Cities Disrupted: The Diverse Impact of the PPP and Other Pandemic Support Programs*

Olga Gorbachev
University of Delaware

Maria Luengo-Prado
ICAE

J. Christina Wang
Federal Reserve Bank of Boston

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Abstract

We examine the impact of three key public support programs—the Paycheck Protection Program (PPP), expanded unemployment benefits, and tax rebates—on the economic recovery following the COVID-19 outbreak. We identify substantial heterogeneity in the effects of a 10-day PPP funding delay, with pronounced and enduring adverse effects concentrated in a few most populous urban areas, likely due to their structural vulnerability to pandemic-induced disruptions. The receipt of PPP funds nonetheless supported business recovery and survival, irrespective of the timing of disbursement. By comparison, the other two support programs we investigate were quantitatively more important for the overall path of the recovery.

Keywords: COVID-19, Paycheck Protection Program, CARES Act, small business credit, employment.
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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 the fiscal package was the Paycheck Protection Program (PPP), which provided loans that were essentially grants to most small businesses whose operations were disrupted by the pandemic.[1] The stated goal of the PPP was to enable small businesses to retain workers despite having to curtail operations or shut down entirely. This study focuses on understanding the likely mechanisms through which the PPP affected the dynamics of the recovery after the initial acute phase of the pandemic, and examining its interplay with other components of the CARES Act. We confirm that PPP funding proved instrumental for business survival and recovery. More importantly, we find significant heterogeneity in the PPP's effects across localities, especially between the most populous metro areas and the rest of the US. In addition, other CARES Act assistance programs (namely, expanded unemployment insurance payments and tax rebates) emerge as comparatively more impactful contributors to the overall recovery.

The literature on PPP's impact on small businesses' operations and local economic recovery has shown that it cushioned pandemic-related employment losses, albeit with a potentially high cost to taxpayers due to a lack of targeting.[2] Importantly, Granja et al. (2022), Balyuk, Prabhala, and Puri (2020), and Li and Strahan (2021) show that firms with strong bank ties received better access to PPP loans, especially in the first phase of the program (before April 17, 2020).[3] Doniger and Kay (2023) (DK throughout the rest of the paper) make original use of a 10-day pause of the PPP as plausibly exogenous variation in loan timing to identify the causal effect of a delay in PPP funding on local

[1] With only a few exceptions, "small" refers to businesses with no more than 500 employees.

[2] For example, Granja et al. (2022) estimate that the PPP saved jobs at a cost per job year of at least $175,000.

[3] In addition to the studies cited explicitly in this section, a partial list of other studies includes Autor et al. (2022), Faulkender, Jackman, and Miran (2023), Chetty et al. (2020), and Hubbard and Strain (2020).

employment.[4] DK argue that the funding pause interrupted the queue of applicants randomly (resulting in a similar borrower composition just before and just after the 10-day delay in every location), while the share of delayed loans varied significantly across CBSAs.[5] Nontrivial harm from the brief delay during the critical juncture of the COVID-19-induced lockdowns would compel numerous small businesses to close, at least temporarily, leading to a substantial downturn or even a complete cessation of revenues. Given the typical limited cash reserves of small businesses, the closings would have a particularly impactful effect on their employment levels. Paradoxically, DK's find that the adverse consequences of the funding delay were predominantly observed among the self-employed, suggesting a misalignment between the actual outcomes of the PPP and its stated goal of preserving employment relationships.

To gauge the exogenous impact of PPP funding, we adopt DK's approach and use the share of PPP funds delayed due to the 10-day legislative funding pause, termed as the "share delayed" in all subsequent analyses. Utilizing monthly data from the Quarterly Census of Employment and Wages (QCEW) and quarterly data from the Quarterly Workforce Indicators (QWI), we confirm that the delay had an adverse effect on local employment, extending until October 2020 or 2020:Q3, depending on the dataset employed. However, intriguingly, we note a negative correlation between the share delayed and local employment across the establishment size distribution, encompassing even establishments with 500 or more employees that were ineligible for PPP loans. Furthermore, the statistical significance of the correlation between the share delayed and employment in QCEW and QWI data is predominantly driven by the top 1 percent most populous urban counties.

This heterogeneous effect of the PPP delay on employment likely stems from the highly contagious nature of COVID-19 and its varying impact on major metropolitan areas. The implementation of public health measures to curb the spread of COVID-

[4] Lenders had to pause for 10 days 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.

[5] DK use Current Population Survey (CPS) data and conduct their analysis at the individual level. They match CBSA-level measures of the share of PPP loans delayed to each CPS respondent based on location identifiers available in the CPS interviews.

19 in densely populated urban centers probably resulted in more severe disruptions to commercial activities at the pandemic's outset, with effects lasting longer. The initial significant disturbance in these urban regions likely contributed to a heightened demand for PPP loans, slower processing, and consequently, a higher proportion of delayed loans. Additionally, these areas grappled with prolonged negative consequences due to a significant shift towards remote work. This structural change impeded the recovery of local employment, particularly for jobs supporting office workers. In essence, the increased share of delayed loans reflects the influence of unaccounted risk factors associated with being a major urban center. Our analysis indeed affirms the validity of this mechanism.

Several other studies have employed the share delayed metric to investigate the causal impact of the PPP funding delay. For instance, using Homebase data, Kurmann, Lalé, and Ta (2022) find that, in terms of employment, small businesses in four of the hardest-hit service sectors initially contracted more severely during the pandemic compared to larger businesses in those sectors. However, they also experienced a more robust recovery afterward. Notably, closings and reopenings accounted for the majority of the initial contraction and subsequent rebound. In contrast, our study indicates a negative but statistically insignificant impact of the share delayed on employment in those hardest-hit sectors (NAICS 71, 72, and 81) when considering delay of loans made specifically to firms operating in those sectors. Surprisingly, a more general measure of the share delayed at the county level (computed using PPP loans to all businesses in each county, regardless of activity type) reveals a larger and significantly negative impact on employment in those most impacted sectors. This finding is unexpected, as one would anticipate a greater effect from the industry-specific delay than from a more general county-level delay. Collectively, our findings on the heterogeneity of the impact of the funding delay underscore the importance of caution when interpreting the magnitude of estimates in studies exploiting the 10-day delay discontinuity as representative of the treatment effect of the PPP.

We further evaluate the relative importance of different components of the CARES Act for employment recovery. The fiscal response to COVID-19 was unprecedented in both scale and scope, with the CARES Act and subsequent legislation authorizing various types of spending and transfer payments to support businesses and households. Notably, two sets of payments to households, enhanced UI benefits and Economic Impact Payments (EIPs or tax rebates), together exceeded the PPP in total government outlay.[6] While government transfer payments stimulate consumption demand and in turn labor demand, they could also potentially reduce labor supply. Both effects would boost wages whereas the net effects on employment can be ambiguous. However, evidence suggests that the adverse effects on labor supply were modest.[7] Coombs et al. (2022) and Holzer, Hubbard, and Strain (2021) found that the early termination of enhanced UI benefits in some states only mildly boosted employment rates. Similarly, Coibion, Gorodnichenko, and Weber (2020) found no meaningful impact on labor supply from stimulus payments, except for some unemployed individuals who intensified their job search efforts in response to transfer payments.

Our study focuses on the net impact of these programs on employment, which can be ambiguous and, more importantly, diverse across different localities. The relative strength of these programs' effects on labor supply versus labor demand likely differs across localities, due to cross-county heterogeneity in household attributes. In particular, the different levels of urbanicity may explain the varying effects of UI benefits and stimulus checks we document. Specifically, tax rebates are found to foster employment in rural areas, while enhanced UI replacement rates might have curtailed labor supply in some urban areas. Moreover, since the amount of assistance a community received through each stimulus program may be correlated, not adequately accounting for the influence of other payments could bias the estimate of a specific program's impact. Therefore, it is valuable to assess the collective effect of these programs to provide a

[6] According to Pandemic Oversight, pandemic unemployment programs total outlays were $653 billion, the three rounds of stimulus checks totaled $814.4 billion, while total PPP outlays amounted to $792.6 billion.

[7] Baker et al. (2023) study the impact of UI payments, while Ganong et al. (2022) and Gelman and Jr (2022) also assess these stimuli's impact on employment decisions.

more comprehensive evaluation of the relative efficacy of the pandemic programs. Our comparison reveals that extra unemployment benefits (with a negative sign) and tax rebates (with a positive sign) contribute more significantly to the cross-county variation in employment recoveries than the total PPP funds received, and orders of magnitude more than the slowdown due to the PPP delay.

Our county-level employment results underscore a crucial distinction: the impact of PPP timing likely differs from the effect of PPP receipt. If we consider the policy-relevant treatment effect, defined in Heckman and Vytlacil 2001, as PPP loan receipt, then the estimated effect of loan delay is not the pertinent measure, even if the share delayed were conditionally random. To precisely isolate the effect of loan receipt from that of loan timing, we turn to firm-level data. We compare business foot traffic between PPP recipients and closely matched (based on geography and line of business) peer firms that did not receive PPP funds. Assuming that the 10-day delay impeded employment growth for many months by depriving some firms of timely liquidity, we would anticipate late borrowers (those receiving loans just after the 10-day pause) to perform worse than early borrowers (those receiving loans just before the 10-day pause). This effect should persist for the duration documented by DK. Applying either a difference-in-differences matching estimator or a staggered treatment estimator á la Sun and Abraham (2021) to firm-level data from SafeGraph (SG), we find that both early and late borrowers significantly benefited from receiving PPP loans.[8] Both borrower groups experienced notably increased visits from the second half of 2020 and significantly fewer closures (defined as showing zero visits) compared to their respective matched non-recipient peers. In contrast, the treatment-effect differential between early and late PPP borrowers is relatively small and statistically insignificant. Our estimates emphasize that receiving a PPP loan was far more crucial than receiving it slightly earlier.

The remainder of the paper is organized as follows. Section 2 describes our methodology and the data sources used in our analysis. Section 3 studies the determinants of

[8]SG records the number of visits and visitors to business locations using cell phone tracking data.

the share delayed. Section 4 presents our main empirical findings on how the timing of PPP loans affected the local employment recovery. Section 5 compares the relative roles of various pandemic-related transfer programs in the employment recovery. Section 6 illustrates the importance of PPP receipt relative to the timing of its receipt using SG data, and Section 7 concludes.

2 Empirical Design and Data

2.1 Empirical Design

To investigate the impact of PPP loans on the local economy, we start with an empirical specification analogous to that used by DK. Specifically, we estimate the following equation:

$$Y_{c,m} = \mu_c + \tau_{s,m} + \sum_{m \neq \text{March 2020}}^{M} [\beta_m \text{Share-Delayed}_c + \alpha_m P_{c,m} + \gamma_m X_{c,m} + \eta_m Y_{c,m-12}] + \epsilon_{c,m}, \tag{1}$$

where $Y_{c,m}$ is the measure of local employment of interest in area c (county or CBSA depending on the specification) and m denotes a calendar month (or quarter in some specifications). $Y_{c,m-12}$ is the 12-month lag of the dependent variable, which directly controls for its pre-condition. $X_{c,m}$ denotes a set of area-level controls, Share-Delayed$_c$ is our measure of the impact of PPP receipt, and $P_{c,m}$ denotes measures of total PPP funding receipts and other federal CARES Act programs—we carefully define all the variables used in our regressions in Section 2.2. μ_c and $\tau_{s,m}$ are area and state-by-month fixed effects, respectively. Standard errors are clustered at the c level.

Note that our regressors [Share-Delayed$_c, P_{c,m}, X_{c,m}, Y_{c,m-12}$] are allowed to influence the economic outcome $Y_{c,m}$ of interest differently in each month. The coefficients $[\beta_m, \alpha_m, \gamma_m, \eta_m]_{m \neq \text{March 2020}}^{M}$ summarize the magnitude of the dynamic effects of the various controls relative to March 2020.

The regressor Share-Delayed$_c$, 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. DK argue this measure is as good as randomly assigned, conditional on the local controls. We follow DK and define the share delayed as follows:

$$\text{Share-Delayed}_c = \frac{L_c}{L_c + E_c},$$

where L_c denote funds received late (on April 27 and 28, 2020, just after funding resumed) and E_c denote funds received early (on April 14 through 16, just before funding ran out).

2.2 Data

This section provides a brief description of the data array used in our analysis. Summary statistics are reported in Table 1, and more details are supplied in Appendix A.

Employment Counts We use the most timely high-frequency (monthly) employment data available at the county level, 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 2019:Q1 through 2021:Q3.

To be comparable to DK, we measure employment in levels, rather than logs or growth rates. Results using logs or growth rates are reported in Appendix Tables A.9-A.12. Importantly, these results show that the estimated impact of share delayed is sensitive to small specification changes. We also investigate the robustness of our results to various levels of labor market definition by aggregating county level data (the QCEW original level of aggregation) to the CBSA level, and we find broadly consistent results across different levels of geography (see Appendix Table A.8).

Since QCEW data does not provide employment by establishment size (except once annually), we use QWI data available at a quarterly frequency to supplement our study on how the PPP impacted employment at targeted small establishments.

PPP Loans and Borrowers For PPP lending, we use official data released by the Small Business Administration (SBA).[9] We use the business name and full address of each borrower to obtain a unique Placekey identifier. These identifiers then map the borrowers to their US Census county (or CBSA), which allows us to compute loan statistics at the desired level of geography, including the share of loans delayed.[10] In some specifications, we control for the total volume of funds received. In such cases, we compute, for each month, the cumulative sum of PPP receipts for each county up to that point and normalize it by total employment in small (fewer than 500 employees) establishments as reported in the QWI (using the 2019 average).

Preexisting Local Conditions Since bank relationships were an important aspect for loan underwriting in the first phase of the PPP (see Li and Strahan 2021), 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 largest four banks' shares of deposits, and the 2019 volume of small business loans (SBL) in each county (from data reported pursuant to the Community Reinvestment Act). Using 2019:Q1 County Business Patterns (CBP) data, we normalize SBL by the number of small establishments. We further control for the following pre-pandemic local demographic and economic conditions using data from the American Community Survey (ACS): population, median family income, and commuter-to-resident population ratios. We also classify counties as urban or rural using the 2013 National Center for Health Statistics classification scheme.

[9]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.

[10]Placekey is a free, universal standard identifier for any physical place. For more details, see https://docs.placekey.io/ and the corresponding white paper. Placekeys in fact allow us to narrow down borrowers' locations to census block groups (CBGs), which we employ in later analysis to match PPP recipients to non-PPP counterparts using SafeGraph data.

COVID-19–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's COVID-19 Intervention data set. To account for potential heterogeneous effects of the COVID-19 shock due to pre-pandemic industry composition, we control for the share of employees working in the most adversely affected industries (with two-digit NAICS codes 44/45, 61, 62, 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).

Additional Public Support Programs We construct industry-weighted, county-specific unemployment insurance (UI) benefit replacement rates (relative to pre-pandemic levels). We estimate UI replacement rates using UI formulae as coded in Ganong, Noel, and Vavra (2020). We use weekly wages earned by private employees in those industries and the UI weekly benefits determined by each state's UI laws, supplemented by the CARES Act's Pandemic Unemployment Compensation (PUC) payment of $600 a week through July 2020 and the subsequent $300 weekly supplemental payment extension. Our estimates account for the differential end dates in UI extensions, which occurred in some states before the federal September 6, 2021 deadline. We then compute a county-level measure as a weighted average of the industry-specific UI replacement rates using each industry's 2019 employment share in a given county as the weight. Finally, for each county, we construct a UI replacement rate relative to its March 2020 level. We also collect IRS data on county-level 2020 stimulus checks) to households. The rebates are normalized by population to derive a per capita amount.[11]

[11] County-level data on rebates for 2021 had not been released at the time of our analysis.

3 What Explains the Share of PPP Loans Delayed?

The fundamental assumption essential for the credibility of causal inferences using the share of PPP loans delayed is that the allocation of the share delayed across localities takes place in a random-like manner and is conditionally uncorrelated with any unobservable factors that might have influenced the employment recovery. This requirement is met if the interruption point of the queue of PPP applicants, determining the share of loans delayed when the initial funding ran out, is effectively random. We scrutinize this assumption at the county level by modeling the first-order (linear) relationship between the share delayed and the conditions before the onset of the delay window (on April 16, 2020). These conditions can reasonably be assumed to impact both the volume of loans delayed and the subsequent economic recovery.

The coefficients reported in Table 2 show that the share of loan volume delayed is closely related to several county attributes, especially small businesses' existing lending relationships.[12] Areas with higher volume of 2019 SBL per small establishment experienced smaller volume of delayed loans: A one-standard-deviation (SD) higher level of SBL per small establishment is associated with a 0.12 SD lower share of volume delayed.[13] Moreover, urban counties with a larger share of community banks saw more delay, likely because smaller banks were less able to meet the 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 of the potentially affected population and people's modes of interaction. On average, urban counties had a lower share of lending delay by loan volume than rural counties (0.44 versus 0.50).

[12] Qualitatively similar results based on the number of loans delayed, reported in Appendix Table A.1, and those using a CBSA-level sample are reported in Appendix Table A.7.

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

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 harder by COVID-19 suffered a greater disruption to commercial activity at the outset of the pandemic, which also impeded PPP lending. The greater demand combined with impaired supply resulted in worse delays in PPP lending. In fact, the most populous counties (the top 1 percent in terms of population), arguably the counties most disproportionately impacted by COVID-19, experienced a significantly larger delay in the volume of funding even when all the covariates are taken into account.[15] Overall, 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 can be explained by several preexisting local attributes, we include these covariates in our employment regressions and allow for their effects to influence employment *dynamically* over the sample months (that is, differently in each month), as we do with the share delayed and other controls.

4 Effects of the PPP on Employment

We now turn to the effects of the PPP on local employment. We first focus on the share delayed and later, in Section 5, we discuss the impact of total funding receipts from the PPP and other federal programs. This section concludes with a discussion on the robustness of our findings to the level of geography analyzed (county vs. CBSA), and the functional forms used (levels or logs).

Table 3 presents the coefficient estimates from Equation (1). Columns (1) through (4) show results pooling all counties together, columns (5) and (6) show results for urban counties only, columns (7) and (8) for urban counties excluding the top 1 percent most populous ones, and (9) and (10) for rural counties. All specifications include state-by-month and county fixed effects, and the additional controls included are listed

[15] Their unconditional mean of share delayed is 0.50, as high as that of rural counties.

below the estimated coefficients for the share delayed. The state-by-month fixed effects remove any state-specific time-varying differences in the pandemic's impact or policy response (such as nonpharmaceutical interventions) to the COVID-19 crisis that are not picked up by other controls. In a nutshell, we find that the impact of the funding delay on the local recovery was heterogeneous, driven primarily by the top 1 percent most populous urban counties, and not robust, likely pointing to the fact that share delayed as measured here, in DK and others, is picking up factors important for the economic recovery beyond the delay in funding.

First, when all the counties are pooled together (column 1), the share delayed is found to have a lasting impact on county-level employment, even after we control for pre-COVID-19 county attributes such as median family income, the commuter-to-residential-population ratio, urban-rural designation, banking and small-business-lending preexisting conditions, and the 12-month lag of the dependent variable. More controls are added in column (2) to account for the pandemic's impact on public health (cumulative death and case rates per million population), the containment measures adopted early on, and the likely differential vulnerability of counties to COVID-19 (pre-pandemic shares of essential employees in a given county and pre-pandemic employment shares in the industries most adversely affected by the crisis, specifically NAICS 44/45, 61, 62, 71, 72, and 81). Including these additional controls reduces the impact of the share delayed by about 20 percent.

These additional controls exerted influence through several channels. Counties that imposed lockdowns early (before April 16, 2020) likely experienced worse disruptions to commercial activity, which would have raised the demand for PPP loans while hampering the ability of banks to underwrite the loans, resulting in a higher share delayed, on average. These counties also tended to have higher population density and saw more infections and deaths early in the pandemic. Their inherent vulnerability to infectious diseases also would make the recovery process slower. Likewise, a higher share of employment in the most adversely affected industries could have impacted counties' recoveries, but the effect could have been positive or negative (the recovery could have been more rapid if the initial loss of employment was sufficiently greater, or more gradual due to slower recovery of demand for in-person services). A higher share of essential employment likely meant relatively more business activity following the outbreak (as essential businesses could remain open or reopen faster), which would show up as a more muted trajectory of employment over time.

Next, we control for the plausibly exogenous amounts of additional federal pandemic assistance received by each county: industry-weighted UI replacement rates relative to March 2020 and tax rebates per capita (column 3). UI replacement rates are fully determined by 2019 wages and thus do not depend on the actual payout of unemployment benefits and are not driven by local demand for unemployment benefits. Similarly, the tax rebates are predetermined by local characteristics of taxpayers in 2019. The cross-county variation in funding via these programs is also exogenous with respect to PPP receipt (or PPP delay), as UI payments and rebates did not take into account the volume of PPP funds already allocated to each county.[16] We find that these stimulus payments played a much more important role in the employment recovery (as will be discussed later), but the inclusion of these measures only marginally reduces the magnitude of the effect of the delay.

Finally, in column (4), we control for the total county-level amount of PPP funding per employee in small establishments received through the preceding month. Adding this last control does not change the size or the statistical significance of the coefficient on the share delayed in any meaningful way.

The Decisive Role of Large Urban Counties

Columns (5) to (10) of Table 3 present estimated coefficients for regressions of various sub-samples of counties. COVID-19 would a priori be expected to inflict greater and longer-lasting economic damage to places with a larger population and greater

[16] Additionally, we find replacement rates to be conditionally uncorrelated with the share delayed (see Table 2).

density—major urban centers. A comparison of urban, smaller urban (excluding the top 1 percent of urban counties by population), and rural counties confirms this conjecture. Even when controlling for only pre-pandemic characteristics, we find that the estimated persistent effect of the share delayed is mostly driven by the largest urban counties (compare columns 5, 7, and 9 of Table 3).[17] Note we choose a 1 percent cutoff to demonstrate that even a minimal change in sample, in accordance with the mechanism we propose, is sufficient to eliminate the effect of the share delayed. Expanding the cutoff to a larger percent (top 5 or 10 percent) only makes the point stronger (see Appendix Table A.4). The difference in the magnitude of the estimated coefficients across subgroups of urban counties is almost mechanical: The large difference in scale means that the top 1 percent most populated urban counties dominates the size of the coefficients in the pooled sample (as the LHS in these regressions is in levels). In addition, the share delayed has no significant effect on employment for rural counties.

Once we also account for COVID-19 severity and pandemic-related transfer payments (see columns 6, 8, and 10), the drag on the recovery stemming from the funding delay becomes only marginally significant through July 2020 for urban counties considered together (shrinking in magnitude by close to 40 percent) and disappears entirely for urban counties excluding the top 1 percent (compare columns 6 and 8). Most estimated coefficients on the share delayed are positive for rural counties (see column 10), likely pointing to a spurious correlation.

For the urban sample, we also estimate an alternative specification that allows for a differential effect of the share delayed for the top 1 percent most populous counties versus the rest. The top left panel of Figure 1 plots the estimated coefficients for share delayed in this specification. The lines trace out the estimated effect of a one-standard-deviation increase in the shared delayed in each sample subset, each normalized by its respective average employment level in January 2020 for an easier comparison. The

[17] The top 1 percent most populous urban counties are Maricopa County, Arizona; Los Angeles County, California; Orange County, California; Riverside County, California; San Diego County, California; Miami-Dade County, Florida; Cook County, Illinois; Kings County, New York; Queens County, New York; Dallas County, Texas; Harris County, Texas; and King County, Washington.

figure clearly shows that the effect of the share delayed is driven by the most populous counties, at a sizeable 2 percent of pre-pandemic employment in April 2020. This effect shrinks very gradually over time. For other urban counties, the share delayed plays an order of magnitude smaller, marginally significant, and negative role in the employment recovery.

