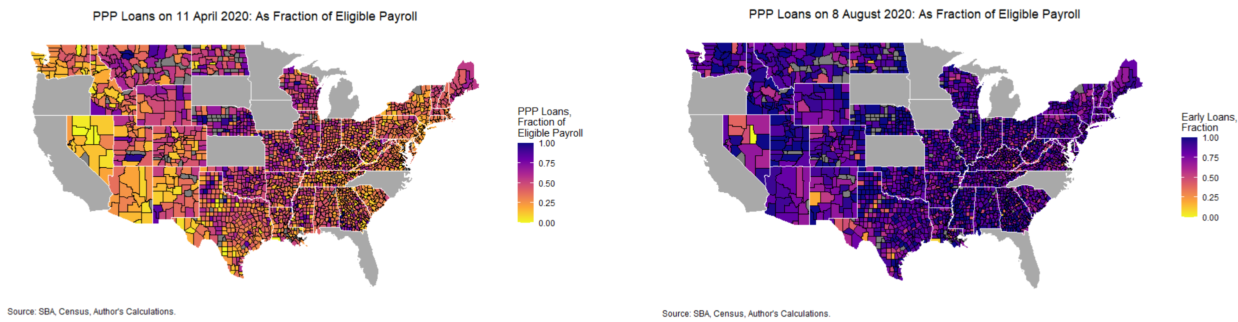
V.A. Empirical Model

We will return to discuss potential violations of the exclusion restriction, but first we describe the familiar two-equation IV model. In our initial empirical approach, we instrument county-level early PPP loan receipt with community bank shares, using the variation in local banking markets to predict early PPP receipt. If the PPP preserved jobs at scale, earlier loans should lead to superior early employment outcomes. However, since PPP penetration everywhere was ultimately so high—PPP covered over 80% of eligible small business employment, nationally—those employment effects of early treatment should disappear over time; states' labor market conditions should converge as PPP coverage converges. In OLS form, our primary specification is repeated cross-sections of

$$Y_{ct} = \beta_{0,s(c)t} + \beta_{1,t} PPP_{ct'} + X'_{ct}\beta_{2,t} + \epsilon_{ct} \tag{1}$$

where Y_{ct} is an employment outcome in county c during week t, $s(c)$ is the state of county c, and X_{ct} is a vector of controls. The first explanatory variable is a state-week fixed effect.

Our key variable of interest, PPP'_{ct}, is the cumulative percentage of small business payroll covered by a PPP loan, as of $t' =$ April 11th, 2020. We estimate this regression separately for each week in order to illustrate the dynamic effects of early loan receipt. Note that t' is held constant in each regression, while t varies, so the regression studies the dynamic relationship of employment with early PPP receipt; the key independent variable is fixed in time while the dependent variable evolves dynamically. The treatment is not that a county received PPP money, but that it received its PPP money early; and the subsequent convergence of



(a) Early Variation in Loan Coverage **(b)** Late Variation in Loan Coverage

Figure VII

Early in the program, there was significant geographical heterogeneity in PPP receipt. Later in the program, there was very little, limiting the ability of ultimate PPP receipt to identify the Program's effects.

outcomes across counties is the core of our hypothesis. As $(t - t')$ gets large, $\beta_{1,t}$ should converge to 0.

We have two primary outcome variables. The first is the insured unemployment rate (IUR) for regular state programs (i.e. continuing claims as a share of covered employment) at the county level. Using the IUR avoids the double counting problems involved in the Pandemic Unemployment Assistance program and captures high frequency reentry into the workplace. The second outcome variable is approved initial claims at the county level. In Appendix B, we also study the Census' Household Pulse Survey at the state level, which asks whether adults live in a household which has lost employment income during the pandemic, and find statistically significant results of similar magnitude.

In terms of cumulative small business payroll covered by PPP loans, we observe the greatest county-level variation early in the program's first round. By a variety of measures, the highest degree of dispersion in PPP coverage occurs during the week of April 11th, shortly after the program begins and when the first round of funds was nearly exhausted. On April 16th, the first tranche of $349 billion dollars had been fully depleted, and the program closed for 11 days. The resulting delay until the SBA resumed accepting loans bolsters our timing strategy, as it opened a substantial temporal gap between first-round and second-round loan recipients. For the first stage, we isolate this week in order to measure early receipt of PPP loans due to variation in banking relationships. Figure VI shows that early in the program, there was a large degree of dispersion across counties in how much of eligible payroll covered by PPP loans; by the end of the program, that dispersion had mostly disappeared, reducing the amount of variation available for studying the Program's effects.

Due to the endogeneity concerns discussed above, we instrument PPP'_{ct} with the share of county-level deposits held in community banks. We model the endogenous variable at t' as

$$PPP_{ct'} = \alpha_{0,s(c)t} + \alpha_{1,t}\text{CB\_Share}_c + X'_{ct}\alpha_{2,t} + \eta_{ct} \qquad (2)$$

The exclusion restriction is that CB_Share_c does not enter (1), save through (2). Figure IX shows the bin scatter plot of this first-stage regression, estimated with no controls for exposition.[10] The F-Stat for this no-controls first stage is 141.4. With the full battery of controls and standard errors clustered at the state level, the F-Stat is 8.7.[11] The full set of controls include state-time fixed effects, county population density, the pre-Covid county-level Insured Unemployment Rate (IUR), new Covid cases in the past week and four weeks, and finally new Covid deaths in the past week and four weeks—all at the county-level.

By including state-week fixed effects, this specification exploits within-state variation to identify the parameter of interest, $\beta_{1,t}$. States controlled many of the decisions regarding lockdowns and economic restrictions during March and April, and these states took a variety of approaches to combating the virus, both in terms of severity and timing. In many cases these measures had a direct impact on employment, for example when restaurants were prohibited from offering dine-in service. States also determine the major subnational regulations that affect the local banking markets, as well as the relative capacity of localities to cope with economic shocks. Cognizant of these heterogeneous economic restrictions, we focus on within-state-week variation at the county-level.

The spread of the virus itself is another obvious threat to our identification strategy. As Granja et al. (2020) notes, first-round PPP loans tended to flow to areas that were not as hard hit by the pandemic. Since Community Banks tend to have stronger market shares in rural areas, it's possible that our instrument is negatively correlated with early virus prevalence—which is in turn negatively correlated with unemployment. We address this concern via two types of control variables. First, we include new Covid-19 cases reported in county c over the week, as well as the preceding 4 weeks. Additional controls for Covid-19 deaths are included for the same timeframe. Second, we control for county population density. Finally, we include controls for the insured unemployment rate for the week of February 5th, 2020, to avoid mistaking pre-existing level differences for early effects.

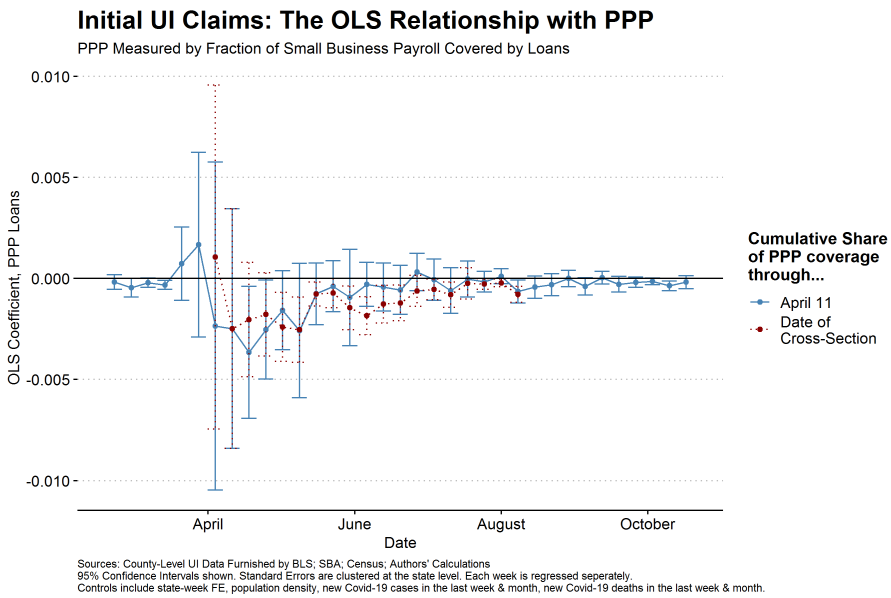
Having described the controls and the identification threats they are meant to address, critical endogeneity and selection concerns remain in the OLS specification. For example, we might be concerned that businesses in higher distress would be more likely to apply for loans. Alternatively, we might be concerned that businesses with stronger management will be quicker to apply for loans, or banks might be more inclined to deal with stronger businesses, regardless of government assurances. Nonetheless, we present OLS results before turning to our IV results. Figure VIII and Table A.3 present results from an ordinary least squares estimation of Equation 1. The OLS results show a small negative relationship between PPP Loans and initial UI claims in mid-April, though the relationship quickly peters out. There is also a negative relationship between PPP Loans and continuing UI claims, growing slightly over time.

Finally, note that our study cannot fully address the general equilibrium concerns we discussed earlier: if there are cross-county or cross-firm spillovers because layoffs in one location can cause layoffs in another location, then no county-level or firm-level analysis will really isolate treatment effects. If the "control group" is affected by outcomes at a treatment group, any empirical strategy will be problematic. However,

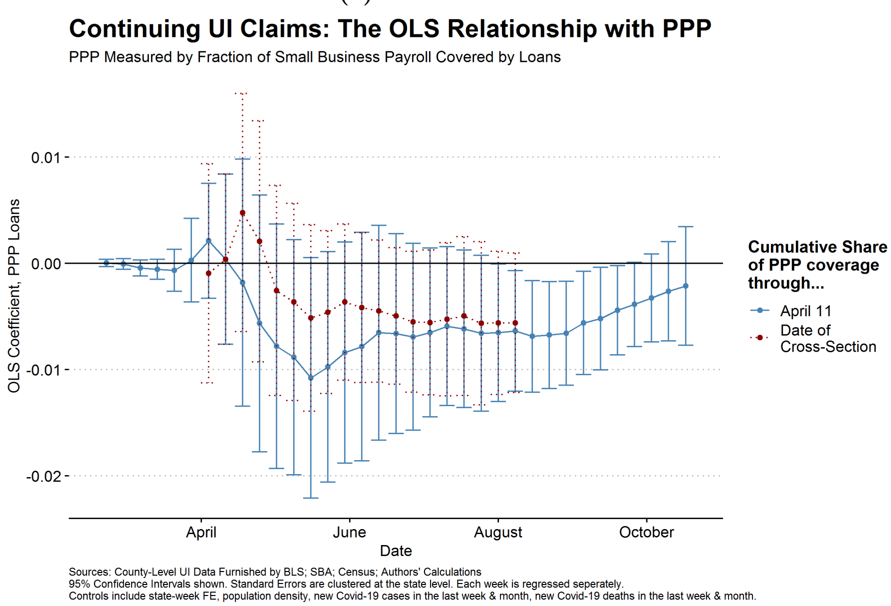
[10] Because there are over 3,000 counties in the United States, we present all scatter plots averaged by decile bin. Full scatter plots for all figures are available in Appendix A.

