HARVARD Kennedy School
MOSSAVAR-RAHMANI CENTER
for Business and Government

Jobs Saved by the Paycheck Protection Program (PPP): The Importance of Smaller Loans, Flexible Program Requirements, and Targeting

Winner, 2021 Dunlop Undergraduate Thesis Prize

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June 2021

M-RCBG Associate Working Paper Series | No. 167

Mossavar-Rahmani Center for Business & Government
Weil Hall | Harvard Kennedy School | www.hks.harvard.edu/mrcbg

Jobs Saved by the Paycheck Protection Program (PPP):
The Importance of Smaller Loans, Flexible Program Requirements, and Targeting

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Presented to the Department of Economics
in partial fulfillment of the requirements
for a Bachelor of Arts degree with Honors

Harvard College
Cambridge, Massachusetts
March 11, 2021

Abstract

The Paycheck Protection Program (PPP) was an expedient, ambitious, and unprecedented policy response to the economic fallout of COVID-19 that worked to keep small businesses afloat and minimize layoffs. Empirical work on the PPP's ability to translate loans into employment, however, is mixed. I utilize a novel instrumental variable research design whereby I exploit both geographical heterogeneity in the distribution of Small Business Development Centers and the timing of the PPP. I find that a one standard deviation increase in 1st round PPP below $150,000 had a substantial impact on jobs, saving 64.9%, 33.6%, and 41.9% of the average jobs lost in the short, medium, and long run. This highlights the importance of smaller loan sizes. After the PPP Flexibility Act, which loosened requirements for loan forgiveness, additional PPP saved 97.9% of jobs lost. This implies the importance of flexible program requirements. Finally, I find even greater employment effects in counties with more exposure to COVID-impacted industries: additional PPP in counties with above-median Food and Accommodation sector exposure saved 117% of average jobs lost. This implies the importance of targeting, as the PPP had a magnified impact in areas with greater exposure to industries hit hardest by the pandemic.

Acknowledgements

I am incredibly thankful for my advisor, Dr. Gregory Bruich, for his generous support, insightful and detailed feedback, countless hours spent on Zoom answering my questions, guidance, encouragement, and mentorship. I am so blessed to have been his advisee and am forever grateful for his dedication and kindness in these challenging pandemic circumstances. I also thank my tutorial leader, Giorgio Saponaro, for his thoughtful consideration, feedback, and support throughout this whole process. I thank Harris Eppsteiner and Professor Nathan Hendren, who devoted time and careful thought to reviewing my drafts even beyond my time as their students. I am grateful for my classmates in my thesis seminar, who listened to my many thesis presentations, asked questions, and offered helpful suggestions at every Zoom meeting. Lastly, I am so thankful for my parents and my younger sister, Sera, for their unconditional love, concern, and support as I wrote my thesis from their living room during this pandemic.

1 Introduction

"It's like building an airplane while it's flying" (Kish, 2020). The Paycheck Protection Program (PPP) was an expedient, ambitious, and unprecedented policy response to the economic fallout of COVID-19. When the COVID-19 pandemic shut down major sectors of the American economy, Congress enacted the Coronavirus Aid, Relief and Economic Security (CARES) Act, a \$2.2 trillion stimulus bill to provide relief to the economy. The CARES Act was the largest economic stimulus package in U.S. history, totaling almost 10% of U.S. gross domestic product in 2019. The PPP was its largest single component — with a \$669 billion budget, it alone approaches the size of the 2009 Recovery Act to the Great Recession. The PPP allowed small businesses, defined by the U.S. Small Business Administration (SBA) as firms with under 500 employees, with slight variations based on industry, to apply for low-interest loans to pay for payroll and other operating costs. Firms could borrow approximately 2.5 times their average monthly payroll costs, up to a maximum of \$10 million. A key provision of the PPP was that the entire loan could be forgiven if firms kept 75% (later changed to 60%) of its workers on payroll. In doing so, the PPP intended to "preserve the productive capacity of the small business sector" and "support labor demand over the medium term" (Hubbard and Strain, 2020).

To distribute funds quickly, the U.S. Treasury designated financial institutions to distribute the loans. Small businesses could apply through any existing SBA 7(a) lender or financial depository institution participating in the program. However, having banks deliver and mediate the PPP loans raised key targeting concerns, as bank lending behavior did not always meet underlying loan demand (Granja, Makridis, and Zwick, 2020). Businesses most in need did not necessarily receive PPP loans. Moreover, some small businesses were unable to even apply for loans, as PPP requirements were confusing and overly demanding for resource-constrained firms struggling to simply stay afloat. In response, in June 2020 Congress approved the PPP Flexibility Act, which loosened a variety of requirements for applications and loan forgiveness. Still, the effectiveness of the program is debated: empirical work on its employment effects is mixed, and much media coverage has been on the program's apparent inability to provide useful aid to small businesses in need. Whether the PPP went to the right sectors of the economy is doubtful as well. S&P Global studies reported that "59% of first-round PPP loan approvals went to industries with jobs less affected by social distancing, such as good-producing industries" (Bovino, 2020).

This paper estimates the causal impact of PPP loans under $150,000 on employment. I utilize a novel instrumental variable research design, using counties' geographical exposure to the SBA's Small Business Development Centers (SBDCs), interacted with a dummy for the timing of the PPP, as the instrument. One major concern is that there may be differences in characteristics of areas with SBDCs that lead them to be differentially impacted by COVID-19. I find, however, that the distribution of SBDCs is uncorrelated with COVID cases, consumer spending, state business closure, and stay at home orders over time.[1] Additionally, I present several falsification exercises to support my research design.[2] I use data from the SBA, high-frequency employment data from Opportunity Insights' Economic Tracker, and loan data from the U.S. Treasury Department.

First, I find that counties with more small business development centers (SBDCs) received more PPP funds, suggesting that support from SBA resource partners was a key determinant of small businesses' PPP loan take-up. Second, I find substantial positive relationships between PPP loans and employment in the short, medium, and long run for counties

[1] I thank Nathan Hendren for encouraging me to develop this point and discuss it further in Section 4.2.
[2] See Section 4.2.

that received 1st round PPP loans (distributed in the first two weeks of the program) under $150,000. A one standard deviation increase in 1st round PPP loans below $150,000 saved 64.9%, 33.6%, and 41.9% of the average jobs lost across all counties in the short, medium, and long run, respectively. This finding reveals the short, medium, and long term effectiveness of smaller amounts of PPP in boosting employment. Third, additional PPP raised employment even further for counties receiving PPP after the passing of the PPP Flexibility Act, which loosened several program requirements and loan forgiveness stipulations. Indeed, PPP post-Flexibility Act saved 97.9% of average jobs lost, suggesting the importance of increased program flexibility. Finally, the PPP had an even greater impact on employment in counties with above-median exposure to the Food and Accommodation sector: in these counties, additional PPP saved 117% of average jobs lost. The employment effects are magnified further within Food and Accommodation's two subsectors, 1) Food Services and Drinking Places and 2) Accommodation. The PPP was more impactful for counties exposed to industries especially impacted by COVID-19, highlighting the importance of targeting given the program's effectiveness in especially affected areas.

The remainder of the paper is as follows: Section 2 discusses the background, Section 3 describes the data, Section 4 presents the empirical strategy, Section 5 discusses results, Section 6 presents discussions and extensions, and Section 7 concludes.

2 Background

Work on the PPP fits more broadly into the impact of fiscal policies in times of economic crisis. The traditional Keynesian view holds that aggregate demand stimulus will help restore economic output during downturns. In times of severe crisis, economists have argued fiscal policy is the best way to restore the drop in demand (Feldstein, 2009). Without fiscal stimulus, the massive spending contraction experienced in the 2008 financial crisis, for instance, would be unlikely to be reversed, causing further cuts in production, employment, and earnings, further reducing consumer spending (Feldstein, 2009). Following the Great Depression, economists attributed almost 90% of the initial recovery to fiscal policy innovations (Gordon and Krenn, 2010). Keynesian multipliers are also held to be larger in times of economic crisis, increasing the effectiveness of fiscal policy. Empirical work on OECD countries found that GDP multipliers of government purchases are indeed larger in recession (Auerbach and Gorodnichenko, 2011).

The PPP's impact on aggregate demand is not immediately evident, however, because it is not a typical fiscal stimulus program that makes use of the multiplier. In fact, the PPP can be thought of as a revenue replacement program intended to keep credit-constrained businesses open and stimulate aggregate supply. The COVID-19 economic fallout stopped workers from going to their jobs, limiting the supply of goods and services. By directing funds to small businesses and their payroll costs, the PPP thus worked to replace revenues and mitigate the impacts of the supply shock espoused by the pandemic. Standard intuitions and frameworks for fiscal policy analysis do not necessarily apply, leaving much room for debate in reviewing the PPP.

2.1 The Paycheck Protection Program

Congress allocated $349 billion to the PPP's new lending capacity on March 27, 2020 to provide small businesses impacted by the pandemic with loans with favorable terms. The program had two primary goals: first, to help small businesses cover their short term operating expenses, and second, to incentivize workers to keep employees on payroll (Lettieri and Lyons, 2020). As a partial revenue replacement program, the PPP aimed to keep businesses afloat and prevent mass layoffs. The PPP came with several requirements to ensure that the right businesses received loans and the funds were being used in accordance with the program's core goals. Businesses were eligible to apply for the PPP if they had 500 employees or less, though the number could be higher or lower depending on the industry. The maximum loan size was equivalent to 250 percent of average monthly payroll costs, or a $10 million maximum. PPP loans were then entirely forgivable if firms were able to spend at least 75% of loans on payroll. This condition, known as the "75/25 rule," was later relaxed under the PPP Flexibility Act, which, among other stipulations, loosened the policy to 60% of loans required to be spent on payroll. To receive loan forgiveness, firms also needed to keep the average number of full-time employees at least as high as pre-crisis levels. Wage rates of employees could not fall below 75 percent of pre-crisis levels. These program participation and loan forgiveness requirements, however, continued to evolve over time.

2.2 The PPP's Shortcomings & The PPP Flexibility Act

Since its inception, the PPP has been criticized for its implementation blunders. The program first came under fire when it was revealed that resource-filled, large publicly traded companies like Shake Shack and Ruth's Hospitality Group were approved loans (Simon and Rudegeair, 2020). In December 2020, new data released by the U.S. government in response to a Freedom of Information Act request and lawsuit reported that more than half of PPP funds went to bigger businesses, and only 28 percent of the money was distributed in amounts less than $150,000 – a marked difference from the original claim by the SBA that over 80% of loans were to small businesses (O'Connell et al., 2020). It seemed that the program – supposedly designed for small businesses – instead helped larger businesses with more resources. Moreover, it was reported that the PPP also failed to help businesses most impacted by COVID-19. One empirical study reported a low correlation between regional PPP funds and COVID-19 shock severity (Granja et al., 2020). Another survey even showed that "high impact" firms were more likely to be denied PPP loans (Bartik et al., 2020). Industries most impacted by the pandemic seemed to not be supported as well – S&P Global reported that "59% of first-round PPP loan approvals went to industries with jobs less affected by social distancing, such as good-producing industries" (Bovino, 2020). Though the PPP quickly injected the struggling economy with liquidity, the funds did not necessarily go to the firms and places most in need (Simon and Rudegeair, 2020). The classic speed-targeting trade off was brought to the forefront with a program as imperative and massive as the PPP. It became clear that the sudden distribution of funds had major targeting shortcomings.

One explanation was that bank lending heterogeneity played a major role in PPP targeting failures. Banks distributing the PPP loans appeared to give strong preference to existing customers or small businesses with existing lines of debt (Gandel, 2020). But there were also other, more fundamental causes that hindered the most distressed small businesses from receiving loans. Many small businesses faced key resource constraints when applying for the PPP. The PPP, like any loan program, requires information about a business, relevant supporting documents, records, and official forms. As such, business owners who did not run sophisticated businesses with accountants, efficient filing systems, or knowledge of how to apply for a loan struggled to even begin applications.

Moreover, program requirements were stringent, poorly communicated, confusing, and constantly changing. To apply for a PPP loan, businesses struggling to stay alive had to pay administrative costs and spend substantial time and energy to meeting – or, at times, simply trying to understand – strict, constantly changing program requirements. First, the requirement to spend 75% of funds on payroll often felt extreme to business owners who needed to cover their rent and other immediate operational costs (Freedman, 2020). In fact, the PPP's demanding requirements to provide documentation and follow specific rules caused many employees to instead opt for generous unemployment insurance extensions and benefits distributed during the pandemic, rather than staying at their old jobs (Freedman, 2020; Mercado, 2020). Based on extensive interviews with an Outreach and Marketing Specialist at the SBA, I also found that some business also thought that they could not apply for the PPP loans if they had already laid off their employees, not realizing they could use the funds to rehire employees, even if they did not perform work. Other business owners applied for loans, only to realize that the specific bank they applied for a loan from stopped taking applications (Weisul, 2020). These applicants who thought they could submit loan applications on the deadline date were rejected because the date actually referred to when banks were required to process loans by. Requirements and deadlines for loan forgiveness were frequently changed, further perplexing business owners and making them less willing to go through the administrative hassle and risk applying for a loan program that had such volatile requirements.

In response to such criticisms, the PPP Flexibility Act, signed by President Trump on June 5, 2020, loosened the requirement to keep 75% of employees on payroll to 60%, extended the maturity period for new loans to five years from two, and lengthened loan forgiveness periods (Myers, Comazzi, Reidy, Tauzel, and Barncastle, 2020). Despite perpetuating confusion by again changing regulations, the PPPFA did lower the stringent requirements borrowers faced. It is therefore plausible that the Act increased incentives to apply to the program, as some business owners may have been more likely to keep 60% of workers on payroll as opposed to the original 75%. The PPPFA may have also reduced barriers to entry for resource-constrained business owners who, under the original program, might have been deterred from the PPP because of its shorter loan maturity period. The PPPFA was arguably the largest policy change made to the PPP, given that it completely rewrote and altered the main requirements of the program. Still, the impact of the Flexibility Act on economic outcomes such as employment is yet unknown.