Population Size, Population Density, and the Influence of Remote Work

Our previous specifications zoomed in on the top 1 percent of counties by population because we believe these counties were likely most relevant when accounting for the effect of the share delayed on employment. Not only do these counties exhibit significantly higher shares of PPP loans delayed (see Table 2), but they also dominate the magnitude of the key coefficients in employment regressions that are specified in levels. A primary candidate for the mechanism through which the pandemic had an outsized lasting toll on employment in large urban counties is a more pronounced shift to remote work in these localities. As shown in Althoff et al. (2021) and Ramani and Bloom (2021), the decline in time spent in urban office centers substantially reduced the demand for associated services and, in turn, employment in those local businesses (for example, restaurants, hotels, and dry cleaning). To the extent that the shift to remote work was a contributing factor to the slower recovery of some urban counties, these studies would suggest that urban counties with high population (and/or high population density) would be disproportionally affected.

Using Google mobility data, we confirm that population size and density are independently associated with a reduction in time spent at workplaces relative to pre-pandemic averages (see Appendix Figure A.1). Urban counties in the top 1 percent in terms of population density suffered the largest and most persistent declines in time spent at workplaces (about 40 percent on average as of September 2021), while counties in the top 1 percent in terms of overall population also suffered significantly larger declines than other urban counties (on average, 33 percent as of September 2021).[18]

[18]The top 1 percent of urban counties by population density are San Francisco County, California;

If we remove counties in the top 1 percent by population density instead of those in the top 1 percent by population size from our regressions, the explanatory power of the share delayed in the employment regressions also disappears, and in this case, even before we add other controls (see Appendix Table A.3). Thus, a correlation between the share of loans delayed and remote-work patterns could potentially account for the correlation between employment and the share of loans delayed.[19] Taken together, our findings so far are consistent with the intuition that the pandemic likely inflicted disproportionately worse and longer-lasting economic damage to business centers (in areas that either have large populations or are densely populated).

Effects on the Most Adversely Affected Industries

Next we investigate whether PPP funding delay had a stronger and longer-lasting impact on the most adversely affected contact-intensive industries, such as leisure and hospitality, because of their intrinsic vulnerability to an infectious disease. We compute the share of the volume of lending delay specifically for arts, entertainment, and recreation (NAICS 71); accommodation and food services (NAICS 72); and other services except public administration (NAICS 81). The degree of delay was substantially higher for these industries, almost 54 percent versus 47 percent for all industries. However, we find the industry-specific delay had a negative but insignificant impact on the employment recovery of these industries across all specifications and sub-samples (all, urban, smaller urban, and rural counties). On the other hand, if we use the share delayed computed for all PPP borrowers in a county, regardless of their industry, the coefficients on the share delayed become significantly negative, large and persistent in

Suffolk County, Massachusetts; Hudson County, New Jersey; Bronx County, New York; Kings County, New York; New York County, New York; Queens County, New York; Richmond County, New York; Philadelphia County, Pennsylvania; Arlington County, Virginia; and Alexandria County, Virginia. Only two counties (Queens County and Kings County) appear in both lists. The separate coefficients for these two counties are not significantly different from those of the most densely populated counties.

[19] Appendix Table A.2 shows that that share delayed is also greater in denser counties, conditional on preexisting conditions and the public health situation early on in the pandemic. Since this difference is not quite statistically significant, we focus on the top 1 percent of counties by population size in the paper.

regressions that pool all counties.[20] Figure 2 illustrates this surprising result by plotting the estimated effects of a one-standard-deviation change in the corresponding share delayed (either overall or industry-specific), normalized by the average employment level in these industries in January 2020. These less-than-intuitive differences in coefficients indicate once again that the share delay likely captures additional mechanisms beyond the importance of timely access to credit.

Effects on Small Establishment Employment

Since the target of the PPP was small businesses (those with fewer than 500 employees), we investigate the impact of the PPP funding delay on small establishment employment. We use quarterly QWI data because the QCEW provides such detail only at an annual frequency. Table 4 summarizes our findings using the full county sample and including all the controls in column (4) of Table 3.[21] Columns (1) and (2) report results for firms of all sizes, which allows us to compare the QCEW and the QWI at the quarterly frequency. The results from the two datasets are consistent with regards to the share delayed, pointing to a persistent negative correlation between employment and funding delay. The impacts estimated using QWI data are a bit larger in absolute value, likely due to sampling and timing differences between these datasets, as average QWI employment comes a bit larger than QCEW employment.

Columns (3)–(6) of Table 4 further split results by the size of the establishment available only in QWI data. Interestingly, we find that the negative and persistent impact of the share delayed is observed across all establishment sizes—the relative magnitude of the estimated coefficients for large (500 plus employees) and small (under 500 employees) establishments can be more easily compared on the left panel of Figure 4, where the estimated coefficients have been normalized by the corresponding employment levels. The impact of the 10-day delay on 2021:Q2 employment in smaller establishments

[20] Detailed results of specification using industry specific share delay are in Appendix Table A.5, and those using county level measure of share delay are in Appendix Table A.6.

[21] Results on urban, smaller urban, and rural sub-samples are available upon request. The coefficients on the share delayed are never statistically significant across the establishment size distribution when using these sub-samples.

is large and significant (extending to 2021:Q3 employment for the smallest firms with fewer than 20 employees). However, the significant effect for large (500 plus employees) establishments, which were not in principle eligible for PPP funding, casts some doubts on the funding mechanism alone as the explanation for the correlation between employment and funding delay.

QWI data also allows us to explore the impact of the delay on other measures of employment, such as new hires, recalls, and separations. We find no consistent-with-theory impacts of the share delayed on these measures.[22] To summarize, our findings on QWI employment data confirm that the share delayed is picking up something other than (or in addition to) the impact of the delay in funding.

Analysis at the CBSA Level

QCEW employment data, as well as many other data sources we use, are compiled at the county level, which is therefore the geographic level of our analysis. However, since many individuals residing in a metropolitan area travel out of county for jobs, core-based statistical areas (CBSAs) better correspond to local labor markets and thus constitute a more natural level for analysis—for example, DK match CPS respondents to CBSAs. We replicate our analysis at the CBSA level, the data for which are created using crosswalks between counties and CBSAs provided by the US Department of Housing and Urban Development (HUD). Our main message remains the same (see Appendix Table A.8 and Figures A.2 and A.3). We find the estimated effect of the share delayed on employment to be negative and of smaller magnitude in smaller CBSAs. Curiously, at the CBSA level, the share delayed has a less persistent effect on employment in every specification.

Reconciling QCEW with CPS Employment Data

To fully reconcile our findings with those in DK (who perform their analysis using individual-level data from the CPS), it is important to understand how CPS employ-

[22]For example, we find a positive and in many cases statistically significant impact of the delay on recalls. These results are available upon request.

ment data compare to QCEW employment in the geographies observed in both sources, including whether any observed differences correlate with the share delayed. As shown in Appendix C, CPS employment appears to be undercounted relative to QCEW employment during 2020, but the degree of undercounting is not correlated with the share delayed. Thus, DK's estimates are likely not biased by pandemic-induced distortions to the CPS data along this dimension.

However, the more limited coverage of geographical areas in the CPS is of material importance for DK's results. The issue is that to preserve respondents' confidentiality, only 280 counties (less than 10 percent of all counties) and 257 CBSAs (or about 14 percent of all CBSAs) can be identified in the publicly-available CPS data. To illustrate whether a more limited geographical sample might be important for the results, we run our main specification using QCEW data on the sub-sample of counties that can be identified in the CPS. Similar to DK, we find a persistent effect of the share delayed in the CPS sub-sample.[23] Nevertheless, this result does not carry through to the full sample of counties available in QCEW data, as shown in Figure 3. This finding is also true if we use CBSAs instead of counties as the unit of analysis (results not shown for brevity).

Appendix C shows our replication of DK confirming that a funding delay was more detrimental to non-employer businesses than to employer businesses. In addition, the effect of the funding delay in individual-level regressions declines significantly (and looses statistical significance) when we remove respondents who live in the top 1 percent most populous counties and/or metropolitan areas.

Robustness to Functional Form

Given the change in the magnitude and the (statistical) importance of share delayed in different county sub-samples, we also investigate the robustness of our results to using logarithmic and growth rate specifications for employment (that could arguably be less

[23]As an aside, the coefficients from county-level regressions estimated using only CPS counties are of similar magnitude to those obtained using the individual-level CPS data (full details in Appendix C), which is reassuring.

affected by outliers). Appendix Tables A.9–A.12 present these results and show that the estimated effect of the share delayed is sensitive to the functional-form specification. Figure 4 clearly illustrates this point by comparing our results from employment level regressions (left panel) and logarithmic specifications (right panel) for the full sample of QWI counties. While the share delayed has a negative and statistically significant impact on employment when expressed in levels across establishment size categories (all, 500 plus, and under 500), no such trends are observed in logarithmic regressions; and if anything, the response in logarithmic regressions is positive for the first three quarters and becomes negative for the large establishments post 2020Q4. The results using these alternative specifications are counter-intuitive at best.

Figure 5 focuses on urban counties and logarithmic specifications that allow for a differential effect of the share delayed based on county population (top 1 percent versus the rest). Interestingly, with a logarithmic specification, the correlation between the share delayed and employment is negative only for the most populous counties, and particularly for small establishments in these most populous locations.[24] In sum, our observation that the share delayed is more negatively correlated with employment in the most populous areas is robust to this specification change.

The findings so far collectively indicate that the slower trajectory of employment recovery as a result of PPP delay that DK find is driven by the most populous counties or CBSAs. More importantly, however, this effect is not necessarily causal in that those larger areas were more vulnerable to a highly infectious disease such as COVID-19 in ways that might not be fully captured by linear functions of observable preexisting conditions.

[24] The employment trends for smaller establishments such as those with fewer than 50 (or 20) employees, are indistinguishable from those under 500.

5 Comparing the Relative Importance of CARES Act Policies

To illustrate the effects of other CARES Act policies relative to the effect of share delayed on the recovery of county employment, we plot their respective estimated coefficients in Figures 1, 6, and 7. We focus on tax rebates, industry-weighted UI replacement rates, and the cumulative volume of PPP receipts per employee in small establishments, in addition to share delayed. For ease of comparison, we standardize these covariates (to a mean of zero and a standard deviation of one) during the sample period, so that each coefficient measures the impact of a one-standard-deviation increase in these variables. We also normalize the coefficient estimates by county-level employment in January 2020 to more easily compare their magnitude across regressions. While UI replacement rates and tax rebates are plausibly exogenous, PPP receipts are unlikely to be exogenous, it is nonetheless useful to compare their relative contribution to explaining the heterogeneity in the employment recovery across counties.

Overall, larger shares of loans delayed and higher UI replacement rates curtail employment,[25] while higher volumes of PPP receipts and to some extent rebates foster employment. The effects of stimulus programs are larger and more precisely estimated for industries hit particularly hard by the pandemic (NAICS 71, 72 and 81; compare the top and bottom panels of Figure 6). Moreover, our findings point to heterogeneous effects of the various policies within urban counties, see Figure 1, and across urbanicity levels, see also Figure 7.

Figure 1 illustrates the differential impact of policies on urban counties based on their population (the top 1 percent most populous urban counties versus the rest). Interestingly, the impact of rebates is similar (and not significant) across urban counties. In contrast, the UI replacement rate is unimportant for the employment recovery of

[25]The uniform supplemental Pandemic Unemployment Compensation (PUC) payments, $600 per week shortly after the start of COVID-19 outbreak and the additional $300 weekly supplemental payment to all claimants during this period means that the UI replacement rates vary across counties in inverse relationship to their pre-pandemic average wage.

the most populous counties, while it exerts a large negative drag on the employment recovery of other urban counties. Finally, total PPP fund receipts help less populous counties more than the most populous ones.

The effects of the PPP also differ across urban and rural counties, corroborating earlier conclusions that urban counties drive the pooled-sample estimates for the share delayed. This appears to be the case also for cumulative loan receipts (see Figure 7). In fact, the share delayed does not play much of a role in the employment recovery of affected industries or rural counties, as the estimated coefficients are all insignificant, small and, for rural counties, sometimes even positive. While the share delayed matters marginally for employment in urban counties (driven by the top 1 percent most populous ones), PPP receipts are nonetheless relatively more important. Both UI benefits and rebates were associated with a more positive employment recovery in rural counties, acting as demand boosters. Conversely, relatively more generous UI benefits during this period slowed employment growth in urban counties. This finding suggests that in an urban setting, UI benefits discouraged labor supply more than they boosted demand, as perhaps both labor supply and demand for certain goods and services were more restrained in these areas by people's fear of COVID-19 infection.

In addition, we quantify the contribution of each covariate to explaining the variance of employment (the dependant variable) using a Shorrocks-Shapley decomposition of the R-squared.[26] Following the structure of the regression results presented in Table 3, we group the covariates into categories (preexisting conditions, COVID-19 controls, and CARES Act transfers) to simplify the graphical exposition. We report the contributions of the share delayed, cumulative PPP receipts, rebates to households, and UI benefits replacement rates separately to assess the relative importance of the different government interventions.

As the top LHS panel of Figure 8 shows, all the covariates combined account for close to 30 percent of the variance in employment, with lag employment levels accounting for

[26]This is implemented with the Stata command shapley2, written by Juarez (2012) and based on Shorrocks (1982). We run period-by-period regressions after partialling out state-by-time fixed effects from time-varying variables and state fixed effects from time-invariant controls.

the rest.[27] Preexisting conditions and COVID-19–related controls are relatively more important than the various government stimulus payments for explaining the evolution of employment. Among the different programs deployed during the pandemic, the county-specific UI replacement rates are the most important in explaining the dynamics of employment (see the top RHS panel). By comparison, the contribution of the share delayed is rather small. On average, from March 2020 to September 2021, preexisting conditions, COVID-19 factors, UI replacement rates, rebates, cumulative PPP funds, and share delayed explain 18.3, 3.91, 3.6, 1.01, 0.9, and 0.08 percent of the variation in county employment levels, respectively. When the Shorrocks-Shapley decomposition is carried out separately for urban and rural counties (the bottom panel of Figure 8) preexisting conditions are relatively more important for urban counties, while COVID-19–related factors are particularly relevant for rural areas.

6 Funding Receipt More Important than Timeliness: Firm-Level Evidence

A related issue worth pointing out is the fact that the impact of PPP funding delay might differ from that of PPP funding receipt. We try to understand how the two might differ using firm-level activity indicators from SafeGraph (SG) data. These data measure foot traffic derived from mobile devices utilizing GPS location to track movements to and from points of interest (POIs).[28] Each POI has its own unique Placekey, which we use to identify PPP recipients and non-PPP recipients. For each PPP recipient that we can identify in the SG data, we search for closely matched competitors, pre-pandemic, defined as businesses operating in the same Census Block Group (CBG) and the same six-digit NAICS industry that did not receive a PPP loan.[29] When mul-

[27] Total R-squared in these period-by-period partialled out regressions is high, about 0.99.

[28] SG does not cover the universe of POIs, but the coverage is extensive. We use visits to POIs as a proxy for economic activity. See https://docs.safegraph.com/docs/places-data-evaluation for coverage rates by industry.

[29] CBGs are the smallest statistical area for which the US Census provides information, and NAICS six-digit codes are narrow classifications of activity. We can obviously produce additional matched

tiple non-PPP businesses can be matched to a given PPP recipient, we choose the one with the most similar number of visits just before the pandemic started (average weekly visits over December 2019 to February 2020).[30] With this procedure, we are able to match 165,660 businesses from a list of 830,877 (20 percent of PPP-recipient POIs) with a Placekey. Our matching criteria (based on geography and line of business) are rather strict because our goal is to have a sample of businesses that would have faced similar conditions over time in terms of demand, COVID-19–related factors, and imposed restrictions on activity. Lacking information on pre-pandemic firm characteristics, we rely on firm fixed effects to control for unobserved heterogeneity. For simplicity, our sample is also restricted to firms that received just one PPP loan.

To study the effects of PPP receipt and timing, we compare the evolution in the number of total visits to PPP establishments with the evolution in visits to non-PPP establishments over time.[31] SG data are available at a daily frequency, but we aggregate visits to a monthly frequency. Importantly, if a firm is not observed at all in a given month, its value is "filled in" as zero visits.[32] The LHS in our regressions is either Log Visits (defined as the log of visits count plus one) or Zero Visits (an indicator equal to one when visits are recorded as zero in the SG data, or when we fill in the data). Zero Visits is our proxy for business closure, which might be temporary or permanent. With our matched-pair sample, we estimate regressions of the form:

$$Y_{ijt} = \alpha_i + \delta_{jt} + \beta_t \, \text{PPP}_i + \epsilon_{ijt}, \tag{2}$$

pairs if we move to fewer-digit NAICS classifications or consider wider geographical areas, but the results would be qualitatively similar.

[30]Businesses might have not received PPP loans because they were not eligible, because they went out of business early on, or because they did not need funding and chose not to apply. Anecdotes of applicants ultimately being denied are rare. It is also possible that some businesses that we labeled as non-PPP recipients might have in fact received PPP loans. So our estimates are likely a lower bound of the differences between PPP and non-PPP recipients in terms of the measured outcomes.

[31]The number of unique visitors, as opposed to visits, is also available in SG data. Results are very similar when using visitors instead of visits and, for brevity, are not reported.

[32]Businesses had to have been in operation pre-pandemic to be matched (PPP and non-PPP recipients alike). If SG no longer reports traffic to a specific firm, we fill in its monthly observations with zero visits to make sure that firm is still included in our regression sample (making our panel balanced).

where Y_{ijt} is a measure of visits to establishment i of pair j in month t, α_i denotes firm fixed effects, δ_{jt} denotes pair-by-time fixed effects, and PPP_i is a dummy variable equal to one if establishment i received PPP funding and zero otherwise. Standard errors are clustered at the pair-ID level.

We also estimate regressions of the form:

$$Y_{ijt} = \alpha_i + \delta_{jt} + \sum_{l=-T+1}^{-2} \mu_l D^l_{ijt} + \sum_{l=0}^{T} \mu_l D^l_{ijt} + \epsilon_{ijt}, \tag{3}$$

where $D^l_{ijt} = \mathbb{I}\{t - K_i = l\}$, and K_i is the month when firm $i's$ PPP loan is received. These regressions are estimated using the Sun and Abraham (2021) estimator, which is consistent under staggered heterogeneous treatment effects. Standard errors are clustered at the pair-ID level.

The left panels of Figure 9 plot the estimated β_t coefficients (PPP estimates over calendar time) from regression (2), while the right panels plot the estimated μ_l coefficients (PPP estimates relative to funding approval dates, which are just two or three days before actual receipt in most cases) from regression (3). The results clearly indicate that businesses that received PPP funding saw significantly more visits (and fewer closures) than their non-PPP peers, starting in the second half of 2020. The relative difference between PPP and non-PPP matched firms increased over time, reaching close to a 15 percent difference by the end of the sample. About one-fifth of the Log Visits effect is accounted for by excess closures (Zero Visits) by the end of the sample, and closures account for a larger fraction earlier on (for example, about one-fourth in December 2020).

To gauge the importance of the timing of PPP funding relative to funding receipt, we restrict our estimations to matched pairs with PPP loans approved either early (April 14 through 16) or late (April 27 and 28), as defined by DK. Results are depicted in Figure 10 and show a small and insignificant average difference in foot traffic between early and late PPP loan recipients (relative to their pairs). No significant difference in Zero Visits is observed. The small and insignificant average difference in foot traffic between early and late PPP loan recipients (relative to their pairs) seems difficult to reconcile with DK's report of share delayed having such a significant effect on employment. Apart from the possibility of a large indirect effect on other firms' employment, a possible explanation for this discrepancy might be that the number of visits is only weakly correlated with employment recovery at the firm level. Specifically, relative to early PPP recipients, late PPP firms might have had to operate with reduced staff despite the recovery in customer traffic. This hypothesis, however, cannot be tested using available data, and it is not obvious why there would be such a systematic difference in the relative recovery of visits versus employment between early and late PPP borrowers.[33] Overall, our findings are consistent with those of Cole (2022), who uses administrative payroll data for very small firms to argue that it is the receipt of PPP funding that was important for employee retention and growth rather than the timing of the funding.[34]

7 Summary and Concluding Remarks

This study documents that the persistent effect of the 10-day delay in PPP funding estimated by Doniger and Kay (2023) is mostly driven by the top 1 percent most populous urban counties. These locales suffered higher rates of infections and deaths in the first wave of the COVID-19 pandemic and thus imposed stricter containment measures earlier on. The resulting worse disruption to commercial activity raised demand for PPP loans while at the same time hampering their supply, resulting in a larger backlog of PPP loan applications when funding was halted for 10 days in April 2020. Subsequently, the highly contagious nature of the virus and the shift toward remote work in response made it more difficult for economic activity in these densely populated places

[33]In terms of the (log) levels of visits and employment at the county level, the correlation coefficient is quite high (0.62) even with county and state-by-month fixed effects for all industries or industries where foot traffic and employment are more directly related (that is, those with 71 or 72 as their two-digit NAICS code).

[34]In a companion paper, Gorbachev, Luengo-Prado, and Wang (2023) show that PPP receipt was also more important for businesses' survival. They also find that PPP recipients' credit risk profiles improved relative to the profiles of their non-recipient peers.

to recover, beyond what the linear functions of control variables of pre-pandemic local conditions used in most previous studies could explain. This unaccounted for heterogeneity, rather than the slightly earlier access to liquidity, might explain the correlation between the share of PPP loans delayed and the subsequent slower employment recovery. Consistent with this interpretation, employment recovery of the most adversely affected, and arguably more liquidity-constrained industries, such as leisure and hospitality, was not significantly associated with the share of loans delayed to firms in those industries. Additionally, the employment recovery of large establishments (not eligible for PPP funding) also correlates negatively with the share delayed. Our findings suggest that the economic fundamentals of these urban centers were more vulnerable to an infectious disease such as COVID-19 and that they would have needed extra government support if the goal was to restore employment to pre-pandemic levels.

Moreover, consistent with the PPP's stated goal of speeding up the employment recovery, we find that receiving (more) PPP funds was indeed much more important than receiving those funds a little earlier. Put differently, having access to liquidity is crucial for business operations in general and during the pandemic, but having that access a little earlier is much less so. In addition, two other important pandemic support programs—expanded UI payments and rebates—explain a greater share of the variation in local employment recoveries over time than does the volume of PPP funds received or, especially, the share of PPP loans delayed. Preexisting local characteristics (including population density, relationship banking, and 2019 banking conditions) and the severity of the initial pandemic shock, combined with its heterogeneous impact across localities due to heterogeneous pre-pandemic industrial compositions, were also quantitatively important determinants of the recovery. In sum, our findings suggest that in a pandemic, the availability of public transfer payments and credit assistance matter more than a slight difference in the timing of funding receipt in shaping the post-shock recovery.

It is beyond the scope of this paper to address the normative question of what should be the optimal or most efficient policy response in terms of "bang for the buck" toward the goal of preserving matches between firms and workers. In fact, a more difficult, but just as important, question is to what extent it is even optimal to restore employment to pre-pandemic levels or composition given the structural shift toward remote work enabled by the latest technology. Many economists have pointed to policies common in European countries (such as the Kurzarbeit program in Germany) that involved governments sharing labor compensation costs with firms so that workers could stay on the job. It has not been widely recognized, however, that the United States has comparable programs. In particular, short-time compensation (STC) programs allow firms to avoid layoffs by subsidizing a reduction in working hours through temporary prorated payments. Workers are able to keep their fringe benefits while also receiving a partial UI payment to supplement their lower wages. In the United States, 27 states include STCs as part of their overall UI programs.[35] The STC programs might not have helped with the widely reported severe congestion-induced delays of the UI system early in the pandemic. Nevertheless, going forward, more studies of the design, effects, and implementation of STCs and partial UI programs should be conducted. One goal would be to understand why STC programs were used less during the pandemic than during the Great Recession—perhaps the PPP, given its scale, made it unnecessary for employers to explore alternative options. Apart from implementing other worthy options for government support programs, arriving at the correct understanding of the mechanism through which the PPP facilitated the economic recovery, in particular whether specific aspects of the program rules contributed to the ultimate goal of preserving or expanding employment, is important for refining the design of such public credit support programs should the need arise again in the future.