[11] The associated p-value for this F-stat is 0.0052. Since we do not assume our errors are i.i.d. – we cluster at the state-level – we also report tests developed in Kleibergen and Paap (2006). For underidentification, the Kleibergen-Paap rk LM statistic has a p-value of 0.0249 (Chi-Square(1) = 5.03). For weak identification, the Kleibergen-Paap Wald rk F-statistic is 13.3. For reference, the Anderson-Rubin Wald test has a p-value of 0.000 (Chi-Square(1) = 39.61).

this is a core problem in empirical economics, and we won't be able to surmount it here; indeed the other studies cited above have not addressed it either.



(a) Initial UI Claims



(b) Continuing UI Claims

Figure VIII

OLS estimates of UI Initial and Continuing claims, regressed on early PPP receipt, with control variables. Both initial and continuing claims are measured as a share of covered employment, at regular state UI programs, i.e. the insured unemployment rate. 95% confidence intervals displayed. "April 11^{th}" series keeps independent variable fixed in time, while "Date of cross-section" series allows the independent variable observation to match the time on the dependent variable observation.

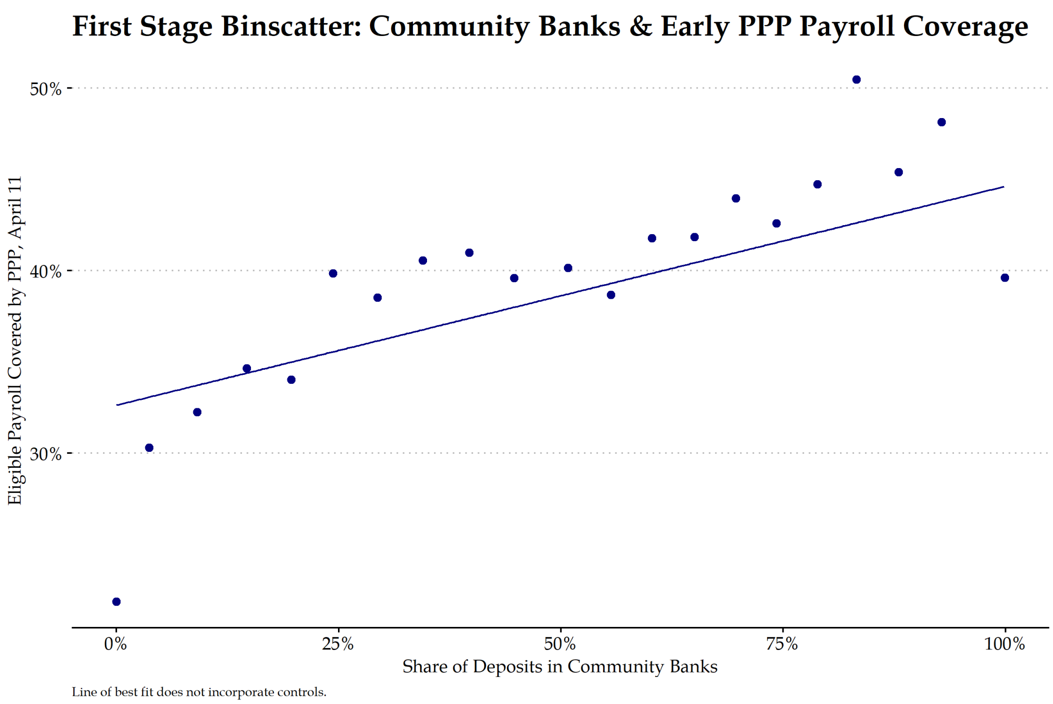
In Appendix B, we replicate this empirical strategy at the state level in both unemployment insurance claims and in the Census' Household Pulse Surveys. We find similar results in the latter dataset to our main results below, and the replication in a different dataset and at a different level of aggregation increases our confidence in our main results.

V.B. IV Estimates

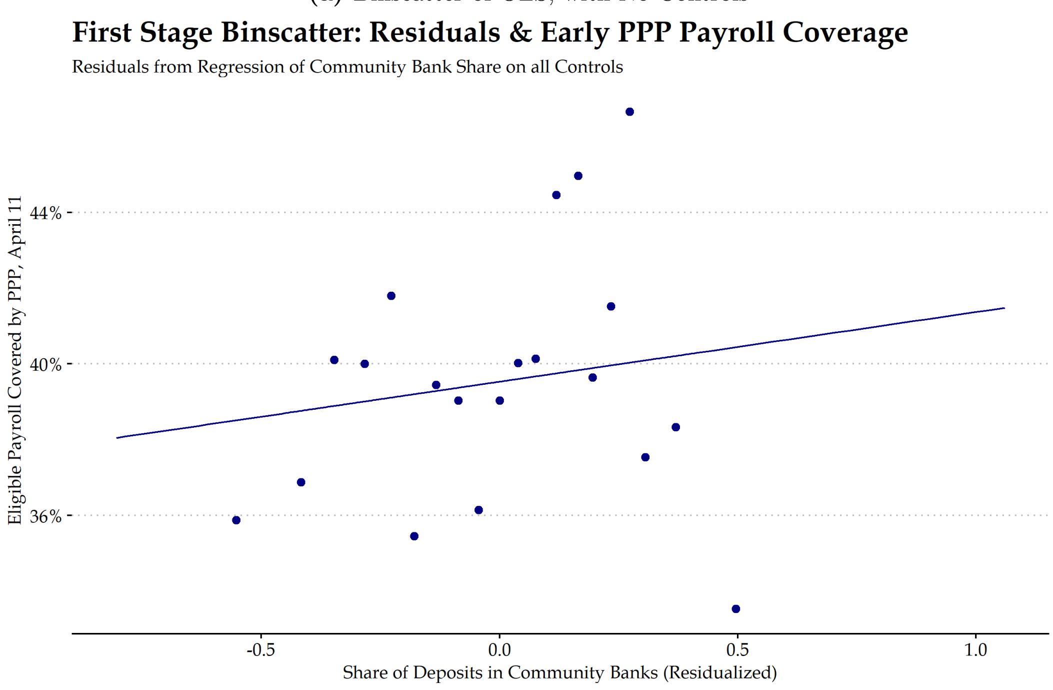
The dynamic results for the second stage coefficient ($\beta_{1,t}$ from equation 1) are plotted in Figure X and presented in Table A.4. The estimated effects of PPP Loans on initial UI claims are at their largest in early-April, when the first loans are approved. Those effects diminish over the next month and a half, and are no longer statistically significant in late-May. By June, the results reflect precisely estimated near-zero effects. Continuing UI claims are naturally slower to change than the initial UI claims. The estimated effect of PPP loans on continuing claims is large and statistically significant in mid-April. As loan saturation increased, the effect on continuing UI claims is reduced, starting in mid-May.

These results correspond to the predictions of our hypothesis: counties which received loans at the program's outset lost fewer jobs in April and early-May. During the second round of funding, PPP adoption became near-universal among small businesses, reducing the scale of those effects, and UI claims rates in counties which received loans early became statistically indistinguishable from claims rates in counties which did not receive loans early. This dynamic is immediately detectable with initial UI claims, and is echoed in continuing UI claims. Convergence in continuing claims may be slower for standard reasons of labor market dynamics; rehiring someone who lost his or her job, even on temporary layoff, is less effective at keeping employment levels high than avoiding job losses at the outset.

The units of the regression are all percentages. If properly identified, we can read the coefficient from the continuing claims regression on April 18th as "a 1 percentage point increase in early payroll coverage by PPP leads to a 0.43 percentage point decrease in continuing UI claims". Likewise, the coefficient from the initial claims regression on the same date can be read as"a 1 percentage point increase in early payroll coverage by PPP leads to a 0.15 percentage point decrease in the number of approved initial claims for state UI programs." Both continuing and initial claims are measured as a fraction of covered employment within the county. Since we study outcomes during the pandemic recession, these effects should be thought of as resulting in smaller jumps in claims, rather than in outright reductions.