2.3 Small Business Development Centers (SBDCs)

According to extensive interviews conducted with an Outreach and Marketing Specialist at the SBA, there were key characteristics of regions where more small businesses were able to apply for PPP loans. In places where "community organizations" such as volunteer groups or local chambers of commerce were strong, and volunteers were able to explain rules, translate, and support small business owners through the application process, take-up was markedly higher. One major "community organization" in the rollout of the PPP was the Small Business Development Centers (SBDCs). SBDCs are one of the SBA's local resource partners, located across the U.S., which receive funding and support from the SBA. The SBDC program is the federal government's "largest and most successful management and technical assistance program for small businesses" and provides a wide variety of information and guidance at branch locations to small businesses, including up-to-date counseling, training and technical assistance (America's SBDC; SBA.gov). SBDC assistance is "tailored to the local community and the needs of individual clients," working closely with local SBA district offices to ensure statewide coordination with other available resources (America's SBDC; SBA.gov). Based on interviews, these SBDCs enabled small businesses to acquire PPP funds by supporting them through the application process and promoting general awareness of the program, whether or not businesses had preexisting banking relationships. To my knowledge, there have been no empirical studies analyzing the impact of SBA local resource centers such as the SBDCs on local economic outcomes.

2.4 Prior Literature

Empirical work on the PPP evaluates the short run impact of the program on economic outcomes using a variety of research designs and datasets. Results are mixed, with some authors finding small, insubstantial effects on employment while others reporting that the PPP substantially improved employment and self-reported business survival rates. Findings vary depending on the research design, subset of PPP loans studied based on size, what kind of economic outcomes were being collected, and choice of economic outcome data. Table A.1 in the Appendix summarizes, at a high level, the main differences in research methods, data, and results for this growing body of literature.

The Opportunity Insights' Economic Tracker paper compares outcomes for firms above and below the 500-employee program eligibility cutoff and finds a small, non-substantial effect on employment (Chetty, Friedman, Hendren, Stepner, and The Opportunity Insights Team, 2020). Similarly, Granja, Makridis, Yannelis and Zwick (2020) do not find evidence that the PPP had a substantial effect on local economic outcomes. They find that the way banks distributed PPP loans did not reflect differences in underlying loan demand, and used heterogeneity across banks in terms of disbursing PPP funds to instrument for PPP loan amounts. According to their research, PPP loans did not go to places most hit by the pandemic in part due to the way banks distributed loans, limiting the impact of the program on economic indicators.

On the other hand, Autor et al. (2020) also compare firms above and below the 500-employee cutoff but use administrative data from ADP, finding that the PPP boosted employment at eligible firms by 2 to 4.5 percent. Likewise, Hubbard and Strain (2020) find that the PPP substantially increased the employment, financial health, and survival of small businesses in the near term. They used data from the Dun & Broadstreet Corporation, restricted their study to PPP loans above $150,000 and employed "intent to treat" methods where loan take-up was measured by firms who applied for the PPP as opposed to those who received the loan. Moreover, Bartik, Cullen, Glaeser, Luca, and Stanton (2020) used survey data and variation in firm exposure to larger banks, who provided relatively fewer PPP loans, to find a 14-30 percentage point rise in firms' forecasted survival probabilities and a positive but statistically insignificant impact on employment. Research on the effects of the PPP on broad economic outcomes is thus very much mixed depending on specific methodology, loan data, and outcome data employed.

Those finding minimal or no employment effects offer a variety of explanations for why businesses most affected by COVID-19 closures were unable to receive appropriate funding. Some argue that banks were overwhelmed with a massive influx of loan applications, inadequate resources, and unclear guidance from the SBA. Others say that some small businesses were unfairly rejected from the loan program due to bank incentives. Indeed, Bartik, Cullen, Glaeser, Luca, and Stanton (2020) find that more affected firms in impacted industries, who had less cash on hand and reported greater business distress, were more likely to apply for a loan but actually less likely to receive one. Their results suggest that larger banks prioritized lending to firms in better conditions, therefore excluding those who most needed a loan. Granja, Makridis, Yannelis, and Zwick (2020) similarly show that bank behavior caused loan take-up to not reflect underlying loan demand. They find that banks gave existing customers preferential treatment and acted to maximize fees as opposed to servicing the businesses in most distress.

I contribute to the research through my novel instrument, which reveals another underlying determinant of businesses' receival of PPP loans – the prevalence of small business development centers – as opposed to merely the preexisting banking relationships identified by past papers. Indeed, existing literature to my knowledge has, thus far, only used bank-related measures to instrument for PPP loans. As shown in Figure 1 and 2, however, the presence of community banks and credit unions per capita have statistically significant, negative relationships with COVID cases per capita over time that consistently become more negative as the weeks pass. This perhaps calls the various bank-related instruments other papers used into question given that community banks and credit unions experience other trends in COVID-19 exposure before and after the PPP – that may have confounded their employment results. While other papers did not necessarily use the same variables and dataset as I did for measuring bank lending behavior in an area, my findings still cast some doubt on the use of banking measures in general as sources of exogenous variation.

I also add to the existing literature by finding substantial positive employment impacts of 1st round PPP and PPP following the PPP Flexibility Act across counties in multiple time scales. Existing literature uses the 500-employee cutoff whereby a firm was eligible for the PPP. However, this specification only compares the local average treatment effect on firms around this threshold, which may be different from the average treatment effect on all firms. As such, it has high internal validity but not necessarily high external validity. While recent work does try to extend regression discontinuity estimates at the threshold away from the threshold, in general, zero impact at one threshold does not imply zero impact on all firms. In addition, PPP rules had several exceptions for companies that did not always follow this 500-employee guideline (Barab, Buss, Comanducci, Gibbs, Latman, Spillman, and Walls, 2020). My study therefore contributes to the field of literature by focusing on loan amounts under $150,000. Empirical work on the impact of loans below $150,000 specifically does not exist to my knowledge.

Additionally, it is plausible that smaller PPP loans correspond to smaller firms, as evidenced by Figure 3. Figure 3 shows that loan size is strongly correlated with business size, as measured by PPP loan applicants' self-reported employee numbers. While the correlation weakens as the PPP loan amount increases, the relationship is stronger close to the PPP amount of $150,000, indicating that at lower loan amounts, smaller loans are indeed correlated with smaller business size. Although the accuracy of this employee number is potentially questionable given that it was self-reported, PPP loan amounts were distributed according to businesses' revenue and employee count, making it plausible that loans under $150,000 likely corresponded with smaller businesses. The average employee count among firms receiving loans less than $150,000 is 3.3, with a standard deviation of 8.5, while the average employee count in firms with loans greater than $150,000 is 42, with a standard deviation of 61. As such, by studying loans under $150,000, I also analyze the geographical impact of PPP on employment at smaller firms.

To my knowledge, there is also no other empirical work on the impact of the PPP Flexibility Act. Finally, robust empirical findings on whether PPP went to the right industries is also limited, and, to my knowledge, there are no other academic papers that quantify the impact of the PPP based on geographical industry exposure.

3 Data

All Paycheck Protection Program data through August 8, 2020, is available at the U.S. Department of the Treasury website. The dataset is at the zip code level, providing the dollar loan amount and the date approved for each loan, as well as the NAICS industry code. For my dataset, I aggregate those loan amounts to the county level and summed loans by week. I also use population data by county from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program to find the weekly loan amount by county per capita. Given that the program was intended to support small businesses, my study focuses on PPP loans under $150,000 (Guida, 2020).

For my instrument, I obtain data on the geographical distribution of Small Business Development Centers (SBDCs) made available at the SBA's Open Government Data Sources website. The dataset contains zip codes for each SBDC location, which I aggregate up to the county level and use to record the number of centers for each county, assumed to be unchanging over my time period. I also use the NCI's SEER dataset to create a variable for centers in each county per capita.

To control for COVID-19 Economic Injury Disaster Loans (EIDL) and EIDL Advance, a grant program offered in conjunction with EIDL, I use data from the SBA website. Data is limited, with weekly reports as of April 24, 2020, providing the number of EIDL and EIDL Advance funds distributed by state as of the report date. It is important to note that the COVID-19 EIDL and EIDL Advance programs were announced and distributed beginning in mid-March 2020, despite the data starting in late April. As such, I take the earliest report available (April 24, 2020) and divide the dollar amount equally in the weeks since the beginning of the program.

To measure economic outcomes, I use data from the Opportunity Insights' Economic Tracker, which uses "anonymized data from several private companies to construct indices of spending, employment, and other outcomes" (Chetty, Friedman, Hendren, Stepner, and The Opportunity Insights Team, 2020). For my paper, I use employment at the county level, and aggregated data up to a weekly frequency. The data is available from January 15, 2020, until September 2020 and detailed information on the construction of the series is available on the Opportunity Insights' Economic Tracker website. Instead of reporting levels of employment, the Opportunity Insights team reports changes in employment rates indexed to January 4-31. The employment variable they report is thus measured in such a way that a value of -0.5, for instance, refers to a 50% decline in employment rates compared to January 2020. As such, for my employment variable I multiply the Opportunity Insights data by 100 to better interpret coefficients relevant to my specifications.

Lastly, for my industry analyses, I use data from the U.S. Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW), which contains county-specific employee and establishment counts by a variety of industry classification systems. As stated on the BLS website, "the highest levels of aggregation in the North American Industry Classification System (NAICS) are NAICS sectors," which are two-digit codes (BLS, 2020). These two-digit codes can then be broken down further into three-digit subsectors (BLS, 2020). I use the most recent data from the BLS website, which is county-level employee and establishment data for the first quarter of 2020, separated by sector and subsector.

4 Empirical Strategy

The goal of this study is to measure the causal effect of an additional \$1 of PPP loans in week t on outcomes such as employment in county i, as in the following causal model:

$$Y_{it} = \beta_0 + \beta_1 PPP_{it} + \varepsilon_{it} \quad (1)$$

In equation 1, the coefficient β_1 is the average causal effect of increasing PPP_{it} by \$1 on Y_{it}, while ε_{it} represents omitted determinates of Y_{it}. The identification problem is that $Cov(\varepsilon_{it}, PPP_{it}) \neq 0$.

Section 4.1 describes OLS methods, Section 4.2 details my IV specifications and identification assumptions for the 1st round of PPP, Sections 4.3 discusses IV methods for PPP following the PPP Flexibility Act, and Section 4.4 explains IV methods used in my analysis of PPP by counties' exposure to select industries.

4.1 OLS Estimations

I start by estimating the following ordinary least squares (OLS) regression:

$$Employment_{it} = \alpha + \beta PPPLoanAmount_{it} + W_{it}\theta + \delta_s + \delta_t + \varepsilon_{it} \quad (2)$$

This specification measures the short run impact in that it restricts the sample to loan amounts and employment between the 9th and 16th weeks of 2020 – 2 weeks after the advent of 1st round PPP funding. Week 16 coincides with when the first round of PPP funding was entirely exhausted, indicating that this time frame measures the immediate to short run impact of early PPP funding. W_{it} is a vector of control variables.[3] δ_s represents state fixed effects, which control

[3] I control for population density in 2019 by county, GDP per capita in 2019 by county, unemployment levels per capita in 2019 by county, COVID-19 case counts per capita by county in the week prior to the PPP program, the first reported COVID-19 EIDL and EIDL Advance loan amounts by state (two additional, smaller COVID-19 relief loan programs that were put in place two weeks before the start of the PPP), the number of community banks per capita by county, the number of credit unions per capita by county, COVID-19 case counts per capita by county over time, and dummy variables indicating when states implemented and removed stay at home orders and all business closure regulations.

for factors that vary across states but remain constant over time, like state-specific policies, state trends in employment, and regional trends. δ_t represents week fixed effects, which control for factors that vary across weeks for the whole country, such as country-wide pandemic exposure and economic dislocation. Standard errors are clustered by county.[4]

However, estimating this specification using an OLS regression leads to biased and inconsistent estimates of $Employment_{it}$ if $E[\varepsilon_i|\text{PPPLoanAmount}_{it}, W_{it}, \delta_s, \delta_t] \neq E[\varepsilon_{it}|W_{it}, \delta_s, \delta_t]$. The concern is that funding via PPP was not randomly assigned, and so counties that received more PPP funding may have been hit harder by the pandemic or the converse. Indeed, certain counties are likely to have been more impacted by the pandemic and would thus have higher unemployment numbers.

4.2 IV Estimations: 1st Round PPP

Because the PPP loan amount variable is endogenous, and there are characteristics of counties that had more PPP funding in the first two weeks affecting their employment patterns as well, I use an instrumental variables research design which has aspects similar to a difference-in-differences design.

The variable $BinarySBDC_{it} \times PostPPP1$ is my instrument for $PPPLoanAmount_{it}$ and is the interaction between whether or not a county has a small business development center (SBDC) and the PPP loan amount for the weeks over the first round of PPP (Weeks 14-16). I chose the binary variable of SBDCs in a county because many counties had no SBDCs. Figure 4

[4] I also estimate this regression by taking the natural logs of both employment and PPP loan amount. Because of the considerable presence of zeroes in both loan and employment data, I apply a ln(x+1) transformation to account for the undefined values that result from taking the natural logarithm of 0. See Appendix Tables A.5, A.6, and A.7.

shows a histogram of the distribution of SBDCs by county. My two stage least squares estimation is as follows:

$$Employment_{it} = \alpha + \beta_1 PPPLoanAmount_{it} + \beta_2 BinarySBDC_{it} + \beta_3 PostPPP1 + W_{it}\theta + \delta_s + \delta_t + \varepsilon_{it} \quad (3)$$

where W_{it} is the same vector of control variables as in equation 2. δ_s and δ_t are again state and week fixed effects, respectively, and standard errors are clustered by county.[5]

In the first stage, I analyze whether PPP amounts in the first round increased after the PPP in counties with at least one SBDC:

$$PPPLoanAmount_{it} = \pi_0 + \pi_1(BinarySBDC_{it} \times PostPPP1) + \pi_2 BinarySBDC_{it} + \pi_3 PostPPP1 + W_{it}\theta + \delta_s + \delta_t + v_{it} \quad (4)$$

Relevance: $BinarySBDC_{it} \times PostPPP1$ is correlated with the endogenous regressor, $PPPLoanAmount_{it}$. Mathematically, $\pi_1 \neq 0$ in equation 4.