[35]Houseman et al. (2017) explore the role of STCs during the Great Recession. Rodriguez, Segal, and von Wachter (2023) find that STC programs generally reduce layoffs, enable workers to receive, on average, higher UI benefits along with employer benefits, leading to higher earnings for workers over the long haul.

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Table 1: Summary Statistics for County-Level Preexisting Conditions

	All		Urban		Smaller Urban		Rural	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
PPP Receipt								
No. of Early PPP Loans (4/14–4/16/2020)	204	586	453	890	397	617	50	54
No. of Late PPP Loans (4/27–4/28/2020)	235	640	501	974	436	651	70	67
Volume of Early PPP Loans (4/14-16/2020)	28,272	106,663	67,570	164,810	57,511	118,993	3,867	5,296
Volume of Late PPP Loans (4/27-28/2020)	21,321	90,274	50,390	141,023	40,952	92,112	3,269	3,938
Early Jobs Saved	3,209	10,829	7,509	16,598	6,458	11,594	539	685
Late Jobs Saved	2,613	9,708	6,033	15,056	4,984	9,409	488	542
Cum. No. of PPP Loans	958	2,634	2,076	3,998	1,814	2,659	264	266
Cum. Volume of PPP Loans (Million 2016$)	132	467	309	720	261	479	22	26
Avg. Size of PPP Loans (1,000 2016$)	91	42	112	42	112	42	78	36
Share of PPP Loans Delayed (By Count)	0.58	0.12	0.55	0.10	0.55	0.10	0.59	0.12
Share of PPP Loans Delayed (By Vol.)	0.48	0.18	0.44	0.13	0.44	0.13	0.50	0.20
Share of Jobs Delayed	0.49	0.16	0.46	0.12	0.46	0.13	0.51	0.18
COVID-19 Impacts								
Cum. COVID-19 Cases per Million Pop.	150	315	210	367	207	360	112	272
Cum. COVID-19 Deaths per Million Pop.	67	189	107	241	103	226	42	142
Covid-10 Stringency Index (Oxford University)	70	8	70	8	70	8	69	9
Share of days in lockdown (pre-4/17/2020)	0.50	0.11	0.51	0.10	0.51	0.10	0.50	0.11
Share of days in lockdown (4/17–4/30/2020)	0.99	0.05	1.00	0.02	1.00	0.02	0.99	0.06
Share of Emp. in Essential Industries	0.88	0.02	0.87	0.02	0.87	0.02	0.88	0.02
Share of Emp. in Impacted Industries	0.32	0.08	0.32	0.07	0.32	0.07	0.31	0.09
Share of Wages in Impacted Industries	0.19	0.07	0.19	0.06	0.19	0.06	0.19	0.08
Share of Emp. in NAICS 71, 72 & 81	0.16	0.06	0.17	0.05	0.17	0.05	0.16	0.06
Share of Wages in NAICS 71, 72 & 81	0.08	0.05	0.08	0.04	0.08	0.04	0.08	0.05
UI Benefits Replacement Rate (Industry-Wtd.)	1.39	0.19	1.30	0.18	1.30	0.18	1.44	0.17
Preexisting Conditions								
Rural County Dummy	0.62	0.49						
Total Residential Population	108,885	342,828	245,668	525,144	207,098	302,323	23,937	22,533
Commuter to Residential Population Ratio	1.15	0.11	1.13	0.13	1.13	0.12	1.17	0.10
Median Family Income	67,238	16,223	76,058	18,370	76,011	18,391	61,761	11,784
Community Bank Share of Branches	0.45	0.32	0.33	0.27	0.33	0.27	0.52	0.33
Community Bank Share of Deposits	0.43	0.34	0.30	0.28	0.30	0.28	0.52	0.35
Big4 Bank Share of Deposits	0.03	0.07	0.03	0.06	0.03	0.06	0.02	0.08
No. of Branches	28	71	59	108	52	72	9	8
Bank Branch Density (Population per Branch)	3,208	1,927	4,109	1,980	4,095	1,982	2,649	1,665
No. of Small Business Loans in 2019	2,429	10,540	5,784	16,479	4,578	8,619	345	428
Vol. of SBL in 2019	78,721	301,083	186,470	466,393	152,878	262,618	11,805	15,571
Avg SBL Loan in thousands of 2016$	33	13	34	11	34	11	32	14
SBL Vol. per Small Estabs. (< 500 Emp.) (CBP 2019Q1)	22	11	27	9	27	9	19	11
Private Employment								
Private Emp., 2020	34,504	125,274	80,744	193,528	67,430	123,496	5,786	6,301
Private Employment in NAICS 71, 72, and 81	3,841	14,334	9,019	22,179	7,515	13,790	625	872
No. of Private Estabs., 2020	3,140	13,016	7,229	20,364	5,884	10,289	601	639
Share of Employment in Estabs (under 500), QWI 2019	0.54	0.14	0.48	0.11	0.48	0.11	0.57	0.14
Share of Estabs (under 5), CBP 2019Q1	0.56	0.07	0.54	0.06	0.54	0.06	0.57	0.07
Share of Estabs (under 50), CBP 2019Q1	0.96	0.02	0.95	0.02	0.95	0.02	0.96	0.02
Share of Estabs (under 500), CBP 2019Q1	0.99	0.01	1.00	0.01	1.00	0.01	0.99	0.02

Notes: "Smaller Urban" refers to urban counties excluding those in the top 1 percent by population. The values for each variable pertain to April 2020 unless specified otherwise.
Source: Multiple 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 bil up to 4/15/2020	0.018	0.064***	0.073***	–0.005
	(0.020)	(0.022)	(0.022)	(0.035)
Cum. COVID-19 Deaths per bil up to 4/15/2020	0.066	0.030	0.001	0.079
	(0.051)	(0.065)	(0.068)	(0.096)
Share of days in lockdown (pre-4/17/2020)	0.029	–0.023	–0.025	–0.035
	(0.053)	(0.066)	(0.069)	(0.069)
Share of days in lockdown (4/17–4/30/2020)	0.126	–0.044	–0.046	0.291***
	(0.144)	(0.136)	(0.135)	(0.070)
Share of Emp. in Essential Industries	–0.242	–0.409	–0.442	0.044
	(0.216)	(0.344)	(0.346)	(0.259)
Share of Emp. in Impacted Industries	–0.044	–0.122	–0.124	0.072
	(0.060)	(0.113)	(0.114)	(0.084)
Rural County Dummy	0.005			
	(0.010)			
Most Populous County (Top 1%)	0.068***	0.066***		
	(0.022)	(0.024)		
Ln Residential Population	–0.023***	–0.021***	–0.021***	–0.030***
	(0.005)	(0.007)	(0.007)	(0.010)
Commuter to Residential Population Ratio	–0.024	–0.017	–0.013	–0.028
	(0.026)	(0.029)	(0.029)	(0.062)
Ln Median Family Income	0.019	0.016	0.016	0.019
	(0.027)	(0.035)	(0.036)	(0.053)
Community Bank Share of Deposits	0.003	0.039**	0.039**	–0.009
	(0.014)	(0.015)	(0.015)	(0.019)
Big4 Bank Share of Deposits	0.072	0.116	0.112	0.052
	(0.071)	(0.094)	(0.092)	(0.074)
Ln Bank Branch Density	–0.005	–0.006	–0.007	–0.001
	(0.009)	(0.013)	(0.013)	(0.015)
SBL Vol. per Small Estabs. (< 500 Emp.) (CBP 2019Q1)	–0.002***	–0.002***	–0.002***	–0.002***
	(0.000)	(0.001)	(0.001)	(0.000)
Proportion of Small Employment in 2020Q1 to 2019Q1, QWI	0.013	–0.161	–0.160	0.092
	(0.067)	(0.097)	(0.097)	(0.079)
replacement_ind	–0.019	–0.008	–0.013	–0.028
	(0.052)	(0.077)	(0.078)	(0.069)
Constant	0.691	1.194*	1.236*	0.263
	(0.498)	(0.685)	(0.689)	(0.716)
Adjusted R-squared	0.13	0.17	0.17	0.10
Observations	2644	1108	1096	1536
State FE	Yes	Yes	Yes	Yes

Notes: "Smaller" refers to urban counties excluding those in the top 1 percent by population.
Source: Multiple data sources described in Section 2.2.

Table 3: Effects of Share of PPP Loans Delayed on QCEW County Private Employment

	All Counties				Urban		Smaller		Rural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Jan 2020 × Delayed	53	175	174	174	270*	456*	100	202	–20*	–18
	(35)	(119)	(119)	(119)	(153)	(274)	(106)	(191)	(12)	(12)
Feb 2020 × Delayed	143***	271**	270**	270**	325*	556**	239**	371*	–18**	–15
	(51)	(119)	(119)	(119)	(169)	(262)	(107)	(205)	(9)	(10)
Apr 2020 × Delayed	–731**	–553*	–546*	–546*	–3,037**	–2,175*	–1,688*	–1,196	–36	–19
	(329)	(295)	(293)	(293)	(1,428)	(1,300)	(931)	(888)	(57)	(57)
May 2020 × Delayed	–891***	–679**	–669**	–661**	–3,400**	–2,257*	–2,446**	–1,700*	–2	16
	(336)	(285)	(284)	(283)	(1,462)	(1,231)	(1,213)	(1,020)	(48)	(49)
Jun 2020 × Delayed	–974***	–741***	–728***	–717***	–3,739**	–2,297*	–2,873*	–1,842	31	47
	(366)	(280)	(278)	(278)	(1,675)	(1,256)	(1,564)	(1,120)	(40)	(40)
Jul 2020 × Delayed	–1,023***	–834***	–812***	–798***	–3,383**	–1,946*	–2,672*	–1,626	40	52
	(354)	(275)	(274)	(273)	(1,607)	(1,182)	(1,596)	(1,095)	(38)	(39)
Aug 2020 × Delayed	–942***	–766***	–755***	–741***	–2,921*	–1,490	–2,400	–1,319	34	50
	(342)	(267)	(268)	(267)	(1,537)	(1,137)	(1,574)	(1,071)	(36)	(36)
Sept 2020 × Delayed	–818**	–670***	–658***	–645**	–2,449*	–1,149	–2,146	–1,149	30	46
	(326)	(252)	(252)	(251)	(1,471)	(1,068)	(1,536)	(1,042)	(35)	(35)
Oct 2020 × Delayed	–603*	–450*	–444*	–432*	–1,887	–725	–1,755	–815	34	52
	(312)	(230)	(230)	(230)	(1,419)	(1,011)	(1,493)	(1,014)	(35)	(35)
Nov 2020 × Delayed	–492	–361	–361	–349	–1,827	–680	–1,674	–705	53	70**
	(326)	(239)	(238)	(238)	(1,477)	(1,030)	(1,540)	(1,036)	(34)	(35)
Dec 2020 × Delayed	–464	–341	–340	–322	–1,485	–396	–1,461	–569	56*	70**
	(331)	(249)	(249)	(248)	(1,481)	(1,076)	(1,548)	(1,063)	(33)	(33)
Jan 2021 × Delayed	–604*	–457*	–446	–425	–1,938	–720	–1,772	–851	58*	71**
	(359)	(276)	(275)	(274)	(1,584)	(1,171)	(1,662)	(1,146)	(34)	(35)
Feb 2021 × Delayed	–427	–268	–271	–287	–1,875	–871	–1,676	–887	61*	74**
	(357)	(255)	(255)	(254)	(1,593)	(1,078)	(1,637)	(1,131)	(35)	(35)
Mar 2021 × Delayed	–449	–290	–292	–318	–1,809	–783	–1,719	–904	53	64*
	(344)	(243)	(243)	(242)	(1,556)	(1,044)	(1,604)	(1,093)	(34)	(35)
Apr 2021 × Delayed	–374	–205	–207	–249	–1,264	–286	–1,333	–545	36	38
	(306)	(219)	(218)	(218)	(1,413)	(967)	(1,506)	(1,022)	(40)	(41)
May 2021 × Delayed	–329	–150	–157	–200	–1,194	–217	–1,193	–421	39	40
	(297)	(211)	(211)	(210)	(1,388)	(932)	(1,449)	(979)	(39)	(41)
Jun 2021 × Delayed	–336	–178	–178	–229	–880	34	–860	–159	32	25
	(273)	(207)	(206)	(206)	(1,256)	(897)	(1,300)	(903)	(41)	(43)
Jul 2021 × Delayed	–391	–190	–202	–244	–1,479	–457	–1,134	–382	15	6
	(278)	(208)	(208)	(208)	(1,296)	(900)	(1,315)	(936)	(46)	(46)
Aug 2021 × Delayed	–359	–153	–167	–200	–1,617	–576	–1,182	–470	14	4
	(282)	(206)	(207)	(208)	(1,315)	(902)	(1,291)	(923)	(43)	(44)
Sept 2021 × Delayed	–167	17	2	–35	–932	–6	–702	–81	17	12
	(248)	(188)	(188)	(189)	(1,141)	(807)	(1,099)	(827)	(38)	(39)
Average Private Employment	38,956				91,195		76,404		6,489	
St. Dev. of Private Employment	139,524				215,187		138,117		7,193	
Within R-squared	0.86	0.86	0.86	0.86	0.85	0.86	0.78	0.80	0.48	0.51
Observations	61,173	61,173	61,173	61,173	23,436	23,436	23,184	23,184	37,716	37,716
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	No	Yes	No	Yes	No	Yes	No	Yes

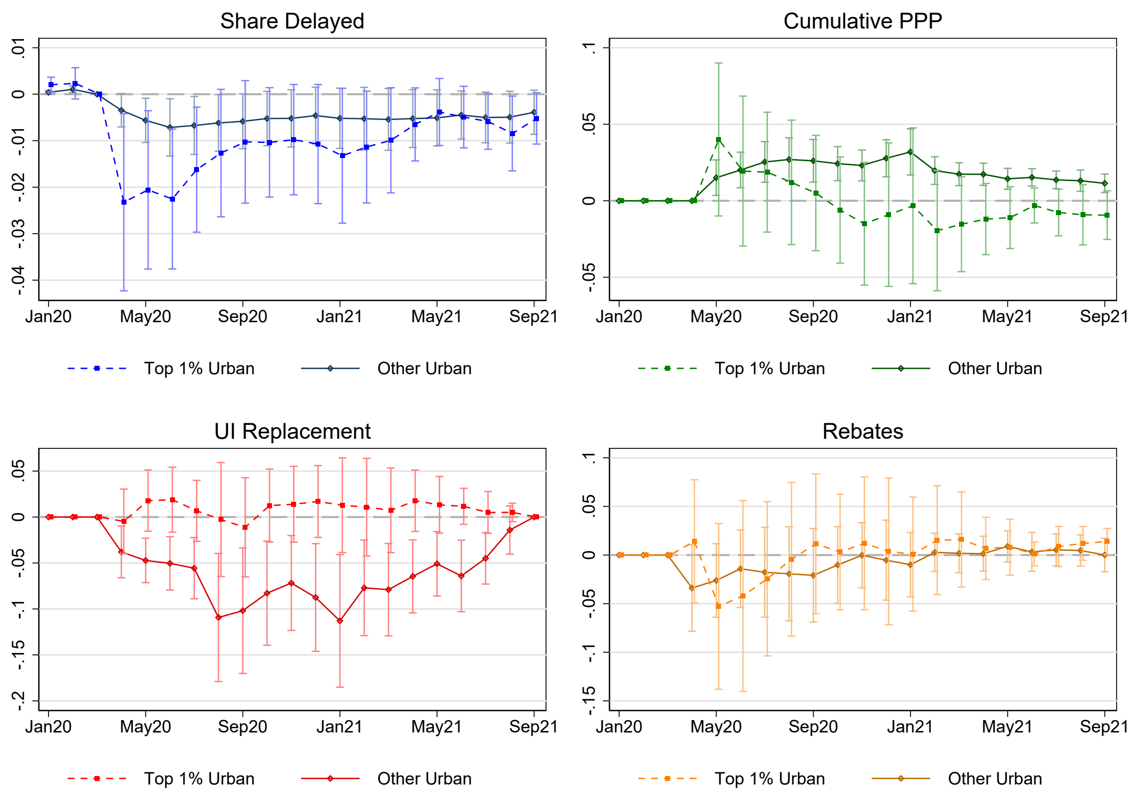
Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 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).
Source: Multiple data sources described in Section 2.2.

Table 4: Effects of Share of PPP Loans Delayed on Private Employment in QCEW vs QWI, All Counties

	QCEW	QWI				
	All	All	≥ 500	< 500	< 50	< 20
	(1)	(2)	(3)	(4)	(5)	(6)
2020Q2 × Share Delayed	–654.68**	–671.08**	–311.18**	–342.40*	–253.31**	–206.83**
	(264.07)	(291.26)	(138.77)	(186.52)	(125.31)	(93.95)
2020Q3 × Share Delayed	–590.65**	–878.93**	–428.01**	–417.41**	–266.32**	–175.81**
	(247.92)	(360.31)	(199.70)	(189.36)	(118.90)	(81.76)
2020Q4 × Share Delayed	–308.20	–592.11*	–377.67*	–174.47	–109.12	–81.11
	(251.19)	(331.58)	(208.12)	(156.03)	(93.87)	(64.45)
2021Q1 × Share Delayed	–334.53	–491.17	–280.60	–165.26	–92.84	–43.81
	(243.43)	(324.10)	(204.02)	(162.80)	(94.49)	(59.73)
2021Q2 × Share Delayed	–265.82	–542.90*	–210.27	–322.61**	–222.57**	–141.98**
	(210.19)	(288.12)	(194.26)	(154.26)	(95.98)	(63.97)
2021Q3 × Share Delayed	–40.62	–224.85	–53.38	–185.05	–139.13	–126.37*
	(188.73)	(241.45)	(177.65)	(139.50)	(88.89)	(65.80)
Average Private Employment	46,278	48,107	24,903	23,414	13,624	9,031
St. Dev. of Private Employment	160,447	174,527	97,122	79,474	46,013	31,554
Within R-squared	0.84	0.85	0.75	0.85	0.85	0.85
Observations	20,391	20,391	20,215	20,391	20,391	20,391

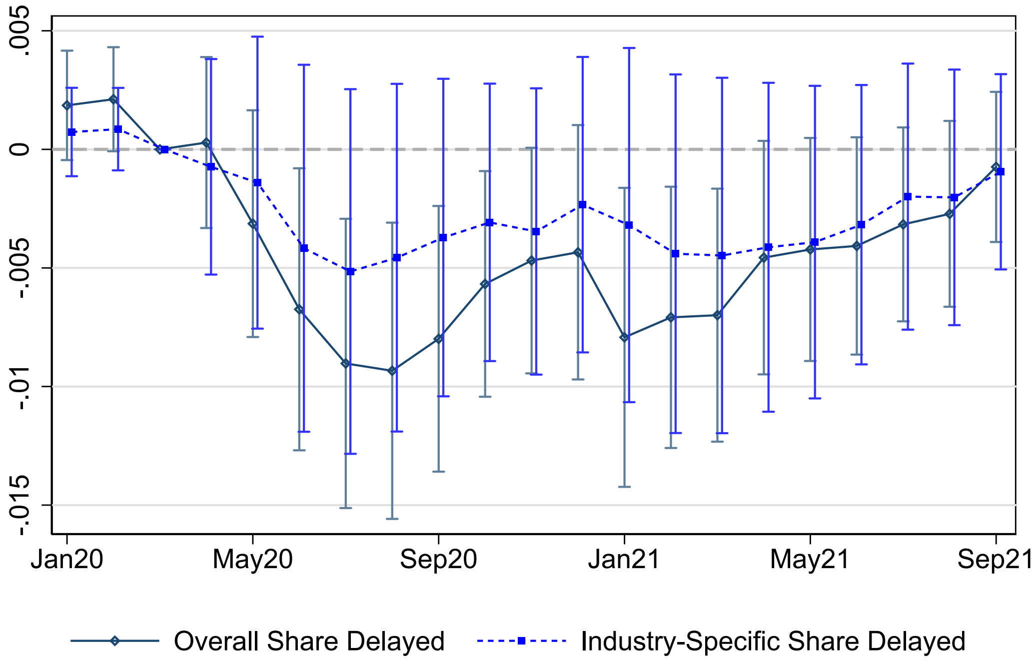
Notes: Standard errors clustered at the county level in parentheses. Sample includes all counties. All regressions control for county and state by quarter fixed effects, in addition to: **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 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; **Cum PPP per Emp in Small Estab (t-1)**; and Lags of Dependent variable. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2).
Source: Multiple data sources described in Section 2.2.

Figure 1: Policy Effects on QCEW County Private Employment: Urban Counties by Population



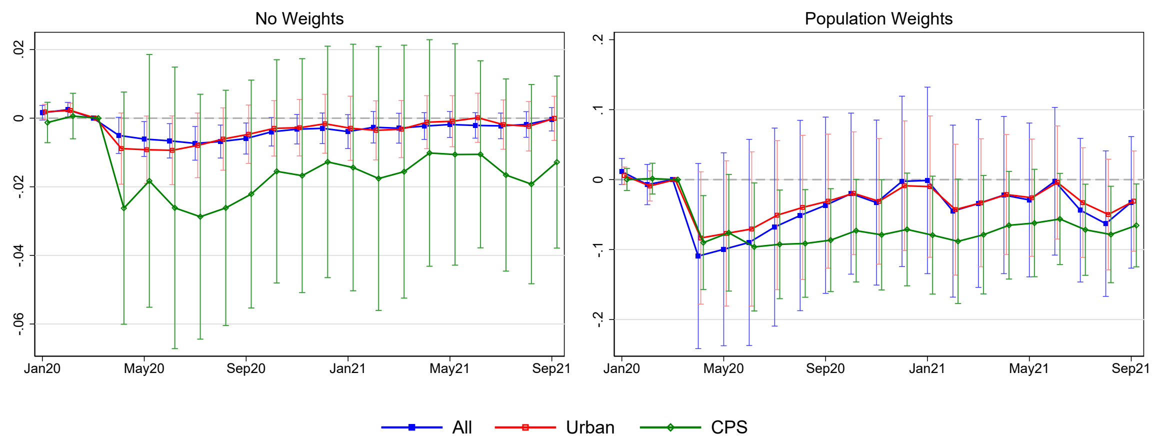
Notes: Estimated effects of a change of one standard deviation in a given policy, normalized by the average employment level in January 2020 of the corresponding sample of counties.
Source: Multiple data sources described in Section 2.2.

Figure 2: Effect of Share Delayed on QCEW County Private Employment: Overall and Industry-Specific Share Delayed



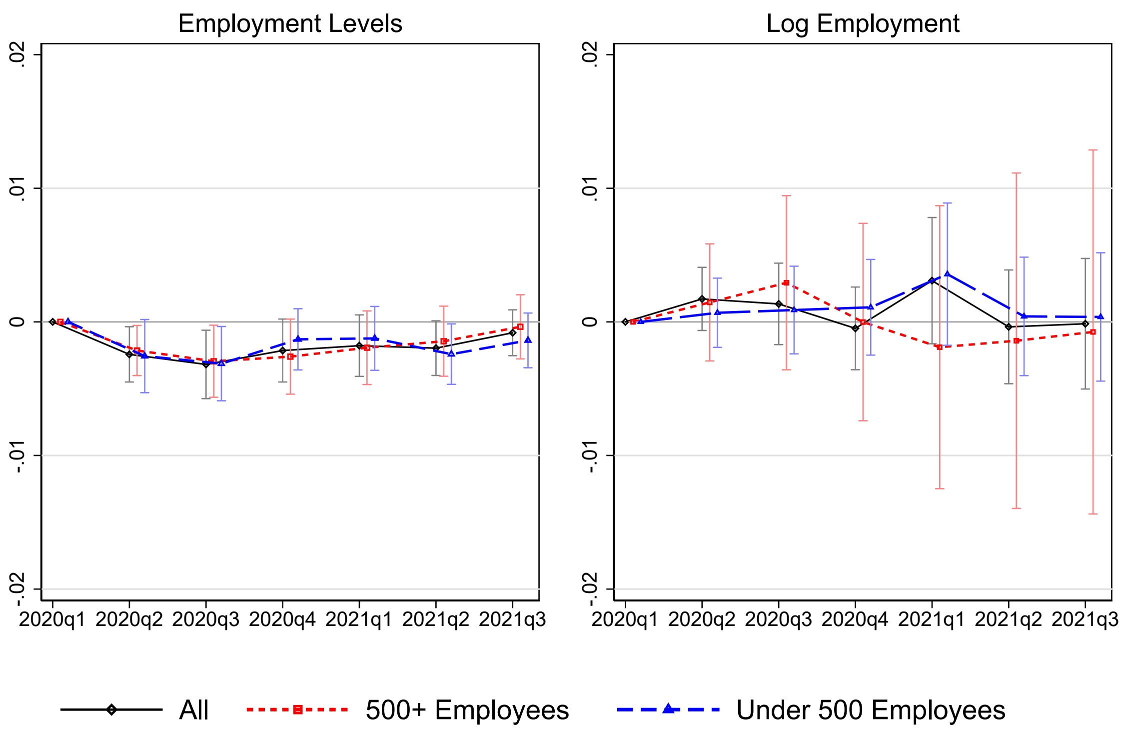
Notes: Estimated effects of a change of one standard deviation in the corresponding share delayed, normalized by the average employment level in January 2020.
Source: Multiple data sources described in Section 2.2.