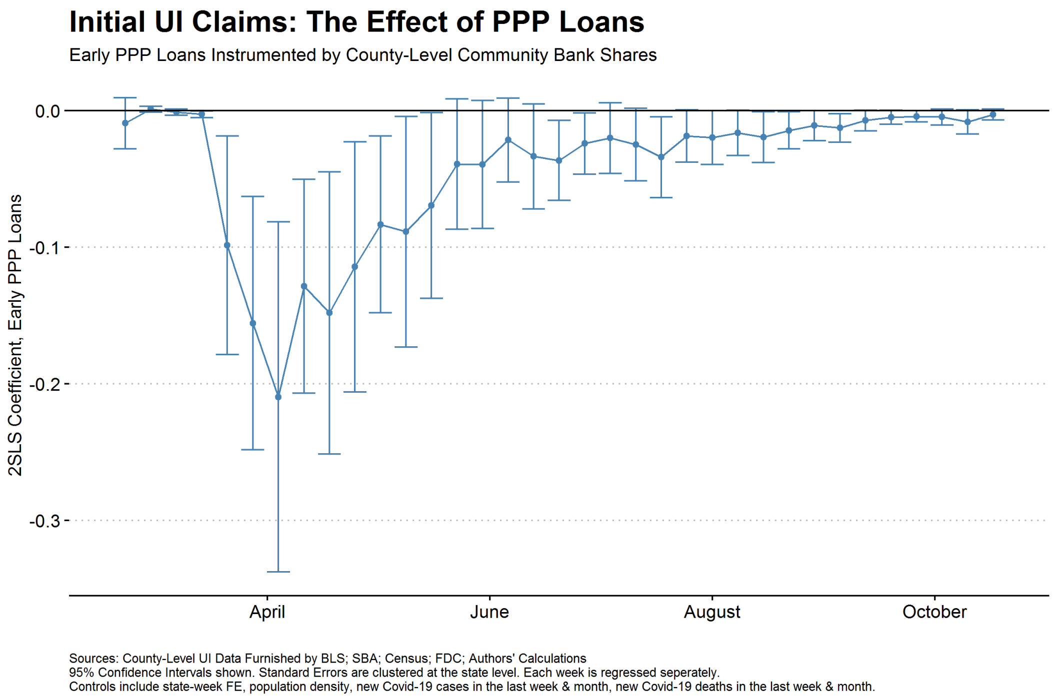
(a) Binscatter of OLS, with No Controls



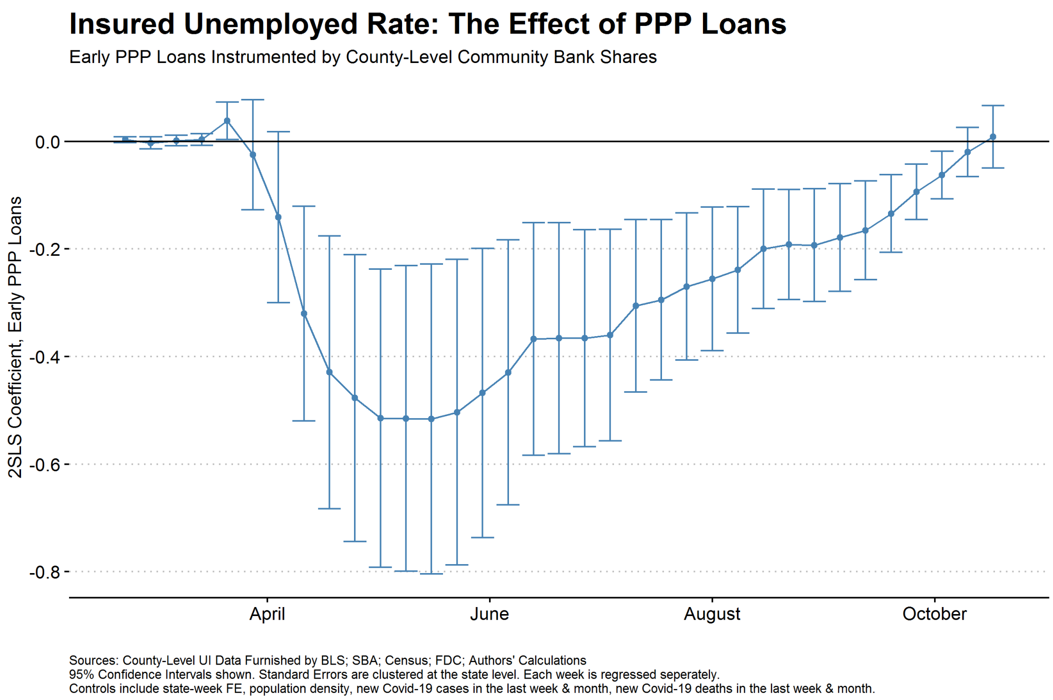
(b) Binscatter of OLS, on Residuals from Regression of Community Bank Share on all Controls

Figure IX

First stage, with and without controls. As of April 11th, community bank market share is strongly correlated with PPP coverage of eligible payroll.



(a) Initial UI Claims

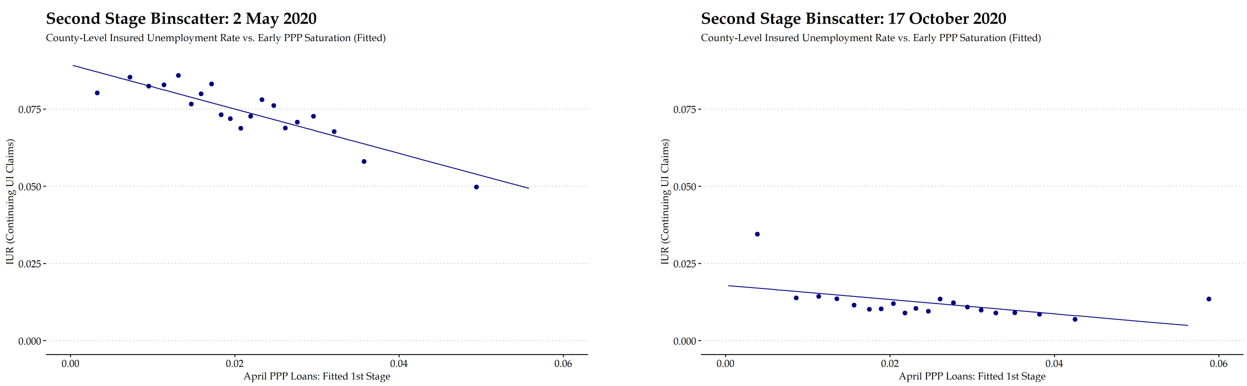


(b) Continuing UI Claims

Figure X

Instrumental variables regression estimates of the effect of PPP penetration as of April 11th on employment outcomes. 95% confidence bands shown. Initial and continuing claims at regular state UI programs are reported as a share of covered employment, i.e. the insured unemployment rate. Regressions include control variables as described above.

To illustrate our "vanishing-effects" hypothesis, Figure XI displays scatter plots of the employment outcome on fitted first-stage values of PPP. This figure's purpose is to illustrate the contrast of early effects against the later absence of an effect. On May 2nd , there is a clear, inverse correlation between county-level loans and county-level IUR; by October 17th, the statistical relationship is much weaker. These results are what was predicted by our hypothesis.



(a) Early Effects

(b) Late Effects

Figure XI

Insured Unemployment Rate vs. fitted first stage values of PPP penetration. Includes control variables. Dots represent averages per bin in 5-percentile increments.

We emphasize that these estimates should be interpreted as a local average treatment effect (LATE). The firms which complied with the treatment are those which received a PPP loan, but would have been less likely to do so if they were domiciled in a county with fewer Community Banks. As we argued in the introduction, the estimates from Autor et al. (2020) and Chetty et al. (2020) should also be interpreted in a similar manner to a LATE. Their identification centered on the eligibility cutoff of 500 employees, and therefore their estimates speak to the effect on mid-sized firms. In contrast, ours speak to the effect on smaller firms in a weaker financial situation. This is an advantage insofar as this group was the PPP's target, and comprised a much larger portion of the PPP's loan recipients.

Extrapolating local estimates to calculate aggregate effects is valid only in particular circumstances, and our study does not satisfy them. However, the same is true for the estimates in Autor et al. (2020) and Chetty et al. (2020). Therefore, we conduct a similar aggregation exercise to offer a comparable result. For a county to move from the 25th percentile to the 75th percentile of early PPP receipt (a swing of PPP penetration from 26.7% of eligible payroll to 51.6% of eligible payroll[12]) would imply a move in the insured unemployment rate of 12.8 percentage points on May 2nd. By contrast, the difference between the 25th and 75th percentiles in observed county unemployment rates was 6.0 percentage points during the same week, suggesting there may be some overstatement in this exercise. Assessed over total national covered employment of 145.7 million[13], moving from the 25th to 75th percentile county leads to a difference of 18.6 million jobs. This number is far closer to our back-of-the-envelope calculations than the numbers furnished in Autor et al. (2020) and Chetty

[12] For the 40 states & D.C. of our sample. For all counties, the interquartile range of April 11th is 23.5% - 51.6%.

[13] NB: "covered employment" here refers to national employment covered by unemployment insurance, not by PPP, since the point estimates refer to unemployment insurance claimant rates. County-level employment outcomes are the unit of observation in our study.

et al. (2020). We focus on moving from the 25th to 75th percentiles because of the problems associated with LATE; the usefulness of the local study in a linear specification breaks down further from the treatment and from the mean. If, per our calculation, PPP saved 18.6 million jobs, this implies the cost per job saved was approximately $28,000, making PPP an extremely cost-effective means of job preservation.

The interpretative exercise relies on an assumption that counties with less community bank market share would have evolved similarly to counties with more community bank market share. Further, it assumes that the estimated LATE is in fact an ATE, which we believe is unlikely. However, we reiterate that this exercise is to offer an aggregate jobs number comparable to Autor et al. (2020) and Chetty et al. (2020). The estimated aggregate jobs saved in those two papers relies on the same assumption about LATEs and ATEs. While the county-level assumption is not applicable, an analogous assumption is required: firms with fewer than 20 employees would evolve similarly to those with around 500 employees; we believe our extrapolation assumption is more reasonable than theirs.

Extrapolated out, these estimates imply that the current 80% PPP penetration rate nationally would reduce unemployment by 41 percentage points. To discourage such extrapolation, we note: 1) this is a global extrapolation from an estimate at the mean and a linear model is obviously not the "true" data-generating process; 2) as above, our estimate can be interpreted as a local average treatment effect, where compliance is determined by preexisting banking relationships, and extrapolation from a LATE to a population is fraught; and 3) since the observations are counties of heterogeneous size and the we have estimated an average treatment effect, an extrapolation to the population of the US is once again unlikely to be linear. Finally, it is worth observing that given the confidence bands around the estimate, a 41 percentage point swing in unemployment is statistically indistinguishable from both Jamie Dimon's estimate above and the 34 percentage point swing we find in the same calculation in state-level data in the Census' Household Pulse in Appendix B.

To summarize, we estimate the employment effects of PPP loans by exploiting variation between counties which received more of their loans in early April, vs. those who received more of their loans in May and later. By using the county-level market share of Community Banks as an instrument for early loan receipt, we avoid the endogeneity issues that arise with both self-selection into loans, as well as lender discrimination based on firm-performance. We show that early PPP loans had a large and statistically significant effect on UI claims in April and May. These effects faded later, as the second-round of PPP funding allowed for near-universal loan coverage, consistent with our hypothesis. We underline that because our study focuses on the dynamic effects of early receipt, any alleged bias in the instrument must be present in April and May, but then disappear.

