Better Late than Never? Quantifying Funding and Liquidity Effects of the PPP on Pandemic-Era Employment Recovery*

Olga Gorbachev
University of Delaware

Maria Luengo-Prado
ICAE

J. Christina Wang
Federal Reserve Bank of Boston

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Abstract

This paper examines how the Paycheck Protection Program (PPP) supported employment during the COVID-19 period by distinguishing between its short-term liquidity and broader funding effects. Using county-level data, we show that delays in loan disbursement had prolonged negative impacts only on the smallest urban businesses. To identify the causal impact of funding itself, we develop an instrument based on the pre-pandemic payroll share in small establishments. Instrumental-variable and firm-level analyses reveal that PPP funding improved business survival and employment recovery substantially and broadly, with an estimated cost of roughly $89,000 per job-year, consistent with other large-scale fiscal interventions.

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JEL Codes: J21, H81, G28.

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1 Introduction

As the COVID-19 pandemic swept through US coastal cities in March 2020, Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act, dispensing broad-based fiscal assistance on an unprecedented scale. A key component of this package was the Paycheck Protection Program (PPP), which provided loans that were effectively grants to most small businesses whose operations were disrupted by the pandemic.[1] One of the primary goals of the PPP was to help small businesses retain workers despite having to reduce operations or shut down completely. Using primarily county-level data, we examine the mechanisms through which the PPP influenced employment retention and recovery, distinguishing between its *short-term liquidity* effect linked to the timing of a loan and its broader *funding receipt* effect tied to receiving a loan. A key contribution of our study is to isolate and compare these two effects, thereby enhancing the understanding of how different features of the PPP influenced its efficacy. This can help inform the design of government support programs if similar needs arise in the future.

The short-term liquidity effect of PPP stems from differences in the timing of loan disbursement to recipients. Timely funding may have allowed firms to bridge temporary cash-flow shortfalls, helping them retain workers during the initial weeks of the crisis. The broader funding receipt effect goes beyond the immediate liquidity relief and captures the longer-term impact of capitalization provided by a government transfer. These mechanisms are conceptually distinct but empirically intertwined.[2] To isolate the liquidity effect, we exploit variation in loan timing across recipients. We then estimate the funding effect using an instrumental-variable approach, where PPP receipt is instrumented by pre-COVID payroll shares of small firms, while controlling for loan timing. Our results show that delays in funding, which isolate the short-term impact due to liquidity constraints, were especially harmful for the smallest urban businesses,

[1] In the rest of the article, "small" refers to businesses with fewer than 500 employees, and "smallest" refers to businesses with fewer than 20 employees.

[2] We thank the referees for highlighting these subtleties.

while the receipt of PPP funds substantially improved business survival and employment recovery more broadly across businesses.

From the early days of PPP, researchers have examined the effectiveness of the program in supporting small business operations and local economies, as well as its efficiency in funding allocation.[3] As an early study, Hubbard and Strain (2020) use Dun & Bradstreet data to compare firms that applied for loans with those that did not (without observing actual receipt). They found that eligible firms, those under 500 employees, were less likely to close and recovered more quickly in employment and payment behavior than slightly larger, ineligible firms. Granja et al. (2022) use Homebase microdata and a supply-side instrument based on preexisting small business bank relationships, which captures distortions in access to financing during the early months of the PPP. They show that greater local access to PPP funds significantly reduced missed payments and increased the share of firms with at least three months of cash reserves at the state-industry level, highlighting the program's direct impact on firm liquidity. Kurmann, Lalé, and Ta (2025), also using Homebase data at the firm level, further underscore the importance of timely liquidity support.

Our analysis complements and extends these studies in three key ways. First, we distinguish between two conceptually distinct but empirically related effects of the PPP: a short-term liquidity effect and the broader impact of receiving funds. Second, unlike previous studies that focus primarily on firm-level outcomes over a short horizon, we examine these effects across firm sizes and geographic areas over a two-year horizon, capturing both longer-term impacts and potential local spillovers. Third, we exploit demand-side variation in area-level PPP take-up to identify these effects, complementing supply-side studies such as Granja et al. (2022) and short-term timing analysis such as Doniger and Kay (2023).

To capture the short-term liquidity effect, we follow Doniger and Kay (2023) (hereafter DK), who exploit a 10-day pause in PPP funding triggered by the exhaustion of

[3] In addition to the studies explicitly cited in this section, a partial list of additional important or closely related studies includes Autor et al. (2022c) (which contains a more complete review of the literature), Dalton (2023), Faulkender, Jackman, and Miran (2023) and Chetty et al. (2024).

the initial appropriation. This pause provides plausibly exogenous variation in loan timing, allowing DK to estimate the causal effect of a funding delay on local employment. They find that the adverse consequences of the funding delay were concentrated among the self-employed and the smallest firms. Intriguingly, these effects persisted throughout 2020, even after the economy began reopening in May and despite the fact that $145 billion of the $670 billion allocated for the 2020 PPP remained unused when the program closed on August 8, 2020. To understand the persistent effects of a brief funding delay, we first analyze the determinants of the PPP delay à la DK, referred to as the "share delayed" in all subsequent analysis. We find that the share delayed correlates with preexisting county-level banking conditions (namely, the presence of community versus large national banks and the prevalence of small-business lending).[4] These correlations indicate that the share delayed is partly endogenous, reflecting local financial infrastructure and lending capacity constraints. Counties with higher demand for loans, combined with these institutional constraints, experienced a larger proportion of delayed loans. As a result, when employment is measured in levels, the estimated coefficient of share delayed appears significantly negative, largely reflecting the impact in large counties with the largest delays.

Using county-level monthly employment data from the Quarterly Census of Employment and Wages (QCEW), we confirm that the share delayed correlates negatively with total private employment through January 2021 in the full sample of US counties when the employment outcome is specified in levels. However, once the outcome is specified in logarithms, normalized by population, or (normalized) changes, as is commonly done in policy evaluation studies using state- or county-level data to avoid undue influence of the largest states or counties (see, e.g., Chodorow-Reich et al. 2012), the coefficients on the share delayed lose their significance. In some cases, they even change signs. As a further test, we estimate the specification in levels separately for urban versus rural counties. This exercise reveals that the statistical significance of the

[4] Granja et al. (2022), Balyuk, Prabhala, and Puri (2020), and Li and Strahan (2021) also find that firms with preexisting bank ties received better access to PPP loans early on.

negative association between the share delayed and the level of employment is driven by urban counties. These results align with the proposed mechanism, indicating that unobserved vulnerability factors in urban communities during COVID-19 led to both higher shares of loans delayed and greater declines in employment levels.

Nevertheless, the share delayed likely contains an exogenous component that captures a genuine short-term liquidity shock. This component should be most strongly associated with employment outcomes among firms for which timely access to liquidity was critical, particularly the smallest businesses. For these firms, which typically operate with thin cash buffers and weaker banking relationships, even brief cash flow shortfalls could be especially damaging. Using Quarterly Workforce Indicators (QWI) data on stable employment by firm size (since QCEW measures employment by size only annually), we find that the share delayed had a prolonged negative impact specifically on employment in *urban* establishments with fewer than 20 employees (the smallest size category in the QWI).[5] This finding is consistent with Kurmann, Lalé, and Ta (2025), who document that PPP share delayed imposed significant costs on the smallest firms, contributing to higher rates of business closures.

Having examined the cross-sectional properties of the effect of share delayed, the second part of our paper focuses on estimating the funding effect of PPP. To do so, we construct an instrumental variable (IV) to address endogeneity concerns: the amount of PPP funds received may be endogenous because small businesses most likely consider both the degree of pandemic-induced disruptions and their future prospects in deciding whether and when to apply for loans. Our IV is inspired by Chodorow-Reich et al. (2012), who use pre-recession Medicaid spending to instrument fiscal transfers delivered through the Medicaid reimbursement process during the Great Recession. Analogously, we exploit the exogenously imposed eligibility threshold of the PPP, limiting access to firms with fewer than 500 employees, to construct a county-level instrument based on the 2019 payroll share in small establishments. Counties with a higher concentration

[5] QWI stable employment tallies the number of jobs held on both the first and the last day of each quarter with the same employer.

of payroll in small establishments were positioned to receive relatively more PPP funds per dollar of payroll, mechanically generating cross-county variation in potential PPP exposure. When analyzing employment in the smallest establishments (fewer than 20 employees), we refine the instrument to reflect their specific payroll share, excluding self-employed recipients not captured in the QCEW or QWI data. Since many rural counties lack large establishments and the share delayed affects only urban counties, our analysis focuses on urban areas where both program exposure and employment measurement are more reliable, allowing us to compare the magnitude of the two effects.

The instrument demonstrates a strong first stage: counties with higher small-establishment payroll shares received significantly more PPP funding relative to payroll. Its validity rests on the exclusion restriction, namely that local employment changes were affected by the small-establishment payroll share solely through its effect on PPP receipt. We mitigate potential violations by expressing outcomes in changes and including a rich set of controls, such as local banking structure, industry mix, demographics, and COVID-19 exposure, allowing each to have time-varying effects. Moreover, placebo tests using pre-pandemic data confirm that the instrument was uncorrelated with changes in employment to population prior to 2020, supporting the exclusion restriction. Together, these results indicate that our instrument captures plausibly exogenous variation in PPP funding intensity, enabling a credible identification of the program's employment effects.

We find that counties receiving greater PPP funding relative to their pre-COVID payroll experienced significantly stronger employment recovery. Based on our estimates, PPP receipt preserved approximately 5.85 million job-years in the urban counties in our sample during the study period. About 53 percent of these job-years were realized in 2020 and the remainder in 2021, corresponding to 2.9 and 2.5 percent of 2019 employment in these counties, respectively. We also calculate an implied cost per job-year (the cost of sustaining one job for a year) of $88,997, assuming PPP preserved employment through the end of 2021. These estimates are broadly consistent with prior studies, which generally report costs between \$100,000 and \$250,000. The lower cost in our analysis likely reflects the longer time horizon and/or spillover effects not captured in firm-level analysis. Whereas previous studies measure employment impacts only through 2020, mainly capturing short-term effects, our extension through the end of 2021 allows employment gains to accumulate and persist, spreading program cost over a longer period of time. Moreover, our use of county data naturally incorporates spillovers from PPP recipients to the broader local economy.

Finally, we use Advan firm-level data to more precisely isolate the effect of loan receipt from loan timing. Specifically, to measure the funding effect of PPP receipt, we study differences in business foot traffic between PPP recipients and closely matched (by industry, pre-pandemic visits, and geography) peer firms that did not receive PPP funds. We match firms based on geographic proximity to ensure that peers faced similar local conditions but exclude a "donut" zone around each PPP firm to reduce potential bias from competition dynamics among closely located firms. Our baseline defines peers as firms with similar pre-pandemic visits, in the same 6-digit NAICS code and county, but outside the PPP firm's Census block group, located 5 to 50 miles from the PPP firm.[6] In addition, we compare average treatment effects for late borrowers (those receiving loans just after the 10-day pause) and early borrowers (those receiving loans just before the 10-day pause). Applying either a difference-in-differences estimator or a staggered treatment estimator à la Sun and Abraham (2021), we find that both early and late borrowers benefited significantly from receiving PPP loans. Compared to their matched non-recipient peers, both groups of borrowers experienced a notable increase in visits and significantly fewer closures beginning in the second half of 2020. However, the difference in the treatment effect between early and late borrowers is small (about one-third of the funding effect) and statistically insignificant. This suggests that the funding effect was more important than the liquidity effect, even for most of the firms in the Advan sample, which tend to be small.

The remainder of the paper is organized as follows. Section 2 describes our method-

[6] Results are robust to alternative distance thresholds.

ology and the data sources. Section 3 examines the determinants of the share delayed. Section 4 presents our empirical findings on how the timing of PPP loans (the short-term liquidity effect) affected local employment recovery. In Section 5, we use two methods to estimate the effect of PPP funding and compare it with the effect of liquidity due to loan timing: an instrumental variable approach using county-level QCEW and QWI data, and a closest neighbor matching estimator using firm-level activity data. Section 6 discusses potential implications for policy and concludes.

2 Empirical Design and Data

2.1 Empirical Design

To investigate the impact of PPP funding on the local economy, we adopt the following empirical specification:

$$Y_{c,s,t} = \mu_s + \tau_t + \sum_{t \neq 2020:\mathrm{M1}}^{T} \left[\beta_t \mathrm{Policy}_{c,s} + \gamma_t X_{c,s} + \eta_t Y_{c,s,2019:M12}\right] + \varepsilon_{c,s,t}, \tag{1}$$

where $Y_{c,s,t}$ is the measure of local employment of interest in area c (county or CBSA depending on the specification) of state s at time t (month-year or quarter-year depending on the data source). Our analysis covers 2020 and 2021. $Y_{c,s,2019:M12}$ is a lag of the dependent variable, measured in 2019:M12 (or 2019:Q4 for QWI quarterly data), to control for possible preexisting heterogeneity. $X_{c,s}$ denotes a set of area-level controls, while μ_s and τ_t are state- and time-fixed effects, respectively. $\mathrm{Policy}_{c,s}$ refers to the PPP program, whose effect on employment is the key object of interest. Note that our regressors $\left[\mathrm{Policy}_{c,s}, X_{c,s}, Y_{c,s,2019:M12}\right]$ are allowed to influence the outcome $Y_{c,s,t}$ dynamically (that is, differently in each period), with the coefficients $[\beta_t, \gamma_t, \eta_t]$ summarizing their respective dynamic effects.

To receive PPP funds, businesses had to apply for a loan through an approved lender. A business' decision to apply for a loan was likely based, at least initially, on the severity of the pandemic-induced disruptions it suffered and its survival prospects.

Thus, to obtain the causal effect of PPP funds on employment, one needs to take into account the endogenous nature of the receipt of funds. We employ two measures to identify effects of the policy along two dimensions: loan *timing* versus loan *receipts*.

The first is the measure of loan timing proposed by DK, denoted Share-Delayed$_{c,s}$, which represents the share of PPP funds delayed due to the 10-day pause in lending that occurred after the first round of PPP funding was exhausted mid-day on April 16, 2020. Lending resumed on April 27 after Congress appropriated additional funds. Specifically, it is defined as:

$$\text{Share-Delayed}_{c,s} = \frac{L_{c,s}}{L_{c,s} + E_{c,s}}, \tag{2}$$

where $L_{c,s}$ denote funds received late (on April 27 and 28, 2020, just after funding resumed) and $E_{c,s}$ denote funds received early (on April 14 through 16, just before funding ran out). DK argue that this measure is as good as randomly assigned, conditional on the controls, in which case it can directly enter Equation (1) as the policy treatment variable. As defined, Share-Delayed$_{c,s}$ mainly reveals the impact of liquidity on employment, as represented by the timing of the PPP loan. That is, by how much a ten-day delay impeded employment recovery.

To identify the direct treatment effects of actual PPP *receipts*, we employ an instrumental variable (IV) strategy. We instrument PPP receipts (normalized by the average monthly 2019 payroll) with a variable that plausibly captures the PPP component uncorrelated with circumstances following the pandemic outbreak, such as the degree of the initial disruption. Our instrument is the 2019 small-business share of each county's payroll, effectively measuring the degree of purchasing power of an area that was restored by the program. Small businesses are defined as those with fewer than 500 employees, the size cutoff for 2020 PPP eligibility. Accordingly, the first stage can be expressed as:

$$\frac{\texttt{PPP}_{c,s,\tau}}{\texttt{Payroll}^{\texttt{all}}_{c,s,2019}} = \theta_t \frac{\texttt{Payroll}^{\leq 500}_{c,s,2019}}{\texttt{Payroll}^{\texttt{all}}_{c,s,2019}} + \mathbf{W_{c,s}}\phi_\mathbf{t} + u_{c,s,\tau}, \tag{3}$$

where $\texttt{PPP}_{c,s,\tau}$ denotes PPP loans received by county c in state s in program year $\tau = 2020,\ 2021$; $\texttt{Payroll}^{\texttt{all}}_{c,s,2019}$ and $\texttt{Payroll}^{\leq 500}_{c,s,2019}$ denote average monthly 2019 payroll across all and small establishments, respectively; and $\mathbf{W_{c,s}}$ encompass all the pre-pandemic county-level controls as in Equation (1).

The small business share of payroll should be correlated with the actual amount of loans received because the vast majority of borrowers obtained the maximum PPP funds for which they were eligible (equal to 2.5 months of their average monthly payroll for 2019, except for those whose receipts were capped by the $10 million total limit or the $100,000 limit on individual employee compensation). That is, $\text{PPP}_{c,s,\tau}$ can be decomposed into an extensive margin (i.e., which small businesses borrowed) and an intensive margin (i.e., how much each firm borrowed):

$$\begin{aligned}\text{PPP}_{c,s,\tau} \equiv \sum_{i\in(c,s)} \mathbf{1}_{\texttt{PPP}_i} \left(\lambda \cdot \texttt{Payroll}_{i,2019}\right) = \sum_{i\in(c,s)}^{\leq 500} \lambda \cdot \texttt{Payroll}_{i,2019} \\ + \left[\sum_{i\in(c,s)}^{>500} \mathbf{1}_{\texttt{PPP}_i} \left(\lambda \cdot \texttt{Payroll}_{i,2019}\right) - \sum_{i\in(c,s)}^{\leq 500} \mathbf{0}_{\texttt{PPP}_i} \left(\lambda \cdot \texttt{Payroll}_{i,2019}\right) + e_{c,s,\tau}\right], \quad (4)\end{aligned}$$

where indicators $\mathbf{1}$ and $\mathbf{0}$ denote whether small business i borrowed or not, while $\lambda \cdot \text{Payroll}_{i,2019}$ denotes the intensive margin with $\lambda \approx 2.5$. The term $\sum_{i\in(c,s)}^{\leq 500} \lambda \cdot \texttt{Payroll}_{i,2019}$ equals (the numerator of) our IV, while the terms in square brackets form the composite error term measuring the deviation of actual PPP take-up from pre-COVID small business payroll. Deviations via the extensive margin arise if some qualifying firms did not borrow $[\sum_{i\in(c,s)}^{\leq 500} \mathbf{0}_{\texttt{PPP}_i} \left(\lambda \cdot \texttt{Payroll}_{i,2019}\right)]$ or if some non-qualifying firms did $[\sum_{i\in(c,s)}^{>500} \mathbf{1}_{\texttt{PPP}_i} \left(\lambda \cdot \texttt{Payroll}_{i,2019}\right)]$. $e_{c,s,\tau}$ captures any deviation in the intensive margin from the maximum of 2.5-month payroll. The total deviation is likely small on average because the first two terms are offsetting (at least partially), while the third term should be small.

Since small-business payroll is predetermined at the county level, it is orthogonal to the initial pandemic-induced variation in local conditions. As a result, it should be uncorrelated with the unexpected component of actual PPP (i.e., the composite error in Equation [4]), especially conditional on all the controls ($\mathbf{W_{c,s}}$). Equivalently, the 2019 small-business payroll *share* should be uncorrelated with the error term $u_{c,s,\tau}$ in Equation (3). Our exclusion restriction requires that, conditional on controls, the 2019 small-business payroll share does not itself influence the trajectory of recovery, so that its effect on the outcome in Equation (1) is entirely through shifting the amount of PPP funds received. Thus, to account for possibly systematic differences in employment recoveries between counties with higher versus lower small-business payroll shares, we include a lagged dependent variable in the regressions, along with numerous fixed effects and controls whose effects are allowed to vary over time.

Our IV strategy is conceptually similar to Chodorow-Reich et al. (2012), who use pre-recession Medicaid spending to instrument the fiscal relief delivered through the Medicaid reimbursement mechanism. The underlying logic is analogous: fiscal transfers during the downturn were based on pre-recession spending levels, adjusted for the severity of local economic conditions. Consequently, predetermined spending provides a valid source of exogenous variation in actual fiscal receipts during the recession, conditional on prior trends in the outcome variable. Extending this logic, we construct a similar instrument for any subset of small businesses using their share of total payroll. This approach allows the IV to capture variation beyond the aggregate treatment for all small businesses and does not rely solely on the discontinuity in treatment status around the program's size threshold. In particular, for the smallest firms (those employing up to 20 workers), PPP receipts are instrumented using their payroll share. The validity of this instrument is examined in detail in Section 5.1.

2.2 Data

This section briefly documents the array of data, along with the construction of the variables used in our analysis. Summary statistics are reported in Table 1, and more details are provided in Appendix A.

Employment Counts We use the most timely high-frequency (monthly) employment data available at the county level from the QCEW. The QCEW employee counts cover more than 95 percent of US jobs, making it pertinent for evaluating the PPP, a chief goal of which was to preserve employer-employee matches. Our sample spans 2020–2021.

We consider specifications with the employment outcome expressed in levels, logarithmic levels, relative to the adult (16-year-old and older) population (denoted E/P), and changes in employment relative to the adult population (denoted $\Delta E/P$). Population is measured as of 2019 from the American Community Survey (ACS). To control for possible county-specific pre-trends, we include a lag of the dependent variable defined as of the end of 2019 of the corresponding employment measure.

To more accurately assess how the PPP impacted employment among the smallest establishments, which are generally deemed to need liquidity assistance the most, we use QWI data on employment by establishment size.[7] An additional advantage of the quarterly QWI is that its employment counts are based on longitudinal job histories linking an individual with an employer over time. Thus, QWI data enable a more precise evaluation of whether PPP funds helped maintain existing employment matches. To this end, we focus much of our analysis on QWI stable employment, which tallies the number of jobs held on both the first and the last day of each quarter with the same employer. Given its definition, stable employment is probably the most suitable measure to assess whether PPP funds supported the continuity of preexisting employment relationships, while other employment measures are more "contaminated" by churns in the labor market.

PPP Loans and Borrowers For PPP lending, we use official data released by the Small Business Administration (SBA).[8] We match each borrower's business name and full address to a unique Placekey identifier. These identifiers then map the borrowers

[7]Employment by establishment size in the QCEW is only available once annually.

