Measuring Small Business Dynamics and Employment with Private-Sector Real-Time Data*

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Abstract

Recent years have witnessed an explosion of research using private data sources to measure business dynamics and employment in near real-time and at greater detail than what is available in publicly available official data. These new data sources have become increasingly important not only for academic research but also for policymakers seeking to assess the state of the economy, especially during times of economic disruption. However, important questions remain about the reliability of these datasets, particularly with regards to accurately identifying business openings and closings and addressing potential selection bias. This paper proposes a novel methodology that leverages supplementary information on business activity to distinguish true business openings and closings from sample churn, correct for sample selection issues, and evaluate the representativeness of the resulting estimates. We apply this methodology to a widely used private dataset on small businesses during the COVID-19 pandemic and demonstrate that our approach yields substantially improved estimates of business dynamics and employment that align closely with official statistics. Our analysis highlights the importance of properly accounting for business openings and closings in estimating employment dynamics during the pandemic. In particular, we show that much of the impact of the pandemic and the effects of the Paycheck Protection Program on small businesses occurred primarily through the extensive margin (i.e., openings and closings) as opposed to the intensive margin (i.e., employment changes in continuing establishments).

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1 Introduction

The COVID-19 pandemic led to an unprecedented disruption of the U.S. economy, with particularly severe impacts on service sectors that rely on in-person interaction. A burgeoning literature has examined the effects of the pandemic on these sectors using a variety of novel private establishment-level data sources (e.g., Bartik et al., 2020; Cajner et al., 2020; Chetty et al., 2023). The advantage of these datasets is that they provide information in near real-time at considerably greater detail and higher frequency than the official data made available by statistical agencies.[1] Analysis with these datasets therefore received close attention from both researchers and policy-makers, especially in the beginning of the pandemic, likely contributing to the swift response of the U.S. government in providing economic relief.[2]

At the same time, and as acknowledged in the literature, important challenges remain in using private data sources to accurately measure the state of the economy, especially during times of disruption. These datasets often experience high turnover and changes in sample size, which can lead to substantial discrepancies between true business openings and closings and sample entry and exit. Additionally, these datasets are convenience samples that may overrepresent certain types of businesses. This is particularly concerning when estimating small business dynamics, which account for a large fraction of employment in the service sectors most impacted by the pandemic and exhibit high and time-varying opening and closing rates. Moreover, the selection of small businesses into these datasets may be heavily influenced by the economic environment. As such, the reliability of real-time employment estimates derived from private data sources remains an open question, emphasizing the need for further research and validation to address these sample churn and selection issues.

The paper makes two main contributions. First, we propose a new methodology that leverages supplementary information on business activity to distinguish business openings and closings from sample churn, correct for sample selection, and assess the representativeness of the resulting estimates. Applying this methodology to high-frequency data from Homebase, a scheduling and payroll software provider used by over 100,000 businesses in the U.S., we show that it produces reliable estimates of small business dynamics and employment that align well with administrative benchmarks, even during the volatile pandemic period. As such, the paper provides a use-case on how to construct employment estimates in

[1] As described in detail below, statistical agencies in the U.S. publish monthly survey estimates for the labor market, but these estimates become available only with a lag of several weeks and contain limited breakdowns by industry and establishment size. Official data on the *population* of establishments and employment are available only at quarterly or annual frequency and are released several months to several years later. The situation in other countries is similar.

[2] See for instance the special edition of the Brookings Papers on Economic Activity on COVID-19 and the economy in June 2020; or the many press articles about Opportunity Insight's Economic Tracker (e.g., Steverman, 2020; Matthews, 2020; Long, 2021).

near real-time that directly incorporate the effects of business openings and closings. By improving the accuracy of these estimates, our methodology therefore enhances the potential for private data sources to complement official statistics and support timely, data-driven policy decisions, especially during periods of economic uncertainty.

Second, we use our estimates to shed new light on two important questions: were small businesses hit harder by the pandemic than larger businesses; and to what extent did the Paycheck Protection Program (PPP) mitigate these effects? We find that small business employment in hard-hit sectors, like Leisure & Hospitality, experienced considerably larger initial declines but also more rapid recoveries than employment of larger businesses, and that PPP contributed significantly to the rapid recovery. While these results broadly validate existing estimates in the literature that do not directly account for business openings and closings, our analysis also highlights quantitative differences and provides new insights. In particular, we show that much of the impact of the pandemic and the effects of PPP on small businesses occurred through the extensive margin (i.e., openings and closings) as opposed to the intensive margin (i.e., employment changes in continuing establishments). This has important policy implications and further emphasizes the importance of properly accounting for business openings and closings when using private data sources to estimate the impact of economic disruptions.

Sections 2 through 4 of the paper describe the methodology, the data used to implement the proposed method, and the performance of the resulting estimator. Our approach to distinguish business openings and closings from sample churn consists of matching establishment records with information on business activity from Google Places and Facebook (although other sources could be used as well). This provides us with real-time information on whether and for how long a business is active, thus allowing us to estimate whether a business entering the sample is an opening, respectively whether a business exiting the sample is a closing. Next, we adjust these estimates for selection by benchmarking the implied birth and death rates against data from the Bureau of Labor Statistics' (BLS) Business Employment Dynamics (BED) and the U.S. Census Bureau's Business Dynamics Statistics (BDS) *prior to the pandemic*. Finally, we test the performance of the estimator by comparing the resulting small business dynamics and employment numbers *during the pandemic* with official population counterparts from the BED/BDS as well as the Quarterly Census of Employment and Wages (QCEW) that became available after the fact.

We apply the proposed method to the case of Homebase both because Homebase has experienced strong growth and client turnover, and because the majority of the establishments in the dataset are small (less than 50 employees), operating in the in-person service sectors that were most affected by the pandemic. As such, Homebase provides an excellent test case to assess the performance of the estimator under extreme circumstances.

We find that the resulting estimates provide a close fit of the official population counterparts. In addition, given the relatively sparse frequency of official data sources and to further assess the representativeness of the Homebase sample, we match the Homebase establishments with Safegraph, a database containing weekly visits from anonymized cell-phone data to over 7 million places of interest, including many small service-sector businesses. We find that visits to the establishments in Homebase move in lockstep with average visits to all corresponding small businesses in Safegraph. Together, the results provide compelling evidence that the proposed method produces reliable estimates of small business dynamics and employment even during extraordinary times such as the pandemic.

Properly distinguishing openings and closings from sample churn is crucial for these results. Counterfactual employment estimates that include only continuously open businesses or that treat all entries and exits as openings and closings would produce very different estimates. As such, the paper offers a cautionary tale about approximations that various studies using private data sources adopted to produce real-time employment estimates during the pandemic (see below for a review). Similar challenges also apply for official establishment surveys, including the Current Employment Statistics (CES) that forms the basis of the BLS's widely regarded monthly payroll estimate. Indeed, our analysis suggests that the CES did not appropriately take into account temporary closings and reopenings of small businesses in the beginning of the pandemic, thus underestimating the contraction and subsequent recovery in employment.[3]

Sections 5 and 6 exploit the Homebase data to provide new evidence on whether small businesses were hit harder by the pandemic than larger businesses, and the extent to which PPP helped small businesses maintain employment and recover from the pandemic. The analysis yields three main insights that are directly informed by our estimates of business openings and closings. First, employment of small businesses in Retail Trade, Education & Health, Leisure & Hospitality and Other Services – the four service sectors hit hardest by the pandemic – contracted two to three times as much as employment of larger businesses during the first two months of the pandemic. But in the months following the initial shock, small business employment recovered more strongly than employment of larger businesses. Temporary business closings account for almost three quarters of these large swings in small business employment. Specifically, our estimates imply that in mid-April 2020, 40% of all small businesses in the four service sectors considered were closed. In the months thereafter, about two thirds of these businesses

[3]Section 2 provides details about the "birth/death model" that the BLS uses to adjust CES employment estimates. Important questions remain about the accuracy of this adjustment method even after the pandemic, as highlighted for instance by Ashworth (2023) or Guilford (2023).

reopened, resulting in a cumulative closing rate one year after the beginning of the pandemic that is only about two percentage points higher than the cumulative closing rate over the same time period one year prior. Hence, and in line with Decker and Haltiwanger (2022) and Fairlie et al. (2023) based on ex-post available administrative data, the pandemic did not lead to substantially higher rates of permanent shutdowns.

Second, small business employment in the four sectors had fully returned to its pre-pandemic level by Spring 2021, in large part because of new businesses openings. This is consistent with evidence on the surge in new business applications by Fazio et al. (2021) and Haltiwanger (2022). But to our knowledge, no one has reported the employment consequences of these new openings.

Third, the timely rollout of PPP in the beginning of the pandemic contributed significantly to the fast recovery of small business employment, primarily by lowering the rate of business closings / increasing the rate of business reopenings. We reach this conclusion through a research design proposed by Doniger and Kay (2023) that exploits plausibly exogenous local differences in the delay of obtaining a PPP loan due to the temporary 10-day exhaustion of PPP funding in mid-April 2020. Applying this design to a county-by-week panel built from our Homebase sample, we estimate that in counties with the least delay in PPP loans, the rate of business closing was about five percentage points lower in June/July 2020 than in counties with the most delay in PPP loans; and about half of this difference in closing rates is estimated to persist through the end of our analysis in February 2021. The long-lasting effect of the temporary PPP delay on business closings, which is conditional on a rich set of controls and not driven by pretrends, suggests that PPP occurred at a critical moment when many small business owners, faced with an unprecedented downturn, had to decide whether to cut their losses and permanently close shop. In contrast, we find only a small and insignificant difference in employment among continuously open businesses across these counties, suggesting that conditional on a business remaining in operation, PPP was not necessary for maintaining jobs.

Overall, our findings indicate that the extensive margin – i.e., openings and closing of small businesses – is crucial to understand the unprecedented decline and subsequent recovery of in-person service employment during the pandemic. This is especially true for our analysis of the effects of PPP, which implies that despite the many valid critiques of the program in terms of inadequate targeting (Bartik et al., 2021) and evidence of fraud (Griffin et al., 2023), the timeliness of the initial rollout in the beginning of the pandemic was important to keep small businesses from closing permanently. At the same time, the fact that employment in continuously open businesses was not affected suggests that other support programs that directly subsidize business operations instead of primarily jobs could have been equally effective at a lower cost. Indeed, according to several studies (e.g. Autor et al., 2022b), the follow-up round of PPP in 2021 has not had discernible effects on small business employment. Given our result that small business employment had fully recovered by that time, this is perhaps not surprising.

Relation to literature. The paper relates to a by now extensive literature using private establishment-level data sources to measure the economic impact of the COVID-19 pandemic; e.g., for the U.S., Bartik et al. (2020), Cajner et al. (2020), or Chetty et al. (2023). Other studies that focus on employment of smaller businesses include Bartik et al. (2020), Dalton et al. (2020), Fairlie (2020), Fairlie et al. (2023), or the Small Business Pulse Survey by the U.S. Census Bureau.[4] Many of these studies acknowledge the difficulty of distinguishing business openings and closings from sample churn, and implicitly or explicitly adopt various approximations that, as we show, can lead to large biases in employment estimates.[5] One of our main contributions is to propose a new method to address this difficulty, adjust for sample selection, and assess the representativeness of the resulting estimates. We show that properly taking into account the extensive margin, i.e., business openings and closings, is crucial to understand small business employment during the pandemic – a result for which we received substantial media coverage (e.g., Chaney et al., 2020; Ip, 2020; Lahart, 2020; White, 2020).

There is also a set of studies that specifically consider business closings with private real-time data and note a large but temporary spike in the beginning of the pandemic; e.g., Chetty et al. (2023), Crane et al. (2022), or Vaan et al. (2021). Perhaps most relevant is Crane et al. (2022) who highlight the conflating effect of client turnover on the measurement of business closing rates in various private establishment-level data sources. They then use Safegraph visits data to identify temporary and permanent business closings. While interesting, the Safegraph data alone cannot be used to infer the employment consequences of business closings, and as discussed in Section 3, our own investigation with Safegraph reveals that the visits data at the establishment-level may be too noisy to reliably identify business closings.

Finally, our paper relates to the literature on the effects of PPP. Autor et al. (2022a), Chetty et al.

[4]Other important contributions based on individual surveys, data on UI claims, or vacancy postings to measure the impact of the pandemic include Coibion et al. (2020), Forsythe et al. (2020), Bick and Blandin (2021), or the Household Pulse Survey by the U.S. Census Bureau, among many others.

[5]For instance, Bartik et al. (2020) who also use the Homebase data, estimate employment and total hours worked during the pandemic based on a sample of firms that are active in January 2020. This effectively treats all exits from Homebase as closures and eliminates any entry effects. Cajner et al. (2020), in turn, consider all exits and entries as closings and openings, respectively, cautioning explicitly *"...against interpreting [their entry estimates] in terms of genuine new business formation."* (page 23). Indeed, as discussed above and detailed further in Section 4, either of these two approaches result in increasingly inaccurate employment estimates over the course of the pandemic recovery. Alternatively, Chetty et al. (2023) introduce an adjustment procedure in their employment estimator that downweighs estimation cells with employment growth rates that are above or below certain thresholds deemed anomalous and therefore indicating sample churn. The weights and thresholds used are not further justified, however. It therefore remains unclear to what extent this procedure corrects for sample churn and is applicable more generally.

(2023), and Hubbard and Strain (2020) exploit the 500 employee threshold for PPP loan eligibility and find only limited effects. Concurrent studies by Bartik et al. (2021), Bartlett and Morse (2021), Doniger and Kay (2023), and Granja et al. (2022) based on alternative identification techniques generally find larger employment effects for small businesses.[6] Our estimates validate this conclusion and in particular the finding of Doniger and Kay (2023), whose research design we adopt, that the 10-day PPP loan delay led to persistent negative employment effects. Relative to their study, which is based on monthly household survey data, our analysis is based on weekly establishment records and focuses squarely on the smallest businesses in the four service sectors hit hardest by the pandemic. As a result, our estimates are larger in magnitude and generally more precise, showing that the effects start exactly during the weeks when the PPP loan delay occurs. More importantly, the main contribution of our paper to the literature on PPP is to provide an explanation for why the temporary exhaustion of PPP loans had long-lasting effects: it is due to the adverse effects that this delay had on business closings. This finding was confirmed subsequently Autor et al. (2022b) and Dalton (2023) who compare employment and closing probabilities of businesses who received a PPP loan earlier with those of businesses who received a PPP loan later.