This empirical strategy serves to estimate the employment effect of loans to all firms in a county, which we highlight as one of this paper's primary contributions. Identification using the 500-employee eligibility cutoff is clean, and has a clear-cut argument in its favor. However, it estimates the impact of loans on firms which were much likelier to have alternate sources of funding, established revenue bases, and a host of other buffers against a harsh downturn. In contrast, our strategy incorporates the effects on small firms, which were more likely to cut jobs and go out of business in the absence of PPP. Most importantly, these firms were both the primary targets of the program, as well as its primary recipients: 96% of all approved PPP loans went to firms with fewer than 50 employees, in contrast with firms of over 250 employees, which received 0.4% of all approved loans. When analyzing the employment effects of the PPP, estimating the employment effects on these smaller firms is of first-order importance.

VI. Conclusion

We have proposed an identification strategy which exploits variation in the timing of PPP loan receipt, and implemented it at the county-level. Such an approach yields evidence for statistically significant, superior labor market outcomes in counties which received PPP funds faster; these advantages fade over time as PPP penetration converges toward 80% everywhere. We calculate that moving from the 25th percentile to the 75th percentile county of PPP penetration leads to an improvement in the unemployment rate of over 12 percentage points, or, extrapolated nationally, roughly 18.6 million jobs, a number much closer to our back-of-the-envelope calculations than those of previous studies.

Given the significantly smaller findings previous papers have documented for firms around the 500-employee cutoff and our much larger findings for the whole sample, these results suggest that smaller firms were likely to have realized greater impact from PPP than larger eligible firms. Such findings are consistent with previous literature documenting that smaller firms are more likely to face financial constraints and be less resilient during economic shocks. Further evaluations of PPP should seek to differentiate the treatment effects across the eligible size distribution.

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Appendix A: Additional Figures

A. Descriptive Statistics & Table of Estimates

TABLE A.1
County-Level Descriptive Statistics at Three Points in Time

	11 April	25 April	8 August
Dependent Variables			
Initial Claims (% of Covered Employment)	1.6%	1.1%	0.3%
	(1.8%)	(1.2%)	(0,4%)
Continuing Claims (% of Covered Employment)	5.4%	7.2%	4.8%
	(4.1%)	(5.0%)	(3.4%)
Instrument and Independent Variables			
Eligible Payroll Covered by PPP Loans (%)	39.2%	58.6%	85.1%
	(22.0%)	(25.7%)	(29.7%)
Community Banks Deposits (% of Total $)	57.5%	57.5%	57.5%
Covid-19 Cases	(33.2%)	(33.2%)	(33.2%)
In the Past Week (per 100K)	31.4	39.8	117.6
	(71.5)	(160.5)	(135.6)
In the Past 4 Weeks (per 100K)	67.9	125.8	476.7
	(163.4)	(287.1)	(472.2)
Covid-19 Deaths			
In the Past Week (per 100K)	1.4	2.0	2.5
	(6.2)	(6.2)	(7.6)
In the Past 4 Weeks (per 100K)	2.5	5.9	8.3
	(9.6)	(18.5)	(16.5)
Population Density (People/Square Miles)	296.5	296.5	296.5
	(1980.5)	(1980.5)	(1980.5)
Insured Unemployment Rate on 15 Feb. 2020	1.1%	1.1%	1.1%
	(1.2%)	(1.2%)	(1.2%)
Number of Counties	2,436	2,436	2,436

All descriptive statistics describe county-level means, with standard deviation in parentheses. Because these statistics describe county-level means, they do not equal national averages. All counties excluded from the core empirical specifications are also excluded from these data. Excluded states are CA, FL, HI, IA, KS, MI, MN, NC, OR, and SD.

TABLE A.2

Community Bank (CB) and Non-Community Bank (Non-CB) Loan Measures at Three Points in Time

	11 April		25 April		8 August	
	CB	Non-CB	CB	Non-CB	CB	Non-CB
Loan Measure						
Number of Loans	455,951	348,911	758,223	861,996	1,592,393	3,619,742
Total Loan $ (Billions)	$83.4	$108.1	$116.4	$203.5	$163.1	$361.9
Jobs Reported (Millions)	8.5	9.2	12.0	17.8	17.4	33.5

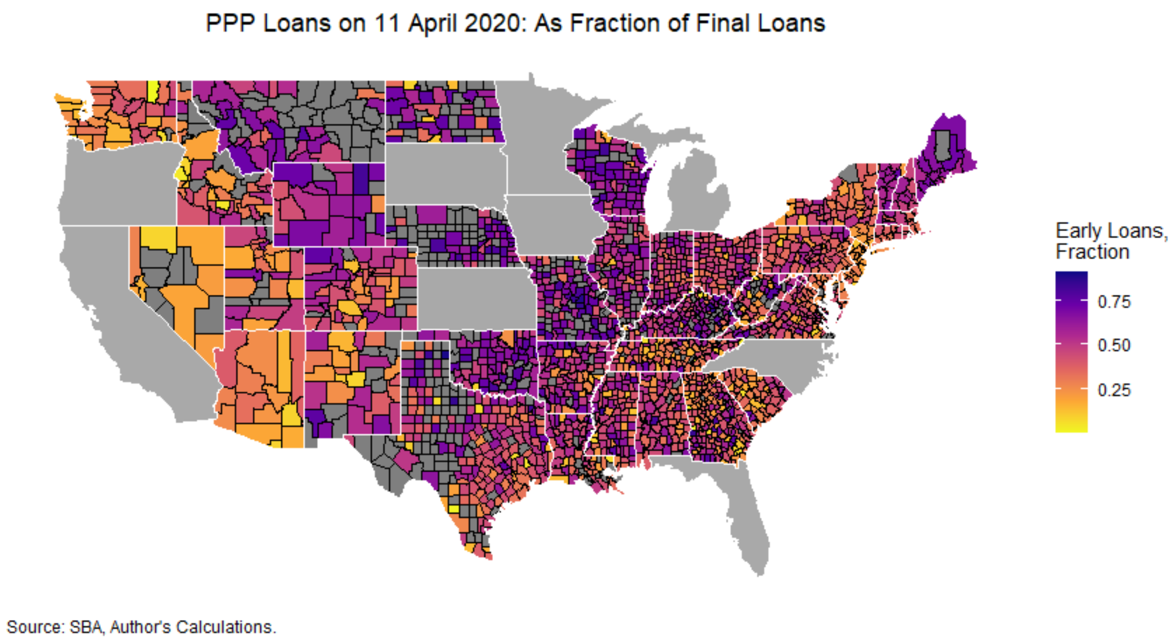


Figure XII

TABLE A.3

OLS Regressions: Employment Outcomes on Cumulative Share of Small Business Payroll Covered by PPP, as of the week of April 11th

	(1) Initial UI Claims	(2) Continuing UI Claims
February 22	-0.0002	0.0000
	(0.0002)	(0.0002)
February 29	-0.0005*	-0.0001
	(0.0002)	(0.0003)
March 7	-0.0002*	-0.0004
	(0.0001)	(0.0004)
March 14	-0.0003***	-0.0006
	(0.0001)	(0.0005)
March 21	0.0007	-0.0007
	(0.0009)	(0.0010)
March 28	0.0017	0.0003
	(0.0023)	(0.0020)
April 4	-0.0024	0.0021
	(0.0041)	(0.0028)
April 11	-0.0025	0.0004
	(0.0030)	(0.0041)
April 18	-0.0037**	-0.0018
	(0.0017)	(0.0059)
April 25	-0.0025**	-0.0057
	(0.0012)	(0.0062)
May 2	-0.0016	-0.0078
	(0.0010)	(0.0059)
May 9	-0.0026	-0.0088
	(0.0017)	(0.0057)
May 16	-0.0008	-0.0108*
	(0.0008)	(0.0058)
May 23	-0.0004	-0.0098*
	(0.0006)	(0.0055)
May 30	-0.0009	-0.0084
	(0.0012)	(0.0053)
June 6	-0.0003	-0.0078
	(0.0006)	(0.0055)
June 13	-0.0004	-0.0065
	(0.0006)	(0.0052)
June 20	-0.0006	-0.0066
	(0.0006)	(0.0048)
June 27	0.0003	-0.0069
	(0.0005)	(0.0045)
July 4	-0.0001	-0.0065
	(0.0005)	(0.0041)
July 11	-0.0006	-0.0059
	(0.0006)	(0.0038)
July 18	-0.0000	-0.0062
	(0.0005)	(0.0038)
July 25	-0.0002	-0.0066*
	(0.0003)	(0.0037)
August 1	0.0001	-0.0065*
	(0.0002)	(0.0033)
August 8	-0.0006**	-0.0064**
	(0.0003)	(0.0029)
August 15	-0.0004	-0.0069**
	(0.0003)	(0.0027)
August 22	-0.0003	-0.0068**
	(0.0003)	(0.0026)
August 29	-0.0000	-0.0066**
	(0.0002)	(0.0025)
September 5	-0.0004*	-0.0056**
	(0.0002)	(0.0025)
September 12	0.0000	-0.0052**
	(0.0002)	(0.0025)
September 19	-0.0003	-0.0044**
	(0.0002)	(0.0021)
September 26	-0.0002	-0.0039*
	(0.0001)	(0.0020)
October 3	-0.0001*	-0.0033
	(0.0001)	(0.0021)
October 10	-0.0004***	-0.0026
	(0.0001)	(0.0024)
October 17	-0.0002	-0.0021
	(0.0002)	(0.0028)
Controls	Y	Y

Notes: Standard errors in parentheses, clustered at the state level. Each week is regressed separately. *,**, and *** represent significance at the 10, 5, and 1% level, respectively.