[8]We use PPP data released as of August 2021 and available at https://www.sba.gov/funding-programs/loans/COVID-19-relief-options/paycheck-protection-program/ppp-data.

to their US Census county (or CBSA), allowing us to calculate loan statistics at the desired level of geography, including the share of loans delayed.[9] In the specifications where the policy variable is the volume of funds received, we use overall PPP funds disbursed in 2020 for all quarters of 2020, but the combined total of 2020 and 2021 funds for all 2021 quarters, all normalized by the 2019 average monthly payroll for total private employees as reported in the QWI data.

Preexisting Local Conditions Since existing relationships with banks were found to be important for loan underwriting in the first phase of the PPP (see Li and Strahan 2021, for example), we control for the following indicators of local banking market conditions as of 2019: bank branch density (number of bank branches normalized by population); community banks' and four largest banks' shares of deposits; and the volume of small business loans (SBL) in 2019 each county (from data reported pursuant to the Community Reinvestment Act), normalized by the number of small establishments (under 500 employees) from the QCEW data for 2019. We further control for the following pre-pandemic local demographic and economic conditions: median family income, commuter-to-resident population ratios, and the share of minors from the ACS; and urban or rural classifications from the 2013 National Center for Health Statistics scheme.

Pandemic-Related Factors Counts of COVID-19 cases and deaths are provided by Johns Hopkins University. The extent of county-level lockdowns is measured as the share of days in lockdown for early (before April 17, 2020) and late (April 17 through 30, 2020) periods using data from the Keystone-Strategy COVID-19 Intervention dataset. To account for differential state-level measures in response to COVID-19, we include the stringency index constructed by the Oxford Coronavirus Government Response Tracker.[10] To account for potential heterogeneous effects of the COVID-19 shock due

[9]Placekey is a free, universal standard identifier for any physical place. For more details, see https://docs.placekey.io/ and the corresponding white paper.

[10]The index construction is described in Roser (2021).

to pre-pandemic industrial composition, we control for the share of work that could potentially be done remotely (Dingel and Neiman 2020), the share of employees working in "most impacted" industries (with two-digit NAICS codes 44–45, 61, 71, 72, and 81), and the share of employees working in essential industries (as defined by the US Department of Homeland Security's Cybersecurity and Infrastructure Security Agency, DHS-CISA).

Table 1 provides summary statistics for the monthly employment data sample of QCEW, as of April 2020.

3 What Explains the Share of PPP Loans Delayed?

What makes the delay in PPP loans attractive as a policy treatment variable is that it provides a causal interpretation under the plausible assumption that the distribution of the delay in the localities was as good as random. That is, the share delayed should be conditionally uncorrelated with any unobserved factors that might have influenced the employment recovery. This assumption is satisfied if the interruption point of the queue of PPP applicants, which determines the share of loans delayed when the initial funding runs out, is effectively random. Thus, we first examine this assumption at the county level by modeling the first-order (linear) relationship between the share delayed and local conditions before the delay period began (on April 16, 2020) that could separately impact the subsequent economic recovery.[11]

The coefficients reported in Table 2 show that the delay in loan volume is closely related to several characteristics at the county level, especially the existing lending relationships of small businesses.[12] Counties with a higher 2019 SBL volume per small establishment experienced lower shares of delayed loans: A one-standard-deviation (SD) higher level of SBL per small establishment is associated with a 0.13 SD lower share of

[11] Appendix B further discusses changes in PPP rules over time, which could lead to different behavior of late versus early recipients, especially in hard-hit areas such as urban centers.

[12] In all subsequent analysis, the share delayed is based on the volume unless otherwise specified. Qualitatively similar results are found for the number of loans delayed, as reported in the Appendix Table A.1, and for a CBSA-level sample, as reported in the appendix Table A.2.

delayed loan volume.[13] Moreover, counties with a larger share of deposits at the four largest banks and at community banks were significantly more likely to experience a delay (a result driven by urban counties). We find that a one percentage point increase in deposits in the four largest banks (community banks) led to a 13.9 percentage point (7.5 percentage point) increase in funding delay in urban counties. This is likely due to the fact that the largest banks had significantly pulled out of small business lending since the Great Recession (see Chen, Hanson, and Stein 2017), and because smaller banks were likely less able to meet high demand due to capacity constraints.[14]

Differences in the determinants of the share delayed between urban and rural counties likely relate to how the virus transmission depended on the size and density of the population and on people's modes of interaction. On average, urban counties had a lower share of loans delayed than rural counties (44 vs. 50 percent). However, urban counties with higher COVID-19 case rates early on had a significantly higher share of delay. This correlation for urban counties supports the conjecture that areas hit hardest by COVID-19 suffered a greater disruption of commercial activity at the beginning of the pandemic, which also impeded PPP lending. Higher demand combined with impaired supply would result in worse delays in PPP lending. In fact, the most populous counties (defined as those in the top 1 percent by population), plausibly the counties most disproportionately impacted by COVID-19 initially, experienced significantly larger shares of funding delay, even after many covariates are taken into account. Moreover, preexisting county attributes explain a higher fraction of the cross-county variation in the share delayed for urban counties than for rural counties—about 7 percentage points higher in terms of the adjusted R-squared.

Given that the share delayed is correlated with several preexisting local attributes and public health conditions right after the COVID-19 outbreak, we include these co-

[13]Alternatively, counties at the 5th percentile of SBL per small establishment experienced a 0.39 SD higher delay in funding than counties at the 95th percentile.

[14]Before the pandemic, the share of small firms that borrowed from community banks was on average lower in urban counties than in rural counties. The lack of preexisting relationships likely hampered urban small firms' chances of obtaining a PPP loan early in the pandemic. As Balyuk, Prabhala, and Puri (2020) show, early in the program, small firms were better able to obtain funding from community banks with which they had a preexisting relationship.

variates in our employment regressions as controls.[15] The assumption supporting the inclusion of these covariates is that these public health conditions and preexisting banking and relationship lending conditions could in principle influence the recovery path of a local economy and their influence should not be attributed to PPP. Furthermore, we allow these covariates to influence employment *dynamically* throughout the sample months (that is, differently in each month), as we do with the share delayed, PPP receipts, and other controls. This flexible specification is likely prudent given the unique nature of the pandemic downturn.

4 Effects of PPP Timing on Employment

Our ultimate goal is to estimate the treatment effect of the PPP on local employment recovery. We begin by analyzing the effects of the timeliness of PPP loans, as represented by the share of loans delayed proposed by DK. To better understand how funding timeliness may influence a locality's economic recovery, our analysis examines how the estimated effects of this policy metric vary depending on the specification of the employment outcome (such as in levels, logs, relative to adult population, etc.), the geography of local labor markets (urban vs. rural counties, counties vs. CBSAs), data sources (QCEW and QWI), and across different establishment sizes.

To summarize, our analysis indicates that the share delayed has a negative impact on employment when measured in levels and on urban employment when measured either in levels or as a share of adult population, particularly in the smallest urban businesses (those with fewer than 20 employees). However, this effect is not robust beyond these specific segments of firms or employment measures.

Different Employment Specifications

First, we examine how alternative employment specifications affect the estimated impact of the PPP timing. Different outcome specifications imply different assumptions

[15]Including these attributes as controls also enables consistent inference after variable selection according to Chernozhukov, Hansen, and Spindler (2015).

about parallel trends if the analysis is interpreted in a difference-in-difference framework. For example, if parallel trends hold in log levels (growth), they generally do not hold in levels. Thus, treatment effect estimates are not invariant with outcome specification.

We begin with employment in levels. However, this specification disproportionately weights the largest counties, which are several orders of magnitude larger than the smallest counties.[16] To reduce this skew, we consider employment measures that are less scale-dependent and more commonly used in the policy evaluation literature. For example, studies of the impact of minimum wage changes use log employment (Jha, Neumark, and Rodriguez-Lopez 2024) or employment-to-population ratios (Cengiz et al. 2019), while studies analyzing fiscal stimulus effects often use the change in the employment-to-population ratio (Chodorow-Reich et al. 2012).[17] These alternative employment specifications are likely to provide a more accurate reflection of economic growth dynamics at our level of analysis, as they better capture proportional changes, account for population size, and reflect regional heterogeneity.

Figure 1 reports coefficient estimates from Equation (1) using all counties and alternative specifications of total private employment (levels, logs, E/P, and $\Delta E/P$). February 2020 serves as the reference period. The regressions include state and time fixed effects, a lag of the dependent variable to capture pre-trends,[18] and extensive controls for preexisting conditions and COVID-19 impacts. Standard errors are clustered at the state level.[19]

We account for a broad set of preexisting county characteristics. These include socioeconomic factors (median family income, share of minors, commuter-to-residential population ratio, and 2013 NCHS urban-rural classification) and financial access (bank

[16] In our data, employment in the top 10 largest counties exceeds that in the smallest county by more than four orders of magnitude. Moreover, as noted above, funding delays were disproportionately larger in the top 1 percent of counties by population.

[17] Hershbein and Stuart (2024) use changes in log employment.

[18] The lag of LHS is December 2019 for levels, logs, and E/P; for $\Delta E/P$, it is the change from December 2018 to December 2019, normalized by the 2019 adult population.

[19] Results are robust to replacing state and time fixed effects with county and state-by-month fixed effects, or clustering by county instead of state; see Appendix Table A.3.

branch density, deposit shares held by community banks and the four largest banks). We also incorporate indicators of relationship banking, proxied by the volume of small business lending in 2019 per small establishment. The latter measures are particularly relevant, as counties with greater financial diversity and stronger bank ties were likely to experience fewer delays, consistent with the results in Table 2.

We also account for variation in the impact of the pandemic. On the public health side, we control for cumulative COVID-19 deaths and cases per million. On the policy side, we include a stringency index of state containment measures and an indicator for early lockdown implementation (before April 16, 2020). On the labor-market side, we incorporate pre-pandemic industry and occupational composition: the share of essential workers, the potential to switch to remote work, and exposure to the most impacted industries.

These controls are important for several reasons. Counties with early or stricter lockdowns likely experienced sharper initial disruptions, raising PPP demand while slowing loan processing. Such counties also tended to have higher population density, higher rates of early infection and death, and higher inherent vulnerability to infectious diseases, all of which could have hindered recovery. Industry structure and occupational mix also shaped recovery dynamics. A higher share of jobs in directly affected industries or remote-work occupations may have either accelerated recovery (if large early job losses rebounded quickly) or slowed it (if demand for in-person services lagged). In contrast, counties with larger essential sectors likely saw more muted employment swings, since those businesses remained open or reopened sooner.

The results show that loan delays have a clear negative effect on employment recovery only when employment is measured in levels. For logs, E/P, and $\Delta E/P$, we find no negative effect—and in some cases a positive and significant one (e.g,, for $\Delta E/P$). As Appendix Table A.3 confirms, sequentially adding controls does not change this conclusion. The negative effect in the level regressions likely reflects model misspecification or unobserved heterogeneity, rather than a true widespread impact of PPP delays.

Urban versus Rural Counties

To further examine regional heterogeneity, we re-estimate the regressions separately for urban and rural counties (Figure 2). For urban counties (shown in the left panel), the coefficients are (mostly) negative in both the employment-level and E/P regressions,[20] and negative but never statistically significant in the log-employment specification. For E/P, the negative effect becomes statistically significant only with a long delay—November 2020 through March 2021—casting doubt on a causal interpretation. In contrast, for rural counties, where the share of PPP delayed was larger on average (50 vs. 44 percent in urban counties), the estimated coefficients are rarely negative and never statistically significant.[21]

Effects on Employment in the Smallest Establishments

To evaluate the impact of funding delays on the smallest establishments, we examine the impact of PPP funding delays on establishments with fewer than 20 employees. We use quarterly QWI data because the QCEW provides this level of detail only on an annual basis. An additional advantage of QWI is that its employment measures are based on longitudinal job histories, allowing individuals to be linked to the same employer over time. Among the various QWI employment measures, *stable employment* is the most relevant for tracking ongoing employment relationships. It counts jobs held continuously throughout an entire quarter, providing a clearer picture of sustained employment. For establishment size, QWI reports employment across five brackets: 0-19, 20-49, 50-249, 250-499, and 500+ employees. For simplicity, we focus on employment counts in the smallest bracket (under 20 employees), where DK identified notable effects.

Figure 3 illustrates that although the trends in QCEW and QWI employment data are broadly similar, as expected, the stable employment series of QWI is considerably

[20] These specifications align more closely with the approach of DK using individual-level CPS data, where the share delayed is matched to an individual's county and the outcome is a binary employment indicator.

[21] Because many workers commute across county lines, we repeat the analysis at the CBSA level to better assess local labor market dynamics (Appendix Table A.4). At this level, the share delayed exhibits a negative but insignificant effect in level regressions, while the other specifications yield positive and often significant effects.

smoother than the QCEW series. Interestingly, both datasets show that by the end of 2021, total employment had not yet returned to its 2019:Q4 level, remaining about 1 percentage point lower relative to population. However, stable employment in the smallest establishments rebounded more quickly, reaching its 2019:Q4 level as early as 2021:Q2.

Table 4 summarizes our findings on the impact of funding delay using the entire county sample in columns (1)–(4) and the urban subsample in columns (5)–(8). These regressions include the same comprehensive set of controls as in the QCEW results (Figure 1) and use 2020:Q1 as the reference period. When reporting results for the smallest establishments, the share delayed is defined specifically for establishments with fewer than 20 employees, excluding loans to the self-employed.[22] To construct this measure, we exclude borrowers reporting one or fewer jobs saved and receiving no more than \$20,833, the maximum loan amount available to self-employed individuals (including independent contractors and sole proprietors). The table shows that the negative impact of the share delayed, documented by DK, is apparent only in the urban subsample. Although the coefficients for share delayed are sometimes negative in the full sample, they are not statistically significant at conventional thresholds. Similar regressions using employment counts for all establishments regardless of size yield negative and sometimes statistically significant effects only in the levels specification (see appendix Table A.5). Although these total employment results are consistent with our previous QCEW findings, albeit less persistent, the size-specific QWI data reveal that the negative coefficients are driven by establishments with more than 500 employees, casting further doubt on a causal interpretation of the effect of the funding delay for total employment.

In sum, we find limited evidence that the share delayed influenced overall employment. However, when considering the QCEW and QWI results together, we cannot rule out an effect on employment when measured in levels or among the smallest urban

[22]Including loans to self-employed individuals does not change the results regarding the share delayed.

employers (fewer than 20 employees) more generally.

5 The Relative Importance of PPP Receipt and Timing

The impact of a delay in PPP funding may differ from the impact PPP receipt itself. We propose two methods to study the impact of PPP receipt and to distinguish it from the timing of receipt: (1) an instrumental variable technique using county-level QCEW and QWI data, and (2) a closest-neighbor matching strategy using firm-level activity data from Advan. We discuss each in turn.

5.1 County-Level Evidence

So far, we have found that, when using county-level data, the timing of PPP funding is not robustly correlated with local employment recoveries outside the smallest establishments in urban counties. However, this finding does not imply that PPP funding was not important for employment dynamics. We further explore the relationship between PPP funds and employment by augmenting our previous county-level regressions to include county-level PPP receipts normalized by the average monthly payroll of 2019.[23,24] Normalized PPP receipts effectively measure the degree of purchasing power restored by the program in a given area. PPP receipts relative to pre-pandemic payroll—whether by preserving preexisting employment relationships, supporting new employment, or through spillover effects across firms—were likely the most relevant metric for employment dynamics after the outbreak.

To identify the causal effect of PPP funds on employment, we propose an instrument that is conceptually similar to Chodorow-Reich et al. (2012), who use pre-recession Medicaid spending to instrument fiscal relief delivered through the Medicaid reimbursement mechanism. The logic behind their instrument is that predetermined spending

[23]The 2019 payroll is calculated by multiplying average monthly earnings by beginning-of-period employment for each quarter using QWI data, separately by establishment size (total, under 500, and under 20). The quarterly results are then averaged across all four quarters of 2019.

[24]We use payroll rather than population or employment to normalize receipts because it better reflects the purchasing power of the money allocated, given substantial differences in wages and the cost of living across the country.

provides a valid source of exogenous variation in actual fiscal receipts during the recession, conditional on prior trends in the outcome variable. In our case, the instrument is the county-level average share of monthly 2019 payroll in small establishments, as measured in the QWI. Given the exogenously imposed size restriction (establishments with fewer than 500 employees were eligible for funding), counties with higher shares of payroll in small establishments were better positioned to receive relatively more funds per dollar of payroll, potentially supporting stronger employment recoveries.[25] When studying employment in the smallest establishments (fewer than 20 employees), we calculate the PPP receipt specific to these establishments, excluding funds allocated to self-employed individuals.[26] In this case, the instrument is the share of payroll in the smallest establishments.[27] Also, since many rural counties do not have large establishments, we restrict our analysis to urban areas.

In our baseline regressions, we use PPP funds disbursed in 2020 for all quarters of 2020, but the combined total of 2020 and 2021 funds for all quarters of 2021. The rationale is that 2020 funds could have continued to influence employment outcomes into 2021, whereas 2021 funds were unlikely to have been anticipated in 2020. As robustness checks, we also estimate the regressions using either cumulative PPP disbursements by quarter (funds allocated up to each quarter) or the final total amount (summing 2020 and 2021) for all periods. We discuss how these alternative specifications affect the results later in the paper.

Instrument Validity

Figure 4 illustrates the relationship between PPP receipt relative to payroll and the share of small-establishment payroll at the end of 2020 and 2021 (left panel), and

[25]We use average monthly 2019 payroll because each eligible small business was entitled to a 2020 PPP loan not to exceed ten weeks of average 2019 payroll, or $10 million. All evidence indicates that PPP applicants generally requested the maximum eligible amount. In our data, the average county received 2.2 (3.06) months of monthly small-establishment payroll in 2020 (by the end of 2021), see Table 3.

[26]For consistency, we exclude PPP funds allocated to self-employed individuals because the self-employed are not covered by the QCEW or QWI employment data.

[27]Regressions that use the share of under 500 payroll produce similar results, however, the strength of this instrument is, not surprisingly, significantly weaker.

between PPP receipt for the smallest establishments and their corresponding payroll share (right panel). The correlation is strongest for the smallest firms—0.91 versus 0.68. The positive association between these measures is confirmed by the results in Table 5, which include the complete set of covariates. Together, these results demonstrate a strong first stage: the under-500 (and under-20) payroll share is highly predictive of the amount of PPP funds received per dollar of payroll in urban counties. Furthermore, Figure 5 illustrates that the correlation between PPP receipt relative to payroll (total versus smallest establishments) and the share delayed is nonzero, albeit modest: 0.08 for total and 0.13 for the smallest establishments. Consequently, our regressions incorporate both measures.

The validity of our instrument rests on the exclusion restriction: the share of payroll in small establishments must not directly affect employment, except through its correlation with PPP receipt. This condition is potentially problematic, since employment dynamics may have been influenced by the establishment size composition during the pandemic, and thus by the small-establishment share of payroll. For example, establishments in the hardest-hit industries tended to be relatively small, so counties with a larger concentration of such establishments may have recovered more slowly, independent of PPP funding. To mitigate this concern, we take several steps.

First, we specify employment outcomes as changes, ensuring that our results are not driven by time-invariant differences between counties with higher versus lower small-establishment payroll shares. Following Chodorow-Reich et al. (2012), we use the change in employment-to-population specification ($\Delta E/P$).[28] Second, we include state and time fixed effects in our regressions, along with the extensive set of controls discussed earlier, which are allowed to have dynamic effects by interacting them with time dummies. Moreover, when focusing on the evolution of employment in the smallest (under 20) establishments in urban counties rather than in all businesses, we further mitigate concerns, as the industrial composition within the smallest-establishment category is likely more uniform.

[28]Our results for logs and E/P are qualitatively similar.

Even with an extensive set of controls, a nonzero possibility still remains that an omitted variable is correlated with both our instrument and the dependent variable. To investigate this, we examine how our instrument relates to changes in the total employment-to-population ratio prior to the 2020 pandemic. Figure 6 presents these results for the 2003:Q1–2021:Q4 period. The figure shows two sets of estimates: one using a basic set of controls (fixed effects only) and another using the full set of controls discussed earlier.[29] The figure indicates that the share of under-500 payroll did not have a significant effect on $\Delta E/P$ before COVID-19. In fact, during the previous 20 years, the conditional correlation between the instrument and the dependent variable was consistently weak, occasionally negative, and several orders of magnitude lower than its relationship during and after the COVID-19 recession. The conditional correlation of the share of funding delayed and employment was also mostly insignificant throughout the period.

Figure 7 presents a similar analysis for employment in establishments with fewer than 20 employees. In this case, the conditional correlation between the instrument and the dependent variable was distinctly negative during the Great Recession, which was not observed during the COVID-19 recession. Employment recovery in counties with larger shares of under-20 payroll was slower than in other counties. In contrast, the conditional correlation of the change in employment with the share delayed was *positive* during the Great Recession. It is possible that places with higher shares of community banks (i.e., lower shares of large banks) and less reliance on small business loans, both of which correspond to a higher share delayed on average (see Table 2), fared better during the Great Recession because large banks slashed lending, especially to small businesses, to repair damages inflicted by the subprime crisis on their balance sheets. In sum, the employment dynamics for the smallest establishments were clearly different during the COVID-19 pandemic, possibly due to the support of PPP funding. These long-run patterns support the validity of our instrumental-variable approach and provide evidence that the exclusion restriction is likely satisfied.

[29]Results for the QCEW sample of private employment show similar trends; see Appendix Figure A.2.