The regression in Equation 4 yields a first stage F statistic of 13.56 (Table 4), indicating that the instrument is strong.

Exogeneity: $BinarySBDC_{it} \times PostPPP1$ only affects employment because it changes $PPPLoanAmount_{it}$ and, further, is as good as randomly assigned, conditional on the fixed effects and controls, in the sense that it is uncorrelated with any omitted determinants of employment. Mathematically, $E[\varepsilon_i | BinarySBDC_{it} \times PostPPP1, W_{it}, \delta_s, \delta_t] \neq E[\varepsilon_{it} | W_{it}, \delta_s, \delta_t]$.

[5] I also estimate this regression by taking the natural log of PPP loan amount. Because of the considerable presence of zeroes in both loan and employment data, I apply a ln(x+1) transformation to account for the undefined values that result from taking the natural logarithm of 0. See Appendix Tables A.5, A.6, and A.7.

My research design exploits both the timing of the PPP as well as the predetermined geographical variation in the location of SBDCs. Exploiting only the timing in PPP would be confounded by any trends in the outcome variable, employment. Exploiting only the geographic distribution of SBCs would confound differences in the characteristics of counties with more vs. fewer centers. By utilizing both sources of variation, I difference out both potential sources of confounds. While $BinarySBDC_{it}$, the variable indicating whether a county has an SBDC, may not be exogenous, $BinarySBDC_{it} \times PostPPP1$ is plausibly exogenous (after controlling for $BinarySBDC_{it}$) because the interaction term captures a county's exposure to centers only once the first round of PPP loans is distributed. Any fixed differences are accounted for by the main $BinarySBDC_{it}$ interaction term.

Validation/Falsification Exercises to Assess Research Design

To validate my research design, I first look at the relationship between the non-interacted variable $BinarySBDC_{it}$ and employment over time. I regress employment on $BinarySBDC_{it}$ separately for each week for the 3rd to 37th weeks of 2020 (before the COVID-19 outbreak in the United States until September 2020). Figure 5 plots the coefficients on $BinarySBDC_{it}$ from these regressions. While the PPP program started in week 14, 2020, week 12 is when the bivariate effect of centers on employment becomes positive, thus seeming to violate the parallel trends assumption. There appear to be trends prior to the advent of the program. I find, however, that the regression of employment on centers was actually capturing the initial effect of smaller, separate loan programs initiated under the CARES Act: the COVID-19 EIDL and EIDL Advance programs. These programs began exactly in week 12 – which is when I observe the start of the trend. To control for these other loan programs and isolate the impact of the first round of the PPP, I therefore created *PostPPP1*, the dummy variable for when the first round of PPP funds was distributed (between weeks 14-16). I then interacted the dummy variable with the number of centers per capita to isolate the PPP variable.

As a robustness check, I introduce, in the regressors in both stages, variables measuring employment in the weeks right after the introduction of the EIDL program but before PPP. Tables A.2 and A.3 report results for the impact of 1st round PPP and PPP post-PPP Flexibility Act, respectively, with these additional controls, and yield no significant changes compared to my core regressions. This indicates that the relationship between PPP and employment is adequately separated from the effects of the EIDL program. Moreover, to further address the concern that EIDL could still be correlated with my instrument, $BinarySBDC_{it} \times PostPPP1$, and thus my results could pick up the effect of the EIDL programs and not the PPP on employment, I perform additional tests. I run both first and second stage regressions, replacing the PPP loan amount with a) the first reported COVID-19 EIDL and EIDL Advance loan amounts by state and b) the sum of that early EIDL amount and time-varying PPP program loan amounts. Table A.4 reports that my instrument, $BinarySBDC_{it} \times PostPPP1$, does not strongly predict the early EIDL loans or the sum of the two programs. However, my instrument does strongly predict the PPP loan amount, providing further evidence that SBDCs does not pick up the early effect of the other loan program, EIDL. Additionally, my instrument was a weak predictor of PPP loans over \$150,000, further indicating that the variable SBDCs uniquely identifies PPP loans under \$150,000.

A main concern with this research design might be that there could be a difference in other characteristics in places with SBDCs that led them to be differentially impacted by the COVID-19 shock. To that aim, I looked at both time-varying and fixed variables that are correlated with the employment shock. First, Figure 6 reports the coefficients over the 12th and 37th weeks of 2020 (week 12 being the start of the pandemic outbreak in the US, March 16-22, 2020, and week 37 being September 7-13, 2020) of the regression of COVID-19 case count per capita on whether a county has at least one SBDC. There does not appear to be a trend in COVID-19 exposure for counties with centers before or after the PPP. This provides evidence consistent with the parallel trends assumption. If COVID cases in counties with SBDCs diverged from counties without SBDCs, it would have been hard to believe that economic growth would have been similar for the two groups in the absence of the PPP. My findings support the exogeneity assumption for instrumental variables by indicating that SBDC exposure likely does not impact employment through COVID case exposure, given centers and COVID cases are uncorrelated. Similarly, Figure 7 shows that SBDCs are also uncorrelated with consumer spending measures over time, further providing evidence that areas with SBDCs are unlikely to be differentially affected by the COVID shock over time. Furthermore, for stay-at-home orders and business closure orders, I also found no clear trends before and after the PPP program. It appears state policy changes are mostly uncorrelated with PPP, and if they are, coefficients are miniscule. These time-varying covariates correlated with the COVID-19 shock are not also correlated with the distribution of SBDCs.

For additional information on the distribution of SBDCs, Table 2 presents summary statistics for several time-invariant covariates[6] separated by whether a county has at least one SBDC. When I test the relationship between centers per capita and these covariates, each separately, I find a positive relationship between SBDCs and population density, SBDCs and

[6] Population density in 2019 by county, GDP per capita in 2019 by county, unemployment levels per capita in 2019 by county, COVID-19 case counts per capita by county in the week prior to the PPP program, the number of community banks per capita by county, and the number of credit unions per capita by county.

unemployment in 2019, and SBDCs and community banks, and a negative relationship between SBDCs and GDP per capita in 2019, and SBDCs and COVID case counts in the week before the PPP. I therefore include all these covariates in my first and second stage regressions as controls. In any case, my difference-in-differences specification likely differences out factors that are time-invariant because I compare both control and treatment groups to themselves pre-PPP.

4.3 PPP Post-PPP Flexibility Act

I also estimate the impact of PPP loans distributed after the PPP Flexibility Act was passed to analyze the impact of this major policy that completely restructured program requirements. Here, I replace the variable $PostPPP1$ instead with $PostPPPF$, a dummy variable for the weeks following the passing of the PPP Flexibility Act (weeks 24-37). Given the limited time frame between the passing of the PPP Flexibility Act and employment data today, I can only measure the immediate impact of PPP funds after this Act. One additional consideration is that counties that received more 1^{st} round PPP are likely to have different employment patterns after the Flexibility Act compared to those that did not. As such, in this specification I also control for the 1^{st} round of PPP funds to account for the impact of receiving 1^{st} round PPP on the impact of PPP post-Flexibility Act.

4.4 Industry Analysis

Finally, I perform the same IV analyses, restricted to counties more exposed to high COVID-impacted sectors, according to S&P Global Market Intelligence and a variety of news sources and academic reports (Haydon and Kumar, 2020; Ding and Sanchez, 2020). I first estimate IV regressions measuring 1^{st} round PPP short run impact on employment for counties with above-median exposure to four NAICS two-digit sector codes: Manufacturing, Transportation and Warehousing, Arts, Entertainment, and Recreation, and Accommodation and Food Services. I then delve further into the Accommodation and Food Services sector, which is made up of two three-digit NAICS subsectors: Accommodation and Food Services and Drinking Places. There, I analyze the short, medium, and long term impact of 1st round PPP and PPP following the PPP Flexibility Act.

To measure local exposure to a particular industry, I restrict the sample to counties with industry-specific employee counts per total county working population that are greater than the median. For instance, for my analyses of the Accommodation and Food Services sector, I calculate the median number of Accommodation and Food Services sector employees per total workers over all counties. Then, I perform two stage least squares estimations for employment on PPP loan amounts, restricted to counties that had above-median Accommodation and Food Services employees per total workers.

5 Results

In the following section, I present results for the effect of the PPP loans under $150,000 on employment using my novel instrument, $BinarySBDC_{it} \times PostPPP$, with $PostPPP$ being either $PostPPP1$ or $PostPPPF$ based on which round of PPP I am analyzing. Section 5.1 reports estimates for the effect of 1st round PPP, Section 5.2 describes the impact of PPP following the PPP Flexibility Act, and Section 5.3 reports results from my industry analysis.

5.1 Short, Medium, and Long Run Impacts of 1st Round PPP

In Table 3, the estimated OLS coefficient of 0.016 (Column 2) implies that in the short run, a one standard deviation increase in 1^{st} round PPP loans saved 3.4% of the average jobs lost in the same time frame. The result is statistically significant at the 1% level. My IV specification, however, yields greatly different results. Table 4 shows that the IV coefficient is markedly higher at 0.305 and is also statistically significant at the 1% level. The 0.305 coefficient indicates that in the short run, a one standard deviation increase in 1^{st} round PPP loans saved, in fact, 64.9% of the average jobs lost in the same time frame. The 95% confidence interval for estimates of the short run effects of 1^{st} round PPP on employment is 0.112 to 0.498, which corresponds to the PPP saving between 23.8% and 105.9% of the average jobs lost in a county in counties that received funding.

My coefficient of 0.305 corresponds to 64.9% of jobs saved through the following calculations: counties with 37.237 additional dollars in 1^{st} round PPP loans per capita – the amount of one standard deviation of PPP funds[7] – had a 0.305*37.237 = 11.357 percentage point *greater* change in employment rate relative to January. The average level of employment in a county in January was 47,259.79. Therefore, counties with additional 1^{st} round PPP per capita equal to the standard deviation of PPP funds saved 11.357/100*47259.79 = 5,367.294 *more jobs* than counties without 1^{st} round PPP. The average employment rate change indexed to January in the short run (weeks 9-16) was -17.503, indicating a 17.503% decline, and therefore the average number of jobs lost is 17.503%*47,259.79 = 8,271.881. Putting it all together, this means that 1^{st} round PPP counties with an additional 37.237 dollars in PPP per capita saved

[7] I use the standard deviation of the distribution of PPP loans as a benchmark because the impact of an additional dollar of PPP loans in each county is not relevant in the real world, where all counties received much more than this meager amount. Specifically, I use the standard deviation of the exogenous variation of my PPP loan variable, imputed by getting the standard deviation of the fitted values of PPP loans on SBDCs. This standard deviation measures the variation and dispersion of the treatment, PPP, giving us a sense of the average treatment dispersion and size. For the 1^{st} round PPP loan amount in the short run (two weeks after the start of the PPP), this standard deviation was 37.237 dollars per capita.

5,367.294/8,271.881*100 = 64.9% of the average jobs lost in the same time frame. If counties without PPP lost 100 jobs on average, counties with the standard deviation amount of 1st round PPP loans were able to save 64.9 of those jobs. This demonstrates the substantial short run effect of 1st round PPP loans under $150,000 each.

Table 5 reports the impact of 1st round PPP loans in the medium term, changing the weeks for analysis from weeks 9-16, as in the short run, to weeks 9 through 27. Table 5 reports a negative coefficient on PPP loans in the OLS specification. As in the short run analysis, the OLS is biased downward and thus the IV regression yields a statistically significant positive coefficient of 0.236. I conceptualize these using the same calculations as in the short run specification. In the medium term (weeks 9 through 27), counties with additional PPP funding by one standard deviation of the distribution of PPP loans in the medium run had a 32.304*0.236 = 7.624 percentage point *greater* change in employment rate relative to January 2020, saving 7.624/100*47259.79 = 3,603.086 *more* jobs. Between weeks 9 through 27, the average employment change indexed to January was -22.708, indicating a 22.708% decline, and therefore the average number of jobs is 22.708% of 47,259.79, which is 10,731.753 jobs. In the medium run, a one standard deviation increase in 1st round PPP loans saved 3,603.086/10,731.753*100 = 33.6% of the average jobs lost in the medium term.

To conclude my study of the impact of 1st round PPP loans, in Table 6, I report the long run impact of this early PPP funding on employment. The IV regression coefficient in Table 6 is in Column 4 and is 0.252, statistically significant at the 1% level. This means that in the longer term (weeks 9 through 37), relative to counties without 1st round PPP, counties with an additional loan amount equal to the standard deviation of PPP in the long run had a 31.382*0.252 = 7.908 percentage point *greater* change in employment rate relative to January 2020, saving

7.908/100*47259.79 = 3,737.304 *more* jobs. Now, between weeks 9 through 37, the average employment change indexed to January was -18.865, indicating a 18.865% decline, and therefore the average number of jobs is 18.865%*47,259.79 = 8,915.559 jobs. This means that 3,737.304/8,915.559*100 = 41.9% of the average jobs lost in the same time frame were saved by a one standard deviation in 1st round PPP in the long run.[8] 95% confidence intervals were 0.11 to 0.363 for the medium run and 0.109 to 0.394 for the long run effects of 1st round PPP, corresponding to 28.7% to 94.6% jobs saved in the medium run and 28.4% to 102.7% jobs saved in the long run.