2 Estimating small business dynamics and employment

Consider estimating employment for a particular segment of the economy (e.g., businesses with fewer than 50 employees in the Leisure & Hospitality sector) from a sample of establishments. Starting with reference employment level $\widehat{E}_0$ (taken, e.g., from administrative population statistics such as the QCEW), the estimate for employment in week t can be computed recursively as

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

where ω_i denotes a sampling weight for cell i (e.g. at the industry-size-region level), constructed as the ratio of establishment population counts to sample counts in that cell in the reference period; $\hat{e}_{i,t}^{\mathcal{A}_{i,t}}$ denotes employment of the set of establishments $\mathcal{A}_{i,t}$ that are active in the sample in both week t and $t-1$; $\hat{e}_{i,t}^{\mathcal{O}_{i,t}}$ denotes employment gains from the set of establishments $\mathcal{O}_{i,t}$ that are newly opening or reopening in week t; and $\hat{e}_{i,t-1}^{\mathcal{C}_{i,t}}$ denotes employment losses from the set of establishments $\mathcal{C}_{i,t}$ that are closing temporarily or permanently in week t.

[6] Granja et al. (2022) also use Homebase data to estimate the effects of PPP. Similar to the "active firm" approach used by Bartik et al. (2020), they define a business as being temporarily closed if it has zero hours in a given week, and permanently closed if it has zero hours through the end of their sample (end of August 2020). This approach conflates permanent closures with sample churn which, as we show, is quantitatively important.

A key challenge in constructing this estimate is how to distinguish business openings and closings from sample churn; i.e., entry of businesses that already operated previously, and exit of businesses that continue to operate thereafter. Sample churn is an important concern for any dataset that experiences large turnover and/or changes in sample size that are different from the growth rate of establishments in the underlying population (e.g., due to changes in customer acquisition efforts or survey response rates). As illustrated in Sections 3 and 5, this means that simple strategies such as treating all entries as openings, respectively treating all exits as closings can result in large overestimates of $\hat{e}_{i,t}^{\mathcal{O}_{i,t}}$ and $\hat{e}_{i,t-1}^{\mathcal{C}_{i,t}}$, which in turn lead to spurious employment estimates. Likewise, ignoring all entries and exits (i.e., setting $\hat{e}_{i,t}^{\mathcal{O}_{i,t}} = 0$ and $\hat{e}_{i,t-1}^{\mathcal{C}_{i,t}} = 0$) and effectively estimating employment from the set of continuing establishments $\mathcal{A}_{i,t}$ can produce equally spurious employment estimates during large business cycle swings such as the pandemic when there are important changes in the relative rate of establishment openings and closings.

The main methodological contribution of the paper is to construct direct estimates of $e_{i,t}^{\mathcal{O}_{i,t}}$ and $e_{i,t-1}^{\mathcal{C}_{i,t}}$ by exploiting information on individual business activity from alternative sources. Conceptually, we estimate employment gains from establishment openings as

$$\hat{e}_{i,t}^{\mathcal{O}_{i,t}} = \sum_{\ell \in i} \hat{p}(\mathcal{O}_{\ell,t} | \text{entry}_{\ell,t}) \times e_{\ell,t} \times \theta_{i,t}^{\mathcal{O}}, \tag{2}$$

where $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t})$ denotes the estimated probability that establishment ℓ is an opening conditional on entering the sample in week t; $e_{\ell,t}$ measures employment of establishment ℓ at entry; and $\theta_{i,t}^{\mathcal{O}}$ is an adjustment factor that corrects for potential selection issues such that the resulting establishment birth rate is consistent with population benchmarks from administrative data. Similarly, we estimate employment losses from establishment closings as

$$\hat{e}_{i,t-1}^{\mathcal{C}_{i,t}} = \sum_{\ell \in i} \hat{p}(\mathcal{C}_{\ell,t} | \text{exit}_{\ell,t}) \times e_{\ell,t-1} \times \theta_{i,t}^{\mathcal{C}}, \tag{3}$$

where $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$ denotes the estimated probability that establishment ℓ is a closing conditional on exiting the sample in week t; $e_{\ell,t-1}$ measures employment of establishment ℓ prior to exit; and $\theta_{i,t}^{\mathcal{C}}$ is an adjustment factor that corrects for potential selection issues such that the resulting establishment death rate is consistent with population benchmarks from administrative data.

Before describing the estimation of (2) and (3), it is instructive to compare our estimator to the CES estimator that the BLS uses for its monthly Employment Situation. While the recursive approach in (1) is conceptually similar to the CES estimator, the CES estimator historically only included establishments reporting positive employment in months t and $t-1$, effectively imputing employment of establishments not responding or responding zero employment based on the employment of establishments reporting positive employment. In a second step, the estimator then adjusted for the residual difference between births and deaths with an econometric prediction based on past data. In April 2020, responding to the extraordinary increase in business closings, the BLS modified this "net birth/death model" to include a portion of the establishments that reported zero employment in month t and establishments that return to positive employment in month t, respectively, and current employment was added as one of the predictors in the econometric adjustment. In October 2021, the BLS then reverted to the original methodology.[7]

As we discuss in Section 5, despite the modifications to the net birth/death model, the CES estimator is unlikely to have accurately reflected the effects of the large changes in business openings and closings that occurred in the beginning of the pandemic. Our approach, by contrast, exploits direct information on business activity of establishments that enter and exit the sample. In addition, since our approach yields estimates of opening and closing probabilities, the resulting establishment birth and death rates can be readily benchmarked against population counterparts from administrative data.

2.1 Identifying business openings and closings

Estimation of $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t})$ and $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$ requires that we can match at least a subset of entering and exiting establishments in the sample to information on business activity from alternative sources. In our application, we leverage information from Google Places and Facebook to do so, although other sources could be used as well. Section 3 describes this data as well as the matching process.

To identify business openings, we define all establishments that appear in the sample in week t but were not present in week $t-1$ as entrants and proceed in three steps. First, we check whether an entrant was present in the sample at any point before week $t-1$. If so, we classify the entrant as a reopening and assign probability $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t}) = 1$. Second, for an entrant that appears in the sample for the first time in week t, we match it to our alternative sources and assign probability $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t}) = 1$ if there is no indication of business activity prior to entering the sample, and $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t}) = 0$ otherwise. Third, for entrants that we cannot match to our alternative sources or for which we do not have reliable information, we assign probability $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t})$ equal to the cell i average probability obtained in the previous step.

To identify business closings in week t, we proceed similarly and define all establishments that are

[7] See https://www.bls.gov/web/empsit/cestn.htm for details on the CES employment estimator and https://www.bls.gov/web/empsit/cesbd.htm for details on the net birth/death model.

present in the sample in week $t-1$ but disappear in week t as exiters. Then, we first check whether an exiter reappears by the end of the sample. If so, we classify the exiter as a temporary closing and assign probability $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t}) = 1$. Second, for exiters that do not reappear by the end of the sample, we match them to our alternative sources and assign probability $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t}) = 1$ if there is no indication of business activity after exiting, and $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t}) = 0$ otherwise. Third, for exiters that we cannot match to our alternative sources or for which we do not have reliable information, we assign probability $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$ equal to the cell i average probability obtained in the previous step.

2.2 Adjusting for selection

The second part of our methodological contribution consists of calculating adjustment factors $\theta^{\mathcal{O}}_{i,t}$ and $\theta^{\mathcal{C}}_{i,t}$ such that the birth and death rates implied by our estimates are consistent with population counterparts from administrative data. The point of this benchmarking is that while population data on openings and closings typically become available only with substantial delay and at lower frequency and/or coarser detail than desired – hence the usefulness of constructing estimates from private-sector data – we want to take into account possible selection issues that arise if the establishments exiting and entering the sample have a different propensity to be closings and openings than in the population. Likewise, we want to correct for possible systematic measurement error about business activity in the alternative data sources used to estimate $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t})$ and $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$.

For openings, we start by calculating an adjustment factor $\theta^{\mathcal{O}}_{i}$ for each establishment $\ell \in i$ that newly enters the sample during the *reference period* for which we have administrative data available such that the average birth rate in cell i implied by the thus adjusted probability of new openings equals its population counterpart $p(\text{birth}_i)$.[8] For the *estimation period*, we then allow the adjustment factor to be time-varying as a function of the number of new entrants relative to total sample size; i.e., $\theta^{\mathcal{O}}_{i,t} = \theta^{\mathcal{O}}_{i} \times \left(\frac{n^{\text{entry}}_{i,t}/n^{\mathcal{A}}_{i,t}}{n^{\text{entry}}_{i,0}/n^{\mathcal{A}}_{i,0}}\right)^{-1}$. The reason for this time-variation, which is akin to inverse probability weighting, is that the rate of entry of new establishments is not naturally bounded by the existing sample and can vary importantly if the sample expands (or contracts), e.g., as a result of changes in customer acquisition efforts by the private-sector data provider or changes in the underlying survey sampling frame.[9]

For closings, we proceed similarly and calculate an adjustment factor $\theta^{\mathcal{C}}_{i}$ for each establishment $\ell \in i$

[8] To take account of possible seasonalities, this adjustment factor for the reference period can vary by the frequency (e.g., by quarter) at which the administrative data is available.

[9] As a simple example, suppose that there are no selection or measurement issues; i.e., $\hat{p}(\mathcal{O}_{\ell}|\text{entry}_{\ell}) = p(\text{birth}_i)$ for all $\ell \in i$ during the reference period. Now consider a sample expansion that doubles the rate of entry $n^{\text{entry}}_{i,t}/n^{\mathcal{A}}_{i,t}$. Assuming that these additional entries come randomly from the population such that $\hat{p}(\mathcal{O}_{\ell}|\text{entry}_{\ell})$ remains unchanged, then employment gains from new openings would spuriously double as well, thereby leading to an overestimation of employment growth. By making the adjustment factor time-varying as a function of $n^{\text{entry}}_{i,t}/n^{\mathcal{A}}_{i,t}$, we control for this issue.

that permanently exits the sample during the *reference period* for which we have administrative data available such that the death rate in cell i implied by the thus adjusted probability of permanent closings equals its population counterpart $p(\text{death}_i)$.[10] Since exits occur on the available sample and are therefore naturally bounded, we do not need to add further time-variation to this adjustment factor. That is, while closing probabilities $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$ and therefore death rates vary during the *estimation period*, the adjustment factor $\theta_i^{\mathcal{C}}$ that corrects for possible selection and measurement issues remains constant.

3 Data and implementation

The data we use to implement our employment estimator comes from Homebase (HB), a scheduling and payroll administration provider, used primarily by small, independently owned businesses employing fewer than 50 workers. The majority of the businesses operate in service sectors with a large propensity for in-person interaction that were most exposed to the disruptions and stay-at-home orders in the beginning of the COVID-19 pandemic. As such, the HB data is particularly well-suited to assess our approach.

In addition to the publicly shared data, HB provides us with name and address for each establishment, which we use to match to additional information on these businesses from Facebook, Google, and Safegraph. This is a key advance over other studies using the HB data that allows us to attribute a consistent industry classifier to each establishment; assess the representativeness of the HB sample; and – most importantly – distinguish business openings and closings from sample churn. The matching procedure involves extensive data cleaning and standardization before relating the establishment records sequentially by exact merges and then fuzzy name match and substring match algorithms. The Appendix provides details on the different steps as well as match statistics. We only retain HB establishment records that match exactly or with a high match quality rate.[11]

3.1 Employment and business activity

The HB 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 (almost all businesses are single-establishment firms). 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

[10]As for openings, this adjustment factor for the reference period can vary by the frequency (e.g., by quarter) at which the administrative data is available so as to take into account possible seasonalities.

[11]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.

access to researchers and updates the data frequently with the latest observations.

For each establishment in the HB sample, we construct weekly employment as the sum of individuals with tracked or scheduled hours during that week plus owners and managers that show activity in the HB software but do not have tracked hours.[12] Including owners and managers broadens employment coverage beyond hourly paid workers, but does not materially affect the results.

For an establishment to be retained in our sample, it must show up for at least 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 active in a given week, it must have employees with tracked or scheduled hours in that week. Establishment activity is therefore independent of owners and managers logging in to the software (e.g. for reporting purposes).

3.2 Industry classification

The historical HB data come 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. We address this issue by using the above-described match of HB establishments with Safegraph, which contains consistent NAICS-6 industry coding for over 7 million Places of Interest (POIs) in the U.S., including a large fraction of all private-sector establishments.[13]

3.3 Sample characteristics and representativeness

The sample we consider covers January 2019 through November 2021. The beginning of the sample is imposed by the availability of sizable HB data.[14] The end of the sample is due to data limits we faced at the time of matching HB establishments to Facebook.[15]

[12] Aside from establishment names and addresses, HB also shares with us scheduled hours of employees when available as well as daily login activity of owners and managers. Since some establishments only use the HB software for scheduling but not tracking of hours, this information allows us to work with a larger sample of establishments. 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 constitute an accurate measure of actual hours worked.

[13] See the Appendix for details on the Safegraph data and summary statistics about the NAICS industry codes for the matched HB establishments. In December 2020, HB independently started attributing NAICS industry codes for each establishment in their dataset. This classification is available only for establishments active from that month onward. Since many establishments that were active in 2019 and 2020 exited the HB sample before December 2020, the HB NAICS codes are not directly useful for our analysis. However, for establishments active in December 2020 and beyond, we find a high level of overlap between our NAICS codes and the HB NAICS codes.

[14] HB data is available starting in January 2018, but the sample size is relatively small until 2019.

[15] CrowdTangle, Facebook's research database, imposed limits on how many records we were allowed to upload and match. While these limits could possibly be increased, the main purpose of the paper is not about providing real-time estimates, but how to measure business dynamics and employment with private data.