TABLE A.4

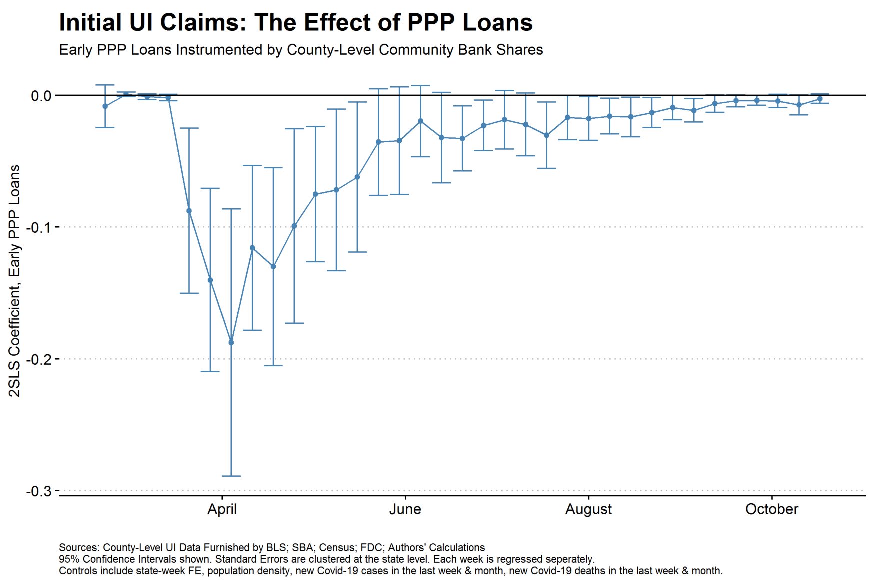
IV Regressions: Early PPP Receipt on Employment Outcomes

	(1)	(2)
	Initial UI Claims	Continuing UI Claims
February 22	-0.009	0.003
	(0.010)	(0.003)
February 29	0.001	-0.003
	(0.001)	(0.006)
March 7	-0.001	0.002
	(0.001)	(0.005)
March 14	-0.003**	0.003
	(0.001)	(0.006)
March 21	-0.099**	0.038**
	(0.041)	(0.018)
March 28	-0.156***	-0.025
	(0.047)	(0.052)
April 4	-0.210***	-0.141*
	(0.065)	(0.081)
April 11	-0.129***	-0.320***
	(0.040)	(0.102)
April 18	-0.148***	-0.429***
	(0.053)	(0.129)
April 25	-0.114**	-0.477***
	(0.047)	(0.136)
May 2	-0.083**	-0.515***
	(0.033)	(0.141)
May 9	-0.089**	-0.515***
	(0.043)	(0.145)
May 16	-0.069*	-0.516***
	(0.035)	(0.147)
May 23	-0.039	-0.503***
	(0.024)	(0.145)
May 30	-0.040	-0.468***
	(0.024)	(0.137)
June 6	-0.021	-0.429***
	(0.016)	(0.126)
June 13	-0.034*	-0.368***
	(0.020)	(0.110)
June 20	-0.036**	-0.366***
	(0.015)	(0.110)
June 27	-0.024**	-0.366***
	(0.011)	(0.103)
July 4	-0.020	-0.360***
	(0.013)	(0.100)
July 11	-0.025*	-0.306***
	(0.014)	(0.082)
July 18	-0.034**	-0.295***
	(0.015)	(0.076)
July 25	-0.019*	-0.270***
	(0.010)	(0.070)
August 1	-0.020*	-0.256***
	(0.010)	(0.068)
August 8	-0.016*	-0.239***
	(0.008)	(0.060)
August 15	-0.019**	-0.200***
	(0.009)	(0.057)
August 22	-0.015**	-0.192***
	(0.007)	(0.052)
August 29	-0.011*	-0.193***
	(0.006)	(0.054)
September 5	-0.013**	-0.179***
	(0.005)	(0.051)
September 12	-0.007*	-0.165***
	(0.004)	(0.047)
September 19	-0.005*	-0.134***
	(0.003)	(0.037)
September 26	-0.004*	-0.094***
	(0.002)	(0.026)
October 3	-0.005	-0.062***
	(0.003)	(0.023)
October 10	-0.008*	-0.020
	(0.005)	(0.023)
October 17	-0.003	0.009
	(0.002)	(0.030)
Controls	Y	Y

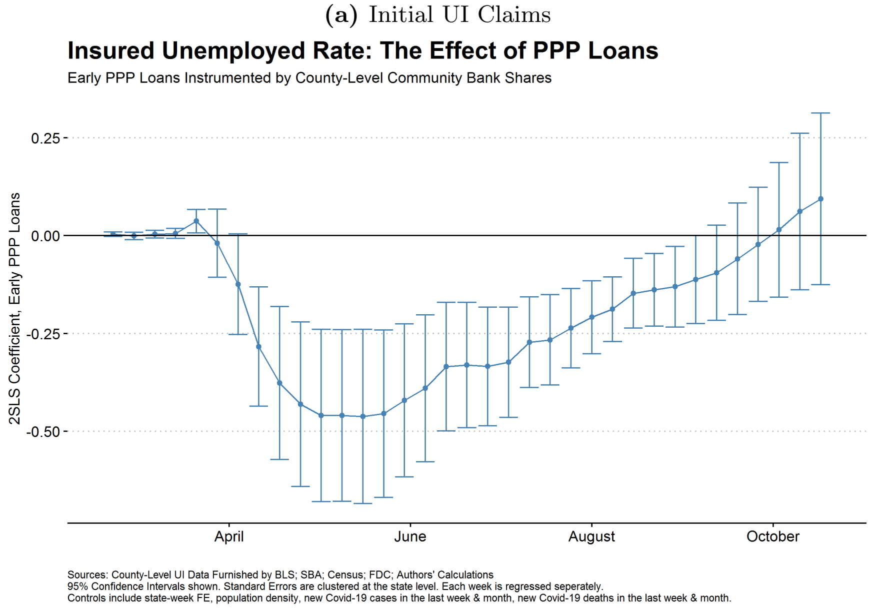
Notes: Standard errors in parentheses, clustered at the state level. Each week is regressed separately. *,**, and *** represent significance at the 10, 5, and 1% level, respectively.

B. IV Estimates with Alternate Definition of PPP Loan Coverage

These figures are constructed identically to those in the main text, but the main right hand side variable is defined not as a share of eligible employment supported by PPP. Instead, the main independent variable is each county's share of its own total loans received by April 11th, i.e. for each county (dollar value of loans made through April 11 / dollar value of loans made through August 8).