My OLS coefficient for the 1st round PPP's impact on employment in the short run, 0.016, is much smaller than in my IV specification, where the coefficient is 0.305. This indicates that the OLS coefficient was biased downward. Tables 4, 5, and 6 show that for the 1st round of PPP funding, the OLS coefficient is significantly lower than the IV coefficient. It is therefore likely that, conditioning on particularly small businesses, who plausibly received loans <$150,000, firms most in need asked for more money and had worse outcomes than those who did not receive loans. When studying fiscal policy, it is often the case that places harder hit by economic downturn get more fiscal stimulus. As such, the OLS coefficient underestimates the impact of the PPP because counties with more PPP funding were hit harder by the pandemic. However, other empirical work on the PPP has suggested that this is not the case for the PPP, and funds were not distributed to firms and industries most in need. Still, a key difference in my study is that I condition on small businesses with loan amounts <$150,000. These smaller businesses are likely to have had less resources, weaker credit profiles, and fewer bank-lender

[8] In Appendix Table A.5, A.6, and A.7, I confirm all these results through an altered specification that takes the natural log of the PPP loan amount and the instrument, the number of SBDCs per capita. All log variations report similar results as their corresponding core regressions.

relationships. Therefore, conditioning on these small firms, it may indeed be that those receiving loans were harder hit by the pandemic, biasing my OLS coefficient downwards.

5.2 Short Run Impact of PPP after the PPP Flexibility Act

In Table 7, I analyze the effect of PPP loans that were made after the passing of the PPP Flexibility Act on employment. In doing so, I aim to explore the true impact of this momentous yet understudied policy change that completely altered PPP requirements and thus the nature of the program. Table 7 reports a coefficient of 0.551, corresponding to the PPP saving 97.9% of average jobs lost,[9] with a 95% confidence interval lower bound of 0.162 and upper bound of 0.94, corresponding to between 28.8% to 167% of average jobs lost that were saved by the PPP. While the confidence interval is large for a one standard deviation increase in PPP, the employment effects are still substantial even at the lower bound. This demonstrates the sizeable impact of the PPP on employment particularly after the Flexibility Act. The increased program flexibility seems to have substantially improved the ability of the PPP to translate loan dollars into jobs saved.

5.3 Industry Analysis

Table 8 reports IV coefficients for the impact of 1st round PPP loans on employment using the same methods as in the previous sections, now limited to counties with above-median exposure to the Food and Accommodation industry. Column 2 yields a coefficient of 0.55,

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[9] Counties with PPP post-Act had 33.507*0.551 = 18.462 percentage point *greater* change in employment rate relative to January 2020, saving 18.462/100*47259.79 = 8,725.102 *more* jobs. Between weeks 9 through 37, the average employment change indexed to January was -18.865, indicating a 18.865% decline, and therefore the average number of jobs lost can be calculated to be 18.865% of 47,259.79, which is 8,915.559 jobs. A one standard deviation increase PPP loans following the PPP Flexibility Act therefore saved 8,725.102/8,915.559*100 = 97.9% of the average jobs lost in the same time frame.

statistically significant at the 1% level. Using the same calculation methods as in prior sections, this means that among counties with above-median Food and Accommodation sector exposure, the impact of the PPP on employment was markedly larger -- additional 1st round PPP equal to the standard deviation loan amount saved 117% of the average jobs lost in the short run (weeks 9-16). The 95% confidence interval is between 0.22 to 0.881, corresponding to 46.8% to 187.4% of lost jobs saved by the PPP. Recall that 1st round PPP saved 64.9% of jobs lost in all counties, as reported in Section 5.1. 1st round PPP's impact on employment was thus almost twice as substantial in counties with above-median Food and Accommodation sector employees.

For sectors Manufacturing, Transportation and Warehousing, and Arts, Entertainment, and Recreation, the first stage F-statistics were under 10 and therefore my instrument, $BinarySBDC_{it} \times PostPPP1$, was not a strong predictor of PPP loan amounts in counties with above-median exposure to those industries. As such, I cannot draw conclusions about the PPP impact for areas with greater exposure to these industries.

To get a more granular understanding of why the PPP might have had a greater impact on employment in counties with more exposure to the Food and Accommodation sector, I delve deeper into its subsectors. Tables 10 thus reports short, medium, and long run estimates for counties with above-median exposure to the first subsector within Food and Accommodation: Food Services and Drinking. In Table 9, columns 2, 4, and 6 report short, medium, and long run coefficients of 0.818, 0.526, and 0.61. This indicates that in counties with above-median exposure to the Food Services and Drinking Places subsector, additional 1st round PPP equal to the standard deviation of loans saved 174%, 74.8%, and 101.5% of the average jobs lost in all counties in the short, medium, and long run, respectively. The 1st round of PPP saved 64.9% of average jobs across all counties. As such, in counties more exposed to Food Services and

Drinking Places, 1st round PPP saved thrice as many jobs lost in the short run, around 15% more jobs in the medium run, and 55% more in the long run. In the short, medium, and long term, the 1st round of PPP had a significantly greater positive impact on employment in counties more exposed to the Food Services and Drinking Places industry.

Within the same Food Services and Drinking Places subsector, Table 10 reports a positive coefficient for the effect of the PPP post-PPP Flexibility Act, but the first stage F statistic is below 10, making my results for this round of the PPP inconclusive among counties exposed to the Food Services and Drinking Places subsector.

For Accommodation, the second subsector of Food and Accommodation, Table 11 presents coefficients of 0.44, 0.271, and 0.294 for the short, medium, and long run impact of 1st round PPP on employment in counties with above-median exposure to the Accommodation subsector. This corresponds to a one standard deviation increase in 1st round PPP saving, in counties more exposed to Accommodation, 93.6%, 38.6%, and 48.9% of average jobs lost across all counties in the short, medium, and long run, respectively. Across all counties, the 1st round of PPP saved 64.9% of average jobs lost. Therefore, in counties more exposed to Accommodation, 1st round PPP saved almost 50% more jobs lost in the short run, half as many in the medium run, and almost 30% less in the long run. In the short run, 1st round PPP had a magnified impact on employment in counties more exposed to the Accommodation subsector. The effect seems to wane in the medium and longer term.

Table 12 reports a coefficient of 0.608 for the impact of the PPP post-PPP Flexibility Act on employment amongst counties more exposed to the Accommodation subsector. This coefficient corresponds to a one standard deviation increase in PPP after the Flexibility Act saving 101.1% of the average jobs lost in the same time frame in counties with above-median

Accommodation subsector exposure. This finding suggests that the PPP was substantially more impactful amongst counties with higher Accommodation subsector exposure after the passing of the PPP Flexibility Act.

6 Discussion and Extensions

I summarize my four main findings and discuss their implications in the following sections. Section 6.1 discusses my novel instrument, Section 6.2 discusses the employment effects of 1st round of PPP loans under $150,000, Section 6.3 discusses employment effects of PPP loans, also under $150,000, that came after the PPP Flexibility Act was passed, and Section 6.4 discusses the even more substantial impact of PPP in counties more exposed to COVID-impacted industries.

6.1 Geographical Exposure to SBDCs is a Strong Predictor of PPP Loans

The first major contribution of this paper is that it uses a novel instrument, geographical exposure to SBDCs, to perform analysis of the employment effects of the PPP.

As discussed in Section 2.3, my findings call into question the various bank-related variables other papers use to instrument for PPP loans. Indeed, Figures 1 and 2 reveal that community banks and credit unions per capita both have statistically significant, negative relationships with COVID cases per capita over time that consistently become more negative as the weeks pass. My findings suggest that community banks and credit unions experience COVID-19 exposure time trends that might call into question the use of bank-related measures as a valid instrument. This may explain why these papers' findings, that there were no substantial employment effects of the PPP, thus differ from mine. In contrast, I find that my novel instrument, SBDCs, passes a variety of falsification exercises and robustness checks, as discussed in Section 4.2. Section 4.2 discusses its strong relevance and plausible exogeneity as well.

Moreover, my instrument supports evidence from extensive interviews with an SBA Outreach and Marketing Specialist that these local SBA resource partners were pivotal in helping small businesses obtain PPP loans and therefore retain more jobs. In all my regressions, the first stage F-statistic for the instrument SBDCs was well over the standard of 10 for a strong instrument. This strong correlation between centers and PPP loan amount provides some empirical evidence for the effectiveness of these SBA resource centers. There is compelling evidence that these smaller, more local community centers may be key to rolling out small business support programs. Regardless of how much money is poured into a program such as the PPP, without the support of these local centers in helping small business owners with loan applications, explaining the application process and providing support, the program may be rendered ineffective due to take-up problems. It is important to note that this paper does not establish causation for the relationship between centers and PPP loan amounts: still, the strong correlation is noteworthy and an avenue of further study of the critical role of SBDCs. To my knowledge, no paper has empirically studied the effectiveness of SBA development centers.

6.2 1st Round PPP Under $150,000 Had Substantial Employment Effects

Secondly, this paper contributes to existing literature by finding substantial employment effects of the PPP amongst loan sizes below $150,000. As discussed in Section 2.4, empirical papers that exploit the 500-employee eligibility cutoff for PPP at best only identify the causal effect of PPP for firms that have 500 employees, and not the average treatment effect across all firm sizes. It would require strong assumptions to extrapolate the estimates at the 500-employee threshold to small companies that are very far away from the threshold (e.g., fewer than 100 employees). The causal estimate identified in regression discontinuity designs is the causal effect at the threshold where the treatment changes. Since not all firms who were eligible received PPP funding, that identified estimate is further a local average treatment effect, or the treatment effect on the subset of "complier" firms who took up the treatment. Therefore, the interpretation of those estimates may not have external validity for very small firms or much larger firms. The data are simply not informative about the impact of the PPP for such firms away from the threshold, or for the "noncompliers" who did not receive the treatment. Empirical work on the impact of loans below $150,000, however, does not exist to my knowledge. As such, my study contributes to the field of literature by specifically focusing on loan amounts under $150,000, uniquely analyzing the geographical impact of the PPP on employment at the smallest of firms, given that I study the smallest loan amounts.

Indeed, I find that a one standard deviation increase in 1st round PPP loans under $150,000 saved 64.9% of the average jobs lost in the short run. In the medium run, a one standard deviation increase in 1st round PPP saved 33.6% of the average jobs lost in that time frame, and in the long run, 41.9% of the average jobs lost. These findings are notable because they reveal statistically significant and economically substantial impacts of PPP loans on employment not just immediately, as some other papers do, but *also* in the medium and long run. The findings are especially relevant given the robustness of my novel instrument and my use of high frequency, combined private employment data. My results quantify the *amount* and *extent* to which firms who got early PPP – either through lending relationships, access to SBDCs, or other reasons – benefitted from this loan program.

By revealing the effectiveness of these smaller loans under $150,000, my findings suggest the importance of distributing smaller PPP loans. Despite some reports, this small business loan program did indeed have substantive impacts on jobs, once we condition on smaller loan amounts. Smaller PPP loan amounts that plausibly went to smaller firms were more impactful in saving jobs, indicating that the PPP worked more effectively when it reached particularly small businesses. One major implication for future iterations of the PPP is therefore that the program is especially effective in saving jobs when it distributes smaller loan, plausibly to smaller sized firms. Prioritizing PPP distribution to smaller businesses who receive smaller loan amounts will be prudent given the PPP's heightened effectiveness amongst these smaller firms. I cannot conclude, however, that smaller PPP loan amounts are more effective than larger PPP loans because my instrument was not strong for loans above $150,000. My findings simply highlight the effectiveness of PPP loans under $150,000 without drawing conclusions about them in comparison to loans above $150,000.

6.3 Further Employment Effects for PPP Post-PPP Flexibility Act

It is also remarkable that an even greater percentage of jobs were saved by the PPP after the PPP Flexibility Act. A one standard deviation increase in PPP after the Flexibility Act saved 97.9% of the average jobs lost in that time frame. In comparison, a one standard deviation increase in 1st round PPP saved 64.9% of the average jobs lost in that time. My findings thus highlight the importance of the Flexibility Act, which loosened some of the stringent regulations for PPP loan forgiveness. Areas that received PPP following the PPP Flexibility Act saved even more of the average jobs lost (compared to that of the 1st round of PPP), suggesting that the additional flexibility offered by the Act further increased the PPP's ability to save jobs. My findings reveal the importance of flexible program requirements in boosting the effectiveness of the PPP. No other empirical studies on the impact of PPP loans after this program amendment exist to my knowledge.

More broadly, my paper implies that the multitude of stringent requirements for loan applications and forgiveness can be detrimental for the effectiveness of the PPP. The removal of such regulations, through the Flexibility Act, had a substantial positive impact on the program's ability to translate loans into jobs saved. This has important implications for the next round of PPP and future loan programs of such nature in times of economic crisis: less stringent loan forgiveness and application requirements can substantially increase the effectiveness of a small business loan and save a substantial number of jobs. Future small business lending programs should be mindful of the significant positive impact that looser rules and requirements can have on loan take-up and resulting jobs saved.