The raw HB data contains about 300,000 distinct establishments; however, many of them do not use HB regularly and therefore do not satisfy our retention criterion. The sectors with the largest coverage are Leisure & Hospitality (NAICS 71 and 72), Retail Trade (NAICS 44-45), Education & Health Services (NAICS 61-62), and Other Services (NAICS 81).[16] To ensure good coverage, we focus our analysis on these four sectors and exclude all establishments with 50 employees or more.[17] According to official statistics, establishments with fewer than 50 employees accounted for about half of all jobs across the four sectors and for about 23% of all private-sector jobs prior to the pandemic. Hence, the segment covered by our analysis represents a sizable share of total employment.

Table 1: Establishment counts of retained Homebase sample

	Feb. 2019 - Feb. 20		Feb. 2020 - Nov. 21	
Mid-February base sample	38,193	(100%)	49,268	(100%)
- active in mid-February	34,757	(91.0%)	45,454	(92.3%)
- temporarily inactive in mid-February	3,436	(9.0%)	3,814	(7.7%)
Exits without return	13,289	(34.8%)	28,256	(57.4%)
New entrants	25,149	(65.8%)	37,777	(76.6%)

Notes: The first column shows counts of HB establishments from mid-February 2019 to mid-February 2020, and the second column shows counts of HB establishments from mid-February 2020 to the end of November 2021 that (i) were successfully matched 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.

Table 1 reports the number of establishments that satisfy our retention criterion and that we can match with a high degree of confidence to Safegraph. Across the entire January 2019 to November 2021 period, the sample contains about 100,000 distinct establishments. The mid-February 2019 base sample consists of 38,193 establishments that show activity between the beginning of January 2019 and the second week of February 2019 of which 34,757 are active in the second week of February.[18] For the mid-February 2020 base sample, the corresponding establishment counts are 49,268 and 45,454. From mid-February 2019 to mid-February 2020, there are 13,289 exits without return and 25,149 new entrants, and from mid-February 2020 to late-November 2021, there are 28,256 exits and 37,777 new entrants. Foreshadowing the discussion below, these entry and exit rates are much larger than birth and death

[16]See the Appendix for details. Other Services includes "Repair & Maintenance" (NAICS 811) and "Personal & 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". Aside from these four sectors, the HB data also contains several hundred establishments each in "Utilities" (NAICS 22), "Construction" (NAICS 23), "Food, Textile & Apparel Manufacturing" (NAICS 31) and "Real Estate, Rental & Leasing" (NAICS 53).

[17]We also exclude "Non-store Retail" (NAICS 454) and "Private Households" (NAICS 814) because HB contains only very few establishments in these industries, and the QCEW that we use to assess representativeness does not contain these industries.

[18]The remaining 3,436 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 rate of temporary closings prior to the pandemic.

rates in the official statistics, implying that the HB data is subject to considerable sample churn.

It is instructive to compare the size of our HB sample to the number of small businesses in the four sectors sampled by the CES. While the BLS does not publish breakdowns by industry and size, we know that in 2021 the CES sampled about 45,000 businesses in the four sectors considered, and the share of businesses with fewer than 50 employees sampled across all sectors was 60%.[19] Assuming that this share is similar across industries, the CES therefore sampled about 27,000 small businesses in the four sectors considered. By this calculation, our HB sample is considerably larger.

An important question with opportunity samples is whether they are representative of the larger population – in our case, whether the HB sample has similar characteristics as the universe of small businesses in the four sectors considered. First, note that the estimator in (1) corrects for distributional differences by weighing each cell i by the ratio of establishment population counts to sample counts. In our applications below, we define cells at the industry-size-region level and use information from the QCEW for the first quarter of 2020 as population counts.[20] Hence, for representativeness to be an issue, it would need to be the case that HB establishments are systematically different from their population counterparts *within* the different cells. To assess this possibility, we compare the pre-pandemic average number of employees per establishment in each industry-size cell with the QCEW counterparts. As shown in the Appendix, the HB and QCEW numbers are very close to each other, suggesting that the HB establishments are highly representative within industry-size cells.

In Section 4, we further assess the representativeness of the HB data by comparing annual growth rates of small business employment implied by our estimates with population counterparts from the QCEW. In addition, given that administrative data by establishment size and industry is publicly available only annually, we pursue a novel approach that exploits our match of HB establishments with Safegraph, which contains not only industry classifiers for each POI but also weekly visits patterns based on anonymized cell-phone data. This allows us to compare weekly visits to the establishments in our sample with visit to all small establishments covered by Safegraph.

[19]The CES survey includes about 130,000 businesses (UI accounts), which cover approximately 670,000 individual work-sites or establishments. Hence, many of the businesses sampled are larger, multi-establishments firms.

[20]The Appendix provides details on distributional differences relative to the QCEW. While the HB sample has generally good coverage across all industry-size class cells, it under-represents the smallest establishment size class (1-4 employees) and over-represents the other size classes (5-9, 10-19, and 20-49 employees). Furthermore, the HB sample over-represents NAICS 72 (Accommodation & Food Services) at the expense of NAICS 62 (Health Care & Social Assistance) and NAICS 81 (Other Services), and the geographic distribution skews slightly towards Florida and Texas at the expense of California.

3.4 Using Google and Facebook data to identify business openings and closings

To identify business openings and closings, we exploit information from Google Places and Facebook. For each HB establishment that we can match, Google Places provides us with a tag on whether the business is "temporarily closed" or "permanently closed", while Facebook provides us with a longitudinal record on the business's posting activity.

As described in Section 2 and further detailed in the Appendix, for all establishments that appear in HB for the first time in week t, we assign probability $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t}) = 1$ if they start posting regularly on Facebook only after entering HB. Otherwise, if they already posted regularly before entering HB, we assign probability $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t}) = 0$. For all other entrants that we cannot match to Facebook or that do not post regularly after entering HB, we assign probability $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t})$ equal to the industry-size cell average probability obtained in the previous step.

In turn, for all establishments that exit HB in week t and do not reappear by the end of the sample, we assign probability $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t}) = 1$ if Google tags them as "temporarily closed" or "permanently closed".[21] These tags are reported by business owners and customers but cover only a subset of all closed establishments. Hence, as a second step, we check on Facebook whether establishments with regular posting histories stop posting regularly after exiting HB. If so, we assign probability $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t}) = 1$. Otherwise, if they continue posting regularly, we assign probability $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t}) = 0$. For all other exits that we cannot match to either Google or Facebook or that do not post regularly on Facebook while in the HB sample, we assign probability $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$ equal to the industry-size cell average probability obtained in the previous step.

As a basic quality check of our identification, we exploit the match with Safegraph and compare visit patterns for the different types of establishments in our HB sample. First, we verify that exits identified as business closings exhibit a large drop off in average weekly visits relative to establishments that remain active in HB. Second, we verify that entries identified as new openings appear in Safegraph only after entering HB. Third, we verify that exits and entries identified as sample churners show average visit patterns similar to those of establishments that remain active in HB. These results, which are available in the Appendix, provide independent support for our approach to identify business closings and new openings.[22]

[21] We include both Google tags "temporarily closed" and "permanently closed" because "temporarily closed" is sometimes updated to "permanently closed", and because our first step only identifies temporary closings that reappear in HB by the end of the sample.

[22] In principle, the Safegraph visits data could be used to identify *individual* establishment openings and closings from their visit patterns. For instance, Crane et al. (2022) define a Safegraph POI as closed if year-over-year visits decline by more than a certain threshold. However, we found after extensive analysis that the Safegraph visits data can be very noisy at

3.5 Benchmarking with administrative data from the BED / BDS

A potential concern with our estimation of opening and closing probabilities is that the establishments entering and exiting HB are not representative of small businesses. A related concern is that the establishments captured by Google Places and/or posting on Facebook are a particular subset of our HB sample. Finally, it may be that the information from Google Places and Facebook is subject to systematic error. To adjust for these selection and measurement issues, we benchmark the establishment birth and death rates implied by our identification against administrative counterparts, as described in Section 2. In the next section, we further assess representativeness using the Safegraph visits data.

The administrative data for birth and death rates we use comes from the BED and the BDS. The BED consists of all longitudinally linked establishments from the QCEW and reports birth and death rates by sector but *not by sector and establishment size class* at quarterly frequency with a delay of about six months. The BDS, in turn, consists of all longitudinally linked employer establishments from U.S. tax records and reports entry and exit rates defined similarly as BED birth and death rates by industry and size class, but only at an annual frequency, measured in March of each year.[23]

Since small establishments have substantially higher birth and death rates even in normal times, and since we want to assess the performance of our methodology by comparing the resulting estimates of birth and death rates during the pandemic to official counterparts, we combine the higher frequency information from the BED with the more detailed cross-sectional information from the BDS to extrapolate quarterly birth and death rates by sector and size class through the end of 2021. Specifically, for each sector, we use the annual BDS data from 2014 to 2019 to compute ratios of entry and exit rates by size class relative to the average sectoral entry and exit rate and multiply these ratios with the quarterly average sectoral birth and death rates from the BED. We label the resulting BED/BDS birth and death rates for each sector-size class i and quarter q as $p(\text{birth}_{i,q})$ and $p(\text{death}_{i,q})$.

In practice, we find that the quarterly BED/BDS birth and death rates vary little during 2019, which serves as our benchmarking reference period, and are similar except for the smallest size class (1-4 employees) for which birth and death rates are substantially larger (see the Appendix for details). For

the individual establishment level, especially for POIs in buildings with other occupants (e.g. malls, multi-story buildings) or POIs that conduct a lot of their business by delivery. Since the noise is not symmetric about zero, this makes reliable identification of business openings and closings extremely challenging. We therefore exploit the Safegraph visits data only to assess the representativeness of aggregates for which measurement issues with visits are likely to be similar.

[23]See the Appendix for details on the definitions of birth and death rates in the BED, respectively entry and exit rates in the BDS, and a comparison of these rates. We note that while in Retail Trade and Leisure & Hospitality, pre-pandemic BED birth and death rates line up closely with BDS entry and exit rates, in Education & Health and Other Services, BED birth and death rates are notably higher than BDS entry and exit rates. The reason for this difference is unclear and can only be investigated with access to the underlying micro-data. This illustrates that even in official data, measuring birth and death rates is far from trivial and depends crucially on the definition of (employer) establishments.

each sector, we therefore average the BED/BDS data across all quarters of 2019. Moreover, we pool some of the larger size classes for which the HB sample contains relatively few entries and exits. We then adjust opening and closing probabilities $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t})$ and $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$ by factors $\theta_i^{\mathcal{O}}$ and $\theta_i^{\mathcal{C}}$, respectively, such that the implied average birth and death rates for 2019 equal the BED/BDS counterparts.[24]

As also described in Section 2, for the 2020-21 estimation period, the adjustment factors for closings remain fixed (i.e., $\theta_{i,t}^{\mathcal{C}} = \theta_i^{\mathcal{C}}$) whereas the adjustment factor for openings vary inversely with the entry rate to take into account sample growth (i.e., $\theta_{i,t}^{\mathcal{O}} = \theta_i^{\mathcal{O}} \times \left(\frac{n_{i,t}^{\text{entry}}/n_{i,t}^{\mathcal{A}}}{n_{i,0}^{\text{entry}}/n_{i,0}^{\mathcal{A}}} \right)^{-1}$). Employment gains from openings and employment losses from closings during the pandemic are then estimated by equations (2) and (3), taking into account changes in $\hat{p}(\mathcal{O}_{\ell,t}|\text{entry}_{\ell,t})$ and $\hat{p}(\mathcal{C}_{\ell,t}|\text{exit}_{\ell,t})$ as estimated from the information on business activity in Google Places and Facebook. The implicit assumption in this estimation is therefore that selection issues and/or systematic measurement errors do not change during the pandemic – an assumption that we evaluate in what follows.

4 Performance of estimator

We assess the performance of the proposed methods in three separate ways. First, we compare birth and death rates during the pandemic as implied by our estimates of new openings and closings to the BED/BDS counterparts. Second, we show employment estimates and contrast them against official sources. Third, we use visits data from Safegraph to further assess the representativeness of our estimates. Finally, we illustrate the importance of controlling for sample churn.

4.1 Birth and death rates during the pandemic

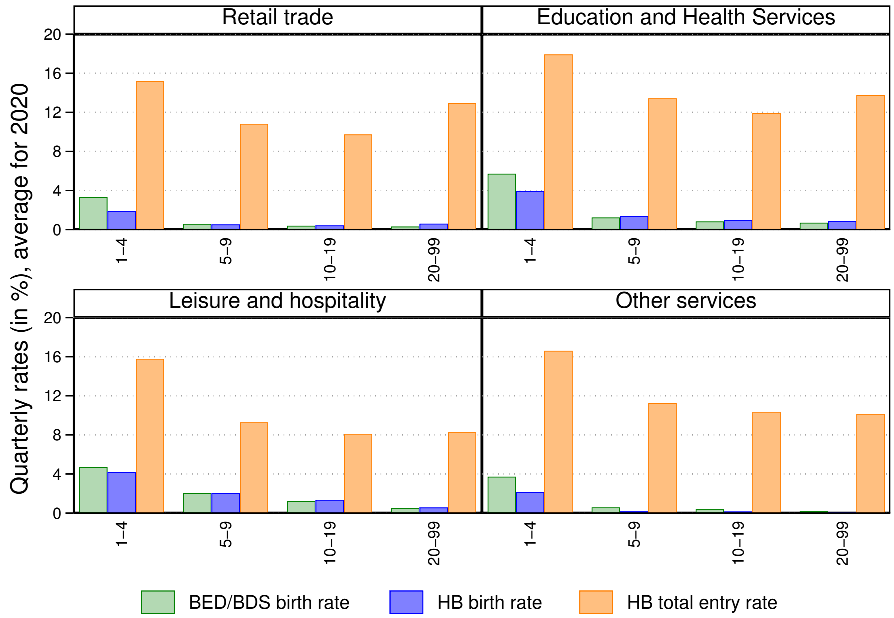
Figure 1 shows average quarterly birth and death rates for 2020 by industry and size class from the combined BED/BDS data, the corresponding average quarterly birth and death rates as implied by our estimates of new openings and closings, as well as rates of all new entries and permanent exits in our HB sample. As discussed above, the estimates of new openings and closings use adjustment factors based on benchmarking with 2019 BED/BDS data, but they do not use any information from administrative data for 2020. The fit with BED/BDS birth and death rates for 2020 therefore represents an important test.