Figure 3: Effects of Share of PPP Loans Delayed on QCEW County Private Employment: All Counties vs. Counties Present in the CPS



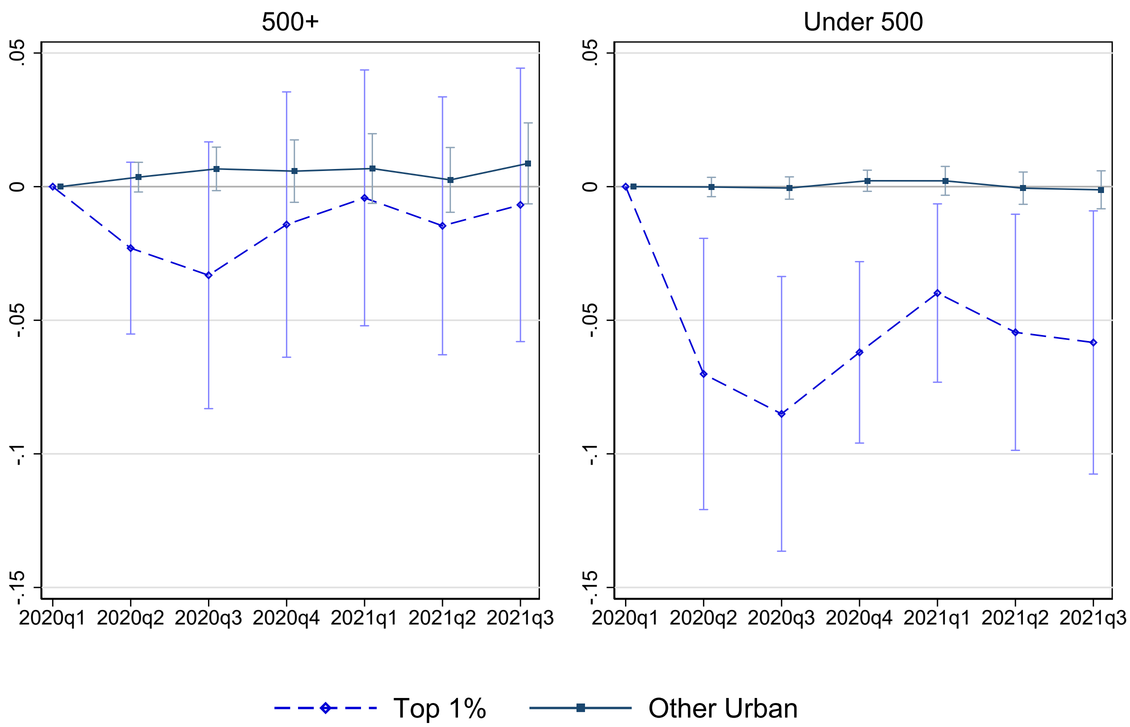
Notes: Regressions were run separately for each sample (all, urban including the top 1 percent most populous counties, and only counties that are identified in the CPS). Estimated coefficients were divided by average county population in each sample in January 2020. The regressions in the right panel are weighted by county population, while the regressions in the left panel are unweighted.

Figure 4: Effects of Share of PPP Loans Delayed on QWI Employment: Levels versus Logs Specifications



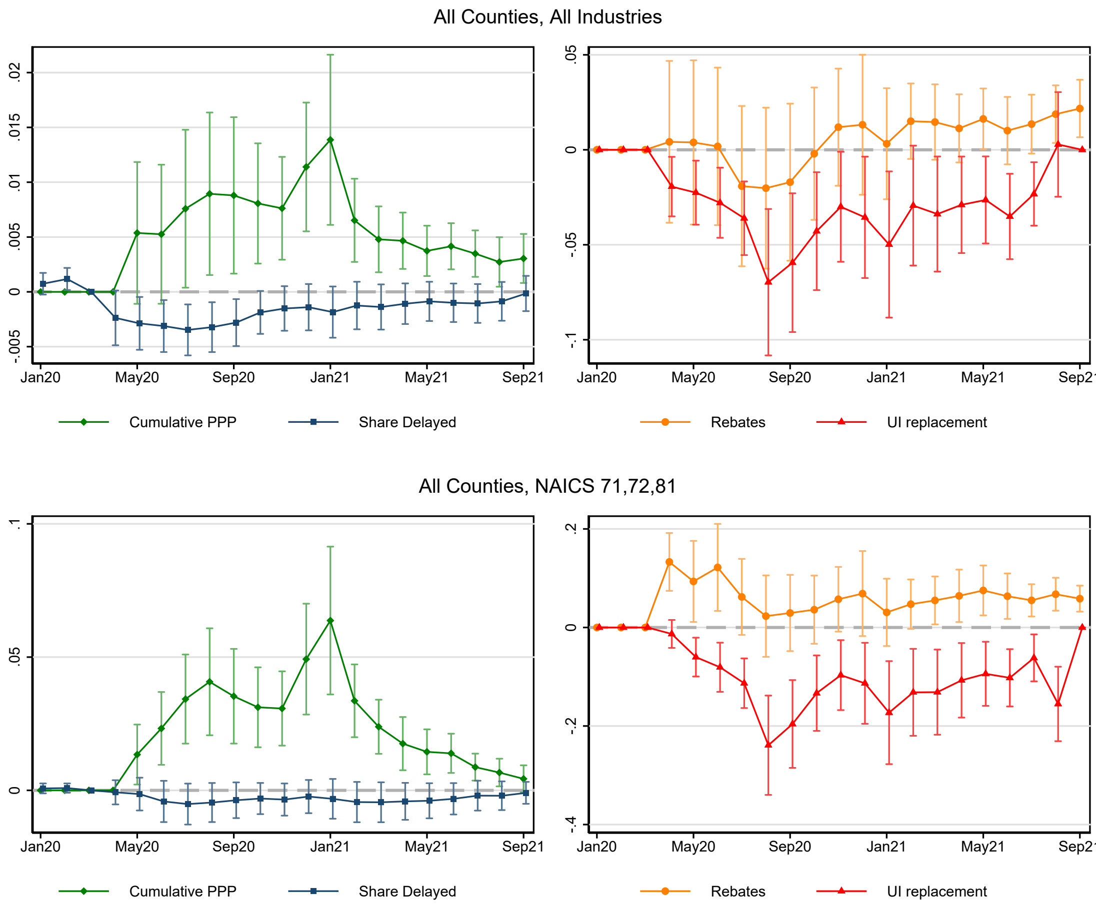
Notes: Regressions were run separately for each sample (all employment, employment in large establishments, and employment in small establishments). Estimated effects of a change of one standard deviation in the share delayed. The estimates in the levels specifications are normalized by the corresponding average employment level in January 2020.

Figure 5: Effects of Share of PPP Loans Delayed on QWI Employment: Log Specification by County Population



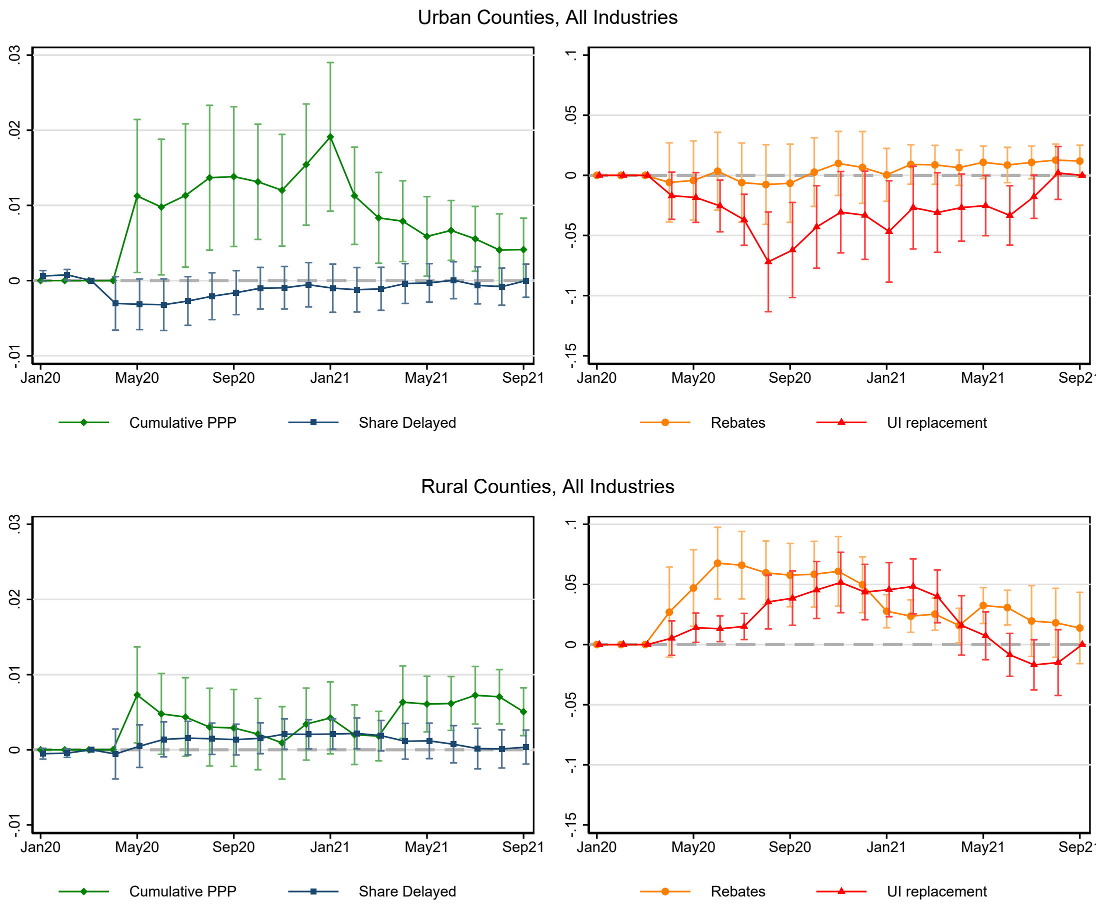
Notes: Estimated effects of a change of one standard deviation in the share delayed. Specifications allows the share delayed estimate to vary by county population group. Urban counties only.

Figure 6: Effects of Policies on QCEW County Private Employment: All Counties



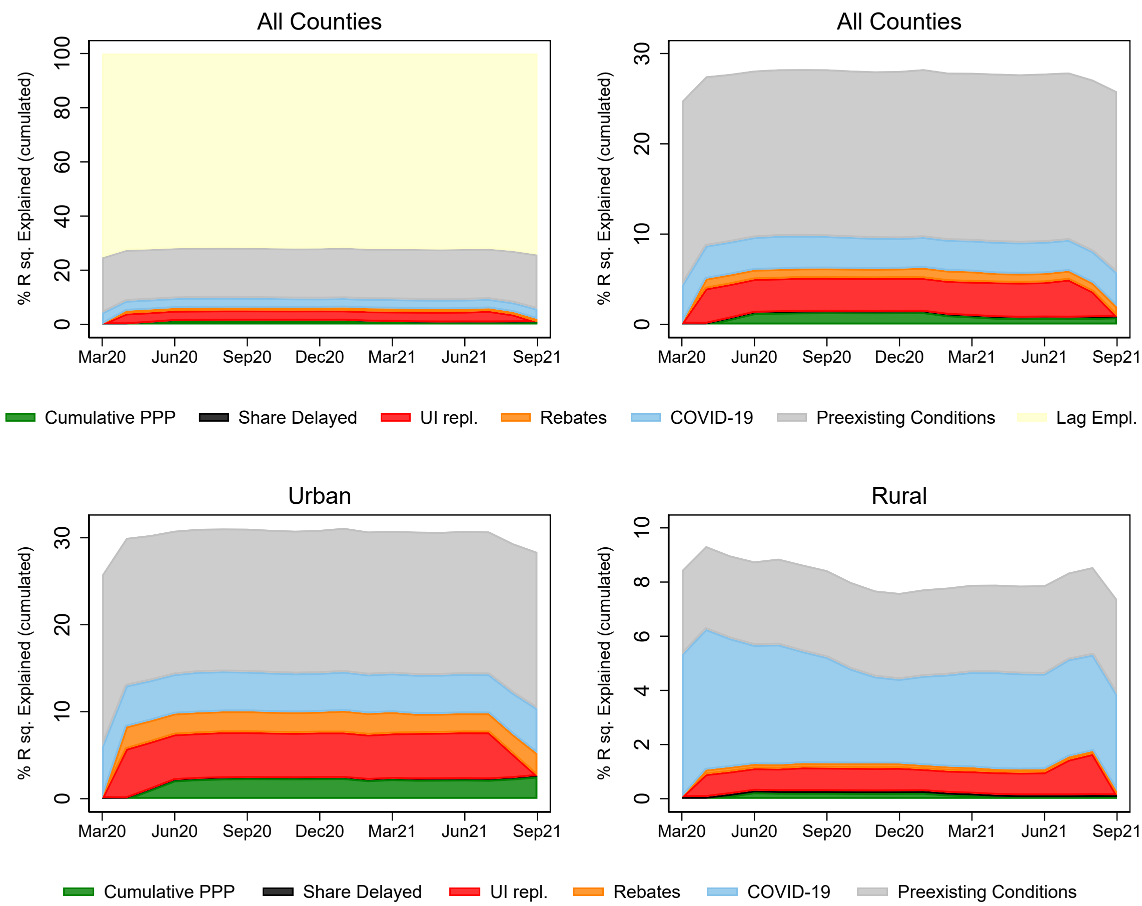
Notes: Estimated effects of a change of one standard deviation in a given control, normalized by the average corresponding county-level employment in January 2020 (all industries in the top panel and employment in NAICS 71, 72, and 81 in the bottom panel).
Source: Multiple data sources described in Section 2.2.

Figure 7: Effects of Policies on QCEW County Private Employment: Urban vs. Rural Counties



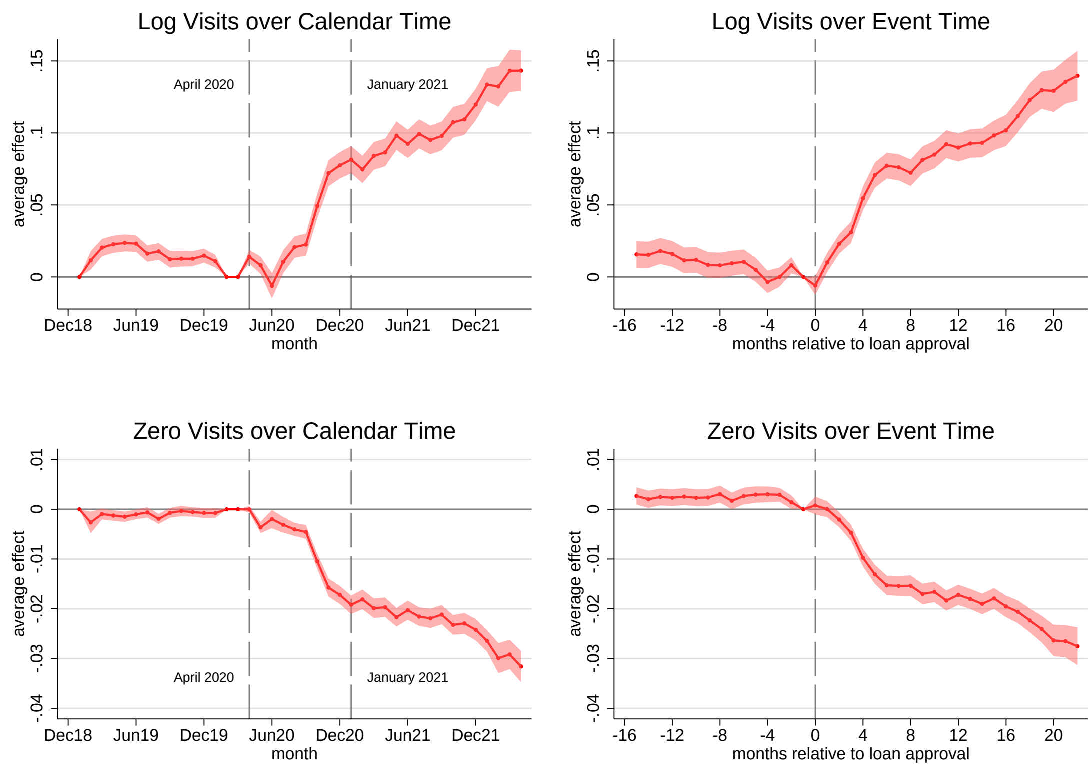
Notes: Estimated effects, based on separate regressions for urban versus rural counties, of a change of one standard deviation in a given control, normalized by the average urban (top panel) or rural (bottom panel) county level of employment in January 2020. The top 1 percent most populous counties are included in the urban sample.
Source: Multiple data sources described in Section 2.2.

Figure 8: R-Squared Decomposition, QCEW County Private Employment Regressions



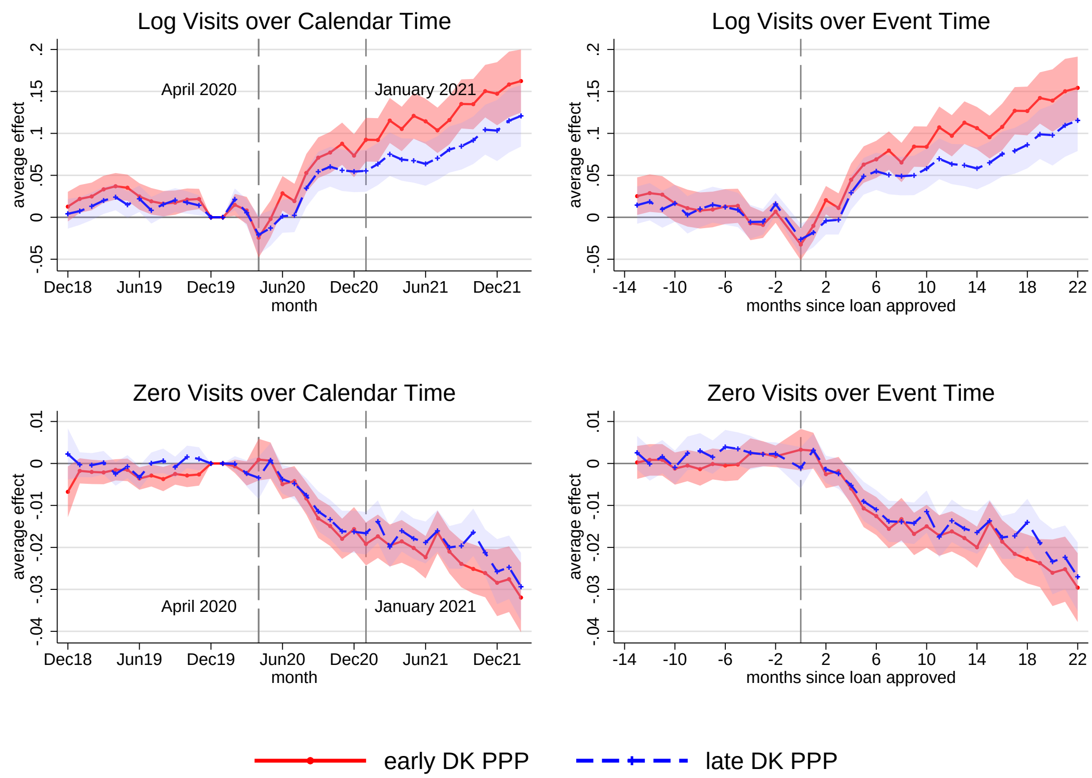
Notes: Contributions of different variables to explaining the variance in employment over time. The effect of lagged employment is omitted in some graphs to more easily depict the contribution of other variables. The top 1 percent most populous counties are included in the urban sample.
Source: Multiple data sources described in Section 2.2.

Figure 9: Effect of PPP Loans over Time: CBG-NAICS6 Firm Pairs in Safegraph



Notes: The left panels plot the β_t coefficients from regressions of the form $Y_{ijt} = \alpha_i + \delta_{jt} + \beta_t \, \text{PPP}_i + \epsilon_{ijt}$, where Y_{ijt} is a measure of visits/visitors to establishment i of pair j in month t, α_i denote firm fixed effects, δ_{jt} are pair-by-time fixed effects, and PPP_i is a dummy variable equal to one if establishment i received PPP funding and zero otherwise. The right panels plot the μ_l coefficients of regressions of the form $Y_{ijt} = \alpha_i + \delta_{jt} + \sum_{l=-T+1}^{-2} \mu_l D^l_{ijt} + \sum_{l=0}^{T} \mu_l D^l_{ijt} + \epsilon_{ijt}$, where $D^l_{ijt} = \mathbb{I}\{t - K_i = l\}$, and K_i is the month when firm $i's$ first PPP loan is received. The shaded areas represent 95 percent confidence intervals. The regressions are estimated using the estimator in Sun and Abraham (2021), which is consistent under heterogeneous treatment effects. Standard errors are clustered at the pair-ID level. The sample is constructed matching each 2020 PPP (recipient) firm to a non-PPP firm in the same census block group (CBG) and NAICS-6 sector. If multiple non-PPP recipients were initially matched to a PPP recipient, we kept the match with the closest average number of visits to the PPP recipient during the months of January and February 2020 (the omitted time dummy in these regressions). The sample is also restricted to firms that received just one loan. The data are filled in in the sense that once a firm disappeared from the data, we assigned them zero visits. Log visits/visitors are defined as the log of visits/visitors counts plus one. Zero visits is a dummy equal to one when visits are recorded as zero in Safegraph or when we fill in the data, and it is our proxy for closure, which might be temporary or permanent. Safegraph data and PPP data from the Small Business Administration were initially matched via Placekeys.

Figure 10: Effect of PPP Loans on Visits over Time: Early versus Late PPP Recipients in Safegraph



Notes: In these graphs, we compare PPP firms to their non-PPP pairs allowing for a differential effect based on the date of their loan approval. These regressions include only firms that received loans on April 14, 15, 16, 27, and 28, 2020 (around the Doniger and Kay (2023) discontinuity). Early firms received loans between April 14 and April 16, and late firms between April 27 and April 28. See notes to Figure 9 for details on the estimation. The shaded areas represent 95 percent confidence intervals.

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 and the total number of establishments and payroll (wages) at a quarterly frequency. According to the QCEW, right before the COVID-19 outbreak (that is, 2020:Q1), there were, on average, about 33,000 employees working in 3,000 establishments (Table 1) in an average county.

On average, each county had about 27.5 bank branches serving about 104,500 individuals, whose median family income was $66,500 (Table 1). In 2019, an average county received 2,317 small business loans (SBL) with a total volume of $75.5 million (2016$) and an average amount of $32,900 (2016$). On average, there were 104,500 residents living in a county, with a commuter-adjusted daytime population of 115,000. In our sample, 63 percent of the counties are classified as rural according to the National Center for Health Statistics (NCHS) urban-rural 2013 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 24,000 people vs. 240,500 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. As of April 2020, an average county had received 926 PPP loans for a total of $126 million (2016$) and an average loan of $90,450 (2016$). However, 58 percent of PPP loans were delayed (48 percent if we use the total volume, not the number, 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 1,637 loans for a total volume of $156 million (2016$) (see Table 1). Moreover, 635 out of 1,000 small establishments (fewer than 500 employees) had received PPP funding, with an average loan size of $42,000 (2016$). 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 already had received, on average, twice the 2019 SBL volume in PPP funding, and this multiple rose to three by the end of the 2020 PPP.