6.4 Even Greater Employment Effects in Counties Exposed to COVID-Impacted Industries

Finally, my industry analysis revealed that PPP under $150,000 had even greater employment effects in counties more exposed to sectors heavily hit by the pandemic. Among counties with above-median Food and Accommodation sector exposure, a one standard deviation increase in 1st round PPP saved 117% of the average jobs lost in the short run. 1st round PPP's impact on employment was almost twice as high in counties with high exposure to the Food and Accommodation sector as it was across all counties. I also analyze the impact of the PPP in the short, medium, and long run for counties more exposed to the Food and Accommodation sector's two subsectors, 1) Food Services and Drinking and 2) Accommodation. I find that in counties with above-median exposure to the Food Services and Drinking subsector, a one standard deviation increase in 1st round PPP saved 174%, 74.8%, and 101.5% of the average jobs lost in all counties in the short, medium, and long run, respectively. In counties with above-median exposure to the Accommodation subsector, additional one standard deviation of 1st round PPP saved 93.6%, 38.6%, and 48.9% of the average jobs lost in all counties in the short, medium, and long run, respectively. Moreover, PPP after the PPP Flexibility Act saved 101.141% of the average jobs lost in the same time frame in counties with above-median Accommodation subsector exposure. To my knowledge, there are no empirical papers analyzing the impact of the PPP by industry exposure.

These findings reveal the importance of targeting because in these especially affected regions, the PPP was proven to have even more potential. Counties hit hard by the pandemic who received loans under $150,000 had even more substantial increases in employment. Moreover, the fact that I find substantial job retention in counties with PPP who were more exposed to COVID-impacted industries in the immediate, medium, and even long run indicates that getting PPP to the most impacted regions can save even more jobs across multiple time scales. Since the PPP is revealed to be even more effective when it goes to regions especially exposed to COVID-impacted sectors, future policy will benefit from increased targeting efforts. By targeting regions with more businesses impacted by COVID-19, policymakers can boost the effectiveness of the PPP and save more jobs.

7 Conclusion

There is no shortage of articles and reports criticizing the requirements, implementation, and execution of the Paycheck Protection Program. From news articles on public companies like

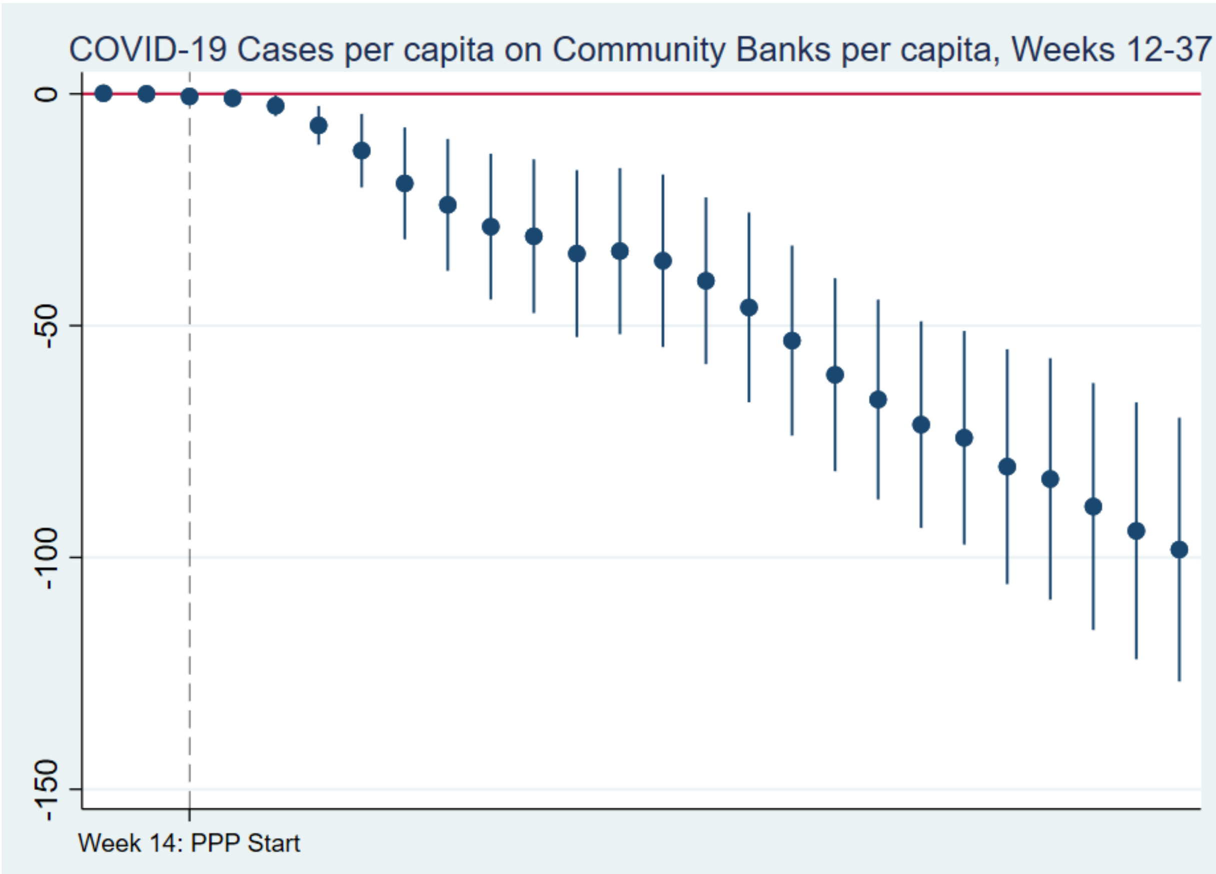
Shake Shack receiving funds, news of the system crashing hours before the 1st round opened, and reports of banks favoring existing customers, a major narrative in public understanding of the PPP seems to have been that the program was a failure. The program has consistently come under fire given the gravity of the pandemic situation, the vast amount of funding allotted to the program and its questionable ability to appropriately target affected firms and stimulate employment. Indeed, the program had its flaws and blunders in its attempts to effectively target businesses in need as quickly as possible. My findings show, however, that the PPP was in fact able to save a substantial number of jobs: 1) in smaller amounts, under $150,000, 2) after program flexibility was increased, and 3) in areas more exposed to COVID-impacted industries. These results were seen in the short, medium, and long term. My findings thus suggest the importance of smaller loan amounts to smaller firms, flexible program requirements, and targeting to areas most impacted by COVID-19. In these smaller loan amounts, with additional program flexibility, and in areas more impacted by COVID-19, the PPP was in fact highly effective in saving jobs. This provides useful insights into how future iterations of the PPP and similar programs should be designed to maximize effectiveness.

The next iteration of the PPP is currently underway: in January 2021, the SBA reopened the portal as $284 billion was approved in a new small business aid package. The 2021 iteration specifically works to distribute more funds to smaller businesses. Firms must now have below 300 employees, as opposed to 500, and the maximum loan amount has been reduced from $10 million to $2 million (Berwick, Walker, and Wittenberg, 2021). The new iteration of PPP in 2021 is also reported to prioritize impacted industries, giving special preference to businesses in the Food and Accommodation industry. If executed correctly, my results indicate that the new iteration's intended improvements should greatly increase the employment effects of the PPP.

My findings might also provide a useful lower bound estimate of the impact of this upcoming PPP iteration. I found that in the short run, the original $669 billion program saved 64.9% of jobs in counties with 1st round PPP. If the new iteration of PPP had at least the same impact as the last, I can roughly expect the now $284 billion program to save 27.5% of the average jobs lost in counties that receive PPP. Given that the new iteration of the PPP works to increase loans to smaller businesses and improve targeting, this fraction of jobs saved can be thought of as a lower bound estimate. Of course, the number of jobs lost in 2021 is significantly different from that of 2020, making it difficult to accurately make predictions based on my findings, which measure jobs saved based on the average number of jobs lost in the corresponding time frame. Still, my study provides one useful baseline lower bound estimate for the expected impact of this upcoming iteration.

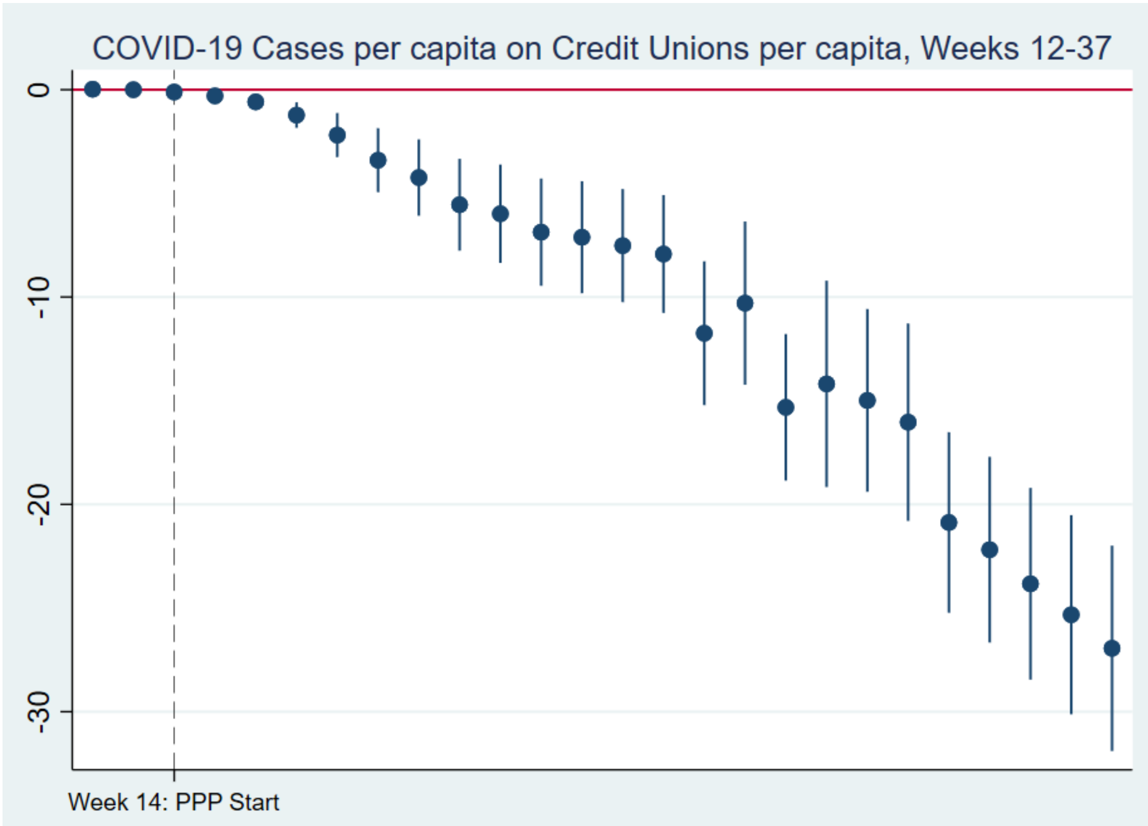
8 Tables and Figures

Figure 1: OLS Coefficients Over Time (Weeks 12-37) For Regression of COVID-19 Cases per capita on Community Banks per capita



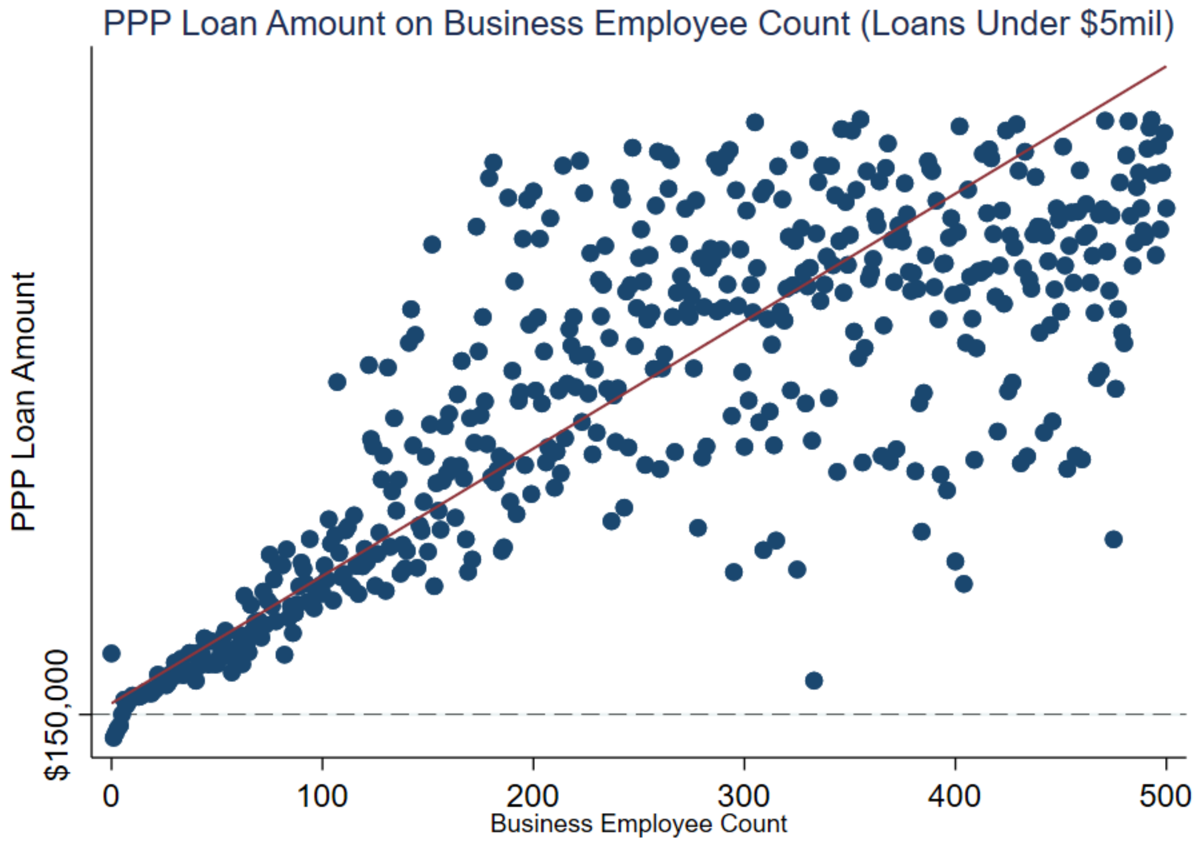
Note: *Community Banks per capita* refers to a variable indicating the number of community banks per capita in a county. The horizontal axis refers to the week number, from the 12^{th} to the 37^{th} week of 2020, and the vertical axis refers to the coefficient on *Community Banks per capita* for the regression of COVID-19 cases per capita on community banks per capita in a county. Each blue point represents the coefficient on *Community Banks per capita* for that given week.