Generally, the implied birth and death rates implied by our estimates closely match the BED/BDS

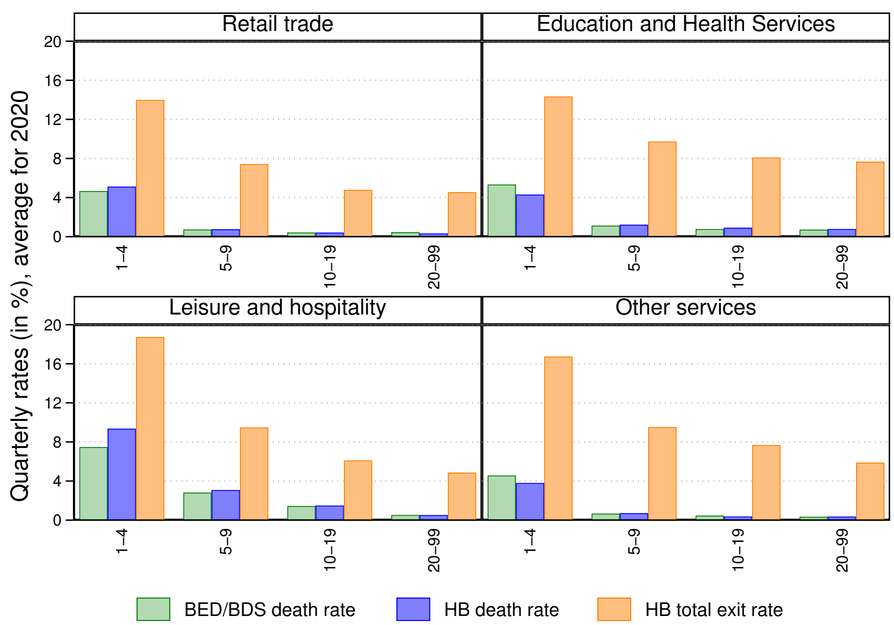
[24]The Appendix provides further details on the benchmarking and reports the different adjustment factors. Generally, adjustment factors for the smallest size class (1-4 employees) are larger than one, while adjustment factors for the larger size classes (5-9, 10-19, 20-49 employees) are smaller than one. This suggests that that the HB sample of entries and exits, respectively the subset of these establishments that we observe in Google / Facebook to estimate closing and opening probabilities, are likely subject to selection. The adjustment factors correct for this issue.

Figure 1: Comparison with 2020 BED/BDS birth and death rates

(a) Birth rates



(b) Death rates



Notes: This figure shows quarterly birth and death rates by sector and establishment size class from BED industry data combined with pre-pandemic 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.

counterparts. Given the large unexpected shock that the pandemic represents, this fit is remarkable. There are some differences for the smallest size category, but they are relatively small and it is important to remember that the BED/BDS rates are computed under the assumption that the relative birth and death rates by size class taken from the 2019 BDS data remain constant during the pandemic. This assumption is unlikely to hold exactly and so, the BED/BDS rates may themselves be subject to error.[25]

The figure also shows that total entry and exit rates in the HB sample are several times larger than new openings and closings. This confirms that the HB data is subject to important sample churn: many establishments already operated prior to entry into HB, and many establishments continue to operate after exiting HB. Correcting for sample churn is therefore key to obtain reliable employment estimates – a topic to which we return at the end of this section.

4.2 Employment before and during the pandemic

Next, Figure 2 plots weekly small business employment as implied by our estimates and compare it to the annual counterpart for small business employment from the QCEW.[26] For comparison, the figure also includes the monthly CES estimates of total sectoral employment; i.e., employment by businesses *of all size classes* as opposed to just small businesses.[27] Here and below, we refrain from seasonally adjusting employment estimates since usual adjustment procedures would not be appropriate for the type of large changes that employment experienced during the pandemic. See Rinz (2020) for a discussion.

Panel (a) shows the results for pre-pandemic period (February 2019 to February 2020). Our HB estimates provide an excellent fit with the year-on-year employment change in small business employment from the QCEW. The weekly HB estimates also comove quite closely with the monthly CES estimates, although there are some differences. These differences should not come as a surprise nor do they invalidate our methods since the HB estimates pertain to establishments with fewer than 50 employees whereas the CES estimates pertain to employment by establishments of all size classes. Instead, the relevant comparison is with the small business benchmark from the QCEW and in this respect, the fit is very close.

Panel (b), in turn, shows the results for the pandemic period (February 2020 to November 2021). Our

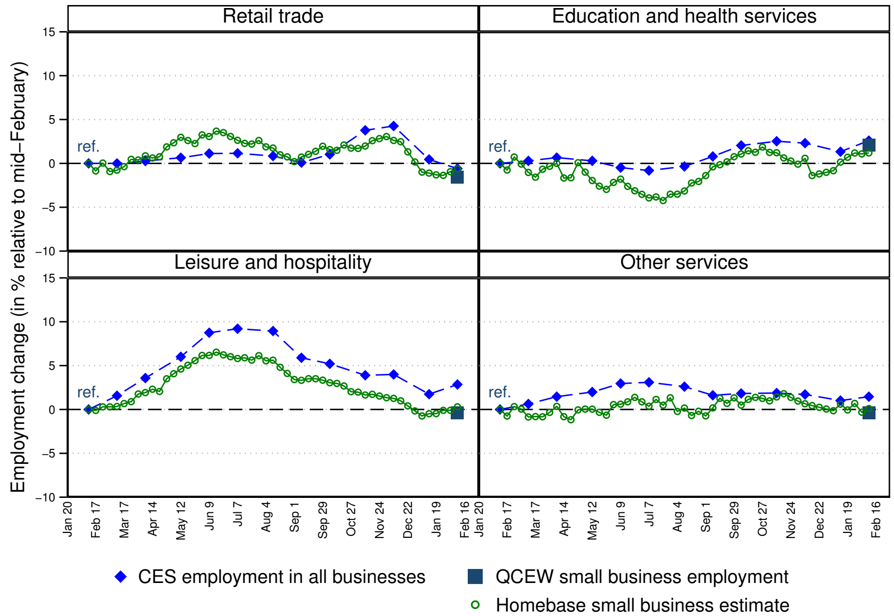
[25] Also note that for the first quarter of 2020, the BED shows a large increase in births in Education & Health that is likely spurious, due to a technical issue with the annual revision (see Decker and Haltiwanger, 2022). For this sector, we therefore do not include the first quarter birth rate for the average BED/BDS birth rate calculation.

[26] All plots are shown relative to mid-February reference employment. This choice is guided by the fact that the QCEW publishes employment and establishment counts by industry and establishment size class only for the first quarter of each year, with the numbers pertaining to the month of February. We use these numbers to construct the jump-off point E_0 and the cell weights ω_i for our employment estimator in (1).

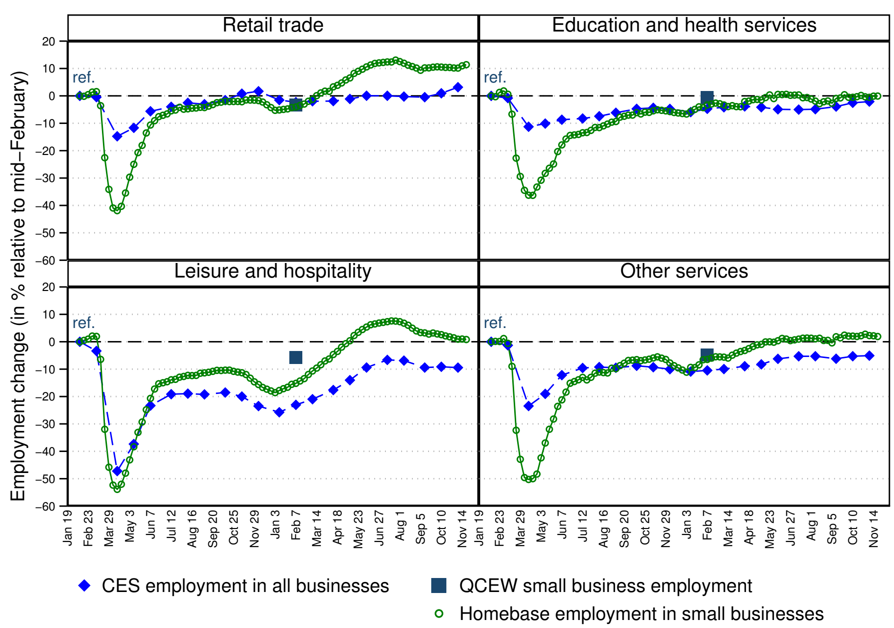
[27] The employment estimates from the CES are publicly available only by industry but not by size class.

Figure 2: Small business employment versus all business employment estimates

(a) Pre-pandemic period (February 2019 to February 2020)



(b) Pandemic period (February 2020 to November 2021)



Notes: The figures show estimates of the change in employment in small businesses with less than 50 employees and the change in employment in all businesses in percent of employment in mid-February of 2019, respectively mid-February 2020, for Retail Trade (NAICS 44-45), Education & Health Services (NAICS 61-62), Leisure & Hospitality (NAICS 71-72), and Other Services (NAICS 81). The small business estimates are constructed from Homebase data as described in the text. The all business estimates are from the CES. The blue squares show the year-over-year change in small business employment implied by the QCEW. None of the estimates are seasonally adjusted. The small business estimates for the weeks of Thanksgiving, Christmas, and New Year are smoothed by using the estimates of adjacent weeks.

estimates of small business employment again align closely with their annual QCEW counterparts, except for Leisure & Hospitality where the QCEW year-on-year change is about 10% above our estimate. Closer inspection of the QCEW suggests that this discrepancy arises because the QCEW in each year reports a new cross-section of employment by establishment size class. Since many establishments in Leisure & Hospitality were still substantially below their pre-pandemic employment level by mid-February 2021, this means that the count of establishments in the QCEW classified as having fewer than 50 employees increased substantially from mid-February 2020 to mid-February 2021, thereby inflating the year-on-year growth rate of small business employment in that sector. Our HB estimates, in contrast, follows establishments of a given size class over time and are therefore not affected by this compositional change.

Compared to the CES all business employment estimates, our estimates show a substantially larger decline in small business employment in the beginning of the pandemic as well as a stronger rebound thereafter. These differences suggest that small businesses were affected more severely by the initial pandemic shock but then also recovered more quickly – an observation to which we return in Section 5.

4.3 Using visits data to assess the representativeness of our estimates

The comparison with the QCEW indicates that our HB estimates are broadly representative for the four sectors considered. Nonetheless, it could be that the large swings in estimated employment in the beginning of the pandemic arise because the HB data skews towards a sample of small businesses that were disproportionally affected by the pandemic. Given that there is no publicly available administrative data at sufficiently high frequency to assess this possibility, we pursue a novel approach that exploits visits data from Safegraph to show that this is unlikely to be the case.

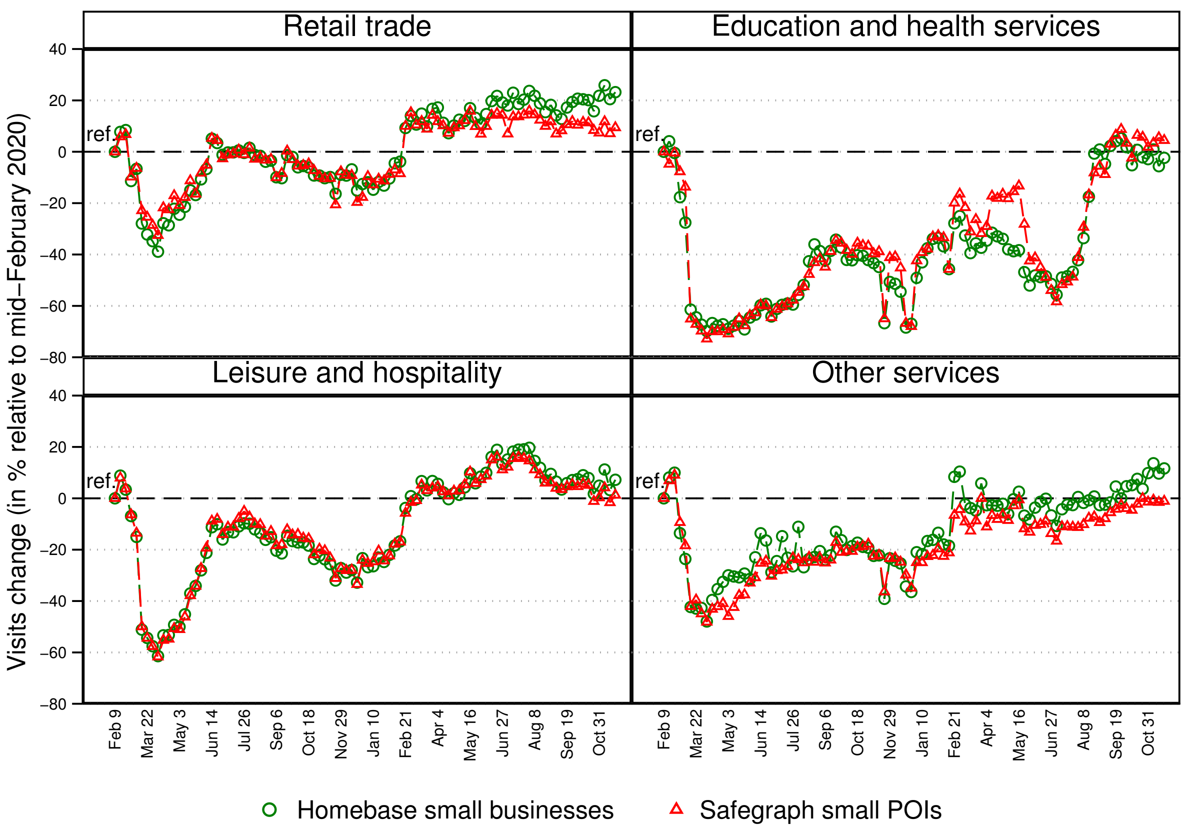
Specifically, for many of the POIs, Safegraph reports information on the number of weekly visits and dwell time per visit. In addition, the Safegraph data can be combined with information from NetWise, a company collecting and analyzing vast amounts of data on U.S. businesses, to obtain an annual estimate of employees for each of the POIs. We use this information first to cross-check the average number of employees for each matched HB establishment to the corresponding NetWise estimate and find a close correspondence. Second and more importantly, we compare weekly visits of the matched HB establishments to all Safegraph POIs in the four sectors with fewer than 50 employees.[28]

Figure 3 shows the results. The fit is remarkable. Throughout the entire pandemic, including the

[28]We do not retain POIs that are associated with a brand (McDonald's, Starbucks, etc.), although the results below are almost identical when we include these POIs. The resulting Safegraph sample of POIs in the four sectors with fewer than 50 employees and visits data contains almost 1,000,000 unique observations, or about about 22% of the corresponding universe of establishments in the QCEW.

initial months, weekly visits to matched HB establishments evolve, with the exception of some temporary deviations, in lockstep with weekly visits to all corresponding Safegraph POIs. As shown in the Appendix, a similarly close fit obtains for median dwell time, share of visits lasting longer than four hours, and weekly visits per visitor. The results indicate that the establishments in our HB sample have on average very similar visits characteristics than the much larger Safegraph sample.

