The Impact of COVID-19 on Small Business Dynamics and Employment: Real-time Estimates With Homebase Data*

André Kurmann
Drexel University

Etienne Lalé
Université du Québec à Montréal

Lien Ta
Drexel University

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Abstract

We use data from Homebase to construct weekly estimates of the impact of the COVID-19 pandemic on small business dynamics and employment. Different from concurrent research, we match the Homebase establishment records to information on business activity from Google, Facebook, and Safegraph to distinguish business closings and openings from sample churn. This distinction is critical to estimate the effects of the pandemic on small business employment. We find four key results: (1) employment of small businesses in four of the hardest hit service sectors contracted more severely in the beginning of the pandemic than employment of larger businesses, but small businesses also rebounded more strongly and have on average recovered a higher share of job losses than larger businesses; (2) closings account for 70% of the initial decline in small business employment, but two thirds of closed businesses have reopened and the annual rate of closings is just slightly higher than prior to the pandemic; (3) new openings of small businesses constitute an important driver of the recovery but the annual rate of new openings is only about half the rate one year earlier (4) small business employment was affected more negatively in counties with delayed access to loans from the Paycheck Protection Program (PPP) and in counties where Federal Pandemic Unemployment Compensation (FPUC) was *less* generous relative to pre-pandemic earnings of likely recipients. Business closings account for a large part of these two effects. Our results dispel the popular notion that small businesses have on average been hurt harder by the pandemic than larger businesses. At the same time, our analysis suggests that PPP and FPUC helped to significantly mitigate the negative effects of the pandemic for small businesses by, respectively, alleviating financial constraints and stimulating demand for local services.

*First version: April 28, 2020. Kurmann: Drexel University, LeBow College of Business, School of Economics, 3220 Market Street, Philadelphia, PA 19104 (email: kurmann.andre@drexel.edu); Lalé: Université du Québec à Montréal, Department of Economics, C.P. 8888, Succ. centre ville, Montréal (QC), H3C 3P8 Canada (email: lale.etienne@uqam.ca); Ta: Drexel University, LeBow College of Business, School of Economics, 3220 Market Street, Philadelphia, PA 19104 (email: lien.t.ta@drexel.edu). We thank Homebase, Safegraph and CrowdTangle for generously making their data available. We also thank Ray Sandza for amazing support with the Homebase data; John Coglianese, Ryan Decker, Cynthia Doniger, Bart Hobjin, Chad Higdon-Topaz, Erik Hurst and Jay Stewart as well as seminar participants at Arizona State University, Drexel, the Deutsche Bundesbank, McGill, Placekey, the St. Louis Fed, UQÀM, Wharton, the North American Meetings of the Econometric Society, and the IZA workshop on labor market dynamics for comments. All errors are our own.

1 Introduction

The COVID-19 pandemic unfolded with tremendous speed and continues to affect the U.S. economy in unprecedented ways. Contrary to other recessions, the disruptions caused by the pandemic are concentrated in service sectors that require in-person interaction. Establishments with fewer than 50 employees account for about half of all jobs in these sectors. It is therefore important to understand the effect that the pandemic has on small businesses, especially because there is widespread concern that independently owned businesses have suffered more than larger multi-establishment corporations, and that a disproportionate number of small businesses have closed. To what extent this is the case remains an open question, however, since small business closing and opening rates are high even under the best of circumstances and official data on small business dynamics and employment are published only with considerable delay.[1] Furthermore, the official data once published is either at quarterly or annual frequency. This lack of timely high-frequency data makes it difficult to measure the current state of the economy and to assess the effects of economic policies aimed at mitigating the adverse impacts of the pandemic.

In this paper, we address this challenge by constructing weekly estimates of small business dynamics and employment using data from Homebase, a scheduling and time clock software provider used by more than 100,000 mostly small service-sector businesses in the U.S. We make three main contributions. First and different from the many other studies that use Homebase or other high-frequency establishment-level data, we match the Homebase records with information on business activity from Google, Facebook and Safegraph to distinguish business closings and openings from sample churn; i.e. already operating businesses entering the sample or businesses that continue to operate after exiting the sample. This distinction is critical to benchmark the data to official pre-pandemic statistics and to estimate the effect of the pandemic on small business employment. Indeed, counterfactual approaches that include either none of the entries and exits into Homebase or that treat all entries and exits as openings and closings would produce very different estimates of small business employment.

Second, we document that employment of small businesses in four of the hardest hit service sectors contracted more than twice as much in the beginning of the pandemic than employment of larger businesses, but small businesses also rebounded more strongly and have on average recovered a higher share of

[1] The Bureau of Labor Statistics (BLS) publishes monthly employment estimates by industry but these estimates are not available by establishment size class. Employment and establishment counts by industry and size class are only released late each year for the first quarter of that year. It will therefore take until late 2021 to obtain a clear picture of how the pandemic affected small businesses. In addition, each quarter the BLS publishes data on establishment openings and closings. Data for the third quarter of 2020 was published in late April 2021 but is not available for different size classes. Alternatively, the U.S. Census Bureau publishes establishment counts, employment, and entry and exit by detailed industry and size class but only with a lag of two to three years and at an annual frequency.

job losses than larger businesses. Business closings account for the majority of the larger initial contraction while reopenings and new openings drive the subsequent recovery, which illustrates the importance of properly distinguishing closings and openings from sample churn.

Third, we exploit the high frequency and geographic granularity of our data to assess the extent to which small business activity is affected by local differences in the timing and scope of two major federal economic policy responses that were enacted as part of the 2020 CARES Act: government loans to small businesses through the Paycheck Protection Program (PPP); and enhanced unemployment insurance (UI) benefits through Federal Pandemic Unemployment Compensation (FPUC). We find that both provisions exerted *net positive effects* on small business employment that are quantitatively large and persistent. Business closings account for an important part of these effects, suggesting that the speed and extent of economic support in response to large disruptions such as the pandemic is key for a quick recovery.

Aside from these substantive findings, our paper offers a cautionary tale about the increasing use of establishment datasets from private-sector providers to construct aggregate measures of employment and business activity. These datasets are opportunity samples that are often subject to large turnover and can result in substantial, time-varying gaps between sample entry and exit and true business openings and closings. This issue is important even for large establishment surveys such as the Current Employment Statistics (CES), which forms the basis of the BLS's monthly employment estimate.[2]

The remainder of the paper starts with a description of how we match the Homebase establishments by name and address to Google, Facebook, and Safegraph data to attribute a NAICS-6 industry code for each establishment and to estimate whether an establishment that enters or exits Homebase represents a business opening, respectively a business closing. We then use pre-pandemic data from the BLS's Business Employment Dynamics (BED) and the U.S. Census Bureau's Business Dynamics Statistics (BDS) to benchmark our estimator against population rates of small business births and deaths. We also compare our Homebase sample against administrative employment and establishment counts from the Quarterly Census of Employment and Wages (QCEW) by industry, size class, and region. We find that for four of the service sectors hit hardest by the pandemic – Retail Trade, Educational & Health, Leisure & Hospitality, and Other Services – the Homebase sample is surprisingly representative for establishments with fewer than 50 employees. We therefore focus our analysis on this segment of the economy, which accounted for 23% of total private sector employment and almost half of the four sectors' total employment

[2]As discussed in more detail below, the employment estimates from the CES historically did not directly take into account sample entry and exit and instead adjusted employment changes from business birth/death based on historical data. Faced with extraordinary numbers of business closings in the beginning of the pandemic, the BLS modified this procedure in April 2020 but without directly distinguishing openings and closings from sample churn.

prior to the pandemic.

We find four key results. First, small business employment in the four sectors contracted by an estimated 14 million between mid-February and mid-April 2020 – a staggering 46% decline – and then regained about 10 million by mid-June 2020. Between mid-June 2020 and the end of our sample in mid-May 2021, small business employment gradually recovered most of the losses and for Retail Trade and Leisure & Hospitality even exceeded their pre-pandemic levels. Both the large decline in the beginning of the pandemic and the subsequent rebound are larger than the estimates for total employment in the four sectors as estimated by the CES.[3] Our estimates therefore imply that employment by small businesses contracted more severely in the beginning of the pandemic than employment of larger businesses but then also rebounded more strongly, primarily through rehiring of previously furloughed workers. Moreover, while average weekly hours worked of job stayers declined sharply in the beginning of the pandemic, they recovered quickly and even moved slightly above pre-pandemic levels. Our results therefore dispel the popular notion that small businesses have on average been hurt harder by the pandemic than larger businesses.

Second, we decompose changes in small business employment into different contributions and find that business closings account for 70% of the initial employment decline, with closings spiking to 40% of all small businesses in mid-April 2020. In the months following, about two thirds of closed businesses reopened while the rest appears to have closed permanently. The resulting cumulative annual closing rate of small businesses in the four sectors amounts to 17% one year after the start of the pandemic, which is only about 2% higher than the annual closing rate over the same time period one year prior. This implies, perhaps surprisingly but consistent with other indirect evidence by Crane et al. (2020), that the pandemic has not led to a substantially higher rate of business shutdowns.

Third, new businesses openings have added more than 1.5 million new jobs since the pandemic started, constituting an important driver of the recovery from mid-June 2020 onward. Compared to 2019, the rate of new business openings remains considerably lower, however. This contrasts with recent evidence by Fazio et al. (2021) and Haltiwanger (2021) who report record numbers of new business registrations since the pandemic started. As we discuss below, the discrepancy is likely due to the fact that business registrations are a leading indicator of small business employment and that many of the new registrations are for businesses without a physical location for customer interaction (e.g. nonstore retailers) that our

[3] Our estimates, which can be computed weekly with a lag of only a few days, predicted not only the sharp drop in service-sector employment from mid-March to mid-April and the strong yet partial recovery from mid-April to mid-June 2020 that the BLS reported in its Employment Situation with a lag of several weeks, but also the slowdown in the recovery since mid-June 2020.

data has difficulty measuring. This implies that the recovery in small business employment from the pandemic may be even more robust than reported here.

Fourth, small business employment was affected significantly and in quantitatively important ways by the early availability of PPP loans and the generosity of additional UI benefits from FPUC relative to average earnings of likely UI recipients. Both of these results are based on a county-by-week panel built from our Homebase sample that allows us to exploit differences in the timing and scope of the two federal policy responses across counties while assessing the role played by business closings and openings.

For PPP loans, we use a research design similar to Doniger and Kay (2021) that leverages plausibly exogenous local differences in the delay of obtaining a PPP loan due to the temporary exhaustion of PPP funding in mid-April 2020. We estimate that counties with a larger share of delayed loans have persistently lower small business employment from mid-April through the end of the sample. This effect, which is robust to a rich set of controls including establishment fixed effects and is not driven by pretrends, is in large part due to higher rates of business closings.[4] The result is important because it suggests that the temporary exhaustion of PPP in mid-April 2020 occurred at a critical moment when many small business owners, faced with an unprecedented negative shock, had to decide whether to continue operating and hope for loan relief from the government or cut their losses and close shop. Quantitatively, our estimates imply that without delays in PPP loans, small business employment would have been about 10% higher in mid-July 2020 and about 6% higher in January 2021. These numbers are broadly in line with the estimates based on household survey data by Doniger and Kay (2021) but larger than the estimates reported in Granja et al. (2020). Like us, their analysis is based on Homebase data but they treat all exits as shutdowns, which is likely to infuse measurement error and attenuate their estimates.[5]

Turning to FPUC, Ganong et al. (2020) show that the weekly $600 in additional UI from FPUC raised the median replacement rate to 145% with three quarters of eligible workers receiving more in UI benefits than their previous labor earnings. This unprecedented increase in benefits may have exerted both a negative incentive effect on labor supply and a positive labor demand effect since, as documented by Ganong et al. (2021), receipt of FPUC led to large increases in consumer spending. To assess the net effect of the two forces, we exploit the fact that the generosity of FPUC relative to pre-pandemic earnings of likely UI recipients varied widely across counties. Our estimates imply that the demand

[4]The regressions incorporate controls for changes in county-level Covid infection and death rates, non-pharmaceutical interventions (NPIs), school closings and weather, as well as differential week fixed effects by pre-pandemic county average household income.

[5]Our estimates also attribute a much larger effect to PPP than Autor et al. (2020) and Chetty et al. (2020) who exploit the 500 employee threshold for PPP loan eligibility, which suggests that businesses around the 500 employee threshold have been less dependent on PPP loan support and therefore closed down at a much a lower rate than businesses with fewer than 50 employees. This is consistent with results by Chodorow-Reich et al. (2020).

effect completely dwarfs any disincentive effect on labor supply: small business employment recovers significantly faster after the beginning of FPUC in April 2020 in counties where FPUC represents a large increase in the replacement rate. This positive employment effect diminishes after the expiration of FPUC at the end of July 2020 but continues through 2021, in part because the rate of permanent business closings is lower and the rate of new openings is higher in counties with more generous FPUC. The results are consistent with a number of studies finding no sizable adverse effects of FPUC on local labor markets (e.g. Dube, 2021, Finamor and Scott, 2021, Ganong et al., 2021, and Marinescu et al., 2021). The novelty of our results is that we focus on the net impact of FPUC for small businesses employment by exploiting county-week differences in the data and show that business closings and openings account for a substantial portion of the total effect.

The paper contributes to an extensive literature measuring the impact of the COVID-19 pandemic on U.S. labor markets and businesses. The Homebase data has been among the most widely used in this respect (e.g. Homebase, Bartik et al. (2020), Bartlett and Morse (2020), Granja et al. 2020, Finamor and Scott, 2021, among others). Other prominent studies that use different establishment- or household-level datasets to estimate the impact of the pandemic on employment are Bick and Blandin (2020), Cajner et al. (2020), Chetty et al. (2020), Coibon et al. (2020), Dalton et al. (2020), Fairlie (2020), Kahn et al. (2020), and Lewis et al. (2021) among many others. The papers closest to ours are Cajner et al. (2020) and Dalton et al. (2020) who also find that small business employment experienced a more dramatic decline in the beginning of the pandemic but then rebounded more strongly and by Fall 2020 had recovered as many jobs jobs relative to pre-pandemic levels as larger businesses. The main methodological contribution relative to this literature is that we are the first to systematically match establishment records with other information on business activity in order to distinguish business closings and openings from sample churn. This is critical both for properly benchmarking our data against official data from pre-pandemic years and for estimating the impact of the pandemic on small business employment. Indeed, one of the most striking results of our analysis is that the rate of total business closings one year after the pandemic started is similar to the rate of closings one year earlier and that this, together with new openings is a major source of the recovery in small business employment.

2 Estimating small business dynamics and employment

Our goal is to construct an estimate of small business employment that directly incorporates the effects of establishment openings and closings. For each sector (e.g. Leisure & Hospitality), we start with the CES employment estimate for February 2020 (the reference week) multiplied by the ratio of employment in businesses with fewer than 50 workers to employment in all businesses from the QCEW, $\widehat{E}_0$, and estimate employment in week t as

$$\widehat{E}_t = \widehat{E}_{t-1} \times \frac{\sum_i \omega_i \left(\hat{e}_{it}^{\mathcal{A}_{i,t}} + \hat{e}_{it}^{\mathcal{O}_{i,t}}\right)}{\sum_i \omega_i \left(\hat{e}_{it-1}^{\mathcal{A}_{i,t}} + \hat{e}_{it-1}^{\mathcal{C}_{i,t}}\right)}, \tag{1}$$

where ω_i denotes the sampling weight for industry-size-region cell i, constructed as the ratio of QCEW establishment counts in 2020:Q1 to HB establishment counts in that cell; $\hat{e}_{it}^{\mathcal{A}_{i,t}}$ denotes employment of the set of establishments $\mathcal{A}_{i,t}$ that are active in HB in both week t and $t-1$; $\hat{e}_{it}^{\mathcal{O}_{i,t}}$ denotes employment of the set of establishments $\mathcal{O}_{i,t}$ that are newly opening or reopening in week t; and $\hat{e}_{it-1}^{\mathcal{C}_{i,t}}$ denotes employment of the set of establishments $\mathcal{C}_{i,t}$ that are closing temporarily or permanently in week t.