B Evolution of the PPP Provisions

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 fewer than 500 workers and had 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.

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 down the recovery of employment after the initial acute phase of the pandemic. 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 (which we will refer to as the early recipients) versus those that received loans just after the window (the late recipients). In other words, the cross-sectional disparity of these amendments' impact 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. Businesses that obtained PPP loans before the effective date of the PPPFA, however, could elect to use the original eight-week period, thereby allowing them to apply for forgiveness sooner. 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, thereby allowing them to hire back employees only 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 articles here and here.

Additionally, the amount potentially forgiven would not be reduced due to a decreased in 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 worker- or customer-safety requirements. 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 at the county level.

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 on due to the COVID-19 outbreak and the containment measures could reasonably start 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 the 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 the late borrowers than the early borrowers.

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 prior to April 27, since they would need to spend a fixed cost (such as restocking the kitchen or store shelves) to reopen. Thus, it likely applies especially 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 recovery of demand in May and June because they were subject 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,

[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.

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. 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 such firms with 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. In particular, the differential effect does not stem just from the timing delay. Instead, it is the consequence of other design features of the PPP, as well as its interactive effect with other legislative measures to mitigate the impact of the pandemic.

C QCEW versus CPS Data on Employment

Doniger and Kay (2023) perform their analysis using individual-level data from the Current Population Survey (CPS). They merge the share of loans delayed by locality into the CPS data using geographical identifiers for CPS respondents. To preserve anonymity, the county identifiers for close to 60 percent of CPS respondents are suppressed in the public-use data, so that only 280 counties (less than 10 percent of all counties) are covered during our sample period. Information on respondents' CBSAs—the level at which DK merge the share delayed into the CPS—is more available and enables coverage of about 74 percent of respondents and 257 CBSAs (or about 14 percent of all CBSAs). While the QCEW data, covering the population of employees, are obviously more comprehensive than any survey data, the CPS data are more timely and include information on self-employed individuals and business owners, who are not captured in the QCEW. However, to evaluate the efficacy of the PPP program in preserving (private) employment, in particular the valuable matches between employers and employees, the focus should be mostly on employees at employer businesses.

To fully reconcile our findings with those by DK, it is also important to understand how CPS employment compares with QCEW employment in the areas observed in both sources, particularly during the early phase of the pandemic, and whether any observed differences correlate with the share delayed.[5] To this end, we compare employment for the 280 counties and 257 CBSAs identified in the public CPS with employment in the QCEW.[6] Specifically, we regress the log difference between QCEW private employment and CPS employment on monthly indicators (the base period is January 2018). As Figure A.4 shows, CPS employment appears to be undercounted relative to QCEW employment during 2020, but the degree of the undercounting is not correlated with the share delayed (not shown in the figure). This confirms that DK's estimates are likely not biased by a possible pandemic-induced distortion to the CPS data.[7] Nevertheless, the limited coverage of geographical areas in the CPS should be kept in mind when comparing results that use all counties versus those that use the CPS sample.

What happens in the CPS data if we control for the additional covariates in our county-level regressions using QCEW data (that is, SBL per small establishment, UI replacement rates, rebates per population, cumulative PPP receipts per employment in small establishments, etc.)? Does the effect of the share delayed decline? The short answer is yes, to a great extent, but more so in CBSA-based regressions than in county-based ones (to be explained below). Note that, however, our goal is not to reproduce DK's results exactly but to further quantify the influence of our earlier covariates in individual-level regressions using CPS data, as DK does. We define the left hand side as equal to 1 if an individual is employed and 0 otherwise (the opposite of DK's definition) to match the LHS of our county-level regressions. In addition, all our specifications control for state-by-month fixed effects to account for multiple time-varying factors (such as state-specific containment policies in a specific period), some of which DK control for directly.[8] Finally, our CPS regressions are weighted using the individual weights included in the public-use CPS because DK weight their regressions. We do not attempt to correct the CPS data for non-response due to the pandemic, since, as noted above, this correction did not meaningfully affect DK's results.

Using the individual-level CPS data, we estimate an analog of Equation (1) for two samples of individuals: (1) those with information on their county of residence (county sample) and (2) a larger sample of individuals with CBSA identifiers (CBSA sample, as in DK's analysis). The county sample forms our baseline because it enables the use of more precise location-based controls (including the share delayed) at the county level, while the CBSA sample with CBSA-level controls matches DK's specifications and thus facilitates comparisons to their results. The aggregate controls (such as the share delayed) match the level of geography considered (that is, county-level controls for the county sample and CBSA-level for the CBSA sample) in most specifications. We also explore the difference in estimates for particular subsamples that may be differentially

[5]The CBSAs not covered in the CPS can also affect the coefficients estimated using QCEW data.

[6]County-level QCEW data are aggregated to the CBSA level using the HUD crosswalk.

[7]The CPS suspended in-person interviewing due to the COVID-19 outbreak. Response rates plummeted in March 2020 and remained low through the summer. In-person interviewing resumed nationally in September 2020 and earlier in some areas. According to DK, correcting for the pandemic-induced distortions strengthens coefficients on the shared delayed. See their Appendix B for their approach to reweighting based on individuals' industry and occupation rather than geography.

[8]DK present some specifications with state-by-month fixed effects in addition to those in their preferred specification, which include just individual and monthly fixed effects, two-digit NAICS, and occupation exposure to COVID-19 interacted with monthly FE. In some other specifications, they also include state-level non-pharmacological interventions and UI-related measures. All state-level controls are captured by our state-by-month FEs, included in all specifications.

affected by PPP funding delay (and for better comparison to our QCEW results): employees of private firms (that is, excluding the self-employed and public employees) and samples that exclude the top 1 percent most populous counties/CBSAs.

Table A.16 presents our findings. Columns (1) and (3) report coefficients for the county sample, including all individuals (column 1) versus private employees only (column 3), while column (9) is the counterpart to column (1) for the CBSA sample. These three regressions control for only individual and state-by-month fixed effects. The share delayed exhibits a persistently negative effect on employment, especially for the county sample. Estimates reported in all the other columns in Table A.16 further control for preexisting conditions, COVID-19–related factors, rebates, unemployment benefit replacement rates, and cumulative PPP funds received, as in our regressions using county-level employment. With these additional controls, the effects of the share delayed tend to decline and become much less persistent (specifically, compare columns 1 and 2, columns 5 and 6, and columns 9 and 10).[9] Furthermore, the estimated effects of the share delayed shrink noticeably and lose much of their significance when we focus on private employees (compare columns 2 and 3) and when we exclude the top 1 percent most populous counties (compare columns 2 and 4). These findings confirm that a funding delay was more detrimental to non-employer businesses than to employer businesses (in the CPS sample), as highlighted by DK, and in large metropolitan areas, as we found using the QCEW employment data.[10]

As a final comparison, we reestimate Equation (1) using county-level QCEW employment data for just the sample of counties that appear in the CPS to further understand how the different geographies covered in the two data sources (CPS versus QCEW) influence DK's findings. Figure 3 presents "normalized" coefficients, which are divided by average county population to make them more comparable to the individual-level regression coefficients produced using the CPS data.[11] We report estimates for unweighted regressions (our baseline) and weighted regressions (DK's setup). Only the weighted regressions produce statistically significant coefficients, which are also larger in magnitude, indicating that the effects of the share delayed are larger for more populous areas. Reassuringly, the coefficients from the weighted county-level regressions estimated using only the CPS counties are of similar magnitude to those obtained using the individual-level CPS data. Most important, however, is the fact that the persistent effect of the share delayed in the CPS sample, whether estimated using individual or county data, does not carry through to the larger full sample of counties regardless of weighting.[12]

The findings so far collectively indicate that the slower trajectory of employment recovery as a result of PPP delay that DK find was driven by the most populous

[9] In columns (5) through (7), the county sample is paired with CBSA-level controls to quantify the contribution of more precise local controls to the variation in the estimated coefficients between county-based and CBSA-based regressions.

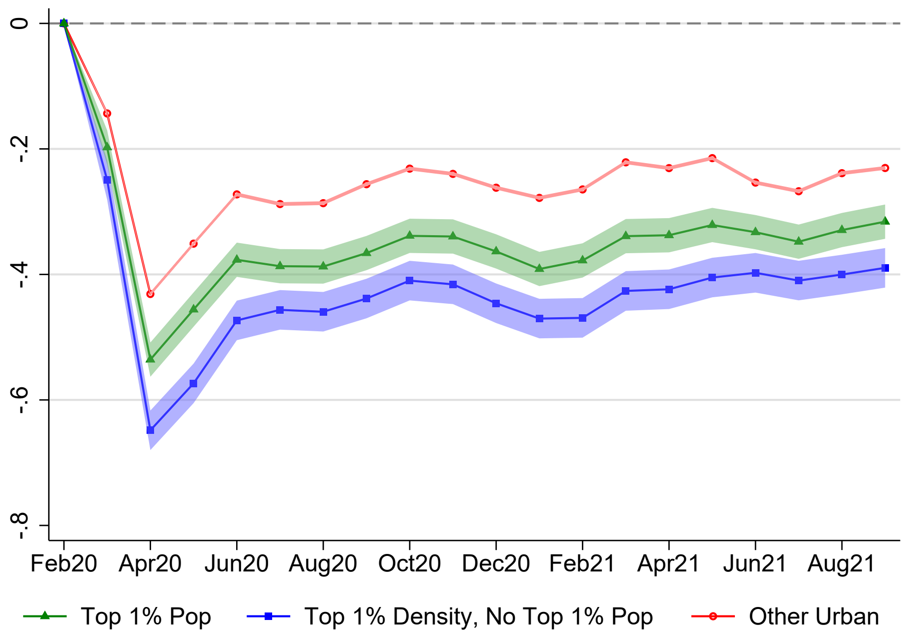
[10] As already noted, it is not obvious which employer-employee matches would be preserved for non-employer businesses.

[11] Recall that the dependent variable in the CPS regressions is a binary variable equal to one if an individual is employed and zero otherwise. The coefficients thus roughly have the scale of an effect on the fraction employed.

[12] See Appendix Table A.14 for the results using QCEW data on only the CPS counties and Table A.15 for those on counties not present in the CPS sample. It is clear that the impact of funding delay was driven by counties represented in the CPS sample.

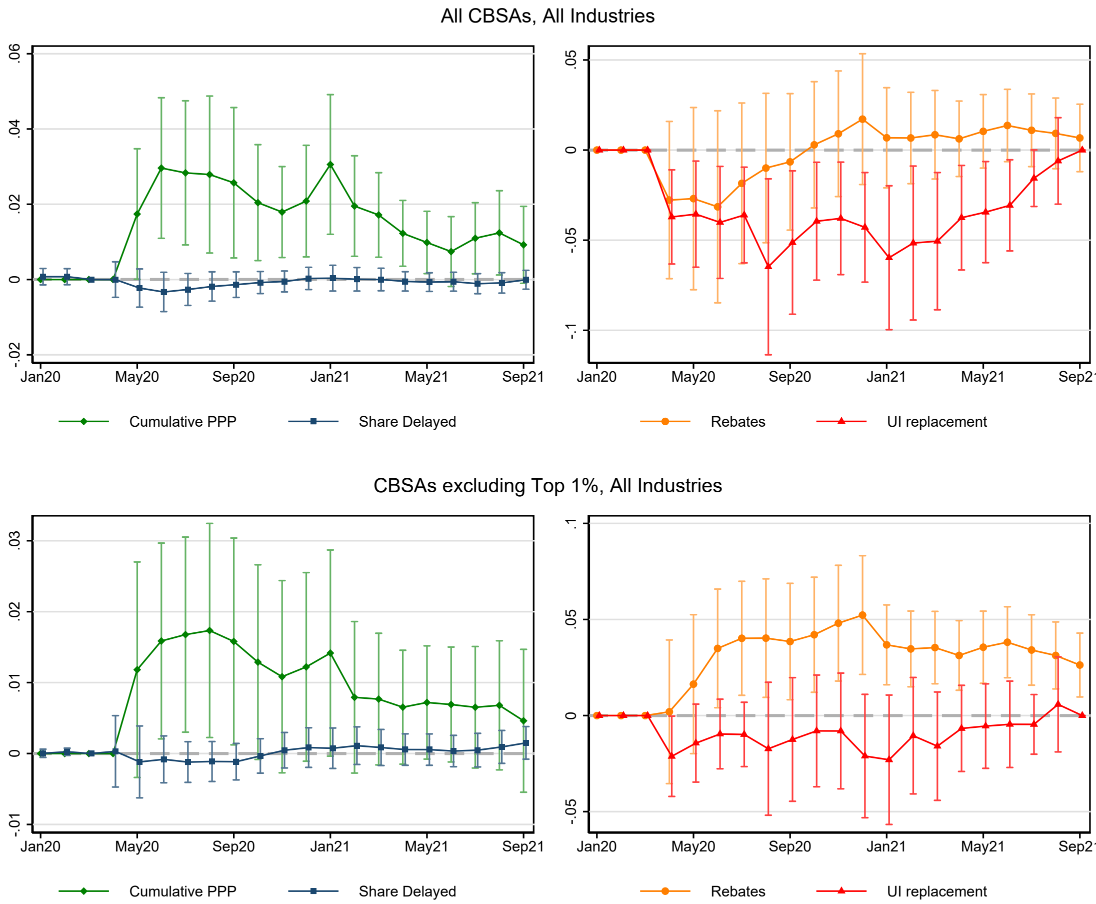
counties or cities. More importantly, however, this effect is not necessarily causal in that those larger metropolitan areas were more vulnerable to a highly infectious disease such as COVID-19 in ways that are not fully captured by linear functions of observable preexisting conditions.

Figure A.1: Time Spent at Workplaces since the COVID-19 Outbreak: The Roles of Population and Density



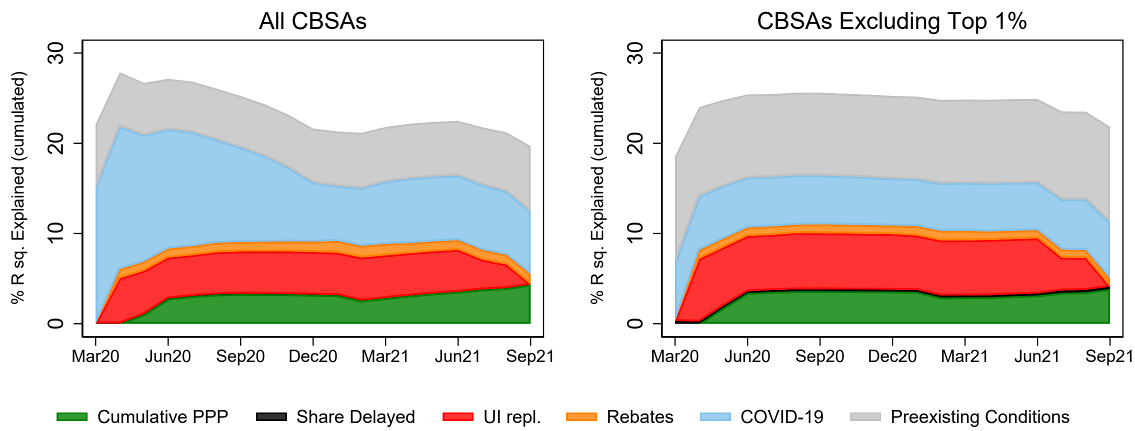
Notes: Dependent variable: fractional decline in time spent at workplaces in urban counties relative to pre-pandemic averages. County-level fixed effects are partialed out. There are only two urban counties in both the top 1% by population and the top 1% by density, and their coefficients are included in the top 1% by population.
Source: Google Mobility data provided by Opportunity Insights.

Figure A.2: Effects of Selected Controls on QCEW Private Employment: CBSA Regressions



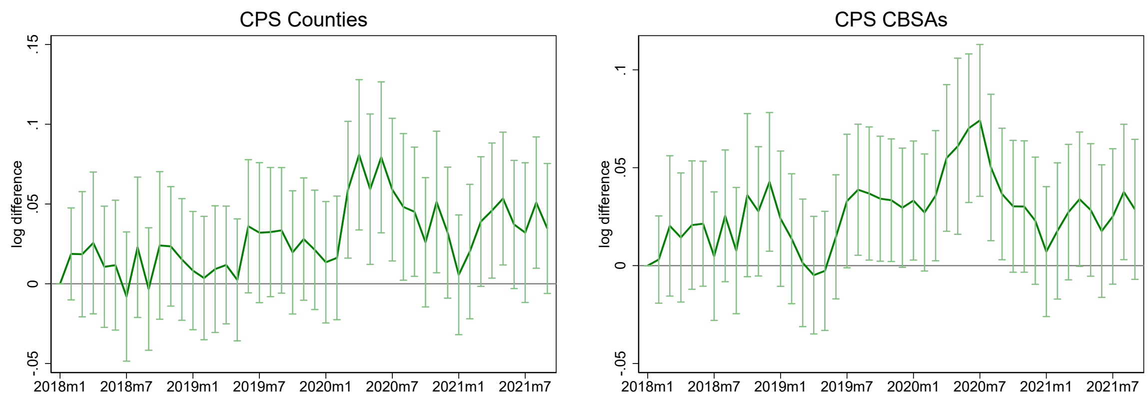
Notes: Estimated effects for a change of one standard deviation in a given control, normalized by the average corresponding CBSA-level employment in January 2020. Top 1% refers to population. The bottom graphs correspond to regressions that exclude CBSAs in the top 1 percent of the population distribution.
Source: Multiple data sources described in Section 2.2.

Figure A.3: R-Squared Decomposition, QCEW CBSA-Level Employment Regressions



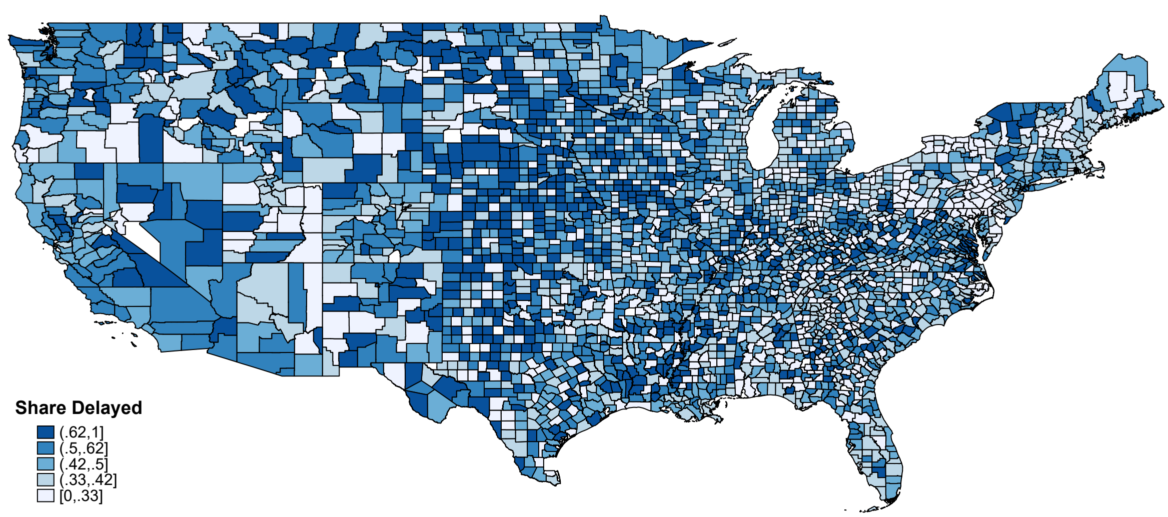
Notes: Contributions of different variables to explaining the variance in private employment over time. The effect of lagged employment is omitted in some graphs to more easily depict the contribution of other variables. Top 1% refers to population.
Source: Multiple data sources described in Section 2.2.

Figure A.4: Difference in Private Employment over Time: QCEW versus CPS Data



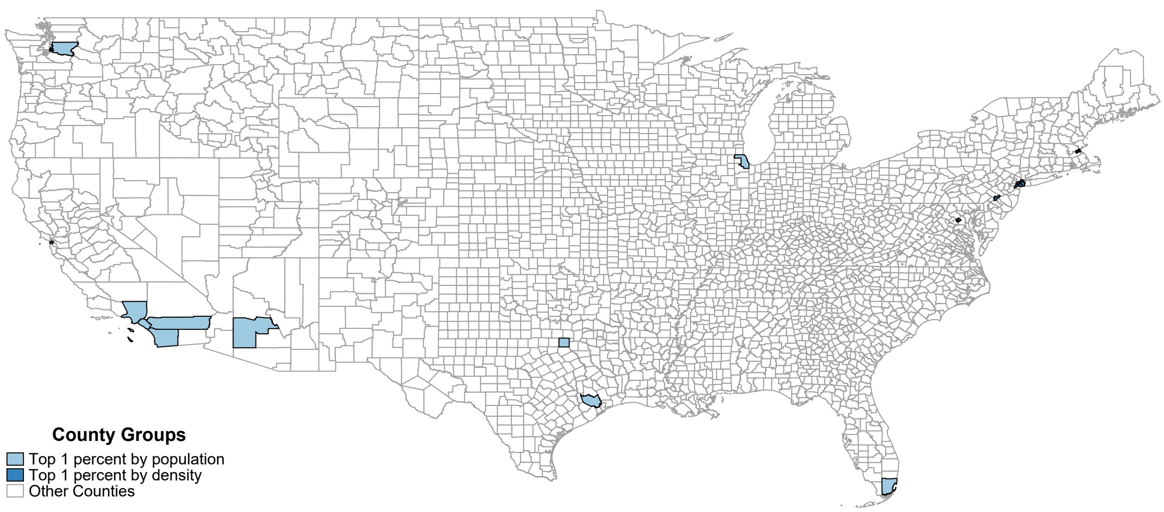
Notes: The figure depicts dummies from regressions of the log difference between county- or CBSA-level QCEW employment and CPS employment during the period depicted. The base period is January 2018. County or CBSA fixed effects are included in the regressions, and standard errors are adjusted for heteroskedasticity and autocorrelation. The county-level comparisons include 280 counties identified in the CPS, while the CBSA comparison covers 257 CBSAs.

Figure A.5: 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.6: Top Counties by Total Population and by Population Density



Notes: This map depicts the top 1 percent counties by population and by population density. The top 1 percent most populous counties are Maricopa County, AZ; Lo Angeles County, CA; Orange County, CA;Riverside County, CA; San Diego County, CA; Miami-Dade County, FL; Cook County, IL; Kings County, NY; Queen County, NY; Dallas County, TX, Harris County, TX; and King County, WA. The top 1 percent of counties by population density are San Francisco County, CA; Suffolk County, MA; Hudson County, NJ; Bronx County, NY; Kings County, NY; New York County, NY; Queens County, NY; Richmond County, NY; Philadelphia County, PA; Arlington County; and Alexandria, VA.
Source: Census.

Table A.1: Determinants of 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: "Smaller Urban" refers to urban counties excluding those in the top 1 percent by population.
Source: Multiple data sources described in Section 2.2.