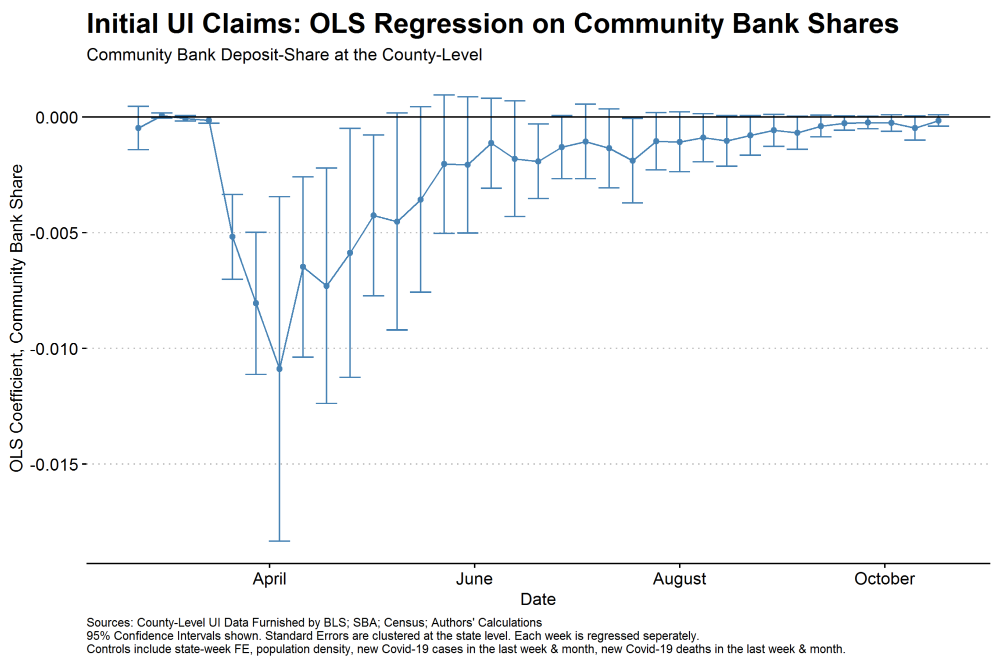
(a) Initial UI Claims



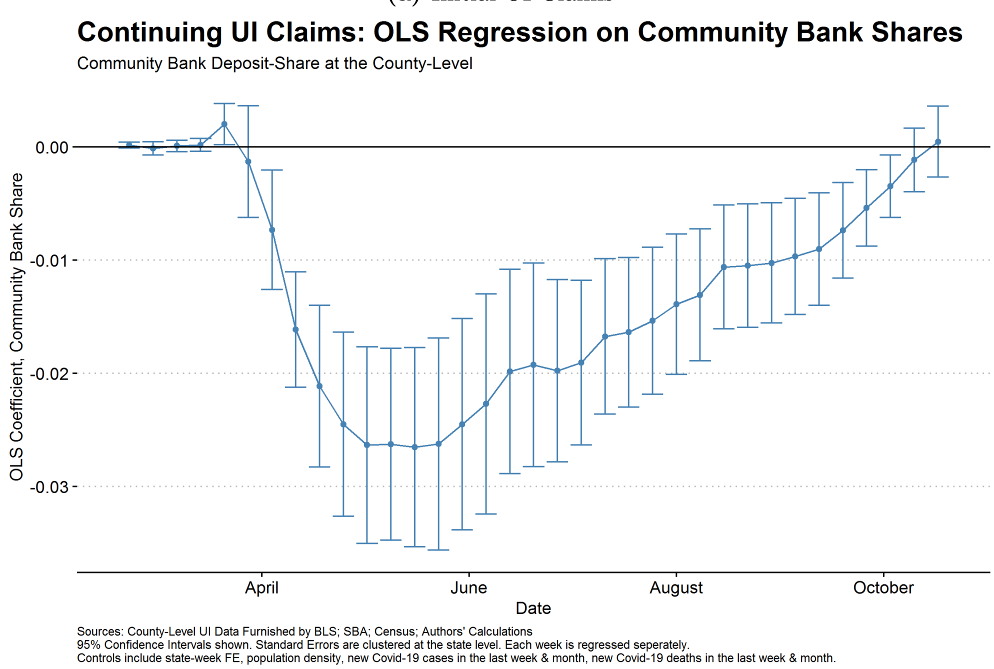
(b) Continuing UI Claims

Figure XIII

C. OLS: UI Outcomes Regressions Directly on Community Bank Shares



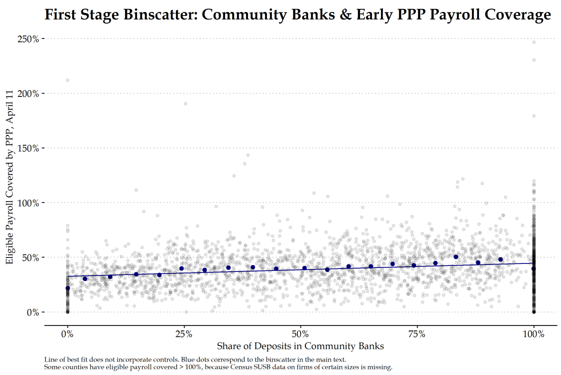
(a) Initial UI Claims



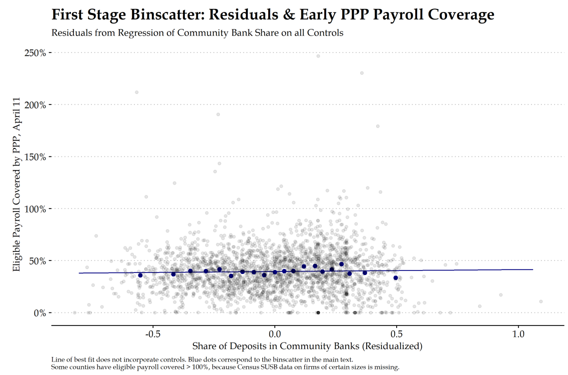
(b) Continuing UI Claims

Figure XIV

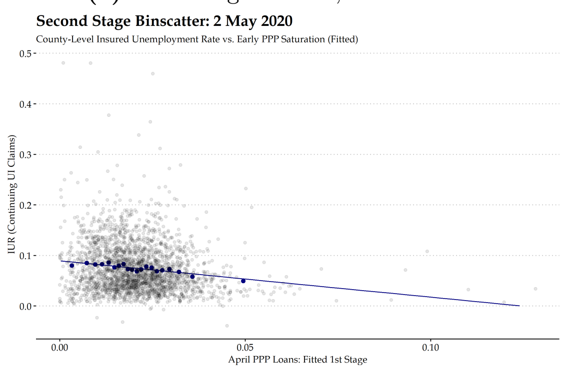
D. Scatter Plots for All Counties (Censored and Uncensored Vertical Axis)



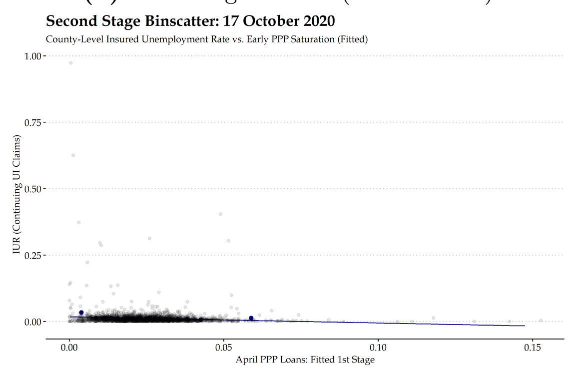
(a) First Stage Scatter, No Controls



(b) First Stage Scatter (Residualized)



(c) Second Stage Scatter, 2 May



(d) Second Stage Scatter, 17 October

Figure XV

Appendix B: State-Level Results in the Census' Household Pulse Survey

In this section we replicate our exploitation of temporal variation in loan receipt at the state level in unemployment claims and in the Census' Household Pulse survey.

In terms of cumulative small business payroll covered by PPP loans, we observe the greatest state-level variation early in the program's first round. By a variety of measures, the most dispersion we see in PPP coverage occurs during the week of April 11th, shortly after the program begins and when the first round of funds was nearly exhausted. Note that the program was then closed for ten days, resulting in a delay before further funds were disbursed, and allowing us to estimate employment variation. For the first stage, we isolate this week in order to measure early receipt of PPP loans due to variation in banking relationships.

In OLS form, our primary specification is repeated cross-sections of

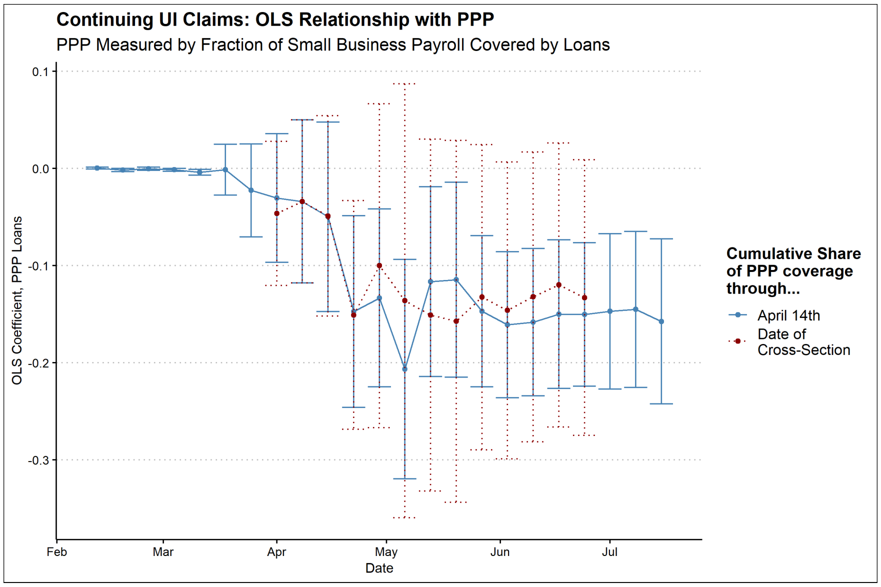
$$Y_{st} = \beta_{0,t} + \beta_{1,t} PPP_{st'} + X'_{st}\beta_{2,t} + \epsilon_{st} \tag{3}$$

where Y_{st} is an employment outcome in state s during week t, and X_{st} is a vector of controls. Our key variable of interest, PPP'_{st}, is the cumulative percentage of small business payroll covered by a PPP loan, as of t' = April 11th, 2020. We estimate this regression separately for each week in order to illustrate the dynamic effects of early loan receipt. As in the main text, t' is held constant in each regression, while t varies, so the regression studies the dynamic relationship of employment with early PPP receipt. The treatment is not that a state received PPP money, but that it received its PPP money early; and the subsequent convergence of outcomes across states is the core of our hypothesis.

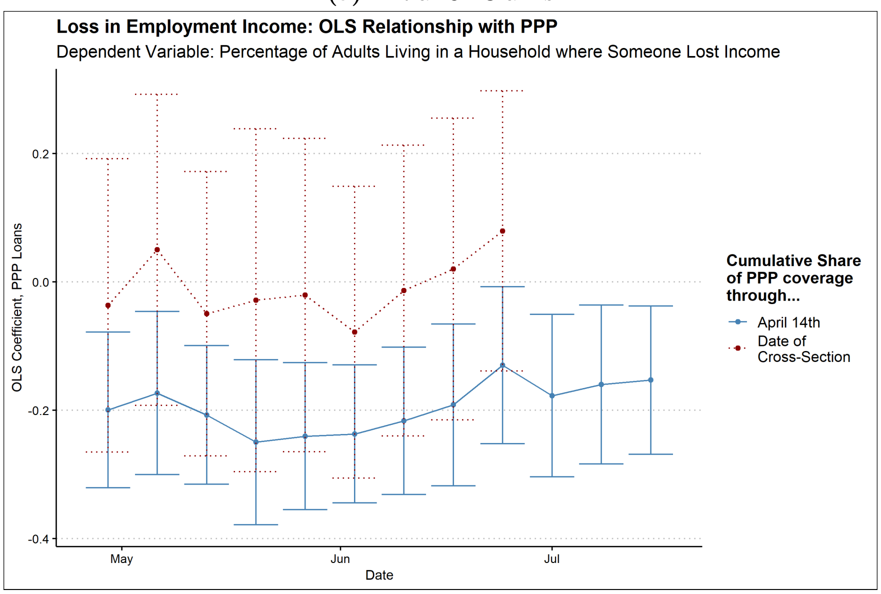
For outcomes, we study the insured unemployment rate at regular state programs (i.e. continuing claims relative to covered employment), to avoid the double counting problems involved in the Pandemic Unemployment Assistance program, as well as to capture high frequency reentry into the workplace. We also study the Census' Household Pulse Survey, which asks whether adults live in a household which has lost employment income during the pandemic.

The control vector consists of the new Covid-19 cases reported in state s during the week, as well as the insured unemployment rate for the week of February 5th, 2020. We include these controls due to concerns that our instrument is correlated with the virus prevalence and pre-Covid unemployment conditions. As Granja et al (2020) notes, first-round PPP loans tended to flow to states that were not as hard hit by the pandemic.

Critical endogeneity and selection concerns remain in this OLS regression. For example, we might be concerned that businesses in higher distress would be more likely to apply for loans. Alternatively, we might be concerned that businesses with stronger management will be quicker to apply for loans, or banks might be more inclined to deal with stronger businesses, regardless of government assurances. Nonetheless, we present OLS results before turning to our IV results. Figures XVI and XVII present results from an ordinary least squares estimation of Equation 3. There is a weak and statistically insignificant benefit to receiving early PPP loans.



(a) Initial UI Claims



(b) Continuing UI Claims

Figure XVI

OLS estimates of UI Continuing claims and Census household pulse employment income expectations, regressed on early PPP receipt, with control variables. Continuing claims are measured as a share of covered employment, at regular state UI programs, i.e. the insured unemployment rate. 95% confidence intervals displayed.

	(1)	(2)	(3)	(4)
	Continuing UI Claims		Loss in Employment Income	
8 April	-0.091*	-0.034	N/A	N/A
	(0.047)	(0.043)		
15 April	-0.095*	-0.049	N/A	N/A
	(0.055)	(0.053)		
22 April	-0.180***	-0.151**	N/A	N/A
	(0.063)	(0.060)		
29 April	-0.164*	-0.100	-0.084	-0.050
	(0.085)	(0.085)	(0.108)	(0.118)
6 May	-0.147	-0.136	0.030	0.036
	(0.107)	(0.114)	(0.112)	(0.123)
13 May	-0.177*	-0.151	-0.027	-0.050
	(0.089)	(0.092)	(0.105)	(0.114)
20 May	-0.182**	-0.157	-0.055	-0.036
	(0.090)	(0.095)	(0.124)	(0.136)
27 May	-0.158**	-0.133	-0.012	-0.017
	(0.076)	(0.080)	(0.113)	(0.125)
3 June	-0.179**	-0.146*	-0.110	-0.077
	(0.074)	(0.078)	(0.106)	(0.115)
10 June	-0.155**	-0.132*	-0.050	-0.009
	(0.073)	(0.076)	(0.108)	(0.114)
17 June	-0.158**	-0.120	-0.013	0.018
	(0.073)	(0.075)	(0.114)	(0.120)
24 June	-0.171**	-0.133*	0.046	0.072
	(0.071)	(0.072)	(0.107)	(0.111)
Controls	N	Y	N	Y

Notes: Standard errors in parentheses. *,**,*** represent significance at the 10, 5, and 1% level, respectively. Each coefficient is estimated in a separate regression.