The main challenge in constructing this estimator is to distinguish business openings and closings from sample churn. In other words, $\mathcal{O}_{i,t}$ should not include establishments that appear in HB for the first time in week t but operated already previously; and $\mathcal{C}_{i,t}$ should not include establishments that disappear from HB in week t but continue to operate. Sample churn is important for many establishment-level datasets by private-sector providers that acquire and drop clients on a continuous basis and especially for HB, which has been growing strongly over the past years including during the pandemic. The next section explains how we match the HB data with information on business activity from other data sources to identify business closings and openings and the extent to which abstracting from sample churn matters.

Our estimator is conceptually similar to the "weighted link-relative technique" behind the CES employment estimate that the BLS reports in the monthly Employment Situation.[6] But there are important differences. First, (1) is a weekly estimator that can be updated in almost real-time whereas the CES estimate is monthly and becomes available with a lag of several weeks. Second, the CES estimate includes employment of all establishments and is not separately reported for small establishments. Third, our estimator directly includes not only employment changes from businesses that are active in t and $t-1$ but also employment changes from business closings and (re)openings. The CES estimator, in contrast, includes only a portion of the establishments that report zero employment in month t and establishments that return to positive employment in month t, respectively, and then adjusts separately for new openings and other closings with an econometric "net birth/death" model based on current and historical data.[7]

[6] See https://www.bls.gov/web/empsit/cestn.htm for details on the CES and estimation.

[7] Historically, the CES estimation only included establishments that reported positive employment in both t and $t-1$ and the net/birth death adjustment was based on an econometric model of net birth/death residuals from QCEW data over the preceding five years. By not including establishments that failed to report employment in both months, the CES estimate effectively treated them as deaths and imputed employment growth of the sample of active establishments so as to offset missing employment gains from establishment birth, which are on average closely related to employment losses from

Fourth, our estimator measures employment as the number of workers with positive hours in a given week, whereas the CES measures employment as the number of workers on payrolls who received pay for any part of the pay period that includes the 12th day of the month, independent of whether they actually worked or not in that week. We argue below that these differences can be important in situations such as the beginning of the pandemic when the number of active establishments and the number of employees actually working changed dramatically within just a few days.

To quantify the sources behind employment fluctuations, we decompose (1) into contributions from continuing establishments, establishment closings, establishment reopenings, and new establishment openings, as well as into contributions from gross hirings and separations. The online Appendix provides details on these decompositions. Furthermore, we quantify small business dynamics by reporting rates of establishment closings, reopenings, and new openings as

$$rate(\mathcal{I}_t) = \frac{\sum_i \omega_i \hat{n}_{it}^{\mathcal{I}_{i,t}}}{\sum_i \omega_i \left(\hat{n}_{i0}^{\mathcal{A}_{i,1}} + \hat{n}_{i0}^{\mathcal{C}_{i,1}} \right)}, \tag{2}$$

where $\hat{n}_{it}^{\mathcal{I}_{i,t}}$ denotes the count of establishments in industry-size cell i that closed in week t ($\mathcal{I}_{i,t} = \mathcal{C}_{i,t}$), reopened in week t ($\mathcal{I}_{i,t} = \mathcal{R}_{i,t}$), or newly opened in week t ($\mathcal{I}_{i,t} = \mathcal{N}_{i,t}$), with $\mathcal{O}_{i,t} = \mathcal{R}_{i,t} \cup \mathcal{N}_{i,t}$ by definition; and $\hat{n}_{i0}^{\mathcal{A}_{i,1}} + \hat{n}_{i0}^{\mathcal{C}_{i,1}}$ denotes the count of active establishments in the reference week.[8]

Aside from employment and establishment counts, we also estimate average weekly hours (AWH). To do so, we start with the CES estimate $\widehat{AWH}_0$ from February 2020, and then use our HB data to estimate

$$\widehat{AWH}_t = \widehat{AWH}_{t-1} \times \frac{\left(\sum_i \omega_i \widehat{wh}_{it}\right) / \left(\sum_i \omega_i \hat{e}_{it}\right)}{\left(\sum_i \omega_i \widehat{wh}_{it-1}\right) / \left(\sum_i \omega_i \hat{e}_{it-1}\right)}, \tag{3}$$

where $\widehat{wh}_{it}$ denotes estimated total weekly hours worked and $\hat{e}_{it}$ denotes estimated employment at establishments in industry-size-region cell i in week t. This estimation of AWH is different from the "link and taper technique" used to construct AWH in the CES, which adjusts the current estimate towards the previous estimate so as to keep it close to the overall sample average over time. The CES estimate may therefore not capture large changes in actual AWH that occur in times of economic disruptions, whereas

establishment death. In light of the large labor market disruptions caused by the COVID-19 pandemic, the BLS changed its birth/death adjustment from the April 2020 report on forward by including a portion of reported zeros in the sample employment growth calculation and by adding current period employment growth to the net birth/death adjustment model. See https://www.bls.gov/web/empsit/cesbd.htm for details.

[8] We define these rates relative to the count of active establishments in the reference week as opposed to the count of active establishments around week t because the count of active establishments varies dramatically in the beginning of the pandemic.

our estimate does because it is based on current information only.[9]

3 Data

The Homebase data consists of anonymized daily records of individual hours worked and wages of employees, linked longitudinally to the establishment where they work and the firm that owns the establishment. The data is recorded in real-time through HB's proprietary software and is used by many of the businesses for payroll processing. HB provides free data access to researchers and updates the data regularly with the latest observations.

In addition to the publicly available data, HB shares with us counts of owners and managers that use the HB software, hours scheduled by employee for establishments that do not track hours, and name and address information for each establishment. As described below, the information on owners and managers allows us to include salaried workers with untracked hours in our measure of employment while the information on establishments with scheduled hours expands our sample. The information on name and address, in turn, allows us to match HB establishments to data from Safegraph, Google, and Facebook to determine industry classification and to distinguish business openings and closings from sample churn. For privacy reasons, all of the results reported below are sufficiently aggregated to avoid disclosing information about individual businesses.

3.1 Employment and business activity

For each establishment, we construct weekly employment as the sum of individuals with tracked hours (actual or scheduled) during that week plus owners and managers that show activity in the HB software but do not have tracked hours.[10] As discussed below, this attenuates the decline in estimated employment during the pandemic because owners and managers with untracked hours have a higher propensity to remain active than employees with tracked hours.

For an establishment to be included in our sample it must show up at least once for a spell of three consecutive weeks with at least 40 weekly tracked hours across its employees. We thus exclude establishments that use HB only for a short trial period. For an establishment in the sample to be

[9] The link-and-taper estimate used in the CES can be expressed as $\widehat{AWH}_t = 0.9\left(\widehat{AWH}_{t-1} - \widehat{awh}_{t-1}\right) + \widehat{awh}_t$, where $\widehat{AWH}_t$ is the official estimate and $\widehat{awh}_t = \left(\sum_i \omega_i wh_{it}\right) / \left(\sum_i \omega_i e_{it}\right)$. If $\widehat{AWH}_{t-1} > \widehat{awh}_{t-1}$ in the previous month, then the current month official estimate will be raised relative to actual data, and vice versa if $\widehat{AWH}_{t-1} < \widehat{awh}_{t-1}$. The CES makes a slight adjustment to this estimator to account for atypical reports although it is unclear what makes a report atypical.

[10] For establishments that report both scheduled and actual hours, we compare the two measures and find them to be very close to each other. We are therefore confident that scheduled hours are an accurate measure of actual hours worked.

active in a given week, it must have employees with tracked hours in that week. Establishment activity is therefore independent of owners and managers logging in to the HB software (e.g. for reporting purposes).

3.2 Industry classification and sample characteristics

The historical HB data comes with an industry category for each establishment, but the available categories do not line up with standard industry classification and for about one third of the records, industry category is missing altogether. This is an important limitation for the purpose of constructing estimates that can be compared to official statistics. One of the contributions of our paper is to match the HB establishment records by name and address to Safegraph's Core Places data, which contains consistent NAICS-6 industry coding for each establishment.[11] The procedure involves extensive data cleaning and standardization before matching the records sequentially by exact merges and then fuzzy name match and substring match algorithms. The Appendix provides details on these procedures as well as match statistics. We only retain HB establishment records that match exactly or with a high match rate.[12]

The available Homebase data extends from January 2018 through May 2021 and contains over 100,000 distinct establishments. Most establishments in the sample employ fewer than 50 workers and belong to service sectors with a large propensity for in-person interaction. The sector with the largest coverage is Leisure & Hospitality (NAICS 71 and 72), followed by Retail Trade (NAICS 44-45), Education & Health Services (NAICS 61-62), and Other Services (NAICS 81).[13] Aside from coverage, we focus on these four sectors because they were particularly vulnerable to the disruptions and stay-at-home orders in the beginning of the pandemic and were the major driver of the sharp economic downturn.

Table 1 reports the number of establishments that we successfully match to Safegraph and retain for our estimations. The mid-February 2019 base sample contains about 38,900 establishments of which about 35,300 are active.[14] For the mid-February 2020 base sample, the corresponding establishment

[11] As shown in the Appendix, establishments in the different HB industry categories do not necessarily match to the expected NAICS industry classifications. In December 2020, HB independently started publishing NAICS industry classifications for each establishment. This classification is available only for establishments active from that month onward. Since many establishments that were active in 2019 and 2020 are no longer in the HB sample, this NAICS classification is not directly useful for our estimation and benchmarking, which starts in 2019. However, we compare our industry classifications to the one provided by HB and generally find a high level of overlap.

[12] We compare our match algorithm to Safegraph's Placekey matching tool and find that our algorithm results in higher match rates, primarily thanks to extensive pre-cleaning of establishment names and deduplication of establishment records. Details are available upon request.

[13] See the Appendix for details. Other Services includes "Repair and Maintenance" (NAICS 811) and Personal and Laundry Services (NAICS 812), which contains many of the HB establishments categorized under "home and repair, "beauty and personal care", and "health care and fitness". Interestingly, the HB data also contains several hundred establishments each in Utilities (NAICS 22), Construction (NAICS 23), Food, Textile and Apparel Manufacturing (NAICS 31) and Real Estate, Rental and Leasing (NAICS 53).

[14] The remaining 3,600 establishments are temporarily inactive; i.e active prior to mid-February and then active again at some point after mid-February. This is consistent with administrative data from the BED that also reports a substantial

counts are 52,300 and 46,300. From mid-February 2019 to mid-February 2020, there are 13,687 exits without return and 25,413 new entrants, and from mid-February 2020 to mid-February 2021, there are 14,691 exits and 22,474 new entrants. Foreshadowing the discussion below, these entry and exit rates are much larger than birth and death rates in the official statistics, implying that the HB data is subject to considerable sample churn.

Table 1: Homebase sample counts

	2019		2020	
Mid-February base sample	38,553	(100%)	50,058	(100%)
- active in mid-February	35,045	(91%)	45,880	(92%)
- temporarily inactive in mid-February	3,508	(9%)	4,178	(8%)
Exits without return	13,687	(36%)	14,691	(29%)
New entrants	25,413	(66%)	22,474	(45%)

Notes: The table shows counts of establishments from mid-February 2019 to mid-February 2020 and mid-February 2020 to mid-February 2021 that (i) we successfully match to Safegraph; (ii) belong to either Retail Trade, Education & Health Services, Leisure & Hospitality, or Other Services; and (iii) have fewer than 50 workers when active in mid-February or when entering Homebase.

To assess the representativeness of our HB sample, we compare the distribution of establishments and employment across industry, size class and regions to administrative data from the QCEW. The QCEW is derived from state unemployment insurance records and the publicly available data contains population counts of establishments and employment as well as wage earnings. Tabulations by establishment industry, size, and geography are published only for the first quarter of each year and become available with a lag of about 9 months. As shown in the Appendix, within the four service sectors and for size classes below 50 employees that we study, our HB sample has good coverage and the distribution of establishment counts and employment across the different industry-size cells and across different regions of the U.S. is similar to the one in the QCEW. Furthermore, average employees per establishment by industry-size cell are close to their QCEW counterparts. We therefore conclude that our HB sample is reasonably representative of the population. Nevertheless, as described above, our estimation applies weights that make industry-size-region establishment counts exactly proportional to the ones reported in the QCEW.

3.3 Business openings and closings

A key challenge when working with establishment-level datasets is to distinguish business closings and openings from sample churn; i.e. businesses that already operate before entering the sample or businesses that continue to operate after exiting the sample. This is especially important for private-sector data such as HB that are subject to large turnover and expand their client base over time.

rate of temporary closings.

The main methodological contribution of our paper consists of matching the establishment records from HB to information on business activity from other real-time sources so as to (i) estimate business closings and openings, and (ii) benchmark the estimator against administrative data prior to the pandemic. We implement this methodology with data from Google, Facebook, and Safegraph; but other sources could be used to complement our approach. In what follows, we provide an overview of the methodology. Further details and statistics are provided in the Appendix.

To identify closings, we proceed in four steps. First we consider all establishments that become inactive in week t (called exits from hereon) and check whether they return to activity by the end of the sample. If so, we classify them as temporarily closings and attribute them to the set of establishments $\mathcal{C}_{i,t}$. Second, for establishments that do not return to activity by the end of the sample, we match them to Google Places using Google's API service and add the ones with a "temporarily closed" or "permanently closed" indicator to $\mathcal{C}_{i,t}$. These indicators are reported by business owners and customers and are typically accurate but cover only a subset of all closed establishments. Third, for the remaining exiting establishments, we match them to Facebook using CrowdTangle, Facebook's research database, to check whether the establishments with regular posting histories while being active in HB stop posting regularly after exiting HB. If so, we add the establishments to $\mathcal{C}_{i,t}$. Fourth, for the exiting establishments that we cannot match to either Google or Facebook or that do not post regularly on Facebook while being active in HB, we add them to $\mathcal{C}_{i,t}$ with probability equal to the (industry-size cell) proportion of closings obtained in step three.

To identify openings, we adopt a similar procedure except that Google Places does not contain a corresponding indicator for "openings". First, we consider all establishments that become inactive at some point and add them to the set $\mathcal{R}_{i,t}$ that reopen in week t if they return to activity in that week. Second, we match establishments that become newly active in week t (called entries from hereon) to Facebook and check whether they start having a consistent posting history only after entering HB. If so, we add the establishments to the set $\mathcal{N}_{i,t}$ of new openings. Third, for the new entrants that we cannot match to Facebook or that do not have reliable data on Facebook postings, we add them to $\mathcal{N}_{i,t}$ with probability equal to the (industry-size cell) proportion of new openings obtained in step two.