Estimates of Jobs Retained

Table 6 presents our IV estimates, showing results for total QCEW employment, stable QWI employment across all establishments, and for those with fewer than 20 employees.[30] In our baseline specification, changes in the employment-to-population ratio are measured relative to 2019:Q4.[31] As shown in the table, the OLS estimates of the impact of PPP receipt are positive and significant across all employment measures. The instrument is strong, with Kleibergen-Paap Wald F-statistics ranging from 48.8 to 96.3 and Anderson-Rubin Wald χ^2 tests of weak instruments producing p-values of 0.00 across all specifications. IV estimates are generally larger than OLS estimates for overall employment measures, but slightly smaller for the smallest establishments. One likely reason for the negative OLS bias in overall employment is that larger firms (within the eligible range) account for a disproportionate share of both PPP outlays and total employment changes. However, these firms likely benefited less from the PPP—owing to stronger banking relationships and greater liquidity buffers—so their heavy weight in the data causes OLS to understate the true effect of PPP. Among the smallest establishments, this negative bias is attenuated because treatment effects are more homogeneous within size groups. Moreover, any remaining negative bias may be offset by a positive selection bias: smaller firms that managed to apply for PPP—despite facing greater access hurdles—were likely those most in need and thus most responsive to support. These offsetting forces help explain why the OLS and IV estimates are more similar for employment among the smallest establishments.

Our IV estimates in column (2), based on QCEW private employment, imply that PPP funds saved approximately 5.85 million job-years in the urban counties in our sample over the study period.[32] Roughly 53 percent of these job-years materialized

[30] Our instrument identifies the local average treatment effect on compliers, under the standard assumption that there are no defiers. Appendix C provides a discussion of why this assumption is most likely satisfied in our context. Also note that our IV specification directly estimates the treatment effect of receiving funds, not the intent-to-treat effect, and therefore does not require the Wald adjustment, as in Autor et al. (2022a).

[31] Results are similar when changes are measured against 2019 annual averages or when each quarter is compared with its corresponding quarter in 2019. These results are not reported for brevity.

[32] Job-year figures are computed by rearranging Equation (1) to derive $\Delta E = \beta \left(\frac{\texttt{PPP}}{\texttt{payroll}}\right) \frac{\texttt{pop}}{100}$, substi-

in 2020 and the remainder in 2021, corresponding to 2.9 percent and 2.5 percent of 2019 employment in these counties, respectively. Notably, our estimates capture both direct and indirect spillover effects of the PPP. However, since the estimates are based on an urban subsample of counties, extrapolating to the full US is not straightforward. Nevertheless, the magnitudes reported are broadly consistent with Autor et al. (2022b), who estimate that the program boosted employment by up to 3 percent by the end of 2020, and fall near the midpoint of the estimates reported in previous studies.[33]

We also estimate the number of stable jobs saved. Using the coefficients in column (4), we find that PPP receipt preserved 4.28 million job-years, about 41 percent of which occurred in 2020 and the remainder in 2021, equivalent to 1.8 percent and 2.6 percent of 2019 stable employment, respectively. As far as we are aware, our estimates of stable jobs saved represent a novel contribution, as most existing studies measure job preservation using employment flows—similar to our QCEW results—without accounting for job stability or worker attachment.

Cost of Jobs Retained

The cost per job-year retained implied by our baseline IV estimates of the PPP's effect on employment is reported in Table 7. Specifically, we calculate the cost of retaining a job-year with PPP funds as follows. We first calculate the total number of job-years that would have been saved during the two-year period if the total PPP funding available by May 2021 (the program's end) had been in place throughout the whole period. This involves scaling the previously calculated job-years saved in 2020 by the ratio of total PPP outlay to 2020 PPP outlay (with no adjustment is necessary for 2021 job-years). We then divide this estimate of job-years by the total program outlay.[34] Accordingly, estimates based on total QCEW employment imply that saving

tuting total PPP outlays for the corresponding year for `PPP`, the all-firm average monthly 2019 payroll for `payroll`, and total 2019 population for `pop`. The division by 100 is necessary because $\Delta E/P$ enters the regressions in percent. The resulting quarterly estimates are summed across the sample period and divided by four to obtain job-years saved.

[33]See Table 5 in Dalton (2023) for a comprehensive summary of the estimated employment effects of PPP and the methodological differences across studies.

[34]The scaling factor is implied by the linearity of Equation (1): $\Delta E = \beta \left(\frac{\texttt{PPP}}{\texttt{payroll}}\right) \frac{\texttt{pop}}{100}$, thus job-years

a job-year costs $88,997. Consistent with our expectations, the cost of saving a stable job for a year is considerably higher at $127,179. The cost of maintaining a stable job in one of the smallest establishments is substantially greater, at $546,777 per year, likely reflecting the characteristically high turnover rates of these establishments.

Table 7 also presents alternative estimates of the costs of jobs retained under two different assumptions regarding the timing of funding expectations: 1) the full amount of PPP funds was anticipated in 2020 ("Total PPP" panel), and 2) only the cumulative funds received up to a given quarter ("Cumulative PPP" panel) affected employment in that quarter.[35] Not surprisingly, costs are lowest in the cumulative PPP scenario and highest in the total PPP scenario.[36] For example, for total QCEW employment, the cost of saving a job-year is $108,201 under the Total PPP assumption compared to $87,503 under the Cumulative PPP assumption.

Our cost estimates, between $87,503 and $108,201 depending on our assumptions regarding the timing of funding expectations, lie toward the lower end of the range reported in previous studies. Event-study analyses yield estimates similar to ours: for example, Dalton (2023) reports a cost of $141,000 per job-year saved, and Splinter et al. (2025) report costs in the range of $133,000–$146,000. Other studies using different methodologies find varying results: Doniger and Kay (2023) estimate approximately $100,000 per job-year, Autor et al. (2022c) and Autor et al. (2022b) report $169,000–$258,000, and Granja et al. (2022) reports about $175,000 per job-year saved. The range of estimated costs of saving one job-year through the PPP in fact compares favorably with the cost of nearly $603,000 per job-year in terms of state and local government employment saved by federal fiscal assistance, as estimated by Clemens, Hoxie, and Veuger (2025). The authors provide an incisive discussion of the likely reasons for the

retained would change proportionally to funding (normalized by 2019 payroll).

[35]Note that the job-years retained corresponding to these two assumptions are computed using the coefficients estimated from separate regressions, where the policy regressor is measured in accordance with the respective assumption. These coefficients are not shown for brevity.

[36]The logic is the same as that underlying our baseline estimate of total job-years retained—by scaling the jobs saved in 2020 by the ratio of eventual total funding to funding through a specific quarter. Hence, the lower the funding assumed for a quarter in 2020, the higher the ratio, and in turn the more job-years retained, which translates into a lower cost.

high price tag. In contrast, Chodorow-Reich (2019) estimate a cost of just over $50,000 per job-year saved through American Recovery and Reinvestment Act of 2009 (ARRA). The ARRA was an $800 billion policy package, with roughly $600 billion disbursed in the year of analysis—placing it on a scale comparable to the PPP in terms of fiscal magnitude. The substantially greater cost-effectiveness of ARRA likely reflects the distinct financial nature and depth of the Great Recession relative to the COVID-19 downturn.

Effects of PPP Receipt versus Timing on Small Establishments

We now compare the impact and cost of PPP receipts with that of PPP delay. Consistent with previous regressions that excluded PPP receipt, the estimated effects of the share delayed are generally insignificant, except for employment in the smallest establishments in urban counties and when using the size-specific share delayed (columns (5) and (6) in Table 6). This pattern is intuitive: The smallest establishments probably needed the most immediate liquidity assistance. Because both PPP receipts and share delayed are significant for these smallest establishments, we can compare the magnitude of their estimated effects, contrasting job-years saved from earlier funding (i.e., a lower share delayed) with job-years saved from additional funding. Using the coefficients from column (6) in Table 6, we estimate that the PPP funds received (conditional on the share delayed) saved approximately 332,290 stable job-years in those smallest urban establishments during the sample period. This corresponds to roughly 0.61 percent of 2019 employment in those establishments in 2020 and 1.36 percent in 2021, respectively. In comparison, conditional on the funds received, if the share delayed had been reduced from 48 percent (the sample aggregate for the smallest establishments) to zero, 379,136 job-years would have been saved.[37] Combining these two effects implies that PPP would have preserved approximately 711,426 job years had the delay not occurred, equivalent to about 4.2 percent of their average 2019 employment over the two combined years.[38]

[37] Relevant sample statistics can be found in Table 3.

[38] A note of caution around these estimates is warranted. QWI does not track establishments longitudinally for size, so when a firm crosses a threshold, all of its employment shifts to the new size bin.

Overall, our results suggest that the funding effect was more important than the short-term liquidity effect on aggregate employment at the county level. However, the timing of receipt was critical for the smallest establishments in urban areas, some of which may have exited altogether due to delayed assistance as documented by Kurmann, Lalé, and Ta (2025). Importantly, our findings complement their work by showing that PPP operated through two distinct channels—funding volume and timing—and that, *after* accounting for the timing, PPP support still helped the smallest businesses preserve an additional 332,290 stable job-years. The firm-level evidence discussed in the next section reinforces this conclusion.

5.2 Firm-Level Evidence

To study the effects of PPP receipt versus timing at the firm level, we apply a matching estimator using Advan foot traffic data. We compare visits to each PPP borrower with a closely matched non-recipient control firm (hereafter, peer) matched on industry, economic activity (measured by visits), and geolocation. Importantly, peers are firms that were active before the pandemic (as of December 2019 or earlier) and did not receive PPP loans. Our matching criteria, detailed below, are designed to maximize the likelihood of satisfying the conditions needed for causal inference based on difference-in-differences (DiD) estimates. First, they are sufficiently stringent to satisfy the assumption of parallel-trends: had the treated firms not received PPP, they would have evolved similarly to their peers because both groups faced comparable conditions in terms of demand, COVID-19 severity, and non-pharmaceutical interventions. Second, the criteria minimize the likelihood of interference between a PPP firm and its peer, helping satisfy the stable unit treatment value assumption (SUTVA).

Advan data measures foot traffic by tracking movements to and from points of interest (POI, mostly business establishments) using GPS location data from mobile

Thus, estimated PPP effects for establishments under 20 employees reflect both within-size employment changes and firms moving into or out of the bin, rather than effects on a fixed group of pre-PPP small firms. Early in the pandemic, estimates may be inflated by slightly larger firms shrinking below 20 employees, while later they may miss PPP-driven employment growth as some originally small firms may expand beyond the 20 threshold.

devices. Each POI is uniquely identified by a Placekey, which is based on its precise geographic location (address, latitude, and longitude) and its business type, determined by a detailed 6-digit NAICS code. This combination enables precise differentiation of stores, even within large structures like strip malls. Although Advan does not cover all POI in the US, its dataset provides extensive coverage, particularly in urban areas and across the retail and service sectors, making it a valuable resource for analyzing economic activity.[39] We use visits to each POI as a proxy for business performance, as data indicate that the number of visits is strongly correlated with employment and revenue (see Gorbachev, Luengo-Prado, and Wang 2023).

Using the unique Placekey assigned to each POI, we identify PPP recipients within the Advan data. To be consistent with our previous county-level analysis, our Advan sample excludes self-employed PPP recipients, defined as those with loan sizes up to $20,833 and reporting only one job. For simplicity, it also excludes PPP firms that received more than one loan, which constitute only a very small subset of the data. To estimate the effects of receiving a PPP loan on visits, we identify peer firms within the same 6-digit NAICS industry and with pre-pandemic activity within 5 percent of the PPP firm (as measured by the absolute percentage difference in the average number of visits from December 2019 to February 2020).[40] In terms of location matching, we balance the need for geographic proximity (so that peer firms faced similar local economic conditions and COVID-related policies) and the need to avoid possible direct competition between a PPP firm and its peer. For our baseline estimates, we restrict the peer firm to operate within the same county but *outside* the Census block group (CBG) of the PPP firm, and at least 5 but no more than 50 miles away.[41] This

[39]For details on data quality and industry-specific coverage rates, see Advan's documentation on places and metrics. Kurmann, Lalé, and Ta (2025) also provide a helpful summary.

[40]6-digit NAICS classifications represent highly specific categories of business activity, ensuring that the control firms we identify are closely aligned in terms of demand for their products. When multiple peer businesses satisfy the matching criteria for a given PPP recipient, we select one randomly.

[41]CBGs tend to correspond to city blocks bounded by streets in urban areas, which means that retailers and restaurants in the same 6-digit NAICS operating in such proximity may compete for the same set of customers. The supply shortfall of a peer firm could mechanically increase the demand for the PPP firm, potentially violating SUTVA. We thank an anonymous referee for highlighting this possibility.

"donut" exclusion zone substantially increases the likelihood of satisfying SUTVA. As an additional robustness check, we estimate separate treatment effects for subsets of PPP firms grouped by geographic distance from their peers, varying the distance to the PPP firm from 1-5 miles to 15-50 miles (see Figure A.3 in the appendix).

Selecting a random peer firm subject to the matching criteria detailed above, we obtain a sample of 65,136 pairs. Our data span December 2018 through February 2022. Table 8 presents summary statistics showing that our sample consists mainly of small firms: more than 75 percent of matched borrowers reported fewer than 20 jobs in their PPP applications (with mean and median counts of 19 and 8, respectively).[42] Correspondingly, mean and median PPP loan amounts in our sample are relatively small at about $139,000 and $49,000, respectively. Furthermore, since Advan data are based on visits to physical locations, the vast majority of firms in our sample (about 90 percent) are in urban counties and are concentrated in the service and retail sectors.

To study the effects of PPP, we first examine the impact of receipt by comparing the evolution in the number of total visits to PPP establishments with the evolution in visits to comparable non-PPP establishments over time. We then examine how much that gap varies depending on the timing of the loan.[43] Although the Advan data are available at a daily frequency, we aggregate visits to the monthly frequency for the analysis to avoid high-frequency noise. Importantly, if a firm is not observed at all in a given month, its value is "filled in" as zero visits to ensure that the firm remains in the dataset and the panel stays balanced. The dependent variable (LHS) in our regressions is either Log Visits (defined as the log of visit counts plus one) or Zero Visits (an indicator equal to one when visits are recorded as zero in the Advan data, or when we fill in the data). Zero Visits serve as our proxy for business closure, which may be temporary or permanent.[44] With our matched-pair sample, we first trace out

[42]Summary statistics for matched pairs by varying geographic distances show similar patterns and can be found in Appendix Table A.6.

[43]The Advan data also provide information on the number of distinct visitors, and results based on visitors are very similar (omitted for brevity).

[44]Some service businesses could have operated fully remotely during the peak weeks of the pandemic and thus registered zero visits. As a result, this measure may overstate closures for some non-retail

PPP's effects on visits in calendar time by estimating essentially two-way fixed-effects regressions of the form:

$$Y_{ijt} = \alpha_i + \delta_{jt} + \beta_t \text{ PPP}_i + \epsilon_{ijt}, \tag{5}$$

where Y_{ijt} is the outcome variable (either Log Visits or Zero Visits) for a POI i of pair j in month t, and PPP_i is an indicator variable equal to one if POI i received PPP funding and zero otherwise. α_i denotes POI fixed effects, and δ_{jt} denotes pair-time fixed effects. Given the lack of detailed pre-pandemic firm characteristics in Advan, we rely on these firm- and pair-by-time fixed effects to control for unobserved heterogeneity.

We then apply the Sun and Abraham (2021) estimator, which accounts for the staggered timing of PPP receipts and maps out the effects in event time:

$$Y_{ijt} = \alpha_i + \delta_{jt} + \sum_{l=-T+1}^{-2} \mu_l D_{ijt}^l + \sum_{l=0}^{T} \mu_l D_{ijt}^l + \epsilon_{ijt}, \tag{6}$$

where $D_{ijt}^l = \mathbb{I}\{t - K_i = l\}$ is the event-time indicator, with K_i indexing the month when firm i received its PPP loan. Standard errors are clustered at the pair ID level in both regressions.

The left panels of Figure 8 plot estimated β_t coefficients (PPP effects over calendar time) from regression (5), while the right panels plot estimated μ_l coefficients (PPP effects relative to funding approval months) from regression (6). These estimates clearly indicate that businesses that received PPP funding saw significantly better recovery in visits (and fewer closures, bottom panel) than their non-PPP peers, starting in May-June 2020. The relative difference in visits between PPP and non-PPP firms grew over time, reaching close to 10 percent by December 2020 and nearly 20 percent by February 2022 (the end of our sample).[45] PPP funding also reduced the probability of closure by about 2 percentage points by December 2020 and close to 4 percentage points by the

sectors early on in the pandemic.

[45] Cole (2022) uses a matched-pair estimator on administrative payroll data and finds that, among very small firms, PPP recipients increased employment five months after receipt by 7.5 percent relative to comparable small firms that did not receive funding.

end of the sample.[46] Importantly, these patterns are robust to the distance between firm pairs. Appendix Figure A.3 shows consistent results for distance bands from 1-5 miles to 15-50 miles.

To assess the relative importance of short-term liquidity versus funding effects of PPP, we compare PPP's effects on firms with PPP loans approved either early (April 14-16) or late (April 27-28), as defined by DK. We identify about 8,575 early PPP recipients and 8,870 late PPP recipients, respectively. The coefficient estimates for this subsample, depicted in Figure 9, show a moderate difference in foot-traffic recovery between early and late PPP loan recipients compared to their respective peers between mid-2020 and mid-2021, but the difference is not statistically significant and vanishes by the end of 2021.[47] This pattern suggests that the receipt of funds was more important for economic recovery than the timing of the loan.[48] Moreover, we find little difference in the odds of Zero Visit between early and late recipients.

We must acknowledge that even our stringent matching along with fixed effects may not fully eliminate the bias resulting from endogenous selection into the PPP. The ultimately untestable parallel trend assumption—that non-borrowers and borrowers would share the same post-COVID trend in the absence of PPP—can be challenged in the wake of an unprecedented shock such as COVID-19. Heterogeneity in attributes that were irrelevant before the shock can result in divergent post-shock trends between the treated and control groups. Specifically, certain business characteristics might both determine their PPP choice and influence their post-pandemic trend.[49] However, potential selection biases are two-sided. If some eligible firms did not borrow because of managerial shortcomings or owners' retirement decisions, our funding-effect estimate could be upward biased. In contrast, some eligible firms did not borrow because they

[46]This likely accounts for about 4 (out of 20) percent of the difference in visits by the end of the sample because firms that suffered closures tended to be smaller than average.

[47]We find a similar and still insignificant difference using an alternative, wider, event window from April 10 to May 2 (that is, using firms that received loans five-days before or five-days after the 10-day delay). The exact estimates are omitted for brevity but are available upon request.

[48]Splinter et al. (2025) find that the 10-day delay's effect was about one-fifth of the total estimated PPP effect on employment changes.

[49]We thank an anonymous referee for highlighting this potential bias.

did not need the funds and obeyed the program rule that required owners to attest that PPP funds were necessary to support operations.[50] These firms might have done well regardless, and including them in our control group might bias estimates of the PPP's effect downward. Our estimates could also be downward biased to the extent that some firms we classify as non-recipients may have actually borrowed but could not be matched in our data. In Appendix D, we discuss additional reasons why some eligible firms may not have taken PPP funds and argue that any resulting biases are likely small on net. Nevertheless, further research on non-participation would be valuable to inform future policy design.

One additional caveat regarding the timing effect of funds is that firms that needed liquidity early on but were unable to obtain a loan in time might have either been too discouraged to reapply or gone out of business altogether. If some of these discouraged or failed firms are matched to PPP recipients and serve as non-recipient controls in our difference-in-differences analysis, we would underestimate the effect of funding delay because our estimate would attribute the entire effect to the receipt of funds, instead of partly to the delay.[51] However, such an underestimate would be relevant for both early and late borrowers, so the differential effect we estimate is not necessarily biased. Equally important, this issue is not unique to our analysis and is also present in other studies that examine the impact of the PPP using the share delayed. Overall, our findings indicate that the receipt of PPP funding played a more significant role in retaining and expanding employment than the timing of the loan.

6 Summary and Concluding Remarks

This study examined how the PPP affected employment recovery after the COVID-19 outbreak, distinguishing between short-term liquidity and funding effects. Using county-level small-firm payroll share as an instrument for PPP receipt, we show that

[50] On the loan application form, "the Applicant must certify in good faith to all of the below ..." which includes "current economic uncertainty makes this loan request necessary to support the ongoing operations of the Applicant."

[51] We thank anonymous referees for pointing out this potential bias.

receiving funds was generally more important than timing for most firms. The adverse effects of funding delays were concentrated among the smallest businesses, those with fewer than 20 employees, which were particularly vulnerable to cash flow disruptions. For the broader population of firms, funding delays played a relatively minor role, while ensuring sufficient funding was the primary driver of overall recovery. PPP receipt saved approximately 5.85 million job-years in urban counties over the two-year period, at an approximate cost of $89,000 per job-year, and 4.28 million stable job-years at $127,000 per stable job-year. Since funding delay was particularly detrimental to the smallest establishments, eliminating delays entirely for these firms would have added 379,136 stable job-years, while broader funding effects (conditional on delay) accounted for an additional 332,290 stable job-years.

Our findings highlight that effective emergency interventions require both timely disbursement for the smallest firms and sufficient funding for broader recovery. In the case of the PPP, because urban businesses experienced more persistent dislocation, directing aid to these regions and enabling earlier access for the smallest firms through specialized lenders would have further increased program efficacy.[52] More broadly, it is not surprising that grants, a form of government transfers, are more effective than loans at sustaining employment, especially for firms facing large cost increases or significant revenue losses during an emergency. Designating a program as grants from the outset, thereby removing uncertainty about repayment, could further improve program effectiveness. With greater certainty, firms might be more willing to invest and hire.[53]

Congress also authorized several other programs via the CARES Act, including expanded UI benefits, direct cash transfers, fiscal aid to state and local governments, and credit support programs. There is substantial work analyzing these programs individu-

[52]Our results confirm and extend the recommendations put forth by existing studies such as Granja et al. (2022) and Autor et al. (2022c).