Figure 2: OLS Coefficients Over Time (Weeks 12-37) For Regression of COVID-19 Cases per capita on Credit Unions per capita



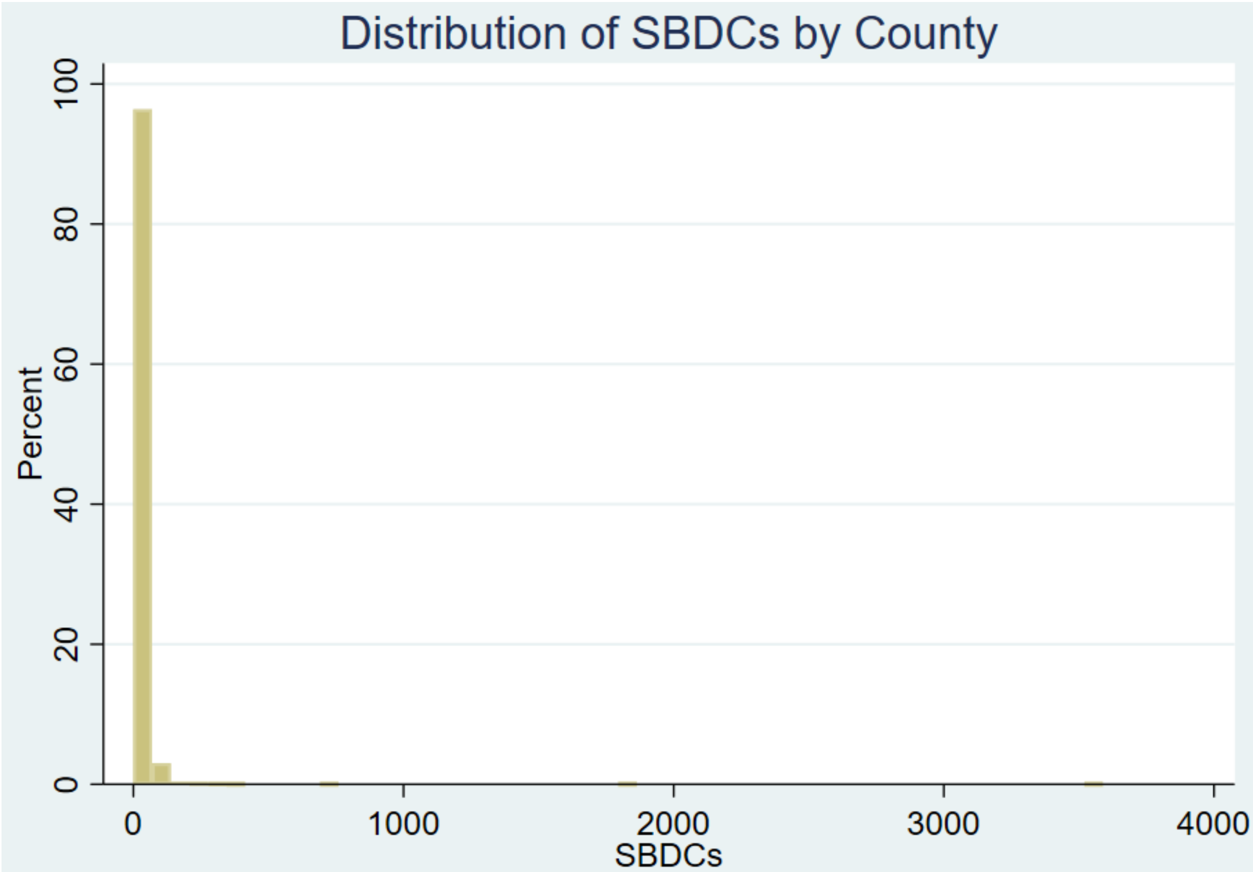
Note: *Credit Unions per capita* refers to a variable indicating the number of community banks per capita in a county. The horizontal axis refers to the week number, from the 12^{th} to the 37^{th} week of 2020, and the vertical axis refers to the coefficient on *Credit Unions per capita* for the regression of COVID-19 cases per capita on credit unions per capita in a county. Each blue point represents the coefficient on *Credit Unions per capita* for that given week.

Figure 3: PPP Loan Amount on Business Employee Count



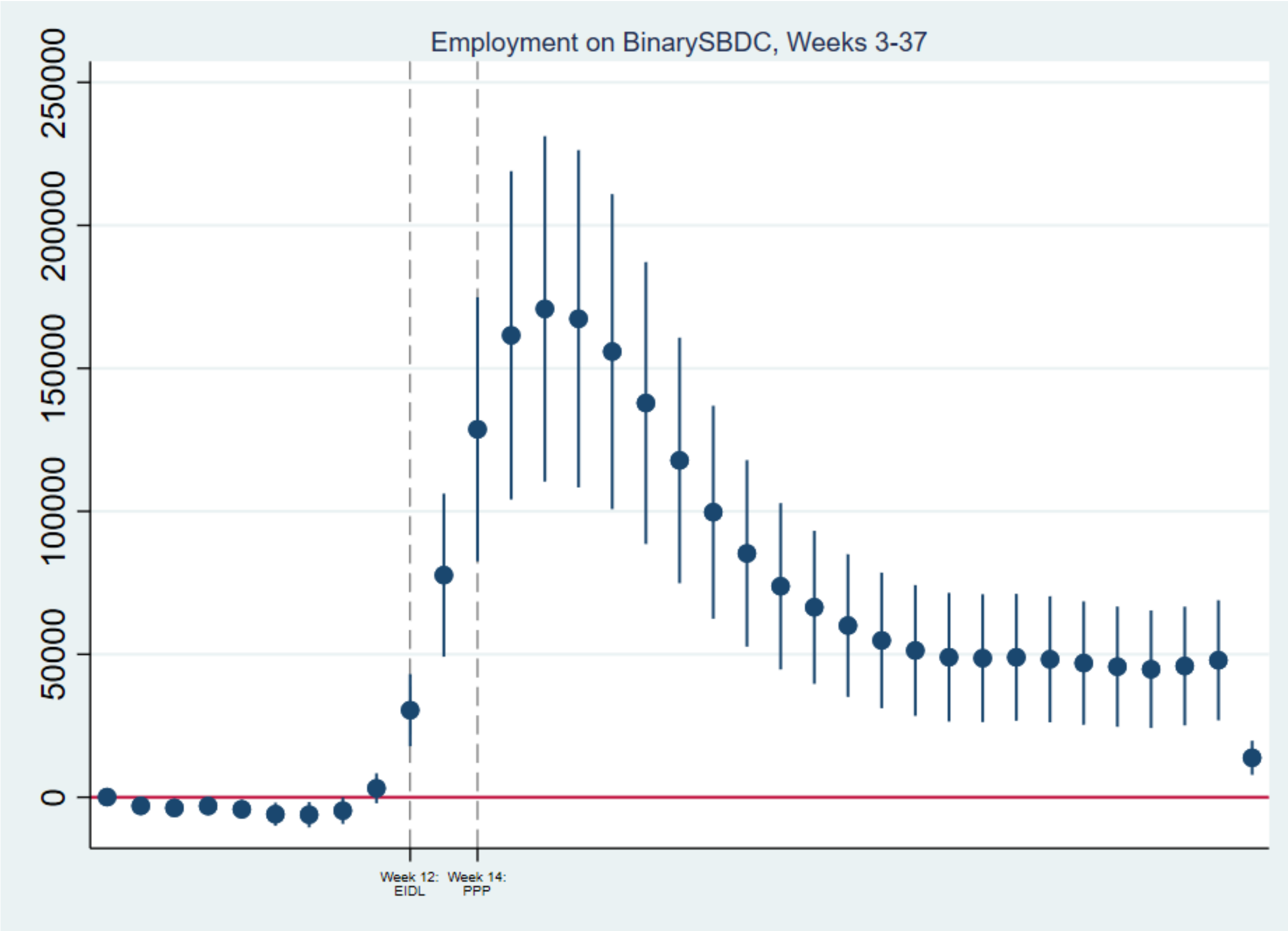
Note: *Business Employee Count* refers to a self-reported employee count that businesses listed in their PPP loan applications.

Figure 4: Distribution of SBDCs by County



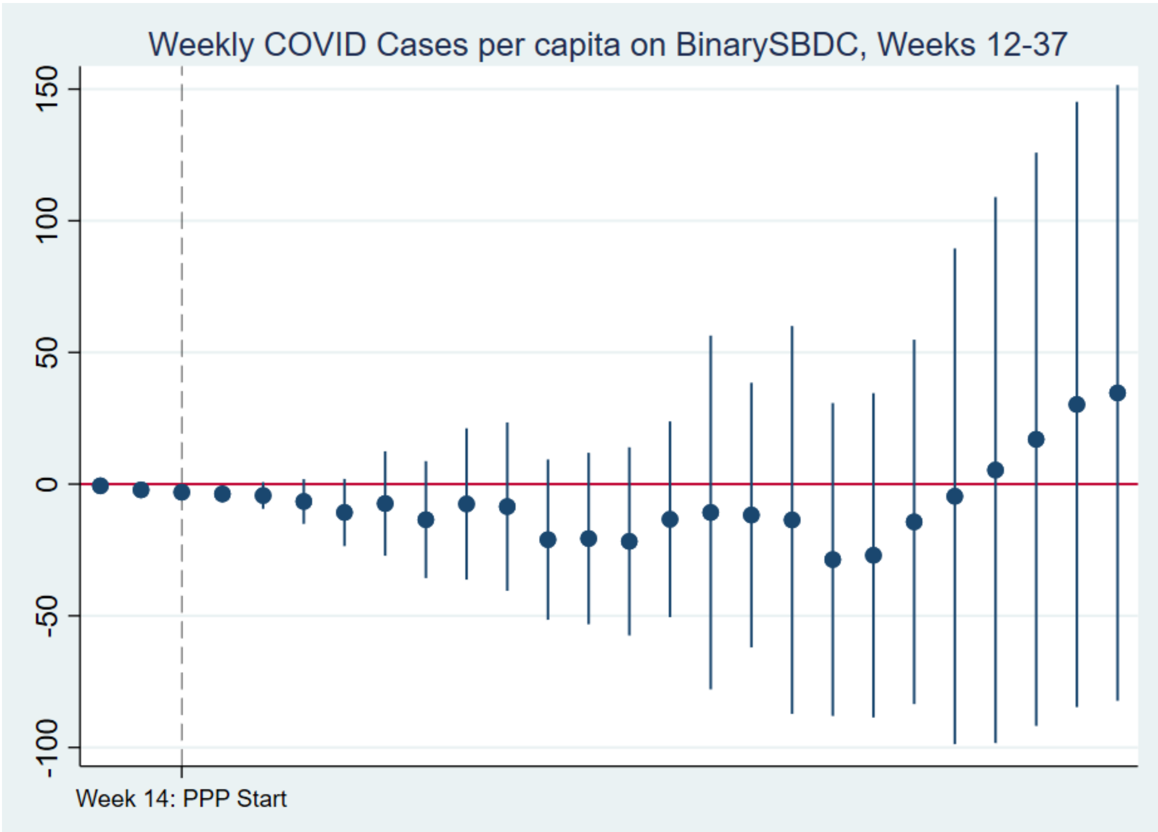
Note: The horizontal axis refers to the number of SBDCs by county and the vertical axis refers to percent frequency in this histogram.

Figure 5: OLS Coefficients Over Time (Weeks 3-37) For Regression of Employment on BinarySBDC



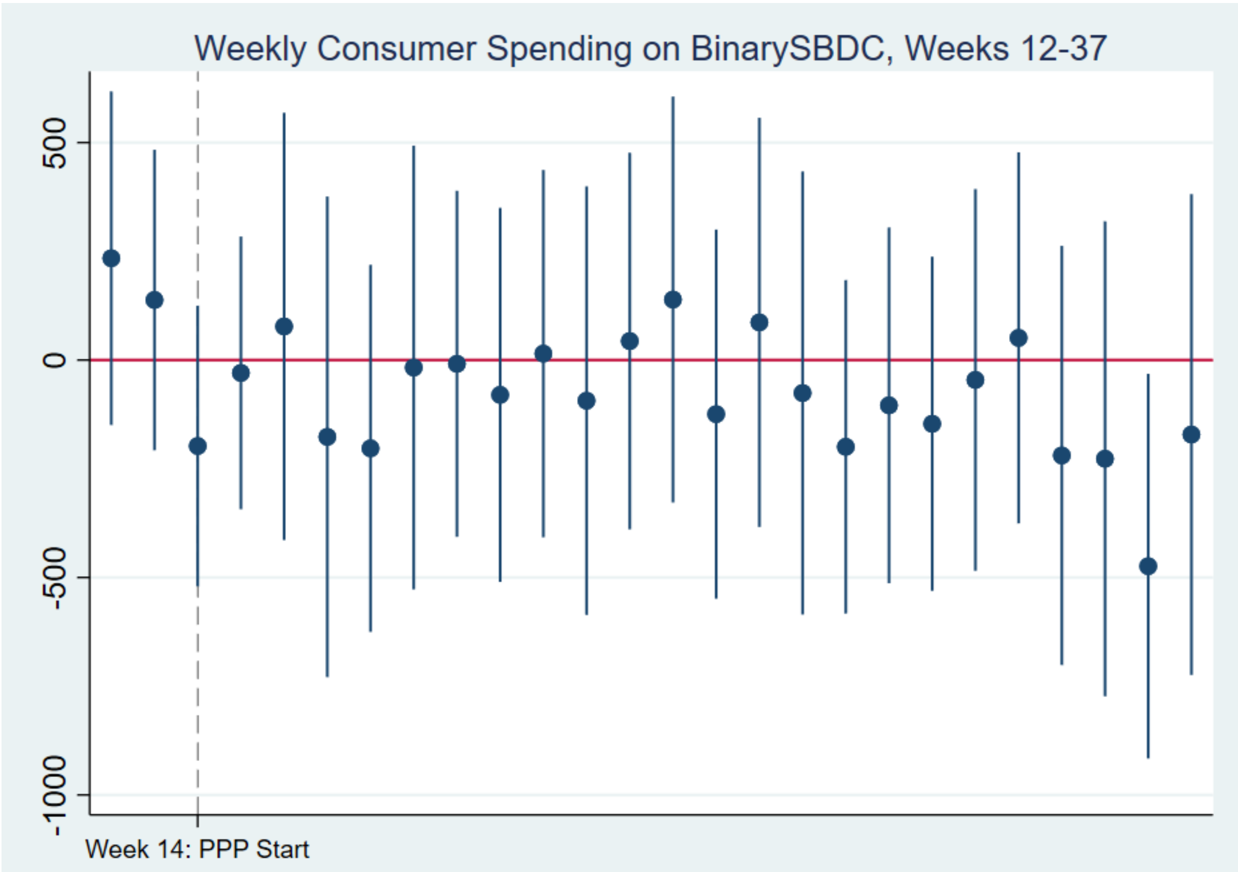
Note: *BinarySBDC* refers to a variable indicating whether a county has a small business development center (SBDC). The horizontal axis refers to the week number, from the 3rd to the 37th week of 2020, and the vertical axis refers to the coefficient on *BinarySBDC* for the regression of employment on whether a county has an SBDC. Each blue point represents the coefficient on *BinarySBDC* for that given week.