As an alternative to using information from Facebook postings, we also use information on visits to stores, as measured by cell phone data from Safegraph. Specifically, to identify closings, we check whether exiting HB establishments that we can match to Safegraph show a large drop off in customer visits relative to establishments that remain active in HB. To identify new entrants, in turn, we check whether entering HB establishments that we can match to Safegraph show up in the Safegraph database only after entering HB. While complementary, we found after extensive analysis that the Safegraph visits data can be very noisy at the individual business level (especially for small stores that are in a building with other occupants or adjacent to another building). Moreover, Safegraph visits can only measure physical visits and therefore do not capture nonstore retail, delivery, or virtual person-to-person businesses. That is why we prefer Facebook postings together with Google Places as our main source of identification of closings and new openings.[15]

3.4 Benchmarking

The final step of our methodological contribution consists of benchmarking our estimates of (permanent) closings and new opening to *pre-pandemic* administrative data. This step is potentially important since the establishments exiting and entering the HB sample may on average have a different propensity to be closings and openings than in the population. More formally, for establishment ℓ in the HB sample, $p(\ell \in \mathcal{N}_i|\text{entry}) \neq p(\text{birth}_i)$ and $p(\ell \in \mathcal{C}_{i,}|\text{exit}) \neq p(\text{death}_i)$, where $p(\text{birth}_i)$ and $p(\text{death}_i)$ are the average population rates for establishment birth and death in industry-size cell i.

We implement the benchmarking by adjusting (permanent) closing and new opening rates from the HB sample so as to match quarterly birth and death rates by sector from the BED for 2019, augmented with information on differences in birth and death rates by size class within sector from the BDS.[16] For permanent closings, the adjustment takes the form of a simple multiplicative factor that we apply to $p(\ell \in \mathcal{C}_{i,}|\text{exit})$ by industry-size class. For openings, the adjustment takes the form of a predictive regression estimated on 2019 data that takes into account that $p(\ell \in \mathcal{N}_i|\text{entry})$ may vary with changes in the proportion of new establishments that enter the HB sample. Details on these adjustments as well as BED and BDS rates are provided in the Appendix. Generally, the difference between adjusted and non-adjusted (permanent) closing and new opening rates are small and our estimates of small business dynamics and employment during the pandemic would remain similar if we used non-adjusted rates.

Figure 1 reports average quarterly birth and death rates for 2019 implied by the adjusted (permanent) closing and new opening rates from our HB sample, the BED/BDS counterparts, as well as the quarterly

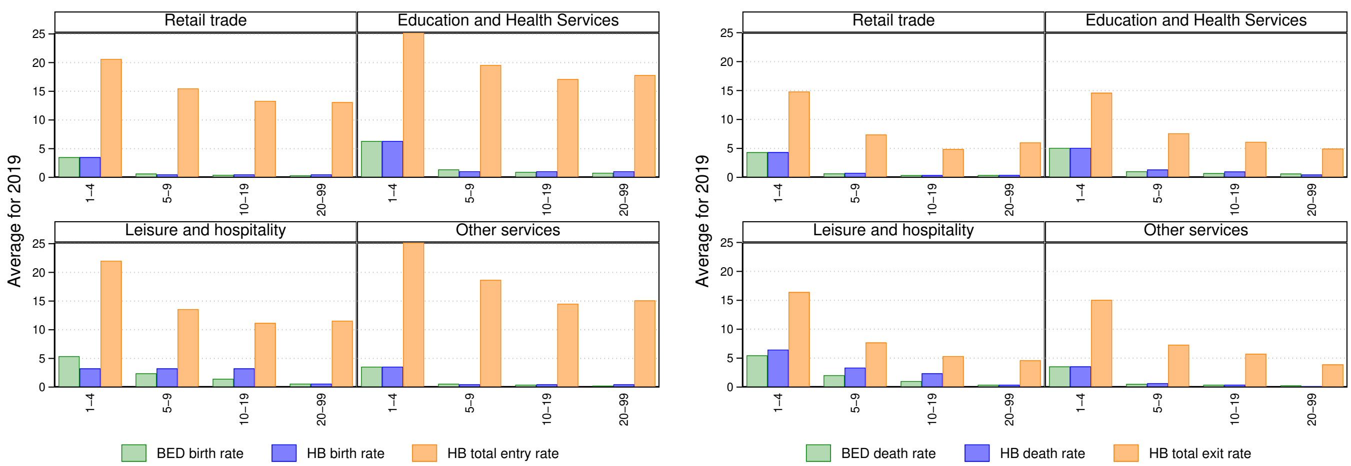
[15] The Appendix provides more information and results on the Safegraph identification approach.

[16] The BED consists of all longitudinally linked establishments from the QCEW and reports quarterly birth and death rates by industry but not by establishment size class. These rates are published with a delay of about six months. The BDS consists of all longitudinally linked establishments from U.S. tax records and reports annual entry and exit rates by industry and size class (among others), which are similar to BED annual birth and death rates. The latest available data is for 2018. We compute ratios of entry and exit rates by size class within sector from the BDS and then multiply the quarterly birth and death rates from the BED with these ratios to obtain quarterly birth and death rates by industry-size cell. The BED also reports quarterly closing and opening rates by industry, which include temporarily closed and reopened establishments. We do not benchmark to those rates because these closing and opening rates are not directly comparable to BDS exit and entry rates.

rate of total new entries and permanent exits in our HB sample. The first striking thing to note is that the total entry and exit rates are much larger than the BED/BDS birth and death rates. This confirms that the HB data is subject to considerable sample churn: many establishments already operated prior to entry into HB, and many establishments continue to operate after exiting HB. Second, the figure shows the close match between the birth and death rates implied by the adjusted (permanent) closing and new opening rates and the BED/BDS counterparts.[17] Third, the figure illustrates the large differences in birth and death rates by size class. With the exception of Leisure & Hospitality, quarterly birth and death are several time larger for establishments with fewer than 5 employees than for larger establishments. Taking into account these differences in birth and death rates turns out to be important for the estimation of small business dynamics and employment during the pandemic.

Figure 1: Benchmarking with BED birth and death rates

(a) Birth rates (b) Death rates



Notes: Quarterly birth and death rates by sector and establishment size class from BED industry data, combined with annual BDS industry-size ratios; corresponding quarterly birth and death rates from HB; and quarterly entry and exit rates from HB. See text for details on the computation.

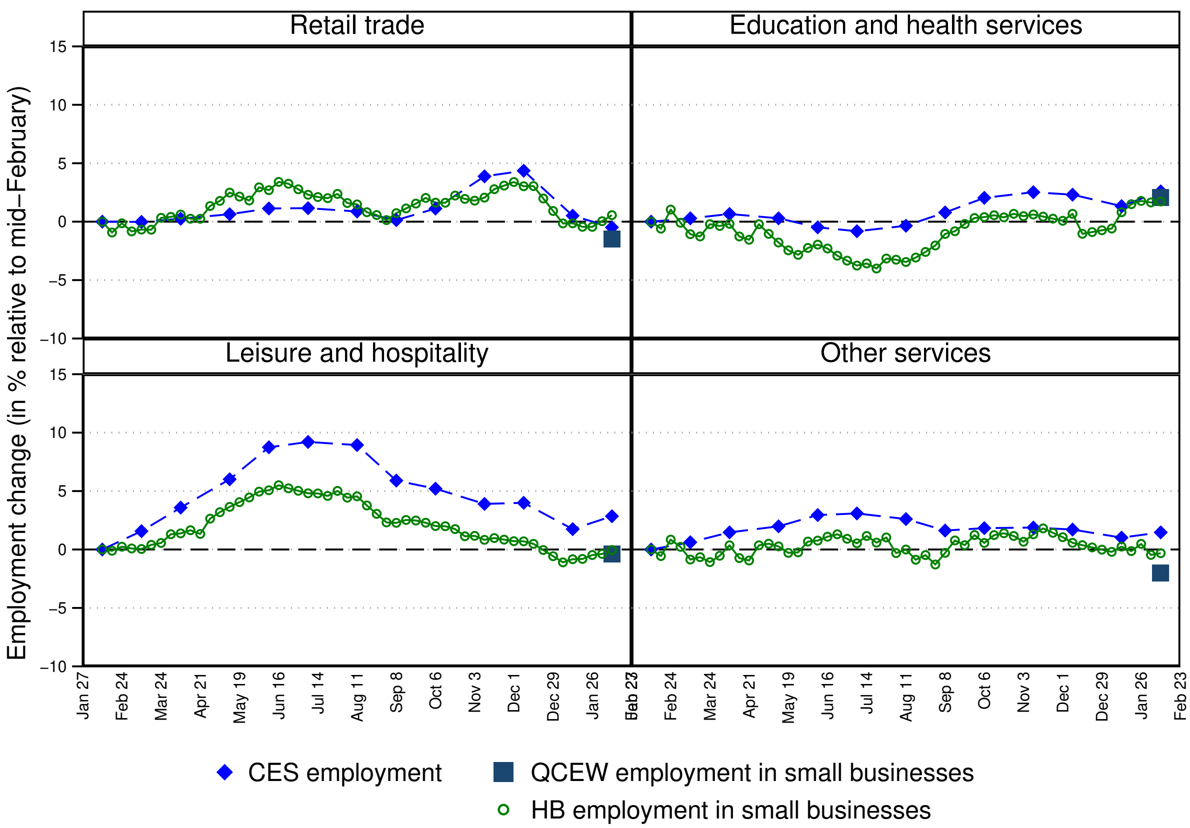
As a check for our benchmarking and the overall representativeness of our HB data, we estimate employment from mid-February 2019 to mid-February 2020 and compare it with the corresponding first quarter employment in small businesses from the QCEW and the monthly all business employment estimate from the CES.[18] Figure 2 reports the results. The weekly estimates from our HB sample comove closely with the monthly CES estimates, although there are some differences (e.g. for Education & Health

[17]The match is not perfect across all sector-size cells because we pool some size classes within sectors to ensure sufficient sample size.

[18]As mentioned above, the QCEW publishes employment numbers by industry and size class for the first quarter of each year (where first quarter pertains to the month of February), while the CES monthly estimates are provided only by industry but not by size class.

and Leisure & Hospitality). These differences should not come as a surprise since the HB estimates are for establishments with fewer than 50 employees whereas the CES estimates pertain to establishments of all sizes. Instead, a better comparison are the annual (first quarter to first quarter) growth rates for small business employment from the QCEW and in this respect, the estimates from our HB sample fit closely.

Figure 2: Small business employment change compared to CES all business estimates for 2019



Notes: Employment change by small businesses with less than 50 employees and all businesses in percent of respective employment level during the week of Feb 9 - Feb 15, 2019 for Retail Trade (NAICS 44-45), Education and Health Services (NAICS 61-62), Leisure and Hospitality (NAICS 71-72), and Other Services (NAICS 81). None of the estimates are seasonally adjusted. The estimates for the weeks of Thanksgiving, Christmas, and New Year are smoothed by using the estimates of adjacent weeks.

4 Small business dynamics and employment during the pandemic

4.1 Employment

Table 2 reports our estimates of small business employment for mid-April 2020, mid-June 2020 and mid-May 2021 (the end of the sample) and compares them to the mid-February 2020 reference week.[19] Across the four service sectors, small business employment declined dramatically between mid-March and mid-April 2020 as states imposed business closures and stay-at-home orders, combining for a staggering 14 million job loss or 46% of the roughly 30.5 million jobs prior to the pandemic.

Table 2: Small business employment losses and recovery during the pandemic

	Retail Trade	Education & Health	Leisure & Hospitality	Other Services	Total
Employment in mid-February 2020	7,612	8,503	10,085	4,435	30,635
Mid-Feb to mid-Apr loss	-3,175	-3,074	-5,456	-2,262	-13,967
in % relative to mid-Feb 2020	*-42%*	*-36%*	*-54%*	*-51%*	*-46%*
Mid-Apr to end-Jun rebound	2,564	1,730	3,901	1,476	9,671
in % relative to mid-Feb 2020	*+34%*	*+20%*	*+39%*	*+33%*	*+32%*
Employment in mid-May 2021	8,123	8,002	10,475	4,213	30,814
in % relative to mid-Feb 2020	*107%*	*94%*	*104%*	*95%*	*101%*

Notes: Employment is expressed in 1,000s of jobs and pertains to establishments with fewer than 50 employees in Retail Trade (NAICS 44-45), Education & Health (NAICS 61-62), Leisure & Hospitality (NAICS 71-72), and Other Services (NAICS 81). None of the estimates are seasonally adjusted. Employment in mid-February 2020 is constructed as the employment estimate for all businesses from the CES times by the ratio of employment in businesses with fewer than 50 workers to employment in all businesses from the QCEW. The other estimates are computed with HB data using the estimator in equation (1).

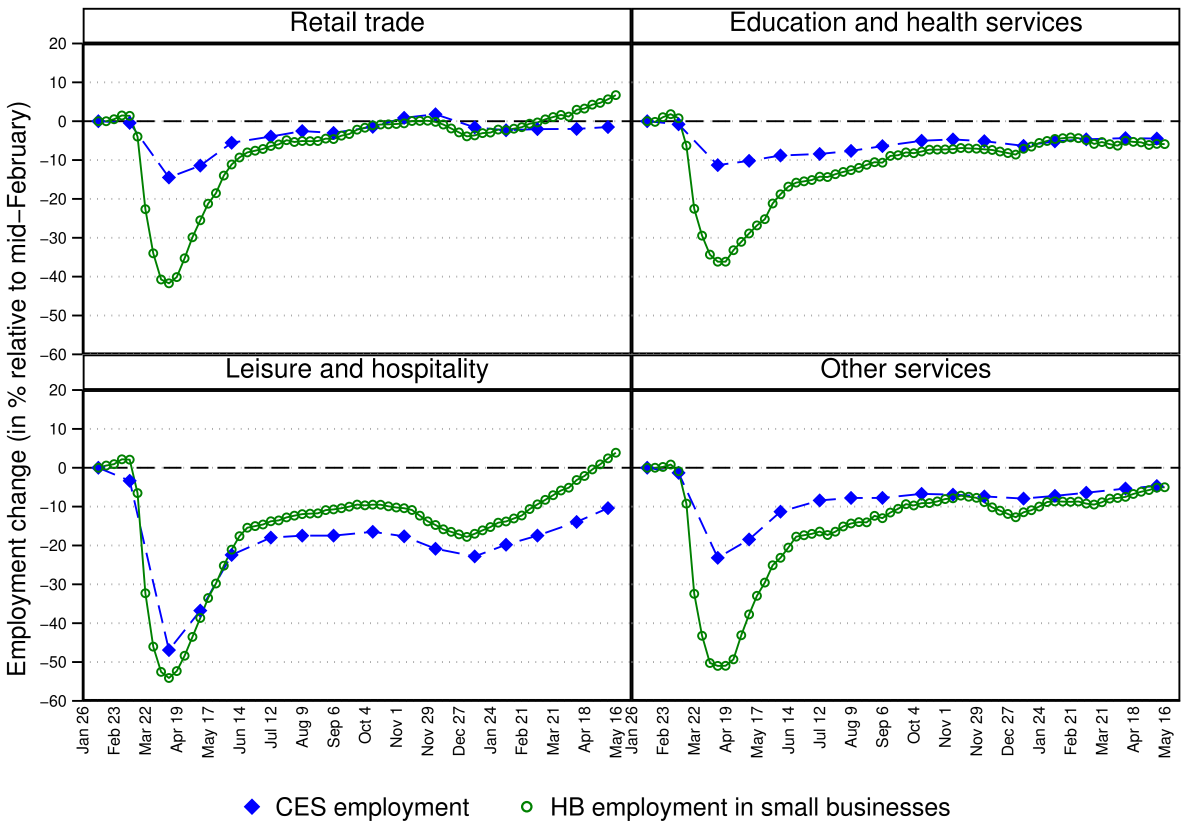
Between mid-April and mid-June, small business employment in all four sectors then rebounded strongly, regaining about 10 million or more than two thirds of the initial job loss. By the end of the current sample, one year and two months after the pandemic was declared a public health emergency in the U.S., small business employment across the four sectors is estimated to have recovered all of the losses since the beginning of the pandemic, with Retail Trade and Leisure & Hospitality somewhat above their pre-pandemic levels, while Education & Health and Other Services remain about 5% below their pre-pandemic levels.