Figure 3: Homebase weekly visits compared to Safegraph POIs with less than 50 employees



Notes: Weekly visits change by small businesses with less than 50 employees in Homebase (green circles) vs. Safegraph POIs with less than 50 employees (red triangles) in percent of respective employment level during the week of Feb 9 - Feb 15, 2020 for Retail Trade (NAICS 44-45), Education & Health Services (NAICS 61-62), Leisure & Hospitality (NAICS 71-72), and Other Services (NAICS 81). None of the estimates are seasonally adjusted. Information on the number of employees at Safegraph POIs come from NetWise employment data.

In the Appendix, we also use the Safegraph visits data to further assess potential sample selection issues. First, we compare visits of establishments that continue in our HB sample with visits of establishment that enter or exit the sample but are identified as churners (i.e., not openings or closings). Second, we compare visits of entering and existing establishments that we can match to either Google Places or Facebook with visits of entering and exiting establishments that we cannot match. For both comparisons, we find a close overlap of visits during the pandemic.

Together with the close fit relative to QCEW year-on-year employment changes, the comparison with Safegraph visits provides compelling evidence that our HB sample and the identification of openings and closings is representative of small businesses in the four service sectors considered. More generally, we view our strategy of using Safegraph data to assess the quality of our data and methods as a blueprint that can be applied to other private-sector establishment-level datasets as well.[29]

4.4 The importance of controlling for sample churn

To illustrate the importance of distinguishing closings and openings from sample churn, we finish by considering different counterfactual employment estimates.[30] Figure 4 reports the results. The brown short-dashed lines show employment estimates if we abstracted completely from entry and exit and used only the set of establishments that are continuously active in the HB data from the beginning through the end of the sample (i.e., $\widehat{E}_t = \widehat{E}_{t-1} \times \frac{\sum_i \omega_i \hat{e}_{i,t}^{\mathcal{A}_{i,t}}}{\sum_i \omega_i \hat{e}_{i,t-1}^{\mathcal{A}_{i,t}}}$, where $\mathcal{A}_{i,t}$ denotes continuously active establishments). Relative to our baseline estimates (green circled lines), these estimates would miss much of the large decline and subsequent rebound of small business employment in the initial phase of the pandemic.

The red dashed-dotted lines show what happens if we treat all exits as either temporary or permanent closings (i.e., $\widehat{E}_t = \widehat{E}_{t-1} \times \frac{\sum_i \omega_i \left(\hat{e}_{i,t}^{\mathcal{A}_{i,t}} + \hat{e}_{i,t}^{\mathcal{R}_{i,t}}\right)}{\sum_i \omega_i \left(\hat{e}_{i,t-1}^{\mathcal{A}_{i,t}} + \hat{e}_{i,t-1}^{\text{exit}_{i,t}}\right)}$, where $\text{exit}_{i,t}$ 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 continue operating, this estimate declines even more than our baseline estimate in the beginning of the pandemic and recovers much less thereafter. In fact, from 2021 onward, this estimate declines gradually as sample churn cumulates.

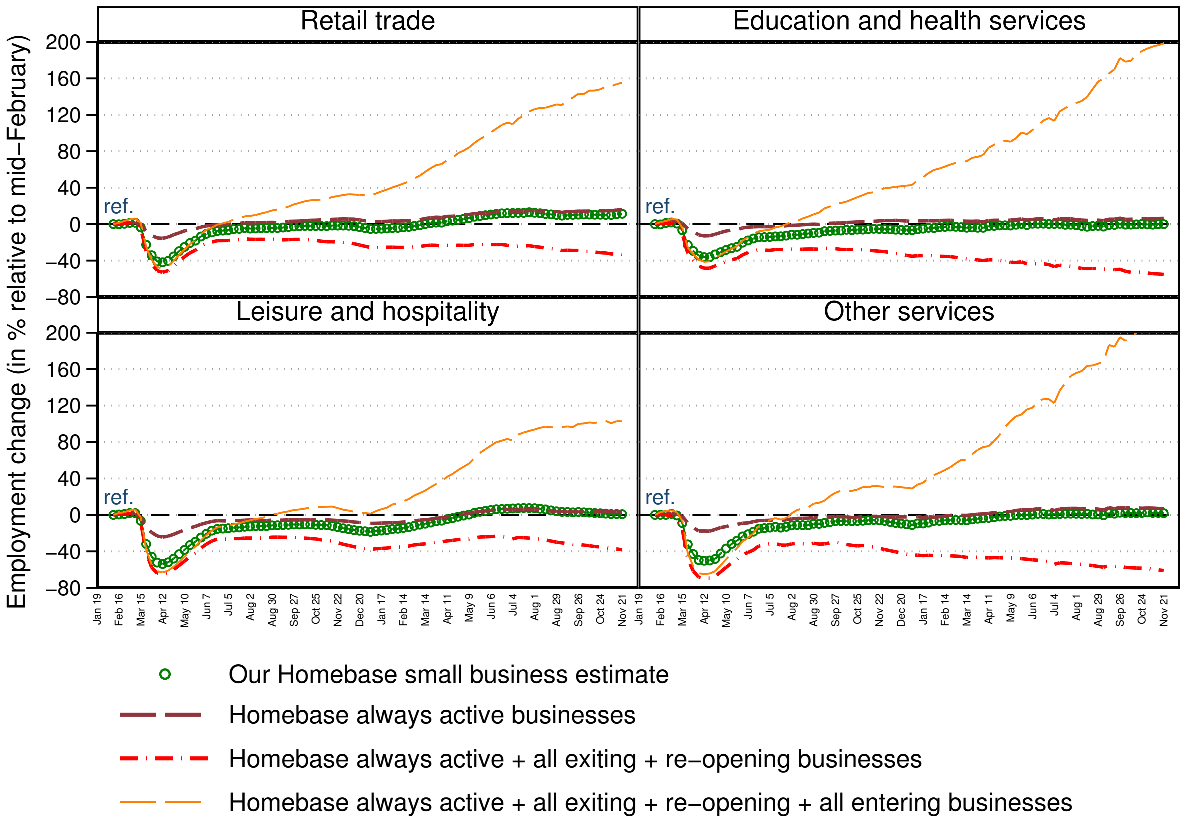
The orange dashed lines, finally, add all entries and treat them as new openings (i.e., $\widehat{E}_t = \widehat{E}_{t-1} \times \frac{\sum_i \omega_i \left(\hat{e}_{i,t}^{\mathcal{A}_{i,T}} + \hat{e}_{i,t}^{\text{entry}_{i,t}}\right)}{\sum_i \omega_i \left(\hat{e}_{i,t-1}^{\mathcal{A}_{i,T}} + \hat{e}_{i,t-1}^{\text{exit}_{i,t}}\right)}$, where $\text{entry}_{i,t}$ denotes the set of all entering establishments in week t, including the returning establishments). The resulting estimate shows a dramatic increase in small business employment through the end of the sample, far outweighing the negative effect of treating all exits as closings. This reflects the fact that HB substantially expanded its client base over time, including during the pandemic.

The counterfactuals offer an interesting comparison point to various studies using private establishment-level data sources adopted to produce real-time employment estimates. For instance, Bartik et al. (2020) who also use the Homebase data, estimate employment and total hours worked in the beginning of the pandemic based on a sample of firms that are active in January 2020, effectively treating all sample exits

[29]The Safegraph data as well as other useful datasets on business activity are available at https://www.deweydata.io/.

[30]Similar counterfactual estimates are reported in the Appendix for the pre-pandemic period.

Figure 4: Counterfactual employment estimates



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 & Health Services (NAICS 61-62), Leisure & 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.

as closings (i.e., similar to the red dash-dotted lines). In turn, Dalton et al. (2020) include all establishments reporting zero employment, impute employment for non-respondents, and exclude entries in their estimates of small business employment based on confidential CES microdata (i.e., some combination of brown short-dashed and red dash-dotted lines). And Cajner et al. (2020) include all exits and entries in their analysis based on ADP data, the largest payroll processing company in the U.S. (i.e., similar to the orange dashed lines).[31] Our counterfactuals indicate that some of these approximations were reasonable for the first few months of the pandemic, i.e., the focus of these papers, when most exits were temporary closings and new openings were relatively unimportant. The approximations would have become increasingly inaccurate later in the pandemic, however, as the contribution from new entry became more

[31] Chetty et al. (2023) differ from these studies in that they use an adjustment procedure in their employment estimator that downweighs estimation cells with employment growth rates that are above or below certain thresholds deemed anomalous (see their Appendix E.2). It is unclear how this adjustment procedure compares to the counterfactuals considered here.

important. As such, our results offer a cautionary tale about the use of private data sources to estimate employment without careful incorporation of gains from establishment openings, respectively losses from closings.

In sum, the results in this section show that our estimator of small business dynamics and employment performs well during the pandemic relative to administrative data that became available with a substantial time lag. Moreover, the comparison with visits data from Safegraph to assess the representativeness of our HB data provides a blueprint that can be applied to other private-sector establishment-level data.

Of course, one might be tempted to argue that since administrative data is now available, estimates of employment during the pandemic based on private data sources is no longer as interesting. It is important to remember, however, that estimates from the above discussed studies garnered considerable attention during the first few months of the pandemic and likely influenced policy makers (see references in the introduction). The point here is not to produce new real-time estimates about the pandemic, but to retrospectively assess the performance of our estimator and compare it to other studies with private data sources. The close fit of our estimator – even during a time of extraordinary economic turmoil – makes us confident that the proposed method is useful to produce estimates of small business dynamics for other situations when more timely estimates of the state of the economy may again be highly valuable. In addition, the retrospective estimates of the pandemic produced here are available at higher frequency and at greater detail in terms of geography and firm-level variables than what is available from publicly available official data. This allows us to study important questions about the pandemic – an aspect to which we turn in the following two applications.

5 Small business dynamics and employment during the pandemic

As a first application, we use our estimates to retrospectively assess whether small service-sector businesses were hit harder by the pandemic than larger businesses, and the role that business openings and closings play for these dynamics. In the process, we highlight incongruences with the CES employment estimates that point to issues with the net birth/death model that the BLS uses to construct these employment estimates. In addition, we exploit the detailed micro data available from HB to report results on gross job flows and hours worked of job stayers, thus providing important additional context.

5.1 Were small businesses hit harder by the pandemic?

The weekly estimates from our HB data in panel (b) of Figure 2 highlight the dramatic decline – both in terms of speed and extent – of small business employment in the beginning of the pandemic as well as the ensuing rebound. Table 2 puts these changes into relief.

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

	Retail Trade	Education & Health	Leisure & Hospitality	Other Services	Total
Employment in mid-February 2020	7,205	8,539	9,714	4,394	29,852
Mid-Feb to mid-April 2020	-3,019	-3,097	-5,235	-2,209	-13,558
in % relative to mid-Feb 2020	*-42%*	*-36%*	*-54%*	*-50%*	*-45%*
Mid-April to end-June 2020	2,365	1,752	3,560	1,401	9,078
in % relative to mid-Feb 2020	*+33%*	*+21%*	*+37%*	*+32%*	*+30%*
Mid-June 2020 to mid-Feb 2021	362	1,059	201	524	2,146
in % relative to mid-Feb 2020	*+5%*	*+12%*	*+2%*	*+12%*	*+7%*

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).

From mid-February 2020 to mid-April 2020, small business employment across the four service sectors considered declined by 14 million, a staggering 45% of the 30 million jobs prior to the pandemic. Between mid-April and mid-June 2020, small business employment then regained about 9 million, or more than two thirds of the initial job loss. Between mid-June 2020 and mid-February 2021, small business employment recovered further, although at a considerably lower pace.

Returning to Figure 2, the comparison between our HB small business estimates and the CES *all business* estimates indicate that in the first two months of the pandemic, the contraction of small business employment in the four service sectors was more dramatic than contraction of employment of larger businesses. But subsequently, small business employment also returned more strongly. The exception to this result is the Leisure & Hospitality sector where the contraction and subsequent rebound in small business employment is almost the same as for employment of all and therefore larger businesses.[32] From June 2020 onward then, employment of small businesses generally grew at a faster pace than employment of larger businesses, especially in Retail Trade and Leisure & Hospitality where by November 2021, small

[32]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.

business employment was about 10% higher than the CES all business counterpart.

5.2 The crucial role of small business closings and openings

To investigate the sources behind the large swings in small business employment further, we consider the role played by business closings and openings. Figure 5 starts by reporting weekly rates of small business closings and reopenings as well as cumulative rates of closings and new openings since the beginning of the pandemic and comparing them to the same time period one year earlier.[33]

As shown in panels (a) and (b), the weekly rate of small business closings across the four sectors considered spiked to 16% in the week of March 22-28, 2020 (week 6 after the mid-February reference week) but then dropped sharply to about 2% by mid-April (week 10) before further declining to the pre-pandemic average of about 1% per week.[34] Concurrent with the decline in the rate of business closings in April of 2020, reopenings started 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% range thereafter, just slightly above the pre-pandemic rate.