[53]Potential concerns about moral hazard associated with grant programs during a pandemic are likely minimized due to the exogenous nature of the shock if the grant is intended to allow businesses to remain closed to curb the spread of a virus. Furthermore, concerns about adverse selection could be mitigated through measures such as those incorporated into the PPP: eligibility restrictions, funding caps, and program rules linking support to employment or verified business expenses.

ally.[54] Ideally, these programs should be evaluated jointly to assess their combined and interactive effects on employment and firm outcomes during post-pandemic recovery. Among the few studies considering interactions, Splinter et al. (2025) find that PPP costs were partially offset by positive fiscal spillovers—specifically reduced UI claims and higher income tax revenues from continued employment. Similarly, Sledz (2025) develops a structural model of optimal stimulus allocation between firms and workers, concluding that given the scarring effects of job loss, joint transfers to both are more effective.

Taken together, our results indicate that the impact of the PPP on employment recovery operated primarily through funding rather than liquidity channels for most firms, with liquidity effects concentrated among the smallest establishments. Understanding these distinctions is essential for interpreting the aggregate effects of the program and situating the PPP within the larger set of pandemic-era interventions, whose interactions remain an important subject for future research.

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Table 1: Summary Statistics: County-Level QCEW Sample

	All		Urban		Rural	
	Mean	SD	Mean	SD	Mean	SD
PPP Receipt						
No. of Early PPP Loans (4/14–4/16/2020)	197.01	570.29	445.10	877.11	49.41	53.29
No. of Late PPP Loans (4/27–4/28/2020)	226.94	622.71	492.92	959.08	68.70	65.97
Volume of Early PPP Loans, in Millions (4/14-16/2020)	29.11	111.75	71.15	174.99	4.10	5.65
Volume of Late PPP Loans, in Millions (4/27-28/2020)	21.90	94.51	52.92	149.64	3.44	4.16
Total PPP Volume (in Millions) for 2020	167.91	661.24	406.14	1,039.69	26.25	31.11
Total PPP Volume (in Millions) for 2020+2021	254.50	1,034.35	613.34	1,631.46	41.12	45.42
Total PPP Volume for (< 20) (in Millions) for 2020	50.58	192.73	120.26	302.87	9.15	10.54
Total PPP Volume for (< 20) (in Millions) for 2020+2021	78.85	311.25	187.79	490.47	14.07	16.05
Avg No of Jobs Claimed (in Thousands)	12.01	4.41	13.92	3.90	10.87	4.30
Share of PPP Loans Delayed (By Count)	0.58	0.12	0.55	0.10	0.59	0.13
Share of PPP Loans Delayed	0.48	0.18	0.44	0.14	0.50	0.20
Share of PPP Loans Delayed to(< 20)	0.51	0.15	0.49	0.11	0.52	0.16
COVID-19 Impacts						
Cum Covid-19 cases per million	151.70	327.54	213.17	402.48	115.14	266.89
Cum Covid-19 deaths per million	66.41	185.02	104.53	237.53	43.73	140.30
COVID-10 Stringency Index (Oxford University)	69.02	8.86	69.82	8.07	68.55	9.27
Share of days in lockdown (pre-04/17/2020)	0.50	0.12	0.50	0.11	0.49	0.12
Share of days in lockdown (04/17 and 04/30/2020)	0.99	0.05	0.99	0.03	0.99	0.06
Share of Work Done Remotely	0.35	0.04	0.35	0.06	0.35	0.03
Share of Emp. in Essential Industries	0.88	0.02	0.87	0.02	0.88	0.02
Share of Emp. in Impacted Industries	0.32	0.08	0.32	0.07	0.31	0.09
Share of Wages in Impacted Industries	0.19	0.07	0.19	0.06	0.19	0.08
Preexisting Conditions						
Rural County Dummy	0.63	0.48				
Total Residential Population (in Thousands)	104.47	333.04	240.41	516.99	23.63	22.16
Share of Minors	0.22	0.03	0.22	0.03	0.22	0.04
Commuter to Residential Population Ratio	1.15	0.17	1.13	0.12	1.17	0.19
Median Family Income (in Thousands)	66.55	16.35	75.60	18.39	61.17	12.15
Community Bank Share of Branches	0.59	0.32	0.43	0.29	0.68	0.30
Community Bank Share of Deposits	0.57	0.34	0.39	0.31	0.67	0.32
Number of Branches	27.46	69.53	57.95	106.73	9.32	7.51
Bank Branch Density (Population per Branch), in Thousands	3.18	1.92	4.08	1.99	2.64	1.65
Big4 Bank Share of Deposits	0.08	0.15	0.15	0.18	0.04	0.11
Number of Small Business Loans (in Thousands)	2.32	10.23	5.64	16.21	0.34	0.42
Volume of Small Business Loans (in Millions)	80.31	311.49	194.43	488.97	12.45	16.44
Avg Small Business Loan (in Thousands)	35.04	13.95	36.79	11.65	34.00	15.07
SBL Volume per Estab< 500 (in Thousands), CBP 2019Q1	24.03	11.74	29.12	9.96	21.01	11.68
Private Establishments (< 500) (in Thousands), CBP 2019Q1	2.54	8.94	5.92	13.99	0.54	0.56
Private Employment (in Thousands), QCEW 2019 Average	39.54	146.16	94.68	228.83	6.74	7.60
Private Establishments (in Thousands), QCEW 2019 Average	2.97	12.50	6.97	19.83	0.59	0.63
Share of Estabs (< 500), CBP 2019Q1	0.99	0.02	1.00	0.01	0.99	0.02
Share of Employment in Estabs (under 500), QWI 2019	0.62	0.16	0.55	0.13	0.66	0.16
Observations	3,100		1,156		1,944	

Notes: The values for **PPP receipt** and **COVID-19 Impacts** pertain to April 2020 unless otherwise specified. Values for **Preexisting Conditions** are based on 2019 data. Measures related to PPP loans to establishments with fewer than 20 employees (< 20) exclude loans to the self-employed (i.e, we exclude borrowers who reported one or fewer jobs saved and received no more than \$20,833, the maximum allowed for self-employed individuals). Data sources described in Section 2.2.

Table 2: Determinants of Share of PPP Loan Volume Delayed, April 16–26, 2020

	All	Urban	Smaller	Rural
Cum Covid-19 cases per billion up to Apr 15	0.011	0.063**	0.069**	–0.014
	(0.020)	(0.027)	(0.027)	(0.035)
Cum Covid-19 deaths per billion up to Apr 15	0.065	0.016	–0.004	0.099
	(0.049)	(0.064)	(0.064)	(0.094)
Share of days in lockdown (pre-04/17/2020)	0.028	–0.016	–0.018	–0.053
	(0.054)	(0.064)	(0.067)	(0.077)
Share of days in lockdown (04/17 and 04/30/2020)	0.147	0.025	0.022	0.329***
	(0.127)	(0.177)	(0.176)	(0.070)
Share of essential empl by county	–0.196	–0.280	–0.304	–0.014
	(0.218)	(0.386)	(0.386)	(0.255)
Share of Employment - Impacted Industries	0.003	–0.149	–0.152	0.147*
	(0.055)	(0.113)	(0.114)	(0.079)
Share of Work Done Remotely	0.102	0.134	0.134	–0.417
	(0.091)	(0.086)	(0.087)	(0.693)
Share of Minors	0.205	–0.201	–0.200	0.437*
	(0.155)	(0.165)	(0.164)	(0.237)
Rural County Dummy	0.004			
	(0.010)			
Most Populous County (Top 1%)	0.068***	0.054**		
	(0.021)	(0.021)		
Ln(Residential Population)	–0.027***	–0.023***	–0.023***	–0.034***
	(0.005)	(0.008)	(0.008)	(0.010)
Commuter to Residential Population Ratio	–0.015	–0.008	–0.006	–0.034
	(0.029)	(0.030)	(0.030)	(0.059)
Ln Median Family Income	0.014	0.003	0.002	0.036
	(0.027)	(0.033)	(0.034)	(0.049)
Community Bank Share of Deposits	0.037**	0.075***	0.075***	0.012
	(0.017)	(0.019)	(0.019)	(0.024)
Big4 Bank Share of Deposits	0.093***	0.139***	0.137***	0.003
	(0.033)	(0.039)	(0.040)	(0.054)
Ln Bank Branch Density	0.000	0.001	0.000	0.004
	(0.009)	(0.015)	(0.015)	(0.015)
SBL Volume per Estab< 500 (QCEW 2019 Avg)	–0.002***	–0.001**	–0.001**	–0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Ratio of s Small (Under 500) Employment in 2020Q1 to 2019Q1, QWI	–0.026	–0.091	–0.091	0.011
	(0.049)	(0.080)	(0.080)	(0.066)
UB replacement rate, industry weighted	–0.020	0.030	0.027	–0.035
	(0.049)	(0.074)	(0.074)	(0.064)
Constant	0.585	0.970	1.005	0.206
	(0.436)	(0.707)	(0.708)	(0.619)
Adjusted R-squared	0.14	0.18	0.18	0.11
Observations	2710	1120	1110	1590

Notes: Results from regressions of the share delayed on selected county-level characteristics. "Smaller" refers to urban counties excluding those in the top 1 percent by population. All regressions include state fixed effects. Standard errors clustered at the state level in parentheses. Data sources described in Section 2.2.

Table 3: Summary Statistics: County-Level Urban QWI Sample

	County-Level Mean	SD	Total Aggregate
PPP Receipt			
Share of PPP Loans Delayed	0.44	0.14	0.43
Share of PPP Loans Delayed to < 20	0.49	0.11	0.48
Total PPP Volume (in Millions) for 2020	391.93	1,010.10	449,148.69
Total PPP Volume (in Millions) for 2020+2021	555.01	1,472.41	636,041.15
Total PPP Volume for < 20 (in Millions) for 2020	119.27	303.26	136,684.23
Total PPP Volume for < 20 (in Millions) for 2020+2021	186.16	491.10	213,341.68
Employment			
Population 16+ (in Thousands), 2019	190.93	413.55	218,803.13
Employment All Estabs (in Thousands), QCEW 2019	94.18	229.31	107,934.07
Stable Employment All Estabs (in Thousands), QWI 2019	84.68	211.40	97,045.02
Stable Employment < 500 Estabs (in Thousands), QWI 2019	39.74	95.79	45,545.45
Stable Employment < 20 Estabs (in Thousands), QWI 2019	14.73	37.43	16,878.84
Mthly Total Payroll (All Establ, in Millions), QWI 2019	463.66	1,417.86	531,351.34
Mthly Payroll < 500 Estabs (in Millions), QWI 2019	182.95	510.02	209,660.43
Mthly Payroll < 20 Estabs (in Millions), QWI 2019	57.73	160.94	66,163.10
Share of Stable Employment < 500, QWI 2019	0.55	0.13	0.47
Share of Stable Employment < 20, QWI 2019	0.23	0.09	0.17
Share of Mthly Earnings < 500, QWI 2019	0.50	0.14	0.39
Share of Mthly Earnings < 20, QWI 2019	0.18	0.08	0.12
Establishments			
Private Establishments (in Thousands), CBP 2019Q1	5.90	14.06	6,760.71
Share of Establishments < 500, CBP 2019Q1	1.00	0.01	1.00
Share of Establishments < 20, CBP 2019Q1	0.86	0.04	0.85
PPP to Payroll			
PPP to (Mthly) Total Payroll, 2020	1.10	0.41	0.85
PPP to (Mthly) Total Payroll, 2020 +2021	1.53	0.58	1.20
PPP < 20 to (Mthly) Total Payroll, 2020	0.40	0.19	0.26
PPP < 20 to (Mthly) Total Payroll, 2020 +2021	0.60	0.30	0.40
PPP to (Mthly) < 500 Payroll , 2020	2.20	0.46	2.14
PPP to (Mthly) < 500 Payroll , 2020 +2021	3.06	0.61	3.03
PPP < 20 to < 20 (Mthly) Payroll , 2020	2.16	0.35	2.07
PPP < 20 to < 20 (Mthly) Payroll , 2020 +2021	3.29	0.58	3.22
Observations	1146		

Notes: This table provides county-level and aggregate statistics for the urban sample used in our regressions based on QWI data. Whenever possible, statistics are provided for two establishment-size categories (fewer than 20, and fewer than 500 employees per establishment). Measures related to PPP loans to establishments with fewer than 20 employees (< 20) exclude loans to the self-employed (i.e, we exclude borrowers who reported one or fewer jobs saved and received no more than $20,833, the maximum allowed for self-employed individuals). Data sources described in Section 2.2.

Table 4: Effects of Share of PPP Loans Delayed on Stable Employment in Establishments with Under 20 Employees, QWI

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Counties				Urban Counties			
	Level	Log	E/P	$\Delta E/P$	Level	Log	E/P	$\Delta E/P$
2020Q2 × Share Delayed	0.04	0.01	0.14	0.12	–0.41	–0.02	–0.09	0.04
	(0.12)	(0.01)	(0.09)	(0.10)	(0.71)	(0.02)	(0.10)	(0.12)
2020Q3 × Share Delayed	–0.02	–0.00	–0.02	–0.06	–0.62	–0.06***	–0.28**	–0.17
	(0.13)	(0.01)	(0.12)	(0.15)	(0.82)	(0.02)	(0.12)	(0.13)
2020Q4 × Share Delayed	0.04	0.01	0.02	–0.01	–0.17	–0.05**	–0.26*	–0.15
	(0.10)	(0.01)	(0.09)	(0.11)	(0.58)	(0.02)	(0.14)	(0.13)
2021Q1 × Share Delayed	0.06	0.00	0.03	–0.00	–0.18	–0.06***	–0.29***	–0.19
	(0.10)	(0.01)	(0.11)	(0.11)	(0.60)	(0.02)	(0.09)	(0.12)
2021Q2 × Share Delayed	0.12	0.01	0.05	0.04	0.03	–0.06***	–0.22**	–0.13
	(0.10)	(0.02)	(0.12)	(0.13)	(0.58)	(0.02)	(0.10)	(0.12)
2021Q3 × Share Delayed	–0.05	–0.02	–0.16	–0.18	–0.47	–0.09***	–0.42**	–0.36*
	(0.12)	(0.02)	(0.14)	(0.16)	(0.72)	(0.02)	(0.17)	(0.20)
2021Q4 × Share Delayed	0.13	–0.01	–0.11	–0.11	0.13	–0.07***	–0.33***	–0.24**
	(0.11)	(0.02)	(0.12)	(0.14)	(0.58)	(0.02)	(0.10)	(0.12)
Avg. LHS	6.09	7.39	7.33	–0.25	14.05	8.45	6.67	–0.29
Std. Dev. LHS	22.63	1.47	3.16	0.80	35.63	1.50	2.12	0.56
Share Delayed	0.51	0.51	0.51	0.51	0.49	0.49	0.49	0.49
Within R-squared	1.00	1.00	0.93	0.12	1.00	1.00	0.96	0.24
Observations	24,525	24,525	24,525	24,525	9,142	9,142	9,142	9,142
State FE, Time FE, Lag LHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covid-19 Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results from estimating Equation (1). Standard errors clustered at the state level in parentheses. Samples of all counties and urban counties. Employment in columns (1) and (6) is in thousands. All regressions control for state, quarter-year fixed effects, and a lag of the dependent variable (last quarter of 2019), in addition to preexisting conditions and COVID-19 controls. **Preexisting conditions**: median family income, share of minors, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, community-bank share of deposits, largest four banks' share of deposits, bank branch density, and 2019 small-business-loan volume per small establishment. **COVID-19 controls**: cumulative COVID-19 cases and deaths per population, share of days in April 2020 in early lockdown, share of remote jobs, share of employment in essential industries, and share of employment in most impacted industries. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). The reference period is the first quarter of 2020.

Table 5: PPP Receipt and Small-Establishment Payroll: First Stage

	(1)	(2)
	All PPP	PPP to < 20
IV	< 500 Payroll Share	< 20 Payroll Share
2020Q2 × Payroll Share	2.08***	2.00***
	(0.09)	(0.09)
2020Q3 × Payroll Share	2.08***	2.00***
	(0.09)	(0.09)
2020Q4 × Payroll Share	2.08***	2.00***
	(0.09)	(0.09)
2021Q1 × Payroll Share	2.92***	3.05***
	(0.13)	(0.16)
2021Q2 × Payroll Share	2.91***	3.06***
	(0.13)	(0.16)
2021Q3 × Payroll Share	2.91***	3.05***
	(0.13)	(0.16)
2021Q4 × Payroll Share	2.91***	3.06***
	(0.13)	(0.16)
Within R sq.	0.61	0.83
Observations	9,142	9,142
State FE, Time FE	Yes	Yes
Pre-conditions Controls	Yes	Yes
COVID-19 Controls	Yes	Yes

Notes: Regression of PPP receipt by the end of the indicated quarter-year on payroll share and (time-varying) controls. The regression in column (1) can be interpreted as the first stage regression of the IV regressions in columns (2) and (4) of Table 6, while the regression in column (2) corresponds to the first stage of column (6) in Table 6.

Table 6: Funding Receipt versus Funding Delay. Urban Counties

	(1)	(2)	(3)	(4)	(5)	(6)
	QCEW		QWI (stable)			
	All		All		Under 20	
	OLS	IV	OLS	IV	OLS	IV
2020Q2 × PPP per Total Payroll	1.25***	3.06***	0.43***	0.76***	0.09	0.05
	(0.23)	(0.57)	(0.12)	(0.21)	(0.08)	(0.11)
2020Q3 × PPP per Total Payroll	1.19***	2.38***	0.88***	1.74***	0.44***	0.43***
	(0.25)	(0.60)	(0.17)	(0.29)	(0.10)	(0.11)
2020Q4 × PPP per Total Payroll	0.70***	1.31**	0.75***	1.28***	0.34***	0.25***
	(0.17)	(0.53)	(0.14)	(0.27)	(0.07)	(0.08)
2021Q1 × PPP per Total Payroll	0.33***	0.84**	0.41***	0.65***	0.18***	0.14**
	(0.12)	(0.40)	(0.09)	(0.18)	(0.04)	(0.06)
2021Q2 × PPP per Total Payroll	0.70***	1.29***	0.55***	1.03***	0.20***	0.18***
	(0.13)	(0.42)	(0.08)	(0.20)	(0.05)	(0.06)
2021Q3 × PPP per Total Payroll	0.91***	1.36***	0.75***	1.26***	0.46***	0.45***
	(0.17)	(0.46)	(0.11)	(0.25)	(0.12)	(0.13)
2021Q4 × PPP per Total Payroll	0.38***	0.67	0.52***	0.92***	0.27***	0.27***
	(0.14)	(0.47)	(0.10)	(0.24)	(0.07)	(0.08)
2020Q2 × Share Delayed	1.71*	1.47	0.45*	0.42*	0.04	0.04
	(0.86)	(0.88)	(0.25)	(0.25)	(0.12)	(0.11)
2020Q3 × Share Delayed	0.51	0.37	0.65	0.54	–0.21	–0.21
	(0.64)	(0.63)	(0.40)	(0.40)	(0.15)	(0.14)
2020Q4 × Share Delayed	0.16	0.10	0.28	0.23	–0.18	–0.17
	(0.41)	(0.39)	(0.41)	(0.41)	(0.15)	(0.14)
2021Q1 × Share Delayed	–0.09	–0.21	0.09	0.04	–0.22*	–0.21*
	(0.45)	(0.44)	(0.42)	(0.42)	(0.12)	(0.12)
2021Q2 × Share Delayed	–0.18	–0.32	0.32	0.21	–0.17	–0.16
	(0.63)	(0.60)	(0.42)	(0.41)	(0.12)	(0.12)
2021Q3 × Share Delayed	–0.00	–0.10	0.18	0.06	–0.44**	–0.44**
	(0.80)	(0.75)	(0.46)	(0.45)	(0.21)	(0.21)
2021Q4 × Share Delayed	–0.20	–0.26	0.09	–0.01	–0.29**	–0.29**
	(0.66)	(0.62)	(0.44)	(0.43)	(0.12)	(0.13)
Avg. LHS	–1.89	–1.89	–1.49	–1.49	–0.29	–0.29
Avg. PPP/Payroll	1.34	1.34	1.34	1.34	0.51	0.51
Share Delayed	0.44	0.44	0.44	0.44	0.49	0.49
Pop 16+, thousands	190.8	190.9	190.8	190.9	190.8	190.9
KP Wald F		96.3		95.8		48.8
AR Wald χ^2		134.4		74.8		48.7
AR Wald p-val.		0.00		0.00		0.00
Observations	9,142	9,142	9,142	9,142	9,142	9,142
State FE, Time FE, Lag LHS	Yes	Yes	Yes	Yes	Yes	Yes
Pre-conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results from estimating augmented Equation (1). The LHS is the county-level change in employment relative to 2019 population in the category indicated in the column headings. Standard errors clustered at the state level in parentheses. All regressions control for state, quarter-year fixed effects, and a lag of the dependent variable (last quarter of 2019), in addition to preexisting conditions and COVID-19 controls. **Preexisting conditions**: median family income, share of minors, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, community-bank share of deposits, largest four banks' share of deposits, bank branch density, and 2019 small-business-loan volume per small establishment. **COVID-19 controls**: cumulative COVID-19 cases and deaths per population, share of days in April 2020 in early lockdown, share of remote jobs, share of employment in essential industries, and share of employment in most impacted industries. Share Delayed is the share (by volume) of county-level PPP loans delayed, as defined in Equation (2) for all loans in columns (1)–(6), and for loans whose applicants reported fewer than 20 employees in columns (7)–(8). PPP per total payroll in columns (7)–(8) is specific to establishments with under 20 employees and excludes loans to the self employed. The instrument is the 2019 share of total payroll in establishment with fewer than 500 employees in columns (1)–(6) and the 2019 share of total payroll in establishment with fewer than 20 employees in columns (7)–(8). The reference period is the first quarter of 2020.