Table A.2: Determinants of Share of PPP Loan Volume Delayed (with Population Density Indicator), April 16–26, 2020

	All	Urban	Smaller	Rural
Cum. COVID-19 Cases per bil up to 4/15/2020	0.016	0.064***	0.118***	–0.006
	(0.021)	(0.020)	(0.023)	(0.036)
Cum. COVID-19 Deaths per bil up to 4/15/2020	0.062	0.022	–0.127**	0.080
	(0.056)	(0.058)	(0.047)	(0.097)
Share of days in lockdown (pre-4/17/2020)	0.033	–0.018	–0.014	–0.036
	(0.056)	(0.070)	(0.069)	(0.068)
Share of days in lockdown (4/17–4/30/2020)	0.119	–0.054	–0.063	0.289***
	(0.144)	(0.133)	(0.135)	(0.072)
Share of Emp. in Essential Industries	–0.249	–0.433	–0.503	0.046
	(0.215)	(0.340)	(0.342)	(0.258)
Share of Emp. in Impacted Industries	–0.035	–0.126	–0.119	0.080
	(0.060)	(0.117)	(0.116)	(0.082)
Rural County Dummy	0.006			
	(0.010)			
Most Dense Urban County	0.038	0.044		
	(0.049)	(0.053)		
Ln Residential Population	–0.022***	–0.020***	–0.022***	–0.030***
	(0.005)	(0.007)	(0.007)	(0.010)
Commuter to Residential Population Ratio	–0.028	–0.020	–0.013	–0.030
	(0.027)	(0.030)	(0.031)	(0.062)
Ln Median Family Income	0.014	0.012	0.013	0.017
	(0.026)	(0.035)	(0.037)	(0.053)
Community Bank Share of Deposits	0.004	0.040**	0.041**	–0.008
	(0.014)	(0.016)	(0.015)	(0.019)
Big4 Bank Share of Deposits	0.070	0.116	0.102	0.052
	(0.071)	(0.093)	(0.088)	(0.074)
Ln Bank Branch Density	–0.005	–0.007	–0.009	–0.001
	(0.009)	(0.013)	(0.013)	(0.015)
SBL Vol. per Small Estabs. (< 500 Emp.) (CBP 2019Q1)	–0.002***	–0.002***	–0.002***	–0.002***
	(0.000)	(0.001)	(0.001)	(0.000)
Proportion of Small Employment in 2020Q1 to 2019Q1, QWI	0.000	–0.002	–0.002*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
UI Benefits Replacement Rate (Industry-Wtd.)	–0.028	–0.007	–0.026	–0.033
	(0.041)	(0.059)	(0.058)	(0.056)
Constant	0.774	1.260*	1.390**	0.307
	(0.493)	(0.672)	(0.685)	(0.717)
Adjusted R-squared	0.13	0.17	0.18	0.10
Observations	2644	1108	1097	1536
State FE	Yes	Yes	Yes	Yes

Notes: "Smaller" refers to urban counties excluding those in the top 1 percent by population density.
Source: Multiple data sources described in Section 2.2.

Table A.3: Effects of Share of PPP Loans Delayed on QCEW County Private Employment in Less Densely Populated Urban Counties

	Total Employment		NAICS 71, 72 and 82	
	(1)	(2)	(3)	(4)
Jan 2020 × Share of Volume Delayed	145	339*	113*	32
	(118)	(185)	(67)	(82)
Feb 2020 × Share of Volume Delayed	227	536**	112**	67
	(176)	(220)	(49)	(76)
Apr 2020 × Share of Volume Delayed	–779	125	38	55
	(920)	(913)	(221)	(231)
May 2020 × Share of Volume Delayed	–1,243	–408	–182	–66
	(897)	(949)	(330)	(338)
Jun 2020 × Share of Volume Delayed	–1,274	–817	–469	–323
	(834)	(851)	(386)	(380)
Jul 2020 × Share of Volume Delayed	–1,143	–790	–394	–265
	(827)	(829)	(409)	(397)
Aug 2020 × Share of Volume Delayed	–950	–644	–343	–215
	(825)	(835)	(441)	(422)
Sept 2020 × Share of Volume Delayed	–778	–547	–336	–232
	(783)	(790)	(381)	(357)
Oct 2020 × Share of Volume Delayed	–543	–322	–174	–101
	(697)	(714)	(306)	(288)
Nov 2020 × Share of Volume Delayed	–520	–293	–212	–122
	(691)	(721)	(296)	(285)
Dec 2020 × Share of Volume Delayed	–315	–57	–59	90
	(770)	(823)	(377)	(376)
Jan 2021 × Share of Volume Delayed	–446	–170	–255	–72
	(851)	(913)	(435)	(433)
Feb 2021 × Share of Volume Delayed	–319	–253	–261	–241
	(715)	(747)	(339)	(327)
Mar 2021 × Share of Volume Delayed	–399	–320	–264	–266
	(702)	(719)	(330)	(315)
Apr 2021 × Share of Volume Delayed	–180	–140	–91	–135
	(681)	(682)	(290)	(275)
May 2021 × Share of Volume Delayed	–108	–73	–73	–137
	(672)	(664)	(295)	(277)
Jun 2021 × Share of Volume Delayed	121	162	11	–71
	(720)	(703)	(326)	(307)
Jul 2021 × Share of Volume Delayed	6	58	105	–6
	(724)	(710)	(317)	(302)
Aug 2021 × Share of Volume Delayed	13	109	113	79
	(694)	(675)	(284)	(276)
Sept 2021 × Share of Volume Delayed	313	467	149	66
	(656)	(643)	(216)	(206)
Average Private Employment	87,100		12,776	
St. Dev. of Private Employment	206,184		30,046	
Within R-squared	0.87	0.88	0.90	0.91
Observations	23,205	23,205	23,205	23,205
County and State by Mth FE	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	No	Yes
CARES Act Controls	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. Regressions on a sample of urban counties excluding those in the top 1 percent by population density. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita. For employment in NAICS 71, 72, and 82, is specific to these impacted industries.
Source: Multiple data sources described in Section 2.2.

Table A.4: Effects of Share of PPP Loans Delayed on QCEW County Private Employment at Different Cutoffs - Urban Counties

	All		Top 99		Top 95		Top 90	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Jan 2020 × Share of Volume Delayed	270*	456*	100	202	25	–9	5	11
	(153)	(274)	(106)	(191)	(89)	(98)	(77)	(74)
Feb 2020 × Share of Volume Delayed	325*	556**	239**	371*	77	74	59	74
	(169)	(262)	(107)	(205)	(72)	(75)	(65)	(65)
Apr 2020 × Share of Volume Delayed	–3,037**	–2,175*	–1,688*	–1,196	128	173	184	207
	(1,428)	(1,300)	(931)	(888)	(548)	(546)	(427)	(422)
May 2020 × Share of Volume Delayed	–3,400**	–2,257*	–2,446**	–1,700*	–348	–336	–187	–210
	(1,462)	(1,231)	(1,213)	(1,020)	(514)	(511)	(385)	(385)
Jun 2020 × Share of Volume Delayed	–3,739**	–2,297*	–2,873*	–1,842	–472	–466	–299	–315
	(1,675)	(1,256)	(1,564)	(1,120)	(479)	(478)	(375)	(374)
Jul 2020 × Share of Volume Delayed	–3,383**	–1,946*	–2,672*	–1,626	–308	–304	–277	–291
	(1,607)	(1,182)	(1,596)	(1,095)	(446)	(450)	(350)	(352)
Aug 2020 × Share of Volume Delayed	–2,921*	–1,490	–2,400	–1,319	–176	–162	–189	–192
	(1,537)	(1,137)	(1,574)	(1,071)	(442)	(448)	(337)	(339)
Sept 2020 × Share of Volume Delayed	–2,449*	–1,149	–2,146	–1,149	–153	–183	–235	–274
	(1,471)	(1,068)	(1,536)	(1,042)	(431)	(434)	(331)	(333)
Oct 2020 × Share of Volume Delayed	–1,887	–725	–1,755	–815	–8	–70	–183	–247
	(1,419)	(1,011)	(1,493)	(1,014)	(422)	(425)	(306)	(306)
Nov 2020 × Share of Volume Delayed	–1,827	–680	–1,674	–705	55	–17	–136	–211
	(1,477)	(1,030)	(1,540)	(1,036)	(429)	(433)	(300)	(299)
Dec 2020 × Share of Volume Delayed	–1,485	–396	–1,461	–569	141	44	–84	–184
	(1,481)	(1,076)	(1,548)	(1,063)	(458)	(460)	(314)	(313)
Jan 2021 × Share of Volume Delayed	–1,938	–720	–1,772	–851	110	7	–127	–228
	(1,584)	(1,171)	(1,662)	(1,146)	(469)	(482)	(316)	(322)
Feb 2021 × Share of Volume Delayed	–1,875	–871	–1,676	–887	122	–10	–124	–209
	(1,593)	(1,078)	(1,637)	(1,131)	(441)	(437)	(308)	(311)
Mar 2021 × Share of Volume Delayed	–1,809	–783	–1,719	–904	50	–70	–137	–202
	(1,556)	(1,044)	(1,604)	(1,093)	(427)	(415)	(301)	(301)
Apr 2021 × Share of Volume Delayed	–1,264	–286	–1,333	–545	129	7	–113	–143
	(1,413)	(967)	(1,506)	(1,022)	(423)	(403)	(323)	(329)
May 2021 × Share of Volume Delayed	–1,194	–217	–1,193	–421	322	226	61	58
	(1,388)	(932)	(1,449)	(979)	(404)	(383)	(292)	(299)
Jun 2021 × Share of Volume Delayed	–880	34	–860	–159	508	437	265	267
	(1,256)	(897)	(1,300)	(903)	(422)	(410)	(324)	(335)
Jul 2021 × Share of Volume Delayed	–1,479	–457	–1,134	–382	398	419	223	297
	(1,296)	(900)	(1,315)	(936)	(438)	(436)	(359)	(370)
Aug 2021 × Share of Volume Delayed	–1,617	–576	–1,182	–470	350	386	161	238
	(1,315)	(902)	(1,291)	(923)	(428)	(425)	(349)	(356)
Sept 2021 × Share of Volume Delayed	–932	–6	–702	–81	520	544	307	351
	(1,141)	(807)	(1,099)	(827)	(387)	(376)	(305)	(303)
Average Private Employment	91,195		76,404		55,804		42,245	
St. Dev. of Private Employment	215,187		138,117		78,752		50,533	
Within R-squared	0.85	0.86	0.78	0.80	0.77	0.77	0.73	0.75
Observations	23,436	23,436	23,184	23,184	22,239	22,239	21,063	21,063
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Top 99" refers to urban counties excluding those in the top 1 percent by population. "Top 95" refers to urban counties excluding those in the top 5 percent by population. "Top 90" refers to urban counties excluding those in the top 10 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 wages in essential industries, and share of wages in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita.
Source: Multiple data sources described in Section 2.2.

Table A.5: Effects of Share of PPP Loans Delayed on QCEW County Private Employment in NAICS 71, 72, and 81

	All Counties				Urban		Smaller		Rural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Jan 2020 × Share Delayed	25	24	24	24	159**	141	115	94	3	6*
	(17)	(31)	(31)	(30)	(80)	(127)	(71)	(130)	(3)	(4)
Feb 2020 × Share Delayed	27*	28	28	28	153**	144	115*	93	1	4
	(14)	(30)	(30)	(30)	(67)	(124)	(62)	(126)	(2)	(3)
Apr 2020 × Share Delayed	–4	1	–25	–25	–207	–211	–63	–71	15	13
	(87)	(76)	(78)	(78)	(433)	(351)	(311)	(277)	(16)	(16)
May 2020 × Share Delayed	–16	–19	–41	–47	–164	–22	–114	49	14	9
	(113)	(106)	(107)	(106)	(527)	(440)	(418)	(365)	(17)	(16)
Jun 2020 × Share Delayed	–117	–102	–131	–141	–692	–456	–577	–308	18	15
	(150)	(131)	(134)	(133)	(770)	(604)	(635)	(508)	(16)	(15)
Jul 2020 × Share Delayed	–160	–136	–156	–174	–696	–387	–656	–354	8	7
	(149)	(129)	(133)	(132)	(764)	(592)	(671)	(517)	(13)	(13)
Aug 2020 × Share Delayed	–150	–121	–132	–154	–603	–318	–620	–354	6	6
	(142)	(123)	(127)	(126)	(718)	(572)	(645)	(507)	(13)	(12)
Sept 2020 × Share Delayed	–116	–96	–106	–125	–512	–269	–522	–282	6	7
	(131)	(113)	(116)	(115)	(661)	(520)	(588)	(462)	(12)	(11)
Oct 2020 × Share Delayed	–91	–77	–87	–104	–442	–274	–452	–270	10	10
	(115)	(99)	(101)	(101)	(579)	(464)	(512)	(412)	(11)	(10)
Nov 2020 × Share Delayed	–91	–87	–100	–117	–473	–359	–455	–306	6	5
	(113)	(102)	(104)	(104)	(578)	(465)	(512)	(408)	(11)	(10)
Dec 2020 × Share Delayed	–38	–37	–52	–79	–68	15	–173	–93	2	1
	(111)	(106)	(107)	(107)	(552)	(463)	(505)	(411)	(9)	(10)
Jan 2021 × Share Delayed	–50	–57	–73	–108	–193	–23	–368	–234	6	5
	(133)	(126)	(128)	(128)	(650)	(536)	(598)	(471)	(11)	(11)
Feb 2021 × Share Delayed	–91	–104	–123	–148	–469	–347	–546	–408	9	9
	(133)	(127)	(130)	(130)	(677)	(534)	(593)	(459)	(11)	(11)
Mar 2021 × Share Delayed	–96	–109	–129	–151	–518	–425	–574	–459	11	12
	(133)	(127)	(129)	(129)	(682)	(546)	(593)	(464)	(11)	(11)
Apr 2021 × Share Delayed	–97	–99	–122	–139	–477	–363	–508	–353	–6	–1
	(126)	(118)	(120)	(119)	(653)	(517)	(550)	(435)	(17)	(18)
May 2021 × Share Delayed	–98	–91	–117	–132	–487	–367	–486	–341	0	1
	(121)	(112)	(114)	(113)	(631)	(499)	(529)	(413)	(13)	(14)
Jun 2021 × Share Delayed	–82	–70	–93	–107	–334	–209	–307	–180	3	3
	(108)	(101)	(102)	(101)	(559)	(453)	(467)	(379)	(12)	(13)
Jul 2021 × Share Delayed	–54	–40	–58	–67	–327	–210	–260	–119	16	16
	(101)	(95)	(97)	(97)	(530)	(435)	(433)	(353)	(11)	(12)
Aug 2021 × Share Delayed	–53	–41	–62	–68	–319	–116	–236	–53	14	13
	(95)	(91)	(94)	(93)	(497)	(415)	(396)	(336)	(11)	(11)
Sept 2021 × Share Delayed	–19	–10	–28	–32	–190	–173	–127	–121	16	16*
	(74)	(71)	(71)	(71)	(385)	(326)	(288)	(260)	(10)	(10)
Average Private Employment	5,666				13,272		11,221		939	
St. Dev. of Private Employment	20,173				31,065		20,276		1,451	
Within R-squared	0.89	0.89	0.89	0.89	0.89	0.90	0.85	0.86	0.75	0.76
Observations	61,173	61,173	61,173	61,173	23,436	23,436	23,184	23,184	37,716	37,716
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed **specific** to NAICS 71, 72, and 81 industries. **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 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.
Source: Multiple data sources described in Section 2.2.

Table A.6: Effects of Share of PPP Loans Delayed on QCEW County Private Employment in NAICS 71, 72, and 81: Non-Industry-Specific Share of PPP Loans Delayed

	All Counties				Urban		Smaller		Rural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Jan 2020 × Share Delayed	41**	71	71	70	215**	180	127	78	–0	2
	(21)	(46)	(46)	(46)	(93)	(118)	(82)	(104)	(4)	(4)
Feb 2020 × Share Delayed	50***	80*	80*	80*	220***	196*	139*	107	–4	–2
	(18)	(42)	(42)	(42)	(81)	(103)	(72)	(97)	(3)	(4)
Apr 2020 × Share Delayed	–35	14	11	11	–383	–211	–344	–213	–14	–9
	(91)	(69)	(69)	(69)	(409)	(296)	(347)	(268)	(18)	(18)
May 2020 × Share Delayed	–181	–122	–121	–118	–551	–233	–643	–356	–23	–18
	(112)	(92)	(92)	(92)	(504)	(401)	(457)	(388)	(20)	(20)
Jun 2020 × Share Delayed	–361**	–262**	–261**	–254**	–1,147	–558	–1,142	–613	–20	–13
	(156)	(114)	(114)	(114)	(719)	(510)	(695)	(506)	(17)	(17)
Jul 2020 × Share Delayed	–440***	–356***	–350***	–340***	–1,000	–344	–1,045	–475	–29*	–25
	(157)	(116)	(117)	(117)	(718)	(502)	(746)	(515)	(15)	(15)
Aug 2020 × Share Delayed	–438***	–366***	–362***	–352***	–817	–169	–932	–365	–31**	–28*
	(152)	(119)	(120)	(120)	(687)	(505)	(717)	(500)	(15)	(15)
Sept 2020 × Share Delayed	–375***	–313***	–309***	–301***	–722	–155	–842	–331	–25*	–22
	(139)	(107)	(108)	(108)	(631)	(454)	(656)	(458)	(14)	(14)
Oct 2020 × Share Delayed	–284**	–223**	–221**	–214**	–480	–15	–607	–178	–30***	–25**
	(121)	(91)	(91)	(91)	(556)	(402)	(572)	(403)	(12)	(12)
Nov 2020 × Share Delayed	–237**	–185**	–184**	–177*	–503	–49	–554	–122	–23*	–18
	(121)	(91)	(91)	(91)	(552)	(394)	(570)	(389)	(12)	(12)
Dec 2020 × Share Delayed	–228*	–177*	–176*	–163	–254	197	–354	38	–24*	–22*
	(124)	(103)	(103)	(103)	(553)	(450)	(564)	(409)	(13)	(13)
Jan 2021 × Share Delayed	–382***	–320***	–314***	–299**	–607	–7	–694	–214	–38***	–35**
	(146)	(121)	(121)	(121)	(641)	(506)	(666)	(461)	(14)	(14)
Feb 2021 × Share Delayed	–316**	–255**	–254**	–267**	–638	–198	–762	–380	–35***	–33**
	(142)	(106)	(106)	(106)	(638)	(439)	(662)	(442)	(13)	(13)
Mar 2021 × Share Delayed	–303**	–243**	–243**	–263**	–623	–190	–762	–382	–35***	–34***
	(141)	(102)	(103)	(102)	(640)	(432)	(661)	(436)	(13)	(13)
Apr 2021 × Share Delayed	–209	–142	–147	–172*	–299	74	–470	–137	–16	–24
	(135)	(96)	(96)	(95)	(637)	(440)	(629)	(429)	(20)	(21)
May 2021 × Share Delayed	–196	–126	–132	–159*	–256	100	–388	–74	–4	–12
	(128)	(92)	(92)	(90)	(612)	(421)	(610)	(409)	(19)	(21)
Jun 2021 × Share Delayed	–181	–122	–126	–153*	–76	241	–185	80	–11	–21
	(117)	(90)	(90)	(88)	(563)	(419)	(550)	(393)	(16)	(18)
Jul 2021 × Share Delayed	–161	–96	–102	–119	–140	166	–224	24	–13	–21
	(103)	(79)	(79)	(79)	(495)	(363)	(490)	(356)	(14)	(16)
Aug 2021 × Share Delayed	–139	–76	–90	–102	–152	225	–228	66	–13	–21
	(96)	(73)	(76)	(75)	(461)	(345)	(451)	(335)	(14)	(15)
Sept 2021 × Share Delayed	–63	–14	–20	–28	27	227	–93	51	–13	–17
	(77)	(60)	(60)	(61)	(361)	(268)	(332)	(254)	(12)	(13)
Average Private Employment	5,666				13,272		11,221		939	
St. Dev. of Private Employment	20,173				31,065		20,276		1,451	
Within R-squared	0.89	0.89	0.89	0.89	0.89	0.90	0.85	0.86	0.75	0.76
Observations	61,173	61,173	61,173	61,173	23,436	23,436	23,184	23,184	37,716	37,716
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita.
Source: Multiple data sources described in Section 2.2.

Table A.7: Determinants of Lending Delay, April 2020. CBSA Sample

	Share of Volume Delay		Share of Number Delay	
	(All)	(Smaller)	(All)	(Smaller)
Cum. COVID-19 Cases per billion up to 4/15/2020	–0.044	–0.044	–0.021	–0.024
	(0.044)	(0.044)	(0.024)	(0.023)
Cum. COVID-19 Deaths per billion up to 4/15/2020	0.874	0.808	0.648**	1.114**
	(0.529)	(1.102)	(0.279)	(0.442)
Share of days in lockdown (pre-4/17/2020)	–0.047	–0.049	0.002	–0.009
	(0.073)	(0.078)	(0.046)	(0.048)
Share of days in lockdown (4/17–4/30/2020)	0.024	0.024	0.007	0.011
	(0.110)	(0.111)	(0.074)	(0.075)
Share of Emp. in Essential Industries	–0.132	–0.137	–0.153	–0.143
	(0.271)	(0.267)	(0.203)	(0.201)
Share of Wages in Impacted Industries	–0.116	–0.118	–0.203**	–0.198**
	(0.084)	(0.083)	(0.085)	(0.084)
replacement_ind	0.073	0.074	0.069	0.069
	(0.075)	(0.076)	(0.055)	(0.055)
Most Populouse CBSA (Top 1%)	0.081***		0.038**	
	(0.019)		(0.017)	
Ln Residential Population	–0.013**	–0.013**	–0.015***	–0.015***
	(0.005)	(0.005)	(0.004)	(0.004)
Commuter to Residential Population Ratio	0.059	0.057	0.142**	0.143**
	(0.078)	(0.079)	(0.060)	(0.061)
Ln Median Family Income	0.029	0.029	0.029	0.030
	(0.055)	(0.056)	(0.033)	(0.033)
Community Bank Share of Deposits	–0.016	–0.016	–0.005	–0.005
	(0.023)	(0.023)	(0.023)	(0.023)
Big4 Bank Share of Deposits	–0.156	–0.156	–0.115	–0.116
	(0.132)	(0.132)	(0.081)	(0.081)
Ln Bank Branch Density	–0.013	–0.013	–0.013	–0.013
	(0.020)	(0.020)	(0.012)	(0.012)
SBL Volume per Small Estab. ($< 500 Emp.$) (CBP 2019Q1)	–0.003***	–0.003***	–0.001***	–0.001***
	(0.001)	(0.001)	(0.000)	(0.000)
Ratio of Small Employment in 2020Q1 to 2019Q1, QWI	0.190*	0.190*	0.075	0.074
	(0.109)	(0.109)	(0.072)	(0.071)
Constant	0.292	0.296	0.444	0.431
	(0.789)	(0.798)	(0.417)	(0.422)
Adjusted R-squared	0.14	0.14	0.32	0.33
Observations	874	865	874	865
State FE	Yes	Yes	Yes	Yes

Notes: "Smaller" refers to CBSAs excluding those in the top 1 percent by population.