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 averaged about 6% in both mid-February 2019 and mid-February 2020, 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 rose steeply, peaking at 39% in mid-April 2020. Thereafter, total closings declined, steeply initially as many businesses reopened and then more gradually to a low of about 14% by November before rising to about 16% by mid-February 2021.[35] This suggests that only about one third of all closings in mid-March were permanent.[36] Moreover, the cumulative rate of closings one year after the start of the pandemic is only about 2 percentage points higher than the cumulative closing rate from mid-February 2019 to mid-February 2020. This implies, perhaps surprisingly but consistent

[33] Specifically, we define

$$rate(\mathcal{I}_t) = \frac{\sum_i \omega_i \hat{n}_{i,t}^{\mathcal{I}_{i,t}}}{\sum_i \omega_i \left(\hat{n}_{i,0}^{\mathcal{A}_{i,1}} + \hat{n}_{i,0}^{\mathcal{C}_{i,1}} \right)}, \tag{4}$$

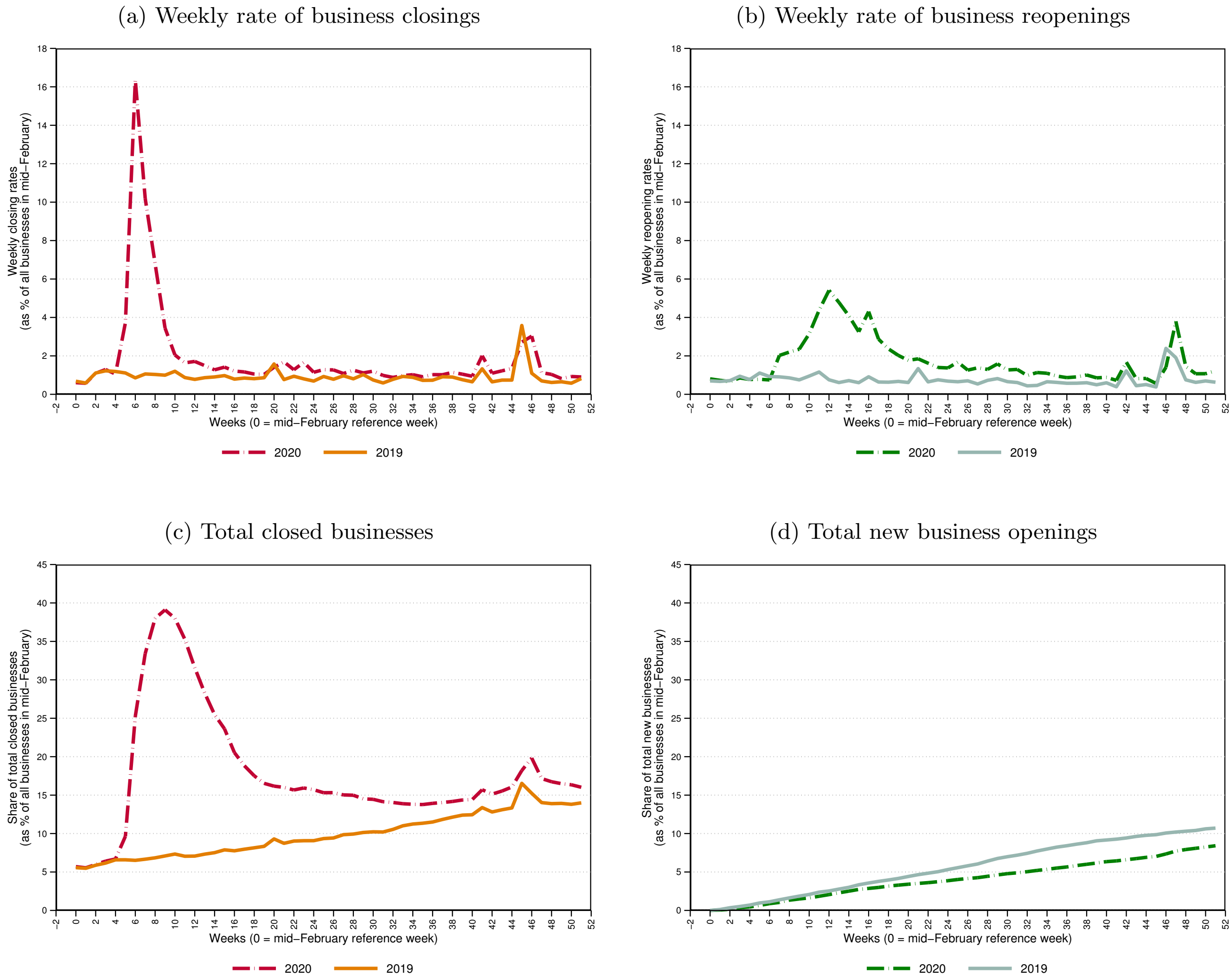
where $\hat{n}_{i,t}^{\mathcal{I}_{i,t}}$ denotes the count of establishments in industry-size-region cell i that closed either temporarily or permanently in week t ($\mathcal{I}_{i,t} = \mathcal{C}_{i,t}$), reopened in week t ($\mathcal{I}_{i,t} = \mathcal{R}_{i,t}$), or newly opened in week t ($\mathcal{I}_{i,t} = \mathcal{B}_{i,t}$), with $\mathcal{O}_{i,t} = \mathcal{R}_{i,t} \cup \mathcal{B}_{i,t}$ by definition; and $\hat{n}_{i,0}^{\mathcal{A}_{i,1}} + \hat{n}_{i,0}^{\mathcal{C}_{i,1}}$ denotes the count of active establishments in the reference week. We choose the count of active establishments in reference week in the denominator as opposed to the count of active establishments around week t because the count of active establishments varies dramatically during the first weeks of the pandemic.

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

[35] These cumulative closing rates are less than half of what Crane et al. (2022) report based on HB data. The reason for this difference is that their study treats all exits as closings.

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

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 & Health Services (NAICS 61-62), Leisure & 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).

with concurrent analysis by Crane et al. (2022) based on alternative measures of business closures as well as Decker and Haltiwanger (2022) and Fairlie et al. (2023) based on administrative data released a year later, that the pandemic did not markedly increase permanent small business closings.

Panel (d), finally, shows total new business openings relative to total active businesses in mid-February. This rate increased gradually throughout the year, even during the worst of the pandemic in March and April 2020. Compared to 2019, the pace of new openings was clearly lower during the Spring and Summer of 2020 but then picked up somewhat in Fall and Winter, finishing at about 8% by mid-February 2021. This is only about 2 percentage points lower than the rate of new business openings a year earlier, implying that the pandemic only exerted a modest negative effect on new business openings.

At first sight, the lower rate of new business openings contrasts with evidence from the U.S. Census Bureau that shows record rates of new applications for likely employer business relative to pre-pandemic levels, in particular in Non-store Retail (NAICS 454) and Personal & Laundry Services (NAICS 812), but also in Food Services & Drinking Places (NAICS 722) (see Fazio et al., 2021 or Haltiwanger, 2022). The difference is likely due to the fact that it takes several quarters from business application to employment of workers, and that new applications jumped disproportionally for businesses without a physical store location (e.g. non-store retailers) which are excluded from our analysis.[37] Since these businesses do not appear in Safegraph, our analysis excludes non-store retail and other services that do not have a physical store location. Our estimates may therefore represent a lower bound of the recovery in small business employment.

To quantify the role of closings and openings for small business employment, we decompose the employment change for each sector into the contributions from businesses that continued 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.[38]

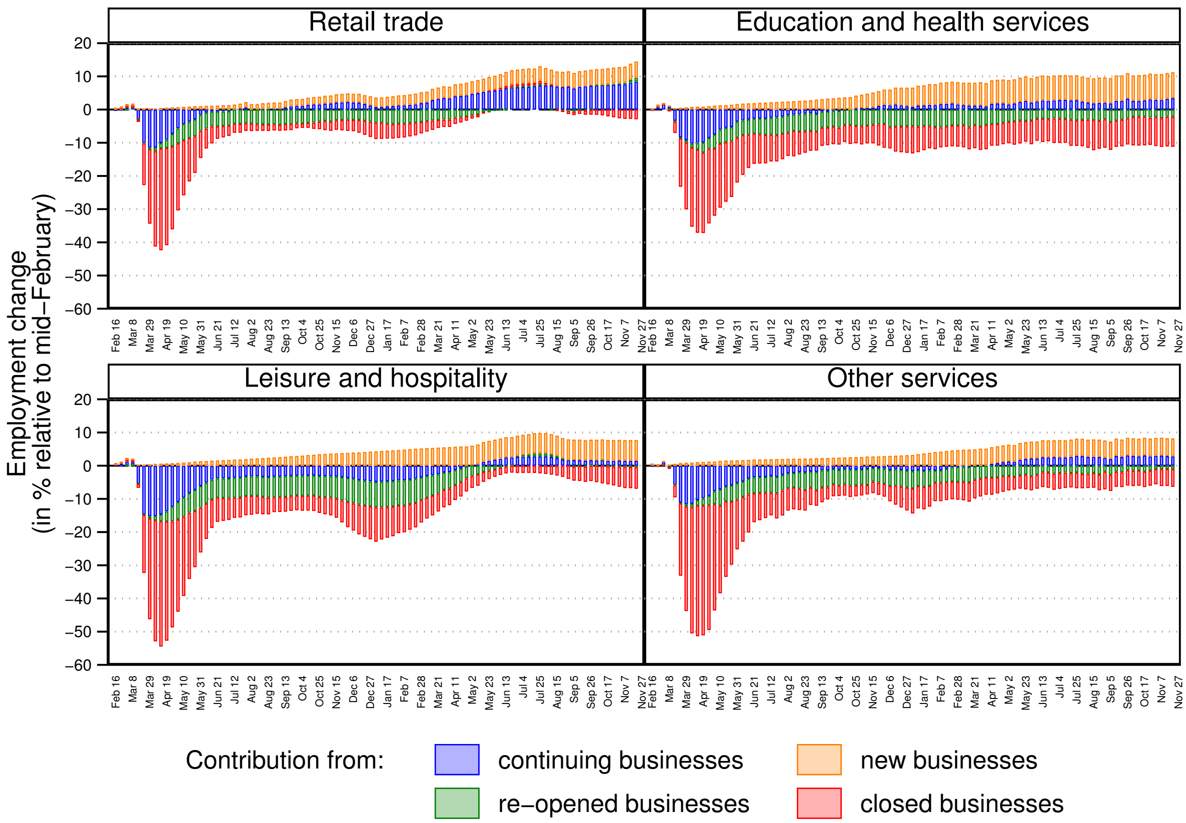
As Figure 6 shows, business closings accounted for 70% or more of the initial employment decline from mid-March to mid-April across the four sectors (red bars), with job losses by continuing businesses accounting for rest (blue bars). Reopenings of closed businesses drove most of the rebound in employment between mid-April and mid-June (smaller red bars), even though the reopened businesses operated at lower employment than in mid-February (green bars). Finally, the recovery from mid-June 2020 onward is due in large part to new businesses (yellow bars), adding almost 1.5 million jobs across the four sectors by the end of the sample.[39] In other words, much of the impact of the pandemic on small business employment occurred through the extensive margin (i.e., openings and closings) as opposed to

[37]Indeed, as shown below, while we see substantial employment gains from new business openings in the Leisure & Hospitality sector (of which Food Services and Drinking Places is a large part) as well as Education & Health, new business openings play a more modest role for Retail Trade and Other Services.

[38]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.

[39]The decompositions also reveal interesting differences across sectors. First, large job losses from closings persisted through the end of the sample in Education & Health but reduced to almost zero in Retail Trade. Second, Retail Trade experienced substantial job gains by continuing businesses. Third, job 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 6: 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 & Health Services (NAICS 61-62), Leisure & 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.

the intensive margin (i.e., employment changes in continuing establishments).

5.3 Incongruence with CES estimates

The above comparison with the CES implies that small business employment contracted more during the first two months of the pandemic but subsequently also rebounded faster than employment of larger businesses. At the same time, we note that the estimated loss of 14 million small business jobs between mid-March and mid-April 2020 and the recovery of about 10 million jobs between mid-April and mid-June 2020 are both *larger* than the corresponding CES employment estimates across *all businesses* in the four sectors considered (13.5 million and about 6 million, respectively).[40] Unless employment in businesses with 50 employees or more increased from mid-March to mid-April and then declined from mid-April to mid-June – an implausible scenario – this means that either our HB estimates or the CES estimates do not adequately capture small business employment changes during the first months of the pandemic.

As we have already documented, our HB estimates provide a good fit of year-on-year sectoral growth rates for small business employment from the QCEW, the birth and death rates implied by our estimates closely align with the administrative counterparts from the BED/BDS, and weekly visits patterns of small businesses in our HB sample match closely the visits patterns of all small businesses in Safegraph. Moreover, our estimates for the first few months of the pandemic are broadly consistent with estimates in Cajner et al. (2020) based on ADP data.[41]

This leaves two possible explanations for the incongruence with the CES estimates. First, differences in how employment is measured; and second, differences in how business closings and openings are identified. 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 with actual hours worked in a given week. So, if some workers who were temporarily furloughed in mid-April still received pay, then they were counted in the CES but not in HB.[42] Second and as described in Section 2, during the pandemic, the CES birth/death model included only a fraction of establishments reporting zero employment and inferred the employment effects of non-responding establishments through imputation and an econometric prediction based on past and current aggregates. Our HB estimator, in contrast, directly includes the employment effects of all business closings and openings. Both of these differences imply that the CES may have underestimated the drop in employment in mid-April 2020 and the subsequent rebound.

Absent microdata on the actual employment of the establishments surveyed in the CES, it is not possible to fully determine the quantitative importance of each of these differences. However, we can

[40]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.

[41]Cajner et al. (2020) estimate that employment of *all* businesses in the four sectors that we consider declined by 20.2 million between mid-February and late April 2020. They also report that employment in establishments with less than 50 employees across *all* private sectors of the U.S. economy declined by about twice as much between March and April 2020 as employment for larger establishments, but by the end of June 2020 had recovered as much as larger establishments. Considering that businesses with fewer than 50 employees accounted for almost half of employment in the four sectors prior to the pandemic and that ADP employment is pay-based, these numbers appear quite close to our estimated decline in small business employment of 14 million and subsequent rebound of about 10 million during the same time period.