Table 7: Job-Years Retained by PPP Receipts and Implied Cost

	Total Employment, QCEW	Stable Employment, QWI	
	Baseline: Total PPP by year		
	All	All	Under 20
Job-year saved			
2020	3,124,115	1,743,543	103,223
2021	2,722,683	2,532,101	229,067
Total	5,846,798	4,275,644	332,290
Cost per job-year	$88,997	$127,179	$546,777
	Total PPP (2020+2021)		
	All	All	Under 20
Job-year saved			
2020	3,156,332	1,762,309	105,299
2021	2,721,992	2,530,567	228,888
Total	5,878,324	4,292,876	334,187
Cost per job-year	$108,201	$148,162	$638,390
	Cumulative PPP		
	All	All	Under 20
Job-year saved			
2020	3,118,488	1,742,294	103,109
2021	2,724,361	2,533,553	229,675
Total	5,842,849	4,275,847	332,784
Cost per job-year	$87,503	$125,593	$539,165

Notes: This table presents estimates of job-years saved and the associated costs under different scenarios of PPP receipt anticipation. All scenarios assume past receipts affected job retention/creation until the end of the sample, 2021:Q4. The baseline panel uses the estimates of Table 6 and the aggregate values in Table 3. Job-years retained are calculated by rearranging the terms in Equation (1): $\Delta E = \beta \left(\frac{\texttt{PPP}}{\texttt{payroll}}\right) \frac{\texttt{pop}}{100}$. The resulting numbers are summed up for the corresponding quarters and divided by 4 to convert to job-years. The implied cost is computed by dividing the ultimate total PPP outlay at the end of the entire program (May 31, 2021) by the number of job-years saved. In the baseline scenario, jobs retained over 2020 must be scaled up to account for the fact that only a portion (roughly $525 out of $800 billion) of the ultimate outlay was disbursed in the 2020 PPP. The scaling factor is thus the ratio of ultimate PPP outlay over 2020 outlay, equivalent to the `PPP/payroll` ratio at program end in 2021 relative 2020. Analogous (but in this case quarter by quarter) scaling is needed to compute the implied cost in the Cumulative-PPP scenario (i.e., the RHS is the cumulative PPP received up to a given quarter).

Table 8: Summary Statistics: Firm-Level Advan Sample

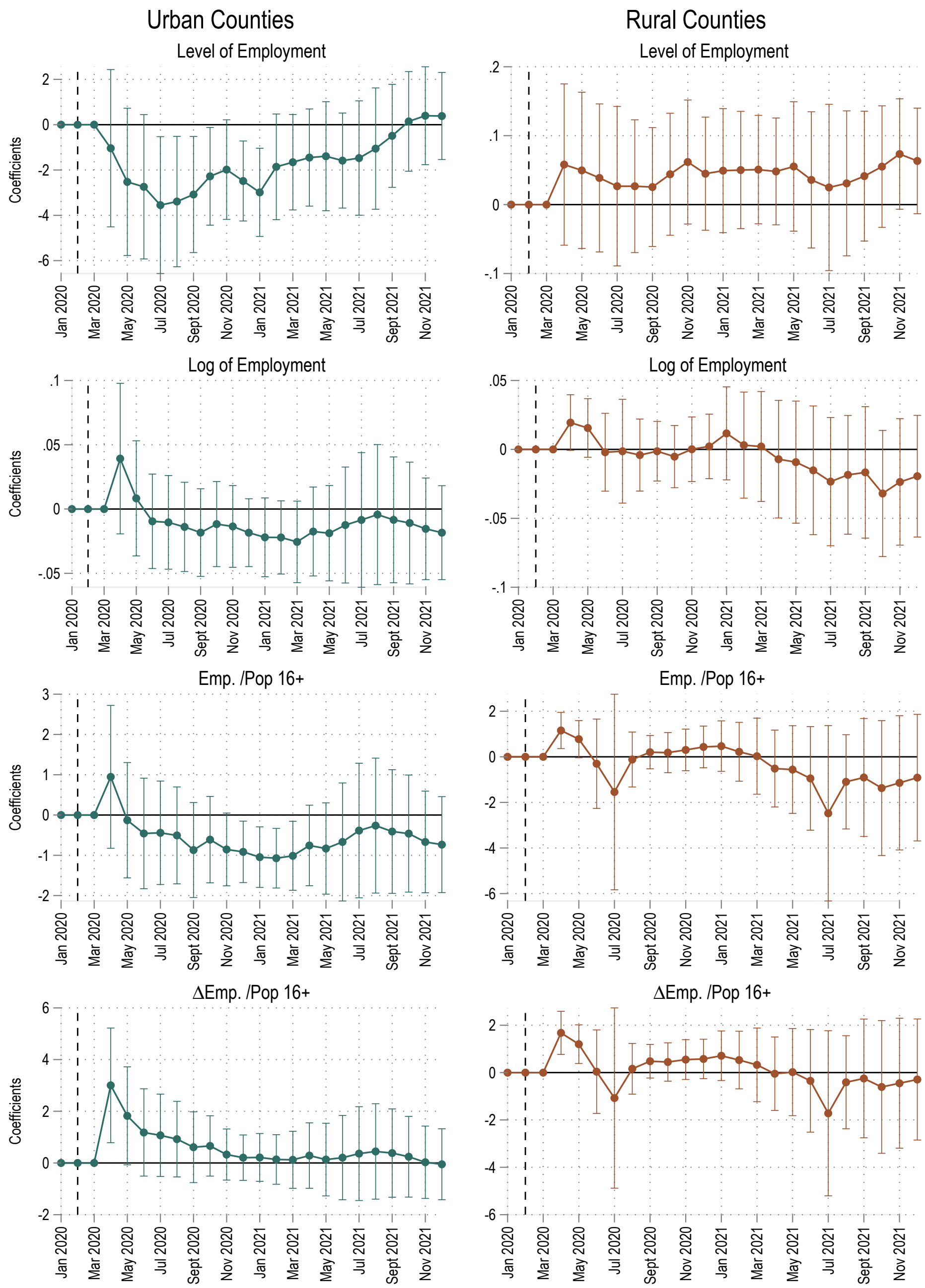
	N(Pairs)	Mean	P25	Median	P75
PPP Business Avg. Weekly Visits	65,136	43.45	8.23	21.85	49.08
% Diff. in Visits: PPP vs. non-PPP Business	65,136	0.02	-2.42	0.00	2.48
Distance (Miles): PPP vs. non-PPP Business	65,136	13.65	7.96	11.64	17.14
PPP Loan Size ($1,000)	65,136	139.01	21.89	49.10	114.30
No. of Jobs Saved	65,136	18.74	4.00	8.00	17.00
Early DK PPP Firms					
PPP Business Avg. Weekly Visits	8,575	44.70	8.31	22.00	48.85
% Diff. in Visits: PPP vs. non-PPP Business	8,575	0.02	-2.46	0.00	2.52
Distance (Miles): PPP vs. non-PPP Business	8,575	13.51	7.97	11.59	16.98
PPP Loan Size ($1,000)	8,575	169.98	29.40	63.20	142.76
No. of Jobs Saved	8,575	21.28	5.00	10.00	20.00
Late DK PPP Firms					
PPP Business Avg. Weekly Visits	8,870	42.00	8.08	21.15	47.31
% Diff. in Visits: PPP vs. non-PPP Business	8,870	0.03	-2.43	0.00	2.48
Distance (Miles): PPP vs. non-PPP Business	8,870	13.65	8.00	11.75	17.31
PPP Loan Size ($1,000)	8,870	133.29	23.31	51.35	116.38
No. of Jobs Saved	8,870	17.63	4.00	8.00	17.00

Notes: 2020 PPP recipient firms are matched to non-PPP firms that operate in the same 6-digit NAICS industry, have similar pre-pandemic visits (within 5% between December 2019 and February 2020) and are located in the same county and between 5 and 50 miles away. Early DK PPP firms are those that received PPP loans between April 14 and 16, 2020, while late DK PPP firms received loans on April 27 and 28, 2020, as defined in Doniger and Kay (2023). The sample excludes the self-employed. Business visits are averages for the months of December 2019 to February 2020.

Figure 1: Effect of Share Delayed on Total Private Employment, QCEW
All Counties

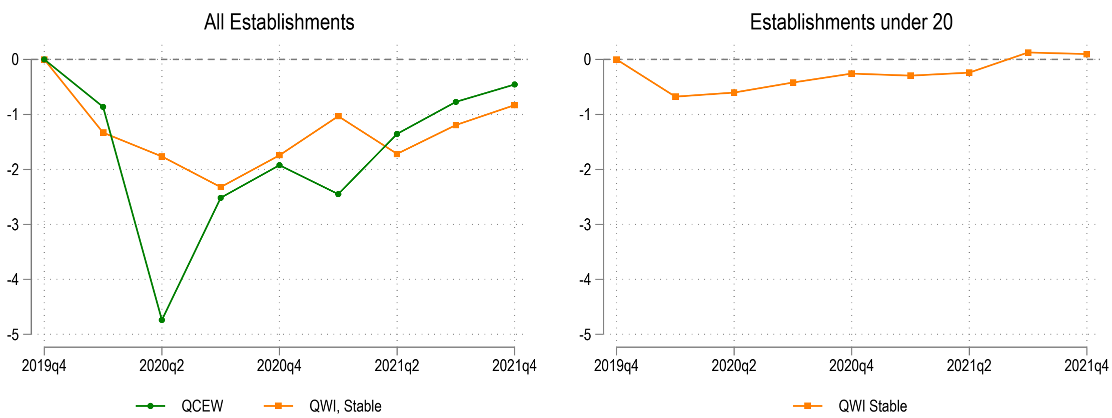
Notes: Coefficients for the share delayed, defined in Equation (2), estimated using Equation (1). All regressions control for state, month-year fixed effects, and a lag of the dependent variable (as of December 2019), in addition to preexisting conditions and COVID-19 controls. The reference period is the February 2020, as indicated by the vertical dotted lines. Standard errors clustered at the state level. Employment levels are measured in thousands. **Preexisting conditions**: median family income, share of minors, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, community-bank share of deposits, largest four banks' share of deposits, bank branch density, and 2019 small-business-loan volume per small establishment. **COVID-19 controls**: cumulative COVID-19 cases and deaths per population, Stringency index, share of days in April 2020 in early lockdown, share of work done remotely, share of employment in essential industries, and share of employment in most impacted industries. Most Impacted industries include NAICS 44-45, 61, 71, 72 and 81.

Figure 2: Effect of Share Delayed on Total Private Employment, QCEW
Urban versus Rural Counties



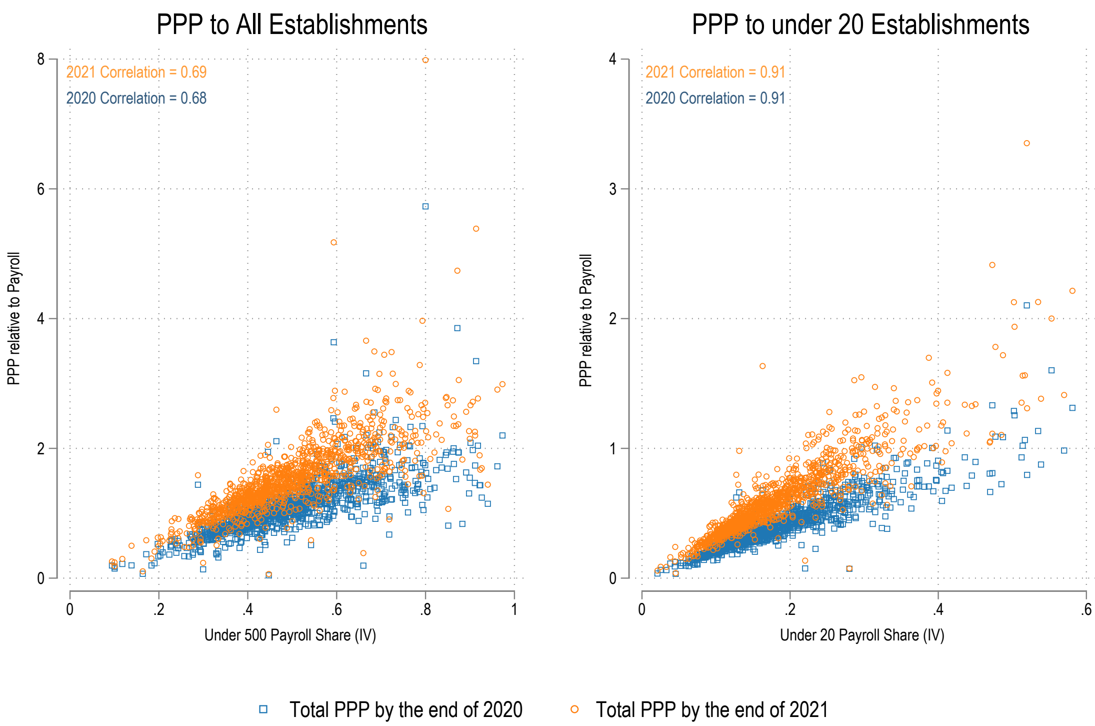
Notes: Coefficients for the share delayed, defined in Equation (2), estimated using Equation (1). All regressions control for state, month-year fixed effects, and a lag of the dependent variable (as of December 2019), in addition to preexisting conditions and COVID-19 controls. The reference period is the February 2020, as indicated by the vertical dotted lines. Standard errors clustered at the state level. Employment levels are measured in thousands. **Preexisting conditions**: median family income, share of minors, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, community-bank share of deposits, largest four banks' share of deposits, bank branch density, and 2019 small-business-loan volume per small establishment. **COVID-19 controls**: cumulative COVID-19 cases and deaths per population, Stringency index, share of days in April 2020 in early lockdown, share of work done remotely, share of employment in essential industries, and share of employment in most impacted industries.

Figure 3: Evolution of Employment



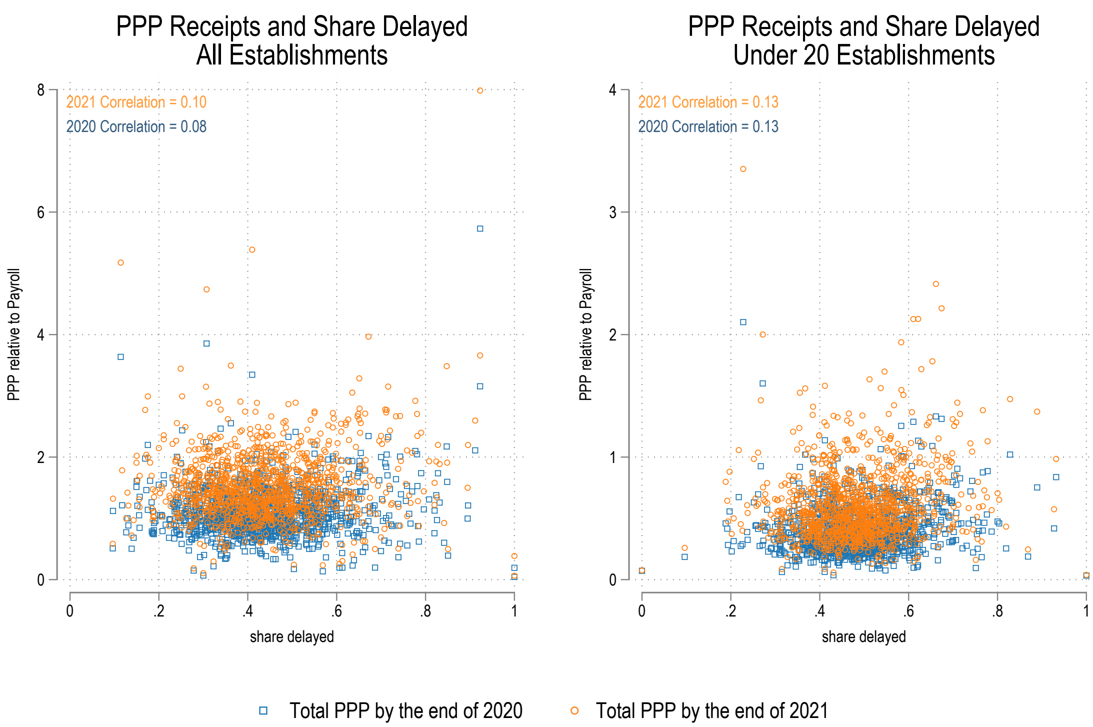
Notes: The figure shows the change in employment to 2019 population relative to the last quarter of 2019, or $\Delta e_t = \frac{E_t - E_{2019:Q4}}{P_{2019}} \times 100$. Mean across counties in our urban regression sample.

Figure 4: Correlation of PPP receipts and Payroll Share



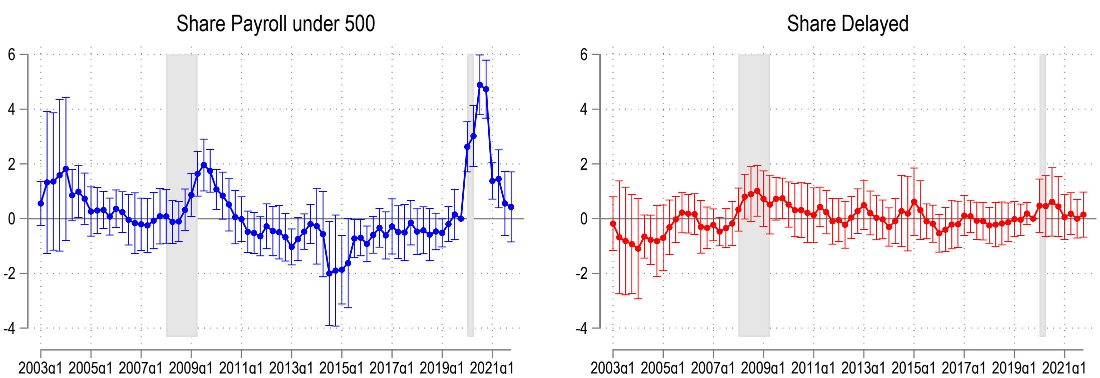
Notes: The sample underlying this figure includes only counties in our urban-regression sample. For both panels, the corresponding PPP receipts are normalized by **total** monthly payroll in each county. To facilitate interpretation of the left panel, note that with an under-500 payroll share of 1, all employment would be in establishments with under 500 employees, implying a maximum PPP-to-payroll ratio of about 2.5 in 2020 (10 weeks, or equivalently 2.5 months, of payroll). With a share of 0.5, the predicted maximum would be 1.25 (that is, 0.5×2.5). Actual ratios would differ if some eligible businesses did not borrow, some borrowers exceeded 500 employees, certain ineligible firms borrowed, or due to other irregularities. In the right panel, we plot PPP receipts specific to under-20-employee establishments relative to total payroll (on the y axis) against under-20 payroll share (x axis), but the interpretation is analogous.

Figure 5: Correlation of PPP receipts and Share Delayed

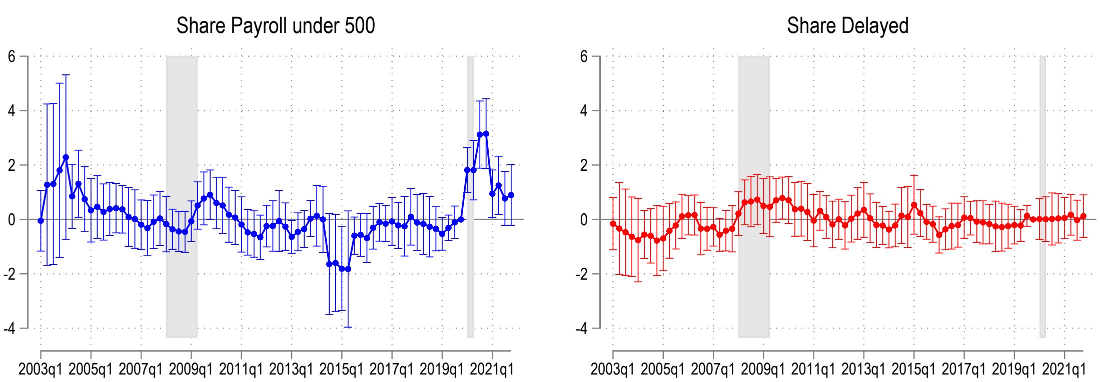


Notes: PPP receipts on both panels are normalized by total monthly payroll in each county. The share delayed is defined in Equation (2). Counties in our urban regression sample.

Figure 6: Stable Employment Evolution based on 2019 payroll and DK delay, QWI
All Establishments



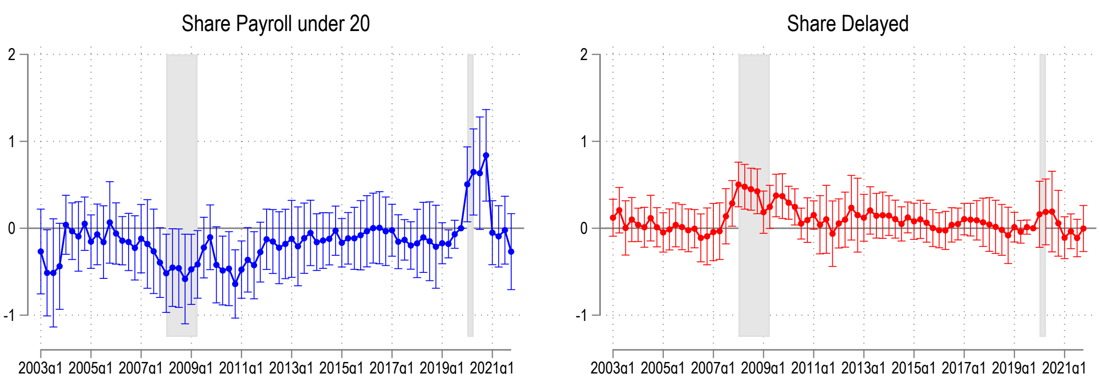
(a) Basic Controls



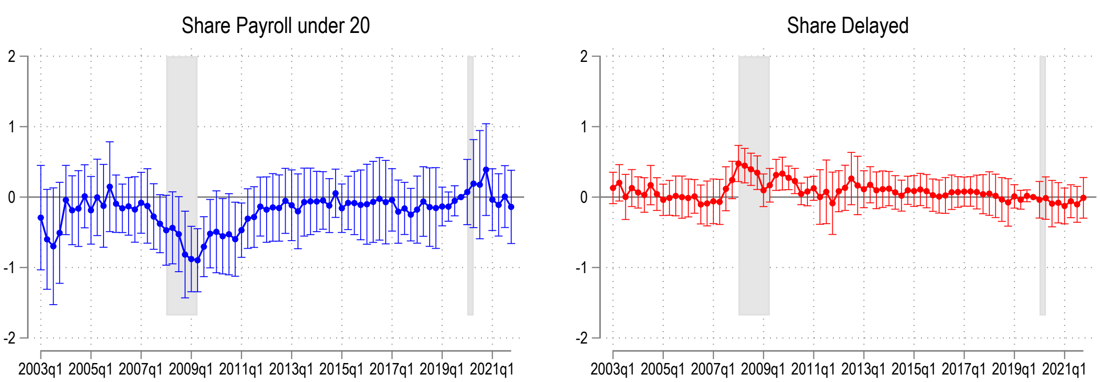
(b) All Controls

Notes: Let $e_t = \frac{E_t}{P_t} \times 100$ and $\Delta e_t = e_t - e_{t-4}$. The figures plot β_t and γ_t from the regression $\Delta e_t = \alpha + \beta_t\,\text{share}_{2019} + \gamma_t\,\text{delay} + \zeta_t\,L4.\Delta e_t + X_t \Pi_t + \epsilon_t$. Regressions also include state and quarter-year fixed effects. The bottom panel regressions include all controls in our baseline regressions (including banking variables) adjusted to the relevant time period whenever possible. The top panel excludes $X_t \Pi_t$.