Table A.8: Effects of Share of PPP Loans Delayed on QCEW Private Employment: CBSA Regressions

	Total						NAICS 71, 72, and 81 Industries					
	All			Smaller			All			Smaller		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Jan 2020 × Share Delayed	336	775	790	109	24	27	250	415	405	–85	–99	–97
	(231)	(1,170)	(1,168)	(161)	(235)	(230)	(242)	(367)	(357)	(125)	(137)	(136)
Feb 2020 × Share Delayed	312	780	792	281**	193	190	300	424	421	–4	–28	–25
	(300)	(1,147)	(1,145)	(129)	(207)	(203)	(200)	(333)	(325)	(90)	(102)	(101)
Apr 2020 × Share Delayed	–2,415	–872	–21	338	–77	240	389	1,562***	1,614***	956**	1,006**	1,023**
	(3,312)	(2,566)	(2,533)	(1,870)	(2,018)	(1,987)	(876)	(588)	(582)	(475)	(504)	(488)
May 2020 × Share Delayed	–3,571	–3,801	–2,391	–692	–1,401	–915	696	1,446*	1,642**	601	521	641
	(3,348)	(2,842)	(2,722)	(1,782)	(2,014)	(2,001)	(982)	(864)	(835)	(707)	(752)	(719)
Jun 2020 × Share Delayed	–4,828	–5,386*	–3,479	–821	–1,112	–635	–947	224	602	–67	–88	62
	(3,530)	(3,063)	(2,798)	(1,232)	(1,267)	(1,305)	(1,452)	(1,127)	(1,070)	(793)	(824)	(787)
Jul 2020 × Share Delayed	–4,432	–4,546*	–2,778	–1,293	–1,419	–931	–945	–400	–108	–398	–365	–264
	(2,912)	(2,448)	(2,279)	(1,069)	(1,096)	(1,133)	(1,008)	(901)	(851)	(676)	(694)	(670)
Aug 2020 × Share Delayed	–3,918	–3,721*	–1,951	–1,194	–1,371	–873	–537	–327	8	–384	–385	–247
	(2,633)	(2,155)	(2,089)	(1,038)	(1,064)	(1,112)	(906)	(906)	(861)	(678)	(701)	(677)
Sept 2020 × Share Delayed	–3,264	–2,905	–1,435	–1,068	–1,308	–890	–234	–78	200	–272	–272	–152
	(2,186)	(1,812)	(1,821)	(960)	(972)	(1,016)	(779)	(796)	(757)	(617)	(643)	(619)
Oct 2020 × Share Delayed	–2,340	–1,952	–843	–302	–573	–262	–98	117	311	–241	–235	–149
	(1,964)	(1,583)	(1,571)	(933)	(931)	(959)	(655)	(670)	(643)	(527)	(548)	(530)
Nov 2020 × Share Delayed	–2,214	–1,544	–542	224	68	361	19	342	520	–60	–21	58
	(1,950)	(1,530)	(1,487)	(986)	(981)	(988)	(617)	(617)	(594)	(459)	(471)	(459)
Dec 2020 × Share Delayed	–1,418	–851	299	417	216	650	950	1,305	1,521*	115	242	361
	(1,873)	(1,571)	(1,584)	(1,105)	(1,094)	(1,104)	(760)	(810)	(793)	(588)	(592)	(582)
Jan 2021 × Share Delayed	–2,250	–1,145	372	266	167	579	615	1,086	1,442	39	147	273
	(2,314)	(1,848)	(1,834)	(1,114)	(1,107)	(1,127)	(951)	(961)	(953)	(618)	(618)	(609)
Feb 2021 × Share Delayed	–2,375	–1,142	66	805	631	859	87	623	857	6	86	151
	(2,464)	(1,767)	(1,676)	(1,066)	(1,049)	(1,048)	(863)	(797)	(774)	(527)	(532)	(518)
Mar 2021 × Share Delayed	–2,323	–1,114	1	532	401	660	–115	427	633	–125	–50	–0
	(2,322)	(1,671)	(1,605)	(1,019)	(1,000)	(1,004)	(868)	(771)	(743)	(510)	(513)	(500)
Apr 2021 × Share Delayed	–2,025	–1,282	–524	460	260	434	1	456	568	–249	–177	–154
	(2,068)	(1,435)	(1,379)	(896)	(879)	(875)	(842)	(717)	(689)	(500)	(502)	(492)
May 2021 × Share Delayed	–2,018	–1,330	–725	474	276	429	–21	342	357	–278	–201	–239
	(2,007)	(1,391)	(1,327)	(889)	(876)	(872)	(753)	(670)	(643)	(465)	(461)	(458)
Jun 2021 × Share Delayed	–1,475	–1,111	–616	350	136	273	53	350	310	–288	–194	–260
	(1,757)	(1,381)	(1,345)	(867)	(871)	(873)	(681)	(670)	(643)	(478)	(476)	(472)
Jul 2021 × Share Delayed	–2,060	–1,315	–1,164	475	295	370	95	329	252	–80	–5	–71
	(2,075)	(1,468)	(1,430)	(931)	(934)	(935)	(584)	(589)	(567)	(458)	(458)	(450)
Aug 2021 × Share Delayed	–2,274	–1,226	–944	696	616	722	–40	173	192	–53	3	–20
	(2,238)	(1,511)	(1,456)	(918)	(912)	(916)	(536)	(536)	(533)	(419)	(420)	(420)
Sept 2021 × Share Delayed	–1,273	–295	–77	1,123	1,099	1,163	165	367	318	82	119	81
	(1,921)	(1,372)	(1,343)	(907)	(899)	(907)	(448)	(445)	(431)	(337)	(334)	(330)
Average Private Employment	123,224			91,460			18,021			13,622		
St. Dev. of Private Employment	430,927			223,528			60,322			32,258		
Within R-squared	0.92	0.93	0.93	0.86	0.86	0.86	0.93	0.94	0.94	0.89	0.89	0.89
Observations	18,333	18,333	18,333	18,165	18,165	18,165	18,333	18,333	18,333	18,165	18,165	18,165
CBSA and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
CARES Act Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to CBSAs excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of CBSA-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita. *Source:* Multiple data sources described in Section 2.2.

Table A.9: Effects of Share of PPP Loans Delayed on QCEW County Private Employment: Log Employment as Dependent Variable

	All Counties			Urban		Smaller Urban		Rural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Jan 2020 × Share of Volume Delayed	−0.007**	−0.008**	−0.008**	0.004	0.003	0.004	0.002	−0.011***	−0.010***
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Feb 2020 × Share of Volume Delayed	0.001	0.000	0.000	0.005	0.004	0.005	0.004	−0.001	−0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Apr 2020 × Share of Volume Delayed	0.014	0.013	0.012	0.048**	0.041**	0.050***	0.043**	0.003	0.004
	(0.009)	(0.009)	(0.009)	(0.019)	(0.018)	(0.019)	(0.018)	(0.010)	(0.010)
May 2020 × Share of Volume Delayed	0.015*	0.014*	0.015*	0.027	0.026	0.028	0.027	0.010	0.012
	(0.008)	(0.008)	(0.008)	(0.018)	(0.018)	(0.018)	(0.018)	(0.010)	(0.009)
Jun 2020 × Share of Volume Delayed	0.007	0.007	0.008	0.009	0.012	0.010	0.013	0.006	0.007
	(0.007)	(0.007)	(0.007)	(0.014)	(0.014)	(0.014)	(0.014)	(0.009)	(0.009)
Jul 2020 × Share of Volume Delayed	0.014*	0.014*	0.016*	0.002	0.005	0.003	0.006	0.017*	0.019**
	(0.008)	(0.008)	(0.008)	(0.014)	(0.014)	(0.014)	(0.014)	(0.010)	(0.010)
Aug 2020 × Share of Volume Delayed	0.010	0.010	0.012	0.002	0.005	0.003	0.006	0.012	0.014
	(0.008)	(0.008)	(0.008)	(0.013)	(0.013)	(0.013)	(0.013)	(0.010)	(0.010)
Sept 2020 × Share of Volume Delayed	0.009	0.009	0.011	0.003	0.006	0.004	0.007	0.010	0.012
	(0.008)	(0.008)	(0.008)	(0.013)	(0.014)	(0.013)	(0.014)	(0.009)	(0.009)
Oct 2020 × Share of Volume Delayed	0.006	0.006	0.007	0.010	0.013	0.011	0.013	0.004	0.006
	(0.008)	(0.008)	(0.008)	(0.014)	(0.014)	(0.014)	(0.014)	(0.010)	(0.010)
Nov 2020 × Share of Volume Delayed	0.008	0.008	0.009	0.012	0.013	0.013	0.014	0.006	0.008
	(0.008)	(0.008)	(0.008)	(0.014)	(0.014)	(0.014)	(0.014)	(0.010)	(0.009)
Dec 2020 × Share of Volume Delayed	0.011	0.009	0.011	0.012	0.011	0.012	0.011	0.009	0.011
	(0.008)	(0.008)	(0.008)	(0.013)	(0.013)	(0.013)	(0.013)	(0.009)	(0.009)
Jan 2021 × Share of Volume Delayed	0.021**	0.021**	0.022**	0.007	0.009	0.007	0.009	0.023**	0.026**
	(0.009)	(0.009)	(0.009)	(0.016)	(0.015)	(0.016)	(0.015)	(0.011)	(0.011)
Feb 2021 × Share of Volume Delayed	0.013	0.013	0.015	0.007	0.008	0.007	0.008	0.014	0.017
	(0.010)	(0.009)	(0.009)	(0.015)	(0.014)	(0.015)	(0.014)	(0.012)	(0.011)
Mar 2021 × Share of Volume Delayed	0.012	0.012	0.013	0.000	0.002	0.000	0.002	0.014	0.017
	(0.010)	(0.010)	(0.009)	(0.015)	(0.014)	(0.015)	(0.015)	(0.012)	(0.012)
Apr 2021 × Share of Volume Delayed	0.003	0.004	0.005	−0.009	−0.004	−0.009	−0.005	0.007	0.008
	(0.011)	(0.011)	(0.011)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)	(0.013)
May 2021 × Share of Volume Delayed	0.003	0.004	0.005	−0.003	0.002	−0.004	0.001	0.005	0.006
	(0.012)	(0.012)	(0.012)	(0.015)	(0.014)	(0.015)	(0.014)	(0.015)	(0.015)
Jun 2021 × Share of Volume Delayed	0.009	0.011	0.010	0.012	0.017	0.012	0.017	0.010	0.010
	(0.013)	(0.013)	(0.013)	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)	(0.016)
Jul 2021 × Share of Volume Delayed	0.004	0.006	0.005	0.012	0.018	0.012	0.018	0.002	0.002
	(0.015)	(0.014)	(0.014)	(0.019)	(0.018)	(0.019)	(0.018)	(0.018)	(0.017)
Aug 2021 × Share of Volume Delayed	0.006	0.008	0.008	0.017	0.022	0.017	0.022	0.003	0.003
	(0.015)	(0.015)	(0.015)	(0.021)	(0.020)	(0.021)	(0.020)	(0.018)	(0.017)
Sept 2021 × Share of Volume Delayed	0.008	0.010	0.010	0.015	0.020	0.015	0.020	0.006	0.007
	(0.014)	(0.014)	(0.014)	(0.019)	(0.018)	(0.019)	(0.018)	(0.017)	(0.017)
Average Private Employment	8.93			10.14		10.10		8.18	
St. Dev. of Private Employment	1.70			1.69		1.65		1.20	
Within R-squared	0.16	0.21	0.22	0.16	0.26	0.16	0.26	0.15	0.20
Observations	61,173	61,173	61,173	23,436	23,436	23,184	23,184	37,716	37,716
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita. *Source:* Multiple data sources described in Section 2.2.

Table A.10: Effects of Share of PPP Loans Delayed on QCEW County Private Employment in NAICS 71, 72, and 81: Log Employment as Dependent Variable

	All Counties			Urban		Smaller Urban		Rural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Jan 2020 × Share of Volume Delayed	0.009	0.007	0.007	0.026*	0.017	0.025*	0.017	0.006	0.006
	(0.010)	(0.010)	(0.010)	(0.014)	(0.013)	(0.014)	(0.013)	(0.012)	(0.012)
Feb 2020 × Share of Volume Delayed	0.004	0.003	0.003	0.010	0.004	0.009	0.004	0.003	0.003
	(0.008)	(0.008)	(0.008)	(0.012)	(0.012)	(0.012)	(0.012)	(0.010)	(0.010)
Apr 2020 × Share of Volume Delayed	0.044*	0.045*	0.045*	0.041	0.036	0.042	0.038	0.043	0.045
	(0.023)	(0.024)	(0.024)	(0.039)	(0.039)	(0.039)	(0.040)	(0.028)	(0.028)
May 2020 × Share of Volume Delayed	0.022	0.023	0.025	0.026	0.032	0.026	0.032	0.021	0.025
	(0.018)	(0.018)	(0.018)	(0.036)	(0.036)	(0.037)	(0.037)	(0.020)	(0.020)
Jun 2020 × Share of Volume Delayed	0.012	0.015	0.017	0.020	0.031	0.021	0.032	0.009	0.015
	(0.016)	(0.016)	(0.016)	(0.033)	(0.034)	(0.034)	(0.034)	(0.019)	(0.019)
Jul 2020 × Share of Volume Delayed	−0.011	−0.009	−0.006	−0.028	−0.018	−0.027	−0.017	−0.009	−0.005
	(0.017)	(0.017)	(0.017)	(0.032)	(0.032)	(0.032)	(0.032)	(0.020)	(0.020)
Aug 2020 × Share of Volume Delayed	−0.028	−0.027	−0.024	−0.021	−0.016	−0.020	−0.015	−0.032	−0.028
	(0.017)	(0.017)	(0.017)	(0.029)	(0.029)	(0.029)	(0.029)	(0.021)	(0.020)
Sept 2020 × Share of Volume Delayed	−0.017	−0.015	−0.013	−0.019	−0.018	−0.018	−0.017	−0.019	−0.015
	(0.018)	(0.018)	(0.018)	(0.031)	(0.030)	(0.031)	(0.030)	(0.022)	(0.021)
Oct 2020 × Share of Volume Delayed	−0.025	−0.024	−0.022	−0.023	−0.021	−0.023	−0.021	−0.025	−0.021
	(0.017)	(0.017)	(0.017)	(0.030)	(0.031)	(0.030)	(0.031)	(0.021)	(0.021)
Nov 2020 × Share of Volume Delayed	−0.013	−0.013	−0.012	−0.007	−0.007	−0.006	−0.006	−0.015	−0.013
	(0.018)	(0.018)	(0.018)	(0.034)	(0.035)	(0.034)	(0.035)	(0.020)	(0.020)
Dec 2020 × Share of Volume Delayed	−0.008	−0.008	−0.007	0.004	0.005	0.004	0.004	−0.012	−0.009
	(0.018)	(0.018)	(0.018)	(0.033)	(0.035)	(0.033)	(0.035)	(0.021)	(0.021)
Jan 2021 × Share of Volume Delayed	−0.016	−0.015	−0.013	−0.033	−0.028	−0.032	−0.028	−0.012	−0.009
	(0.022)	(0.022)	(0.022)	(0.042)	(0.043)	(0.043)	(0.043)	(0.026)	(0.026)
Feb 2021 × Share of Volume Delayed	−0.017	−0.016	−0.015	−0.012	−0.013	−0.012	−0.013	−0.016	−0.014
	(0.022)	(0.022)	(0.022)	(0.039)	(0.041)	(0.039)	(0.042)	(0.026)	(0.026)
Mar 2021 × Share of Volume Delayed	−0.015	−0.014	−0.013	0.004	0.008	0.004	0.008	−0.020	−0.018
	(0.023)	(0.023)	(0.023)	(0.040)	(0.041)	(0.040)	(0.041)	(0.027)	(0.027)
Apr 2021 × Share of Volume Delayed	−0.025	−0.022	−0.021	−0.043	−0.034	−0.045	−0.036	−0.018	−0.015
	(0.029)	(0.029)	(0.029)	(0.048)	(0.049)	(0.049)	(0.049)	(0.034)	(0.034)
May 2021 × Share of Volume Delayed	−0.007	−0.004	−0.004	−0.013	−0.003	−0.012	−0.003	−0.002	−0.001
	(0.025)	(0.025)	(0.025)	(0.049)	(0.048)	(0.049)	(0.048)	(0.030)	(0.030)
Jun 2021 × Share of Volume Delayed	0.007	0.009	0.009	0.012	0.016	0.012	0.015	0.011	0.011
	(0.023)	(0.023)	(0.023)	(0.040)	(0.039)	(0.040)	(0.039)	(0.028)	(0.028)
Jul 2021 × Share of Volume Delayed	0.002	0.002	0.003	0.021	0.022	0.019	0.021	−0.000	−0.001
	(0.023)	(0.023)	(0.023)	(0.039)	(0.038)	(0.039)	(0.038)	(0.028)	(0.028)
Aug 2021 × Share of Volume Delayed	0.016	0.017	0.017	0.021	0.025	0.021	0.025	0.016	0.015
	(0.023)	(0.023)	(0.023)	(0.042)	(0.042)	(0.043)	(0.042)	(0.027)	(0.027)
Sept 2021 × Share of Volume Delayed	0.030	0.030	0.030	0.038	0.042	0.037	0.041	0.031	0.030
	(0.025)	(0.025)	(0.025)	(0.043)	(0.043)	(0.044)	(0.044)	(0.030)	(0.029)
Average Private Employment	6.42			7.99		7.95		5.44	
St. Dev. of Private Employment	2.58			2.24		2.21		2.27	
Within R-squared	0.96	0.96	0.96	0.95	0.96	0.95	0.96	0.97	0.97
Observations	61,173	61,173	61,173	23,436	23,436	23,184	23,184	37,716	37,716
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. “Smaller” refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates (“stimulus checks”) per capita.
Source: Multiple data sources described in Section 2.2.

Table A.11: Effects of Share of PPP Loans Delayed on QCEW County Private Employment Growth Rate

	All Counties			Urban		Smaller Urban		Rural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Jan 2020 × Share of Volume Delayed	–0.010**	–0.010**	–0.010**	0.009	0.009	0.009	0.008	–0.014***	–0.015***
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)
Feb 2020 × Share of Volume Delayed	0.001	0.000	0.000	0.013***	0.013***	0.013***	0.012***	–0.002	–0.003
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Apr 2020 × Share of Volume Delayed	0.017*	0.016*	0.016*	0.054***	0.049***	0.056***	0.051***	0.005	0.007
	(0.009)	(0.009)	(0.009)	(0.019)	(0.018)	(0.019)	(0.018)	(0.010)	(0.010)
May 2020 × Share of Volume Delayed	0.019**	0.018**	0.019**	0.036**	0.035**	0.037**	0.037**	0.014	0.016
	(0.009)	(0.009)	(0.009)	(0.018)	(0.018)	(0.018)	(0.018)	(0.010)	(0.010)
Jun 2020 × Share of Volume Delayed	0.011	0.011	0.012	0.018	0.019	0.019	0.021	0.009	0.011
	(0.008)	(0.008)	(0.008)	(0.014)	(0.014)	(0.014)	(0.014)	(0.009)	(0.009)
Jul 2020 × Share of Volume Delayed	0.021**	0.021**	0.022***	0.004	0.004	0.006	0.005	0.024**	0.027***
	(0.008)	(0.008)	(0.008)	(0.015)	(0.015)	(0.015)	(0.015)	(0.010)	(0.010)
Aug 2020 × Share of Volume Delayed	0.021**	0.020**	0.022***	0.015	0.014	0.016	0.016	0.021**	0.024**
	(0.009)	(0.008)	(0.008)	(0.013)	(0.013)	(0.013)	(0.013)	(0.010)	(0.010)
Sept 2020 × Share of Volume Delayed	0.018**	0.018**	0.020**	0.011	0.011	0.012	0.012	0.019*	0.021**
	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)	(0.014)	(0.011)	(0.010)
Oct 2020 × Share of Volume Delayed	0.011	0.011	0.012	0.009	0.011	0.010	0.012	0.011	0.013
	(0.009)	(0.009)	(0.009)	(0.016)	(0.016)	(0.016)	(0.016)	(0.011)	(0.011)
Nov 2020 × Share of Volume Delayed	0.011	0.010	0.011	0.012	0.013	0.013	0.014	0.010	0.012
	(0.010)	(0.010)	(0.010)	(0.015)	(0.015)	(0.016)	(0.015)	(0.012)	(0.011)
Dec 2020 × Share of Volume Delayed	0.013	0.012	0.014	0.014	0.013	0.014	0.013	0.012	0.015
	(0.009)	(0.009)	(0.009)	(0.015)	(0.015)	(0.015)	(0.015)	(0.011)	(0.011)
Jan 2021 × Share of Volume Delayed	0.031***	0.031***	0.032***	0.015	0.019	0.016	0.020	0.034**	0.036***
	(0.011)	(0.011)	(0.011)	(0.017)	(0.016)	(0.017)	(0.016)	(0.014)	(0.014)
Feb 2021 × Share of Volume Delayed	0.019	0.019	0.020*	0.015	0.017	0.016	0.018	0.019	0.022
	(0.012)	(0.012)	(0.012)	(0.017)	(0.017)	(0.017)	(0.017)	(0.015)	(0.015)
Mar 2021 × Share of Volume Delayed	0.017	0.018	0.019	0.011	0.013	0.011	0.014	0.019	0.022
	(0.012)	(0.012)	(0.012)	(0.017)	(0.017)	(0.017)	(0.017)	(0.015)	(0.015)
Apr 2021 × Share of Volume Delayed	–0.000	0.001	0.001	–0.033	–0.024	–0.035	–0.026	0.010	0.010
	(0.015)	(0.015)	(0.015)	(0.023)	(0.022)	(0.023)	(0.022)	(0.018)	(0.018)
May 2021 × Share of Volume Delayed	–0.001	0.000	0.000	–0.012	–0.005	–0.014	–0.007	0.003	0.003
	(0.015)	(0.015)	(0.015)	(0.021)	(0.021)	(0.021)	(0.021)	(0.018)	(0.018)
Jun 2021 × Share of Volume Delayed	0.010	0.011	0.011	0.016	0.020	0.015	0.019	0.011	0.010
	(0.015)	(0.015)	(0.015)	(0.018)	(0.017)	(0.018)	(0.017)	(0.018)	(0.018)
Jul 2021 × Share of Volume Delayed	0.001	0.003	0.002	0.019	0.023	0.019	0.023	–0.004	–0.004
	(0.016)	(0.015)	(0.015)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)
Aug 2021 × Share of Volume Delayed	0.008	0.009	0.009	0.027	0.030	0.027	0.029	0.003	0.002
	(0.016)	(0.016)	(0.016)	(0.022)	(0.021)	(0.022)	(0.021)	(0.019)	(0.019)
Sept 2021 × Share of Volume Delayed	0.010	0.011	0.011	0.023	0.027	0.023	0.026	0.007	0.007
	(0.015)	(0.015)	(0.016)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)
Average Private Employment	–0.02			–0.02		–0.02		–0.02	
St. Dev. of Private Employment	0.09			0.09		0.09		0.09	
Within R-squared	0.03	0.08	0.09	0.06	0.17	0.06	0.17	0.03	0.07
Observations	61,173	61,173	61,173	23,436	23,436	23,184	23,184	37,716	37,716
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita.
Source: Multiple data sources described in Section 2.2.