Figure XVII

OLS regressions of employment outcomes on early PPP receipt.

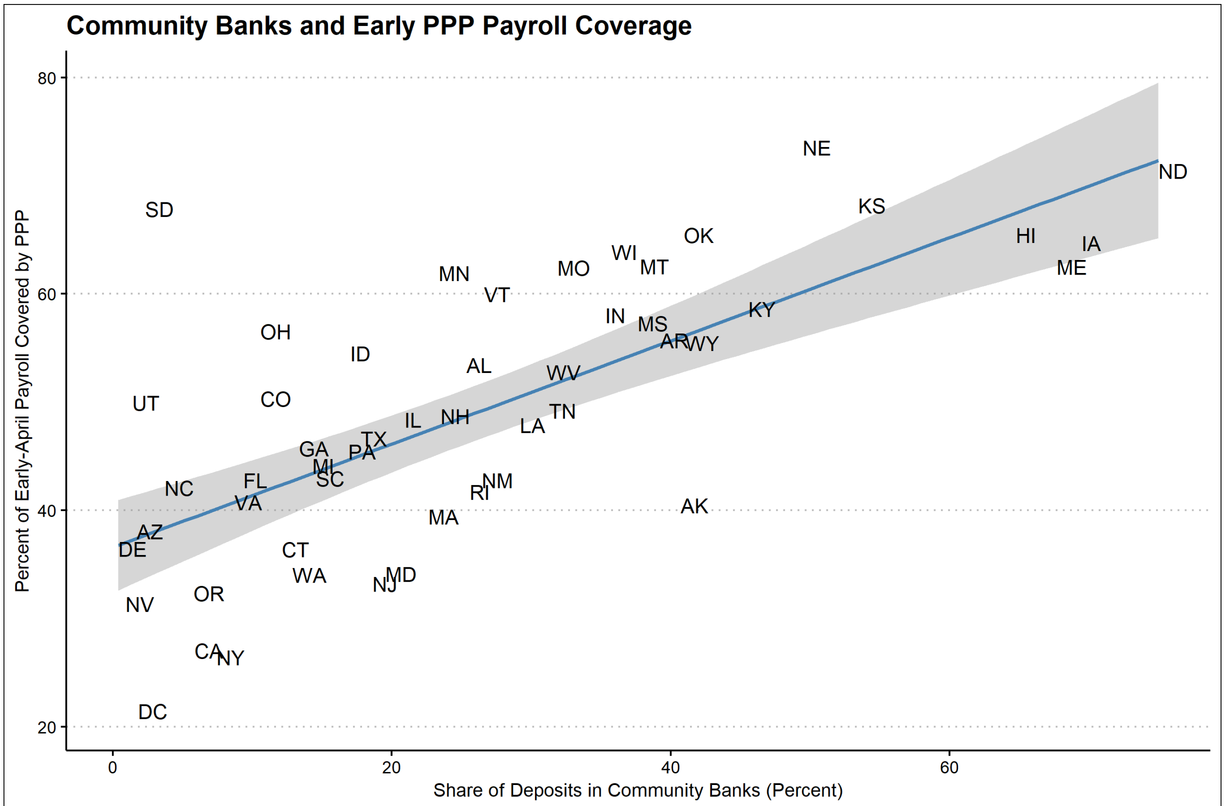


Figure XVIII

First stage, no controls. As of April 14th, community bank market share is strongly correlated with PPP penetration. 95% confidence band displayed.

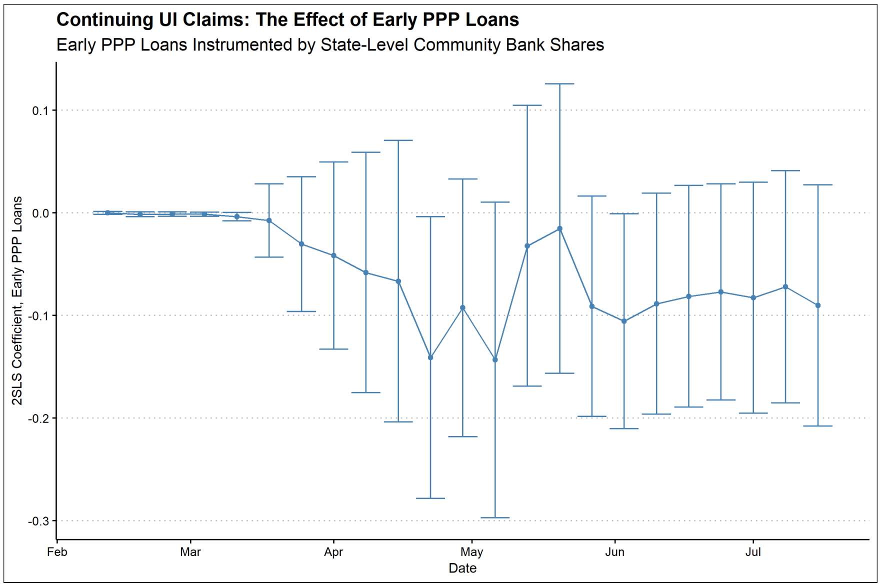
Due to the endogeneity concerns just discussed, we instrument $PPP_{st'}$ with the share of state-level deposits held in community banks. We model the endogenous variable as

$$PPP_{st'} = \alpha_{0,t} + \alpha_{1,t}\text{CB\_Share}_s + X'_{st}\alpha_{2,t} + \eta_{st} \tag{4}$$

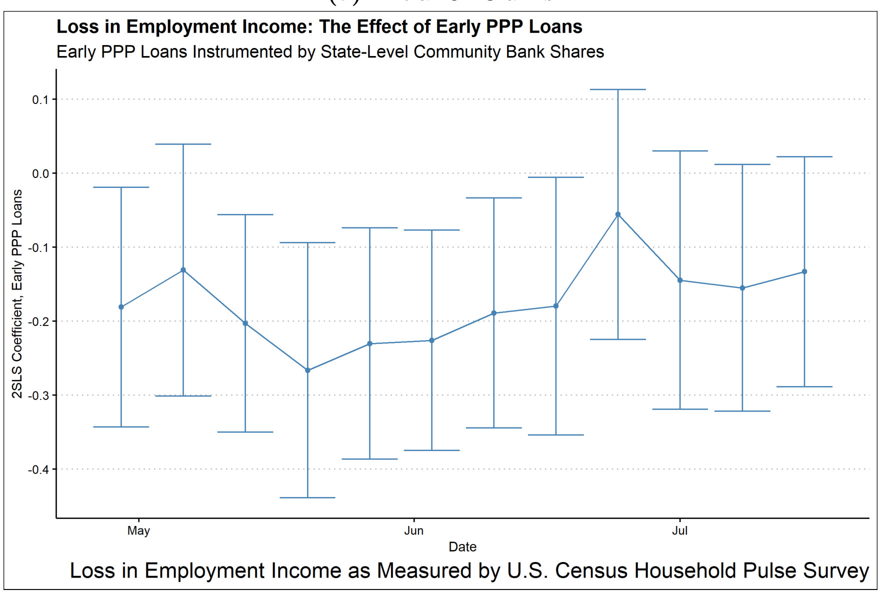
Figure XVIII shows the plot of this first stage regression, estimated with no controls for exposition. The F-Stat for this no-controls first stage is 50.6. With controls included, the F-Stat moderately declines from 26.5 in early-April to 23.7 in early-May, then remains relatively consistent.

The dynamic results for the second stage coefficient ($\beta_{1,t}$ from equation 3) are plotted in Figure XIX and presented in Figure XX. The estimated effects, both in reducing continuing UI claims and reducing loss in employment income, grow stronger until late-April, then start to attenuate in mid-May. While the effects in UI data are not statistically significant, the effects in the Census Pulse survey are. Early PPP loan receipt, as instrumented by community bank shares, leads to lower unemployment rates and less loss in labor income during April and May. During the second round of funding, PPP adoption became near-universal among small businesses, reducing the scale of those effects. While the Census Pulse survey provides some evidence for our hypothesis that there would be strong initial effects of early PPP receipt which would then fade as PPP saturated the small business economy, that evidence is in the form of diminishing statistical significance over time and would be clearer in the form of precisely estimated zeroes. We do note, however, that this is a dataset of 51 observations, so diminishing significance as the treatment grows distant in time is unsurprising.

Why are there no significant results in the UI data at the state level, when we do find some at the county level in the main text? First, as mentioned, there are only 51 observations here, prior expectations should be dim for finding anything at the state level. Second, for the reasons discussed in Cajner et al. (2020), there are many reasons why ordinary UI claims data might be much noisier now than in normal times, and since these programs are administered at the state level, this variation will be correlated with the state fixed effects we control for at the county-level but cannot control for at the state level.



(a) Initial UI Claims



(b) Continuing UI Claims

Figure XIX

Instrumental variables regression estimates of the effect of PPP penetration as of April 14th on employment outcomes. 95% confidence bands shown. ”Loss in income” corresponds to the Pulse ”Percentage of adults in households where someone had a loss in employment income.” Continuing claims at regular state UI programs are reported as a share of covered employment, i.e. the insured unemployment rate. Regressions include control variables as described above.