[42]One could be concerned that our HB employment estimates leave out some non-hourly workers, and that these workers were less likely to be furloughed in the beginning of the pandemic. However, when we check in the Current Population Survey (CPS) whether employment of salaried workers declined by more than employment of hourly-paid workers, we find only small differences. So, even if our HB estimates do not capture all non-hourly workers, it seems unlikely that this would explain the large difference with the CES employment estimates.

bring to bear other data to provide at least a partial assessment.

First, we look at estimates from the CPS of the number of workers reported as being "employed but absent from work" during the reference week (which, as in the CES, is the week that includes the 12th of the month). We find a spike in this measure in April 2020 in all of the four service sectors considered, totaling 4.3 million or about 10% of all workers employed, that drops by half to about 5% of all workers employed by June 2020.[43] Even if many of these absentee workers are counted as employed in the CES, the above numbers are too small to account for a large portion of the difference to our HB estimates. This conclusion is further supported by the fact that the aforementioned employment estimates from Cajner et al. (2020) are broadly consistent with our HB estimates, even though ADP's employment concept is pay-based as in the CES.

This leaves the second explanation. Since small business employment accounts for almost half of all employment in the four sectors considered, and almost 40% of these businesses were closed in mid-April 2020 with about two thirds returning to activity by mid-June 2020, it is conceivable that the birth/death adjustment based on imputation and an econometric prediction did not accurately take into account temporary closings in the beginning of the pandemic, thus leading the CES to underestimate the initial large decline in employment and subsequent rebound. This conjecture is further supported by the fact that CES response rates fell precipitously during the first months of the pandemic, which implies that a larger fraction of the CES employment estimate relied on imputation / econometric prediction.[44]

Despite these incongruences with the CES, the evidence is quite clear that small business employment in the four sectors considered contracted by substantially more during the first months of the pandemic than employment of larger businesses. The comparison provides a new data point for the ongoing debate on whether small businesses are more sensitive to economic shocks than larger businesses. While Moscarini and Postel-Vinay (2012) and Haltiwanger et al. (2018) find that this is generally not the case, Fort et al. (2013) report that small/young business contracted more during the 2008-09 Great Recession. Our estimates are supportive of the latter conclusion for the beginning of the pandemic, perhaps because small businesses were initially more credit-constrained – a point that we investigate further in the next Section. At the same time, the quicker recovery after the initial shock suggests that small businesses may

[43] Workers reporting as being "employed but absent from work" also spikes in the other sectors of the economy, but the spike is substantially smaller relative to total employment. See https://www.bls.gov/cps/employment-situation-covid19-faq-may-2020.pdf for details. As a check, we also use the CPS to compute the number of workers categorized as "employed at work". We find that employment across establishments of all sizes in the four sectors considered contracted by about 20.1 million from mid-February to mid-April 2020, very close to the aforementioned estimates by Cajner et al. (2020), and then recovered about 7.3 million by mid-June 2020. These numbers lends further support to the idea that the CES severely underestimated the contraction and subsequent rebound in service-sector employment in the beginning of the pandemic.

[44] See for example Mitchell et al. (2021) for an account of the response rate issues with the CES.

have been better able to adapt to the new economic environment imposed by the pandemic.

5.4 Gross job flows and hours worked

To finish this application, we show how the detail available from the HB microdata can be used to provide insights about other important questions beyond small business employment. First, the linked worker-establishment structure of the HB data allows us to decompose weekly employment changes into job separations, new hires, and recalls; i.e., workers who are employed in an establishment at some point in the past, disappear for at least one time period (one week in our case), and then reappear as employed in the same establishment.

Figure 7 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 spiked the week of March 22-28 (week 6), the same week as business closures spiked, while the new hire rate declined. Both rates then returned to their pre-pandemic average by mid-June and remained essentially the same as one year earlier.

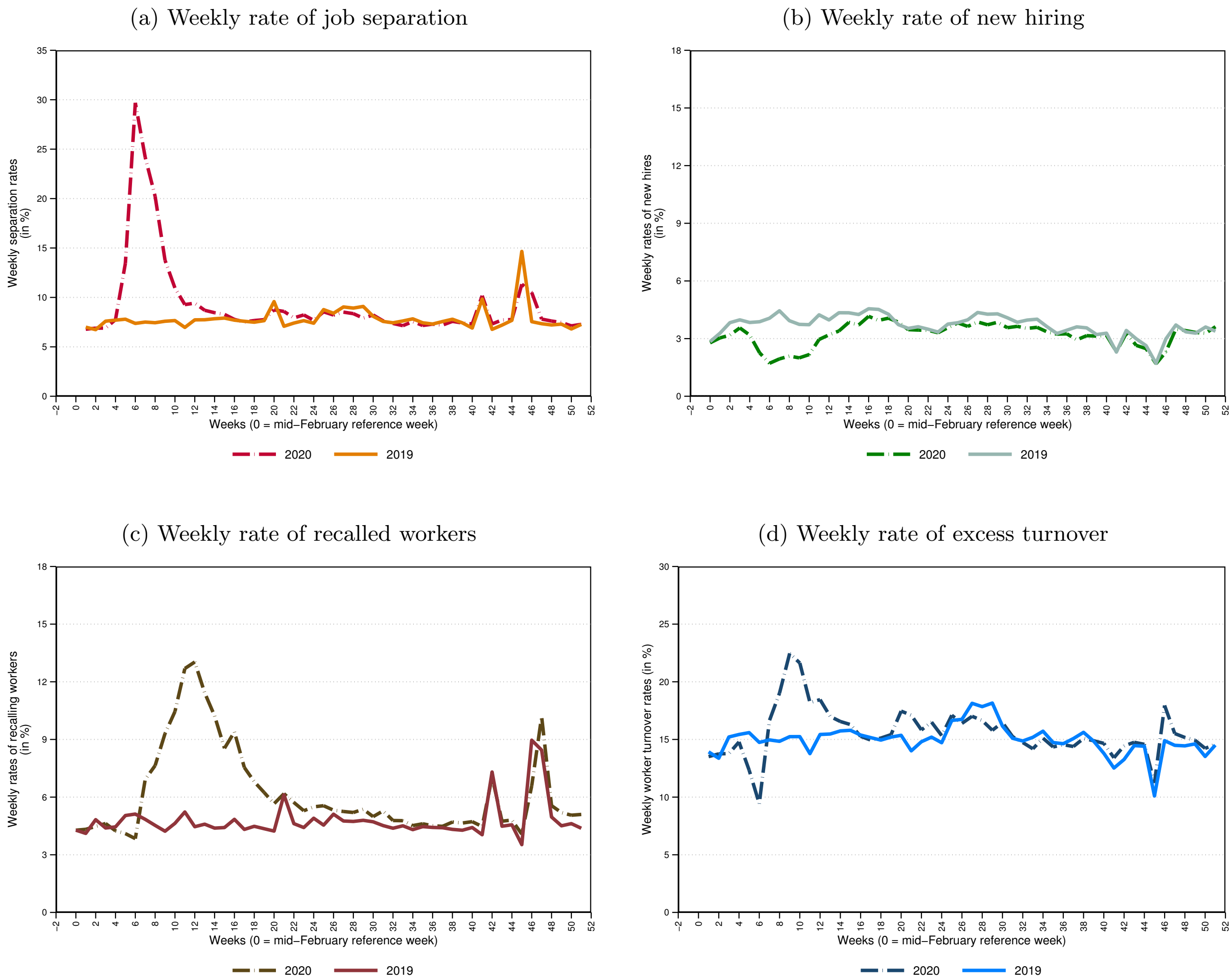
As shown in panel (c), the recall rate of workers previously employed in the same establishment increased substantially in the weeks following the initial spike in separations, peaking the week of May 3-9 (week 12). The recall rate then declines steadily through the week of June 28 - July 4 (week 20) and thereafter remained slightly elevated through the end of summer before essentially returning to the corresponding 2019 value.[45]

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 dropped briefly in the beginning of the pandemic as new hiring and recalls decline and then jumps up as recalls jump up while some businesses still show excess job separations. After mid-June, excess turnover averaged 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 was 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

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

Figure 7: Job separations, new hires, recalls, and excess turnover in small businesses



(a) Weekly rate of job separation

(b) Weekly rate of new hiring

(c) Weekly rate of recalled workers

(d) Weekly rate of excess turnover

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 & Health Services (NAICS 61-62), Leisure & 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.

the recovery was more sluggish due to persistently lower new hiring rates. Furthermore, the quick return in the excess turnover rate to its 2019 average suggests that, at least within the four in-person service sectors considered, the pandemic did not lead to major reallocations of labor.

Second, we can use HB's information on actual hours worked to study the impact of the pandemic on the intensive margin of work; i.e., average weekly hours (AWH). To do so, we start with the CES estimate from February 2020, $\widehat{AWH}_0$, and then use our HB data to estimate

$$\widehat{AWH}_t = \widehat{AWH}_{t-1} \times \frac{\left(\sum_i \omega_i \widehat{wh}_{i,t}\right) / \left(\sum_i \omega_i \hat{e}_{i,t}\right)}{\left(\sum_i \omega_i \widehat{wh}_{i,t-1}\right) / \left(\sum_i \omega_i \hat{e}_{i,t-1}\right)}, \tag{5}$$

where $\widehat{wh}_{i,t}$ denotes estimated total weekly hours worked and $\hat{e}_{it}$ denotes estimated employment at establishments in industry-size-region cell i in week t. Importantly, in computing this estimate, we only take into account job stayers who remained employed continuously in establishments that are active throughout the sample. By doing so, our estimates of AWH consist by definition of a balanced panel of workers and is therefore not affected by compositional bias. This estimation of AWH is different from the "link-and-taper technique" used to construct AWH in the CES, which not only includes hours of all workers employed in a given month, thus making it subject to composition bias, but also adjusts the current estimate towards the previous estimate, thus smoothening the estimate towards the overall sample average over time.[46] Both of these differences mean that the CES estimate may not adequately capture large changes in actual AWH that occur in times of economic disruptions.

Figure 8 shows the results. According to the CES, AWH barely moved throughout the pandemic, except for Leisure & Hospitality where AWH declined by one to two hours in the first two months of the pandemic. According to our HB estimate, in contrast, AWH declined sharply for all sectors in March and April of 2020, before recovering by mid-May 2020. The large difference implies that the CES not only underestimated the initial contraction and recovery of employment but also of hours worked. Further investigation with our HB data shows that while compositional bias plays a role for this difference, the more important contributor is the "link-and-taper technique" that the CES uses.

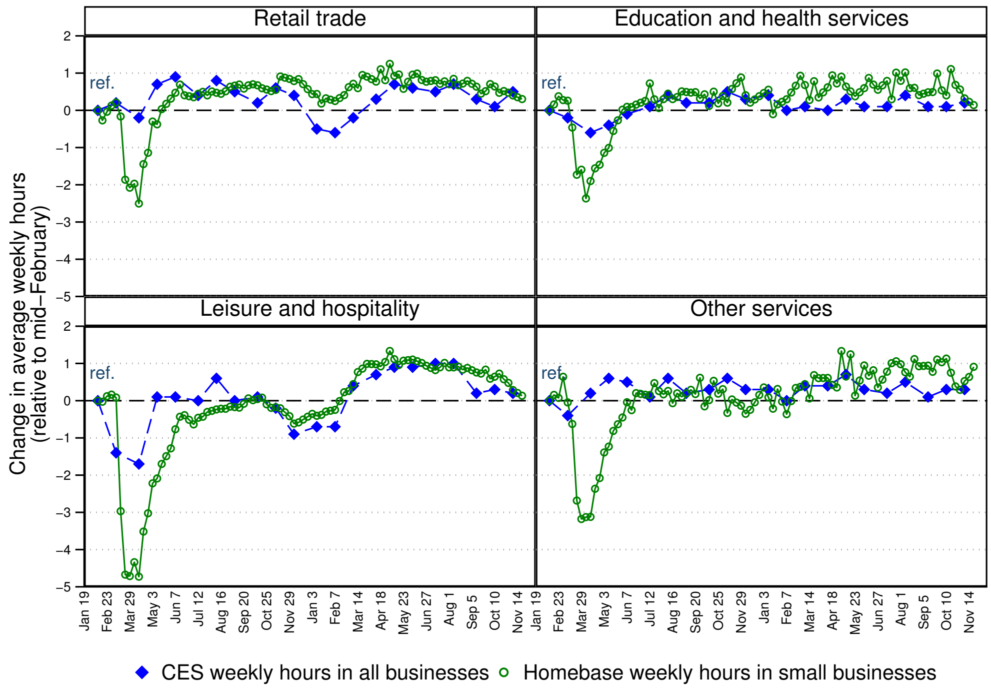
From June 2020 onward, AWH according to both estimates remained around the pre-pandemic level, except for Retail Trade and Leisure & Hospitality where AWH was elevated during 2021, in line with the stronger recovery in employment observed in Figure 2. This suggests that both of these sectors experienced stronger demand during 2021, which accords well with the increase in (inflation-adjusted) output above their pre-pandemic levels observed in national income data.[47]

More generally, the quick recovery of AWH implies that the labor market during the pandemic was

[46] 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 $\widehat{awh}_t = \left(\sum_i \omega_i wh_{i,t}\right) / \left(\sum_i \omega_i e_{i,t}\right)$. 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 $\widehat{AWH}_{t-1} < \widehat{awh}_{t-1}$. Furthermore, the CES makes a slight adjustment to this estimator to account for atypical reports although it is unclear what makes a report atypical.

[47] See https://www.census.gov/econ/currentdata/.

Figure 8: Average Weekly Hours of Small Business Employees



Notes: The figure shows the change in average weekly hours (AWH) of employees in Retail Trade (NAICS 44-45), Education & Health Services (NAICS 61-62), Leisure & Hospitality (NAICS 71-72), and Other Services (NAICS 81) relative to the mid-February 2020 CES estimate (week of Feb 9 – Feb 15). The green solid lines pertain to AWH of employees who remain employed continuously for small businesses with less than 50 employees that are active throughout the sample. The blue dashed lines pertain to AWH as estimated by the CES for businesses of all size classes. See the text for details.

not characterized by a large increase in involuntary part-time employment 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 pandemic and its outsize effect on in-person service sector jobs where part-time is unlikely to be as feasible as in, say, manufacturing that suffered more heavily during previous recessions. Alternatively, the health risks implied by the pandemic and the large extensions of unemployment insurance in response may have reduced the incentives for part-time work. Examining these questions is an interesting topic for future research.