Figure 6: OLS Coefficients Over Time (Weeks 12-37) For Regression of Weekly COVID-19 Cases on BinarySBDC



Note: *BinarySBDC* refers to a variable indicating whether a county has a small business development center (SBDC). The horizontal axis refers to the week number, from the 12th to the 37th week of 2020, and the vertical axis refers to the coefficient on *BinarySBDC* for the regression of weekly COVID-19 cases per capita on whether a county has an SBDC. Each blue point represents the coefficient on *BinarySBDC* for that given week.

Figure 7: OLS Coefficients Over Time (Weeks 12-37) For Regression of Weekly Consumer Spending on BinarySBDC



Note: *BinarySBDC* refers to a variable indicating whether a county has a small business development center (SBDC). The horizontal axis refers to the week number, from the 12th to the 37th week of 2020, and the vertical axis refers to the coefficient on *BinarySBDC* for the regression of a weekly consumer spending measure on whether a county has an SBDC. Each blue point represents the coefficient on *BinarySBDC* for that given week. The consumer spending measure is provided by the Opportunity Insights Economic Tracker dataset and is "seasonally adjusted credit/debit card spending relative to January 4-31, 2020 in all merchant category codes."

Table 1: Summary Statistics

	Observations	Mean	Std. Dev	Min	Max
PPP Loan Amount per capita	110,563	13.088	50.874	0	4,032.189
SBDCs per capita	110,520	6.35e-6	2.71e-5	0	8.425e-4
Employment (Change in Employment Rate relative to January 2020)	107,532	-15.077	43.059	-404.3	161.6

Table 2: Summary Statistics for Time-Invariant Covariates, by Differential SBDC Exposure

Counties with No SBDCs

	Observations	Mean	Std. Dev	Min	Max
Population Density 2019	82,900	100.124	383.037	0.034	9,457.377
GDP per capita 2019	82,076	79.718	1,341.796	6.367	62,876.55
Unemployment per capita 2019	82,909	0.018	0.006	0.006	0.069
COVID-19 Case Count per capita Pre-PPP (Week 13)	82,910	0.007	0.002	0	0.055
Community Banks	53,352	1.999	1.543	1	27
Credit Unions	84,028	11.807	9.117	1	107

Counties with At Least One SBDC

	Observations	Mean	Std. Dev	Min	Max
Population Density 2019	27,601	645.641	2,485.134	0.123	48,358.11
GDP per capita 2019	26,602	48.844	25.436	9.517	368.542
Unemployment per capita 2019	27,640	0.017	0.006	0.007	0.107
COVID-19 Case Count per capita Pre-PPP (Week 13)	27,640	0.001	0.004	0	0.052
Community Banks	39,990	2.97	4.032	1	61
Credit Unions	56,404	19.615	22.089	1	366

Table 3: OLS Regressions of Employment on 1st Round PPP Loans, Weeks 9-16

	(1)	(2)	(3)	(4)
	Employment			
PPP Loan Amount	0.025** (0.008)	0.016* (0.007)	--	--
ln(PPP Loan Amount)	--	--	-2.627** (0.667)	-1.683+ (0.879)
Controls	N	Y	N	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.281	0.391	0.281	0.391
Root MSE	42.34	41.066	42.334	41.067
Observations	23,888	16,080	23,888	16,080

Notes: Table 3 reports results of OLS regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. I restrict the sample to loan amounts and employment between the 9th and 16th weeks of 2020 to measure the short run impact of the PPP. Controls include population density in 2019 by county, GDP per capita in 2019 by county, unemployment levels per capita in 2019 by county, COVID-19 case counts per capita by county in the week prior to the PPP program, the first reported COVID-19 EIDL and EIDL Advance loan amounts by county, the number of community banks per capita by county, the number of credit unions per capita by county, COVID-19 case counts per capita by county over time, and dummy variables indicating when states implemented and removed stay at home orders and all business closure regulations. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the +10%, *5%, **1% significance levels.

Table 4: Two Stage Least Squares Regression of Employment on 1st Round PPP Loans, Weeks 9-16 (Short Run Impact)

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
	Employment	Employment	PPP Loan Amount	Employment
PPP Loan Amount	0.016* (0.007)	--	--	0.305** (0.099)
BinarySBDC x PostPPP1	--	1,106.601** (264.256)	362,713.9** (98,495.11)	--
Instrument	--	--	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	--	--	13.56
BinarySBDC	--	-270.317 (188.164)	-68,502.45** (20,803.97)	-6,132.302 (15,180.91)
Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.391	0.391	0.378	0.26
Root MSE	41.066	0.411	65.698	45.17
Observations	16,080	16,080	16,080	16,080

Notes: Table 4 reports results of OLS and IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). I restrict the sample to loan amounts and employment between the 9th and 16th weeks of 2020 to measure the short run impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the +10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 5: Two Stage Least Squares Regression of Employment on 1st Round PPP Loans, Weeks 9-27 (Medium Run Impact)

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
	Employment	Employment	PPP Loan Amount	Employment
PPP Loan Amount	-0.019* (0.009)	--	--	0.236** (0.065)
BinarySBDC x PostPPP1	--	85,466.71** (17,365.18)	361,542** (91,608.52)	--
Instrument	--	--	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	--	--	15.58
BinarySBDC	--	-2,006.814 (23,042.9)	-18,916.03 (16,819.68)	2,464.839 (21,193.62)
Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.404	0.404	0.401	0.354
Root MSE	42.681	42.686	48.029	44.367
Observations	38,190	38,190	38,190	38,190

Notes: Table 5 reports results of OLS and IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). I restrict the sample to loan amounts and employment between the 9th and 27th weeks of 2020 to measure the medium run impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^{+}$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 6: Two Stage Least Squares Regression of Employment on 1st Round PPP Loans, Weeks 9-37 (Long Run Impact)

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
	Employment	Employment	PPP Loan Amount	Employment
PPP Loan Amount	-0.017+ (0.009)	--	--	0.252** (0.073)
BinarySBDC x PostPPP1	--	91,970.22** (19,950.12)	365,633.2** (94,370.02)	--
Instrument	--	--	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	--	--	15.01
BinarySBDC	--	-1,058.216 (19,722.52)	-11,935.6 (10,955.67)	1,944.028 (18,468.69)
Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.371	0.371	0.418	0.325
Root MSE	39.012	39.014	39.158	40.382
Observations	58,290	58,290	58,290	58,290

Notes: Table 6 reports results of OLS and IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). I restrict the sample to loan amounts and employment between the 9th and 37th weeks of 2020 to measure the long run impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the +10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 7: Two Stage Least Squares Regression of Employment on PPP Loans After the PPP Flexibility Act, Short Run Impact

	(1) IV First Stage	(2) IV Second Stage
	PPP Loan Amount	Employment
	Weeks 9-27	Weeks 9-27
PPP Loan Amount	--	0.551** (0.198)
BinarySBDC x PostPPPF	-85,788.99** (26,584.52)	--
Instrument	--	BinarySBDC x PostPPPF
First Stage F-Statistic	--	10.41
BinarySBDC	40,211.92** (14,089.85)	11,039.53 (20,742.92)
Additional Controls	Y	Y
Controls	Y	Y
State FE	Y	Y
Week FE	Y	Y
R^2	0.472	0.19
Root MSE	37.274	44.24
Observations	58,290	58,290

Notes: Table 7 reports results of OLS and IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *BinarySBDC x PostPPPF* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks following the passing of the PPP Flexibility Act (Weeks 24-37). I restrict the sample to loan amounts and employment between the 9th and 37th weeks of 2020 to measure the immediate impact of receiving the PPP funds after the PPP Flexibility Act. I include the same set of controls as in Table 3, as well as the additional controls described in Table 7. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^{+}$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 8: Two Stage Least Squares Regression of Employment on 1st Round PPP Loans for Counties with Above-Median Food and Accommodation Sector Exposure (By Employee Count per Total Workforce)

	(1) IV First Stage	(2) IV Second Stage
	Food and Accommodation	
	PPP Loan Amount	Employment
PPP Loan Amount	--	0.55** (0.169)
BinarySBDC x PostPPP1	942,396.8** (220,417.4)	--
Instrument	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	18.28
BinarySBDC	-351,938.7** (81,445.06)	98,614.9 (70,712.51)
Additional Controls	Y	Y
Controls	Y	Y
State FE	Y	Y
Week FE	Y	Y
R^2	0.657	0.375
Root MSE	34.367	47.625
Observations	8,648	8,648

Notes: Table 8 reports results of IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020, for counties with Food and Accommodation industry employee counts per total county working population greater than the median. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). In all columns, I restrict the sample to loan amounts and employment between the 9th and 16th weeks of 2020 to measure the immediate impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. Here, however, I also include additional controls: a dummy variable indicating whether a county received 1st round PPP at all, a dollar amount of 1st round PPP per capita a county received, and variables measuring the employment (measured as a change in the employment rate relative to January 2020) in week 12 and 13, respectively. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^{+}$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 9: Two Stage Least Squares Regression of Employment on 1st Round PPP Loans for Counties with Above-Median Food Services and Drinking Places Subsector Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	IV First Stage	IV Second Stage	IV First Stage	IV Second Stage	IV First Stage	IV Second Stage
	Short Run		Medium Run		Long Run	
	PPP Loan Amount	Employment	PPP Loan Amount	Employment	PPP Loan Amount	Employment
PPP Loan Amount	--	0.818** (0.278)	--	0.526** (0.15)	--	0.61** (0.178)
BinarySBDC x PostPPP1	608,808** (180,680)	--	619,883** (165,038)	--	617,002** (169,242)	--
Instrument	--	BinarySBDC x PostPPP1	--	BinarySBDC x PostPPP1	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	11.35	--	14.11	--	13.29
BinarySBDC	-229,930** (67,207)	97,629 (59,619)	-105,834** (27,483)	133,153 (85,817)	-68,388** (18,211)	97,722 (73,862)
Additional Controls	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y
R^2	0.614	0.199	0.583	0.346	0.587	0.299
Root MSE	37.747	51.81	30.743	48.744	25.518	45.207
Observations	8,608	8,608	20,444	20,444	31,204	31,204

Notes: Table 9 reports results of IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020, for counties with Food Services and Drinking Places subsector employee counts per total county working population greater than the median. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). In Columns 1 and 2, I restrict the sample to loan amounts and employment between the 9th and 16th weeks of 2020 to measure the immediate impact of receiving the first round of PPP funds. Columns 3 and 4 restrict weeks to the 9th to 27th weeks to measure the medium run impact, and Columns 5 and 6 restrict weeks to the 9th to 37th weeks to measure the long run impact. I include the same set of controls as in Table 3. Here, however, I also include additional controls: a dummy variable indicating whether a county received 1st round PPP at all, a dollar amount of 1st round PPP per capita a county received, and variables measuring the employment (measured as a change in the employment rate relative to January 2020) in week 12 and 13, respectively. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the +10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 10: Two Stage Least Squares Regression of Employment on PPP Loans Post-PPP Flexibility Act for Counties with Above-Median Food Services and Drinking Places Subsector Exposure

	(1) IV First Stage	(2) IV Second Stage
	PPP Loan Amount	Employment
PPP Loan Amount	--	1.661* (0.719)
BinarySBDC x PostPPPF	-120,069.2* (49,076.09)	--
Instrument	--	BinarySBDC x PostPPPF
First Stage F-Statistic	--	5.99
BinarySBDC	53,402.27+ (27,276.87)	102,407.1 (76,111.66)
Additional Controls	Y	Y
Controls	Y	Y
State FE	Y	Y
Week FE	Y	Y
R^2	0.584	--
Root MSE	25.603	60.759
Observations	31,204	31,204