Figure 7: Stable Employment Evolution based on 2019 payroll and DK delay, QWI Establishments under 20 Employees



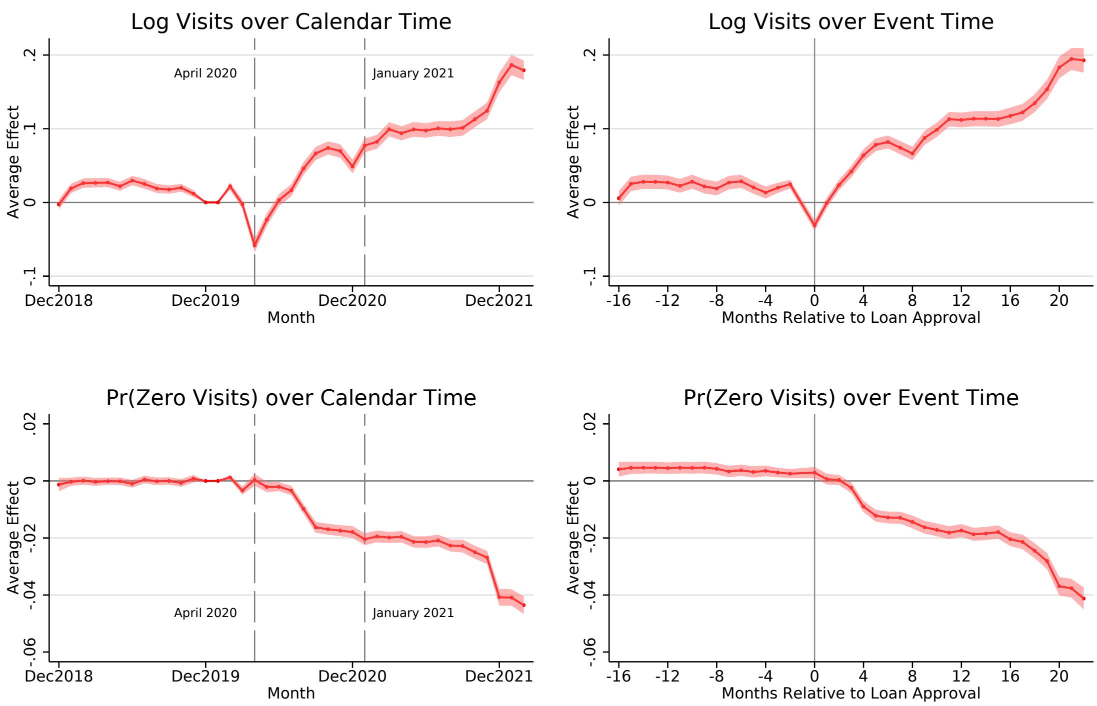
(a) Basic Controls



(b) All Controls

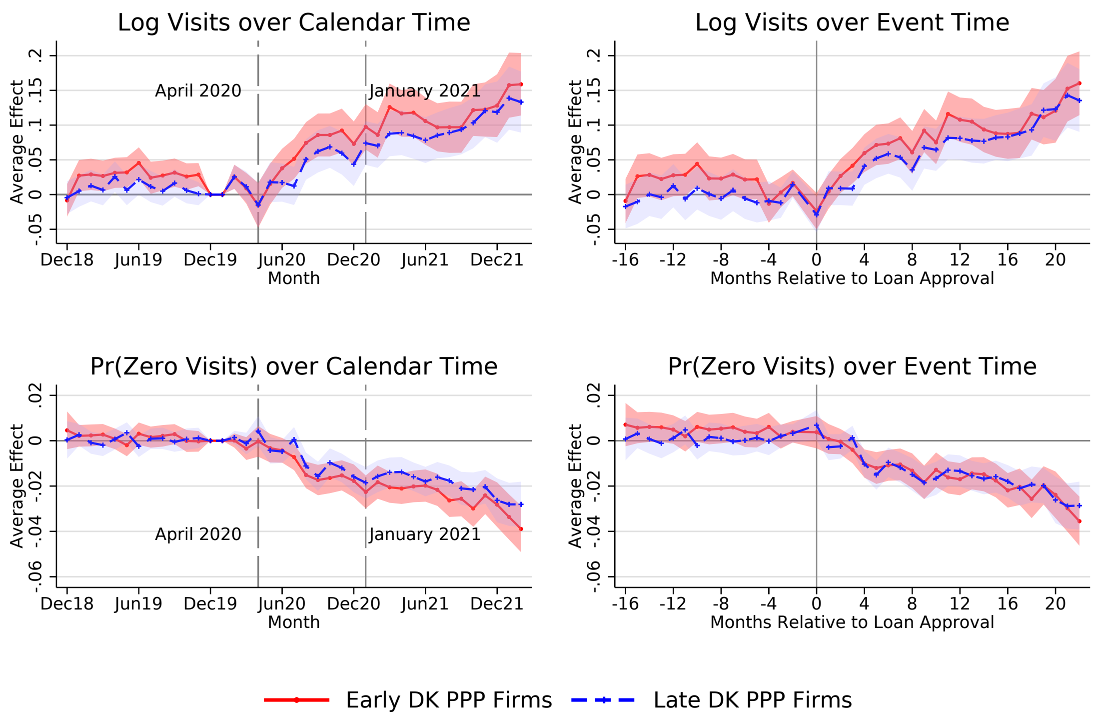
Notes: Let $e_t = \frac{E_t}{P_t} \times 100$ and $\Delta e_t = e_t - e_{t-4}$. The figures plot β_t and γ_t from the regression $\Delta e_t = \alpha + \beta_t \,\text{share}_{2019} + \gamma_t \,\text{delay} + \zeta_t \, L4.\Delta e_t + X_t \Pi_t + \epsilon_t$. Regressions also include state and quarter-year fixed effects. The bottom panel regressions include all controls in our baseline regressions (including banking variables) adjusted to the relevant time period whenever possible. The top panel excludes $X_t \Pi_t$.

Figure 8: Effect of PPP on Visits
Matching Estimates using Advan Firm Pairs



Notes: The left panels plot the β_t coefficients estimated from Equation (5), while the right panels plot the μ_l coefficients from Equation (6). 2020 PPP recipient firms are matched to non-PPP firms that operate in the same 6-digit NAICS industry, have similar pre-pandemic visits (within 5% between December 2019 and February 2020) and are located in the same county and between 5 and 50 miles away. The sample excludes the self-employed. Log visits are defined as the log of visit counts plus one. Zero visits is a dummy equal to one when Advan records zero visits or when missing observations are filled in. Shaded areas represent 95 percent confidence intervals, with standard errors clustered by firm pair.

Figure 9: Effect of PPP on Visits
Early versus Late PPP Recipients in Advan



Notes: These graphs compare PPP's effects on visits depending on the date of loan approval. Early firms are those that received PPP loans between April 14 and 16, 2020, while late firms received loans on April 27 and 28, 2020, as defined in Doniger and Kay (2023). PPP firms are matched to non-PPP firms that operate in the same 6-digit NAICS industry, have similar pre-pandemic visits (within 5% between December 2019 and February 2020) and are located in the same county and between 5 and 50 miles away. The sample excludes the self-employed. The shaded areas represent 95 percent confidence intervals, with standard errors clustered by firm pair.

Online Appendix – Not for Publication

A Data: Additional Details

Pre-pandemic Local Conditions

The Quarterly Census of Employment and Wages (QCEW) reports employment data at a monthly frequency, while the total number of establishments and payroll (wages) at a quarterly frequency. According to QCEW, right before the COVID-19 outbreak, there were, on average, about 39,500 employees working in 3,000 establishments (Table 1) in an average county.

On average, a county had about 27.5 bank branches serving around 104,500 people, corresponding to a commuter-adjusted daytime population of 120,000, with a median family income of \$66,500 (Table 1). In 2019, an average county received 2,320 small business loans (SBL) with a total volume of \$80.31 million and an average amount of \$35,000. In our sample, 63 percent of counties are classified as rural according to the 2013 National Center for Health Statistics (NCHS) urban-rural classification scheme. We classify a county as rural if its urban-rural 2013 classification scheme is greater than 4 (the scale of population density ranges from 1 to 6, from most to least populated). On average, a rural county has a population of 23,650 people vs. 240,400 living in an urban county. Rural population accounts for 14 percent of the total population in our sample.

Public Health Measures and Relative Size of the PPP

In mid-March 2020, in response to the pandemic, the federal, state, and local governments instituted non-pharmaceutical interventions to curb the spread of the COVID-19 virus, which led to a significant drop in employment, especially for small businesses. The Coronavirus Aid, Relief, and Economic Security (CARES) Act was introduced to reduce the economic impact of mandatory shutdowns. In April 2020, an average county had received 907 PPP loans for a total of \$133 million and an average loan of \$97,200. However, 48 percent of PPP loans were delayed (58 percent if we use the total number of delayed loans instead of the volume of loans) because PPP funding ran out on April 16 and was reinstated on April 27 (see Table 1). By August 2020, the average county had received a total volume of \$188 million. Importantly, the volume of 2020 PPP loans substantially exceeded each county's SBL volume in 2019. In fact, by the end of April 2020, each county had already received, on average, almost twice the 2019 SBL volume in PPP funding, and this multiple increased to three by the end of the 2020 PPP.

B Evolution of the PPP Provisions and Their Potential Implications

The 2020 CARES Act, signed into law on March 27, 2020, appropriated \$349 billion in PPP loans in response to the widespread shutdowns caused by the COVID-19 pandemic. The PPP funds were provided to businesses that employed no more than 500 workers so that they would have the resources to maintain or hire back employees that had been laid off, and to cover overhead costs incurred as a result of the pandemic. This section describes the changes in program rules over time, and their potential implications for the relative trajectory of employment recovery in urban centers, which were hit hard by the outbreak and were fundamentally more likely to suffer longer-lasting damages.

Several key provisions in the CARES Act for the PPP were later modified in the Paycheck Protection Program Flexibility Act (PPPFA).[1] Four of these amended provisions had the greatest potential to slow the recovery of employment after the initial acute phase of the pandemic, especially in places that were hardest hit by the initial outbreak. This effect carries the same sign as that of the *delay* in funding or lack of funding for small businesses. More importantly, the likely amendments to the original CARES Act provisions became known before April 27, 2020, when bank lending under the PPP resumed with the additional funding appropriation, and thus it could have led to differential behavior of firms that received loans just before the 10-day window (referred to as the early recipients) versus those that received loans just after the window (the late recipients). In other words, the cross-sectional disparity in the impact of these amendments could be correlated with the degree of funding delay.

First, the PPPFA extended the period in which borrowers could spend their PPP funds in order to be considered for loan forgiveness from eight weeks following the date of the loan (that is, disbursement of loan proceeds) to the earlier of 24 weeks following the date of the loan or December 31, 2020. However, businesses that obtained PPP loans before the effective date of the PPPFA could elect to use the original eight-week period, allowing them to apply for forgiveness earlier. The proposal to extend the covered period was first raised by the Main Street Alliance on April 22, 2020, and was reported by the *Adhesives & Sealants Industry Magazine* on April 23. On April 29, 2020, it was reported by all journals (including the *Portland Business Journal*) under the umbrella of American City Business Journals. The Small Business & Entrepreneurship Council more specifically proposed the 24-week expansion on April 30, which was then reported by the *Wall Street Journal* on May 3, 2020.[2]

The PPPFA also changed the loan proceeds use formula from 75 percent on payroll and 25 percent on eligible fixed expenses (such as rent, interest on debt, and utilities) to 60 percent on payroll and 40 percent on other eligible expenses. The formula change was first reported by a major news outlet, *USA Today*, on April 20, 2020. That article noted that the trade publication *Nation's Restaurant News* reported a similar proposed change to the formula on April 9, 2020. This provision permitted borrowers to keep or hire back fewer employees because the firms were required to spend less of a given PPP loan on payroll. The amount of a loan was still capped at 2.5 months of pre-pandemic payroll.

Perhaps more importantly, the PPPFA relaxed the requirement that borrowers must rehire employees by June 30, 2020: The amount that could potentially be forgiven would not be reduced due to a decrease in the borrower's full-time equivalent (FTE) workforce count if the firm could document that it (1) attempted, but was unable, to rehire previous employees as of February 15, 2020; and (2) was unable to hire "similarly qualified employees" before December 31, 2020. This change was implemented to recognize that some businesses may not have been allowed to reopen by June 30, 2020, and even if they were allowed to reopen by that date, they may have had to reopen in stages, allowing them to hire back employees over a longer period of time.

[1]The PPPFA passed the House on May 28, 2020, and the Senate on June 3 of that year. It was signed into law on June 4, 2020.

[2]See ASI and SBE Council.

Additionally, the amount potentially forgiven would not be reduced due to a decrease in the FTE workforce count if the borrower, in good faith, could document an inability to return to the "same level of business activity" with which it operated prior to February 15, 2020, due to sanitation, social distancing, or safety requirements for workers or customers. This provision recognized that businesses may not have been able to locate and hire qualified employees, because in many industries, the workforce could have relocated due to the pandemic and become unavailable to employers. These last two provisions made it less or not at all necessary for businesses to operate at close to their pre-pandemic levels by June 30, and businesses could cut back permanently yet still have their PPP loans forgiven. This change, among all the changes described so far, can probably best help explain the persistent effect of the 10-day delay on employment in those hardest hit industries.

News about the broad contour of potential changes to the PPP program (resembling the actual changes just described) that might be incorporated into a new bill to enhance the program's flexibility started circulating widely as early as April 11, 2020, when it was reported by the *New York Times* and the *Washington Post*.[3] Also, a bipartisan group of House representatives sent a letter to House leadership on April 16 requesting greater flexibility in the program in order to better assist small businesses.[4] These dates suggest that many small businesses, if they had already closed early due to the outbreak and the resulting containment measures, could reasonably begin to reconsider over the 10 days (when additional funding appropriation for the PPP was going through the legislative process) their reopening plan. To the extent that a higher fraction of late borrowers had closed by April 27, 2020, when PPP lending resumed, the additional "options" offered by the new provisions would have greater impact on late borrowers than on early borrowers. This effect is likely more pronounced for businesses in urban centers, which had been much more severely disrupted at the outset and thus more likely to have closed before the PPP restarted.

In sum, several important changes became widely anticipated by small businesses over the 10-day period of funding delay that could lead to reoptimization, in particular to delaying reopening, since businesses could reasonably expect a much longer period over which to spend the funding without having to worry about a reduced amount of loan forgiveness. This incentive to delay reopening should be particularly strong for May and June 2020, when demand was far from fully recovered. With the program changes, businesses were able to preserve PPP funds for later use (to pay employees and fixed operating expenses) when they could expect a higher volume of demand and/or when the public health situation improved sufficiently.

The incentive to wait was likely stronger for businesses that closed before April 27, since they would need to spend a fixed cost (such as restocking the kitchen or store shelves) to reopen. Thus, it likely also applies more to essential businesses that had chosen to close, along with all nonessential businesses, which had to shut down in most states during the lockdown phase in response to the initial COVID-19 outbreak. In addition, it should be more relevant for localities that experienced less demand recovery in May and June, such as densely populated urban centers, because they were subject

[3] Similar reporting appeared as early as April 8, 2020, in Maine's *Bangor Daily News* and in *Restaurant Hospitality*, a trade publication.

[4] This effort was led by representatives Abigail Spanberger, Brian Fitzpatrick, and Josh Gottheimer; see this House webpage.

to greater mandatory containment measures or voluntary cutbacks in mobility (due to greater perceived risk of infection, which could depend on many factors, including higher density or media communication, not just actual infection rates).

More importantly, we argue that the option value of waiting was higher for small firms that had not received PPP loans before the first round of funding ran out (on April 16) because they were more likely to have closed due to the lack of liquidity. In addition, they had not used any of the funds to pay and retain their employees and thus faced no countervailing incentive to adhere to the original eight-week covered period so that they could apply for loan forgiveness earlier. Moreover, it is possible that, even among the recipients within the set considered for the natural experiment (that is, those that received loans over April 15 and 16 versus April 27 and 28—just before versus just after the 10-day window of delay), the firms that received PPP loans earlier had, on average, better prospects even without the PPP, which at the margin made it more likely for them to have stayed open or to be better equipped to reopen sooner when public health conditions improved sufficiently. To the extent that such correlation is present, the difference in reopening dynamics observed cannot be solely attributed to the lack of funding effects. Given that this is unobserved heterogeneity, the only indication we can test is that such firms were more likely to have stayed open prior to receiving any funding, if they were allowed to, under the assumption that their better prospect made it more valuable for them to stay open (to gain market share from their rivals, for example). At the level of the locality, the implication is that those places with relatively higher shares of PPP loans delayed may have had higher shares of firms with a poorer prospect, after we control for observables.

In conclusion, the general idea is that the effect of the PPP on employment is not just due to the liquidity it provided, let alone the timing delay. Instead, it is also the consequence of other design features of the PPP, as well as the cross-sectional difference in inherent susceptibility to COVID-19 disruptions.

C IV estimates Subject to Imperfect Compliance

Imperfect compliance with the 500-employee threshold (as documented, for example, in Splinter et al. 2025) could bias estimates of the LATE for compliers. However, this is unlikely in our setting. As shown in Angrist, Imbens, and Rubin (1996), instrumental-variable estimators using program assignment as an instrument for actual treatment recover the correct LATE for compliers as long as there are no defiers among the non-compliers. In the PPP setting, the non-compliant firms fall into two groups: i) those with no more than 500 employees that did not borrow, and ii) those with more than 500 employees that borrowed. To be a defier, a firm would need to act *contrary* to its assignment. In other words, it would borrow when not eligible and not when eligible. Intuitively, such behavior is likely to be extremely rare if it exists at all. More plausibly, group i) consists of never-takers or firms that would not borrow regardless of the eligibility threshold because they were not adversely affected, because they perceived the costs (pecuniary or otherwise) as exceeding the benefits, or due to other idiosyncratic reasons.[5] Group ii) most likely includes firms eligible under exceptions to the 500

[5]Note that small firms that went out of business before they could borrow due to congestion or PPP funding delay are not defiers, since they did not act contrary to assignment: they would have

employee threshold or other qualifying criteria. These non-compliers are most likely always-takers.

As long as non-compliance takes the form of always-takers and never-takers, the standard IV estimator continues to identify the correct LATE for compliers. Accordingly, our estimates of the job-years retained and the associated costs do not require adjustments due to the imperfect compliance. This appendix discusses this issue in more detail and also provides an alternative estimate that treats all non-borrower small firms and borrower large firms as defiers. Our robustness check shows that, if anything, our main results might underestimate the job-years retained.

When the monotonicity condition is violated, i.e., there are defiers among non-compliers, the IV coefficient becomes a biased estimate of the LATE on compliers. As clearly noted in Angrist, Imbens, and Rubin (1996) (Table 1 in particular), there can be three types of non-compliers: never-takers, always-takers, and defiers. These types are defined within the counterfactual framework, and it is not typically feasible to map a type to an individual subject. Specific to the PPP setting, non-borrowers with 500 or fewer employees were not necessarily defiers but could simply be never-takers, i.e., they would not have borrowed regardless of the eligibility rule. In fact, this seems more plausible than classifying them as defiers—that they would instead have taken PPP funding had they been explicitly excluded. Likewise, borrowers with over 500 employees might not be defiers but simply always-takers. Unless we classify either or both of the two subsets of firms as defiers, the standard IV coefficient remains valid as a LATE estimate.

We use the notation and derivations à la Angrist, Imbens, and Rubin (1996) to show this conclusion. Denote the potential outcome Y as a function of assignment Z and treatment D, which is itself a function of Z (i.e., $D(Z)$), as $Y(Z, D(Z))$. Thus, compliers and defiers are characterized by $\Pr(D(1) - D(0) = 1)$ and $\Pr(D(1) - D(0) = -1)$, respectively, while never-/always-takers are those with $\Pr(D(1) - D(0) = 0)$. The definitions of these types using the counterfactual notation makes it clear that it is generally impossible to identify an individual i's type because we only observe one of the outcomes (e.g., the outcome of either $D(0)$ or $D(1)$) but not both ($D(0)$ or $D(1)$). (Note that all these terms are defined at the individual level, but we omit the i subscript for brevity.)