Figure 3 shows the week-by-week trajectory of the different estimates relative to the mid-February 2020 reference week and compares them with the corresponding monthly CES estimates for businesses *of all size classes*. In comparison to the CES all business estimates, the decline and subsequent rebound

[19] None of the estimates are seasonally adjusted since usual adjustment procedures would not be appropriate for the type of large changes in employment experienced in the beginning of the pandemic. See Rinz (2020) for a discussion.

in small business employment in Retail Trade, Education & Health Services, and Other Services in the beginning of the pandemic is two to three times larger.[20] For Leisure & Hospitality, in contrast, the initial decline and rebound in small business employment is approximately the same.

Figure 3: Small business employment change compared to CES all business estimates



Notes: Employment change by small businesses with less than 50 employees and all businesses in percent of respective employment level during the week of Feb 9 - Feb 15, 2020 for Retail Trade (NAICS 44-45), Education and Health Services (NAICS 61-62), Leisure and Hospitality (NAICS 71-72), and Other Services (NAICS 81). None of the estimates are seasonally adjusted. The estimates for the weeks of Thanksgiving, Christmas, and New Year are smoothed by using the estimates of adjacent weeks.

From mid-June onward, the recovery across all four sectors continues at a much slower pace, with employment intermittently peaking at the end of November and then declining again through the end of 2020 due to a combination of renewed restrictions as COVID rates spiked and the effects of colder weather for outdoor dining. Interestingly, small business employment is estimated to have recovered at a somewhat higher rate than CES all business employment in Retail Trade and especially in Leisure &

[20]Digging deeper, we find that even in retail subsectors considered essential such as Building Material Dealers (NAICS 444), Food and Beverage Stores (NAICS 445), Gasoline Stations (NAICS 447), or General Merchandise Stores (NAICS 452) where the CES estimates show almost no job loss across all businesses, our HB estimates show large declines in small business employment between mid-February and mid-April, followed by a large rebound. See the online Appendix for details.

Hospitality where the small business estimate is consistently above the CES all business estimate from mid-June onward, ending up about 10% higher by the end of the sample.

Our estimates are broadly consistent with estimates on the first half of the pandemic reported in Dalton et al. (2020) based on CES microdata and Cajner et al. (2020) based on data from ADP, the largest payroll processing company in the U.S. According to their estimates, employment in establishments with less than 50 employees across all private sectors of the U.S. economy declined by almost twice as much between March and April 2020 as employment for larger establishments – close to what we find for our four service sectors combined – but by the end of June 2020 small business employment had recovered a large fraction of the loss relative to the pre-pandemic level.

Taken at face value, the results imply that while small business employment fared considerably worse in the beginning of the pandemic, small business employment in the four sectors has recovered as much or even somewhat more than employment by larger businesses, thus dispelling the notion that the pandemic continues to affect small business employment more negatively. This suggests that aside from the initial phase, small businesses have on average not been more vulnerable to the disruptions caused by the pandemic.

At the same time, both the total 14 million loss in small business employment between mid-March and mid-April and the rebound of about 10 million between mid-April and mid-June that we estimate are *larger* than the corresponding CES estimates for employment by *all* businesses in the four sectors (13.5 million and about 6 million, respectively).[21] Unless employment in businesses with 50 employees or more in the four sectors increased during the onset of the pandemic and subsequently declined – an implausible scenario by all accounts – this means that either our HB estimates or the CES estimates do not adequately capture the swings in small business employment in the beginning of the pandemic. There are several potential explanation for this discrepancy.

One commonly voiced concern in work with the HB data is that the publicly available files only cover hourly paid workers who may have been more vulnerable to temporary job loss in the beginning of pandemic than owners, managers, and other non-hourly paid workers. By not counting this latter group, HB estimates would therefore overestimate both the initial drop and the subsequent rebound in small business employment. As explained above, however, HB shares with us additional information on owners, managers and any other person within a business that uses the HB software in a given week, and we exploit this information to include these workers in our employment estimate.[22]

[21]For comparison, the headline CES employment estimate for all private sectors declined by 19 million from mid-February to mid-April on a seasonally unadjusted basis.

[22]Including these non-hourly tracked workers attenuates the swing in estimated employment in the beginning of the

A second potential explanation is that the HB businesses may be disproportionally located in affluent zip codes that, as shown by **?**, were hit harder in the beginning of the pandemic. This non-representativeness at zip code level would not be taken into account by the industry-size-region sampling weights in (1), thus leading us to overestimate the initial decline in small business employment. While we cannot directly assess this possibility because administrative counts of establishments by industry, size, and zip code are not publicly available, we find no clear association at the zip code level between HB establishment counts relative to population and average household income. Moreover, we know from Figure 3 that in Leisure & Hospitality, where according to **?** the difference in employment losses between more and less affluent zip codes in the beginning of the pandemic was largest, HB small business employment and CES all business estimates closely track each other, implying that over-representation of the HB data in affluent zip codes is unlikely to be quantitatively important.

A third potential explanation concerns differences in how employment is measured in the HB data relative to the CES, and how we take account of openings and closings in our estimator. As described in the previous section, employment in the CES is measured by the number of workers receiving pay for any part of the pay period that includes the 12th of the month independent of whether they actually worked, while employment in HB is measured by the number of workers who logged positive tracked hours plus all workers who used the HB software otherwise in a given week. So, if some workers who were temporarily furloughed in mid-April still received pay even though they were no longer working, then they were counted in the CES but not in the HB data, which would imply that the CES overestimated employment in mid-April. Perhaps more importantly, while our HB estimator directly includes the employment effects of all closings and openings, the CES estimator only includes a portion of the employment changes from establishments reporting zero employment and does not directly incorporate non-reporting establishments. Since, as shown below, almost 40% of all small businesses in our sample closed by mid-April with about two thirds returning by June, it is conceivable that the CES estimator missed some of the employment effects from this large increase in temporary closings.

Absent access to the CES microdata, it is not possible to directly assess the quantitative importance of these differences. However, we can compare our estimates to the ones by Cajner et al. (2020) who use data from ADP to quantify the employment effects of the crisis. Their employment concept is pay-based as in the CES and their estimator tracks the CES closely prior to the pandemic. Their estimates, which

pandemic but has overall a relatively modest effect. We also check in the CPS household data whether employment of salaried workers declined by more than employment of hourly-paid workers and find only small differences. So, even if we do not capture all non-hourly tracked workers with our information from HB, it is unlikely that this would explain the difference to the CES estimates.

take into account the effects of all exits and re-entry of businesses in the ADP imply that employment of *all* businesses in the four sectors that we consider declined by 20.2 million between mid-February and late April. Given that businesses with fewer than 50 employees accounted for almost half of employment in the four sectors prior to the pandemic and small business employment declined by almost twice as much as employment of larger businesses, this estimate is closely aligned with our estimated decline in small business employment of 14 million during the same time frame.[23]

4.2 The importance of small business closings and openings

To investigate the role of small business closings and openings we decompose the employment change for each sector into the contributions from businesses that continue to operate from mid-February until at least week t (and possibly longer), businesses that closed at some point after the mid-February reference week but reopened by week t, businesses that operated in mid-February but are closed in week t (temporarily or permanently), and businesses that newly opened between mid-February and week t.[24]

As Figure 4 shows, business closings account for 70% or more of the initial employment decline from mid-March to mid-April across the four sectors (red bars) while job losses by continuing businesses account for rest (blue bars). Reopenings of closed businesses drive most of the rebound in employment between mid-April and mid-June (smaller red bars), even though the reopened businesses operate at lower employment than in mid-February (green bars). Finally, the recovery from mid-June onward is driven primarily by the combination of job gains by continuing businesses (smaller negative and positive blue bars) and new businesses (yellow bars). Overall, these new openings add almost 1.5 million new jobs by the end of the sample.

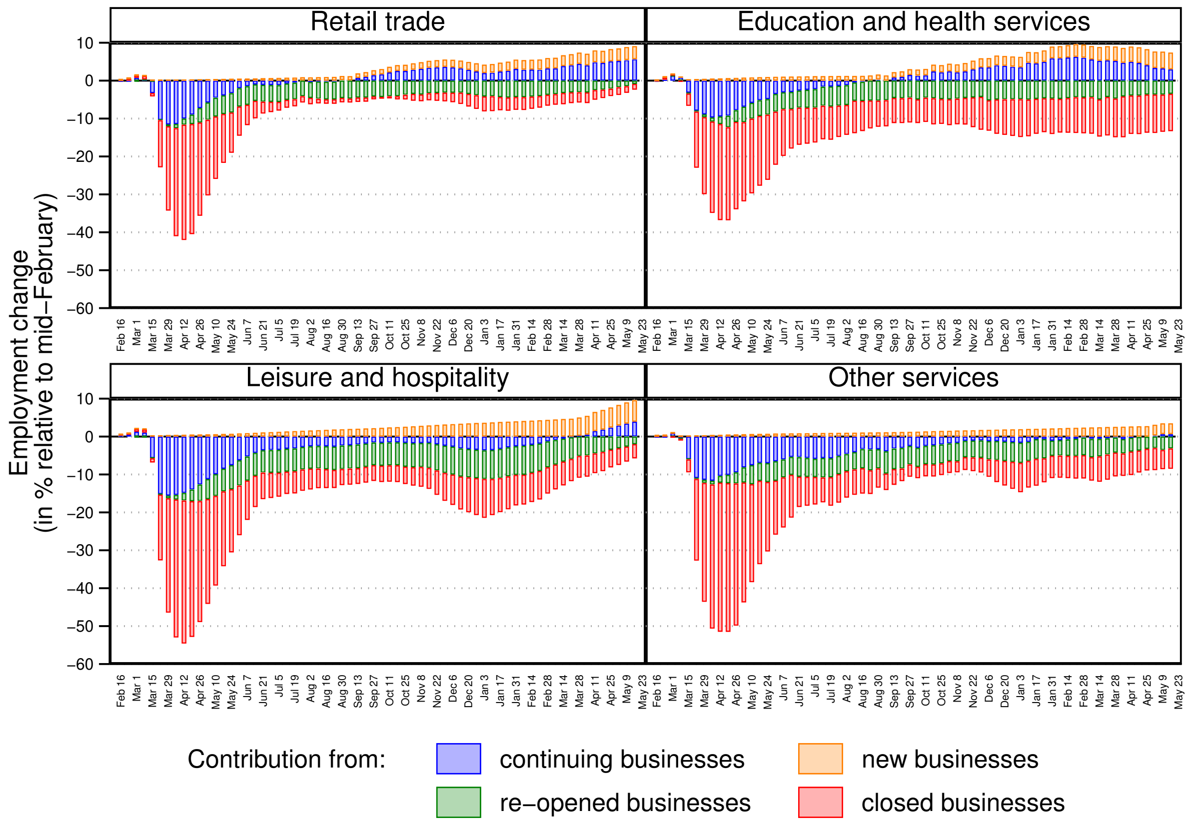
The decomposition also reveals interesting differences across sectors. Large job losses from closings continue to persist through the end of the sample in Education & Health and to a lesser extent in Other Services but are, in relative terms, considerably smaller in Retail Trade and Leisure & Hospitality. At the same time, Retail Trade and Education & Health see substantial job gains by continuing businesses, although for Education & Health these gains largely dissipate by the end of the sample. Finally, job

[23]Similarly, we can compare our results to estimates from the CPS. As shown in the Appendix, the number of individuals categorized as "employed at work" in the four sectors (working in establishments of all sizes) declined by a markedly larger percentage from mid-February to mid-April 2020 than reported in the CES and then also rebounded more strongly by mid-June. The total decline from mid-February to mid-April in the four sectors was 20.1 million, exactly as estimated by Cajner et al. (2020), and the subsequent rebound by mid-June was 7.3 million.

[24]The Appendix provides details on the decomposition. The employment losses from closed business nets out gains from establishments that were active in HB prior to the mid-February reference week, temporarily closed in the reference week, and then reopened at some point thereafter (e.g. seasonal businesses; see Table 1). By netting out these gains, the contribution from closings represents the employment losses over and above the usual employment losses from business that temporarily close. See below for further discussion.

gains from new openings are more important in Leisure & Hospitality and Education & Health. These differences suggest that the pandemic led to varying degrees of restructuring within the different sectors.

Figure 4: Contribution of closings, reopenings, and new openings to small business employment change



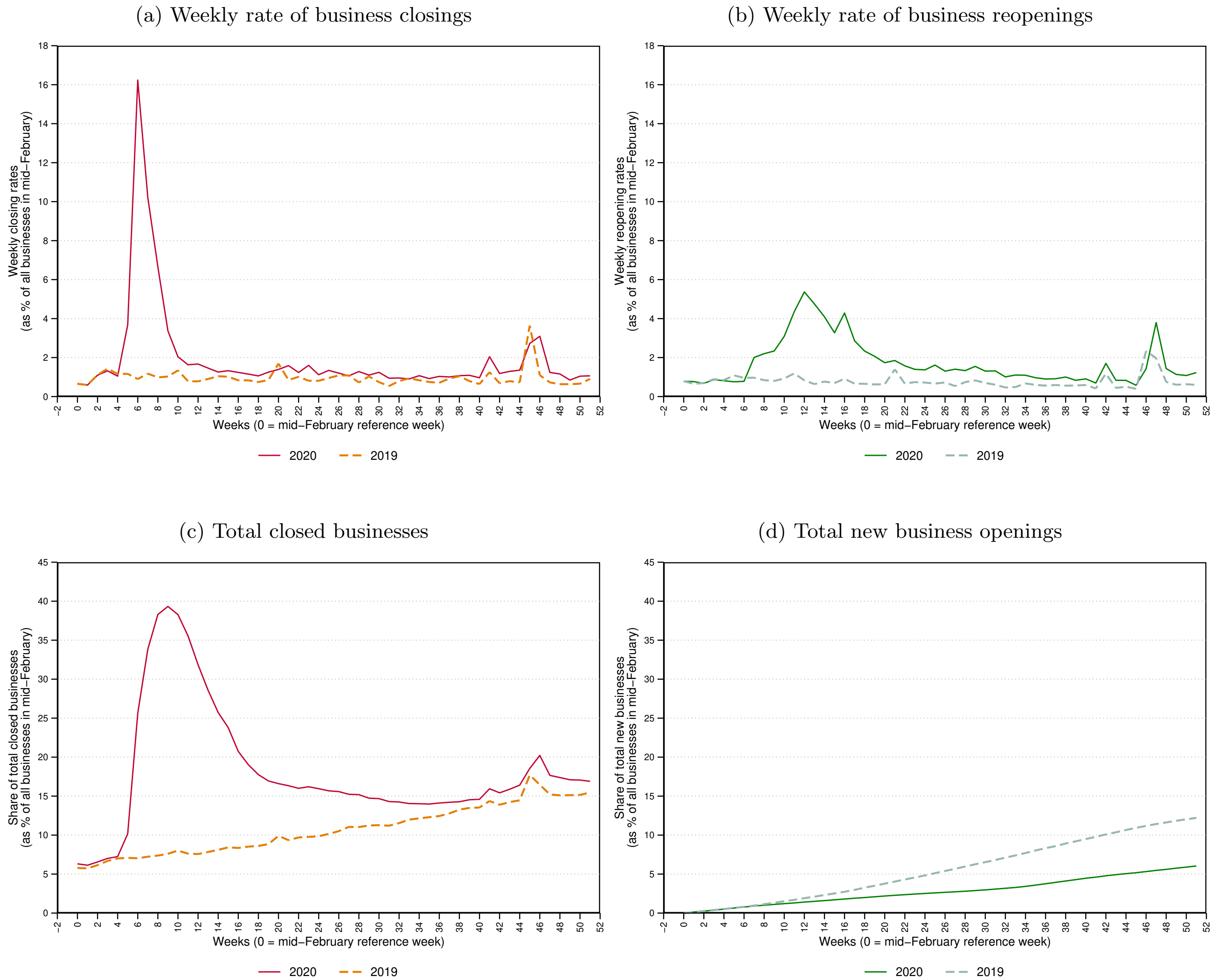
Notes: Contribution to percent employment change relative to mid-February 2020 in Retail Trade (NAICS 44-45), Education and Health Services (NAICS 61-62), Leisure and Hospitality (NAICS 71-72), and Other Services (NAICS 81) by businesses that continued operating from mid-February until at least week t (blue bars), businesses that closed at some point after mid-February 2020 and but reopened by week t (green bars), employment changes from businesses that operated in mid-February 2020 but are closed in week t (red bars), and employment changes from new businesses that opened between mid-February 2020 and week t (orange bars). The estimates for the weeks of Thanksgiving, Christmas, and New Year are smoothed by using the estimates of adjacent weeks.