6 Effects of the Paycheck Protection Program

As a second application, we use the HB data to provide new evidence on the extent to which the Paycheck Protection Program (PPP) helped small businesses employment during the initial pandemic shock. The program has been the subject of much controversy and intense research. The novelty of our investigation is that we exploit the high-frequency and geographic granularity of the HB data to estimate the effects of variations in timely access to PPP loans from the many other changes that occurred in the first months of the pandemic. More importantly, we can distinguish the effects of PPP on continuing businesses and on business closings. All of these aspects turn out to be important.

6.1 Delayed access to 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.[48] 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.[49]

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 was 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.[50]

As documented in detail by Bartik et al. (2021), Doniger and Kay (2023), and Granja et al. (2022) among others, the first round of PPP was subject to large geographic disparities in loan allocations, likely reflecting differences in the ability and willingness of local banks to process and approve the large initial influx of loan applications. As a result, 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

[48]For multi-establishment firms in Accommodation & Food Services (NAICS 72), the 500 employee threshold applied to establishments within certain limits.

[49]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.

[50]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 our investigation.

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 the quasi-random presence of different banks across localities.

6.2 Research design

We exploit the geographic variation in initial loan access to evaluate the effects of PPP for small business activity. Similar to Doniger and Kay (2023) 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.[51] As Doniger and Kay (2023) argue, focusing on a relatively narrow window around the temporary exhaustion is important to avoid selection issues associated with the first few weeks of the program.

We construct $sharePPPdelayed_c$ using data on all PPP loans from the SBA. 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 $[26\%, 60\%]$.[52]

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

$$y_{c,t} = \sum_{t=0}^{57} \alpha_t \left(\mathbb{1}\left\{\text{week} = t\right\} \times sharePPPdelayed_c\right) + \mathbf{X'_{c,t}}\boldsymbol{\gamma} + \phi_t + \mu_c + \varepsilon_{c,t}, \tag{6}$$

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 first week of 2020 ($t = 0$); 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 $\mathbf{X_{c,t}}$ 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.[53] Finally, ϕ_t is a week fixed effect capturing time variations in average $y_{c,t}$; μ_c is a county fixed effect controlling for unobserved average differences

[51] 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.

[52] In the regression, we use 1,956 counties for which we have reliable 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 all counties.

[53] See the Appendix for details on the data sources.

across counties; and $\varepsilon_{c,t}$ is the error term. All 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.[54]

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 estimates of 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 and is independent of other factors affecting small business activity during the initial phase of the pandemic.

An obvious concern with this assumption is that $sharePPPdelayed_c$ may reflect at least partly systematic variations in PPP loan demand across counties. If, for instance, small businesses in counties affected more severely by the pandemic were also more likely to apply for a PPP loan earlier, then this would bias our estimates of α_t away from zero. As Doniger and Kay (2023) show, however, there is no clear geographic concentration in loan issuance within the narrow window around the temporary exhaustion of PPP considered here, and $sharePPPdelayed_c$ varies substantially between adjacent counties in the same state. Furthermore, our regressions control for a host of county-specific time-varying factors as well as week fixed effects interacted with a county's pre-pandemic average household income that absorb local demand effects related to a county's affluence.[55]

Another threat to identification could be that counties differ in their distribution of productivity of businesses and that more productive businesses both recovered faster and applied for a PPP loan earlier. Since average differences are absorbed by county fixed effects, this would bias estimates of α_t towards zero only insofar as these growth effects were time-varying during the pandemic. To address this possibility, we also estimate regression (6) at the establishment level and control for establishment fixed effects that differentiate out firm-specific systematic productivity differences. As shown in the Appendix, all the results are robust to these establishment-level regressions and are even somewhat stronger.[56]

6.3 Results

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 small business employment starting the

[54] 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 establishment level, which implicitly weighs counties by the count of HB establishments.

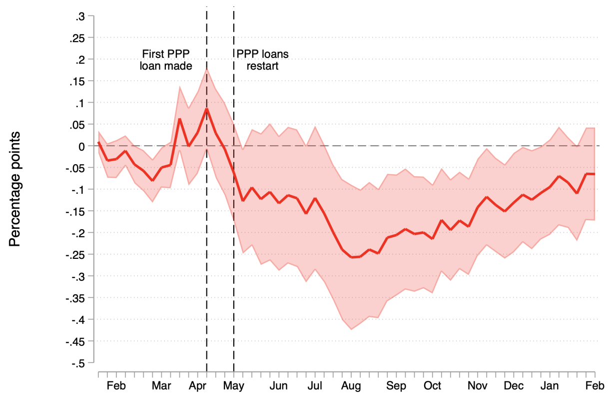
[55] As Chetty et al. (2023) document, more affluent localities suffered relatively larger declines in spending on in-person services and employment in the beginning of the pandemic, presumably due to local demand declining by more in these localities.

[56] The establishment-level regressions also measure $sharePPPdelayed_c$ separately for each of the four service sector considered.

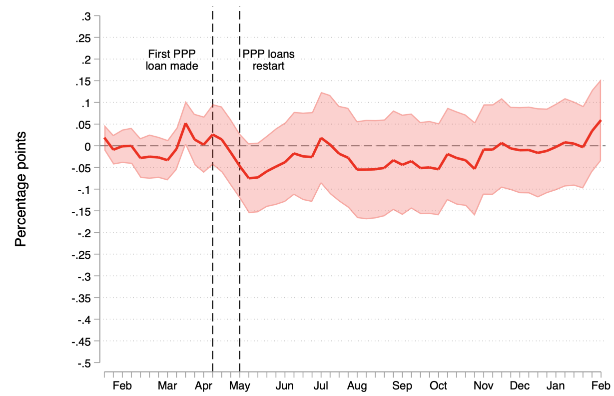
week after the exhaustion of the first round of PPP loans. This negative effect persists throughout 2020 but gradually weakens towards the end of the year.

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

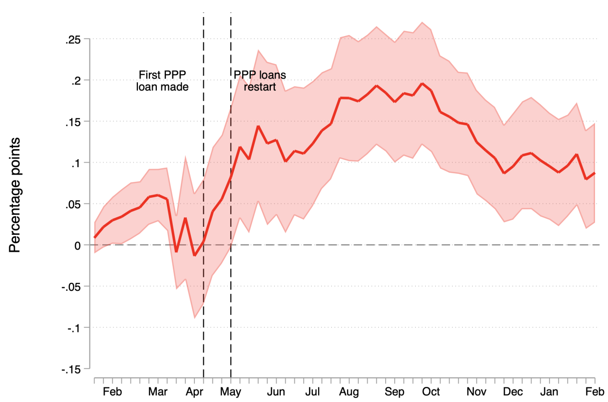
(a) County employment



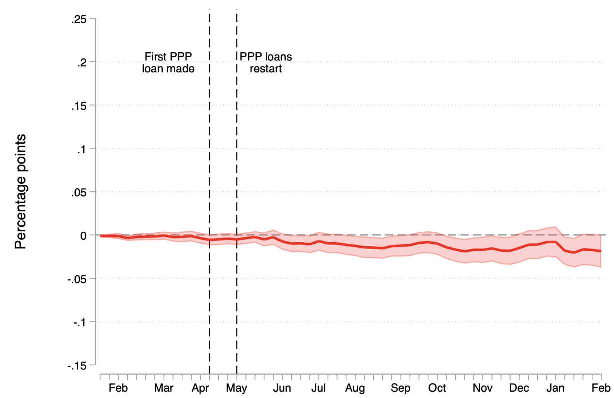
(b) Employment of always active businesses



(c) Business closings



(d) Newly opened businesses



Notes: The figures shows 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 5-11, 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.

The fact that the exhaustion of PPP loans lasted for only 10 days and that the additional funding from PPP approved in late April 2020 was not used up by the time the program stopped taking applications in early August 2020 begs the question of why the negative employment effects of share of PPP are so persistent. To shed light on this question, we run regression (6) separately for employment growth of businesses that are continuously active throughout the sample, the share of business closings, and the share of new business openings (computed, as above, using our methodology that distinguishes business closings and new openings from sample churn and then adjusts for selection).

As shown in panel (b), the effect of PPP loan delays on employment growth by always active businesses is small and insignificant. 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 close to zero and insignificant.[57] In contrast, as shown in panel (c), counties with a larger share of delayed PPP loans experience a significantly higher rate of business closings starting the week of the temporary exhaustion of PPP. This effect peaks from August through October 2020, then declines gradually through Fall of 2020, and stabilizes at about half of its peak value by 2021. Panel (d), finally, shows that the share of delayed PPP loans has only a very small and generally insignificant effect on the rate of new business openings, which confirms the validity of the design since new businesses by definition did not qualify for PPP loans.[58]

One important question about these results is whether they might be driven by pretrends. While there are some deviations from zero of the point estimates prior to the start of PPP, these deviations are not systematic and are therefore likely to reflect short-term noise. Also note that if $sharePPPdelayed_c$ was systematically correlated with county business cycle conditions prior to PPP, then this would likely also affect employment of always active businesses and new openings. Yet this is not the case. As a further robustness check for pretrends, we estimate the same regression for small business revenue from Womply, made available at the county-week level by Chetty et al. (2023). We find no evidence of systematic pretrends in this regression, either.

To interpret the magnitude of the estimated coefficients, consider the difference in share PPP delayed$_c$ 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.

[57]In the establishment-level regressions, this negative effect is somewhat larger and temporarily significant.

[58]As argued by Acemoglu et al. (2018), supporting incumbent businesses could potentially suppress new openings. The slight negative estimates are consistent with this possibility, although the effect is very small.

The result suggests that the 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.[59] The results provide important additional context to Doniger and Kay (2023), whose research design we adopt, but who use monthly household survey data from the CPS that limits their analysis to employment. While our estimates confirm their main finding that PPP loan delay led to persistent negative employment effects that gradually weakened towards the end of 2020, we show that this effect is driven almost entirely by business closings. Moreover, our estimates are based on weekly data that focuses squarely on the smallest businesses in the four service-sectors hit hardest by the pandemic. As a result, our estimates are larger in magnitude and generally more precise, showing that the effects of $sharePPPdelayed_c$ start exactly during the weeks when the PPP loan delay occurs.

6.4 Aggregate employment effects and comparison to the literature

To provide a sense of the the aggregate employment effects of PPP loan delay, we follow an approach similar to Mian and Sufi (2012) and Berger et al. (2020) that is also used in the PPP context by Granja et al. (2022). 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 the case for a small set of counties); i.e.

$$E_{c,t} - \tilde{E}_{c,t} = \frac{\hat{\alpha}_t}{100} \times sharePPPdelayed_c \times E_{c,0} \tag{7}$$

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.[60] 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

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

[60] 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.

and allows us to compare our estimates to other results in the literature.

Given that small business employment in the four sectors considered was about 30 million prior to the pandemic, the point estimate of $\hat{\alpha}_t = -0.25$ for the last week of July 2020 implies that without delays in PPP loans, small business employment in the four sectors would have been about 3 million or 10% higher. In turn, for the last week of January 2021, the estimate of $\hat{\alpha}_t = -0.15$ implies that without delays in PPP loans, small business employment in the four sectors would have been about 1.8 million or 6% higher. Since the delays in PPP loans could have been avoided by appropriating a larger initial amount for PPP in the CARES Act, the costs of doing so 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 larger and longer-lasting decline in service sector jobs.[61]

Our results complement event-study estimates of the effects of PPP by Bartik et al. (2021), Doniger and Kay (2023), and Granja et al. (2022) among others. Granja et al. (2022) 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 impart substantial error in the cross-regional variation in small business employment. 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. Using the same aggregation approach as for employment above, we find that permanent closings would have been reduced by about 5% if there had been no delay in PPP loans. This is lower than the estimates in Bartik et al. (2021), which may be due to the fact that we use actual data as opposed to expectations formed in the initial phase of the pandemic when uncertainty was likely higher. Finally, Doniger and Kay (2023) infer that a reduction in the average share of delayed PPP loans by 10% would

[61] 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 the HB data 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.

have increased private-sector employment by 1.4 to 2.8 million from May through November 2020. This estimate, which implies a substantially larger employment effect of PPP, is difficult to compare to our estimates since it pertains to all private sector employment while we focus on small business employment in the four hardest-hit service sectors. At the same time, our estimates confirm that PPP loan delay had a substantial negative effect on small business employment and provide an explanation for why this effect is persistent: it is in largely due to business closings of which many appear to be permanent.[62]

This finding is confirmed in more recent studies by Dalton (2023) and Autor et al. (2022b) who match establishment-level data from the QCEW and ADP, respectively, to individual PPP loan information and compare employment and closing probabilities of businesses who received a PPP loan earlier with those who received a loan later. Like us, they find sizable effects on employment that are concentrated among businesses with fewer than 50 employees, and that a large part of these employment effects are due to business closures.

Finally, Autor et al. (2022a), Chetty et al. (2023), and Hubbard and Strain (2020) exploit the 500 employee threshold for PPP loan eligibility to estimate the overall impact of PPP. These 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. (2022). This suggests that the effectiveness of PPP could have been enhanced if the program had been restricted at least initially to the smallest businesses.

7 Conclusion

In this paper, we propose a new methodology that exploits information on business activity from alternative sources to distinguish business openings and closings from sample churn in private-sector data, construct an employment estimate that directly incorporates this information, and assess the representativeness of this estimate in various novel ways. Applying the methodology to small business data from Homebase during the COVID-19 pandemic, we find compelling evidence that our methodology produces reliable estimates of small business dynamics and employment even during extraordinary disruptions.

We implement our methods with information on business activity from Google Places, Facebook, and Safegraph, but other datasets measuring business activity could be used as well. Similarly, while the

[62]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, 2021 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.

Homebase data provides an excellent test case, our methods should be useful for other establishment-level datasets as well. As such, we consider our paper as a proof-of-concept on how to combine different data sources to construct employment estimates that directly incorporate the effects of business openings and closings in almost real-time, and that can be benchmarked to official statistics and used to measure the impact of rapidly disseminating shocks and economic policies.

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