Denote the potential outcome if treated versus not $Y(1)$ and $Y(0)$, respectively. Denote the coefficient from the IV regression of Y on treatment D using IV Z as β^{IV}, the reduced-form coefficient of Y on Z and D on Z as θ^Y and θ^D, respectively, and the

borrowed had they had a chance.

correct LATE estimate as β^*. When there is imperfect compliance, we can write:

$$\begin{aligned}
\beta^* &= \mathbf{E}\{Y(1) - Y(0) \mid D(1) - D(0) = 1\} \equiv \mathbf{E}\{\Delta Y \mid \text{complier}\},\\
\theta^Y &= \mathbf{E}\{Y(1, D(1))\} - \mathbf{E}\{Y(0, D(0))\}\\
&= \mathbf{E}\{Y(1) - Y(0) \mid D(1) - D(0) = 1\} \cdot \Pr(D(1) - D(0) = 1)\\
&\quad - \mathbf{E}\{Y(1) - Y(0) \mid D(1) - D(0) = -1\} \cdot \Pr(D(1) - D(0) = -1)\\
&\equiv \mathbf{E}\{\Delta Y \mid \text{complier}\} \Pr(\text{complier}) - \mathbf{E}\{\Delta Y \mid \text{defier}\} \Pr(\text{defier})\\
\theta^D &= \mathbf{E}\{D(1) - D(0)\}\\
&= \Pr(D(1) - D(0) = 1) - \Pr(D(1) - D(0) = -1),\\
\beta^{IV} &= \frac{\theta^Y}{\theta^D}.
\end{aligned}$$

It is clear that neither always-takers nor never-takers appear in θ^Y or θ^D, because for both types, $Y(1, D(1)) - Y(0, D(0)) = 0$ and $D(1) - D(0) = 0$. Once we rule out defiers among non-compliers, β^{IV} recovers β^*. In fact, as noted in Angrist, Imbens, and Rubin (1996), the standard latent index specification of the relationship between D and Z in the first stage implies that the monotonicity condition is implicitly satisfied.

If the monotonicity condition is violated due to defiers, then the bias $\beta^{IV} - \beta^*$ can be expressed as follows:

$$(\mathbf{E}\{\Delta Y \mid \text{complier}\} - \mathbf{E}\{\Delta Y \mid \text{defier}\}) \frac{\Pr(\text{defier})}{\Pr(\text{complier}) - \Pr(\text{defier})}. \tag{A.1}$$

The first term is the difference in treatment effects between compliers and defiers, while the second term depends on the relative share of compliers and defiers. Without defiers, the second term is zero and hence no bias. If there are defiers, the smaller the share of defiers, the smaller the bias, ceteris paribus. (As noted already, we believe that there should be essentially no defiers in the PPP setting.) In the unlikely event that compliers and defiers have the same treatment effect, the bias is also zero.

However, we explore the most plausible situation concerning defiers in our setting. In our setting, defiers would be firms that borrowed even though they did not qualify; however, they would *not* have borrowed if they qualified for funding. As noted, this definition, stated within the framework of counterfactuals, cannot be verified for any individual firm because we only observe one outcome. To conduct robustness checks, we lower the standard and thus expand the set of firms that we would classify as defiers for this purpose. Specifically, we define all small establishments that did not borrow and all large firms that borrowed as defiers. Compliers are small firms that borrowed and large ones that did not. Among small establishments, defiers are on average smaller than compliers, because the majority of small non-borrowers are among the smallest establishments. Both our estimates and those of previous studies indicate that the treatment effects are greater for smaller firms, especially the smallest establishments. Thus, it is reasonable to assume that treatment effects are on average larger for defiers than for compliers among small firms. At the same time, lacking evidence to the contrary, we assume the same treatment effect for compliers and defiers among large firms. Combined with the additional assumption of the same proportion of large and small firms among compliers and defiers, this suggests that the first term in Equation (A.1) would more likely be negative, biasing our IV estimates.

D Late and Non-Participation in the PPP

In this appendix, we enumerate what we hope is a comprehensive list of plausible explanations for why some businesses either borrowed later than they would have preferred or ultimately chose not to borrow. We distinguish between factors that constrained firms' short-term ability to borrow in the early days of the pandemic from those that were more likely to persist through the end of the 2020 PPP on August 8, 2020. Short-term factors were more likely to cause delays in loan receipt, while persistent factors are those that plausibly led firms to forgo participation. Because our identification concern centers on the funding effect estimated relative to "never" borrowers, bias would result from persistent reasons, not necessarily short-term factors, unless these cause some businesses to fail. We argue that there is no compelling evidence that points to upward bias in our estimates, as biases could go both ways. On net, the bias is likely small.

A. **Short-term reasons that most likely caused delays in loan approval but not the eventual failure to borrow**

   i. Lack of banking relationships, application complexity, and administrative barriers: Community Development and Financial Institutions (CDFIs) and fintech lenders were approved to make PPP loans as of April 30, and there is ample evidence that they streamlined the process and expanded access to small businesses that had previously struggled to participate in the program. In principle, any small business that wanted to borrow should have been able to do so through one of these lenders.

   ii. Informational frictions and lack of awareness: it appears unlikely that any small business would still have been unaware of the program by August 8, 2020.

   iii. Owner illness due to COVID: with the exception of a small fraction of severe long-COVID cases and related deaths, it seems improbable that illness alone would have prevented owners from borrowing over the four-month duration of the 2020 PPP.

   iv. Our news search identified a few reports describing anecdotal cases in which owners, discouraged by an initial rejection of their applications, chose not to reapply.

   v. In addition, some instances of delayed access to the PPP in its early stages may have contributed to business closures, with potentially longer-lasting consequences. We have already acknowledged that this possibility may partly underlie the estimated effect of PPP receipt for the smallest establishments in particular.

B. **Persistent reasons that could have led to non-borrowing**

   i. According to the program's rules, business owners had to certify on their loan applications that they were adversely affected by the pandemic and needed the assistance. This implies that, to the extent some small businesses were honest and law-abiding, they would not have borrowed because they did not meet these conditions. Relative to such non-borrowers, borrowers' hypothetical non-PPP trend would almost certainly have been lower.

ii. Uncertainty concerns regarding forgiveness: more risk-averse owners may have been less equipped to navigate the COVID shock, but their caution could also have led them to build larger liquidity buffers, leaving them better positioned initially.

iii. Selection into PPP participation was not solely driven by financial distress. For example, some firms initially received PPP funds but later returned them for ethical reasons, despite being potentially eligible for forgiveness.[6] Conversely, some firms experienced unexpectedly strong performance during the pandemic and therefore did not require financial support, yet still received loans and had them forgiven.[7] Without firm-level survey data, we cannot directly tell what margin might have been at play for a given firm.

iv. Fear of government scrutiny and audit risk: if these owners were more willing to take risks pre-pandemic, they may also have been better positioned to adapt to the new environment.

v. Program design did not fit some businesses' needs: firms with cost structures that placed relatively little weight on payroll would have derived limited benefit from the PPP. As a result, some owners may have sought alternative assistance, such as the EIDL, which offered greater flexibility in allowable uses of funds. In such cases, there is no clear reason to expect that PPP non-borrowers would have been on a lower trajectory in the absence of the PPP.

vi. Apart from the reasons above, some owners may have chosen to exit following the COVID shock. Note that these exit decisions were not driven by pre-existing weakness, as our matching ensured that each PPP firm and its peer had similar pre-COVID scale and operational trends. Owners may have exited for various reasons, such as facing higher adaptation costs than observationally similar peers or making a pandemic-induced early-retirement or closing decision. Among all the reasons discussed so far, these are the only ones for which non-borrowers would clearly be expected to have a lower post-COVID trajectory than borrowers even in the absence of the PPP. For example, older owners might have been less interested in growing the business absent the shock. Our news search identified only a couple of such cases within a large volume of PPP reporting, suggesting that they likely represent a very small share of non-borrowers.

[6]Heard on Morning Edition. "Why did some companies repay PPP loans that could have been forgiven?" *NPR*, 2024, January 10. https://www.npr.org/2024/01/10/1223890080/why-did-some-companies-repay-ppp-loans-that-could-have-been-forgiven.

[7]Morgenson, Gretchen. "Some Firms Thrived During Covid and Then Got Their PPP Covid Relief Loans Forgiven." *NBC News*, 18 Nov. 2021. https://www.nbcnews.com/news/firms-thrived-covid-got-ppp-covid-relief-loans-forgiven-rcna5697.

Table A.1: Determinants of the Number of PPP Loans Delayed, April 16–26, 2020

	All	Urban	Smaller Urban	Rural
Cum. COVID-19 Cases per bil. up to 4/15/2020	–0.017	0.035*	0.044**	–0.031
	(0.023)	(0.018)	(0.018)	(0.026)
Cum. COVID-19 Deaths per bil. up to 4/15/2020	0.108***	0.041	0.014	0.200***
	(0.031)	(0.039)	(0.038)	(0.038)
Share of days in lockdown (pre-4/17/2020)	0.004	–0.034	–0.043	0.018
	(0.030)	(0.032)	(0.032)	(0.053)
Share of days in lockdown (4/17–4/30/2020)	0.066	0.013	0.020	0.102**
	(0.042)	(0.134)	(0.135)	(0.040)
Share of Emp. in Essential Industries	0.021	–0.183	–0.199	0.164
	(0.156)	(0.261)	(0.260)	(0.190)
Share of Emp. in Impacted Industries	–0.090*	–0.110	–0.106	–0.065
	(0.050)	(0.077)	(0.077)	(0.063)
Rural County Dummy	0.009			
	(0.006)			
Most Populous County (Top 1%)	0.023			
	(0.018)			
Ln Residential Population	–0.015***	–0.013***	–0.014***	–0.017***
	(0.004)	(0.004)	(0.004)	(0.005)
Commuter to Residential Population Ratio	–0.053**	–0.030	–0.026	–0.078**
	(0.020)	(0.022)	(0.023)	(0.038)
Ln Median Family Income	0.028*	0.014	0.016	0.047
	(0.016)	(0.019)	(0.019)	(0.028)
Community Bank Share of Deposits	0.018*	0.001	0.001	0.021*
	(0.010)	(0.017)	(0.017)	(0.012)
Big4 Bank Share of Deposits	0.070	0.059	0.056	0.071
	(0.046)	(0.079)	(0.078)	(0.048)
Ln Bank Branch Density	0.001	–0.014	–0.014	0.010
	(0.009)	(0.011)	(0.011)	(0.012)
SBL Vol. per Small Estabs. (< 500 Emp.) (CBP 2019Q1)	–0.001***	–0.001***	–0.001***	–0.001**
	(0.000)	(0.000)	(0.000)	(0.000)
Proportion of Small Employment in 2020Q1 to 2019Q1, QWI	0.012	–0.102	–0.101	0.038
	(0.045)	(0.081)	(0.080)	(0.051)
UI Benefits Replacement Rate (Industry-Wtd.)	0.012	0.061*	0.059	–0.016
	(0.029)	(0.036)	(0.036)	(0.040)
Constant	0.385	0.926*	0.923*	0.010
	(0.334)	(0.502)	(0.507)	(0.397)
Adjusted R-squared	0.28	0.32	0.32	0.25
Observations	2644	1108	1096	1536
State FE	Yes	Yes	Yes	Yes

Notes: Results from regressions of the share delayed on selected county-level characteristics. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Standard errors clustered at the state level in parentheses.

Table A.2: Determinants of Lending Delay, April 2020. CBSA Sample

	All	All	Smaller by Population	Smaller by Density
Cum. COVID-19 Cases per billion up to 4/15/2020	–0.037	–0.038	–0.039	–0.038
	(0.040)	(0.040)	(0.041)	(0.041)
Cum. COVID-19 Deaths per billion up to 4/15/2020	0.836	0.733	0.729	0.738
	(0.534)	(0.544)	(1.091)	(1.110)
Share of days in lockdown (pre-04/17/2020)	–0.033	–0.031	–0.037	–0.030
	(0.064)	(0.063)	(0.066)	(0.065)
Share of days in lockdown (04/17 and 04/30/2020)	0.102	0.105	0.103	0.106
	(0.139)	(0.139)	(0.137)	(0.139)
Share of essential empl	–0.206	–0.199	–0.201	–0.199
	(0.298)	(0.300)	(0.296)	(0.297)
Share of Employment - Impacted Industries	–0.075	–0.073	–0.074	–0.069
	(0.087)	(0.087)	(0.087)	(0.087)
Share of Work Done Remotely	–0.024	–0.029	–0.027	–0.032
	(0.020)	(0.021)	(0.023)	(0.023)
Share of Minors	0.054*	0.059*	0.060	0.063*
	(0.032)	(0.034)	(0.036)	(0.037)
Ln(Residential Population)	–0.016**	–0.016**	–0.017**	–0.016**
	(0.007)	(0.007)	(0.008)	(0.007)
Commuter to Residential Population Ratio	0.098	0.102	0.099	0.104
	(0.081)	(0.082)	(0.083)	(0.083)
Most Populous CBSA		0.050**		0.052**
		(0.021)		(0.021)
Most Dense CBSA		–0.002	–0.002	
		(0.024)	(0.024)	
Most Populous × Most Dense CBSA		0.028		
Ln Median Family Income	0.030	0.034	0.035	0.033
	(0.054)	(0.054)	(0.054)	(0.055)
Community Bank Share of Deposits	0.036	0.035	0.035	0.034
	(0.028)	(0.028)	(0.028)	(0.028)
Big4 Bank Share of Deposits	0.051	0.048	0.048	0.046
	(0.042)	(0.044)	(0.045)	(0.045)
Ln Bank Branch Density	–0.012	–0.011	–0.011	–0.011
	(0.023)	(0.023)	(0.023)	(0.023)
SBL Volume per Estab< 500 (QCEW 2019 Avg)	–0.002***	–0.002***	–0.002***	–0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Ratio of s Small (Under 500) Employment in 2020Q1 to 2019Q1, QWI	0.272*	0.274*	0.273*	0.275*
	(0.149)	(0.150)	(0.150)	(0.150)
UB replacement rate, industry weighted	0.053	0.054	0.056	0.053
	(0.074)	(0.075)	(0.076)	(0.076)
		(0.041)		
Constant	0.137	0.079	0.078	0.080
	(0.732)	(0.735)	(0.739)	(0.741)
Adjusted R-squared	0.14	0.14	0.14	0.14
Observations	916	916	906	906

Notes: Results from regressions of the share delayed on selected CBSA-level characteristics. "Smaller by Population" refers to CBSAs excluding those in the top 1 percent by population; and "Smaller by Density" refers to CBSAs excluding those in the top 1 percent by population density. Standard errors clustered at the state level in parentheses.

Table A.3: Which Controls Drive the Effects of PPP Share Delayed on Total Private Employment? All Counties, QCEW

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Level						$\Delta E/P$					
Apr 2020 × Share Delayed	–0.21	–0.15	–0.16	–0.11	0.01	–0.56*	3.59***	2.12***	1.83***	1.69***	1.50***	1.06***
	(0.56)	(0.47)	(0.47)	(0.40)	(0.38)	(0.28)	(0.62)	(0.44)	(0.42)	(0.37)	(0.38)	(0.37)
May 2020 × Share Delayed	–1.19**	–0.79	–0.60	–0.46	–0.52	–0.75**	2.90***	1.36***	1.19***	1.16***	1.29***	1.04***
	(0.58)	(0.48)	(0.46)	(0.37)	(0.36)	(0.29)	(0.54)	(0.37)	(0.35)	(0.32)	(0.33)	(0.34)
Jun 2020 × Share Delayed	–1.86**	–1.19*	–0.85	–0.66*	–0.73*	–0.79***	1.64**	0.17	0.09	0.13	0.99***	0.95***
	(0.72)	(0.60)	(0.54)	(0.39)	(0.37)	(0.29)	(0.69)	(0.74)	(0.80)	(0.77)	(0.34)	(0.35)
Jul 2020 × Share Delayed	–2.32***	–1.48**	–1.11**	–0.95**	–0.99**	–0.87***	0.77	–0.74	–0.96	–0.90	1.01***	1.01***
	(0.76)	(0.58)	(0.51)	(0.38)	(0.37)	(0.29)	(1.30)	(1.69)	(1.80)	(1.76)	(0.34)	(0.37)
Aug 2020 × Share Delayed	–2.34***	–1.46**	–1.08**	–0.89**	–1.04***	–0.81***	1.39***	0.29	0.23	0.28	0.76**	0.75**
	(0.75)	(0.56)	(0.49)	(0.36)	(0.35)	(0.29)	(0.45)	(0.44)	(0.46)	(0.44)	(0.33)	(0.35)
Sept 2020 × Share Delayed	–2.18***	–1.32**	–0.95**	–0.77**	–0.91***	–0.72***	1.37***	0.54	0.49	0.52*	0.55*	0.65*
	(0.69)	(0.52)	(0.46)	(0.34)	(0.33)	(0.28)	(0.41)	(0.33)	(0.30)	(0.29)	(0.31)	(0.33)
Oct 2020 × Share Delayed	–1.81***	–1.07**	–0.72*	–0.51	–0.64**	–0.52**	1.12***	0.58	0.56	0.56	0.39	0.48
	(0.58)	(0.46)	(0.41)	(0.31)	(0.30)	(0.25)	(0.41)	(0.42)	(0.39)	(0.39)	(0.31)	(0.32)
Nov 2020 × Share Delayed	–1.61***	–0.94**	–0.61	–0.46	–0.59*	–0.43*	0.82*	0.52	0.57	0.56	0.35	0.50*
	(0.54)	(0.46)	(0.40)	(0.32)	(0.31)	(0.26)	(0.42)	(0.46)	(0.42)	(0.43)	(0.29)	(0.30)
Dec 2020 × Share Delayed	–1.73***	–1.03**	–0.67**	–0.57**	–0.69**	–0.41	0.89**	0.61	0.63	0.56	0.36	0.50*
	(0.51)	(0.39)	(0.33)	(0.27)	(0.26)	(0.27)	(0.40)	(0.46)	(0.43)	(0.41)	(0.28)	(0.27)
Jan 2021 × Share Delayed	–1.97***	–1.17***	–0.77**	–0.65**	–0.77***	–0.50*	0.90*	0.60	0.69	0.65	0.38	0.56*
	(0.61)	(0.43)	(0.36)	(0.31)	(0.29)	(0.29)	(0.47)	(0.54)	(0.51)	(0.48)	(0.39)	(0.30)
Feb 2021 × Share Delayed	–1.42**	–0.84*	–0.50	–0.39	–0.48	–0.36	0.59	0.42	0.53	0.50	0.24	0.36
	(0.53)	(0.47)	(0.39)	(0.32)	(0.32)	(0.26)	(0.49)	(0.56)	(0.55)	(0.54)	(0.48)	(0.35)
Mar 2021 × Share Delayed	–1.47***	–0.87*	–0.53	–0.39	–0.45	–0.39	0.51	0.27	0.35	0.35	0.09	0.14
	(0.51)	(0.44)	(0.37)	(0.30)	(0.29)	(0.25)	(0.54)	(0.66)	(0.65)	(0.64)	(0.62)	(0.48)
Apr 2021 × Share Delayed	–1.45***	–0.85*	–0.53	–0.31	–0.35	–0.37	0.29	–0.01	–0.02	0.01	0.06	0.14
	(0.52)	(0.46)	(0.40)	(0.34)	(0.32)	(0.25)	(0.55)	(0.59)	(0.60)	(0.60)	(0.70)	(0.52)
May 2021 × Share Delayed	–1.45**	–0.86*	–0.53	–0.29	–0.33	–0.35	0.40	–0.00	–0.06	0.01	0.01	0.14
	(0.56)	(0.50)	(0.44)	(0.37)	(0.36)	(0.25)	(0.67)	(0.69)	(0.70)	(0.72)	(0.83)	(0.60)
Jun 2021 × Share Delayed	–1.65***	–0.98**	–0.64	–0.40	–0.43	–0.38	0.35	–0.32	–0.44	–0.33	0.12	0.25
	(0.53)	(0.46)	(0.40)	(0.33)	(0.31)	(0.25)	(0.80)	(0.85)	(0.87)	(0.89)	(0.96)	(0.69)
Jul 2021 × Share Delayed	–1.54**	–0.96*	–0.66	–0.38	–0.42	–0.44*	–0.57	–1.48	–1.68	–1.56	0.08	0.12
	(0.58)	(0.53)	(0.47)	(0.38)	(0.39)	(0.26)	(1.24)	(1.61)	(1.67)	(1.65)	(0.94)	(0.73)
Aug 2021 × Share Delayed	–1.28**	–0.81	–0.54	–0.27	–0.33	–0.40	0.27	–0.22	–0.33	–0.24	0.14	0.17
	(0.59)	(0.55)	(0.49)	(0.40)	(0.40)	(0.25)	(0.75)	(0.77)	(0.79)	(0.83)	(0.88)	(0.71)
Sept 2021 × Share Delayed	–0.95*	–0.53	–0.31	–0.08	–0.12	–0.23	0.32	0.02	–0.04	0.01	–0.12	–0.07
	(0.50)	(0.47)	(0.42)	(0.36)	(0.35)	(0.23)	(0.88)	(0.91)	(0.95)	(1.00)	(1.05)	(0.87)
Oct 2021 × Share Delayed	–0.58	–0.26	–0.06	0.15	0.07	–0.11	–0.11	–0.13	–0.27	–0.24	–0.56	–0.65
	(0.47)	(0.44)	(0.40)	(0.37)	(0.37)	(0.23)	(0.92)	(0.99)	(1.06)	(1.10)	(1.13)	(1.03)
Nov 2021 × Share Delayed	–0.41	–0.13	0.05	0.24	0.18	–0.02	–0.27	–0.04	–0.16	–0.16	–0.57	–0.64
	(0.43)	(0.41)	(0.37)	(0.36)	(0.37)	(0.23)	(0.87)	(0.98)	(1.05)	(1.08)	(1.06)	(1.02)
Dec 2021 × Share Delayed	–0.37	–0.13	0.07	0.20	0.16	0.00	–0.24	0.11	–0.03	–0.04	–0.50	–0.59
	(0.39)	(0.36)	(0.33)	(0.32)	(0.33)	(0.23)	(0.83)	(0.92)	(0.99)	(1.02)	(0.96)	(0.97)
Within R-squared	1.00	1.00	1.00	1.00	1.00	0.87	0.05	0.14	0.15	0.16	0.16	0.12
Observations	74,400	74,400	74,400	74,400	70,936	70,936	74,400	74,400	74,400	74,400	70,936	70,936
State, Time FE, Lag LHS	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	No
County FE, State by Time FE, Lag LHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Financial Pre-Conditions	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
COVID-19 Controls	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
CARES Act Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes

Notes: Standard errors, in parentheses, are clustered at the state level in columns (1)–(5) and (7)–(11) and at the county level in columns (6) and (12). Employment for columns (1)–(6) is measured in thousands. Lags of LHS variables are computed using 2019 employment as of December 2019. **Demographic controls**: median family income, share of minors, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation; **Financial Pre-Conditions controls**: community-bank share of deposits, largest four banks' share of deposits, bank branch density, and 2019 small-business-loan volume per small establishment; **COVID-19 controls**: cumulative COVID-19 cases and deaths per population, Stringency index, share of days in April 2020 in early lockdown, share of work done remotely, share of employment in essential industries, and share of employment in most impacted industries **CARES Act controls**: industry-employment-share-weighted UI benefits replacement rate relative to its March 2020 level, and rebates ("stimulus checks") per capita. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). The reference period is February of 2020.