Figure 5 provides further evidence on business closings, reopenings, and new openings since the beginning of the pandemic and compares them to the same time period one year earlier. As shown in Panels (a) and (b), the weekly rate of business closings spikes to 16% in the week of March 22-28, 2020 (week 6) and then sharply declines to about 2% by mid-April (week 10) before further declining to just above the pre-pandemic average of about 1% per week.[25] Concurrent with the decline in the rate of business closings in April of 2020, reopenings start to increase, reaching about 5% per week in early May before gradually declining back to the 1.5-2% range between July and September and then the 1-1.5%

[25] The temporary upticks in closing rates in weeks 41 and 46 capture the weeks of Thanksgiving and Christmas.

range thereafter, just slightly above the pre-pandemic rate.

Figure 5: Small business closings, reopenings, and new openings



Notes: Rates of closings, reopenings, total closings, and total new openings of small businesses with less than 50 employees in Retail Trade (NAICS 44-45), Education and Health Services (NAICS 61-62), Leisure and Hospitality (NAICS 71-72), and Other Services (NAICS 81). All rates are computed as a % of the total count of active businesses in mid-February. Week 0 denotes the mid-February reference week.

Panel (c) displays the cumulative effect of these closings and reopenings on the rate of total closed businesses relative to active businesses in the mid-February reference week. Note first that the rate of total closed businesses averages about 6% in both mid-February 2019 and mid-February 2002, indicating that a substantial fraction of businesses are temporarily closed at any point in time (also see Table 1). From mid-March 2020 onward, total closings rise steeply and peak at 39% in mid-April. Thereafter, the cumulative closing rate declines, steeply initially as reopenings rise and then more gradually to a low of about 14% by November before rising to about 17% by mid-February 2021. This suggests that only about one third of all closings in mid-March are permanent.[26] Moreover, and perhaps surprisingly, the cumulative rate of closings one year after the start of the pandemic is only about 2% higher than the cumulative closing rate over the same period in 2019-20. This implies that the pandemic did not lead to more permanent small business closings, lending further support to the conclusion from above that small businesses in the four service sectors considered did not suffer more than larger businesses on average.

Panel (d), finally, shows total new business openings relative to total active businesses in mid-February. This rate rises gradually throughout the year, even during the worst of the pandemic in March and April. Compared to 2019, the pace of new openings is clearly lower during the Spring and Summer of 2020 but then picks up somewhat in Fall and Winter, finishing at about 6% in mid-February 2021. This is only about half the rate of new business openings a year earlier, implying that the pandemic did exert a substantial negative effect on new business openings and that as a result, the total count of small businesses is currently lower.

At first sight, the lower rate of new business openings contrasts with recent evidence from the U.S. Census Bureau that shows record rates of new business registrations, among others in Nonstore Retail and Accommodation and Food Services, which are part of the sectors considered here (see Haltiwanger, 2021; also see Fazio et al., 2021). There are a number of potential reasons for this difference. First, many of these new business registrations may not have resulted in employer businesses yet, which would explain why they do not show up in the HB data. Second, while we see substantial employment gains from new business openings in the Leisure & Hospitality sector (of which Accommodation and Food Services is the largest part), new business openings play a smaller role for Retail Trade. Given that new businesses in Nonstore Retail are likely to be very small and do not have a clear physical presence, they do not show up in Safegraph and are therefore not part of our matched HB sample (indeed, our sample contains almost no Nonstore Retail businesses). This implies that we may underestimate the recovery in small business employment and that if one fully incorporated Nonstore Retail, small business employment in Retail Trade in Figure 2 would currently be even further above its pre-pandemic level.

In sum, the take-away from Figures 4 and 5 is that temporary closings and reopenings are the primary driver of the large contraction and rebound in the beginning of the pandemic. Job gains by continuing businesses and new business openings, in turn, account for most of the subsequent recovery and explain why Retail Trade and Leisure & Hospitality have regained more of their pre-pandemic jobs than Education & Health Services and Other Services. Since the rate of new business openings remains

[26] The majority of establishments that closed for more than 10 weeks remain closed.

on average substantially below its pre-pandemic level and business formation statistics indicate record rates of entrepreneurship, this implies that the recovery in the four sectors could remain strong for the months to come.

4.3 The importance of distinguishing new openings and closings from sample churn

As described above, one of the key challenges in estimating employment with private-sector establishment-level data such as HB is to distinguish business closings and openings from sample churn. We further illustrate the importance of this issue here by computing different counterfactual employment estimates from our data and discuss how they relate to results presented in the literature.

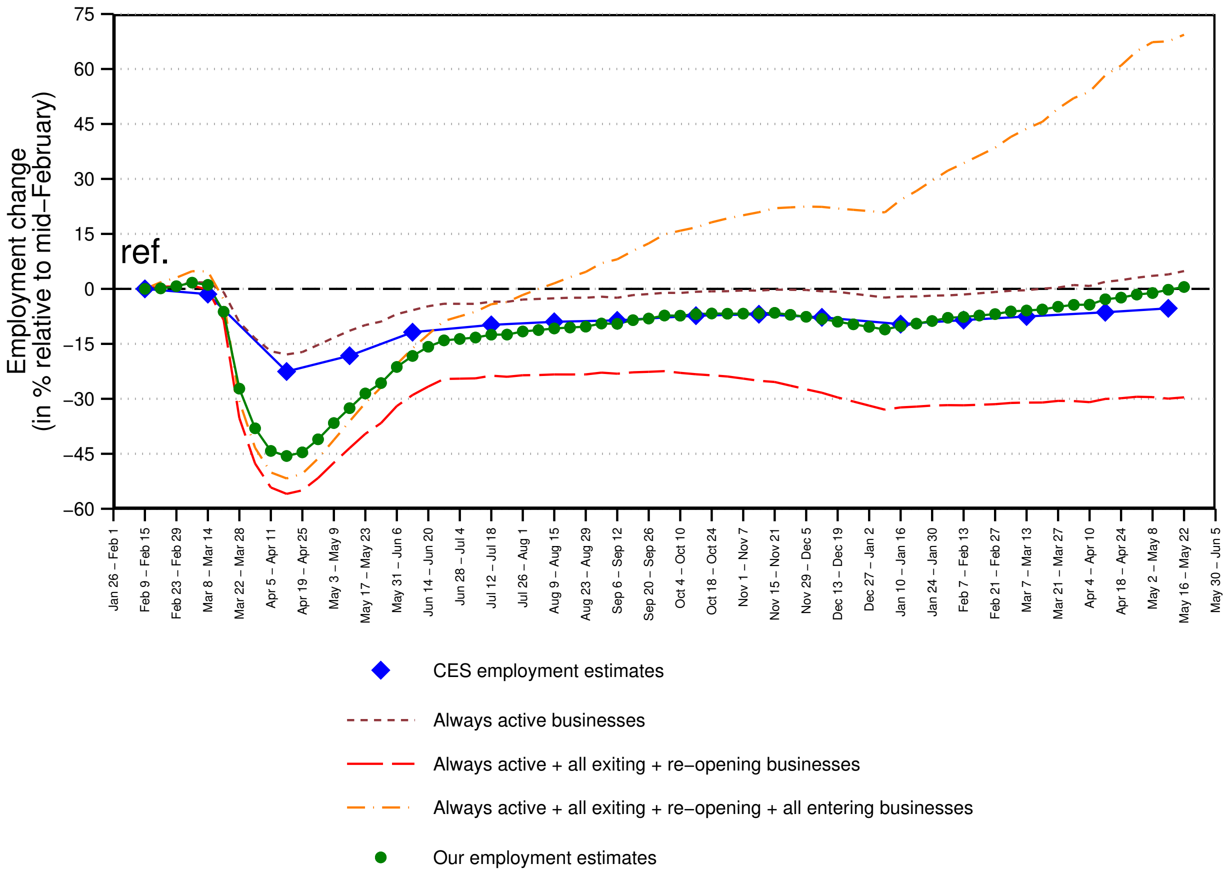
Figure 6 shows small business employment estimates using different approaches on how to deal with entry and exit of businesses in HB. The green line with dots shows our headline small business estimate for the four service sectors combined, and the blue line with diamonds shows the CES all business estimate for the same four sectors. Confirming the above results, the two lines show that small business employment declined much more dramatically in the first month of the pandemic than its CES all business counterpart but then also rebounded more strongly. Since mid-June, the two estimates are close to each other although in the last two months of the sample, the small business estimate has recovered slightly more than the all business estimate.

The brown short-dashed line shows the small business employment estimate if we abstracted completely from entry and exit and used only the set of establishments $\mathcal{A}_{i,T}$ that are continuously active in HB from the beginning of the sample $t = 0$ through the end of the sample $t = T$; i.e. $\widehat{E}_t = \widehat{E}_{t-1} \times \frac{\sum_i \omega_i \hat{e}_{it}^{\mathcal{A}_{i,T}}}{\sum_i \omega_i \hat{e}_{it-1}^{\mathcal{A}_{i,T}}}$. The resulting decline in employment in the beginning of the pandemic is much smaller and the recovery more pronounced, illustrating the result from above that business closings and reopenings play a key role for small business employment.

The red long-dashed line reports what happens if we treated all exits as either temporary or permanent closings; i.e. $\widehat{E}_t = \widehat{E}_{t-1} \times \frac{\sum_i \omega_i \left(\hat{e}_{it}^{\mathcal{A}_{i,T}} + \hat{e}_{it}^{\mathcal{R}_{i,t}} \right)}{\sum_i \omega_i \left(\hat{e}_{it-1}^{\mathcal{A}_{i,T}} + \hat{e}_{it-1}^{exit_{i,t}} \right)}$, where $exit_{i,t}$ denotes the set of all exiting establishments in week t and $\mathcal{R}_{i,t}$ the set of all returning establishments in week t. Since a substantial fraction of establishments that exit HB do not close, this estimate declines even more than our baseline estimate in the beginning of the pandemic and recovers much less thereafter.

The orange dash-dotted line, finally, treats all entries into HB as new openings; i.e. $\widehat{E}_t = \widehat{E}_{t-1} \times \frac{\sum_i \omega_i \left(\hat{e}_{it}^{\mathcal{A}_{i,T}} + \hat{e}_{it}^{entry_{i,t}} \right)}{\sum_i \omega_i \left(\hat{e}_{it-1}^{\mathcal{A}_{i,T}} + \hat{e}_{it-1}^{exit_{i,t}} \right)}$, where $entry_{i,t}$ denotes the set of all entering establishments in week t. This estimate is close to our baseline estimate through mid-June when new entries account for a relatively small portion of all entries (i.e. new entries and reopenings). Thereafter, the estimate gradually diverges and by the end of the sample ends up almost 75% above the mid-February 2020 reference point. The reason for this divergence is straightforward: many of the new entries into HB are not new openings but previously operating businesses, and since HB is continually expanding its client base, treating all of these entries as new openings outweighs the negative effect of treating all exits as closings.

Figure 6: Comparison with counterfactual employment estimators



Notes: Estimated employment change in % relative to mid-February 2020 of small businesses with less than 50 employees in Retail Trade (NAICS 44-45), Education and Health Services (NAICS 61-62), Leisure and Hospitality (NAICS 71-72), and Other Services (NAICS 81) according to different estimation methods (see text). The estimates are constructed based on February 2020 CES employment estimates (week of Feb 9 – Feb 15) and QCEW shares of small business employment for the first quarter of 2020. The estimates for the weeks of Thanksgiving, Christmas, and New Year are smoothed by using the estimates of adjacent weeks.

The difference in estimates is remarkable and offers a cautionary tale about the use of opportunity samples to estimate aggregate employment series. Indeed, during the first months of the pandemic, several research teams turned to Homebase and other private-sector establishment level data to come up with real-time measures of the extent of the crisis (e.g. Bartik et al., 2020; Cajner et al., 2020; and **?** among many others). To the best of our knowledge, none of resulting studies distinguishes business closings and new openings from sample churn and instead used either the set of continuously active businesses or set of continuously active businesses plus all exiting and reopening businesses to estimate or some combination thereof to produce employment estimates. While the consequences of doing so with other opportunity samples may not be as extreme as with the HB data, the results are nevertheless affected by this choice.

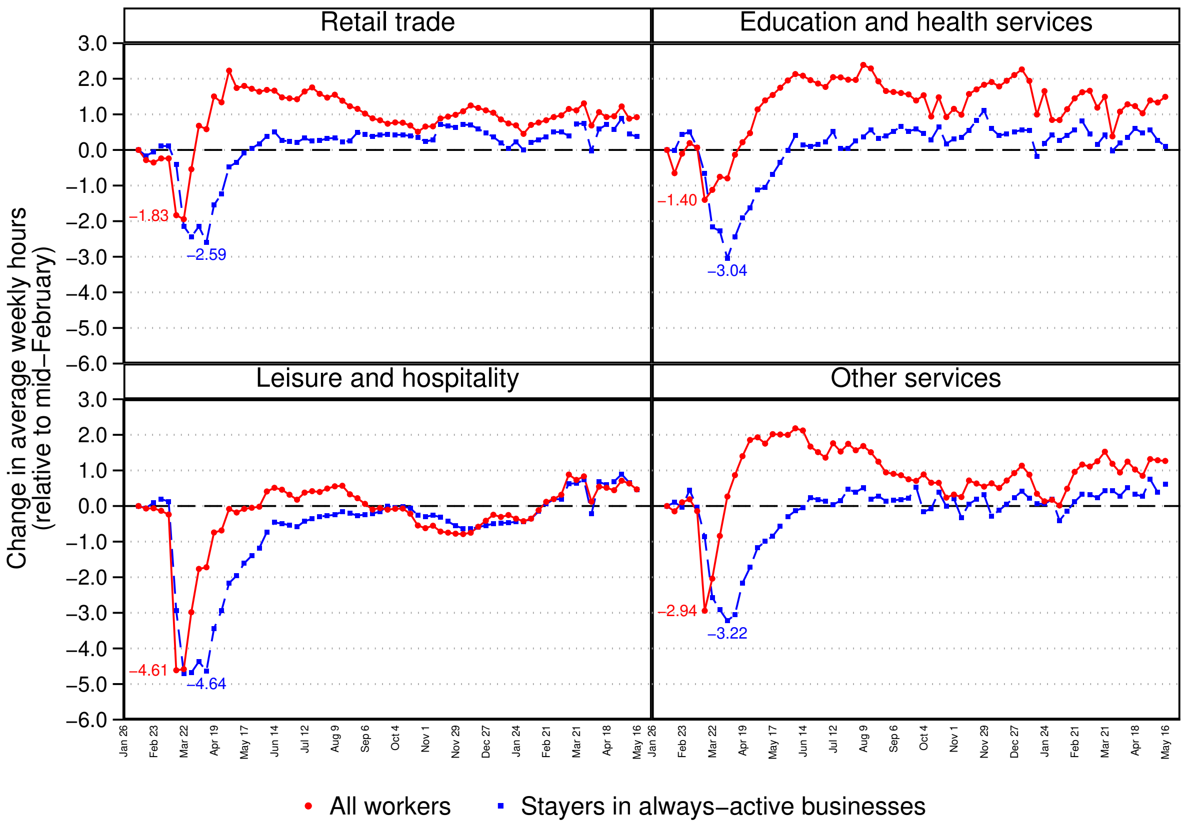
4.4 Average weekly hours

To provide further insights on the impact of the pandemic on workers employed by small businesses, we use the HB data to report changes in average weekly hours (AWH) worked, based on the estimator in (3). Figure 7 shows two estimates of average weekly hours (AWH), one for all workers employed in week t, and one for all job stayers who remained employed continuously in establishments that are active throughout the sample. While the former measure is affected by compositional change, the latter is not since it consists by definition of a balanced panel of workers.