Notes: Table 10 reports results of IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020, for counties with Food Services and Drinking Places subsector employee counts per total county working population greater than the median. The variable *BinarySBDC x PostPPPF* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks after the PPP Flexibility Act (weeks 24-37). In all columns, I restrict the sample to loan amounts and employment between the 9^{th} and 37^{th} weeks of 2020 to measure the immediate impact of receiving the funds. I include the same set of controls as in Table 3. Here, however, I also include additional controls: a dummy variable indicating whether a county received 1^{st} round PPP at all, a dollar amount of 1^{st} round PPP per capita a county received, and variables measuring the employment (measured as a change in the employment rate relative to January 2020) in week 12 and 13, respectively. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^{+}$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 11: Two Stage Least Squares Regression of Employment on 1st Round PPP Loans for Counties with Above-Median Accommodation Subsector Exposure

	(1) IV First Stage	(2) IV Second Stage	(3) IV First Stage	(4) IV Second Stage	(5) IV First Stage	(6) IV Second Stage
	Short Run		Medium Run		Long Run	
	PPP Loan Amount	Employment	PPP Loan Amount	Employment	PPP Loan Amount	Employment
PPP Loan Amount	--	0.44** (0.161)	--	0.271** (0.091)	--	0.294** (0.103)
BinarySBDC x PostPPP1	490,583** (117,426)	--	505,153** (117,424)	--	505,727** (117,224)	--
Instrument	--	BinarySBDC x PostPPP1	--	BinarySBDC x PostPPP1	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	17.45	--	18.51	--	18.61
BinarySBDC	-183,303** (43,776)	66,667* (30,728)	-83,235** (24,676)	88,614* (44,244)	-54,410** (15,984)	68,131+ (37,964)
Additional Controls	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y
R^2	0.614	0.355	0.582	0.374	0.585	0.345
Root MSE	41.998	46.144	33.863	47.509	28.098	43.528
Observations	8,608	8,608	20,444	20,444	31,204	31,204

Notes: Table 11 reports results of IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020, for counties with Accommodation subsector employee counts per total county working population greater than the median. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). In Columns 1 and 2, I restrict the sample to loan amounts and employment between the 9th and 16th weeks of 2020 to measure the immediate impact of receiving the first round of PPP funds. Columns 3 and 4 restrict weeks to the 9th to 27th weeks to measure the medium run impact, and Columns 5 and 6 restrict weeks to the 9th to 37th weeks to measure the long run impact. I include the same set of controls as in Table 3. Here, however, I also include additional controls: a dummy variable indicating whether a county received 1st round PPP at all, a dollar amount of 1st round PPP per capita a county received, and variables measuring the employment (measured as a change in the employment rate relative to January 2020) in week 12 and 13, respectively. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the +10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table 12: Two Stage Least Squares Regression of Employment on PPP Loans post-PPP Flexibility Act for Counties with Above-Median Accommodation Subsector Exposure

	(1) IV First Stage	(2) IV Second Stage
	PPP Loan Amount	Employment
PPP Loan Amount	--	0.608* (0.293)
BinarySBDC x PostPPPF	-105,869.8** (28,363.91)	--
Instrument	--	BinarySBDC x PostPPPF
First Stage F-Statistic	--	13.93
BinarySBDC	49,042.38** (15,877.17)	68,752.31+ (38,678.39)
Additional Controls	Y	Y
Controls	Y	Y
State FE	Y	Y
Week FE	Y	Y
R^2	0.582	0.261
Root MSE	28.213	46.26
Observations	31,204	31,204

Notes: Table 12 reports results of IV regressions examining the relationship between PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020, for counties with Accommodation subsector employee counts per total county working population greater than the median. The variable *BinarySBDC x PostPPPF* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks after the PPP Flexibility Act (weeks 24-37). In all columns, I restrict the sample to loan amounts and employment between the 9th and 37th weeks of 2020 to measure the immediate impact of receiving the funds. I include the same set of controls as in Table 3. Here, however, I also include additional controls: a dummy variable indicating whether a county received 1st round PPP at all, a dollar amount of 1st round PPP per capita a county received, and variables measuring the employment (measured as a change in the employment rate relative to January 2020) in week 12 and 13, respectively. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the +10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

9 Appendix

Table A.1: Literature Review Summary

Authors	Employment Data	Observation Level	PPP Loan Data Sample	Basic Method	Main Result(s)
Chetty et al., 2020	Opportunity Insights Economic Tracker	Firms who received PPP	Loans made to firms above/below the 500-employee program eligibility cutoff	Difference-in-differences	Small, non-substantial effect of PPP on employment
Granja et al., 2020	Homebase high-frequency employment data	Firms who received PPP	Loans above $150,000	IV; used heterogeneity in bank lending behavior to instrument for PPP loan amounts	No substantial effect of PPP on local economic outcomes
Autor et al., 2020	Administrative data from ADP	Firms who received PPP	Loans made to firms above/below the 500-employee cutoff	Difference-in-differences	PPP boosted employment at eligible firms by 2-4.5 percent
Bartik et al., 2020	Firm level data from Alignable	Surveyed firms	Based on surveyed firms	IV; used variation in firm exposure to larger banks, who provided relatively fewer PPP loans	14-30 pp rise in PPP-receiving firms' forecasted survival probabilities; imprecise employment effect
Hubbard and Strain, 2020	Dun & Broadstreet Corporation	Firms in D&B data that applied for PPP	Loans above $150,000 (exact amount unknown)	Difference-in-differences, "intent to treat" method where loan take-up was measured by firms who applied for, but did not necessarily receive, PPP	PPP substantially increased employment, financial health, and survival of small businesses

Table A.2: Robustness Checks for IV Regressions, Two Stage Least Squares Regression of Employment on 1st Round PPP Loans

	(1)	(2)	(3)	(4)	(5)	(6)
	PPP1 Short Run		PPP1 Medium Run		PPP1 Long Run	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
	PPP Loan Amount	Employment	PPP Loan Amount	Employment	PPP Loan Amount	Employment
PPP Loan Amount	--	0.367** (0.099)	--	0.248** (0.067)	--	0.263** (0.075)
BinarySBDC x PostPPP1	356,693** (98,307)	--	360,979** (91,603)	--	365,237** (94,364)	--
Instrument	--	BinarySBDC x PostPPP1	--	BinarySBDC x PostPPP1	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	13.17	--	15.53	--	14.98
BinarySBDC	-68,530** (21,157)	-12,648 (13,459)	-18,730 (16,865)	1,419 (20,545)	-11,844 (10,978)	1,174 (17,959)
Emp12, Emp13?	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y
R^2	0.379	0.229	0.401	0.355	0.418	0.327
Root MSE	65.661	46.129	48.029	44.333	39.158	40.312
Observations	16,080	16,080	38,190	38,190	58,290	58,290

Notes: Table A.2 reports the same IV regressions examining the relationship between PPP loan amount per capita in a county and employment, with the inclusion of *Emp12* and *Emp13*, variables measuring the employment (measured as a change in the employment rate relative to January 2020) in week 12 and 13, respectively. This serves as a robustness check, by introducing among the regressors in both stages a measure for employment after the start of the EIDL program, in week 12, and before the start of the PPP, in week 14. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^+$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table A.3: Robustness Checks, Two Stage Least Squares Regression of Employment on PPP Loans Following the PPP Flexibility Act

	(1) 1st Stage	(1) 2nd Stage
	PPP Loan Amount	Employment
PPP Loan Amount	--	0.515** (0.186)
BinarySBDC x PostPPPF	-86,052** (26,594)	--
Instrument	--	BinarySBDC x PostPPPF
First Stage F-Statistic	--	10.47
BinarySBDC	40,389** (14,110)	10,480 (20,274)
Emp12, Emp13?	Y	Y
Controls	Y	Y
State FE	Y	Y
Week FE	Y	Y
R^2	0.472	0.219
Root MSE	37.274	43.448
Observations	58,290	58,290

Notes: Table A.3 reports the same IV regressions examining the relationship between PPP loan amount per capita in a county and employment, with the inclusion of *Emp12* and *Emp13*, variables measuring the employment (measured as a change in the employment rate relative to January 2020) in week 12 and 13, respectively. This serves as a robustness check, by introducing among the regressors in both stages a measure for employment after the start of the EIDL program, in week 12, and before the start of the PPP, in week 14. The variable *BinarySBDC x PostPPPF* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over which PPP funds after the PPP Flexibility Act (weeks 24-37) were distributed. I also include additional controls: a dummy variable indicating whether a county received 1st round PPP at all, and a dollar amount of 1st round PPP per capita a county received. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^+$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table A.4: Two Stage Least Squares Regressions of Employment on Early EIDL Loan Amounts and Total PPP and Early EIDL Loan Amounts

	(1) IV First Stage	(2) IV Second Stage	(3) IV First Stage	(4) IV Second Stage
	Early EIDL Loan Amount	Employment	Early EIDL Loan Amount + PPP	Employment
	Weeks 9-27	Weeks 9-27	Weeks 9-27	Weeks 9-27
Early EIDL Loan Amount	--	0.966 (0.75)	--	--
Early EIDL Loan Amount + PPP	--	--	--	0.201** (0.078)
BinarySBDC x PostPPP1	114,976.9 (87,538.17)	--	551,517.6** (193,307.1)	--
Instrument	--	BinarySBDC x PostPPP1	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	1.73	--	8.14
BinarySBDC	-648,506 (1,625,794)	601,228.4 (1,602,612)	-1,546,259 (2,825,822)	286,248.9 (565,748.7)
Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.457	--	0.456	--
Root MSE	2,140.3	2,060.6	3,987.2	799.88
Observations	16,080	16,080	16,080	16,080

Notes: Table A.4 reports results of IV regressions examining the relationship between employment and *Early EIDL Loan Amount*, referring to EIDL loans and advances as of April 24, 2020 (week 17, 2020), the earliest date at which data was available (Columns 1 and 2), and the relationship between employment and the sum of the early EIDL loan amount and PPP loan amounts (Columns 3 and 4). PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *BinarySBDC x PostPPP1* is the instrument for *PPP Loan Amount* and is the interaction between a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). In all columns, I restrict the sample to loan amounts and employment between the 9^{th} and 16^{th} weeks of 2020 to measure the immediate impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^{+}$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table A.5: Two Stage Least Squares Regression of Employment on Logged 1st Round PPP Loans, Weeks 9-16 (Short Run Impact)

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
	Employment	Employment	ln(PPP Loan Amount)	Employment
ln(PPP Loan Amount)	-1.683+ (0.879)	--	--	26.357** (7.738)
BinarySBDC x PostPPP1	--	110,670.2** (26,429.26)	4,198.947** (925.245)	--
Instrument	--	--	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	--	--	20.6
BinarySBDC	--	-27,030.93 (18,818.76)	-734.207** (157.95)	-7,679.683 (16,020.19)
Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.391	0.391	0.915	0.29
Root MSE	41.067	41.065	0.595	44.247
Observations	16,080	16,080	16,080	16,080

Notes: Table A.5 reports results of OLS and IV regressions examining the relationship between the natural log of PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *ln(BinarySBDC) x PostPPP1* is the instrument for *ln(PPP Loan Amount)* and is the interaction between the natural log of a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). I restrict the sample to loan amounts and employment between the 9th and 16th weeks of 2020 to measure the short run impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the +10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table A.6: Two Stage Least Squares Regression of Employment on Logged 1st Round PPP Loans, Weeks 9-27 (Medium Run Impact)

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
	Employment	Employment	ln(PPP Loan Amount)	Employment
ln(PPP Loan Amount)	-5.384** (0.703)	--	--	22.501** (5.034)
BinarySBDC x PostPPP1	--	85,475.98** (17,367.13)	3,798.806** (706.585)	--
Instrument	--	--	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	--	--	28.9
BinarySBDC	--	-2,003.767 (23,045.51)	139.205 (349.956)	-5,135.974 (21,539.65)
Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.408	0.404	0.846	0.296
Root MSE	42.542	42.686	0.661	46.328
Observations	38,190	38,190	38,190	38,190

Notes: Table A.6 reports results of OLS and IV regressions examining the relationship between the natural log of PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *ln(BinarySBDC) x PostPPP1* is the instrument for *ln(PPP Loan Amount)* and is the interaction between the natural log of a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). I restrict the sample to loan amounts and employment between the 9th and 27th weeks of 2020 to measure the medium run impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^{+}$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

Table A.7: Two Stage Least Squares Regression of Employment on Logged 1st Round PPP Loans, Weeks 9-37 (Long Run Impact)

	(1) OLS	(2) Reduced Form	(3) IV First Stage	(4) IV Second Stage
	Employment	Employment	ln(PPP Loan Amount)	Employment
ln(PPP Loan Amount)	-5.1** (0.639)	--	--	23.907** (5.962)
BinarySBDC x PostPPP1	--	91,980.35** (19,952.41)	3,847.483** (782.158)	--
Instrument	--	--	--	BinarySBDC x PostPPP1
First Stage F-Statistic	--	--	--	24.2
BinarySBDC	--	-1,055.918 (19,724.77)	241.673 (272.131)	-6,833.493 (19,654.49)
Controls	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
R^2	0.375	0.371	0.844	0.248
Root MSE	38.896	39.014	0.604	42.637
Observations	58,290	58,290	58,290	58,290

Notes: Table A.7 reports results of OLS and IV regressions examining the relationship between the natural log of PPP loan amount per capita in a county and employment, measured as a change in the employment rate relative to January 2020. The variable *ln(BinarySBDC) x PostPPP1* is the instrument for *ln(PPP Loan Amount)* and is the interaction between the natural log of a variable indicating whether or not a county has a small business development center (SBDC) and a dummy variable for the weeks over the first round of PPP (weeks 14-16). I restrict the sample to loan amounts and employment between the 9th and 37th weeks of 2020 to measure the long run impact of receiving the first round of PPP funds. I include the same set of controls as in Table 3. All regressions include state and week fixed effects, and standard errors are presented in parentheses and are clustered at the county level. Coefficients are individually statistically significant at the $^{+}$10%, *5%, **1% significance levels. The first stage F-statistic tests the hypothesis that the coefficient on the instrument is zero in the first-stage regression when 2SLS is used.

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