Table A.4: Effects of Share of PPP Loans Delayed on Total Private Employment
CBSA Sample, QCEW

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Level		Log		E/P		$\Delta E/P$	
Apr 2020 × Share Delayed	0.08	0.43	0.06**	0.05***	2.82***	2.25***	3.30***	3.10***
	(2.47)	(3.11)	(0.02)	(0.02)	(0.98)	(0.79)	(1.02)	(0.95)
May 2020 × Share Delayed	–1.95	–4.11	0.04**	0.04**	2.35***	1.75**	2.80***	2.69***
	(2.76)	(3.46)	(0.02)	(0.02)	(0.87)	(0.71)	(0.89)	(0.82)
Jun 2020 × Share Delayed	–2.90	–4.96	0.04**	0.04**	2.47***	2.00**	2.86***	2.81***
	(3.19)	(3.87)	(0.02)	(0.02)	(0.88)	(0.77)	(0.89)	(0.86)
Jul 2020 × Share Delayed	–2.85	–3.88	0.04**	0.04**	2.39**	2.09**	2.75***	2.84***
	(2.82)	(3.34)	(0.02)	(0.02)	(0.92)	(0.87)	(0.93)	(0.94)
Aug 2020 × Share Delayed	–2.72	–2.95	0.03	0.03*	1.75**	1.50*	2.10**	2.24**
	(2.69)	(2.97)	(0.02)	(0.02)	(0.83)	(0.79)	(0.86)	(0.86)
Sept 2020 × Share Delayed	–2.39	–2.55	0.02	0.02	1.34*	0.97	1.70**	1.71**
	(2.48)	(2.62)	(0.02)	(0.02)	(0.80)	(0.72)	(0.82)	(0.78)
Oct 2020 × Share Delayed	–1.79	–1.51	0.02	0.02	1.23*	0.78	1.57**	1.52**
	(2.22)	(2.37)	(0.02)	(0.01)	(0.67)	(0.58)	(0.70)	(0.67)
Nov 2020 × Share Delayed	–1.81	–1.38	0.02	0.02	0.89	0.55	1.22*	1.21*
	(2.12)	(2.43)	(0.01)	(0.01)	(0.59)	(0.54)	(0.62)	(0.62)
Dec 2020 × Share Delayed	–2.07	–2.23	0.01	0.01	0.56	0.31	0.85	0.82
	(1.95)	(2.48)	(0.01)	(0.01)	(0.62)	(0.61)	(0.65)	(0.66)
Jan 2021 × Share Delayed	–2.63	–2.13	0.01	0.00	0.57	0.37	0.88	0.91
	(2.19)	(2.95)	(0.02)	(0.02)	(0.72)	(0.69)	(0.73)	(0.73)
Feb 2021 × Share Delayed	–2.03	–1.69	0.01	0.01	0.74	0.57	1.04	1.10
	(2.24)	(3.13)	(0.02)	(0.02)	(0.71)	(0.68)	(0.72)	(0.73)
Mar 2021 × Share Delayed	–1.94	0.11	0.02	0.02	0.94	0.86	1.23*	1.36*
	(2.12)	(2.97)	(0.02)	(0.02)	(0.68)	(0.63)	(0.68)	(0.69)
Apr 2021 × Share Delayed	–1.74	–0.19	0.01	0.01	0.88	0.83	1.18*	1.32*
	(2.05)	(2.79)	(0.02)	(0.02)	(0.66)	(0.61)	(0.66)	(0.68)
May 2021 × Share Delayed	–1.65	–0.58	0.02	0.02	0.96	0.86	1.28*	1.38*
	(2.13)	(2.74)	(0.02)	(0.02)	(0.71)	(0.67)	(0.71)	(0.73)
Jun 2021 × Share Delayed	–1.41	–0.41	0.02	0.03	1.36*	1.38*	1.63**	1.77**
	(2.03)	(2.54)	(0.02)	(0.02)	(0.81)	(0.80)	(0.81)	(0.84)
Jul 2021 × Share Delayed	–1.33	–0.55	0.03	0.04	1.97*	2.04*	2.22**	2.38**
	(2.20)	(2.70)	(0.02)	(0.02)	(1.10)	(1.05)	(1.08)	(1.05)
Aug 2021 × Share Delayed	–1.32	–0.50	0.03	0.03	1.52	1.58	1.78*	1.93*
	(2.26)	(2.66)	(0.02)	(0.02)	(1.03)	(0.98)	(1.02)	(0.98)
Sept 2021 × Share Delayed	–0.73	0.06	0.03	0.03	1.38	1.36	1.66*	1.74*
	(1.98)	(2.44)	(0.02)	(0.02)	(0.95)	(0.88)	(0.94)	(0.90)
Oct 2021 × Share Delayed	0.03	0.49	0.03	0.03	1.39*	1.30*	1.65**	1.66**
	(1.81)	(2.41)	(0.02)	(0.02)	(0.81)	(0.74)	(0.80)	(0.76)
Nov 2021 × Share Delayed	0.26	0.51	0.03*	0.03*	1.11*	1.09*	1.36**	1.38**
	(1.71)	(2.26)	(0.01)	(0.01)	(0.63)	(0.60)	(0.64)	(0.62)
Dec 2021 × Share Delayed	0.34	0.16	0.02	0.02	0.84	0.98	1.04*	1.16*
	(1.62)	(2.12)	(0.01)	(0.01)	(0.58)	(0.60)	(0.59)	(0.60)
Avg. LHS	122.05	122.05	10.35	10.35	39.82	39.82	–1.97	–1.97
Share Delayed	0.44	0.44	0.44	0.44	0.44	0.44	0.44	0.44
Within R-squared	1.00	1.00	1.00	1.00	0.95	0.95	0.13	0.16
Observations	21,984	21,984	21,984	21,984	21,984	21,984	21,984	21,984
Pre-conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Results from estimating Equation (1). Standard errors clustered at the state level in parentheses. Employment in columns (1) and (2) is in thousands. All regressions control for state, month-year fixed effects, and a lag of the dependent variable (as of December 2019), in addition to preexisting conditions and COVID-19 controls. **Preexisting conditions**: median family income, share of minors, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, community-bank share of deposits, largest four banks' share of deposits, bank branch density, and 2019 small-business-loan volume per small establishment. **COVID-19 Controls**: cumulative COVID-19 cases and deaths per population, share of days in April 2020 in early lockdown, share of work done remotely, share of employment in essential industries, and share of employment in most impacted industries. NAICS codes for most affected industries: 44-45, 61, 71, 72, and 81. Share Delayed is the share (by volume) of PPP loans delayed, as defined in Equation (2), computed at the CBSA level. The reference period is the first quarter of 2020.

Table A.5: Effects of Share Delayed on Stable Employment
Total and Large Establishments, All Counties, QWI

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Employment				Employment 500+			
	Level	Log	E/P	$\Delta E/P$	Level	Log	E/P	$\Delta E/P$
2020Q2 × Share Delayed	–0.44*	0.03**	0.39	0.69***	–0.18*	0.03	0.27	0.36***
	(0.24)	(0.01)	(0.25)	(0.17)	(0.10)	(0.03)	(0.17)	(0.13)
2020Q3 × Share Delayed	–0.46	0.02*	0.41*	0.77***	–0.20**	0.03	0.30*	0.44***
	(0.29)	(0.01)	(0.24)	(0.22)	(0.10)	(0.03)	(0.17)	(0.15)
2020Q4 × Share Delayed	–0.44*	0.01	0.26	0.60**	–0.24**	0.02	0.20	0.34*
	(0.24)	(0.01)	(0.28)	(0.23)	(0.10)	(0.03)	(0.20)	(0.17)
2021Q1 × Share Delayed	–0.36	0.02	0.24	0.53*	–0.19	0.02	0.10	0.21
	(0.26)	(0.01)	(0.31)	(0.28)	(0.12)	(0.03)	(0.26)	(0.23)
2021Q2 × Share Delayed	–0.13	0.02	0.16	0.59*	–0.05	0.01	0.08	0.22
	(0.22)	(0.01)	(0.33)	(0.31)	(0.10)	(0.04)	(0.34)	(0.31)
2021Q3 × Share Delayed	–0.15	0.00	0.04	0.37	0.05	–0.01	–0.07	0.08
	(0.29)	(0.01)	(0.43)	(0.46)	(0.12)	(0.04)	(0.45)	(0.42)
2021Q4 × Share Delayed	0.21	0.00	–0.31	0.06	0.16	–0.00	–0.31	–0.14
	(0.30)	(0.02)	(0.56)	(0.59)	(0.15)	(0.05)	(0.62)	(0.58)
Avg. LHS	33.46	8.74	28.95	–1.22	17.80	7.70	12.25	–0.44
Share Delayed	0.48	0.48	0.48	0.48	0.47	0.47	0.47	0.47
Observations	24,525	24,525	24,525	24,525	24,229	24,229	24,229	24,183
State FE, Time FE, Lag LHS	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

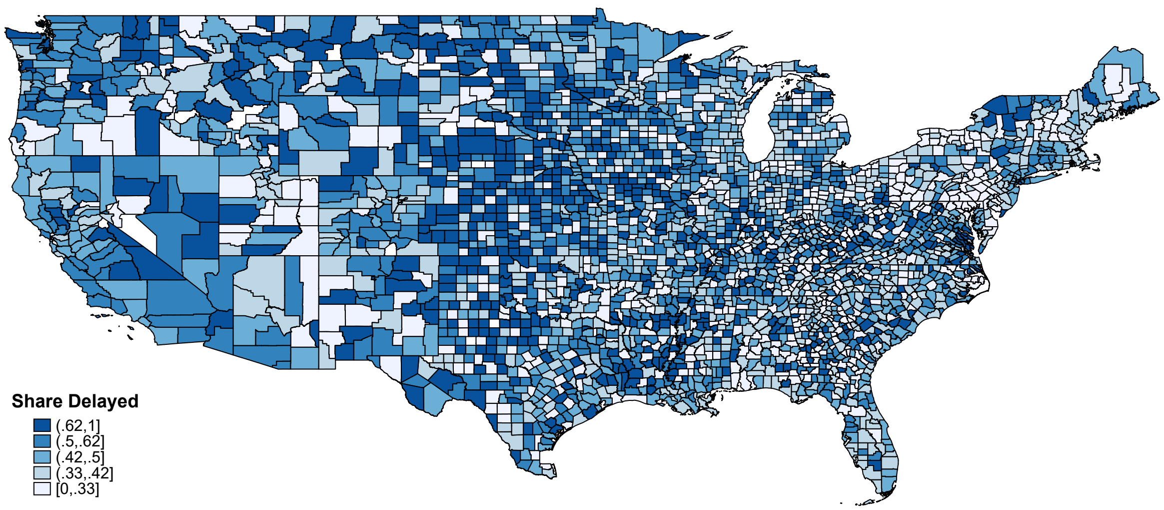
Notes: Results from estimating Equation (1). Standard errors clustered at the state level in parentheses. All regressions control for state, quarter-year fixed effects, and a lag of the dependent variable (last quarter of 2019), in addition to preexisting conditions and COVID-19 controls. **Preexisting conditions**: median family income, share of minors, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, community-bank share of deposits, largest four banks' share of deposits, bank branch density, and 2019 small-business-loan volume per small establishment. **COVID-19 controls**: cumulative COVID-19 cases and deaths per population, share of days in April 2020 in early lockdown, share of remote jobs, share of employment in essential industries, and share of employment in most impacted industries. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). The reference period is the first quarter of 2020.

Table A.6: Summary Statistics by PPP-Peer Firm Distance: Advan Sample

	N(Pairs)	Mean	P25	Median	P75
Pair Distance [1, 5) Miles					
PPP Business Avg. Weekly Visits	26,466	46.40	9.77	24.69	53.08
% Diff. in Visits: PPP vs. non-PPP Business	26,466	-0.00	-2.45	0.00	2.44
Distance (Miles): PPP vs. non-PPP Business	26,466	2.95	1.98	2.91	3.92
PPP Loan Size ($1,000)	26,466	145.92	22.39	50.24	118.70
No. of Jobs Saved	26,466	19.06	4.00	8.00	17.00
Pair Distance [5, 10) Miles					
PPP Business Avg. Weekly Visits	25,820	45.99	8.77	23.31	52.08
% Diff. in Visits: PPP vs. non-PPP Business	25,820	0.01	-2.42	0.00	2.45
Distance (Miles): PPP vs. non-PPP Business	25,820	7.37	6.13	7.30	8.61
PPP Loan Size ($1,000)	25,820	141.81	22.15	49.70	118.00
No. of Jobs Saved	25,820	19.06	4.00	8.00	18.00
Pair Distance [10, 15) Miles					
PPP Business Avg. Weekly Visits	17,722	44.00	8.38	22.00	49.46
% Diff. in Visits: PPP vs. non-PPP Business	17,722	0.04	-2.41	0.00	2.49
Distance (Miles): PPP vs. non-PPP Business	17,722	12.29	11.06	12.20	13.48
PPP Loan Size ($1,000)	17,722	141.07	22.24	49.60	114.60
No. of Jobs Saved	17,722	18.99	4.00	8.00	17.00
Pair Distance [15, 50) Miles					
PPP Business Avg. Weekly Visits	21,594	39.96	7.54	20.23	45.08
% Diff. in Visits: PPP vs. non-PPP Business	21,594	0.02	-2.44	0.00	2.51
Distance (Miles): PPP vs. non-PPP Business	21,594	22.27	17.19	20.25	25.27
PPP Loan Size ($1,000)	21,594	133.96	21.31	48.06	110.58
No. of Jobs Saved	21,594	18.16	4.00	8.00	17.00

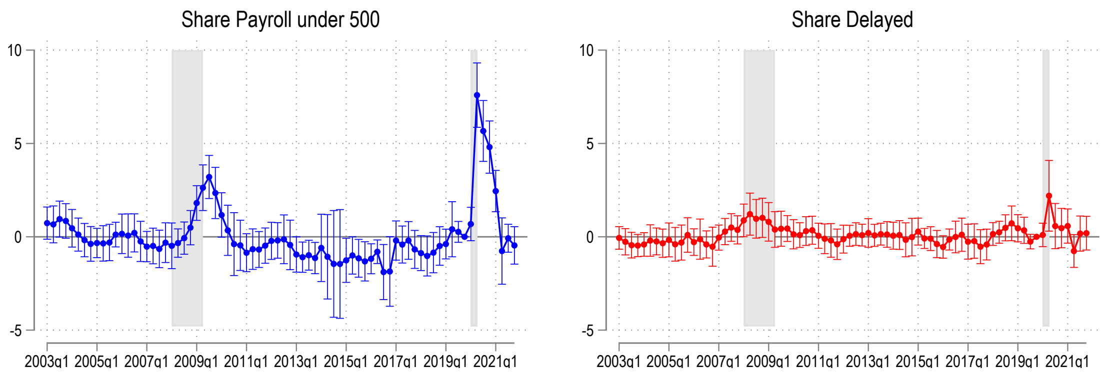
Notes: Each pair of firms operates in the same 6-digit NAICS industry and is located in the same county. The PPP firm and its peer are located in difference Census block groups within different distance bands, as indicated in each panel. Pre-COVID visits are averages for the months of December 2019 and February 2020.

Figure A.1: Map of Share of PPP Volume Delayed by County

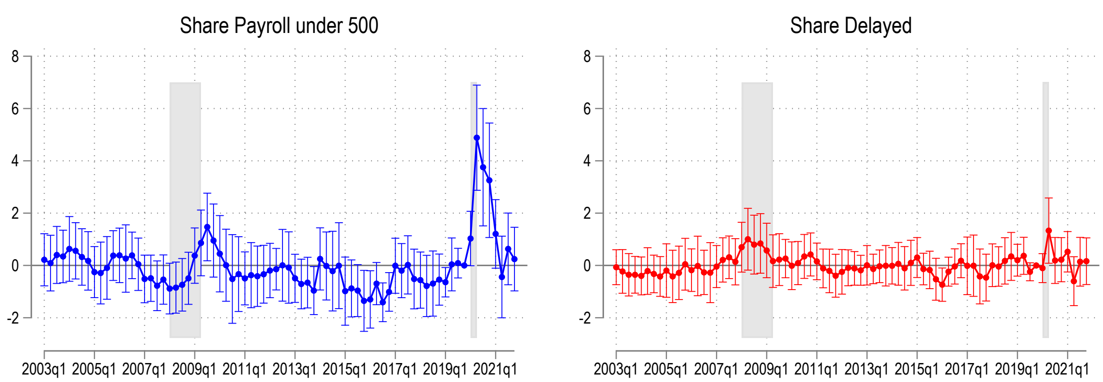


Notes: This map depicts, by quintile, the share of PPP volume delayed across all US counties.
Source: Small Business Administration.

Figure A.2: Total Employment Evolution based on 2019 payroll and DK delay
All Establishments, QCEW



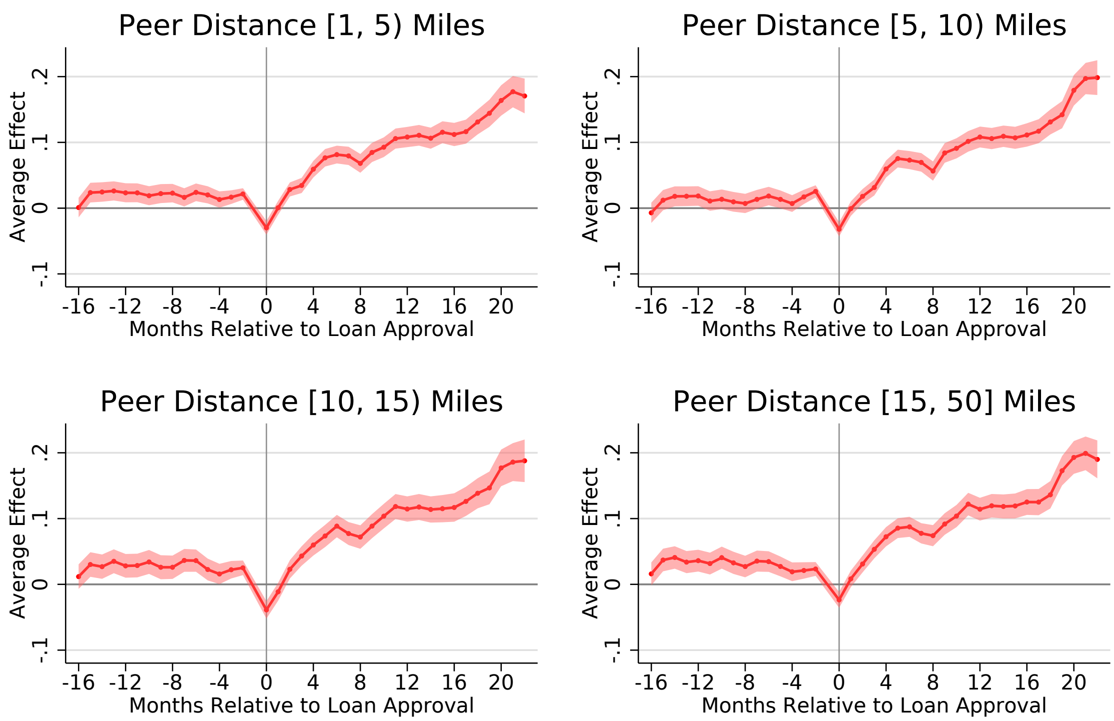
(a) Basic Controls



(b) All Controls

Notes: Let $e_t = \frac{E_t}{P_t} \times 100$ and $\Delta e_t = e_t - e_{t-4}$. The figures plot β_t and γ_t from the regression $\Delta e_t = \alpha + \beta_t \, \text{share}_{2019} + \gamma_t \, \text{delay} + \zeta_t \, L4.\Delta e_t + X_t \Pi_t + \epsilon_t$. Regressions also include state and quarter-year fixed effects. The bottom panel regressions include all controls in our baseline regressions (including banking variables) adjusted to the relevant time period whenever possible. The top panel excludes $X_t \Pi_t$.

Figure A.3: Effect of PPP on Visits
Estimates by Distance between Advan Matched Firm Pairs



Notes: The panels plot the μ_l coefficients from regressions following Equation (6) for log visits, defined as the log of visit counts plus one. 2020 PPP recipient firms are matched to non-PPP firms that operate in the same 6-digit NAICS industry, have similar visit volumes (within 5%) between December 2019 and February 2020, and located in the same county but at the distances indicated in each panel's subtitle. Shaded areas represent 95 percent confidence intervals, with standard errors clustered by firm pair.