Across all four sectors, AWH declined sharply in March 2020 but then recovered quickly and currently exceeds the pre-pandemic level somewhat. The sharp decline in AWH in the beginning of the pandemic is further evidence of the sudden impact of the pandemic on service-sector jobs. The larger drop as well as the smaller increase in AWH of job stayers relative to the all workers measure is due to the compositional change: the workers laid off or furloughed in March worked on average fewer hours than job stayers. This gap persists through summer as employment remained depressed. For Retail Trade and Leisure & Hospitality, the gap then largely closes by October 2020 as employment fully recovers. For Education & Health and Other Services, in turn, the gap persists through the end of the sample as employment has not fully recovered to its pre-pandemic level.

Remarkably, in all four sectors, AWH of job stayers recovers fully by mid-June. This suggests that the labor market during the pandemic has not been characterized by involuntary part-time as has been the case during previous recessions (e.g. Borowczyk-Martins and Lalé, 2019). This may be due to the particular nature of the recession and its outsize effect on in-person service sector jobs where part-time is less feasible than in, say, manufacturing or construction jobs that suffered more heavily during previous recessions. Alternatively, the unprecedented extension of UI benefits during the pandemic may have reduced the incentives for part-time work (see below). Examining these questions is an interesting topic for future research.

Figure 7: Average Weekly Hours of Small Business Employees



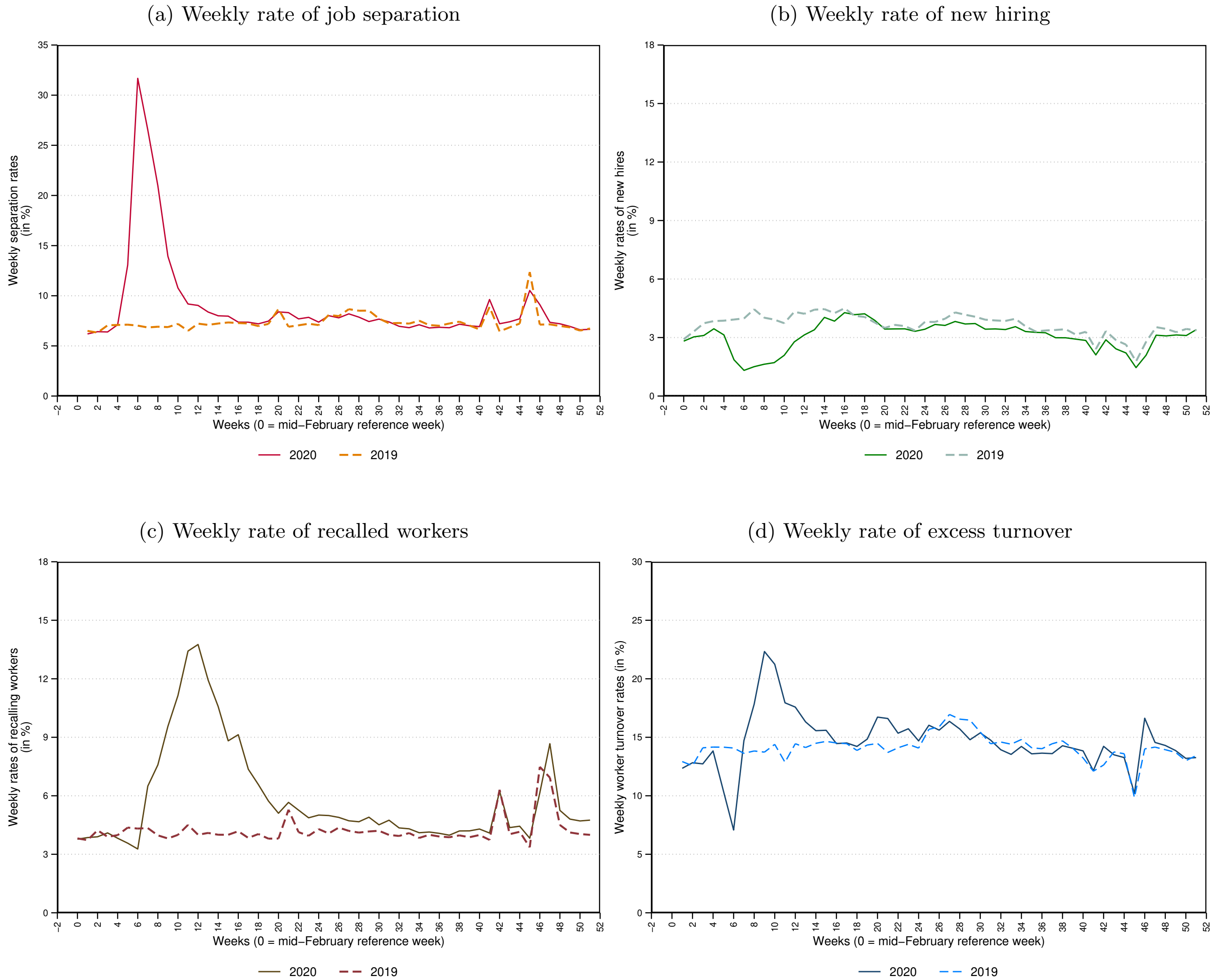
Notes: Changes in average weekly hours of employees in small businesses with less than 50 employees in Retail Trade (NAICS 44-45), Education and Health Services (NAICS 61-62), Leisure and Hospitality (NAICS 71-72), and Other Services (NAICS 81) relative to the February 2020 CES estimate (week of Feb 9 – Feb 15). The solid red line shows the change in average weekly hours of all workers employed in all small businesses. The blue shows the change in average weekly hours of job stayers in businesses that remain active throughout the sample.

4.5 Job separations, new hires, recalls, and excess turnover

The linked worker-establishment structure of the HB data also allows us to provide a detailed account of gross job flows. We decompose weekly employment changes into job separations, new hires, and recalls. As is common in the literature, recalls are defined as workers who are employed with the establishment at some point in the past, disappear for at least one time period (one week in our case), and then reappear as employees in the same establishment.

Figure 8 reports the different weekly gross flows as a rate of average employment in the same week and the preceding week (as for the previous results, all weighted by industry-size-region cells). As panels (a) and (b) show, the job separation rate spikes the week of March 22-28 (week 6), the same week as business closures spike, while the new hire rate declines. Both rates then return to their pre-pandemic average by mid-June and remain essentially the same as one year earlier.

Figure 8: Job separations, new hires, recalls, and excess turnover in small businesses



Notes: Weekly rates of job separation, new hires, recalls, and excess turnover for small businesses with less than 50 employees in Retail Trade (NAICS 44-45), Education and Health Services (NAICS 61-62), Leisure and Hospitality (NAICS 71-72), and Other Services (NAICS 81). All rates are computed as a percent of average employment in the same week and the preceding week.

As shown in panel (c), the recall rate of workers previously employed in the same establishment increases substantially in the weeks following the initial spike in separations, peaking the week of May 3-9 (week 12). The recall rate then declines steadily through the week of June 28 - July 4 (week 20) and thereafter remains slightly elevated through the end of summer before essentially returning to the corresponding 2019 value.[27]

[27] It is interesting to compare these recall numbers to recent results on recalls in the literature. In particular Fujita and

Panel (d), finally, shows the excess turnover rate, which is computed as the difference between the sum of separations rate, new hiring rate, and recall rate minus (the absolute value of) net employment growth. The excess turnover rate drops briefly in the beginning of the pandemic as new hiring declines and then jumps up as recalls increase while some businesses still show excess job separations. After mid-July, excess turnover averages about the same rate as one year earlier.

The results indicate that the rebound in small business employment following the sharp decline in the beginning of the pandemic is driven primarily by recalls of temporarily furloughed workers as opposed to new hires, which is in line with other estimates (e.g. Ganong et al., 2021). This is quite different from previous downturns (e.g. the Great Recession) where a larger share of separations was permanent and the recovery was much more prolonged by persistently lower new hiring rates. Furthermore, the quick return in the excess turnover rate to its 2019 average suggests that, at least for four in-person service sectors considered, the pandemic has so far not led to major reallocations of labor.

5 Effect of local differences in economic policy responses

Our analysis reveals that after an initial dramatic decline, small business employment rebounded strongly and by now has recovered as much as employment of larger businesses in the four service sectors considered. Likewise, while small businesses closings spiked in the beginning of the pandemic, many of the closed businesses reopened and one year later, the rate of total closings is approximately the same as one year earlier. We now investigate the extent to which this recovery of small business activity was affected by federal economic policy responses to the pandemic. We focus on two key provisions of the 2020 CARES Act: the Paycheck Protection Program (PPP), which provided loans to businesses with fewer than 500 employees; and the Federal Pandemic Unemployment Insurance Compensation (FPUC), which paid an additional $600 in weekly unemployment benefits to eligible workers. Both of these programs have been the subject of intense research. The novelty of our investigation is that we use high-frequency data at a detailed geographic level while distinguishing business closing and openings from sample churn. The high-frequency / detailed geography dimension allows us to differentiate the effects of variations in timing and scope of PPP and FPUC from the many other changes that occurred in the first months of the pandemic. The distinction of business closings and openings from sample churn turns out to be

Moscarini (2017) document based on monthly household survey data that on average about 40% of workers return to their previous employment after a jobless spell. Our estimates imply that the corresponding recall rate, measured as recalls divided by the total of recalls and new hires, averages about 55% for 2019 and rises as high as 85% in mid-April 2020. The somewhat higher average for 2019 is primarily due to time aggregation in monthly data (we observe non-trivial non-employment spells lasting less than one month with subsequent recall).

central to understand the effects of the two provisions.

5.1 Delays in PPP loans

The 2020 CARES Act that was signed into law on March 27, 2020 appropriated $349 billion in PPP loans to support firms with fewer than 500 employees prior to the pandemic.[28] To allow broad access, many of the usual eligibility criteria to access government loan programs were waived and the loans came with very favorable terms: qualifying businesses could apply for 2.5 times the average total monthly payroll for each employee up to a maximum of $10 million, and the loans had a duration of two years at a 1% annual interest rate but were forgivable if the business spent at least 75% on payroll within 8 weeks of loan disbursement.[29]

While the Small Business Administration (SBA) was responsible for oversight, firms applied for the loans through local lenders and the first loans were approved on April 3. The demand for loans was so overwhelming that by April 16, the appropriated funds were depleted. In response and after considerable uncertainty, Congress voted on an additional $321 billion in PPP funding that the President signed into law on April 24. Banks started issuing new loans on April 27 and demand spiked immediately, with 60% of the additional funds allocated within two weeks of reopening of the program. Thereafter, loan demand declined substantially and PPP stopped taking new applications on August 8, with almost $150 billion in unused funds remaining.[30]

As documented in detail by Bartik et al. (2020), Granja et al. (2020) and Doniger and Kay (2021) among others, the first round of PPP was subject to large geographical disparities in the allocation of loans, likely reflecting differences in the ability and willingness of local banks to process and approve the large initial influx of loan applications. Funds did not necessarily flow to areas of the country where the initial economic effects from the pandemic were largest but were instead driven by the local presence of the different lenders. In addition, the first loans were unusually large, made to relatively larger businesses. Hence, many of the smallest businesses – the ones that are the focus of our study – were subject to delayed access to PPP loans during the beginning of the pandemic and the extent of this delay depended in large part on geography.

[28] For multi-establishment firms in accommodations and food services (NAICS 72), the 500 employee threshold applied to establishments within certain limits.

[29] Businesses also had to maintain or restore employee counts and pay for loans to be forgivable. On June 5, 2020 Congress relaxed the conditions for loan forgiveness, lowering the threshold on PPP funds used for payroll from 75% to 60% and increasing the number of weeks to use the funds from 8 to 24. See https://www.sba.gov/funding-programs/loans/covid-19-relief-options/paycheck-protection-program for details.

[30] In December 2020, Congress voted for and the President signed into law a third round of PPP consisting of an additional $285 billion in funding and new eligibility rules. Loans started in mid-January 2021 and the program ran through the end of May 2021. This third round is not the focus of the present investigation.

We exploit this geographic variation in initial loan access to evaluate the effects of PPP for small business activity. Similar to Doniger and Kay (2021) we measure delayed access to PPP loans by the share of loans issued between April 26 and May 2 (the week when additional PPP funding became available) relative to the total amount of loans issued between April 12 and May 2 (the week when initial PPP funding ran out to the week when additional PPP funding became available); i.e. $sharePPPdelayed_c = \frac{(loans\ April\ 26-May\ 2)_c}{(loans\ April\ 12-May\ 2)_c}$, where c denotes the county of the businesses receiving the loans.[31] We construct this measure using data on all PPP loans from the SBA. The loans made during these weeks account for about one third of all loans and for an even larger share of loans made to businesses with fewer than 50 employees. As shown in the Appendix, the variation in $sharePPPdelayed_c$ across counties is wide, with a median of 40% and a 10-to-90-percentile range of $[26\%, 60\%]$.[32]

We use the $sharePPPdelayed_c$ measure to estimate the following county-level regression

$$y_{c,t} = \sum_{t=0}^{57} \alpha_t \left(1(week = t) \times sharePPPdelayed_c\right) + X'_{c,t}\gamma + \phi_t + \mu_c + \varepsilon_{c,t} \tag{4}$$

where $y_{c,t}$ is either the percent deviation of employment across establishments in county c in week t relative to its employment in the mid-February 2020 reference week ($t = 6$); the fraction of establishment in county c being closed in week t; or the fraction of establishments in county c being newly opened in week t. The vector $X_{c,t}$ contains a vector of county-specific controls measuring weekly COVID cases and deaths, non-pharmaceutical interventions (NPIs), school closures, weather, as well as week fixed effects interacted with average county household income prior to the pandemic.[33] Finally, ϕ_t is a week fixed effect capturing time variations in average $y_{c,t}$; μ_c is a county fixed effect; and $\varepsilon_{c,t}$ is the error term. All

[31] The weeks in our estimation run from Sunday to Saturday. April 12, 19 and 26 are Sundays. Doniger and Kay use a narrower 2-day window around the temporary exhaustion of PPP loans to measure the share of delayed PPP loans, and they compute the measure at the broader CBSA geographic level. Our estimates are robust to using their narrower time window and the broader CBSA level.

[32] In the regression, we only use a subset of 1,956 counties for which we have HB data (out of 3,143 counties for which we have PPP data). The distribution of $sharePPPdelayed_c$ for this subset of counties is almost identical to the distribution for the full set of counties.