	(1) Continuing UI Claims	(2) Loss in Employment Income
12-Feb	0	N/A
	-0.001	
19-Feb	-0.002	N/A
	-0.001	
26-Feb	-0.001	N/A
	-0.001	
4-Mar	-0.001	N/A
	-0.001	
11-Mar	-0.004*	N/A
	-0.002	
18-Mar	-0.008	N/A
	-0.018	
25-Mar	-0.031	N/A
	-0.033	
1-Apr	-0.042	N/A
	-0.047	
8-Apr	-0.058	N/A
	-0.06	
15-Apr	-0.067	N/A
	-0.07	
22-Apr	-0.141**	N/A
	-0.07	
29-Apr	-0.093	-0.181**
	-0.064	-0.083
6-May	-0.143*	-0.131
	-0.078	-0.087
13-May	-0.032	-0.203***
	-0.07	-0.075
20-May	-0.015	-0.266***
	-0.072	-0.088
27-May	-0.091	-0.230***
	-0.055	-0.08
3-Jun	-0.106*	-0.226***
	-0.053	-0.076
10-Jun	-0.089	-0.189**
	-0.055	-0.079
17-Jun	-0.081	-0.180**
	-0.055	-0.089
24-Jun	-0.077	-0.056
	-0.054	-0.086
1-Jul	-0.083	-0.145
	-0.057	-0.089
8-Jul	-0.072	-0.155*
	-0.058	-0.085
15-Jul	-0.09	-0.133*
	-0.06	-0.079
Controls	Y	Y

Notes: Standard errors in parentheses. *,**,*** represent significance at the 10, 5, and 1% level, respectively.

Figure XX
IV estimates of early PPP receipt effect on employment outcomes.

The units of the regression are all percentages. If properly identified, we can read the coefficient from the continuing claims regression on April 22nd as "a 1 percentage point increase in early payroll coverage by PPP leads to a 0.14 percentage point decrease in continuing UI claims". Likewise, the coefficient from the loss in employment income regression on May 20th can be read as "a 1 percentage point increase in early payroll coverage by PPP leads to a 0.27 percentage point decrease in the number of adults in households reporting employment income lost during the pandemic."

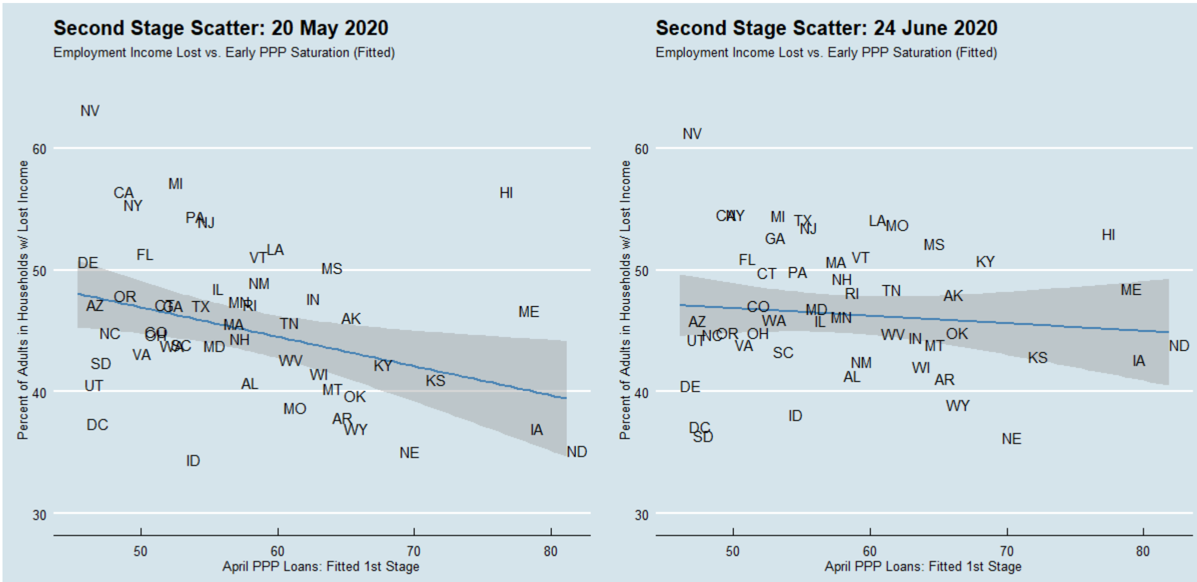


Figure XXI

Percentage of adults in households which lost income vs. fitted first stage values of PPP penetration. Includes control variables. Note: the confidence bands in this chart are for two-dimensional representations of a multivariate analysis, and therefore suffer a generated regressor problem, i.e. the standard errors are biased downward and the error bands are misleadingly tight. The standard errors and confidence bands in Figure XIX and XX are calculated using two-stage least squares in repeated cross sections.

To interpret these results, consider that a 10% increase in PPP receipt would reduce the number of adults in households experiencing income loss by 2.7%. If, as Chetty et al. (2020) argue, the vast majority of reduction in employment is at the extensive margin, that is equivalent to a change in the unemployment rate of 4.2 percentage points, since only 63% of adults participated in the labor force pre-COVID.

We reiterate that these estimates should be interpreted as a local average treatment effect (LATE). The firms which complied with the treatment are those which received a PPP loan, but would have been less likely to if they were domiciled in a state with fewer Community Banks. As we argued in the introduction, the estimates from Autor et al. (2020) and Chetty et al. (2020) should also be interpreted in a similar manner to a LATE. Their identification centered on the eligibility cutoff of 500 employees, and therefore their estimates speak to the effect on mid-sized firms. In contrast, ours speak to the effect on smaller firms in a weaker financial situation. This is an advantage insofar as this group was the PPP's target, and comprised a much larger portion of the PPP's loan recipients.

Extrapolating local estimates to calculate aggregate effects is valid only in particular circumstances, and our study does not perfectly satisfy them. However, the same is true for the estimates in Autor et al. (2020) and Chetty et al. (2020). Therefore, we conduct a similar aggregation exercise to offer a comparable result. For a state to move from the 25th percentile to the 75th percentile of early PPP receipt (a swing of PPP penetration from 40% of eligible payroll to 57% of eligible payroll) would imply a move in the unemployment rate of 7.2 percentage points. By contrast, as of the May 12th survey week, the difference between the 25th and 75th percentiles in observed state unemployment rates was 5.0 percentage points. Assessed over the civilian noninstitutional population twenty years and older of 244 million, moving from the 25th to 75th percentile state leads to a difference of 11 million jobs. This analysis assumes that layoffs are distributed one per household; if multiple people per household lost jobs, the estimate would be biased upward. The interpretative inference relies on an assumption that states with less community bank market share would have evolved similarly to states with more community bank market share.

Extrapolated out, these estimates imply that the current 80% PPP penetration rate nationally would reduce unemployment by 34 percentage points. To discourage such extrapolation, we note: 1) this is a global extrapolation from an estimate at the mean and a linear model is obviously not the "true" data-generating process; 2) this estimate can be interpreted as a local average treatment effect, where compliance is determined by preexisting banking relationships, and extrapolation from a LATE to a population is fraught; 3) since the observations are states of heterogeneous size and the we have estimated an average treatment effect, an extrapolation to the population of the US is once again unlikely to be linear; and 4) with such large extrapolations, it is unlikely the assumption of one layoff per household would continue to hold. Finally, it is worth observing that given the confidence bands around the estimate, a 34 percentage point swing in unemployment is statistically indistinguishable from Jamie Dimon's estimate above.

To consider the potential endogeneity of the instrument, in Figure XXII we contrast pre-COVID-19 trends in the top tercile of states by community bank market share with the bottom tercile. While the trends are not parallel before COVID-19, it is the low-community bank shares which are outperformers. Thus our instrument is not picking up prior outperformance, and the pre-existing trends concern (though there are others, mostly around regulatory flexibility) would seem to bias our main estimates downward.

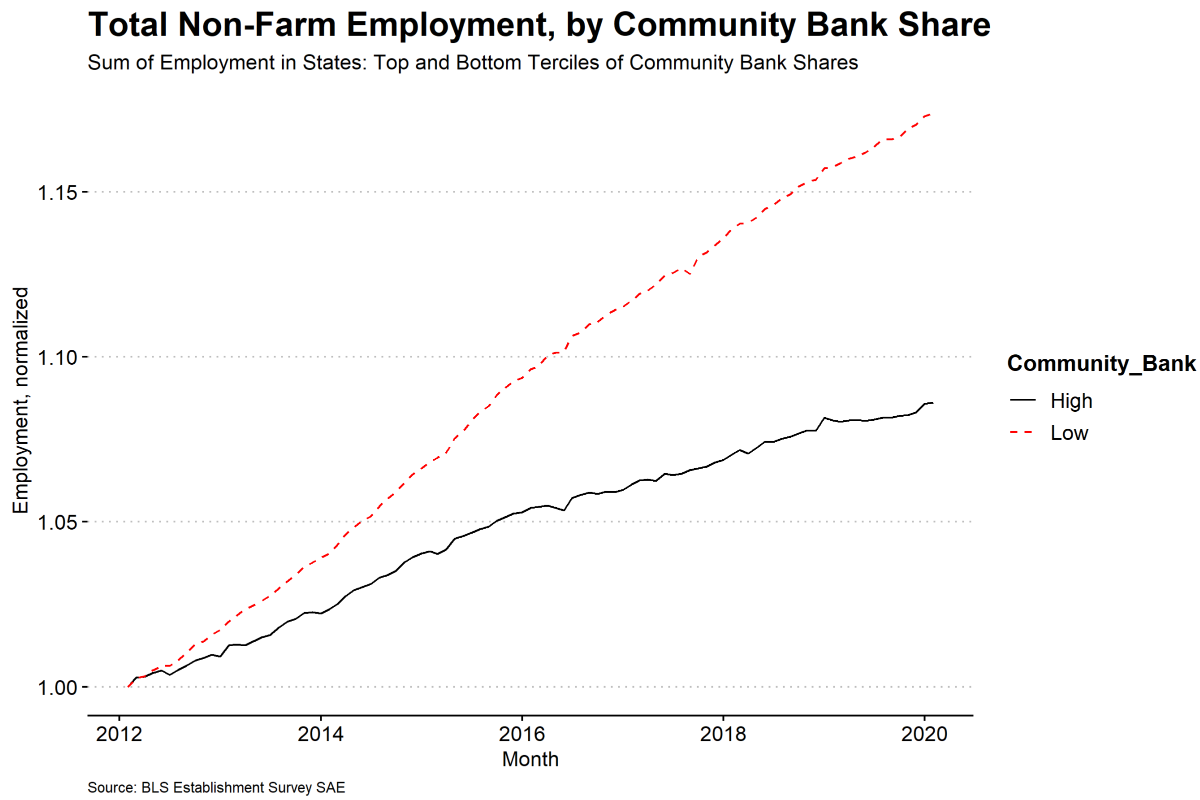


Figure XXII
Pre-COVID employment trends by community bank market share, by tercile.