Table A.12: Effects of Share of PPP Loans Delayed on QCEW County Private Employment Growth Rate in NAICS 71, 82, and 81 Industries

	All Counties			Urban		Smaller Urban		Rural	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Jan 2020 × Share of Volume Delayed	0.008	0.007	0.007	0.024*	0.016	0.023*	0.015	0.006	0.006
	(0.010)	(0.010)	(0.010)	(0.014)	(0.013)	(0.014)	(0.013)	(0.012)	(0.012)
Feb 2020 × Share of Volume Delayed	0.003	0.003	0.003	0.009	0.003	0.008	0.003	0.003	0.003
	(0.008)	(0.008)	(0.008)	(0.012)	(0.012)	(0.012)	(0.012)	(0.010)	(0.010)
Apr 2020 × Share of Volume Delayed	0.068***	0.068***	0.068***	0.098**	0.086*	0.098**	0.087*	0.055*	0.058*
	(0.026)	(0.026)	(0.026)	(0.044)	(0.044)	(0.044)	(0.045)	(0.031)	(0.031)
May 2020 × Share of Volume Delayed	0.039**	0.039**	0.041**	0.067*	0.063	0.066	0.063	0.029	0.035
	(0.019)	(0.019)	(0.019)	(0.041)	(0.041)	(0.041)	(0.041)	(0.022)	(0.022)
Jun 2020 × Share of Volume Delayed	0.023	0.025	0.027	0.047	0.052	0.048	0.053	0.015	0.021
	(0.017)	(0.017)	(0.017)	(0.035)	(0.035)	(0.035)	(0.035)	(0.019)	(0.019)
Jul 2020 × Share of Volume Delayed	–0.003	–0.002	0.001	–0.006	–0.002	–0.005	–0.000	–0.005	–0.001
	(0.018)	(0.017)	(0.017)	(0.033)	(0.033)	(0.033)	(0.033)	(0.021)	(0.020)
Aug 2020 × Share of Volume Delayed	–0.021	–0.020	–0.018	–0.000	–0.000	0.000	0.001	–0.028	–0.024
	(0.017)	(0.017)	(0.017)	(0.030)	(0.029)	(0.030)	(0.029)	(0.021)	(0.021)
Sept 2020 × Share of Volume Delayed	–0.011	–0.010	–0.008	0.001	–0.003	0.002	–0.002	–0.016	–0.012
	(0.018)	(0.018)	(0.018)	(0.032)	(0.031)	(0.032)	(0.031)	(0.022)	(0.022)
Oct 2020 × Share of Volume Delayed	–0.017	–0.017	–0.014	–0.020	–0.020	–0.020	–0.020	–0.017	–0.013
	(0.018)	(0.018)	(0.018)	(0.030)	(0.031)	(0.030)	(0.031)	(0.021)	(0.021)
Nov 2020 × Share of Volume Delayed	–0.006	–0.006	–0.005	–0.004	–0.005	–0.004	–0.005	–0.008	–0.005
	(0.018)	(0.018)	(0.018)	(0.034)	(0.035)	(0.034)	(0.035)	(0.021)	(0.021)
Dec 2020 × Share of Volume Delayed	–0.001	–0.001	0.000	0.009	0.007	0.008	0.006	–0.005	–0.002
	(0.018)	(0.018)	(0.018)	(0.033)	(0.035)	(0.033)	(0.035)	(0.021)	(0.021)
Jan 2021 × Share of Volume Delayed	–0.011	–0.010	–0.008	–0.025	–0.021	–0.024	–0.021	–0.009	–0.006
	(0.023)	(0.023)	(0.023)	(0.042)	(0.042)	(0.042)	(0.043)	(0.026)	(0.026)
Feb 2021 × Share of Volume Delayed	–0.011	–0.011	–0.010	–0.005	–0.006	–0.005	–0.007	–0.012	–0.011
	(0.023)	(0.023)	(0.023)	(0.038)	(0.041)	(0.039)	(0.041)	(0.027)	(0.027)
Mar 2021 × Share of Volume Delayed	–0.012	–0.011	–0.010	0.009	0.012	0.009	0.012	–0.017	–0.015
	(0.023)	(0.023)	(0.023)	(0.039)	(0.041)	(0.040)	(0.041)	(0.027)	(0.028)
Apr 2021 × Share of Volume Delayed	–0.033	–0.030	–0.029	–0.069	–0.054	–0.070	–0.055	–0.017	–0.015
	(0.029)	(0.029)	(0.029)	(0.050)	(0.050)	(0.050)	(0.050)	(0.035)	(0.035)
May 2021 × Share of Volume Delayed	–0.013	–0.009	–0.009	–0.027	–0.012	–0.026	–0.011	–0.003	–0.003
	(0.026)	(0.026)	(0.026)	(0.049)	(0.048)	(0.049)	(0.048)	(0.031)	(0.031)
Jun 2021 × Share of Volume Delayed	0.003	0.005	0.005	0.006	0.012	0.006	0.012	0.009	0.009
	(0.023)	(0.023)	(0.023)	(0.040)	(0.039)	(0.040)	(0.039)	(0.028)	(0.028)
Jul 2021 × Share of Volume Delayed	0.002	0.003	0.003	0.019	0.024	0.019	0.023	–0.001	–0.001
	(0.024)	(0.024)	(0.024)	(0.037)	(0.037)	(0.038)	(0.037)	(0.029)	(0.029)
Aug 2021 × Share of Volume Delayed	0.017	0.019	0.018	0.020	0.027	0.021	0.027	0.017	0.016
	(0.023)	(0.023)	(0.023)	(0.041)	(0.040)	(0.041)	(0.041)	(0.028)	(0.028)
Sept 2021 × Share of Volume Delayed	0.034	0.035	0.035	0.039	0.047	0.039	0.047	0.033	0.033
	(0.025)	(0.025)	(0.025)	(0.042)	(0.042)	(0.043)	(0.042)	(0.030)	(0.030)
Average Private Employment	–0.04			–0.06		–0.06		–0.03	
St. Dev. of Private Employment	0.22			0.24		0.24		0.21	
Within R-squared	0.13	0.16	0.16	0.21	0.28	0.21	0.27	0.07	0.10
Observations	61,173	61,173	61,173	23,436	23,436	23,184	23,184	37,716	37,716
County and State by Mth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
CARES Act Controls	No	Yes	Yes	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita.
Source: Multiple data sources described in Section 2.2.

Table A.13: Determinants of Lending Delay, April 2020. CPS Sample

	Share of Volume Delay			Share of Number Delay		
	(All)	(Urban)	(Smaller)	(All)	(Urban)	(Smaller)
Cum. COVID-19 Cases per billion up to 4/15/2020	0.046*	0.048*	0.047*	0.051*	0.053**	0.053**
	(0.026)	(0.025)	(0.025)	(0.025)	(0.024)	(0.024)
Cum. COVID-19 Deaths per billion up to 4/15/2020	0.143***	0.139***	0.139***	0.098***	0.094***	0.093***
	(0.027)	(0.029)	(0.029)	(0.026)	(0.027)	(0.027)
Share of days in lockdown (pre-4/17/2020)	0.017	0.019	0.021	-0.054	-0.050	-0.052
	(0.154)	(0.155)	(0.156)	(0.065)	(0.065)	(0.067)
Share of days in lockdown (4/17–4/30/2020)	-0.725***	-0.746***	-0.743***	-0.245	-0.284	-0.289
	(0.198)	(0.209)	(0.210)	(0.279)	(0.298)	(0.293)
Share of Emp. in Essential Industries	-0.382	-0.330	-0.329	0.015	0.063	0.060
	(0.574)	(0.559)	(0.557)	(0.555)	(0.544)	(0.545)
Share of Wages in Impacted Industries	0.024	-0.005	-0.010	-0.014	-0.052	-0.049
	(0.162)	(0.167)	(0.168)	(0.137)	(0.137)	(0.140)
UI Benefits Replacement Rate (Industry-Wtd.)	0.047	0.050	0.050	0.062	0.071	0.071
	(0.048)	(0.049)	(0.049)	(0.044)	(0.046)	(0.047)
Rural County Dummy	-0.015			-0.030		
	(0.032)			(0.031)		
Ln Residential Population	-0.001	-0.001	-0.001	-0.002	-0.002	-0.001
	(0.013)	(0.013)	(0.013)	(0.009)	(0.009)	(0.011)
Commuter to Residential Population Ratio	-0.003	-0.004	-0.004	0.014	0.014	0.014
	(0.053)	(0.053)	(0.053)	(0.042)	(0.041)	(0.041)
Ln Median Family Income	0.043	0.041	0.042	0.014	0.012	0.011
	(0.035)	(0.034)	(0.034)	(0.025)	(0.025)	(0.025)
Community Bank Share of Deposits	0.053	0.045	0.045	0.048	0.039	0.039
	(0.044)	(0.045)	(0.045)	(0.037)	(0.038)	(0.038)
Big4 Bank Share of Deposits	0.533*	0.542*	0.533*	0.425**	0.413*	0.416*
	(0.280)	(0.290)	(0.292)	(0.194)	(0.207)	(0.209)
Ln Bank Branch Density	0.060**	0.058**	0.059**	0.017	0.015	0.015
	(0.027)	(0.028)	(0.028)	(0.018)	(0.019)	(0.020)
SBL Volume per Small Estab. ($< 500 Emp.$) (CBP 2019Q1)	-0.001	-0.001	-0.001	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Proportion of Small Employment in 2020Q1 to 2019Q1, QWI	0.002	0.002	0.002	0.001	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Proportion of Small Estabs in 2020Q1 to 2019Q1, CBP	-0.001	-0.001	-0.001	0.004	0.005	0.005
	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
Constant	0.431	0.392	0.377	-0.059	-0.082	-0.081
	(1.365)	(1.333)	(1.338)	(1.096)	(1.086)	(1.088)
Adjusted R-squared	0.45	0.44	0.44	0.51	0.50	0.50
Observations	279	273	270	279	273	270
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: "Smaller" refers to counties excluding those in the top 1 percent by population.

Table A.14: Effects of Share of PPP Loans Delayed on QCEW Private Employment: CPS Sample

	All Counties			Urban		Smaller Urban	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Jan 2020 × Share Delayed	496	–98	–547	518	–607	461	–585
	(994)	(1,575)	(1,517)	(1,016)	(1,535)	(933)	(1,525)
Feb 2020 × Share Delayed	1,823	856	485	1,679	338	2,134***	1,513
	(1,499)	(1,759)	(1,666)	(1,543)	(1,708)	(769)	(1,235)
Apr 2020 × Share Delayed	–15,460*	–14,243	–13,329	–14,715	–13,120	–11,185	–9,456
	(8,855)	(9,176)	(9,156)	(9,052)	(9,314)	(7,883)	(8,258)
May 2020 × Share Delayed	–17,560*	–12,118	–9,384	–17,263	–10,023	–11,838	–5,159
	(10,385)	(10,114)	(9,863)	(10,633)	(9,928)	(8,338)	(8,217)
Jun 2020 × Share Delayed	–22,714*	–14,821	–13,336	–23,083*	–14,285	–18,002*	–9,322
	(12,461)	(11,034)	(10,890)	(12,800)	(11,053)	(10,356)	(9,195)
Jul 2020 × Share Delayed	–23,085**	–15,619	–14,692	–23,392**	–15,500	–19,831**	–11,637
	(11,217)	(9,473)	(9,466)	(11,551)	(9,672)	(9,816)	(8,257)
Aug 2020 × Share Delayed	–22,218**	–14,122	–13,400	–22,597**	–14,383	–18,971**	–10,478
	(10,961)	(9,068)	(9,076)	(11,292)	(9,309)	(9,630)	(7,893)
Sept 2020 × Share Delayed	–20,097*	–11,979	–11,282	–20,441*	–12,260	–16,743*	–8,369
	(10,688)	(8,793)	(8,757)	(11,000)	(8,972)	(9,314)	(7,545)
Oct 2020 × Share Delayed	–16,337	–8,969	–7,791	–16,656	–8,599	–12,284	–4,156
	(10,602)	(8,688)	(8,536)	(10,917)	(8,766)	(8,686)	(7,053)
Nov 2020 × Share Delayed	–16,954	–9,972	–8,416	–17,349	–9,127	–12,241	–4,066
	(11,413)	(9,165)	(8,933)	(11,768)	(9,226)	(8,886)	(7,132)
Dec 2020 × Share Delayed	–14,750	–7,818	–6,362	–14,790	–6,827	–9,935	–1,838
	(11,003)	(8,991)	(8,859)	(11,347)	(9,164)	(8,741)	(7,178)
Jan 2021 × Share Delayed	–15,484	–8,082	–7,202	–15,644	–7,952	–11,197	–3,235
	(11,414)	(9,570)	(9,464)	(11,765)	(9,787)	(9,323)	(7,876)
Feb 2021 × Share Delayed	–16,976	–10,464	–8,843	–17,447	–9,672	–11,502	–3,716
	(12,710)	(10,357)	(10,093)	(13,101)	(10,449)	(9,517)	(7,883)
Mar 2021 × Share Delayed	–15,776	–9,355	–7,794	–16,164	–8,456	–10,505	–2,741
	(12,188)	(9,878)	(9,647)	(12,569)	(9,985)	(9,172)	(7,559)
Apr 2021 × Share Delayed	–12,037	–5,918	–4,918	–12,402	–5,514	–7,377	–374
	(11,000)	(8,792)	(8,587)	(11,382)	(8,918)	(8,576)	(7,106)
May 2021 × Share Delayed	–12,066	–6,587	–5,158	–12,396	–5,549	–7,647	–642
	(10,904)	(8,625)	(8,382)	(11,280)	(8,701)	(8,385)	(6,831)
Jun 2021 × Share Delayed	–11,692	–6,430	–5,140	–11,693	–5,193	–7,820	–1,151
	(8,967)	(7,235)	(7,081)	(9,258)	(7,315)	(7,293)	(5,911)
Jul 2021 × Share Delayed	–14,310	–9,988	–8,373	–14,321	–8,378	–10,202	–4,210
	(9,470)	(7,483)	(7,370)	(9,798)	(7,640)	(7,363)	(5,942)
Aug 2021 × Share Delayed	–15,319	–11,486	–9,430	–15,417	–9,515	–10,703	–4,695
	(9,878)	(7,819)	(7,647)	(10,215)	(7,924)	(7,267)	(5,936)
Sept 2021 × Share Delayed	–11,219	–7,986	–6,369	–11,293	–6,531	–6,807	–2,204
	(8,316)	(6,715)	(6,540)	(8,587)	(6,754)	(6,106)	(5,089)
Average Private Employment	194,470			197,888		175,234	
St. Dev. of Private Employment	321,895			324,671		220,779	
Within R-squared	0.90	0.91	0.91	0.90	0.91	0.84	0.86
Observations	5,733	5,733	5,733	5,607	5,607	5,544	5,544
County and State-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	No	Yes	No	Yes
CARES Act Controls	No	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita.
Source: Multiple data sources described in Section 2.2.

Table A.15: Effects of the Share of PPP Loans Delayed on QCEW Private Employment: Non-CPS Sample.

	All Counties			Urban		Smaller Urban	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Jan 2020 × Share Delayed	–9	–53	–51	–9	–98	–39	–37
	(21)	(41)	(43)	(85)	(190)	(68)	(68)
Feb 2020 × Share Delayed	15	22	22	31	52	–21	–8
	(17)	(19)	(19)	(67)	(72)	(57)	(57)
Apr 2020 × Share Delayed	207	192	204	495	342	495	422
	(131)	(135)	(138)	(496)	(529)	(431)	(453)
May 2020 × Share Delayed	72	70	82	8	–35	300	281
	(150)	(157)	(159)	(623)	(639)	(455)	(473)
Jun 2020 × Share Delayed	–40	–63	–49	–175	–238	214	157
	(134)	(138)	(140)	(588)	(587)	(431)	(431)
Jul 2020 × Share Delayed	–113	–140	–125	–193	–216	248	207
	(112)	(113)	(115)	(476)	(469)	(397)	(391)
Aug 2020 × Share Delayed	–113	–137	–128	–104	–143	339	281
	(107)	(109)	(111)	(451)	(449)	(376)	(370)
Sept 2020 × Share Delayed	–119	–145	–134	–143	–194	231	162
	(108)	(109)	(112)	(456)	(453)	(378)	(372)
Oct 2020 × Share Delayed	–73	–96	–84	–96	–97	228	205
	(114)	(116)	(119)	(479)	(474)	(350)	(344)
Nov 2020 × Share Delayed	–35	–63	–50	–159	–91	244	258
	(133)	(135)	(139)	(567)	(557)	(340)	(337)
Dec 2020 × Share Delayed	1	–37	–25	–20	60	391	413
	(143)	(145)	(148)	(611)	(617)	(389)	(392)
Jan 2021 × Share Delayed	–25	–63	–52	–79	–22	431	415
	(149)	(153)	(156)	(647)	(660)	(393)	(399)
Feb 2021 × Share Delayed	36	–3	–6	82	41	519	487
	(145)	(147)	(151)	(610)	(617)	(378)	(383)
Mar 2021 × Share Delayed	16	–23	–32	46	–3	411	407
	(135)	(137)	(142)	(582)	(581)	(362)	(362)
Apr 2021 × Share Delayed	–88	–114	–133	–66	–142	258	283
	(119)	(119)	(123)	(516)	(501)	(335)	(328)
May 2021 × Share Delayed	–37	–59	–85	133	25	434	451
	(114)	(115)	(118)	(494)	(476)	(326)	(322)
Jun 2021 × Share Delayed	44	30	5	523	438	763**	797**
	(103)	(104)	(107)	(444)	(431)	(358)	(358)
Jul 2021 × Share Delayed	76	67	38	506	457	808**	880**
	(117)	(119)	(122)	(508)	(495)	(396)	(397)
Aug 2021 × Share Delayed	97	91	70	498	442	760*	804**
	(117)	(118)	(122)	(506)	(495)	(390)	(393)
Sept 2021 × Share Delayed	156	159	137	669	688	786**	898**
	(108)	(110)	(113)	(465)	(455)	(353)	(349)
Average Private Employment	22,307			56,128		45,077	
St. Dev. of Private Employment	87,868			149,646		86,914	
Within R-squared	0.89	0.89	0.89	0.89	0.90	0.85	0.85
Observations	55,314	55,314	55,314	17,661	17,661	17,472	17,472
County and State-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lag of Dependent Var	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Preexisting Conditions Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	Yes	Yes	No	Yes	No	Yes
CARES Act Controls	No	No	Yes	No	Yes	No	Yes
Cum PPP per Emp in Small Estab (t-1)	No	No	Yes	No	Yes	No	Yes

Notes: Standard errors clustered at the county level in parentheses. "Smaller" refers to urban counties excluding those in the top 1 percent by population. Share Delayed is the share (by volume) of county-level PPP loans delayed as defined in Equation (2). **Preexisting Conditions Controls**: median family income, commuter-to-residential-population ratio, indicators for 2013 NCHS urban-rural designation, population, 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 employment in essential industries, and share of employment in most impacted industries; **CARES Act Controls**: industry-employment-share-weighted UI benefits replacement rate, and rebates ("stimulus checks") per capita.
Source: Multiple data sources described in Section 2.2.

Table A.16: Effects of Share of PPP Loans Delayed on Employment, CPS Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)	(10)
	Respondents with County							Resp. with CBSA	
Employment	All	All	Private Employees	All	All	All	All	All	All
Jan20 × Shared Delayed	–0.039	–0.070**	–0.064*	–0.055*	–0.033	–0.049	–0.043	0.010	–0.004
	(0.033)	(0.030)	(0.036)	(0.031)	(0.030)	(0.031)	(0.033)	(0.023)	(0.027)
Feb20 × Shared Delayed	0.002	–0.024	–0.025	–0.003	0.013	0.004	0.013	0.033*	0.033
	(0.034)	(0.033)	(0.034)	(0.033)	(0.036)	(0.040)	(0.040)	(0.018)	(0.024)
Apr20 × Shared Delayed	–0.091**	–0.062	–0.059	–0.028	–0.048	–0.029	–0.003	–0.013	0.018
	(0.038)	(0.041)	(0.038)	(0.045)	(0.043)	(0.045)	(0.047)	(0.036)	(0.045)
May20 × Shared Delayed	–0.172***	–0.131***	–0.091**	–0.115**	–0.116***	–0.069	–0.060	–0.108***	–0.037
	(0.042)	(0.045)	(0.042)	(0.048)	(0.044)	(0.044)	(0.046)	(0.032)	(0.035)
Jun20 × Shared Delayed	–0.086*	–0.047	0.008	0.012	–0.069	–0.027	0.010	–0.021	0.021
	(0.050)	(0.051)	(0.054)	(0.054)	(0.043)	(0.052)	(0.056)	(0.030)	(0.043)
Jul20 × Shared Delayed	–0.133**	–0.116**	–0.072	–0.063	–0.099**	–0.070	–0.037	–0.032	–0.003
	(0.056)	(0.050)	(0.057)	(0.052)	(0.041)	(0.047)	(0.054)	(0.028)	(0.040)
Aug20 × Shared Delayed	–0.122***	–0.095*	–0.068	–0.058	–0.109**	–0.067	–0.050	–0.061**	–0.032
	(0.046)	(0.049)	(0.057)	(0.051)	(0.044)	(0.049)	(0.053)	(0.029)	(0.039)
Sep20 × Shared Delayed	–0.121***	–0.090**	–0.028	–0.058	–0.101**	–0.057	–0.038	–0.068**	0.027
	(0.039)	(0.044)	(0.055)	(0.049)	(0.048)	(0.054)	(0.055)	(0.033)	(0.041)
Oct20 × Shared Delayed	–0.086**	–0.061	–0.017	–0.037	–0.072	–0.031	–0.019	–0.031	0.037
	(0.036)	(0.041)	(0.052)	(0.045)	(0.047)	(0.049)	(0.052)	(0.030)	(0.039)
Nov20 × Shared Delayed	–0.057	–0.050	–0.030	–0.023	–0.021	0.007	0.019	0.018	0.103**
	(0.040)	(0.044)	(0.051)	(0.047)	(0.050)	(0.054)	(0.055)	(0.034)	(0.042)
Dec20 × Shared Delayed	–0.082**	–0.081*	–0.064	–0.059	–0.091**	–0.063	–0.054	–0.018	0.030
	(0.041)	(0.044)	(0.049)	(0.047)	(0.043)	(0.048)	(0.050)	(0.031)	(0.040)
Jan21 × Shared Delayed	–0.081*	–0.078	–0.072	–0.058	–0.113**	–0.090*	–0.071	–0.025	0.016
	(0.044)	(0.047)	(0.045)	(0.053)	(0.051)	(0.054)	(0.054)	(0.034)	(0.042)
Feb21 × Shared Delayed	–0.093**	–0.071	–0.057	–0.068	–0.060	–0.028	–0.020	–0.012	0.017
	(0.042)	(0.047)	(0.047)	(0.051)	(0.050)	(0.054)	(0.054)	(0.035)	(0.040)
Mar21 × Shared Delayed	–0.054	–0.053	–0.054	–0.058	–0.065	–0.045	–0.041	0.008	0.002
	(0.044)	(0.047)	(0.050)	(0.049)	(0.054)	(0.052)	(0.053)	(0.036)	(0.040)
Apr21 × Shared Delayed	–0.069*	–0.042	–0.059	–0.038	–0.069*	–0.039	–0.031	–0.018	0.012
	(0.037)	(0.041)	(0.053)	(0.044)	(0.041)	(0.045)	(0.045)	(0.034)	(0.044)
May21 × Shared Delayed	–0.068*	–0.045	–0.039	–0.048	–0.057	–0.032	–0.023	–0.005	0.006
	(0.037)	(0.043)	(0.053)	(0.048)	(0.044)	(0.051)	(0.055)	(0.032)	(0.040)
Jun21 × Shared Delayed	–0.070	–0.058	–0.047	–0.053	–0.086*	–0.078	–0.060	–0.031	–0.013
	(0.044)	(0.047)	(0.054)	(0.052)	(0.048)	(0.056)	(0.060)	(0.033)	(0.045)
Jul21 × Shared Delayed	–0.079	–0.078	–0.012	–0.069	–0.080	–0.063	–0.057	–0.026	–0.030
	(0.052)	(0.056)	(0.062)	(0.061)	(0.074)	(0.077)	(0.079)	(0.042)	(0.044)
Aug21 × Shared Delayed	–0.068*	–0.059	–0.004	–0.068	–0.073	–0.062	–0.060	–0.017	–0.001
	(0.039)	(0.046)	(0.058)	(0.051)	(0.048)	(0.057)	(0.060)	(0.035)	(0.043)
Sep21 × Shared Delayed	–0.053	–0.030	0.003	–0.022	–0.021	–0.005	0.005	–0.008	–0.009
	(0.040)	(0.046)	(0.059)	(0.052)	(0.050)	(0.056)	(0.059)	(0.032)	(0.040)
Adj. R-squared	0.82	0.82	0.80	0.82	0.82	0.82	0.82	0.82	0.82
Within R-squared	0.0001	0.0008	0.0008	0.0009	0.0000	0.0008	0.0009	0.0000	0.0005
Observations	1,612,958	1,612,958	1,612,958	1,352,878	1,612,958	1,612,958	1,352,878	2,934,532	2,934,532
No. Clusters	280	280	280	272	157	157	155	257	257
Avg. Emp	0.60	0.60	0.45	0.60	0.60	0.60	0.60	0.61	0.61
Agg. Controls Level	County	County	County	County	CBSA	CBSA	CBSA	CBSA	CBSA
Additional Agg. Controls	NO	YES	YES	YES	NO	YES	YES	NO	YES
Excludes				Top 1% Population			Top 1% Population		

Notes: The left-hand side in these regressions is a binary variable for whether the individual reports being employed (any kind of employment, including self-employed and public sector workers) or being an employee in the private sector (column 3). All regressions include individual fixed effects and state-by-month fixed effects. Standard errors are clustered at the geography level corresponding to the aggregate controls as indicated in the table (that is, either county or CBSA level). Locality-level controls, when included, are the same as those included in our baseline county-level regressions in Table 3. Top 1% refers to counties in the top 1 percent by population, which are excluded in some regressions as noted.
Source: Multiple data sources described in Section 2.2.