[33] Weekly county-level COVID cases and deaths per capita are obtained from Covid Act Now. The NPIs included are weekly county-level indicators of industry closures, stay-at-home orders, and gathering bans from the Data Library on Industry Closures and Reopening by Atalay et al. (2020) and the Center for Disease Control and Prevention. In addition, we include a weekly containment index containing seven indicators of state-level policies from the Oxford Covid-19 Government Response Tracker (see Hale et al., 2021). We allow for differential impact of these controls over subperiods of the sample. Following Bravata et al. (2021), we proxy weekly school closures by the log change in county average school visits relative to one year prior using data on individual schools from Safegraph. For weather, we use the maximum county temperature in a given week from the Climatology Lab. We also controlled for other measures of weather and found very similar results. Finally, county level average household income are 2016-2019 estimates from the American Community Survey. The predictive value of these controls for small business activity is interesting in its own right and extends earlier results by Bartik et al. (2020), Chetty et al. (2020), and Goolsbee and Syverson (2020), among others. We discuss these results as well as further details on the different controls in the Appendix.

regressions are weighted by county level employment prior to the pandemic in the four sectors considered, and standard errors are clustered at the county level.[34]

The α_t are the main coefficients of interest and measure the effect in week t of the share of delayed PPP loans in county c. The identifying assumption for these coefficients to have economic meaning is that conditional on controls, $sharePPPdelayed_c$ reflects the relative difficulty for small businesses located in county c to obtain a PPP loan in the beginning of the pandemic and is independent of other factors affecting small business activity. One concern with this assumption is that, as discussed above, loan issuance in the very beginning of PPP varied systematically across areas of the U.S. and that these areas were affected differently by the pandemic. As Doniger and Kay (2021) show, however, there is no clear geographic concentration in the timing of loan issuance within the narrow window around the temporary exhaustion of PPP considered here, and $sharePPPdelayed_c$ varies substantially between adjacent counties. Furthermore, our regressions control for a host of county-specific time-varying factors that affected small business activity throughout the pandemic as well as differential week fixed effects as a function of a county's pre-pandemic average household income. As Chetty et al. (2020) document, more affluent localities suffered larger declines in spending on in-person services and employment in the beginning of the pandemic. The differential week fixed effects absorb this local demand factor and are at the same correlated with other county characteristics such as banking concentration, socioeconomic differences, and racial composition that may affect a county's small business activity during the pandemic.

Another potential concern is that more productive businesses may have received loans earlier. This would be a threat to identification only insofar as the distribution of productive businesses varies across counties and has a time-varying effect throughout the pandemic since average differences are absorbed by the county fixed effect. To assess this possibility, we also estimate regression (4) at the establishment level and control for establishment fixed effects that differentiate out such variations in productivity. As shown in the Appendix, all the results are robust to these establishment-level regressions and are even somewhat stronger.[35]

Figure 9 reports the point estimates for α_t together with 95% confidence bands. Panel (a) shows that counties with a higher share of delayed loans experienced lower employment growth starting the week after the exhaustion of the first round of PPP loans. This negative effect becomes stronger from May

[34] County level employment prior to the pandemic is computed from the Quarterly Workforce Indicators (QWI). Results are robust to using county population as weights or estimating the regression at the HB establishment level that implicitly weighs counties by the count of HB establishments.

[35] The establishment-level regressions also measure $sharePPPdelayed_c$ separately for each of the four service sector considered, control for week fixed effects interacted with average household income at the zip-code level, and cluster standard errors at the establishment level.

through August and remains significantly negative through the end of the sample, long after PPP ended. Prior to the roll-out of PPP, the estimates are close to zero and insignificant, indicating that the negative effect is not driven by pretrends.

Figure 9: Effect of delayed PPP loans on small business activity

(a) County employment

(b) Employment of always active businesses

(c) Business closings

(d) Newly opened businesses

Notes: Coefficient estimates of $sharePPPdelayed_c$ interacted with weekly fixed effects. Shaded areas show 95% confidence bands. All regressions are estimated over all weeks between January 5, 2020 and January 31 - February 6, 2021. $sharePPPdelayed_c$ is constructed as the amount of PPP loans issued in county c during the week of April 26 relative to the total amount of PPP loans issued per county during the weeks of April 12, April 19, and April 26. County employment in Panel (a) is the percent deviation relative to mid-February 2020 employment for all county-weeks for which the HB sample contains positive employment observations. Employment of always active businesses in Panel (b) is the percent deviation relative to mid-February 2020 employment for all establishments in a county that are continuously active throughout the entire sample. Business closings in Panel (c) is the percent ratio of the total count of establishments closed in a county in week t to the count of businesses in the reference week. Newly opened businesses in Panel (d) is the percent ratio of the total count of new establishments in a county in week t relative to the count of businesses in the reference week. All regressions control for county-specific time-varying controls as described in the text as well as week- and county fixed effects. Regressions are weighted by county employment prior to the pandemic in the four service sectors considered, and standard errors are clustered at the county level.

Given that the temporary exhaustion of PPP loans lasted for only 10 days and that the additional funding from PPP approved in late April 2020 did not get exhausted by the time the program stopped taking applications in early August 2020, the estimates raise the question of why the employment effects are so persistent. To shed light on this question, we run regression (4) separately for employment growth of businesses that are continuously active throughout the sample, business closings, and new business openings. As shown in panel (b), employment growth by always active businesses in counties with a larger share of delayed PPP loans is barely affected. There is a small decline around the week of the temporary exhaustion of PPP loans that turns marginally significant the week after PPP loans restart, but thereafter the effect is insignificant and close to zero. In contrast, as shown in panel (c), business closings jump up starting the week of the temporary exhaustion of PPP and remain significantly higher throughout the end of the sample, indicating that counties with delayed access to PPP experience permanently higher rates of business closings. As with county employment growth in panel (a), this effect is not driven by pretrends. Panel (d), finally, shows that the share of delayed PPP loans has no effect on new business openings, which confirms the validity of the design since new businesses by definition did not qualify for PPP loans.

The estimates in Figure 9 imply that the negative and persistent employment effect of delays in PPP funding is in large part driven by increased closings. Small businesses located in counties where a PPP loan was more difficult to obtain in the beginning of the pandemic were more likely to shut down and remain closed permanently. This suggests that the temporary exhaustion of PPP in mid-April 2020 occurred at a critical moment when many small business owners, faced with an unprecedented downturn amid COVID health concerns, stay-at-home orders, and business restrictions had to decide whether to continue operating and hope for loan relief from the government or cut their losses and close shop.[36]

To interpret the magnitude of the estimated coefficients, consider the difference in $sharePPPdelayed_c$ between counties at the 90th and the 10th percentile of the distribution, which is 34% (= 60% − 26%). The point estimate at the end of the sample in mid-February 2021 is about −0.15 percentage points for the effect on county employment and about −0.1 percentage points for business closings. This implies that a county at the 90th percentile of delayed PPP loans has about 5.1% lower small business employment (relative to mid-February 2020) and an about 3.4% higher rate of business closings than a county at the 10th percentile. Given that in mid-February 2021, average small business employment across the four sectors considered was about 8% below its pre-pandemic level and the average cumulative closing rate amounted to about 17%, these magnitudes are substantial.

To infer the aggregate employment effects of PPP loan delay, we follow an approach similar to Mian

[36]As an example of these difficulties, see the NPR Planet Money podcast episode 990 "The Big Small Business Rescue" from April 10, 2020.

and Sufi (2012) and Berger et al. (2020) that is also used in the PPP context by Granja et al. (2020). For each county c, we compute the difference between actual small business employment $E_{c,t}$ and counterfactual employment $\tilde{E}_{c,t}$ under the assumption that the county experienced zero delay in loans around the temporary exhaustion in PPP (which is in fact the case for a small fraction of counties); i.e.

$$E_{c,t} - \tilde{E}_{c,t} = \frac{\hat{\alpha}_t}{100} \times sharePPPdelayed_c \times E_{c,0} \tag{5}$$

where $\hat{\alpha}_t$ are the regression estimates reported in Figure 9 and $E_{c,0}$ is small business employment in county c in the pre-pandemic reference period. We then aggregate across counties using pre-pandemic employment weights.[37] The approach implicitly assumes that $sharePPPdelayed_c$ is a good measure of the difficulty of small businesses in obtaining PPP funding during the first round of loans. The approach also abstracts from possible general equilibrium effects of more timely availability of PPP loans and any other differences across counties in the difficulty of obtaining loans that are unrelated to PPP. Nevertheless, the approach is illustrative because it provides a benchmark for the overall effect of PPP and allows us to compare our estimates to other results in the literature.

For the last week of July 2020, the estimate is $\hat{\alpha}_t = -0.25$, which implies that without delays in PPP loans, employment would have been about 3 million higher, or 10% of pre-pandemic small business employment in the four sectors considered. In turn, for the last week of January 2021, the estimate is $\hat{\alpha}_t = -0.15$, which implies that without delays in PPP loans, aggregate small business employment in the four sectors would have been about 1.8 million higher, or 6% of pre-pandemic small business employment in the four sectors considered. The estimates imply that more timely availability of PPP loans could have substantially boosted the recovery of small business jobs. Given that the temporary exhaustion of PPP could have been avoided by simply appropriating a larger initial amount for PPP in the CARES Act, the costs of avoiding this delay would have been essentially zero. Vice versa, the estimates suggest that if PPP had not been part of the CARES Act, small business closings would have been substantially larger and the pandemic would have caused a much larger decline in service sector jobs.[38]

[37] Specifically, the aggregate employment effect relative to pre-pandemic employment is estimated as $\frac{E_t - \tilde{E}_t}{E_0} = \frac{\hat{\alpha}_t}{100} \sum_c \frac{E_{c,0}}{E_0} sharePPPdelayed_c$, where $E_{c,0}$ and E_0 denote pre-pandemic employment in county c and nationwide, respectively, for small businesses in the four sectors considered.

[38] We refrain from attempting to infer the overall number of jobs saved by PPP for two reasons. First, our estimates pertain to small businesses in four of the service sectors affected most by the pandemic. Larger businesses and businesses in other sectors that received PPP loans may have been less dependent on PPP funding, but our estimates do not allow us to quantify the extent of this treatment effect heterogeneity. Second, under the counterfactual assumption that the CARES Act or the subsequent COVID relief bills had not contained any funding for PPP, small businesses as a whole could have reacted very differently from the present context where PPP funds were available but temporarily ran out for a relatively short period of time. This in turn could have led to important general equilibrium effects that are difficult to quantify.

Our estimates are consistent with results in Bartik et al. (2021), Granja et al. (2020) and Doniger and Kay (2021) in that all of them find non-trivial effects of PPP on small business activity.[39] Bartik et al. (2021) use data from a survey of small businesses owners in late April 2020 during the temporary exhaustion phase of PPP. Leveraging information on existing banking relationships as instrumental variables, they find that PPP loan approval led to a 14 to 30% increase in expected survival probability and had a positive but imprecisely estimated effect on employment. Our estimates provide confirmation of this finding with actual data on employment and closings. Using the same aggregation approach as for employment above, the estimated aggregate reduction in permanent closings if there had been no delay in PPP loans is about 5%. This is lower than than the estimates in Bartik et al. (2021), which can be explained by the fact that we use actual data as opposed to expectations formed in the initial phase of the pandemic when uncertainty was likely to be higher and by the fact that our estimation only applies to PPP loan delays as opposed to the obtention of a PPP loan more generally. Granja et al. (2020) use Homebase data like us but apply a different research design that exploits local variations in the presence of banks that processed PPP loans at varying expediency. They find that over the months of April, May and June 2020, employment in small businesses would have been 4.5% higher if all banks had been equally expedient in making loans, which implicitly assumes that the initial PPP funding in the CARES Act would have been larger (i.e. no loan delays). This number is about half of our estimate. However, their estimation treats all exits from Homebase as business closings, which is likely to infuse substantial measurement error in the cross-regional variation in small business employment, thereby attenuating the estimated effect. Doniger and Kay (2021), finally, use the same event study design but with monthly household survey data from the CPS and find persistent effects of PPP loan delay on unemployment and non-participation that last through February 2021. From their estimates, they infer that a reduction in the share of delayed PPP loans by 20% would have increased employment in mid-May 2020 by a total of 4.4 million jobs or about 3.3% of pre-pandemic private sector employment. This number is larger than ours for mid-May but pertains to estimates for all businesses sizes and sectors of the U.S. economy and is therefore not directly comparable. At the same time, our result confirm that PPP loan delay had substantial employment effects and provide an explanation for why these effects are so long-lasting: it is in large part due to business closings.

Finally, Autor et al. (2020) and Chetty et al. (2020) exploit the 500 employee threshold for PPP

[39] More generally, our results are consistent with a growing literature documenting that limited cash-on-hand and working capital adversely affects labor demand and makes small businesses more sensitive to negative shocks (e.g. Chodorow-Reich, 2014, Bacchetta et al., 2019, Barrot and Nanda, 2020, or Mehrotra and Sergeyev, 2020 among others). Our results, however, put increased emphasis on the effects that these financial frictions can have on the extensive margin – i.e. business closings – which likely has more permanent effects.

loan eligibility to estimate the overall impact of PPP. Both studies find more modest employment effects, suggesting that businesses around the 500 employee threshold have been less dependent on PPP loan support, which is consistent with results by Chodorow-Reich et al. (2020). This explains why larger businesses had a much lower closing rate, which as our results reveal is the main driver of the large and persistent employment effects. In turn, the difference in effects suggests that the effectiveness of PPP would have been enhanced if the program had been restricted at least initially to the smallest businesses and would have opened only later to larger businesses – a restriction that was implemented as part of the third round of PPP in early 2021.

5.2 Relative generosity of FPUC

The second provision of the CARES Act that we study is the Federal Pandemic Unemployment Compensation (FPUC). FPUC provided an additional $600 per week on top of state UI benefits from the beginning of April through the end of July 2020 for everyone who qualified for UI.[40] As Ganong et al. (2020) estimate, the $600 supplement led to a massive increase in replacement rates, nearly tripling typical benefit levels and raising the median replacement rate to 145%, with three quarters of eligible workers receiving more in UI benefits than their previous labor earnings. Most claimants received these benefits only with several weeks of delay because the unprecedented increase in jobless claims in the beginning of the pandemic led to large backlogs in state UI offices approving and processing the payments.[41] However, claimants typically received backpay for delayed payments with their first check.

After FPUC expired, the Trump Administration issued an executive order on August 8, 2020 for Lost Wage Assistance (LWA) that was set to $300 per week and ran from August 1 to September 5, 2020. This additional supplement was not administered through the state UI systems but through the Federal Emergency Management Agency, which resulted in further delays and meant that payment in many states occurred only after the September 5 expiration.[42]

[40]The CARES Act also expanded eligibility for UI to self-employed and gig workers through the Pandemic Unemployment Assistance (PUA). Unemployed workers who qualified for UI under PUA also received the $600 in FPUC. Furthermore, the CARES Act extended benefit eligibility through the Pandemic Emergency Unemployment Compensation (PEUC) by an additional thirteen weeks for individuals who exhausted state benefits. Since most states themselves extended eligiblity, this means that most eligible workers did not exhaust benefits during the sample under consideration. See Ganong et al. (2021) for further discussion.

[41]As documented by Nunn et al. (2020), FPUC payments started as early as April 6 in Illinois and as late as April 29 in Wisconsin. However, even after the start of FPUC payments, many states experienced delays in getting the payments to claimants. See the Unemployment Insurance Dashboard by The Century Foundation for details.

[42]In late December 2020, Congress passed another round of additional UI benefits of $300 per week as part of the Consolidated Appropriations Act that took effect in 2021 and lasted for 11 weeks (through March 14, 2021). Under the American Rescue Plan passed in March 2021, these additional UI benefits were increased to $400 per week and extended through September 6, 2021, although several states have since opted to end the benefit earlier than required. These additional benefits are not the focus of the present investigation.

The increase in replacement rates from FPUC and to a lesser extent from LWA raised concerns that it would disincentivize unemployed workers to return to work. At the same time, FPUC and LWA provided a substantial increase in income for many recipients. Since most of the recipients were at the lower end of the income distribution and therefore more likely to be borrowing constrained, FPUC and LWA may thus have stimulated consumer spending. This is confirmed by Ganong et al. (2021) who find, based on individual bank account data, that spending of unemployed workers reacted strongly with benefit receipt, even in response to LWA after individuals had built up substantial liquidity. Given that FPUC alone paid out a total of \$263 billion, with one quarter of all working-age individuals receiving benefits, the positive demand effects resulting from this consumption response may have been large enough to counteract or even outweigh negative disincentive effects.

Disentangling the two effects is complicated because the disincentives on labor supply from FPUC, if present, are likely to be largest exactly in places where labor demand is stimulated most by increased consumer spending. Instead of trying to assess the two effects separately, we therefore leverage our high-frequency county level data to quantify the combined effect on small business activity. Our strategy exploits the fact that there are substantial geographical differences in labor earnings, especially by degree of urbanization. This implies that the generosity of FPUC relative to pre-pandemic earnings varied widely across counties. Using county average earnings data from the QWI for 2019 for the four service sectors under study, we show in the Appendix that the distribution of average county-level replacement rates increased from a median of 50% with a 10-to-90-percentile range of $[41\%, 57\%]$ prior to the pandemic to a median of 147% with a 10-to-90-percentile range of $[125\%, 167\%]$ under FPUC.[43] The difference between the two distributions is the change in the UI replacement rate due to FPUC; i.e. $\triangle UIrate_c = \$600/earnings_c$, where $earnings_c$ denotes pre-pandemic county average earnings in the four service sectors. For the median county, $\triangle UIrate_c$ equals 102% and the 10-to-90-percentile range across counties is $[78\%, 116\%]$.[44]

To assess the effect of $\triangle UIrate_c$, we estimate the same county-level regression as above but with $\triangle UIrate_c$ interacted with a weekly indicator; i.e.

[43] We use the state UI benefits calculator by Ganong et al. (2020) to compute these replacement rates. These distributions are remarkably similar to the ones reported in their paper despite the fact that we are using average earnings by county-industry as opposed to individual earnings for workers likely to receive UI benefits.

[44] Similar to $sharePPPdelayed_c$ above, the distribution of $\triangle UIrate_c$ for the sample of counties for which we have HB data is almost the same as the distribution for the full set of counties.

$$y_{c,t} = \sum_{t=0}^{57} \beta_t \left(1(week = t) \times \triangle UIrate_c\right) + \mathbf{X'_{c,t}}\boldsymbol{\gamma} + \phi_t + \mu_c + \varepsilon_{c,t}, \quad (6)$$

where $y_{c,t}$ denotes the same outcome variables for small business activity as before, and the β_t measure the effect in week t of the change in county c's replacement rate due the the additional \$600 per week of FPUC. The regression imposes the same set of county-week controls $\mathbf{X}_{c,t}$ as well as weekly fixed effects ϕ_t and county fixed effects μ_c as in (4).

The identifying assumption for the β_t to have a causal interpretation is that conditional on controls, $\triangle UIrate_c$ solely captures the combination of disincentive effects and demand stimulus effects of FPUC. While the different controls absorb local differences in the COVID health situation, NPIs, school closings as well as other demand factors correlated with pre-pandemic average household income (see above for a discussion), $\triangle UIrate_c$ may still pick up the effects of other unobserved confounds that are correlated with a county's pre-pandemic labor earnings but are unrelated to FPUC. Our research design does not rule out such confounds. Nevertheless, the estimates that follow are instructive to interpret other results in the literature on the effects of FPUC while at the same time providing further illustration of the importance of business closing and new openings.

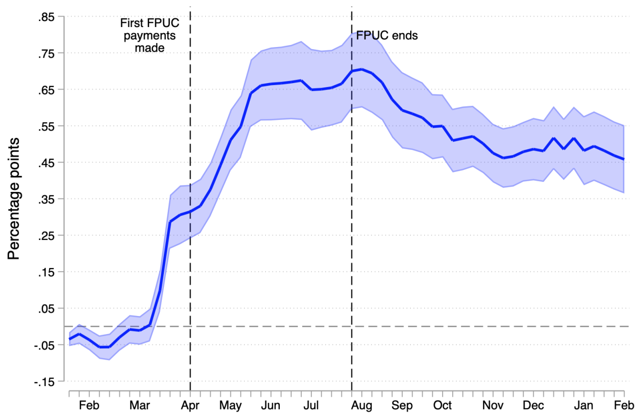
Figure 10 reports the point estimates of β_t together with 95% confidence bands. Panel (a) shows that counties where the \$600 of weekly FPUC represented a larger amount of income relative to pre-pandemic average earnings experienced a smaller decline in small business employment in the beginning of the pandemic and a stronger recovery thereafter. This effect starts in early March but stabilizes by mid-March before the first FPUC payments were made. The effect then gradually increases further from early April after the start of FPUC until late May when the effect stabilizes again at a level that is more than twice as large as in mid-March. After the expiration of FPUC in the end of July, the effect gradually declines until November when it levels off somewhat above the pre-FPUC effect.

Panel (b) shows the estimated effect of $\triangle UIrate_c$ on employment of businesses that are continuously active. The pattern is similar as for total county employment but quantitatively the effect is about two thirds smaller. This difference is primarily due to business closings. As Panel (c) shows, the rate of business closings is lower in counties in which FPUC is more generous relative to pre-pandemic earnings. Similar to employment, this effect starts in early March but stabilizes before FPUC. The effect then gradually grows larger in magnitude after FPUC starts, peaking in late May before receding during the month of June. After FPUC expires in the end of July, the effect reduces further and stabilizes by mid-November at a permanently lower level.

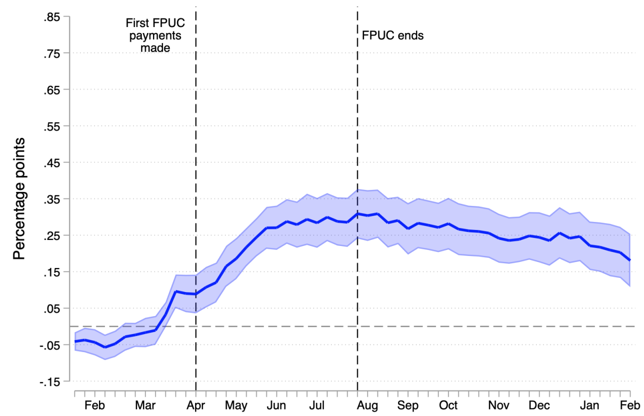
Panel (d), finally, shows the effect on new business openings. Counties with more generous FPUC relative to pre-pandemic earnings experience a larger rate of new business openings that starts in May and gradually increases over time, thereby also contributing to the positive effect on total county employment. This contribution is, however, modest relative to the effect on business closings.

Figure 10: Effect of FPUC on small business activity

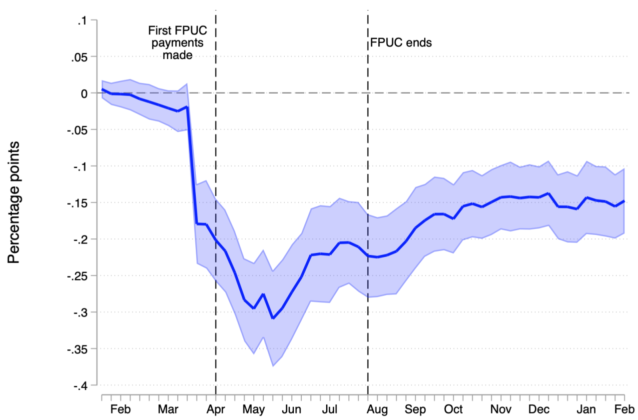
(a) County employment



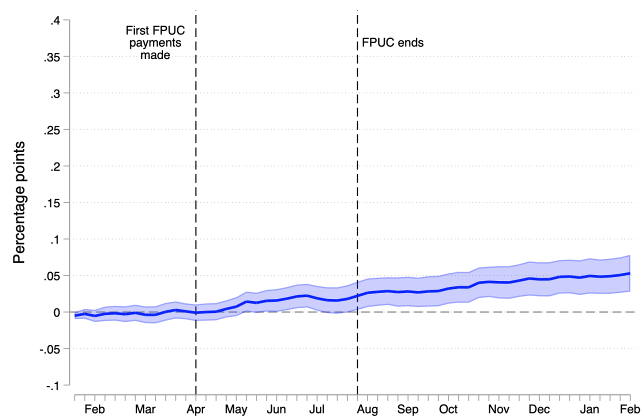
(b) Employment of continuing businesses



(c) Business closings



(d) Newly opened businesses



Notes: Coefficient estimates of $\triangle UIrate_c$ interacted with weekly fixed effects. Shaded areas show 95% confidence bands. All regressions are estimated over all weeks between January 5, 2020 and January 31 - February 6, 2021. $\triangle UIrate_c$ is constructed as the ratio of \$600 to weekly average earnings in county c in 2019 for the four service sectors considered. County employment in Panel (a) is the percent deviation relative to mid-February 2020 employment for all county-weeks for which the HB sample contains positive employment observations. Employment of always active businesses in Panel (b) is the percent deviation relative to mid-February 2020 employment for all establishments in a county that are continuously active throughout the entire sample. Business closings in Panel (c) is the percent ratio of the total count of establishments closed in a county in week t to the count of businesses in the reference week. Newly opened businesses in Panel (d) is the percent ratio of the total count of new establishments in a county in week t relative to the count of businesses in the reference week. All regressions control for county-specific time-varying controls as described in the text as well as week- and county fixed effects. Regressions are weighted by county employment prior to the pandemic in the four service sectors considered, and standard errors are clustered at the county level.

The effect of $\triangle UIrate_c$ on employment and business closings in early March prior to the start of FPUC indicates that despite the various controls and in particular the differential week fixed effects as a function of average household income, $\triangle UIrate_c$ is correlated with unobserved characteristics that predict a county's reaction during the first weeks of the pandemic.[45] At the same time, the temporary stabilization of the effect by mid-March, followed by the increase in the effect after the start of FPUC and then the decline after the expiration of FPUC suggests that FPUC had a *net* stimulative effect on small business activity. While we cannot rule out that other unobserved factors also contribute to this effect, we note that the gradual nature of the increase in the effect after FPUC starts is consistent with the aforementioned evidence from Ganong et al. (2021) on the backlog in disbursing UI benefits. Similarly, the gradual decline after FPUC ends is consistent with the delayed disbursement of $300 per week under LWA from August through October, which partially upheld the higher consumer spending.

The absence of a net negative employment effect of FPUC is consistent with several other empirical studies including by Bartik et al. (2020), Dube (2021), Finamor and Scott (2021), Ganong et al. (2021), and Marinescu et al. (2021) among others, and confirms previous results that countercyclical UI benefits are desirable in situations when aggregate demand is depressed and there is substantial slack in local labor markets as was the case during the first few months of the pandemic.[46] The novelty of our analysis is that we exploit county-week differences in the data to estimate the net effect of FPUC on small business employment and show that the effects persist well beyond the expiration of FPUC. One obvious explanation for this finding is that the recipients of FPUC only consumed part of the additional income immediately and used the rest to continue spending at a higher level thereafter. Another explanation implied by our analysis is that in counties where the stimulative effects of FPUC were relatively large, fewer businesses closed permanently and more new businesses opened.

To interpret the economic significance of our results and in light of the above discussion about possible confounds, we adopt the conservative assumption that only *half of the incremental effect* of $\triangle UIrate_c$ between April and July is due to FPUC. The difference in $\triangle UIrate_c$ between counties at the 90th and 10th percentile of the distribution is 38% ($= 116\% - 78\%$). Given our estimates for β_t, this implies that in mid-May, a county at the 90th percentile of the distribution had about 6.7% higher small business employment relative to mid-February 2020 and a 1.9% smaller closing rate than a county a the 10th

[45]The pretrends obtain even if we control for separate state-week fixed effects, implying that it is within-state differences in unobserved confounds that drive this result. Also note that the log of county average household income and $\triangle UIrate_c$ are strongly negatively related: a regression of relative county-level household income on the county replacement rate yields a slope coefficient of -0.89 with a standard error of 0.008. The R2 remains low at 0.13, however, which means that both variables have substantial explanatory power.

[46]See for example Schmieder et al. (2012); Mitman and Rabinovich (2015); Kroft and Notowidigdo (2016); or Landais et al. (2018). Also see Mitman and Rabinovich (2020) for an application of their optimal UI model for the pandemic.

percentile.[47]

Continuing with this conservative stance, we perform a similar exercise as above for PPP to infer the aggregate effects of FPUC. For each county c, we compute the difference between actual employment and counterfactual employment under the assumption that the CARES Act did not allocate any funding for FPUC. Then we aggregate across counties using pre-pandemic employment weights. For mid-May, this calculation implies that without FPUC, aggregate small business employment in the four sectors would have been 14% or about 4.2 million lower. In turn, for the last week of January 2021, the same counterfactual exercise implies a small business employment loss of 6% or 1.8 million. These estimates are large despite what we consider as relatively conservative assumptions about confounds and suggest that on net, FPUC had a substantial stimulative effect on small business employment.

6 Conclusion

In this paper, we use establishment-level data from Homebase, a scheduling and time clock software provider, to construct weekly estimates of the effect of the COVID-19 pandemic on small business dynamics and employment in four of the hardest hit service sectors. The main methodological contribution relative to the many other studies using Homebase and other high-frequency establishment-level datasets is that we match the Homebase records with independent data on business activity to distinguish business closings and openings from sample churn. Our implementation uses data from Google, Facebook, and Safegraph, but other datasets measuring business activity could be used as well. As such, we consider our paper as a proof of concept on how to harness new datasets to construct measures of business closings and openings in almost real-time that can be benchmarked to official statistics and used to measure the impact of rapidly disseminating shocks and economic policies.

Our analysis reveals that small service-sector businesses were initially affected much more negatively by the pandemic than larger businesses. However, small businesses also rebounded strongly and have, on average, recovered a larger share of their job losses than larger businesses, thereby dispelling the popular notion that small businesses have on average been hurt harder by the pandemic.

Business closings, reopenings, and new openings constitute the primary driver of the larger response of small business employment to the pandemic. Properly distinguishing openings and closings from sample churn is critical for this finding. Counterfactual approaches that either abstract from entries and exits or

[47] The difference in point estimates between mid-May and beginning of April is about 0.35 percentage points for county employment and about 0.1 percentage points for the rate of business closings. Dividing these numbers in half and multiplying them with the interquartile range of 38% produces the result in the text.

that treat all entries and exits as openings and closings would produce very different estimates.

We also exploit the high frequency and geographic granularity of our data to assess the extent to which small business activity was affected by local differences in timing and scope of PPP and FPUC. We find that small business employment was affected more negatively in counties with delayed access to PPP loans and in counties where FPUC was *less* generous relative to pre-pandemic earnings of likely recipients. Business closings account for a large part of the magnitude and persistence of the two effects. The results suggests that PPP and FPUC helped to alleviate financial constraints and stimulate demand for local services during the worst of the pandemic, but the timeliness and extent of these federal policy responses was key. This has potentially important implications for the design of economic policy in response to future crisis and in particular for the desirability of automatic stabilizers.

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